Northwestern | INSTITUTE FOR POLICY RESEARCH

Working Paper Series

WP-19-10

Female Disadvantage in Under-Five Mortality in India: Measuring Explicit Gender Discrimination Using Data on Twins

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Version: April 8, 2019

ABSTRACT

Son preference has been linked to excess female under-five mortality in India and considerable literature explores whether parents invest more resources in sons relative to daughters—which the researchers refer to as explicit discrimination—leading to girls' poorer health status and consequently, higher mortality. However, this literature does not adequately control for the implicit discrimination processes that sort girls into different types of families (e.g., larger) and at earlier parities. To better address the endogeneity associated with implicit discrimination processes, the researchers explore the association between child sex and post-neonatal under-five mortality using a sample of mixed-sex twins from four waves of the Indian National Family Health Survey. Mixed-sex twins provide a natural experiment that exogenously assigns a boy and a girl to families at the same time, thus controlling for selectivity into having an unwanted female child. They document a sizeable impact of explicit discrimination on girls' excess mortality in India, particularly compared to a placebo analysis in Africa where girls have a survival advantage. They also show that explicit discrimination has weakened over subsequent birth cohorts since the mid-1990s, especially in northwestern India, thus contributing to understandings of how the micro-processes underlying the female mortality disadvantage have changed over time.

Introduction

Son preference continues to be a defining feature of family life in India, shaping the well-being of Indian women and girls throughout the life course. One of the most striking demographic manifestations of son preference in India is the persistence of excess female infant and child mortality. Despite declining levels of overall under-five mortality, India continues to experience one of the highest levels of excess female under-five mortality in the world (Alkema et al. 2014, Guilmoto et al. 2018, Kashyap 2019). The term 'excess' implies that girls experience higher than biologically expected levels of mortality relative to boys, which, as famously characterized by Amartya Sen, results in women and girls being 'missing' from population structures (Sen 1990).[1]

Considerable demographic literature on son preference explores whether parents invest more resources (e.g. healthcare, nutrition, immunization) in sons relative to daughters—a set of processes which we refer to as *explicit discrimination*— leading to girls' poorer health status and consequently, higher mortality (Miller 1981, Caldwell, Reddy and Caldwell 1982, Das Gupta 1987, Caldwell and Caldwell 1990). However, this literature typically does not adequately control for the processes that sort girls into different types of families and at earlier parities within families. Population-level female disadvantages in mortality can emerge from passive processes—which we call *implicit discrimination*—that arise from son-preferring fertility behaviour (e.g. Clark 2000, Basu and De Jong 2010, Rosenblum 2013, Barcellos, Carvalho and Lleras-Muney 2014). For example, son-preferring fertility stopping rules imply that families may continue to have children until their desired number of sons is reached, which

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[1] The male/female ratio (hereafter, sex ratio) of infant mortality is generally higher than 1, indicative of higher male mortality compared with female mortality. The same pattern also exists in early childhood (between ages 1 to 4 years) although sex ratios are less masculine in most populations in this age group compared with infancy (first 12 months of life). In the first year of life, newborn girls have a biological advantage over boys due to their lower vulnerability to perinatal conditions, congenital abnormalities, and certain infectious diseases such as intestinal and lower respiratory infections (Drevenstedt et al. 2008).

results in girls being born into larger families (Yamaguchi 1989, Clark 2000, Filmer, Friedman and Schady 2009, Rosenblum 2013), and at earlier parities relative to boys (Basu and De Jong 2010). Furthermore, prenatal sex selection in the form of sex-selective abortion allows some families to “opt out” of having daughters (Jha et al 2006, Hu and Schlosser 2015, Anukriti, Bhalotra and Tam 2018, Kashyap 2019).

It is empirically difficult to measure explicit discrimination net of implicit discrimination because prenatal sex selection remains unobserved at the family level and other forms of differential selection, such as family size or birth order, are endogenous to mortality. It is further complicated to explore how explicit discrimination has changed over time given that the implicit processes that might sort girls into qualitatively different households have also changed with diffusion of ultrasound technology, fertility declines, improvements in women’s education, and other social and economic changes. Nonetheless, accurately documenting explicit discrimination is essential from a policy perspective because the policy responses implied by parents’ differential resource allocation into boys versus girls within families are different than if girls’ mortality disadvantage accrue primarily from selection into different families.

To better address the endogeneity of implicit discrimination processes we use a large sample of mixed-sex twins to investigate the association between child sex and post-neonatal under-five mortality with data from four waves of the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016). Mixed-sex twins provide a natural experiment that exogenously assigns both a boy and a girl to families at the same time, thus allowing us to control for differential family selectivity into having an unwanted female child and other implicit discrimination processes. To validate our estimates of explicit discrimination we conduct a placebo analysis using a large sample of twins from sub-Saharan Africa, a region that does not have a history of female mortality disadvantage. We also explore heterogeneity in explicit discrimination by stratifying by region and number of older female siblings. Finally, we investigate whether there have been declines in explicit discrimination over subsequent birth cohorts, thus providing important insight into how the micro- processes that contribute to the female mortality disadvantage have changed over time in contemporary India.

Explicit and implicit discrimination processes that contribute to the female under-five mortality disadvantage in India

Patterns of excess female infant and child mortality arising from a strong preference for male offspring have been long noted in India. Sex ratios of infant and child mortality in India have remained under 1.00, and thus indicative of a female mortality disadvantage that is attributable to social discrimination processes because biologically males are more vulnerable to mortality in infancy, and to a lesser extent, between the ages of 1 – 4 (Alkema et al 2014, Guilmoto et al. 2018, Kashyap 2019). As overall levels of under-five mortality have decreased, absolute differences between male and female mortality levels in India have become smaller in recent history (UNICEF 2018). Excess female mortality levels, calculated as the difference between the estimated and expected female mortality rate given prevailing levels of mortality, have also decreased (Alkema et al 2014, Kashyap 2019), as have the number of missing girls attributable to excess female deaths in India (Bongaarts and Guilmoto 2015).[2] In what follows we highlight both explicit and implicit discrimination processes that contribute to the female under-five mortality disadvantage, with discussion of how these processes may have changed over time.

Excess female infant and child mortality has been thought to arise from explicit postnatal discrimination against girls in the differential allocation of resources to male and

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[2] Sex ratio of male-to-female under-five mortality have shown relative stability and relative measures of excess mortality, computed as the ratio of estimated to expected mortality, continue to show disadvantage in India (Alkema et al 2014, Kashyap 2019).

female children such as healthcare (e.g. immunization, medical treatment) or nutrition (e.g. food, breastfeeding) that are relevant for survival (Miller 1981, Das Gupta 1987, Arnold et al 1998, Pande 2003, Mishra et al 2004), although the empirical evidence for differential allocation of resources has sometimes been mixed or inconclusive (Barcellos, Carvalho and Lleras-Muney 2014). Several studies have found that girls were less likely to receive healthcare and vaccinations (Ganatra and Hirve 1994, Pande 2003, Mishra et al 2004, Borooah 2004, Rajan and Morgan 2018), although some have found similar vaccination rates for boys and girls (Deaton 2003, Barcellos, Carvalho and Lleras-Muney 2014). In terms of intra-household feeding practices, evidence has been mixed, with studies such as Basu (1998) that have questioned the differential provision of nutrition as an important mechanism underpinning female mortality disadvantage. Population-level studies of anthropometric measures linked to nutrition such as malnutrition and stunting also show an ambiguous picture with no clear female disadvantage in these measures, and in some cases a male disadvantage (Mishra et al 2004). Studies, however, have found a clear female disadvantage with respect to duration of breastfeeding (Jayachandran and Kuziemko 2011, Fledderjohann et al 2014, Barcellos, Carvalho and Lleras-Muney 2014). Although in some cases son preference may actually disfavor boys, who may be exclusively breastfed longer in the ages between 6-9 months when breastfeeding alone is not sufficient to meet an infant's energy needs (Mishra et al 2004).

In seeking to reconcile these mixed findings, which on one hand show a female disadvantage in mortality linked to son preference but with weaker evidence for allocation differences and anthropometric measures, studies have argued that the female disadvantage is not generalized but concentrated among certain subsets of girls, particularly those born later in families at later parities and those without existing brothers (Pande 2003, Mishra et al 2004). These findings with a greater disadvantage for some daughters relative to others have also been found for mortality patterns more generally (Muhuri and Preston 1991, Arnold et al 1998). Son preference does not imply that all girls are unwanted but daughters deemed to be "redundant" are more likely to be discriminated against. In contrast to these perspectives, Rajan and Morgan (2018) have argued that generalized discrimination against girls – that affects all daughters at a given parity, rather only than those with older sisters and no brothers – provides a better description of patterns of female disadvantage observed in India for outcomes such as immunization, treatment for respiratory illness and breastfeeding after 17 months.

Most of the abovementioned studies have estimated the effect of being female on a particular investment (e.g. immunization, breastfeeding) or mortality outcome, and compared boys and girls between different families usually in a regression framework. However, in a context where son preference shapes fertility behaviors and family sex composition is not random, girls are likely to be selected into different types of families and this differential selection—which we term *implicit discrimination* —may also affect the observed aggregate-level disadvantage in girls' outcomes. As we describe later, controlling for different forms of implicit discrimination, which may be changing over time, is important but not always straightforward.

For example, studies have found that son-preferring fertility rules result in girls being born into larger families, that is having larger siblingship sizes relative to boys (Yamaguchi 1989, Clark 2000, Basu and De Jong 2010, Rosenblum 2013). Girls as a result of this are likely to share resources in the same family with larger sibling cohorts, and thus will be worse off than boys when comparing boys versus girls between families.[3] Rosenblum (2013) has found that these son-preferring stopping rules that result in girls being born into larger families exacerbate excess female child mortality in India. Couples with first-born boys had fewer total children born and a higher proportion of males in their families. Following from this and using

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[3] Conversely, if there are returns to scale for certain resources in large families then girls might not be worse off but could benefit from these instead (Barcellos, Carvalho and Lleras-Muney 2014).

the sex of the first child as an instrument for sex-differential stopping rules, Rosenblum found that second- and higher-order girls in families with first-born sons had 25% higher mortality than boys, while those in first-born girl households had 38% higher mortality than boys.

Another implication of son-preferring fertility behavior is that girls are more likely to be born at earlier parities within families (Basu and De Jong 2010). The implication of this form of selection for mortality is a priori ambiguous. While some studies find a J-shaped relationship with infant mortality, with first-borns showing the highest risks, others have found a linearly increasing risk from earlier-borns to later-borns, which could protect earlier-born girls (Mishra et al. 2018). On the other hand, Basu and De Jong (2010) hypothesize that in a context with son preference, earlier-born daughters may experience negative consequences for their wellbeing as a result of having to assist in the care of later-born children.

Yet another form of selection that may affect the family conditions into which girls are born is prenatal sex selection, most commonly in the form of sex-selective abortion. Prenatal sex selection became practiced in India starting the early 1990s, as indicated by distorted sex ratios at birth, especially in northwestern Indian states of Punjab, Haryana, Delhi and others (Jha et al. 2006, Guilmoto and Tove 2015, Hu and Schlosser 2015). Whether prenatal sex selection works to protect or worsen girls' mortality outcomes depends on which families are able to access it. If sex-selective abortion enables households with the strongest son preference, and those that might have otherwise resorted to explicit discrimination the option to avoid having unwanted daughter(s), this form of implicit discrimination may work to protect girls (Goodkind 1996, Hu and Schlosser 2015, Kashyap 2019). However, uneven access to technology enabling sex-selection may imply that wealthier families are better able to avoid unwanted female births, and girls may be differentially sorted into households with overall fewer resources because these households cannot afford to opt out of having daughters even if sons are preferred (Hu and Schlosser 2015, Kashyap 2019). This may worsen the population-level disadvantage experienced by girls. Studies from India have found that sex ratios at birth are most distorted among wealthier, urban and more educated families (Jha et al 2006), which has generally been interpreted as a sign of better access to ultrasound technology among these groups (Guilmoto and Tove 2015).

Aggregate indicators—such as the sex-disaggregated under-five mortality rate —mask both explicit and implicit discrimination processes, and cannot adequately capture if the micro-level processes of explicit discrimination have changed over time. Explicit discrimination could have changed over time in India through different channels. Some scholars suggest that diffusion of ultrasound technology could lead to a "substitution" whereby postnatal discrimination in the allocation of nutrition and health resources between male and female children (e.g. explicit discrimination) is weakened as a result of the uptake of prenatal discrimination via sex-selective abortion (e.g. implicit discrimination) (Goodkind 1996, Sen 2003). Evidence for this hypothesis in the Indian context has so far been mixed. Whereas Hu and Schlosser (2015) did not find faster reductions in girls' mortality relative to boys for cohorts that witnessed prenatal sex selection, Anukriti, Bhalotra and Tam (2018) report evidence for faster reductions in girls' mortality in the period in which ultrasound became widely available in India (after 1995). Disentangling the effects of weakening son preference from the practice of sex selection is empirically challenging, however, and son preference can be weakening even as sex ratios at birth become more masculine (Kashyap and Villavicencio 2016). Sex-selective abortion may enable families to reconcile son preference within a small family size, and thus also facilitate fertility decline in contexts with son preference (Kashyap and Villavicencio 2016, Jayachandran 2017).

On the other hand, explicit discrimination could also decline as a result of weakening son preference. There is some indication that son preference, as measured by different indicators of ideal sex composition, may be weakening over time in India with wider processes of development and fertility decline (Bhat and Zavier 2003, Retherford and Roy 2003, Bongaarts 2013, Kashyap and Villavicencio 2017). Increasing adoption of contraception with fertility decline could enable families to realize their desired number of sons while minimizing the total number of children the couple must bear to achieve their son preference (Bongaarts 2013).

Accounting for implicit discrimination in measurement of explicit discrimination

There are a number of ways to potentially measure the explicit discrimination processes that contribute to the female under-five mortality disadvantage, although each approach presents several complications. One strategy for measuring explicit discrimination would be to compare the differential allocation of resources invested in male and female children in the same family (e.g. nutrition, immunizations etc.). In practice, it would be costly if not impossible to observe or ask about all resources or inputs that are invested in all male and female children in the family within a survey instrument. Most surveys capture nutritional investments over a relatively limited period of time, whereas the accumulated differences in resource investment that result in higher female mortality may take place over a much longer time span (e.g. months or years). Parents may also make differential prenatal investments in male versus female fetuses, only some of which may be captured in surveys (Bharadwaj and Lakdawala 2013). Moreover, parents might be prone to reporting bias when describing allocation of resources, particularly for children born several years prior to the survey and parents may also be reluctant to report differential investments in children and/or characterize deceased children as unwanted (Smith-Greenaway and Sennott 2016).

Another approach to measuring explicit discrimination would be to estimate the association between child sex and mortality controlling for birth order, family size, family SES and other measures related to the implicit discrimination processes that sort girls and boys into different families. However, it is difficult to appropriately control for implicit discrimination processes because prenatal sex selection is unobserved at the family level, and variables such as completed family size and birth order are endogenous to mortality. Moreover, controlling for variables such as family size or sex composition compares outcomes of boys and girls in families of the same size, but as Clark (2000) and Barcellos, Carvalho and Lleras-Muney (2014) note, the presence of son-preferring stopping rules implies that girls are, on average, in families that desire fewer sons (than the family of the average child). In other words, even conditional on family size and sex composition, child sex is not exogenous but it is correlated with parental preferences for the sex composition of children.

One approach for addressing the endogeneity of family size, as followed by Rosenblum (2013), has been to capture exogenous variation related to son-preferring stopping rules by using the sex of the first child as an instrument. This instrument however does not account for other forms of implicit discrimination (e.g. birth order, sex-selective abortion) and while this approach clearly demonstrates how sex-differential stopping rules exacerbate mortality outcomes, it is unable to estimate an effect of explicit discrimination, net of implicit discrimination, for girls. An alternative approach, used by Barcellos, Carvalho and Lleras-Muney (2014) to examine if boys and girls receive differential resources, has been to focus on the youngest child in the family – when they are young enough and the next birth has not yet occurred – to measure boy-girl differences in this sample. In the absence of sex-selective abortion, the sex of the child in this sample can be assumed to be exogenous, and consequently, their study focuses on births that occurred before the 1990s, after which sex-selective abortion became practiced in India. Both strategies, by Rosenblum (2013) and Barcellos, Carvalho and Lleras-Muney (2014), face limitations in a context where sex-selective abortion is practiced, and cannot adequately address how explicit discrimination is changing over time.

We propose a novel strategy to better address the endogeneity associated with implicit discrimination processes using a large sample of mixed-sex twin micro-data from four waves of the Indian National Family Health Survey. Mixed-sex twins provide a natural experiment that exogenously assigns both a boy and a girl to families at the same time, thus allowing us to control for implicit discrimination processes such as differential family selectivity into having an unwanted female child and differential birth orders of male and female children. Mixed-sex twins are exposed to the same prenatal environment and are born at the same time and thus exposed to the same family environment (e.g. wealth at birth). Since the principal difference in mixed sex twins is child sex—and not family size, birth order, maternal age, family wealth at birth etc.— elevated female mortality among mixed-sex twins should be more readily attributable to differential parental behaviors based on child sex (e.g. explicit discrimination). This is particularly the case since biologically male children are more vulnerable to death in infancy and early childhood (Drevenstedt et al 2009), so in a context without differential treatment of male and female children we would actually expect a female survival advantage.

Data & Sample

To explore explicit discrimination processes in contemporary India we use pooled standardized data on mixed-sex twins from the 1992-1993, 1998-1999, 2005-2006, and 2015-2016 India National Family Health Survey (NFHS). The NFHS is cross-sectional micro-data on key demographic and health outcomes that is nationally representative of all women ages 15-49 and follows the format and structure of the Demographic and Health Surveys (DHS). The NFHS is collected by the Indian Ministry of Health and Family Welfare and International Institute for Population Sciences, with input from MACRO (ORC).

We identify a sample of mixed-sex twins using the NFHS birth recode, which provides detailed information on all children born to women in the sample. Respondents are queried about whether each child is still alive, and if not at what age in months the child died. Respondents are also asked whether each birth is a multiple birth (e.g. twin birth), birth order, and whether each birth was male or female. Combining this information allows us to identify which births are mixed sex-twins. In supplementary analyses, we confirm that all births reported as mixed sex twins have the same reported birth date. We exclude households with multiple sets of twins and households with triplets and quadruplets due to the exceptional nature of these events, which suggests these household might be categorically different from others in the sample. Our analytical sample includes 6,200 mixed-sex twins from 3,100 families.

Empirical Approach

Measures

Mortality: Our main outcome is a dichotomous indicator of whether the birth resulted in death in infancy or early childhood (e.g. between 1 and 60 months). We exclude mortality in the first month of life to exclude the possibility of neonatal mortality and still births because we are interested in capturing social, rather than biological processes, that impact mortality. All infant and child deaths are self-reported by mothers and thus are subject to reporting bias. Nonetheless, the death of an offspring is a rare and important event, thus it is reasonable to believe that mothers would accurately remember the age of offspring death.

Child sex: Throughout the models the main treatment outcome is a dichotomous indicator of whether the birth was female.

First born twin: We include a control for which twin was born first because on average first born twins are heavier than second born twins, which may have implications for parental investment and later life outcomes (Pongou 2013). We do not include controls for birthweight due to the very high missing values for this variable, but our indicator of first-born twin likely captures whether the twin was higher or lower birth weight due to the correlation between twin birth order and birth weight.

Estimation Strategy

We use a within-twin fixed effects model that allows us to compare boy-girl differences in mortality within twin pairs born into the same family. The fixed effect (in eq. 1, α_i) captures all observed and unobserved factors (e.g. family socioeconomic status and environment, prenatal inputs) shared between the twin pair. For an individual *j* in twin-pair *i*, the within-twin fixed effect model of sex of the child on mortality can be expressed as:

$$Mortality_{ij} = \beta_0 + +\beta_1 FirstBorn_j + \tau Female_j + \alpha_i + \varepsilon_{ij} \quad (1)$$

First, we use the within-twin fixed effects model in eq. 1 to estimate the effect of being female (τ) on the probability of post-neonatal under-five mortality pooling across the four NFHS survey waves. We interpret τ as a measure of the female mortality disadvantage that is attributable to explicit discrimination. To validate our measure, we also conduct a placebo analysis using a large sample of sub-Saharan African (SSA) twins. The Africa data comes from the Demographic and Health Surveys (which are standardized with the NFHS) with multiple survey waves occurring over the same period as the NFHS (see Appendix Table 1 for further detail on the African sample). Because SSA does not show patterns of son-preferring fertility behaviours (Basu and De Jong 2010), and aggregate-level under-five female mortality disadvantage (Alkema et al 2014), we hypothesize that the females in mixed-sex twin pairs in this region should not experience a mortality disadvantage. Thus, if there is a female mortality disadvantage among mixed-sex twins in India, but not SSA (i.e. a higher magnitude of τ in India compared with SSA), this is further evidence of social—as opposed to biological—processes leading to the under-five mortality disadvantage.

Our next step is to assess whether there is evidence of changes in explicit discrimination by running models across three different birth cohorts of mixed sex twins: (1) twins born prior to 1995; (2) twins born between 1995 and 2005; (3) and twins born after 2005. Our birth cohorts roughly correspond with those suggested by Anukriti, Bhalotra and Tam (2018) regarding different periods of diffusion of ultrasound technology in India, whereby the first period represents the early diffusion period when ultrasound technology was still new and less common, the second period represents a period by which ultrasound use is widespread, and the third period corresponds with more recent history in India.[4] Since the 1990s, sex ratio at birth distortions indicative of the uptake of prenatal sex selection have been noted in the Indian population, which could lead to lessening of explicit discrimination within families over time if families are able to "opt out" of having unwanted daughters.

Given evidence that suggests heterogeneity in son preference in India—including by region and by family structure—we also re-run the within-twin fixed effects models stratifying by region and number of older sisters. Roughly following the designation of Dyson and Moore (1983), we distinguish between northwestern states (highest son preference) and other states (lower son preference).[5] As a further extension, we also stratify by both region and birth cohort to see if there are changes over birth cohorts at the regional level. Finally, we re-run our mixed-sex twin fixed effects models stratifying by number of older sisters because explicit discrimination may be more common in families where there are multiple older sisters (Arnold et al 1998, Pande 2003, Mishra et al 2004).

Limitations

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[4] Unlike Anukriti, Bhalotra and Tam (2018), we also use the most recent wave of the NFHS-4, and our sample covers more recent births as well (i.e. after 2005).

[5] The states included in the northwestern category include Bihar, Delhi, Gujarat, Haryana, Himachal Pradesh, Jammu and Kashmir, Madhya Pradesh, Maharashtra, Punjab, Rajasthan, and Uttar Pradesh.

One potential limitation of the within-twin fixed effects approach is if there are unobserved confounders that vary across mixed-sex twins. Unlike identical (or monozygotic) twins who shared 100% of their genetic material, mixed sex twins are fraternal (or dizygotic) and share 50% of their genetic material (about the same amount of genetic material that non-twin siblings share). Thus, it is plausible that there are unobservable genetic differences between twins that lead to differential parental care and attention, net of child sex.

Although twin studies have widely been used to account for unobserved heterogeneity in demographic research (Guo and Tong 2006, Li et al. 2008, Marteleto and de Souza 2012, Pongou 2013, Nisen et al. 2013, Tropf and Mandemakers 2017), there is some concern about the external validity of estimates generated from twin data. One potential issue is that the likelihood of having twins is not random, either because of genetic disposition for twins or because use of assisted reproductive technology (ART) that leads to higher rates of twinning (particularly for dizygotic twins).[6] To partially account for the first possibility, we exclude families with multiple twins in the family (a sign of genetic predisposition for twinning). Although ART has increased over time in India, it is still concentrated to a relatively small urban elite, and thus it is highly unlikely that those who practice ART would be a large enough group to change the trend for the whole country (Smits and Monden 2011).

Appendix Table 2 presents a descriptive comparison of mixed sex twins, same sex twins, and singletons. Consistent with known maternal factors associated with spontaneous twinning of maternal age and birth order (Hoekstra et al 2007), in our sample twins have mothers who are older, and likely in relation to this age pattern, have somewhat higher education than singletons. Twins also have a higher birth order and are born into larger families, but the latter would be expected given that twins contribute two—as opposed to one—

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[6] ART has predominantly been associated with increased rates of dizygotic twinning. Dizygotic twins may be mixed-sex or same-sex, but all monozygotic twins are same-sex (Pison, Monden and Smits 2015).

individuals to the household. The average birth year for both mixed-sex and same-sex twins are the same, which is also suggestive that ART is not driving the composition of mixed-sex twins, which would affect the mixed-sex twins' sample (entirely dizygotic) more than the same-sex sample (both monozygotic and dizygotic).[7]

Another concern related to external validity is whether twin births are a greater negative shock than singleton births, leading twins to receive differential treatment than other types of children. If this is the case, we could interpret our estimates to be upper bound estimates of explicit discrimination. Perhaps the most striking difference between twin-births and singleton-births is the prevalence of infant and child mortality—a finding that holds for both same and mixed sex twins (Appendix Table 2). This corresponds with literature suggesting that twins might be more vulnerable to mortality in infancy or childhood (Monden and Smits 2017). Nonetheless, in the absence of son preference, we should not expect a female disadvantage in mortality among females in mixed-sex twins, even if overall mortality outcomes are worse for twins, which we directly assess with our placebo analysis using a large sample of African twins. Ultimately, the exceptional nature of twin births is what makes them so interesting for our experimental design by allowing us to control for differential family selectivity into having a less desired female child.

A final limitation is that our analysis provides a useful way to assess explicit discrimination net of implicit discrimination, but it does not shed much insight into the specific mechanisms through which explicit discrimination operates. Although we hypothesize that differential allocation of resources is the main mechanism through which the explicit discrimination that leads to elevated female mortality operates, it is difficult to test this directly using NFHS data and the within-twin FE approach. The NFHS collects early childhood health and nutrition measures for children born only in the last five years, leaving us with a small

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[7] We are unable to measure zygosity in the NFHS data.

sample of twins born in the last five years with full nutrition and health information. Even for children born in the last five years, there is no information on key measures (e.g. height-for-age, stunting) for deceased children. Furthermore, children who have died will likely be different in key characteristics (e.g. they might have been breastfed less, have lower probability of vaccination etc.) than children who survived through early childhood, and would not be possible to know whether these differences led to death (e.g. they died because they were not immunized), or whether early death led to these differences (e.g. they would have been immunized if they had survived longer).

Results

Descriptive summary

Table 1 presents proportions (and means for continuous variables) for descriptive characteristics of our twin sample, including how twin characteristics have changed over subsequent birth cohorts. On average, about 9% of the twins in our pooled sample died before the age of five, although survival improves over time. For example, 15% of twins born before 1995 died before the age of five, compared to 8% and 5% of twins born between 1995 and 2005 and after 2005 respectively.

These declines in mortality likely correspond with a number of other important changes in fertility and family life that also occurred over subsequent birth cohorts. For example, between the oldest (e.g. born before 1995) and youngest (e.g. born after 2005) birth cohorts, twins were increasingly born into smaller families at earlier birth orders, which correspond with overall fertility declines in India in recent history. Mothers in the sample are increasingly better educated and have children at older ages, which also makes sense given that these are key correlates of fertility declines. Mother’s stated ideal number of boys also significantly declined over subsequent birth cohorts of twins, although their stated ideal number of girls also declined between the first and third cohort, which may reflect preference for increasingly smaller family sizes. We also see significant declines over birth cohorts in mother's ideal sex ratio of boys to girls, which does suggest some lessening of stated son preference over time. Nonetheless, even in the most recent birth cohort, mother's ideal sex ratio is still heavily skewed towards boys (e.g. 1.27), which suggests the persistence of son preference. Statements about ideal family size do have to be interpreted with some caution since mothers may be reluctant to report current children as unwanted.

Exploring explicit discrimination and the female mortality disadvantage using within-twin fixed effects

To better estimate the effects of explicit discrimination, we start by using a pooled sample of mixed-sex twins to conduct a within-twin fixed effects analysis of the association between child sex and post-neonatal under-five (1—60 months) mortality. This specification allows us to control for implicit discrimination processes by accounting for unobserved twin-level confounders that do not vary between twins (e.g. prenatal conditions, family SES, family size). We find females are associated with a 2-percentage point higher probability of post-neonatal under-five mortality than males ($p<0.001$) (Table 2, Panel A). These results provide strong evidence of explicit discrimination playing an important role in the female mortality disadvantage observed in our data, net of implicit discrimination processes.

To validate our within-twin measure, we conduct a placebo analysis using data on mixed-sex twins from sub-Saharan Africa. In the SSA analysis, we find females are associated with a 1.6 percentage point lower probability of infant and child mortality compared to males ($p<0.001$) (Table 2, Panel A). This finding is consistent with literature suggesting that males are biologically more vulnerable to infant and child mortality than females (Drevenstedt et al 2009, Pongou 2013), which means that in a context without sex-based differences in allocation of resources there should actually be a female under-five mortality advantage. The fact that the SSA results are very different than those presented in India, provides further support that the elevated female mortality observed in our India twin sample captures explicit discrimination behaviors towards daughters.

As a supplementary analysis we also conduct an OLS regression analysis where we regress mortality on child sex using a large sample of singleton children from all four waves of the NFHS (Appendix Table 3). In a baseline model that does not include additional covariate controls other than year of birth and survey round, we find females are associated with a 0.7 percentage point higher probability of mortality than males (p<0.001). In the next model we add additional covariates—including controls for birth order and family size—that although endogenous highlight how child sex is correlated with family characteristics in this context, as the coefficient on sex of the child changes when these observable characteristics related to implicit discrimination are controlled for. In this specification, we find females are associated with a 0.2 percentage point higher probability of infant and early child mortality than males (p<0.001), net of controls for family and child characteristics. While we are hesitant to compare these OLS results to those generated by the within-twin FE due to the very different sample sizes and sample compositions, it is worth noting that the magnitude of the coefficient on the standard OLS models is smaller than those on the within-twin FE models.[8] Although they use a different estimation strategy than ours to exploit random variation in the sex of the child, Barcellos, Carvalho and Lleras-Muney (2014) also find that estimates of gender

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[8] The female mortality disadvantage may be larger among the twin sample than the singleton sample because overall mortality is higher among twin populations (Appendix Table 1). To partially address this, we calculate the predicted probabilities of mortality for females and males using OLS and Twin FE models and use this information to generate crude estimates of the ratio of female to male mortality for the twin and singleton populations. Relative measures like this ratio are useful for comparative purposes when overall mortality levels are different between groups because absolute differences might be more magnified at different levels of mortality. For the singleton population, the crude ratio of female to male mortality is 1.175, whereas for the twin population the crude ratio of female to male mortality is 1.30. This means that the relative excess mortality for girls is about 30 percent for twins and 17.5 percent for singletons, suggesting that in both absolute and relative measures, explicit discrimination is actually stronger than would be expected by comparing the output from our OLS and fixed effects models.

discrimination in health investments and breastfeeding indicated in their “experimental” sample is higher than in standard OLS estimates. This suggests that OLS estimates likely overstate the role of implicit discrimination processes when estimating the female mortality disadvantage as completed family size and birth order variables are likely to capture some of the excluded explicit discrimination processes as well.

Temporal heterogeneity in explicit discrimination and the female mortality disadvantage using within-twin fixed effects

Over the timespan covered in our study there were many important changes in India including declining fertility, socioeconomic development, and diffusion of ultrasound technology to facilitate sex-selective abortion. Thus, our next step is to explore whether there are changes in explicit discrimination over subsequent birth cohorts. As Table 2 Panel B shows, among mixed-sex twins born before 1995 (e.g. before the widespread diffusion of ultrasound technology and uptake of prenatal sex selection), females are associated with a 5-percentage point higher probability of post-neonatal under-five mortality compared to males ($p<0.001$) (Table 2, Panel B). On the other hand, in the latter two birth cohorts when ultrasound technology was widespread, females are associated with 0.8 percentage point and 0.6 percentage point higher post-neonatal under-five mortality compared to males respectively (neither of these coefficients is statistically significant at $p<0.05$). Post-estimation tests of significance suggest the female coefficient in the earliest birth cohort (e.g. before 1995) is significantly different from the female coefficients in the latter two birth cohorts, thus providing evidence of the female mortality disadvantage attributable to explicit discrimination weakening over subsequent birth cohorts. This is an important finding given the difficulties of empirically assessing whether the micro-level processes that underlie the female under-five mortality disadvantages have changed over time.

Heterogeneity in explicit discrimination and the female mortality disadvantage by region and family structure using within-twin fixed effects

As a final step, we also explore whether there is heterogeneity in the female under-five mortality disadvantage depending on region and family structure, which are important correlates of son preference in India. First, we re-run the within-twin FE models stratifying by region because we would expect to see more evidence of explicit discrimination in regions of the country where son preference has historically been the highest such as in northwestern India. Consistent with this hypothesis we find females are associated with a 3.6 percentage point higher probability of post-neonatal under-five mortality than males in northwestern India ($p<0.001$) (Table 3, Panel A), but there is no statistically significant association between child sex and post-neonatal under-five mortality in other regions of the country (Table 3, Panel B). Post-estimation tests of significance suggest the female coefficient in northwestern region specification is significantly larger from the female coefficients in the other region specification.

Since we found evidence of declines in explicit discrimination over birth cohorts in our pooled analysis (e.g. Table 2, Panel A), we also explore whether there is evidence of temporal decline in explicit discrimination in our regional analyses. Among mixed-sex twins born before 1995 (e.g. the earliest birth cohort), we find a sizeable impact of explicit discrimination in the northwestern region, with girls experiencing an 8.6 percentage point higher probability of mortality than males ($p<0.001$) (Table 3, Panel A). In the latter two birth cohorts, the size of this mortality disadvantage attributable to explicit discrimination weakens, to 1.1 and 1.9 percentage point higher probabilities of mortality compared to boys, although neither of these coefficients is statistically significantly different from zero at $p<0.05$. Post-estimation tests of significance indicate the female coefficient in the earliest birth cohort in the northwestern region is significantly different from the female coefficients in the latter two birth cohorts in the northwestern region, which is indicative of lessening explicit discrimination over time in this region where son preference was historically strongest. On the other hand, there is no significant association between child sex and mortality for any of the birth cohorts in the other regions of the country (Table 3, Panel B).

Next, we explore heterogeneity in family structure given literature that suggests sex discrimination is most prevalent among second or third (or higher) daughters (Arnold et al 1998, Pande 2003, Mishra et al 2004). We re-run the within-twin FE models stratifying by number of older sisters and find there is no significant association between child sex and post-neonatal under-five mortality among mixed sex twins with zero or one older sister. On the other hand, females are associated with a 7.5 and 9.4 percentage point higher probability than males of infant and child mortality among twins with two and three older sisters respectively ($p<0.001$) (Table 4). Post-estimation tests of significance indicate the female coefficient in the model with no older sisters is significantly different from the female coefficients in the models with one, two, and three or more older sisters. This suggests that explicit discrimination is experienced particularly by later-born girls who have one or more sisters in the family already, rather than all girls within a family.

Discussion

One of the most striking demographic manifestations of son preference in India is the persistence of excess female under-five mortality. While considerable literature has explored whether parents explicitly discriminate against girls by investing more resources in sons relative to daughters leading to girls' higher mortality, this literature does not adequately control for the *implicit* discrimination processes that sort girls into different types of families (e.g. larger, poorer) and at different birth orders than boys. To better address the endogeneity associated with implicit discrimination processes, we explored the association between child sex and post-neonatal under-five mortality using a sample of mixed-sex twins. We argued that mixed-sex twins provided a natural experiment that exogenously assigned a boy and a girl to families at the same time, thus controlling for family selectivity into having an unwanted female child, birth order, and other implicit discrimination processes.

Our within-twin fixed effects models showed that female children experienced significantly higher probability of post-neonatal under-five mortality. This provided strong evidence of explicit discrimination playing an important role in the female mortality disadvantage observed in our data because our models controlled for implicit discrimination processes that resulted in boys and girls being differentially sorted into different kinds of families and birth orders. The Indian estimates for explicit discrimination were particularly striking when compared to a placebo analysis conducted in sub-Saharan Africa where female twins actually had a survival advantage, which corresponded with literature showing that males have a biological disadvantage in early life.

Using our novel measure, we found that that the role of explicit discrimination underlying the female mortality disadvantage has weakened over time for cohorts born after the mid-1990s in our sample. Subsequent analyses also showed that our results were largely driven by northwestern India, and explicit discrimination declined over time in this region that has historically been characterized by high son preference. While existing literature has found that aggregate measures such as sex differences in under-five mortality (UNICEF 2018), and excess under-five mortality (e.g. Alkema et al. 2014, Kashyap et al. 2018) have become have become smaller over time in India, these aggregate indicators capture mortality attributable to both explicit and implicit discrimination processes. Our results are suggestive that weakening of postnatal explicit discrimination plays an important role in accounting for these changes. Our results were consistent with findings from Barcellos, Carvalho and Lleras-Muney (2013) who, although using a different estimation strategy, found a sizeable difference in investments in girls relative to boys before 1992. Our strategy moreover allows us to explore changes in the period after the early 1990s as well.

Although we are not able to test them directly, several plausible mechanisms have likely contributed to the reductions in explicit discrimination. These include the weakening of son preference in India through socioeconomic development, as well as through programs targeted at reducing son preference, as well as wider processes of fertility decline. Indeed, compared with the pre-1995 period, we too find that later cohorts of mixed sex twins are born into smaller families with lower indicators of stated son preference. Existing research has found that fertility decline is correlated with weakening son preference and increasing adoption of contraception with the fertility transition have also enabled families to realize their desired number of sons while minimizing the total number of children the couple must bear to achieve their son preference. With reductions in overall family size, differences in resource allocations may become less pronounced. Improvements in healthcare and nutrition could have also played an important role in reducing sex differences in health.

Alternatively, explicit discrimination may have also partly weakened due to families substituting postnatal discrimination in resources with prenatal discrimination in the form of sex-selective abortion, which has allowed those with son preference to “opt out” of having unwanted daughters and thereby reconcile son preference with smaller family size (e.g. Goodkind 1996, Kashyap 2019). Nevertheless, we anticipate a combination of factors – declining son preference, fertility decline, as well as the ability to realize son preference at lower parities due to the practice of sex-selective abortion –underpin the weakening of explicit postnatal discrimination in India.

Although our paper has presented a novel measure of explicit gender discrimination underlying mortality, it had several limitations. First, we were limited in our abilities to assess the specific mechanisms through which explicit discrimination occurred and whether certain resources and inputs were more important than others in predicting mortality (e.g. breastfeeding, immunization etc.). Our ability to generalize beyond the sample of twins might also be limited by the exceptional nature of twin births. Nonetheless, the fact that results from OLS models conducted on singletons were similar to the within-twin FE approach (albeit smaller in magnitude) was suggestive that the twin fixed effects models captured a phenomenon found in non-twin populations, and also followed forms of heterogeneity in region and family structure we would expect in this setting. Our temporal analysis further highlighted that – even if we interpreted twins as an upper bound estimate – explicit discrimination weakened across birth cohorts.

While the approach presented in our paper does not provide an all-encompassing measure of son preference (indeed it is one of several ways to explore son preference), we demonstrated the importance of better accounting for the implicit discrimination processes that sorted boys and girls into different types of families when empirically assessing explicit discrimination and changes in the female under-five mortality disadvantage over time. Conceptually, our analysis highlighted the importance of distinguishing between two distinctive mechanisms of gender discrimination – explicit and implicit – when assessing a population level estimate of female mortality disadvantage in contexts with son preference. This was important because the family-level processes of gender discrimination that each type of discrimination implied were quite different, as are the policy responses required to address them. If parents practice explicit discrimination and allocate resources differently to boys and girls within households, then transferring resources to households that are disadvantaged may not be enough – but if girls' outcomes are the consequence of differential selection into different types of families, policy responses could target responses to these families instead.

While our analysis was focused on India, the points we raise about the different processes behind both explicit and implicit discrimination can also be applied to other high son preference contexts in South, East, and Central Asia. This distinction may become particularly important as wider changes such as fertility decline and technology diffusion continue across countries with historically strong son preference, implying that changing family selectivity — as opposed to differential resource allocation within families—could become a particularly important mechanism through which the mortality manifestations of son preference emerge. Future research should explore the mechanisms of both explicit and implicit discrimination, and better understand the interrelationship between changing son preference, fertility decline, and excess female mortality.

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Table 1. Descriptive summary of background characteristics of mixed-sex twin analytical sample (pooled and disaggregated by birth cohort), including tests for significant difference between the first birth cohort and the two subsequent cohorts. All estimates are unweighted and use pooled data from the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016).

	Proportion among:			
	(1) *Pooled* (n=6200)	(2) *Born before 1995* (n=1868)	(3) *Born 1995-2005* (n=2278)	(4) *Born after 2005* (n=2054)
Mortality (1-60 mos.)	0.09	0.15	**0.08**	**0.05**
Female	0.50	0.50	0.50	0.50
Birth year	1999	1986	**2000**	**2010**
Birth order	3.28	3.56	**3.23**	**3.08**
Rural	0.67	0.63	**0.68**	**0.70**
Northwestern region	0.56	0.57	0.55	0.56
Hindu	0.75	0.75	0.75	0.75
Mother no school	0.44	0.53	**0.44**	**0.36**
Mother primary school	0.15	0.20	**0.15**	**0.11**
Mother secondary school	0.32	0.23	**0.33**	**0.40**
Mother tertiary	0.08	0.05	**0.07**	**0.13**
Mother age at birth	25.68	24.80	**25.44**	**26.74**
Total children born to mother	4.37	5.08	**4.29**	**3.80**
Mother's ideal number of boys	1.41	1.54	**1.39**	**1.32**
Mother's ideal number of girls	1.12	1.15	1.13	**1.09**
Ideal sex ratio (ideal boys/ideal girls)	1.30	1.39	1.27	1.27

Notes: All measures are dichotomous except birth year (ranges from 1958 to 2016), birth order (ranges from 1 to 11), mother age at birth (ranges from 12 to 47), total children born (ranges from 2 to 13), mother's ideal boys (ranges from 0 to 7), and mother's ideal girls (ranges from 0 to 6). Bold numbers indicate statistically significant ($p<0.05$) difference between the birth cohort in question and the first birth cohort (e.g. born before 1995). Two-sample t tests for all continuous outcomes, and chi-square tests for all dichotomous outcomes.

Table 2. Within mixed sex twin fixed effects models of the association between child sex and infant and child mortality (1-60 months) in India and Africa (Panel A) and within mixed sex twin fixed effects models of the association between child sex and infant and child mortality (1-60 months) in India over different time periods, including tests of significance across models. All estimates use pooled data from the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016) and Panel A also uses pooled data from the Demographic and Health Surveys in Africa (See Appendix Table 1 for full list of African countries). Analysis conducted in STATA 15.

Panel A.	(1)	(2)
	Mortality 1-60 mos. *India*	Mortality 1-60 mos. *Africa*
Female	0.020**	-0.016***
	(0.007)	(0.005)
First born twin	-0.038***	-0.041***
	(0.007)	(0.005)
Constant	0.101***	0.211***
	(0.005)	(0.004)
Observations	6,200	17,963
R-squared	0.015	0.008
Number of families	3,100	8,988

Panel B.	(1)	(2)	(3)
	Mortality 1-60 mos. *India Born before 1995*	Mortality 1-60 mos. *India Born 1995-2005*	Mortality 1-60 mos. *India Born after 2005*
Female	0.050***	**0.008**	**0.006**
	(0.015)	**(0.010)**	**(0.009)**
First born twin	-0.020	-0.047***	-0.043***
	(0.015)	(0.010)	(0.009)
Constant	0.136***	0.096***	0.073***
	(0.012)	(0.007)	(0.006)
Observations	1,868	2,278	2,054
R-squared	0.016	0.020	0.023
Number of families	934	1,139	1,027

*** p<0.001, ** p<0.01, * p<0.05

Notes: In Panel B, bold numbers indicate statistically significant (p<0.05) difference between the birth cohort in question and the first birth cohort (e.g. born before 1995).

Table 3. Within mixed sex twin fixed effects models of the association between child sex and infant and child mortality (1-60 months) for Northwestern regions (Panel A) and other regions (Panel B). All estimates use pooled data from the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016). Analysis conducted in STATA 15.

Panel A. Northwestern regions	(1)	(2)	(3)	(4)
	Mortality 1-60 mos.	Mortality 1-60 mos.	Mortality 1-60 mos.	Mortality 1-60 mos.
	Pooled	*Born before 1995*	*Born 1995-2005*	*Born after 2005*
Female	0.036***	0.086***	**0.011**	**0.019**
	(0.009)	(0.020)	**(0.015)**	**(0.013)**
First born twin	-0.046***	-0.008	-0.061***	-0.063***
	(0.009)	(0.020)	(0.015)	(0.013)
Constant	0.112***	0.135***	0.117***	0.084***
	(0.008)	(0.018)	(0.013)	(0.012)
Observations	3,482	1,074	1,248	1,160
R-squared	0.026	0.035	0.030	0.045
Number of families	1,741	537	624	580

Panel B. Other regions	(1)	(2)	(3)	(4)
	Mortality 1-60 mos.	Mortality 1-60 mos.	Mortality 1-60 mos.	Mortality 1-60 mos.
	Pooled	*Born before 1995*	*Born 1995-2005*	*Born after 2005*
Female	-0.002	0.004	0.001	-0.012
	(0.009)	(0.021)	(0.013)	(0.012)
First born twin	-0.026**	-0.032	-0.029*	-0.017
	(0.009)	(0.021)	(0.013)	(0.012)
Constant	0.085***	0.134***	0.071***	0.058***
	(0.008)	(0.019)	(0.012)	(0.011)
Observations	2,718	794	1,030	894
R-squared	0.006	0.006	0.009	0.006
Number of families	1,359	397	515	447

*** p<0.001, ** p<0.01, * p<0.05

Notes: Bold numbers indicate statistically significant (p<0.05) difference between the birth cohort in question and the first birth cohort (e.g. born before 1995).

Table 4. Within mixed sex twin fixed effects models of the association between child sex and infant and child mortality (1-60 months) disaggregated by number of older sisters. All estimates use pooled data from the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016). Analysis conducted in STATA 15.

	(1) Mortality 1-60 mos. No older sisters	(2) Mortality 1-60 mos. One older sister	(3) Mortality 1-60 mos. Two older sisters	(4) Mortality 1-60 mos. Three older sisters
Female	-0.011	**0.025**	**0.075*****	**0.094*****
	(0.008)	**(0.013)**	**(0.020)**	**(0.024)**
First born twin	-0.027***	-0.049***	-0.052**	-0.070**
	(0.008)	(0.013)	(0.020)	(0.024)
Constant	0.091***	0.113***	0.113***	0.102***
	(0.007)	(0.012)	(0.017)	(0.021)
Observations	3,026	1,740	930	504
R-squared	0.008	0.021	0.047	0.082
Number of families	1,513	870	465	252

*** p<0.001, ** p<0.01, * p<0.05

Notes: Bold numbers indicate statistically significant (p<0.05) difference between the sister category in question and no older sisters.

Appendix Table 1. Overview of Sub-Saharan Africa DHS survey year and samples used

Country	**Year**	**Year**	**Year**	**Year**	**Year**	**Year**
Benin	1996	2001	2006	2011-12		
Burkina Faso	1993	1998-99	2003	2010		
Cameroon	1991	1998	2004	2011		
Ghana	1993	1998	2003	2008	2014	
Kenya	1993	1998-99	2003	2008	2014	
Madagascar	1992	1997	2003-04	2008-09		
Malawi	1992	2000	2004	2010		
Mali	1995-96	2001	2006	2012-13		
Namibia	1992	2000	2006-07	2013		
Niger	1992	1998	2006	2012		
Nigeria	1990	2003	2008	2013		
Rwanda	1992	2000	2005	2010	2014-15	
Senegal	1992	1997	2005	2010-11	2012-13	2014
Tanzania	1991-92	1996	1999	2004-05	2010	
Uganda	1995	2000-01	2006	2011		
Zambia	1992	1996	2001-02	2006	2013-14	
Zimbabwe	1994	1999	2005-06	2010-11		

Appendix Table 2. Descriptive comparison of singletons, mixed sex twins, and same sex twins. All estimates are unweighted and use pooled data from the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016).

	Proportion among:		
	(1)	(2)	(3)
	Singletons	*Mixed sex twins*	*Same sex twins*
	(n=1,835,281)	(n=6200)	(n=12,622)
Mortality (1-60 mos.)	0.04	**0.09**	**0.10**
Female	0.48	**0.50**	**0.49**
Birth year	1996	**1999**	**1999**
Birth order	2.54	**3.28**	**3.14**
Rural	0.72	**0.67**	**0.68**
Northwestern region	0.56	**0.56**	**0.51**
Hindu	0.74	**0.75**	**0.71**
Mother no school	0.53	**0.44**	**0.44**
Mother primary school	0.16	0.15	0.16
Mother secondary school	0.27	**0.32**	**0.33**
Mother tertiary	0.04	**0.08**	**0.07**
Mother age at birth	23.73	**25.68**	**25.08**
Total children born to mother	4.04	**4.37**	**4.47**
Mother's ideal number of boys	1.49	**1.41**	**1.45**
Mother's ideal number of girls	1.14	1.12	1.13

Notes: Singleton sample excludes families with twins and families with less than two children. All measures are dichotomous except birth year (ranges from 1954 to 2016), birth order (ranges from 1 to 18), mother age at birth (ranges from 10 to 50), total children born (ranges from 2 to 18), mother's ideal boys (ranges from 0 to 20), and mother's ideal girls (ranges from 0 to 11). Bold numbers indicate statistically significant ($p<0.05$) difference between the group in question and singletons. Two-sample t tests for all continuous outcomes, and chi-square tests for all dichotomous outcomes.

Appendix Table 3. OLS regression models of the association between child sex and infant and child mortality (1-60 months) using a sample of singleton children. All estimates use pooled data from the Indian National Family Health Survey (1992-1993, 1998-1998, 2005-2006, 2015-2016). Analysis conducted in STATA 15.

	(1)	(2)
	Mortality 1-60 mos.	Mortality 1-60 mos.
Female	0.007***	0.002***
	(0.000)	(0.000)
Birth year	-0.169***	-0.120***
	(0.006)	(0.006)
Birth year square	0.000***	0.000***
	(0.000)	(0.000)
Survey round	-0.001*	-0.009***
	(0.000)	(0.000)
Birth order		-0.008***
		(0.000)
Mother primary school		-0.011***
		(0.000)
Mother secondary school		-0.012***
		(0.000)
Mother tertiary		-0.007***
		(0.001)
Mother's age at birth		-0.005***
		(0.000)
Mother's age at birth square		0.000***
		(0.000)
Mother's parity		0.019***
		(0.000)
Rural		0.007***
		(0.000)
Constant	169.984***	120.154***
	(6.149)	(5.932)
Observations	1,835,281	1,835,281
R-squared	0.013	0.036

Notes: Singleton sample excludes families with twins and families with less than two children. Robust standard errors in parentheses clustered at the family level.

*** p<0.001, ** p<0.01, * p<0.05