CHILD GENDER AND PARENTAL INVESTMENTS IN INDIA: ARE BOYS AND GIRLS TREATED DIFFERENTLY?*

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Abstract. Previous research does not always find that boys and girls are treated differently in India. But son-biased stopping rules imply that previous estimates of the effect of gender on investments are likely to be biased. We propose a novel identification strategy to overcome this bias. We document that boys receive significant more childcare time compared to girls. In addition boys are more likely to be breastfed for longer, given vaccinations and vitamin supplementation. We find no evidence that the differential treatment of boys is due to their greater needs, or to the effect of anticipated family size.

I. Introduction

Women in developing countries fare worse than men in many dimensions: they obtain less schooling, have lower labor force participation, earn lower salaries, are more likely to be poor and often lack fundamental rights such as voting rights or the right to earn property (Duflo 2005). One often cited extreme manifestation of this phenomenon is that mortality rates for girls are substantially higher than for boys in many developing countries (Chen, Huq and D'Souza 1981; Arnold, Choe and Roy 1998, Sen 1990), though this is not true in the Western World (United Nations Secretariat 1988). These patterns are particularly marked in countries with "son preferences" such as India, where families have explicit preferences for having sons over daughters (Pande and Astone 2007).

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Surprisingly however the previous literature does not always support the hypothesis that these differences in outcomes are due to explicit differential treatment of boys and girls. While many papers find evidence that boys receive more nutrition (Chen, Huq and D'Souza, 1981; Das Gupta, 1987), more healthcare (Basu 1989, Ganatra and Hirve, 1994), are breastfed for longer (Kuziemko and Jayachandra 2010) and are more likely to be vaccinated (Borooah 2004) than girls, other papers find no evidence of differential treatment. For example review studies by Sommerfelt and Arnold (1998) and Marcoux (2002) find no differences in anthropometric measures, Hariss (1995) finds that girls receive just as much nutrition as boys in India, and Deaton (2003) reports that vaccination rates are identical for boys and girls in India. Most notably Deaton (1997) reviews studies that use the adult goods method and states that there is no evidence that parents spend more on boys than on girls. Duflo (2005) concludes that"[e]ven in the countries where the preference for boys is strongest, it is hard to find evidence that girls receive less care than boys under normal circumstances."[1]

However previous work assumes that boys and girls live in families with similar characteristics, both in terms of observables and unobservables. But this assumption is incorrect if families have a preference for sons and follow male-biased stopping rules of childbearing, (Yamaguchi 1989, Jensen 2005) which appears to be the case in India.[2] As a consequence, empirical estimates of discrimination are biased.

We propose a novel empirical strategy that addresses these issues. It relies on the observation that—in the absence of sex-selective abortion—the child sex is random at birth. If the child sex is random, then families that just had a boy are identical to families that just had a girl in terms of predetermined characteristics. Therefore, any differences we observe in terms of parental inputs can be attributed to the sex of the newborn. However, over time a correlation will develop between the youngest child's gender and family characteristics, because the families that had a girl are less likely to stop having children. To overcome this problem, we restrict our sample to families with children that are still "young enough" and haven't had the opportunity to have other children. The data suggest that families with boys and girls zero to 15 months of age

[1] But households do favor boys in bad times (Bherman 1988, Rose 2000, Miguel 2005, Maccini and Yang 2008).
[2] For example families with fewer boys have shorter birth intervals, are more likely to want more children and to continue having children, and are less likely to use contraception (see Clark 2000 for a review).

(and possibly a bit older) look identical in terms of observables—we use them to study whether boys are given more inputs than girls.

The second contribution of this paper is to use our identification strategy to investigate whether boys and girls are treated differently in terms of an important but not frequently studied type of investment in children: *childcare time*. Starting with Becker (1965), economists have recognized that, in addition to money, time is a key input into the "child production function". Time is particularly important to the extent that it is complementary to many other inputs. For example, feeding children requires both food and the time to cook and feed the children. However no estimates of gender differences in parental time allocation from dedicated time use surveys exist for India.[3] Using data from the Indian Time Use Survey, we investigate whether families spend more time on childcare when a boy is born instead of a girl. We also study gender differences in other frequently studied measures of parental investments such as vaccinations using Indian Demographic and Health Survey (hereafter DHS).

The results indicate that families treat boys and girls differently. Households with an infant boy under one spend roughly 30 minutes more per day—or 14% more time—with childcare than households with a infant girl, and the quality of the childcare also appears higher for baby-boys. This gender difference appears for different kinds of childcare, including supervision and physical care. The effect is larger for households with only one child under the age of six, who spend more than 60 minutes more per day (about 30% more) when their youngest is a boy. In addition our results show that boys are more likely to be vaccinated, to be breastfed longer and to be given vitamin supplementation. In general we find these inputs to be at least 10% higher for boys. We do not however find evidence that boys fare better than girls in terms of anthropometric measures, outcomes rather than input measures. We discuss various explanations for this one anomalous result.

Our approach has several limitations. Like previous work we cannot fully address the issues of sex-selective abortion and differential mortality. These behaviors will likely bias our estimates of boy-girl differences towards 0, so our effects can be taken to be lower bounds. To limit the potential bias from sex-selective abortion we limit our study to the 1992 DHS, the most recent DHS survey conducted before ultrasound technology became widely available (Bhalotra

[3] Yeung et al (2001), Lundberg et al (2007), Mammen (2009) report that fathers spend more time with boys in the US. Rose (2000) reports that in rural India women work fewer days after the birth of a boy relative to a girl.

and Cochrane 2010). Our results suggest that the bias associated with sex-selective abortion in our sample is small, consistent with Anderson and Ray (2010), who report that prenatal factors only account for 10% of missing women in India. To assess the bias caused by postnatal mortality we compute bounds for our estimates. The bounds suggest that mortality potentially generates large biases: differences between boys and girls could be as much as 50% larger than our baseline estimates. Another limitation of our results is that we can only study children who are under the age of two. This is an important subset of the population because investments at this age have large returns in the short and long run. Lower investments in childhood are associated with worse health and economic outcomes in adulthood, and for next generations (Almond and Currie 2010). But we cannot study older children.

Although we cannot provide conclusive evidence on why parents give girls fewer resources, we investigate some possibilities. Parents might prefer boys to girls; investments in boys might have larger returns (e.g., men have higher wage rates than women); boys might be seen as needing more resources; and finally, families that have girls might anticipate that eventually their family size will be larger. We provide suggestive evidence that boys do not in fact "need" more than girls: if we look at South Africa, a developing country with data on investments and no evidence of son preference, we find that there are no systematic gender differences in most inputs. We also investigate whether our results are driven by the change in anticipated family size. We find little evidence to support this theory. Therefore our findings suggest that higher returns or preference for boys drive the differential investments.

II-Identification issues in the presence of son-biased stopping rules

In this section, we present a simple model of son-biased fertility stopping rule and extend previous results by Yamaguchi (1989) and Jensen (2005) to show that—if families follow this rule—the estimates in the boy-girl discrimination literature, which assume that boys and girls live in families with similar characteristics, are biased. We then proceed to propose a method to overcome the problems that arise in this context.

We begin by presenting suggestive evidence that families in rural India follow son-biased stopping rules. Families follow son-biased stopping rules if *ceteris paribus* the probability of a family stopping having children after a boy is higher than after a girl. One implication of this behavior is that the probability of a family's youngest child being a boy is an increasing function of the age of the youngest child. At birth the sex ratio is determined by biological odds. But as the youngest child ages, the sex ratio is increasingly skewed towards boys since families are more likely to stop having children after a boy. This prediction is consistent with the data. In Figure 1, we plot the fraction of boys by age using data from the 1992 DHS (described in greater detail below) . The figure shows that the fraction of boys among all living children is somewhat constant across ages. But among the youngest in the family the fraction of boys increases from 51% for children 0-5 months old to 58% for the youngest children 54 to 59 months old. In other words if a child is still the youngest at age 4, the probability that the child is a boy is 58%. This is a large deviation from the natural sex ratio at birth. This evidence suggests that in India families do indeed follow son-biased stopping rules.[4]

We use the framework from Yamaguchi (1989) to illustrate how these stopping rules bias estimates of discrimination. There is a continuum of families with heterogeneous preferences for sons and they all follow the same stopping rule. Family h wants to have a specific number of sons S_h, and it continues to have children until it reaches its desired number of sons. The total number of children of family h is given by N_h. The probability of a newborn being a boy p is assumed to be constant across families.

In this stylized model, one can show that on average boys and girls live in families with different observed and unobserved characteristics. First, as shown by Yamaguchi (1989) (and by Jensen, who extends the results to a finite fertility set-up), girls will have on average more siblings than boys. A simple example provides intuition for the result. Take for example a family that wants one boy. If the first-born is a boy, then the family stops having children, but if the first-born is a girl the family continues having children. If all families behave this way then all girls have siblings but not all boys do. Simulations in Jensen (2005) suggest that the resulting differences in sib-ship size can be quite large.

Second, one can also show (Appendix 1) that if we compare children in families of the same size, on average girls are in families that desire fewer sons than the family of the average child. The intuition for this result is as follows. Suppose that we observe two families that

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[4] This pattern could also be driven by excess girl mortality. To gauge its importance, we compute the fraction of boys among all youngest children (including those that died according to the mother) and compare it to the fraction of boys among those that are alive. The graph suggests that there is excess girl mortality, since the fraction of boys is higher among the survivors. However, the extent of the bias is small relative to the effect of stopping rules. This is confirmed by the pattern that we observe among all children (rather than the youngest): the fraction of boys rises for this group but the increase is small, much smaller than what is observed among the youngest child.

stopped having children after their second child. Family A has a girl and a boy, and family B has two boys. Family A stopped having children in spite of the fact that they have only one boy, whereas family B stopped only because they had two boys, but would have continued otherwise. The example illustrates that for families of size 2 girls are in families that desire fewer sons than the average family. As a consequence, within families of the same size, families with a larger number (share) of boys have a larger desired number of sons than families with many girls.

In the stylized model by Yamaguchi, families always obtain their desired number of sons, so we would observe their preferences by observing the number of sons they have. In reality, since families have finite fertility rules and also imperfect fertility control, the desired number of sons is unobservable. Thus even if we control for the observed differences in family size and gender composition, there are unobserved differences in the families into which the average girl and average boy are born. If families that desire a larger number of sons invest less in girls (or more on boys) than other families, then these stopping rules imply that previous estimates of discrimination are biased, as we now discuss.

Suppose that we obtain estimates of boy-girl differences by running a regression of some measure of child investment on a constant and a boy dummy (as in Sen and Sengupta 1983; Das Gupta 1987; Sommerfelt and Piani 1997):

$$Z_{ih} = \alpha_0 + \alpha_1 * B_{ih} + u_{ih},$$

where Z_{ih} is investment in child i in household h, B_{ih} is a dummy that is equal to 1 if child i in household h is a boy and u_{ih} is an error term. Son-biased stopping rules imply that B_{ih} is correlated with family size. Therefore, B_{ih} will be correlated with the error term and α_1 will be biased if child investment depends on the number of children in the family. The sign of the bias may be different for different measures of child investment. On the one hand, children in large families may have to share resources with more siblings (e.g., food)—this is the issue that Jensen (2005) investigates. On the other hand, children in large families may *ceteris paribus* receive more investments if there are large returns to scale to the child investment (e.g., vaccination in public campaigns, or supervision and teaching).

Given that girls tend to be in larger families than boys, it may seem reasonable then to control for family size. Suppose then that we estimate the following model instead (Oster 2009):

$$Z_{ih} = \alpha_0 + \alpha_1 * B_{ih} + X_{ih}\rho + u_{ih},$$

where now we are controlling for X_{ih}, a vector of controls that includes the number of siblings or dummies for the sex-composition of siblings. This strategy essentially compares outcomes of boys and girls in families of the same size. But son-biased stopping rules imply that, conditional on family size, girls tend to be in families that want girls more than other families. In other words, the child's sex is not exogenous; it is correlated with parental preferences for the gender composition of children. A similar argument applies to studies that use the adult goods method championed by Deaton (1989). In general the sign of the bias is unknown, and depends on the relationship between preferences for the gender composition of children and treatment of boys and girls (see Appendix 2). For example if all families invest the same in boys but families that want boys invest less in girls, then OLS estimates of α_1 are downward biased because the average girl is in a family that wants fewer boys and thus receives more child investments than she would have had she been "assigned" to a random family.

III. Empirical Strategy

Our empirical strategy relies on the observation that—in the absence of sex-selective abortion—the child sex is random at birth. If the child sex is random, then families that just had a boy are identical to families that just had a girl in terms of predetermined characteristics. Therefore any differences we observe in terms of parental inputs can be attributed to the sex of the newborn. Over time, however, this is no longer true. Because families that follow a son-biased stopping rule are more likely to stop having children after a boy, in time a correlation will develop between the youngest child's sex and preferences: families with N children that stop after a girl tend to like girls more than families with N children that stop after a boy. To overcome this problem, we restrict our sample to families in which the youngest child is "young enough." We determine this using our data: we select the sample such that baby-boy and baby-girl families look identical in terms of observable characteristics. Formally, we estimate whether boys and girls are treated differently using the following equation:

$$Z_{ih} = \alpha_0 + \alpha_1 * B_{ih} + X_{ih}\rho + u_{ih}.$$

The OLS estimate of α_1 is an unbiased estimator of the parameter of interest if the child's sex is exogenous (conditional on X) —i.e., $Cov(b_{ih}, u_{ih}|X)=0$. Our identifying assumption is that the child's sex is exogenous at birth *for children that are young enough*. In the next section, we provide evidence that predetermined characteristics (in particular number and gender of siblings) are not correlated with gender for very young children. We also show that, as the model above predicts, this no longer holds true as the family's youngest child gets older. Notice that, if gender is indeed random, then we do not need to condition on any variables.[5] Conditioning on predetermined variables should have no impact on our point estimates and should reduce the standard errors (if these variables predict parental investments).

Our assumption may fail if there is sex-selective abortion against girls or excess girl mortality. We test this directly in the data by comparing the characteristics of families with a baby girl and a baby boy. However it is possible that the family differ in terms of unobservables. Sex-selective abortion and excess female mortality most likely bias our estimator against finding boy-girl differences: since the surviving girls are expected to be in families that like girls more than the average family, they should receive more care than they would have otherwise.[6] Thus, our estimates can be taken as lower bound estimates of gender differences in child investments.

IV- Testing Random Assignment and Selecting the Estimation Sample

To test that the gender of the youngest child is uncorrelated with predetermined family characteristics we restrict the sample to children who are the youngest in their families and estimate the following linear equation

$$I(boy_{ia}=1)=X_i\beta_a+\varepsilon_{ia},$$

where the dependent variable is an indicator of whether child i in age category a is a boy, and X is a set of predetermined characteristics. Independence implies that $\beta_a = 0$, namely that the *Xs* do not jointly predict the gender of the child. The prediction is that we will not reject the null for very young children, but that we will always reject for children that are "old enough."

We perform this test using India's 1992 DHS (also known as the National Family Health Survey), a large representative survey that contains several variables that are determined at birth. The DHS surveyed ever-married women of reproductive ages. Each woman was separately

[5] Some evidence suggests that the sex ratio at birth may be correlated with birth order, parental age, mother's education and marital status (Almond and Edlund 2007 and Chahnazarian 1988). But these effects are very small and can only be detected using very large samples of births (Yamaguchi 1989, Almond and Edlund 2007).

[6] The estimator could also be upwards biased. Girls that survive might be healthier than boys and thus need less care than boys. But this seems unlikely since the mortality rates for girls remain higher than the mortality rates for boys for the entire postnatal period.

interviewed and asked questions on their characteristics and reproductive histories. The files contain full birth histories: there is a record for every child born, including date of birth and gender, whether the child has died, and whether s/he continues to live at home. Therefore, we know for every child born the characteristics of their mothers, and we can compute their number of siblings by gender and age (including the number of those who have died) using the birth histories. We use only the 1992 survey to minimize the bias due to sex-selective abortion: previous literature suggests that ultrasound technology became widespread in India only in the mid 1990s, particularly after 1995 (Bhalotra and Cochrane 2010). We focus on rural households as the previous literature has done.[7] The final data contain one observation per family and include children born to women ages 15 to 49 living in rural areas in 25 states, excluding twins.[8]

To perform the test, we pool children into 12-month age-groups and run a joint test for each age-group.[9] We use the results of the test to determine the age at which the test starts to systematically reject the null. Table 1 shows all of the predetermined characteristics of the child and the mother that we can include—there are a total of 14.[10] We selected variables that were the most likely to be predetermined at birth for *all* children, regardless of their age—therefore we do not include location, household composition, marital status, spouse characteristics or number of living siblings, since it is possible (in fact we later document) that some of these characteristics are affected by the gender of one's children.

Figure 2 shows the results of our test graphically. It plots the p-value of the joint test that the *X*s do not predict the gender of the youngest child. The first point (at age 0) corresponds to children 0 to 11 months old. We cannot reject the null for the youngest age group. For living children, we reject the null at the 5% level for the first time for age group 19-30 months. Thereafter, we often reject the null. To verify that our test is not rejecting because of small sample sizes, we repeated our test using the 1998 data as well (available upon request), though in this later period sex-selective abortion makes it more likely we will reject the null. In this larger

[7] Subramaniam and Deaton (1991) find evidence of gender discrimination in rural but not in urban India. When explaining these results, Deaton (1997) justifies that "it can be argued that it is in the rural areas where discrimination is most likely to be found." We also present results for urban households.
[8] We exclude twins so we can define the family's youngest child's sex.
[9] We pool children into age groups to minimize the likelihood that we reject the null due to small sample sizes.
[10] We do not include prenatal care because this is only available for young children. To the extent that gender is known in utero, these variables could also be considered endogenous and thus might be better treated as outcomes.

sample we reject the null almost always for ages 14-25 months and older.[11] Based on these results and to be conservative, we keep children ages 0-15 months of age for our analysis.

Table 1 shows the results of our analysis sample. We test whether the means for various characteristics are the same for families whose youngest is a boy and whose youngest is a girl. At the bottom we report the p-value from the joint test that all characteristics predict gender. For the estimation sample, no coefficient is significant at the 5% or 10% level, and the joint test cannot reject the null. For comparison, in the last two columns we report the result of the tests for the youngest children ages 16-59 months. For this sample, two out of the 14 variables we examine are statistically different at the 5% level. Interestingly we now observe that if the youngest is male he is more likely to have more sisters, consistent with son-biased stopping rules, and more likely to have a mother that speaks Hindi, a characteristic that predicts son preferences (Pande and Astone 2003). The null of the joint test for this group can be rejected at the 5% level.

The mean differences we observe could result from sex-selective abortion, excess girl mortality or stopping rules. If the bias from sex-selective abortion were large enough, even our youngest sample would not appear to be balanced between boys and girls.[12] Thus sex-selective abortion does not appear to be large enough in our data, but nevertheless we also estimate separate models later on for North India, where both sex-selective abortion and son preference are deemed to be the largest. To assess the effect of excess girl mortality, Figure 2 also plots the p-value of the test for the sample of ever born children (including children that mothers reported to have died by the time of the survey). Our results are basically the same, implying that the observed differences are mainly the result of stopping rules.

There are a couple of caveats to the results in this section. As in other tests of random assignment, our test is imperfect in that we can only observe that the samples are identical based on observables—it is possible they are different based on unobservables. Also, although our samples are large, they are not large enough to precisely identify the age at which the covariates become unbalanced. In summary, the data are supportive of the assumption that gender is as good as "randomly assigned" among the youngest children 15 months and younger. We use this sample to estimate whether girls are given fewer resources than boys, starting with parental time.

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[11] We only marginally reject the null for living children for two age groups younger than the 14-25 months group.

[12] We performed another test of sex-selective abortion: we looked at whether the preceding birth interval is shorter for boys than for girls, but again we found small and statistically insignificant differences.

V-Results from the Time Use Survey

We begin investigating whether families spend more time taking care of children if the youngest child is a boy. We use data from the Indian Time Use Survey (hereafter TUS) conducted from July 1998 to June 1999 by the Social Statistics Division of the Central Statistical Organization of India. The TUS asked about the time use of all household members over 5 years of age during the previous 24 hours. The diary section was open-ended in terms of both describing the activities and giving beginning and ending times, with each activity identified as multiple (simultaneous) or not.[13] The survey collected data in six states selected to be representative of the different regions of the country (Gujarat, Haryana, Madhya Pradesh, Meghalaya, Orissa and Tamil Nadu) and interviewed 12,750 rural and 5,841 urban households, roughly 75,000 respondents in total. We analyze time use data corresponding to "normal" days only (excluding, for example, holidays).[14] The main variable of interest is the amount of time spent on childcare by household members over 5. We follow Guryan et al. (2008) as closely as possible, and classify the following activities as childcare: physical care of children (washing, dressing, feeding); teaching, training and instruction of own children; accompanying children to places; travel related to care of children; and supervising children.

These data have a couple of limitations for our analyses. Aside from containing information only on 6 states, there is very little information about the participants. Also, families cannot be identified, only households. We can only identify the youngest child in the household (not the family)—for this reason, we restrict the sample to children who are the children or grandchildren of the household head.[15] The most important limitation of the TUS is that we do not know the identity of the child who was being cared for, we just know whether individuals reported being occupied with childcare.[16] This feature however does have an advantage: since

[13] The activities were detailed coded into 176 different types. For simultaneous activities, field workers determined the main activity and distributed the total time spent according to the relative importance of activities.
[14] This excludes "abnormal" days when there are guests, someone is sick or there is a festival and "weekly variants", but most days are included. All households are interviewed for at least one normal day.
[15] Children that do not live with their biological parents receive less care on average, and it is possible this differs by gender—for example families are much more likely to adopt girls than boys. We restricted the sample to avoid these complications. We also exclude households that had more than one child with the youngest age so we can define the sex of the youngest (if there is a boy and a girl both aged 3, we cannot tell who is the youngest).
[16] The survey did not ask the respondent who was present when an activity was performed.

the questions on childcare do not refer to a particular child, it is less likely that respondents are systematically biasing their responses based on the gender of the youngest child.

Age in months is not available in the TUS—we look at children under age 1 for our main results—this group is the closest to the experimental sample in the DHS. Because the TUS is small and because there is substantial age-heaping at age 1 (which appears to be differential by gender, Coale and Demeny 1967, Bhat 1990), we also report results for children under age 2. For comparison and to assess the bias among older children, we also report results for children ages 2-5. Table 2 presents summary statistics for these samples. Households with children under 2 spent on average more than 3 hours on childcare per day, while households with older children spent a little less than 2 hours.[17] Women provided more than 80% of the total time spent on child care by the household. About 70% of childcare consisted of physical care of children. Roughly half of the time devoted to childcare consisted of exclusive childcare, in which the caregiver did not report any simultaneous activity—we use this as a measure of quality of childcare.

Preliminary evidence of differential treatment by gender is presented in Figure 3, which shows the cumulative distribution of childcare separately by gender of the youngest child under age 1.[18] The figure shows that the baby boy distribution appears to first-order stochastically dominate that of baby girls, suggesting boys receive more childcare than girls. To obtain estimates of the effect of gender on childcare time we estimate

$$Z_h = \alpha_0 + \alpha_1 * B_h + X_h\rho + u_h,$$

where Z_h is the total amount of time that all members in the household spent on childcare, and B_h is a dummy for whether the household's youngest child is a boy. We present results with and without controlling for predetermined household-level covariates X_h. The standard errors are estimated using White's correction for heteroskedascity and we use the survey weights.[19]

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[17] Although these numbers seem small, they are comparable to those from other countries. For example Guryan et al. (2008) in table 4 report that the average *weekly* childcare time for an adult with children ranges from 4 hours (South Africa) to about 9 hours (US). Assuming that there are 3 adults per household on average this translates into roughly 2 (South Africa) to 4 (US) hours per day at the household level. The most likely reason why the numbers are so low is that individuals only report childcare when it is performed as a primary activity (exclusively)—previous research (Fedick et al. 2005) suggests that estimates of total childcare time are about 3 to 4 times larger when time spent with children (though not reported as childcare) is included.

[18] About 7% of households report spending no time (collectively) on childcare, even though they have an infant.

[19] We also estimated standard errors taking the survey design into account and found similar results (available upon request). The TUS had a sophisticated sampling scheme with three levels of stratification and clustering, and consequently there were many strata with one sampling unit. To account for all these features many assumptions have to be made, thus we opted for showing the OLS standards errors in the main tables.

The main results are in Table 3. Since our dependent variable is the number of minutes spent on childcare, we estimate various models. The first column estimates a simple OLS model where the dependent variable is the total number of minutes spent on childcare including 0s. It shows that households whose youngest child is a boy spend roughly 32 minutes more per day taking care of children than households whose youngest child is a girl, about 14% more relative to the mean. Column 2 shows that this estimate is robust to controlling for religion, ethnicity and the area of the land that the household owns. In column 3, we estimate a logit of whether the household spends any time on childcare. Though the estimates are positive they are not statistically significant and they are somewhat small (about 4%). If we estimate instead an OLS model only for those that report some care, we find that households whose youngest child is a boy spend roughly 24 minutes more (about 10% more) per day on childcare than households whose youngest child is a girl. Column 5 estimates a Tobit model, which accounts for censoring at 0. Again we find a statistically significant increase in childcare of about 15%. The effects are similar, though smaller, for children under 2 (Panel B). Panel C presents results for households whose youngest is between 2 and 5 years of age. Regardless of the specification, we do not find any statistically significant effect of gender, and in fact all coefficients have the "wrong" sign. Thus, we fail to find evidence of differential treatment among the older group.

Table 4 looks at whether the effects of gender differ based on observable households characteristics and on the type of care. Column 1 reproduces our main estimates from Table 3 for reference. In column 2, we interact gender of the youngest with the number of other children in the household under the age of 6, which is also added as a control. The coefficient on gender is now larger, and the interaction with number of children is negative. If the youngest is the only child under 6, and he is a boy, the household spends 44 minutes more on childcare but not if there are 4 or more other children under 6—it would appear that when there are many small children, there is simply "no room" to provide differential treatment. Although these coefficients are not significant, we observe the same pattern for the older sample (Panel B), for which all coefficients are significant. Column 3 restricts attention to families with no other children under 6—for these families all childcare is directed towards the youngest, whereas for our main results we do not know in the household is receiving the childcare. We find that infant boys receive 60 more minutes of care than infant girls (about 30% more).

In columns 4-7 we show that households spend more time in both private and public childcare if the baby is a boy than if it is a girl (columns 4 and 6). The amount of childcare time per child is increasing with the number of children for public care but not for private (columns 5 and 7). Most interestingly, gender differences in physical care do not fall with the number of other siblings whereas for supervising the effect of gender disappears if there are two more child under 6. Again we observe similar results for children under 2. These patterns can be explained by the private versus public nature of childcare activities. Since supervising is a public type of care, it makes sense that, as the number of young children in the household increases, members will spend disproportionally more time in this type of care and this time will not be closely related to the sex of the youngest child. In contrast, physical care is private so there is room for differential treatment even when other young children are present.

Column 8 shows estimates of the effect of gender on "exclusive child care time", our proxy for quality care, defined as the number of minutes that adults spent caring for children and not doing anything else. Households whose youngest child is a boy provide more exclusive childcare than households whose youngest child is a girl, roughly 60 minutes more per day if there are no other children under 6. In column 9 we repeat the estimation for urban households who are deemed to discriminate less. The effect of gender is actually negative, but the sample is small and the standard errors are large. We also investigated who in the household provides care. All members report spending substantial more time on childcare if the youngest is a male, though in general the estimates are significant only for adult women (Appendix Table 1).

Overall we find that more time is spent in childcare in households whose youngest is a boy. And the quality of this time is higher.

VI-Gender differences in other inputs: additional results from the DHS.

We now proceed to investigate whether there are boy-girl differences in other child investments using DHS data on breastfeeding, vitamin A supplementation and vaccinations. We estimate whether the gender of the youngest child predicts parental inputs. The results are reported in Table 5. All estimations use survey weights and correct the standard errors for survey design.

We first look at breastfeeding, which is deemed to be the ideal source of nutrition for infants, particularly in developing countries where food and water quality are low, and sanitation is poor.[20] We do not find that boys are more likely to have ever been breastfed (defined as ever breastfed, or breastfed less than a month), and this is true in both the linear and non-linear specifications, and regardless of whether we add controls. The effect sizes are precisely estimated zeroes. Most likely this is because more than 95% of children are ever breastfed.

In the next set of columns we look at the duration of breastfeeding. We estimate censored linear regressions, since many children are still being breastfed at the time of the interview. Alternatively we estimate a censored log-linear and a standard accelerated-failure time models. We find a positive and statistically significant effect of gender. If we estimate a proportional hazard model, we find that the odds of stopping breastfeeding are lower for males. The magnitudes suggest that breastfeeding duration increases as much as 40% when the child is a boy, consistent with Jayachandran and Kuziemko (2009).

Next we look at whether children are given Vitamin A, which protects against night blindness, measles and diarrhea.[21] Regardless of whether we estimate a linear or non-linear model, we find that boys are about 13% more likely to receive vitamin A. Finally, we look at whether mothers had a vaccination card on hand at the time of the interview. Only about 28% of mothers have a vaccination card, but they are 4% more likely to have the vaccination cards of boys. For all outcomes the results are not sensitive to the inclusion of covariates.

We also investigate whether boys are more likely to be vaccinated.[22] These results are shown in Table 6. The magnitudes vary depending on the vaccination but they range from 8 to 12% for all three panels, and they are not sensitive to the inclusion of covariates, the use of a non-linear model (Panel B), or whether we restrict the sample to those that are old enough to in principle have received all vaccinations already (Panel C). Oster (2009) and Jayachandran and Kuziemko (2009) find similar results. The results are sensitive however to whether we use the information from the vaccination card only. At the interview, mothers were first asked for the vaccination cards. If the mother had it, then all the vaccination information was recorded directly from the card. If the card was not available, mothers were asked to provide information on each

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[20] See Jayachandran and Kuziemko (2009) for a more detailed discussion on the benefits of breastfeeding in the context of developing countries.

[21] Children between 6 months of age and 5 years are supposed to take a Vitamin A supplements every 6 months. The first two doses can be given at the same time required vaccinations are given.

[22] The recommended vaccination schedule in India for children is as follows: BCG at birth, polio at birth, 6 weeks, 10 weeks and 14 weeks; DPT at 6 weeks, 10 weeks and 14 weeks and measles at 9 months. BCG protects against tuberculosis and DPT protects against diphteria, pertussis and tetanus.

type of vaccination. If we restrict the sample to children with vaccination cards (Panel D) most estimates are small, some are negative and all are statistically insignificant. We infer from these that mothers who do have cards for girls are just as likely to invest in them as in boys (with the possible exception of DPT 3rd dose and measles), but these women constitute a small (less than 30%) and selected subset of the population. The vaccination results suggest reasons why some previous research has not found large gender differences in vaccinations: some was based on vaccination cards (Borooah 2004) and others were from surveys based entirely on mothers reports, which could be more (or less) reliable than in the DHS—this might be the case in the NSS, which Deaton (2003) uses to draw his conclusions.

In Appendix Table 2, we perform a number of additional robustness checks. The first columns reproduce our main results for reference. Previous literature suggests sex-selective abortion is less important among first-born (Retherford and Roy 2003). We therefore first report estimates limiting the sample to first-born children. On the other hand previous literature also suggests that discrimination against girls increase with birth order (Das Gupta 1987) so it is not entirely clear a priori what to expect in this sample. We find that even among first born boys appear to receive more inputs, though the magnitudes are smaller and not always significant (though this sample is substantially smaller).

Next we investigate the effect of mortality on our estimates. An advantage of the 1992 DHS data (unlike later waves) is that mothers were asked to report on investments even for children that were dead at the time of the interview. Assuming that maternal reports aren't biased, we can gauge the effect of mortality by simply including these dead children in our estimation sample. The results are not different from our main results. Next we compute bounds by imputing the missing information for children who died before the investment was possible[23] or for whom the maternal report was missing.[24] Our bounds are not very tight as many include 0. However if one assumes that only upper bounds are likely (dead girls are treated worse than dead boys) then upper bound implies that our estimates are potentially substantially underestimated.

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[23] We impute the information for polio/DPT 1st dose if the child died before 2 months, before 3 months for polio/DPT 2nd dose, before 4 months for polio/DPT 3rd dose, before 6 months for vitamin A and before 9 months for measles.

[24] Upper bounds assume that all dead girls would have not received inputs (for dummy variables) or would have been given the 25th percentile of the girls' outcomes distribution. For boys we assume that had they lived they would all have been given inputs (for dummy variables) or given the 75th percentile of boys' outcome distribution. For upper bounds we assume the opposite.

The table also shows results for all children ages 16-47 months (regardless of whether they are the youngest) which shed light on the extent to which using older children generates bias. For this older sample, the coefficients are somewhat similar and still statistically significant, but they are generally smaller in magnitude relative to the sample mean. The bias due to son-biased stopping rules and family size appears to be small for vaccinations, but much larger for breastfeeding and for childcare (recall results in Table 3). These results are consistent with our theory: vaccinations are acquired while children are still very young and are still the youngest, whereas other inputs are received at older ages when households will have had a chance to respond with additional children.

Next we report results for the urban areas. Just like in the TUS, the results are smaller and generally insignificant. Overall our findings do support the hypothesis that differential gender treatment is larger in rural areas. Finally we report results separately for North and South India. In the North, we find that males obtain more of all inputs and the effects are much larger than in our main sample, in spite of the possibility of sex-selective abortion, which generates a downward bias. In the South, the coefficient on male is also generally positive, but the effects are generally smaller in magnitude, and not always significant, with the exception of breastfeeding.

In summary, for all the measures we looked at, we find that boys are given more inputs than girls; in general, girls receive at least 10% less than boys. To assess the magnitude of these differences, we estimate how much gender differences in investments can explain the higher mortality rates among girls using estimates from the literature of the effects of breastfeeding, vitamin A supplementation and vaccinations on mortality.[25] Mortality rates among children 12 to 36 months of age are 16.7 per 1,000 for boys and 24.2 per 1,000 for girls. A back of the envelope calculation suggests that the observed differences in investments explain about 27% of excess girl mortality among children in this age group (about 2 additional girl deaths per 1,000

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[25] For each investment we first calculate the (gender neutral) probability of death conditional on not receiving the investment (p_0) and the (gender neutral) probability of death conditional on receiving it (p_1) using the relative risks estimated in the literature [vitamin A (Rahmathullah et al 2003); breastfeeding (Briend, Wojtyniak, and Rowland, 1988 and WHO 2000); measles (Koenig et al 1990); Polio, BCG and DPT (Moulton et al 2005)], the mortality rate for children 12 to 36 months old (20.3 per 1,000 children) and the fraction of children in this age group receiving the investment (see Jayachandran and Kuziemko 2009 for a more detailed discussion). Let θ_b be the fraction of boys and θ_b be the fraction of girls (in the age group) who receive the investment. The difference in the mortality rates of girls versus boys associated to gender differences in the investment is equal to $(\theta_b - \theta_g)*(p_0 - p_1)$. We sum these differences over all investments and divide the total by the difference in mortality rates of girls and boys.

children). Assuming a modest effect of parental time on mortality, we can further explain 3.4% of excess female mortality.[26]

VII-Investigating hypothesis of differential investments

We start by investigating whether boys appear to need more inputs from their parents—this could be the case if boys are more active, or they get sicker more frequently. To assess this possibility we look at South Africa, the only developing country we are aware of with a time use survey,[27] a DHS survey, and for which fertility patterns suggest there is no son preference (Gangadharan and Maitra 2003). Figure 4 indeed shows that unlike India, there is no evidence of son-bias stopping rules: the share of boys among the youngest does not rise with age.

Figure 5 plots the cumulative distribution of household childcare time by gender of the youngest among those under 1. There is a small difference between the genders, favoring boys especially among those reporting no care. Compared to the Indian TUS, mean childcare time is lower and a larger fraction of households report spending no time on childcare at all. These differences are easily explained: the Indian TUS collects diaries for all the household members 6 and above, whereas the South African TUS only collected time use for only one or two eligible members (above 10 years old). In Table 7 we report the points estimates for the gender differences: we find that boys are more likely to get any care (the implied marginal effect is about 12%), but conditional on getting care, girls appear to get more care than boys. Thus this evidence does not strongly support the idea that boys need more childcare time.

This table also reports whether boys are given more of all other inputs. Most coefficients on the male dummy are statistically insignificant, many are negative and the magnitudes are rather small for most inputs. We conclude that with the possible exception of time, we find no evidence for the "needs" hypothesis, although of course this evidence is only suggestive since it is not clear that South Africa provides a good counterfactual for India.

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[26] We know of no good estimates of how parental time affects mortality. Assuming that one additional hour of childcare reduces the probability of death (in absolute terms) by 0.0003, the boy-girl difference in time use of roughly 51 minutes translates into a boy-girl difference in mortality of 0.000255.

[27] We use data from the South African Time Use Survey, conducted in 2000 by Statistics South Africa. Information on time use was collected for persons aged 10 years or more, with two respondents randomly chosen per household (or only one if there was only one household member aged 10 years or more). Data were collected for 8,564 households (14,553 respondents). We use data from 1,025 households whose youngest member is under 2 years old.

Next we investigate the possibility that the effects we estimate are driven by changes in anticipated family size: households who wanted a boy but had a girl might decide to continue having children to reach their desired number of boys. In anticipation, they might start saving, or go back to work earlier (Rose 1999), which could explain why girls receive less investments. Unfortunately there is not data on anticipated family size. To gauge the importance of this mechanism we assume that the effects of an *anticipated* increase in family size are similar to the effects of an increase in *actual* family size. We then estimate the effects of family size on inputs. The results are in Table 8. A simple OLS regression of inputs on family size shows that children in larger families receive fewer inputs. These results however are difficult to interpret since these families might also be poorer. To obtain better estimates we follow the previous literature (see cites Schultz 2008) and instrument for family size using the gender of the first born: in families with son-preference a first born girl should increase family size. This is indeed what we find in the first stage; if the first born is a boy, then family size decreases by about 4% (.12 fewer children). The 2SLS estimates of the effects of family size on inputs reveal a different picture from OLS.[28] We find that family size significantly lowers the incidence and duration of breastfeeding.[29] But all of our other estimates are statistically insignificant. Noticeably the effect of family size is positive for most vaccinations, inputs for which it is reasonable to assume there are returns to scale. Therefore we infer from these results that anticipated family size can explain the effects of gender on breastfeeding but not the effects we observe for other inputs.[30]

Our results suggest that needs and anticipated family size cannot account for most of the effects of gender that we observe. We do not have information to assess whether parents invest less in girls because of lower returns. However there is growing evidence that this is indeed part of the explanation, as suggested in the seminal paper by Rosenzweig and Schultz (1982). Jensen (2011) and Oster et al. (2011) both document that in areas where the returns to school for women increase (as a result of the availability of higher paying jobs in female oriented call-centers) girls stay in school longer. Also Jayachandran and Lleras-Muney (2010) show in a different context that when female adult mortality declines, schooling of girls increases. Overall our findings point

[28] We also used twins as an instrument and found similar results, but these were more sensitive to the addition of covariates (results available upon request).
[29] We report reduced forms (instead of 2SLS) for breastfeeding duration because of censoring.
[30] Unfortunately, we cannot assess these effects for childcare because the survey identifies only households—we have no information on the gender of the first born.

to either differences in returns or differences in preferences as the main reasons for lower investments in girls. It is also possible that household invest more in girls in dimensions that we cannot observe, such as dowries.

VIII-Other results from the DHS: anthropometric measures and living arrangements

We now look at the effect of gender on height-for-age, weight-for-age and weight-for-height Z-scores, which are computed by subtracting the median of the reference population and dividing by the standard deviation of the standard population.[31] It is important to normalize outcomes since boys are otherwise known to be taller and heavier than girls. We further examine whether gender determines the likelihood of a child being stunted, underweight or wasted.[32] Importantly, these measures are not ideal measures to investigate differential treatment. Anthropometric measures are outcomes, not inputs, over which parental control is limited. Height and weight are the result of caloric inputs, but also of other factors such as the incidence of disease and caloric expenditure, which may differ by gender for biological reasons.

Table 9 shows that boys fare *worse* than girls for all the anthropometric measures we use if we use the Z-scores provided by the DHS (this is also what Mahajan and Tarozzi 2007 find). These results are hard to reconcile with the previous evidence presented here that inputs are higher for boys. However Sommerfelt and Arnold (1998), also find that girls under 2 have better anthropometric measures that boys in almost all developing countries for which DHS data is available (41 surveys were used).

There are two possible explanations for these patterns. The first is that the results are driven by the standardization method, which uses a standard well-fed US population as the reference which might itself generate bias, as first pointed out by Thomas (1990). Indeed when we use alternative standards (the 1990 British Standards for height-for-age and weight-for-age and the 2000 CDC standards for weight-for-height[33]), the coefficients on male are substantially

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[31] About 15% of children were not measured but this was not different by gender.

[32] A child is stunted if the height-for-age is 2 s.d. below the median of reference population (measures chronic under-nutrition); a child is underweight if the weight-for-age is 2 s.d. below the median (measures both chronic and acute under-nutrition); a child is wasted if the weight-for-height is 2 s.d. below (measures acute under-nutrition).

[33] There are no British standards available for weight-for-height. The 2000 CDC standards for height are not available for children under 2.

smaller, statistically insignificant and sometimes switch signs.[34] It is possible that the standards affect the results because baseline differences in height and weight between boys and girls are larger in well-fed populations (Stinson 1985). Another possibility is that the effect of lower inputs for girls appears with a lag: girls are sturdier than boys at birth and this initial advantage is larger than the immediate effects of low parental inputs. But over time the effects of lower inputs eventually benefit boys. Indeed if we look at older children (older than 2) we do find that boys have better anthropometrics than girls. We cannot resolve this puzzle here but we note that anthropometric measures yield substantially different results than inputs.

Finally we investigate whether living arrangements are affected by the gender of the youngest boy. Studies in the US report that having a son reduces the probability of parents getting divorced (Katzev, Warner and Acock 1994; Morgan, Lye and Condran 1988; Mott 1994; Spaneir and Glick 1981), and that daughters are less likely to live with their fathers (Dahl and Moretti 2004). There is, however, little research in the context of developing countries, where boy-girl discrimination is thought to be a greater concern. Table 10 reports the results. Panel A uses the DHS and examines whether gender of the youngest affects the likelihood that different family members live together. We do not look at marital status as an outcome because in our samples all mothers are married. But we look at whether women report that their husband's live at home. We do find that if the youngest is a boy then the husband is more likely to live at home, but the effect is insignificant and very small, less than 1%. There is no evidence that the youngest's gender affects the likelihood that the mother is the household head's wife—which might occur if for example families move in with their parents after a boy is born. There is also no discernable effect of the youngest gender on the number of siblings living at home. Sisters are slightly more likely to live at home (though this is not significant) whereas brothers are less likely to live at home if the youngest is a boy (about 5% less and this is statistically significant).

In Panel B we look at household composition in the TUS. Although the unit of observation is now the household, we find somewhat similar results. If the youngest is a boy, then there are more men over age 15 in the household (about 15% more) and this is statistically significant. There are more women over 15 (about 11%), and this effect is also statistically significant. There is again no apparent effect on the number of total children under age 14, but

[34] Moetsue (2009) also finds that for Blangladesh boy-girl differences depend on the standard used. Tarozzi (2008) also documents that the choice of standard significantly affects results on anthropometrics differently by gender.

these are more likely to be girls and significantly less likely to be boys (about 20%). Together these results suggest that when the youngest is a boy, the family is more likely to retain their daughters for caregiving, or to have another female adult move in to provide help. In addition the household is more likely to have adult males and less likely to have male children.

VII-Conclusion

We study whether parents treat girls and boys differentially in India. Although in India women lag behind men in many domains, there is equivocal evidence on whether these lower outcomes are the result of lower parental investments in girls. We developed a novel empirical strategy to address the possible biases in previous estimates of differential treatment that arise from son-bias stopping rules. We used our identification strategy to look at differential treatment along measures previously used in the literature. In addition we examined whether families spend more time with childcare when the baby is a boy than when the baby is a girl. Time investments have not been previously studied in the context of developing countries.

We find evidence that boys receive more child investments than girls. Households with an infant boy under one spend roughly 30 minutes more per day (about 15% more time) on childcare than households with an infant girl. This difference is even larger for one-child households: households with one boy under six spend roughly 60 minutes more (30%) per day on childcare than households with one girl under six. We also find suggestive evidence that the quality of childcare given to boys is higher. Moreover, we find that boys are more likely to be vaccinated, to be breastfed longer and to be given vitamin supplementation. In general we find these inputs to be at least 10% higher for boys.

We also investigate why parents may invest less in girls. We find no evidence of greater needs among boys for all measures except for childcare time for which the evidence is mixed. We also look at whether girls receive less because upon their birth families anticipate they will have to continue having more children. We find evidence to suggest this is true for breastfeeding but not for other outcomes. Thus in general we find that these explanations cannot account for the patterns we observe across all outcomes. Thus we conclude that parents invest less in girls because these investments have lower returns (for which there is some evidence in the literature) or because the have a preference for sons.

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FIGURE 1

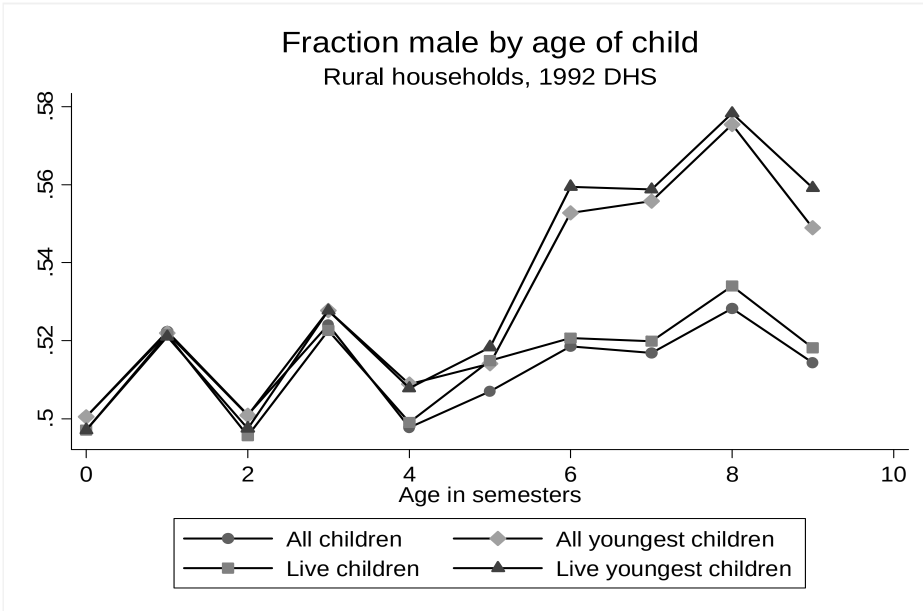

FIGURE 2

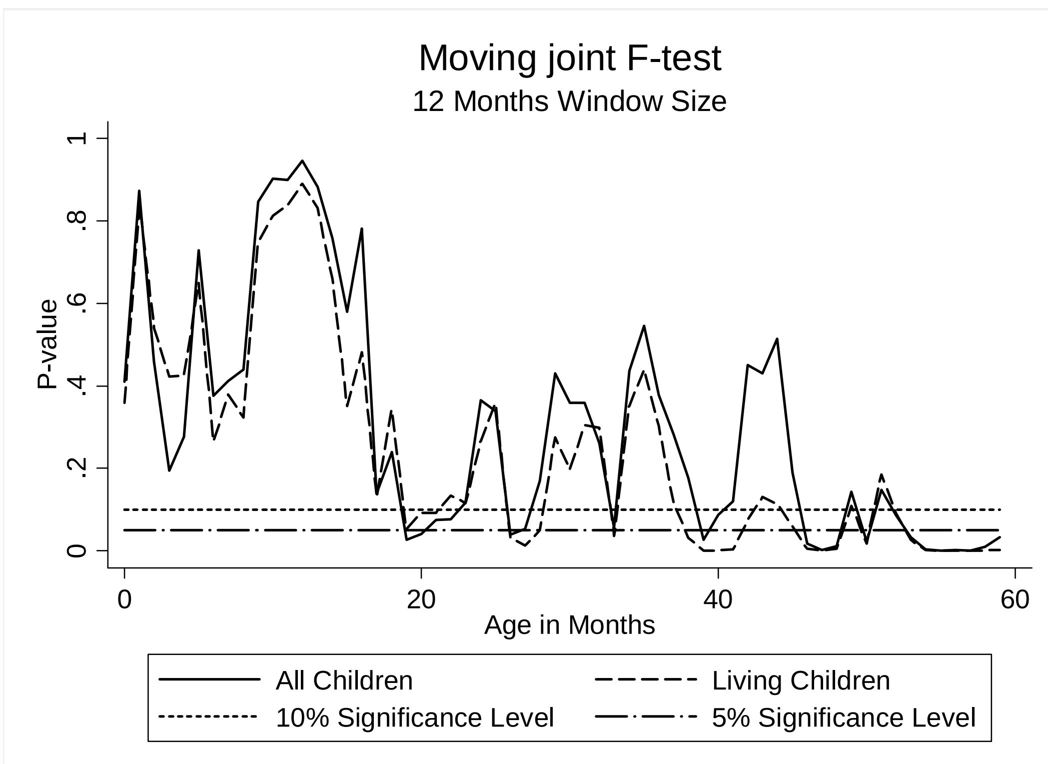

FIGURE 3: Childcare Time by gender, Indian Time Use Survey 1998 -1999

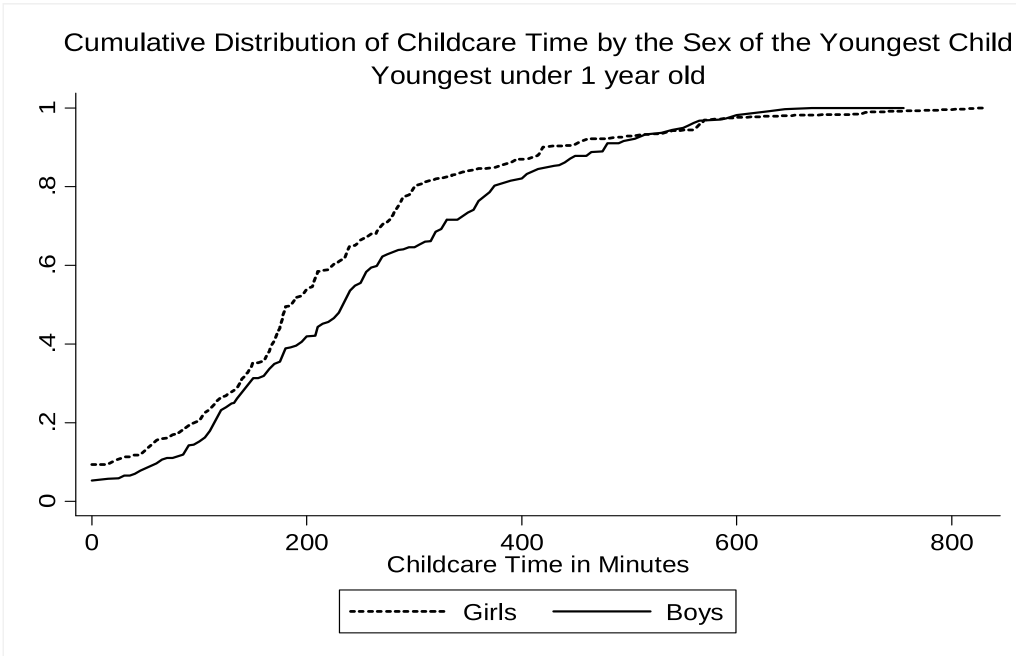

FIGURE 4

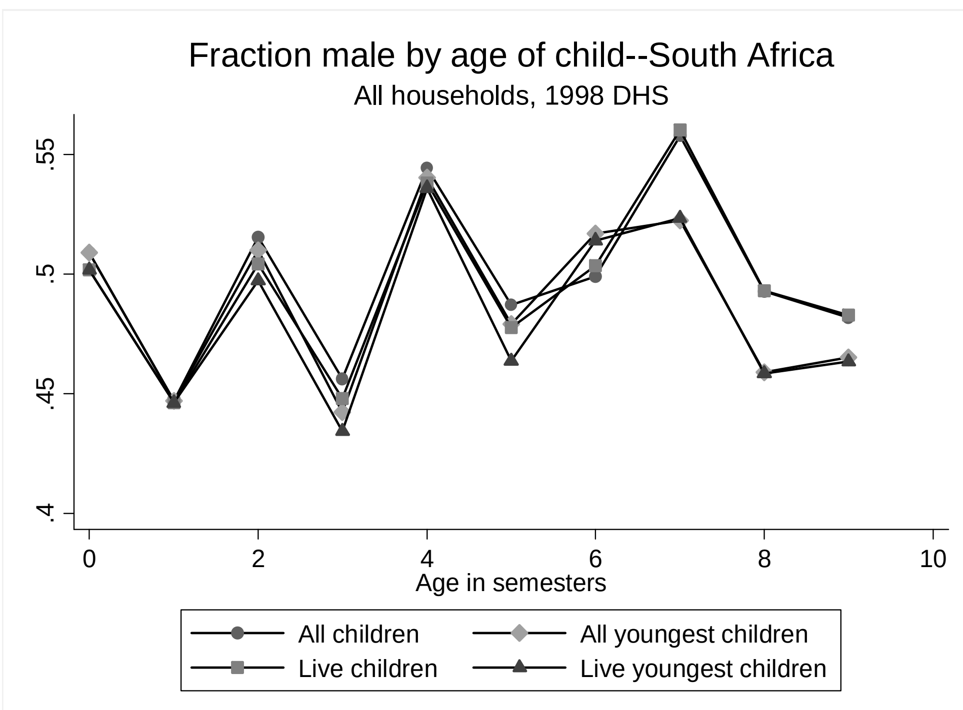

FIGURE 5 Childcare Time by gender, South Africa Time Use Survey 2000

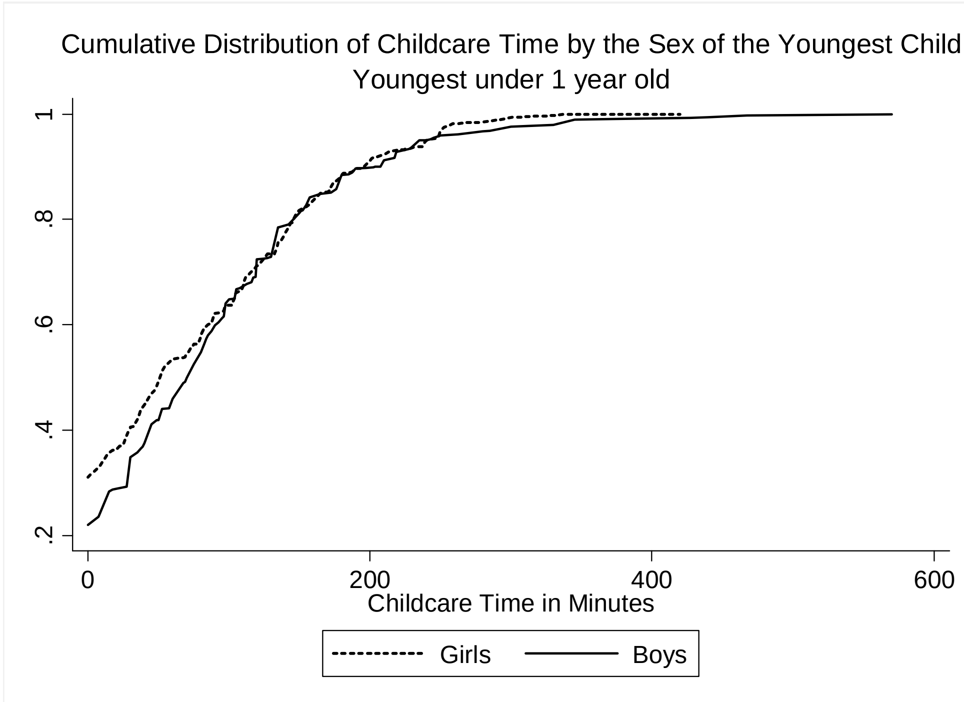

TABLE 1: TESTING RANDOM ASSIGMENT (DHS 1992). MEAN DIFFERENCES BY GENDER.

Sample:	**Youngest live child**							
	Age 0-15 months (N=11,627)				**Age 16-59 months (N=16,275)**			
	const. (mean for females)	s.e.	coefficient on male	s.e	const. (mean for females)	s.e.	coefficien t on male	s.e
Child characteristics								
# of siblings ever born	2.091	[0.031]***	0.01	[0.041]	2.257	[0.030]***	0.055	[0.037]
# of brothers ever born	1.013	[0.019]***	-0.037	[0.025]	1.128	[0.017]***	-0.008	[0.022]
# of sisters ever born	1.078	[0.020]***	0.046	[0.029]	1.13	[0.019]***	0.062	[0.025]**
Birth month	6.871	[0.056]***	-0.111	[0.081]	6.727	[0.045]***	-0.005	[0.063]
Mother's characteristics								
Mother's age	24.736	[0.082]***	0.029	[0.118]	27.156	[0.084]***	0.146	[0.112]
Mother's ethnicity (scheduled caste omitted)								
Scheduled tribe	0.114	[0.008]***	-0.004	[0.007]	0.11	[0.006]***	0.001	[0.006]
Other	0.744	[0.009]***	0.004	[0.010]	0.756	[0.008]***	-0.003	[0.008]
Mother's religion (other omitted)								
Hindu	0.819	[0.009]***	-0.003	[0.008]	0.83	[0.008]***	0.005	[0.006]
Muslim	0.131	[0.008]***	0.004	[0.007]	0.126	[0.007]***	-0.009	[0.006]
Christian	0.021	[0.002]***	0	[0.003]	0.019	[0.002]***	-0.002	[0.002]
Mother's years of education	1.956	[0.063]***	-0.046	[0.073]	1.959	[0.056]***	-0.026	[0.058]
Mother born in urban area	0.063	[0.004]***	-0.005	[0.005]	0.064	[0.004]***	-0.001	[0.004]
Mother's age first married	16.364	[0.047]***	0.045	[0.057]	16.207	[0.043]***	-0.022	[0.052]
Mother's age at first birth	18.558	[0.049]***	0.055	[0.064]	18.473	[0.046]***	-0.076	[0.057]
Mother speaks Hindi	0.483	[0.010]***	-0.015	[0.012]	0.414	[0.008]***	0.02	[0.009]**
Pvalue (Joint Test)			**0.2872**				**0.016**	

Standard errors (in brackets) are computed taking survey design into account. Coefficients reported from separate linear regressions, where each characteristic is regressed on a dummy for male and a constant. The p-value for the joint test comes from regressing the youngest child's gender on all the charateristics (except number of all siblings, since that is collinear to the number of borthers and number of sisters) and testing whether they are jointly significant. ** p<0.05, * p<0.1

TABLE 2. DESCRIPTIVE STATISTICS, TIME USE SURVEY (1998-1999). RURAL AREAS.

	HHs with youngest below age 1		HHs with youngest ages 0-1		HHs with youngest ages 2-5	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Percentage of all households	0.04		0.15		0.30	
Time Use:						
Time spent on child care (minutes per day)	236.62	159.28	196.90	152.19	107.19	129.05
Time spent on child care by female members	192.15	134.80	166.23	132.76	88.87	108.79
Time spent on child care by male members	44.47	82.20	30.67	64.70	18.31	51.21
Time spent on physical care	165.22	125.96	137.89	121.83	73.06	94.29
Time spent supervising children	55.50	114.87	48.96	105.50	24.26	76.10
Time spent instructing children	5.25	27.37	4.10	23.10	5.24	26.62
Time spent taking children to places	10.65	67.78	5.94	42.81	4.62	39.92
Time spent on exclusive child care	132.44	153.63	95.55	137.45	57.43	105.52
Household characteristics:						
Household size	4.68	1.87	4.54	1.83	3.95	1.54
Male youngest	0.46	0.50	0.51	0.50	0.55	0.50
Scheduled tribe	0.24	0.43	0.23	0.42	0.21	0.41
Scheduled caste	0.14	0.35	0.18	0.38	0.20	0.40
Hindu	0.91	0.28	0.91	0.29	0.92	0.28
Per capita expenditure	393.31	175.58	393.83	188.92	408.61	196.36
Land owned and possessed	4.85	7.97	4.51	8.25	3.89	9.13
Observations	562		1,947		3,815	

Notes: Weighted statistics for households in each sample. The statistics in the first two columns are for households where the youngest child is under 1, columns (3) and (4) for households where the youngest child is under 1, and the last two columns for households where the youngest is between 2 and

TABLE 3. EFFECT OF CHILD GENDER ON HOUSEHOLD CHILD CARE TIME, TIME USE SURVEY (1998-1999)

Model:	OLS	OLS	Logit	OLS	Tobit
Dependent variable:	Number of minutes per day, including 0s	Number of minutes per day, including 0s	Any care? (Beta reported)	Number of minutes per day>0	Number of minutes per day
	(1)	(2)	(3)	(4)	(5)
Panel A: Youngest kids under 1 year old					
Male=1	32.772	30.018	0.613	24.226	36.309
	[17.669]*	[17.511]*	[0.397]	[17.344]	[18.855]*
Controls?	no	yes	no	no	no
Obs	562	562	562	516	562
Mean Y	236.62	236.62	0.93	255.51	236.62
Panel B: Youngest kids under 2 years old					
Male=1	18.689	16.602	-0.052	21.951	18.647
	[8.643]**	[8.593]*	[0.182]	[8.629]**	[9.509]**
Controls?	no	yes	no	no	no
Obs	1947	1947	1947	1747	1947
Mean Y	196.90	196.90	0.90	219.17	196.90
Panel C: Youngest kids 2-5 years old					
Male=1	-1.745	-2.056	-0.013	-1.888	-2.205
	[5.086]	[5.094]	[0.089]	[5.991]	[6.905]
Controls?	no	yes	no	no	no
Obs	3815	3815	3815	2765	3815
Mean Y	107.19	107.19	0.72	149.79	107.19

Robust standard errors in brackets. The dependent variable in all columns except (3) is the number of minutes per day spent with child care by all household members. The dependent variable in column (3) is an indicator variable for positive childcare time. Panel A reports results for households whose youngest child is under 1 year old, panel B for those whose youngest is under 2 and panel C for those whose youngest is between 2 and 5 years old. The controls include dummies for household caste (2 dummies), a dummy for whether the household was Hindu and the area of the land owned and possessed by the household. Survey weights are used in estimation. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 4. HETEROGENEITY IN CHILDCARE TIME, TIME USE SURVEY (1998-1999).

	Childcare		Households w/ ONLY 1 child under 6	Physical Care		Supervising		Exclusive Care	Urban Households, childcare
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Youngest kids under 1 year old									OLS
Male = 1	32.772	44.224	59.855	30.139	5.4	20.217	55.231	59.261	-5.709
	[17.669]*	[27.700]	[30.143]**	[13.444]**	[23.359]	[12.220]*	[16.674]***	[24.936]**	[34.546]
Male* (# other children under 6)		-9.082			20.59		-28.721	-27.47	
		[17.920]			[14.671]		[11.020]***	[15.135]*	
# Other children under 6		8.627			-10.209		18.098	-3.689	
		[11.917]			[8.372]		[7.163]**	[9.636]	
Constant		210.49	204.183	151.245	164.184	46.131	23.193	123.841	265.737
		[20.368]***	[21.463]***	[8.622]***	[15.016]***	[8.397]***	[7.541]***	[16.318]***	[27.939]***
Observations	562	562	151	562	562	562	562	562	204
Panel B: Youngest kids under 2 years old									
Male = 1	18.689	49.654	50.777	14.673	20.442	10.643	35.414	37.278	-15.867
	[8.643]**	[14.179]***	[15.969]***	[6.597]**	[11.150]*	[6.077]*	[9.874]***	[12.577]***	[16.173]
Male* (# other children under 6)		-25.206			-4.725		-20.231	-23.943	
		[9.042]***			[7.181]		[6.003]***	[7.496]***	
# Other children under 6		12.439			1.606		8.315	-3.696	
		[5.904]**			[4.471]		[4.061]**	[5.001]	
Constant	187.332	171.739	168.946	130.381	128.368	43.516	33.092	95.753	235.373
	[6.143]***	[9.715]***	[10.742]***	[4.346]***	[7.393]***	[3.723]***	[5.369]***	[8.097]***	[11.733]***
Observations	1947	1947	481	1947	1947	1947	1947	1947	677

Robust standard errors in brackets. Panel A reports results for households whose youngest child is under 1 year old, panel B for those whose youngest is under 2. The dependent variable in columns (1) and (2) is the number of minutes per day spent with child care. In column (3), the dependent variable is childcare time and the sample is further restricted to households with only one child under 6. The dependent variable in columns (4) and (5) is the amount of time spent taking physical care of children (e.g., washing, dressing and feeding). The dependent variable in columns (6) and (7) is the amount of time spent supervising children. The dependent variable in column (8) is the amount of time spent exclusively on childcare -- i.e., the caretaker was not multi-tasking. The dependint variable in column (9) is the amount of time spent on childcare, as in columns (1) and (2). The variable "# Other children under 6" excludes the youngest child. Its mean is equal to 1.23 children. All columns are estimated using OLS. Survey weights are used for estimation. *** p<0.01, ** p<0.05, * p<0.1

TABLE 5: EFFECT OF CHILD GENDER ON PARENTAL INPUTS, CHILDREN 0-15 MONTHS OLD. DHS (1992)

Dependent variable:		Was child ever breastfed?		# months breastfed	log(# months breastfed)	# months breastfed		Vitamin A supplement?		Did mother have vaccination card at interview?	
Model:	controls?	OLS	Logit (beta reported)	censored regression	censored regression	Accelerated Failure Time model	Proportional Hazard Model	OLS	Logit (beta reported)	OLS	Logit (beta reported)
				(# censored obs: 10,689)							
Male = 1	no	0.006	0.134	1.802	0.289	0.412	-0.41	0.014	0.133	0.043	0.215
		[0.004]	[0.100]	[0.574]***	[0.092]***	[0.129]***	[0.129]***	[0.007]**	[0.063]**	[0.009]***	[0.046]***
Male = 1	yes	0.006	0.141	1.909	0.309	0.43	-0.43	0.013	0.128	0.041	0.225
		[0.004]	[0.100]	[0.556]***	[0.090]***	[0.128]***	[0.129]***	[0.006]*	[0.067]*	[0.009]***	[0.049]***
Obs		11609		11073	11073			11248		11616	
Mean of Y		0.953		7.677	1.803			0.117		0.275	

Standard errors [in brackets] are computed taking survey design into account. Child ever breastfed is equal to zero if mother reports that child was not breastfed or if breastfeeding duration was less than a month. Each coefficient corresponds to a separate estimation, and survey weights are used. The number of observations for each age group varies from outcome to outcome because there are a few missing values. Controls include all variables in Table 3: # of brothers, # of sisters, birth month, mother's age, mother's caste (2 dummies), mother's religion (3 dummies), mother's years of education, whether mother was born in rural area, mother's age at first marriage, mother's age at first birth, and whether mother speaks Hindi. *** p<0.01, ** p<0.05, * p<0.1

TABLE 6: EFFECT OF CHILD GENDER ON VACCINATIONS, DHS 1992

	controls?	BCG	DPT 1st dose	DPT 2nd dose	DPT 3rd dose	Polio 1st dose	Polio 2nd dose	Polio 3rd dose	Measles
Panel A: Youngest kids 0-15 months old. OLS									
Male = 1	no	0.036	0.048	0.035	0.031	0.049	0.037	0.032	0.02
		[0.011]***	[0.011]***	[0.010]***	[0.009]***	[0.011]***	[0.010]***	[0.009]***	[0.008]***
Male = 1	yes	0.034	0.047	0.033	0.03	0.047	0.036	0.031	0.02
		[0.010]***	[0.010]***	[0.010]***	[0.009]***	[0.010]***	[0.010]***	[0.009]***	[0.007]***
Panel B: Youngest kids 0-15 months old. LOGIT (beta reported)									
Male = 1	no	0.144	0.195	0.152	0.162	0.198	0.159	0.164	0.161
		[0.043]***	[0.042]***	[0.044]***	[0.048]***	[0.042]***	[0.043]***	[0.047]***	[0.060]***
Male = 1	yes	0.161	0.218	0.163	0.17	0.219	0.173	0.174	0.161
		[0.048]***	[0.048]***	[0.049]***	[0.051]***	[0.048]***	[0.047]***	[0.050]***	[0.063]**
Obs		11591	11591	11587	11587	11609	11605	11605	11520
Mean of Y		0.448	0.47	0.354	0.262	0.472	0.368	0.271	0.147
Panel C: Youngest kids 9-15 months old. OLS									
Male = 1	no	0.036	0.058	0.055	0.052	0.058	0.051	0.054	0.038
		[0.017]**	[0.017]***	[0.017]***	[0.016]***	[0.017]***	[0.017]***	[0.016]***	[0.015]**
Obs		4815	4808	4806	4806	4822	4818	4818	4759
Mean of Y		0.573	0.613	0.525	0.434	0.617	0.545	0.453	0.313
Panel D: Youngest kids 0-15 months old with vaccination card OLS									
Male = 1	no	-0.017	0.005	-0.004	0.023	0.006	-0.005	0.027	0.016
		[0.014]	[0.009]	[0.019]	[0.021]	[0.010]	[0.019]	[0.021]	[0.019]
Obs		3338	3338	3338	3338	3338	3338	3338	3338
Mean of Y		0.869	0.947	0.747	0.574	0.94	0.75	0.574	0.271

Standard errors [in brackets] are computed taking survey design into account. Each coefficient corresponds to a separate linear regression of the dependent variable on a dummy variable equal to one if the child is a boy. Controls include all variables in Table 3: # of brothers, # of sisters, birth month, mother's age, mother's caste (2 dummies), mother's religion (3 dummies), mother's years of education, whether mother was born in rural area, mother's age at first marriage, mother's age at first birth, and whether mother speaks Hindi. Survey weights are used for estimation. *** p<0.01, ** p<0.05, * p<0.1

TABLE 7. DO BOYS NEED MORE? MEAN GENDER DIFFERENCES IN PARENTAL INPUTS

Dependent variable:	Model:	No Controls		Demographic controls				
		Coefficient on I(male=1)	s.e.	Coefficient on I(male=1)	s.e.	N	Mean Y	% effect
2000 SOUTH AFRICA TIME USE SURVEY CHILDREN UNDER 1								
Childcare, mins. per day >= 0	OLS	1.184	[12.055]	2.333	[11.838]	521	99.6	2%
Childcare, mins. per day >= 0	Tobit	9.874	[16.353]	10.342	[15.595]	521	99.6	10%
Childcare, mins. per day > 0	OLS	-15.233	[13.303]	-11.541	[13.391]	386	136.0	-8%
Any care?	Logit (Beta reported)	0.471	[0.265]*	0.464	[0.265]*	521	0.733	12%
1998 SOUTH AFRICAN DHS, CHILDREN AGES 0-15 MONTHS								
Ever breastfed?	OLS	0.024	[0.033]	0.019	[0.031]	723	0.875	2%
# months breastfed	cens. reg.	-0.409	[1.218]	0.095	[0.991]	632	6.941	1%
log(# months breastfed)	cens. reg.	-0.085	[0.234]	-0.003	[0.193]	632	1.663	0%
Vaccination card?	OLS	-0.05	[0.035]	-0.046	[0.034]	732	0.835	-6%
BCG	OLS	0.001	[0.011]	-0.003	[0.011]	729	0.978	0%
DPT 1st dose	OLS	0.017	[0.029]	0.008	[0.029]	723	0.864	1%
DPT 2nd dose	OLS	-0.038	[0.037]	-0.039	[0.036]	715	0.722	-5%
DPT 3rd dose	OLS	-0.071	[0.040]*	-0.077	[0.040]*	715	0.577	-13%
Polio 1st dose	OLS	0.002	[0.032]	-0.007	[0.032]	720	0.849	-1%
Polio 2nd dose	OLS	-0.043	[0.039]	-0.046	[0.038]	716	0.705	-7%
Polio 3rd dose	OLS	-0.05	[0.040]	-0.054	[0.040]	716	0.554	-10%
Measles	OLS	0.098	[0.040]**	0.096	[0.041]**	718	0.348	28%

Standard errors [in brackets] are computed taking survey design into account. Demographic controls for the DHS sample include number of live sisters and brothers by age, mother's characteristics (education, age, ethnicity, age at first marriage and age at first birth), and the child's birth month. Controls for the TUS sample include dummies for interview language (11 dummies) and province of residence (9 dummies). *** p<0.01, ** p<0.05, * p<0.1

TABLE 8: EFFECT OF FAMILY SIZE ON PARENTAL INPUTS, YOUNGEST CHILDREN 0-15 MONTHS OLD WITH OLDER SIBLINGS. DHS 1992

	Family Size	Ever breastfed?	Vitamin A supplement	Vaccination card?	BCG	DPT 1st dose	DPT 2nd dose	DPT 3rd dose	Polio 1st dose	Polio 2nd dose	Polio 3rd dose	Measles	# months breastfed	log(# months
Panel A OLS														
Family Size	-	-0.001	-0.012	-0.035	-0.049	-0.047	-0.043	-0.035	-0.045	-0.042	-0.033	-0.022	0.193	0.028
	-	[0.002]	[0.002]***	[0.003]***	[0.004]***	[0.004]***	[0.003]***	[0.003]***	[0.004]***	[0.003]***	[0.003]***	[0.002]***	[0.240]	[0.038]
Panel B: IV	**1st Stage**	**IV (First born male is the instrument)**											**Reduced Form**	
Family Size	-0.132	-0.089	-0.022	0.016	0.023	0.037	-0.031	0.034	0.119	0.048	0.055	0.058	1.717	0.240
	[0.039]***	[0.048]*	[0.057]	[0.086]	[0.092]	[0.097]	[0.087]	[0.077]	[0.106]	[0.093]	[0.082]	[0.067]	[0.776]**	[0.125]*
F-test	11.44													
Obs	8574	8558	8284	8564	8543	8549	8548	8548	8561	8558	8558	8496	8163	8163
Mean of Y	3.283	0.954	0.106	0.251	0.416	0.440	0.325	0.235	0.442	0.339	0.245	0.130	7.641	1.797

Robust standard errors [in brackets]. Each coefficient corresponds to a separate linear regression of the dependent variable listed in the column on the independent variable listed in the rows. The sample is restricted to children who had (alive or dead) older siblings. the regressions include no controls. Survey weights are used for estimation. *** p<0.01, ** p<0.05, * p<0.1

TABLE 9: EFFECT OF CHILD GENDER ON ANTHROPROMETRIC MEASURES, AGES 0-15 MONTHS, DHS 1992

Dependent variable:		Height-for-age Z score		Stunted = 1 (height-for-age Z score< 2 s.d. below reference median)		Weight-for-age Z score		Underweight=1 (weight-for-age Z score< 2 s.d. below reference median)		Weight-for-height Z score		Wasted=1 (Weight-for-height Z score< 2 s.d. below reference median)	
	controls?	DHS	UK	DHS	UK	DHS	UK	DHS	UK	DHS	CDC	DHS	CDC
Male = 1	no	-0.218	-0.074	0.057	0.017	-0.162	0.013	0.04	-0.005	-0.063	0.028	0.036	0.001
		[0.043]***	[0.049]	[0.015]***	[0.015]	[0.033]***	[0.039]	[0.012]***	[0.013]	[0.038]*	[0.050]	[0.011]***	[0.014]
Male = 1	yes	-0.225	-0.085	0.059	0.02	-0.165	0.008	0.041	-0.004	-0.067	0.02	0.036	0.002
		[0.043]***	[0.048]*	[0.015]***	[0.015]	[0.032]***	[0.038]	[0.012]***	[0.013]	[0.037]*	[0.050]	[0.011]***	[0.014]
Obs		6396	6396	6396	6396	8550	8550	8550	8550	6411	6411	6411	6411
Mean of Y		-1.3	-1.353	0.323	0.361	-1.51	-2.026	0.381	0.525	-0.727	-1.253	0.137	0.309

Standard errors [in brackets] are computed taking survey design into account. Each coefficient corresponds to a separate estimation. Controls include all variables in Table 3: # of brothers, # of sisters, birth month, mother's age, mother's caste (2 dummies), mother's religion (3 dummies), mother's years of education, whether mother was born in rural area, mother's age at first marriage, mother's age at first birth, and whether mother speaks Hindi. The other measures are standardized using the UK (1990) standards or the 2000 CDC standards. The UK standards are not available for height for age. CDC standards for height are not available for children under 2. Survey weights are used in estimation. For children under 2 the standards use length rather than height, which is measured while lying instead of standing. *** p<0.01, ** p<0.05, * p<0.1

TABLE 10: EFFECT OF CHILD'S GENDER ON LIVING ARRANGEMENTS, YOUNGEST CHILDREN 15 MONTHS AND YOUNGER. RURAL HOUSEHOLDS.

Panel A: Effect of gender on family living arrangements in the DHS 1992. Youngest children 0-15 months old										
Dependent variable:	Husband lives home?		Is mother the wife of the household head?		# of other sibs living at home		# of sisters living at home		# of brothers living at home	
	OLS	Logit (beta reported)	OLS	Logit (beta reported)	OLS	Negative binomial (IRR reported)	OLS	Negative binomial (IRR reported)	OLS	Negative binomial (IRR reported)
Male=1	0.008	0.084	-0.009	-0.038	-0.004	0.998	0.034	1.04	-0.037	0.953
	[0.007]	[0.075]	[0.011]	[0.043]	[0.034]	[0.021]	[0.024]	[0.029]	[0.021]*	[0.026]*
Obs										
Mean of Y	11517	11517	11624	11624	11627	11627	11627	11627	11627	11627
	0.89	0.89	0.461	0.461	1.644	1.644	0.86	0.86	0.784	0.784
Panel B: Effect of gender on household composition TUS (1998-1999). Youngest children under 1 year old										
Dependent variable:	# Men 15 and older		# Women 15 and older		# Children 14 and younger		# Girls 14 and younger		# Boys 14 and younger	
	OLS	Poisson (IRR reported)	OLS	Poisson (IRR reported)	OLS	Poisson (IRR reported)	OLS	Poisson (IRR reported)	OLS	Poisson (IRR reported)
Male=1	0.209	1.153	0.158	1.111	-0.149	0.917	0.046	1.054	-0.196	0.793
	[0.082]**	[0.063]***	[0.072]**	[0.053]**	[0.137]	[0.073]	[0.101]	[0.121]	[0.102]*	[0.098]*
Obs	562	562	562	562	562	562	562	562	562	562
Mean of Y	1.46	1.46	1.49	1.49	1.73	1.73	0.88	0.88	0.86	0.86

The standard errors [in brackets] are computed taking survey design into account in the DHS and in the TUS they allow for heteroskedasticity. Each coefficient corresponds to a separate estimation, where the dummy for the youngest child's gender is the only covariate. In the TUS (Panel B) we estimated Poisson rather than negative binomial models because some of the negative binomial models in the TUS did not converge. Survey weights are used in estimation. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX TABLE 1: EFFECT OF CHILD GENDER ON HOUSEHOLD CHILDCARE TIME BY DEMOGRAPHIC GROUP, TIME USE SURVEY (1998-1999)

Dependent variable:	Childcare by females 15 and older				Childcare by males 15 and older				Childcare by females 14 and younger			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Any care?	Any care?	minutes of care≥0	minutes of care≥0	Any care?	Any care?	minutes of care≥0	minutes of care≥0	Any care?	Any care?	minutes of care≥0	minutes of care≥0
Model:	Probit	Probit	OLS	OLS	Probit	Probit	OLS	OLS	Probit	Probit	OLS	OLS
Panel A: Youngest kids under 1 year old												
Male = 1	0.036	0.073	39.949	42.586	0.04	0.031	6.798	5.581	0.032	0.106	-3.184	67.028
	[0.029]	[0.051]	[12.833]***	[22.277]*	[0.052]	[0.082]	[6.405]	[8.416]	[0.126]	[0.196]	[26.099]	[40.359]*
Male * (# other children under 6)		-0.033		-2.312		0.011		1.42		-0.058		-55.313
		[0.032]		[14.883]		[0.052]		[4.941]		[0.107]		[28.478]*
# other children under 6		0.019		0.047		0.027		3.077		0.046		37.022
		[0.022]		[9.071]		[0.035]		[3.259]		[0.070]		[19.390]*
Constant			160.771	160.711			33.938	30.042			68.769	20.052
			[8.843]***	[15.049]***			[3.980]***	[5.441]***			[20.923]***	[19.749]
Observations	560	560	560	560	555	555	555	555	117	117	117	117
Panel B: Youngest kids under 2 years old												
Male = 1	-0.002	0.062	14.507	33.564	0.02	0.075	4.208	10.104	0.086	0.2	15.334	67.198
	[0.017]	[0.032]**	[6.696]**	[11.627]***	[0.026]	[0.042]*	[2.887]	[4.486]**	[0.066]	[0.113]*	[14.516]	[27.503]**
Male * (# other children under 6)		-0.051		-15.668		-0.045		-4.752		-0.082		-36.441
		[0.020]**		[7.515]**		[0.028]		[2.876]*		[0.062]		[14.866]**
# other children under 6		0.022		3.253		0.022		3.327		0.027		18.503
		[0.014]		[4.999]		[0.020]		[2.035]		[0.044]		[9.141]**
Constant			148.327	144.247			24.458	20.281			48.055	22.041
			[4.600]***	[7.984]***			[1.871]***	[2.840]***			[8.148]***	[10.417]**
Observations	1936	1936	1936	1936	1907	1907	1907	1907	408	408	408	408

Robust standard errors in brackets. The dependent variable in columns under (1), (3) and (5) is an indicator for whether household members of a given demographic group reported spending time taking care of children. The dependent variable in columns under (2), (4) and (6) is the number of minutes per day spent with child care by all household members of a given demographic group. Panel A reports results for households whose youngest child is under 1 year old, panel B for those whose youngest is under 2. Columns under (1) to (6) show results for rural households. Columns under (7) shows results for urban households. The dependent variable in columns under (7) is the amount of time spent with childcare by all household members. The variable "# Other children under 6" excludes the youngest child. Its mean is equal to 1.23 children. Survey weights are used for estimation. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX TABLE 2: ADDITIONAL RESULTS ON EFFECT OF GENDER ON PARENTAL INVESTMENTS. CHILDREN AGES 0-15 MONTHS

Dependent variable:	Model:	Youngest live children ages 0-15				First born only (among youngest live children ages 0-15)				Live and dead children (would be ages 0-15 months at the time of the survey)				Bounds to account for mortality (information imputed for kids who died too young to receive the investment)	
		Coefficient on I(male=1), [s.e]		Mean Y	% effect	Coefficient on I(male=1), [s.e]		Mean Y	effect	Coefficient on I(male=1), [s.e]		Mean Y	% effect	Upper Bound	Lower Bound
Ever breastfed?	OLS	0.006	[0.004]	0.954	1%	0.008	[0.010]	0.95	1%	-0.004	[0.005]	0.915	0%	-0.002	-0.007
# months breastfed	cens. reg.	1.33	[0.448]***	7.626	17%	0.514	[0.911]	7.78	7%	1.104	[0.405]***	7.555	15%	1.283	-0.954
log(# months breastfed)	cens. reg.	0.199	[0.069]***	1.797	11%	0.077	[0.139]	1.82	4%	0.176	[0.068]***	1.784	10%	0.176	-0.13
Vitamin A?	OLS	0.016	[0.006]**	0.133	12%	0.018	[0.014]	0.15	12%	0.016	[0.006]**	0.126	13%	0.077	-0.039
Vaccination card?	OLS	0.043	[0.008]***	0.287	15%	0.066	[0.021]***	0.34	19%	0.037	[0.008]***	0.271	14%	0.039	0.036
BCG	OLS	0.037	[0.009]***	0.457	8%	0.025	[0.022]	0.54	5%	0.03	[0.009]***	0.432	7%	0.03	0.03
DPT 1st dose	OLS	0.048	[0.009]***	0.476	10%	0.043	[0.022]**	0.56	8%	0.047	[0.009]***	0.45	10%	0.091	0
DPT 2nd dose	OLS	0.034	[0.009]***	0.364	9%	0.008	[0.021]	0.44	2%	0.034	[0.009]***	0.343	10%	0.083	-0.015
DPT 3rd dose	OLS	0.028	[0.008]***	0.27	10%	0.007	[0.020]	0.34	2%	0.028	[0.008]***	0.254	11%	0.082	-0.023
Polio 1st dose	OLS	0.051	[0.009]***	0.477	11%	0.04	[0.022]*	0.56	7%	0.051	[0.009]***	0.451	11%	0.094	0.003
Polio 2nd dose	OLS	0.038	[0.009]***	0.374	10%	0.018	[0.021]	0.45	4%	0.038	[0.009]***	0.353	11%	0.086	-0.011
Polio 3rd dose	OLS	0.031	[0.008]***	0.277	11%	0.018	[0.020]	0.35	5%	0.031	[0.008]***	0.26	12%	0.085	-0.02
Measles	OLS	0.018	[0.007]***	0.149	12%	0.02	[0.017]	0.20	10%	0.018	[0.007]***	0.14	13%	0.081	-0.04

		All live children ages 16-47 months				Urban areas (youngest live children ages 0-15)				North (youngest live children ages 0-15 months)				South (younesgt live children ages 0-15 months)			
		Coefficient on I(male=1), [s.e]		Mean Y	% effect	Coefficient on I(male=1), [s.e]		Mean Y	effect	Coefficient on I(male=1), [s.e]		Mean Y	% effect	Coefficient on I(male=1), [s.e]		Mean Y	% effect
Ever breastfed?	OLS	0	[0.002]	0.983	0%	0	[0.009]	0.951	0%	0.003	[0.006]	0.952	0%	0.01	[0.007]	0.956	1%
# months breastfed	cens. reg.	1.567	[0.291]***	21.81	7%	0.301	[0.665]	7.431	4%	1.595	[0.828]*	7.665	21%	2.026	[0.791]**	7.692	26%
log(# months breastfed)	cens. reg.	0.073	[0.016]***	2.965	2%	0.017	[0.110]	1.78	1%	0.241	[0.129]*	1.793	13%	0.343	[0.131]***	1.817	19%
Vitamin A?	OLS	0.02	[0.007]***	0.217	9%	0.023	[0.015]	0.191	12%	0.03	[0.008]***	0.0766	39%	-0.007	[0.011]	0.169	-4%
Vaccination card?	OLS	0.03	[0.007]***	0.19	16%	-0.003	[0.019]	0.402	-1%	0.058	[0.011]***	0.197	29%	0.023	[0.014]	0.378	6%
BCG	OLS	0.045	[0.008]***	0.555	8%	-0.001	[0.019]	0.663	0%	0.05	[0.014]***	0.348	14%	0.017	[0.014]	0.582	3%
DPT 1st dose	OLS	0.053	[0.008]***	0.602	9%	-0.005	[0.018]	0.636	-1%	0.066	[0.014]***	0.38	17%	0.026	[0.015]*	0.59	4%
DPT 2nd dose	OLS	0.046	[0.008]***	0.536	9%	0.006	[0.020]	0.536	1%	0.051	[0.013]***	0.263	19%	0.013	[0.016]	0.477	3%
DPT 3rd dose	OLS	0.037	[0.008]***	0.464	8%	0.021	[0.021]	0.426	5%	0.043	[0.011]***	0.185	23%	0.016	[0.015]	0.365	4%
Polio 1st dose	OLS	0.055	[0.008]***	0.606	9%	0.009	[0.019]	0.641	1%	0.057	[0.014]***	0.376	15%	0.039	[0.015]***	0.601	6%
Polio 2nd dose	OLS	0.047	[0.008]***	0.555	8%	0.006	[0.020]	0.548	1%	0.046	[0.013]***	0.282	16%	0.025	[0.015]*	0.484	5%
Polio 3rd dose	OLS	0.034	[0.008]***	0.482	7%	0.02	[0.020]	0.438	5%	0.04	[0.011]***	0.198	20%	0.023	[0.015]	0.369	6%
Measles	OLS	0.039	[0.008]***	0.401	10%	-0.004	[0.018]	0.217	-2%	0.021	[0.009]**	0.111	19%	0.019	[0.013]	0.196	10%

Each coefficient corresponds to a separate estimation, and survey weights are not used. No controls are included. The number of observations for each age group varies from outcome to outcome because there are a few missing values. Cens. reg. is a censored regression. Upper bounds assume that all dead girls would have not received inputs (for dummy variables) or would have been given the 25th percentile of the girls' outcomes distribution. For boys we assume that had they lived they would all have been given inputs (for dummye variables) or given the 75th percentile of boys' outcome distribution. For upper bounds we assume the opposite. We use this rule to imput investments for dead children for whom the information from mothers' reports are missing or for children who died too young to have receive the investment (before 2 months for polio/DPT 1st dose, before 3 momths for polio/DPT 2nd dose, before 4 momths for polio/DPT 3rd dose, before 6 months for vitamin A and before 9 months for measles). The urban sample was constructed using the same restrictions as our main estimation sample--we dropped twins and individuals with missing predetermined covariates.