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Is there really a trade-off? Family Size and Investment in Child Quality in India

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Abstract

We address the relationship between number of children and investment in child quality, known as Quantity-Quality (Q-Q) trade-off, for India. Using a number of investment and outcome measures, we find that the OLS estimates suggest presence of Q-Q trade-offs in 9 out of 10 measures considered. Using the gender of the first-born child as an instrument, the trade-offs in all measures disappear. Given the concerns about the exogeneity of the instrument, we apply Oster (2016) bounds to assess sensitivity of OLS estimates to omitted variables. We find robust trade-off estimates in only 3 measures---enrollment, years of schooling, and height-for-age. However, we find more robust trade-offs in rural areas. Trade-offs appear in ever enrolled, private school attendance, expenditure on education and private coaching in addition to the trade-offs in the 3 measures for all India sample.

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1 Introduction

In this paper, we address the relationship between number of children and child quality, commonly referred in literature as Quantity-Quality trade-off (Q-Q), in the Indian context. Beginning with the seminal work of Becker (1960) and Becker and Lewis (1973) on the relationship between fertility decision and investment in child quality, several papers have empirically documented the relationship in different settings. Steelman et al. (2002) and Schultz (2008) provide a review of the literature. Recognizing the endogeneity of family size, studies have used instrumental variable strategy relying on different instruments such as twin birth (e.g. Rosenzweig and Wolpin, 1980; Black et al., 2005), gender mix of children (e.g. Conley and Glauber, 2006; Angrist et al., 2010), and gender of the first born child (e.g. Lee, 2008; Kang, 2011; Kugler and Kumar, 2017).[1] The international evidence on the relation between child quality and number of child has been mixed from a positive relation (e.g. Qian, 2009), negative relation (e..g. Conley and Glauber, 2006; Lee, 2008), and no statistically significant relation (e.g. Black et al., 2005; Angrist et al., 2010).

For the Indian context, Rosenzweig and Wolpin (1980), Sarin (2004), and Kugler and Kumar (2017) address the Q-Q trade-off. Using Additional Rural Income Survey of 1969-1971, Rosenzweig and Wolpin (1980) use twins as exogenous shock to family size and finds it to be significantly associated with lower levels of completed schooling of the woman's other (nontwin) children, based on 25 twins in 1633 families. Sarin (2004) uses gender of the first born and occurrence of twins as instrument and finds no empirical relationship between family size and weight-to-height ratio among children in India. Using the District Level Household Survey (DLHS) collected in 2007-08, Kugler and Kumar (2017) studies the impact of family size on the three educational outcomes of children in age group 5-21: ever attended school, current attendance, and completed schooling. Using the gender of the first

[1] Other instruments used in the literature include infertility (e.g. Bougma et al., 2015), miscarriage (e.g. Maralani, 2008), and distance to family planning (Dang and Rogers, 2013). Other strand of literature exploits institutional changes that give rise to changes in fertility decisions of parents (e.g. Liu, 2014; Qian, 2009).

child to instrument family size, they find that an extra child in the family reduces schooling by 0.08 years, and reduces the probability of being enrolled in school or ever attending school by about 1 and 2 percentage points, respectively.

The above mentioned studies (Rosenzweig and Wolpin, 1980; and Kugler and Kumar, 2017) have only considered educational outcomes. While educational outcomes can be easily linked to child well being, they do not necessarily reflect the allocation of resources by parents or other household members (Cáceres-Delpiano, 2006). The provision of universal elementary education from the government of India also put a question mark on the direct link between family size and educational outcomes. Furthermore, Becker's Q-Q model is a model of investment where households decide on the level of resources allocated for each child (quality). The model assumes these investments lead to higher levels of child quality but the direct implication of the model is the trade-off between child investment and number of children in the family (Cáceres-Delpiano, 2006). Hence, focusing on inputs is a more powerful test than using outcomes since inputs are one step closer to assessing the effects of family size in the causal chain.

In this paper, we consider four measures of investment in child quality: expenditure on private coaching, total education expenditure, private coaching attendance, and private school attendance.[2] There is growing evidence that the educational outcomes are better for students that attend private school (Muralidharan, 2013), and the parents who enroll their children in private schools are the ones with higher income (Azam, 2015). In addition to the investment measures, we also consider six outcomes measures: ever attended school, currently enrolled, years of schooling completed, reading test score, math test scores, and height-for-age z-score.

We find that the OLS estimates suggest presence of trade-offs in 9 out of 10 measures considered. However, once we instrument family size with gender of the first born (as used in Kugler and Kumar, 2017), the trade-offs disappear for most of the outcomes: all the

[2] Lee (2008) and Kang (2011) use private tutoring expenditure in Korea as a proxy investment in children. Cáceres-Delpiano (2006) uses private school attendance in the USA.

estimates lose their statistical significance, and the sign of the coefficient flips from negative to positive for majority of measures. We provide suggestive evidence that the exclusion restriction for the validity of instrument variable estimates may not hold for the gender of the first born, and use the bounding approach developed in Oster (2016) to assess sensitivity of OLS estimates to omitted variables. We find robust trade-off estimates at all India level in only 3 measures out of 10 considered---enrollment, years of schooling, and height-for-age. However, we find more robust trade-offs in rural areas. Trade-offs appear in ever enrolled, private school attendance, expenditure on education and private coaching in addition to the trade-offs in the 3 measures for all India sample.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 describes the empirical methodology. Section 4 presents the results. Section 5 concludes.

2 Data

We use nationally representative India Human Development Survey-2 (IHDS-2) collected in 2011-12 by the National Council of Applied Economic Research (NCAER) and the University of Maryland. The data set is publicly available from the Inter-University Consortium for Political and Social Research (ICPSR). The IHDS-2 was administered across all states both in urban and rural areas, and surveyed 27,579 households in rural India and 14,573 households in urban India. In addition to collecting a diverse set of information on individuals and households, the IHDS-2 also contains a detailed fertility history for two women in age group 15-49 per household. This helps us to find the number of children born to each mother in the household and birth order of the children. The IHDS-2 also covers detailed schooling questions and administered reading and math test for children aged 8-11.[3]

We focus both on investment and outcome measures. Our main investment measures are: private school attendance, private coaching attendance, log of expenditure on private

[3] Children are classified into five groups based on their reading ability: (1) cannot read at all; can read (2) words; (3) letters; (4) paragraph; (5) story, and four groups based on their math literacy: (1) cannot recognize number; (2) can recognize number; (3) know subtraction; (4) know division.

coaching and log of total education expenditure.[4] Our outcome measures are ever attended school, currently attending, years of schooling, reading test score, math test score, and height-for-age. Reading and math test scores are standardized. Similarly, to make height comparable among children of different ages and genders, we use height-for-age z-scores (HAZ) as the dependent variable. The HAZ is defined as the number of standard deviations that a person's height is away from the median height of a reference population of healthy children of the same age and sex.[5]

We restrict our sample to children aged 6-18 since our primary focus is on the effect of family size on investment in children's education.[6] Table 1 shows summary statistics of outcomes and explanatory variables. Our final sample include 37,764 children born to 18,935 mothers for whom both parents' information can be found in data.[7] For the reading and math scores, our analysis is restricted to children aged 8-11 as the test were only administered to 8-11 age group.

3 Empirical Methodology

We start by estimating the impact of family size using the following OLS model:

$$Y_{ijd} = \alpha + \beta N_{jd} + \gamma X_{ijd} + \mu_d + \varepsilon_{ijd}$$

where Y_{ijd} is outcomes measure for child i born to mother j residing in district d. N_{jd} is number of alive child for mother j (our measure of family size). X_{ijd} contains child level variables—age, age squared, indicator for gender, indicators for birth order; mother's

[4] log(expenditure+1).

[5] We use the 2006 WHO child growth standards.

[6] Typical ages for primary, middle, secondary, and senior secondary schools are 6-10, 11-13, 14-15, and 16-17, respectively.

[7] IHDS surveyed 51,399 children in age 6-18 age group. 8435 children were dropped as one of their parents are either not residing in the household or deceased. We further dropped 2296 children as their mother age was above 49. This is because the women module only collected fertility history for women in age 15-49. Finally, we dropped 2897 children whose mothers are not included in the detailed fertility module.

characteristics—age, age squared, and height[8]; father's characteristics—age, age squared; household characteristics: log of per capita income, indicators for main source of household income being cultivation or salary, whether household own/cultivate land, whether household own television (TV), whether household holds below poverty line (BPL) card, indicators for belonging to Muslim religion, and disadvantaged castes such as Scheduled Castes (SC), Scheduled Tribes (ST), and Other Backward Castes (OBC).[9] μ_d is district fixed effects, and ε_{ijd} is an error term. Standard errors are clustered at mother level.

$\beta < 0$ should suggest Q-Q trade-off. However, since fertility decision (N_{jd}) is normally made by parents, unobserved parental behaviors and characteristics that determine family size may also determine children's outcomes (Y_{ijd}); therefore, estimate from equation (1) using OLS is biased due to endogeneity problem. The direction of the OLS bias will depend on the sign of the conditional correlation between family size and unobserved error term: $E[N_{jd}.\varepsilon_{ijd}|X_{jd}]$. If mothers with weaker preferences for child quality have more children, OLS estimates will overstate the true QQ trade-offs, and the converse will hold for positive selection into fertility (Bhalotra and Clarke, 2016).

3.1 Exogeneity of gender of the first born child

To address the endogeneity of family size, following Kugler and Kumar (2017), we use gender of first born child as an instrument. Because of son preference prevalent in Indian society, family with first-born girl is likely to have more children. Kugler and Kumar (2017) argue that the gender of the first child can be taken as random. We use an indicator for first-born girl (FBG) as an instrument and estimate the following two-stage least square model:

$$N_{jd} = \delta_0 + \delta_1 FBG_{jd} + \delta_2 X_{ijd} + \mu_d + v_{ijd} \tag{1}$$

[8]We replace the missing mother's height with an average and included an indicator for missing mother's height.

[9]The SCs/STs and OBCs are the disadvantaged groups, and enjoy affirmative policies in India, whereas Muslims are the largest minority religious group in India, and according to the Government of India (2006), their performance on many economic and education indicators is comparable with that for SC/ST.

$$Y_{ijd} = \pi_0 + \pi_1 \hat{N}_{jd} + \pi_2 X_{ijd} + \mu_d + \eta_{ijd} \tag{2}$$

where FBG_{jd} is an indicator variable that takes a value 1 if first child born to mother j is a girl, otherwise 0. There are two identifying assumptions here. First, sex of first born must provide a strong prediction of family size, i.e. $corr(N_{jd}, FBG_{jd}) \neq 0$. Second, $Corr(FBG_{jd}, \varepsilon_{ijd}) = 0$. The second condition known as exclusion restriction implies that FBG_{jd} affects Y_{ijd} only through N_{jd}. While the first assumption can be validated in data, exclusion restrictions are debatable. The exclusion restriction may be violated if parents treat first born child differently. Using a household fixed effects model, Kaul (2016) finds that the first born child receive preferential treatment in education expenditure. Although the Prenatal Diagnostic Technique Act in India was passed in 1996 making fetal-sex determination illegal, sex-selective abortion remains of some concern.

We provide suggestive evidence that the exogeneity of the gender of the first born child may not hold. We run a regression of first born girl on mother/household characteristics and find a significant relationship with per capita income, mother's age, household having BPL card and TV (reported in appendix Table A1).[10] Although the observed variables are adjusted for, the correlation between FBG and observed characteristics suggest possible correlation of FBG with unobservables. Hence, the validity of IV estimates can be questioned.[11]

[10] This is estimated using the mother sample. In the literature, the validity of the conditional randomness assumption is supported by regressing the instrument on observables (e.g. Black et al, 2005). Kugler and Kumar (2017) carry out similar exercise, however, their data do not contain direct measure on economic status. They use quintiles of wealth index that is computed using household amenities, assets, and durables. The variables which are statistically significant in our estimation—per capita income, household having BPL card, and TV—are not controlled in Kugler and Kumar (2017). Moreover, Kugler and Kumar (2017) also find statistically significant relation between mother's age and instrument. In case of only one instrument and one endogenous variable N, $plim \hat{\beta}_{IV} = \beta + \frac{cov(FBG,\varepsilon)}{cov(FBG,N)} = \beta + \frac{\sigma_\varepsilon}{\sigma_N} \frac{corr(FBG,\varepsilon)}{corr(FBG,N)}$.

[11] The validity of more widely used twin instrument is also questioned in the recent literature. Rosenzweig and Zhang (2009) argue that the use of twins as an instrument generates upward biases because of differences in birth weight between twins and non-twins change parental behavior and overall resource allocation within the household. Using individual data for more than 18 million births (more than 500,000 of which are twins) in 72 countries, Bhalhotra and Clark (2016) show that indicators of the mother's health and health-related behaviors and exposures are systematically positively associated with the probability of a twin birth. Similarly, the exogeneity of the gender composition is also questioned. Dahl and Moretti (2008) show that gender composition affects the likelihood that parents live together. Butcher and Case (1994) provide extensive discussion of the potential that different child gender mixes may affect child costs.

3.2 Oster (2016) Bounds

We follow Oster (2006) to provide bounds for plausible impacts of family size. Oster methodology extends the Altonji et al. (2005) idea that one can use the degree of selection on observables as guidance about bias from selection on unobservables, and suggest that explanatory power of unobservables and observables should be considered together. Following Oster (2016), a consistent estimator of the effect of family size on outcome will be:

$$\beta^* = \tilde{\beta}_{OLS} - \delta \left[\mathring{\beta}_{OLS} - \tilde{\beta}_{OLS} \right] \left(\frac{R_{max} - \tilde{R}}{\mathring{R}} \right) \tag{3}$$

where $\mathring{\beta}_{OLS}$ and $\mathring{R}$ are coefficient of family size and R-squared from equation (1) without any controls, while $\tilde{\beta}_{OLS}$ and $\tilde{R}$ are coefficient of family size and R-squared, respectively, from equation (1) with a full set of controls. R_{max} is the R-squared from a hypothetical regression of outcome on treatment and both observed and unobserved controls; if the outcome is fully explained, then $R_{max=1}$. δ is the ratio of selection on unobservables to selection on observables. β^* will depend on the value of δ and $R_{max} \in (\tilde{R}, 1)$.

Under zero selection on unobservables, the $\tilde{\beta}_{OLS}$ gives one side of the bound, while β^* under equal selection of unobservables and observables ($\delta = 1$) provide the other side of the bound given R_{max}. The method also allows to calculate how much selection on unobservables relative to selection on observables (δ) explains away the Q-Q trade-off under different R_{max}. Under the assumption of equal selection of unobservables and observables ($\delta = 1$), one can assess the robustness to $R_{max} = \Pi \tilde{R}$, with varying values of Π. Oster (2016) finds that $\Pi = 1.3$ allows 90 percent of randomized control results published in top journals to survive. Therefore, we choose to report the bound assuming $R_{max} = min \left\{ 1.3\tilde{R}, 1. \right\}$. In case the bounds exclude zero, the estimates can be interpreted as being robust to omitted variable bias. Obviously, the estimate of β^*, hence the bound, depends on R_{max}, we also present R_{max} at which the sign of β flips.

4 Results

Table 2 presents the results. Column (1) in Table 2 shows impact of number of children on child level outcomes from a model that do not control for any covariates. The coefficient of family size for all the outcomes are negative and statistically significant. In column (2), we control for all the covariates described in Table 1 in addition to district fixed effects. The magnitude of the coefficients declines for each outcome measures except for two measures—years of schooling and height-for-age Z-score. The coefficient on family size flips sign for private coaching, and loses statistical significance. Moreover, the increase in R^2 varies a lot across outcome measures. For example, controlling for observables, the R^2 increase from 0.007 to 0.757 for years of schooling, while for ever attended outcome, R^2 increases from 0.015 to 0.085 only. As the negative correlation declines when we control for the covariates for most of the outcomes except the two, it signals that the observed negative correlation might not be causal. Nonetheless, after controlling for observables, the coefficient on family size remains negative and statistically significant for each of the outcome measure except for the private coaching, suggesting presence of trade-off in 9 out of 10 measures considered.

Having one more sibling reduces the education expenditure by 37 percent, private coaching expenditure by 23 percent, and probability of private school attendance by 3.1 percentage points. Similarly, having one more sibling reduces the probability of ever enrolled and current attendance by 1 and 3.1 percentage points, respectively, while years of schooling reduces by 0.23 years. Our OLS estimates are in line with Kugler and Kumar (2017) OLS estimates. They find that having one more child reduces the probability of ever attended and current attendance by 1.8 and 1.4 percentage points, while reduces years of schooling by 0.2 years.

As recognized in the literature, the OLS estimates do not provide a causal inference because family size and child level outcomes are jointly determined by unobserved parental behaviors. One option to account for endogeneity is to use instrument variables. As discussed in the Section 3.1, the evidence suggest that the exclusion restriction may not hold for gender of the first born child, hence IV estimates are not reliable. However, for the sake of comparison, we also report our IV estimates in column (3) of Table 2.[12] The 2SLS results reported in Column (3) flips the sign of the coefficient for most of the outcomes except for years of schooling and height-for-age. Moreover, the coefficient on family size is no more statistically significant for all outcome measures except for private school attendance. Kugler and Kumar (2017) IV estimates for all three of their outcomes—ever attended, current attendance, and years of schooling—retain the OLS negative signs and statistical significance. Kugler and Kumar (2017) IV estimate for years of education is only marginally negative with a magnitude of 0.08. Our IV estimate for years of schooling keep the negative sign, however the magnitude of the coefficient is small and the IV estimate loses the statistical significance. Our sample construction differs from Kugler and Kumar (2017) which may be driving the differences in the common outcomes besides the fact that our survey years are different—we use a more recent data collected in 2011-12 compared to Kugler and Kumar (2017) data which was collected in 2007-08.[13]

Nonetheless, the flipping of trade-offs sign in our IV estimates are much in line with many other studies. For example, Fitzsimons and Malde (2014) use having at least one son as instrument in the Mexican context. They find that the observed negative correlation between family size and educational attainment of females disappears when they allow for the endogeneity of family size. Using data from Wisconsin Longitudinal Study, de Haan (2010) also finds that the negative correlation between number of children and child's years of schooling flips to positive and statistically insignificant. Dayıoğlu et al. (2009) also find that after instrumentation the negative impact of sibling size on school enrollment disappears

[12]The first-stage regressions are reported in appendix Table A2, and show that the first born girl strongly predicts family size and passes the weak identification tests.

[13]Kugler and Kumar (2017) identify the FBG based on the co-resident children and not on the all alive children. They exclude mothers over age 35 to minimize the possibility that adult children may have already left the household. Since, we have access to the fertility history of women, our FBG is identified based on all alive children. Similarly, the measure of family size in our case is all alive children for each mother, while Kugler and Kumar (2017) family size is the number of surviving children under 21 years of age residing in the household at the time of survey. They acknowledge that since the DLHS data set contains neither information about children who have moved or married out nor information about total ever-born children in the family, they are constrained to use number of surviving and resident children as the measure of family size (p 839). Moreover, our control variables include more variables and our standard errors are clustered at mother-level compared to Kugler and Kumar (2017) who cluster their standard errors at the district-level.

in urban Turkey, the coefficient flips to positive, and lose the statistical significance. Since we suspect the exclusion restriction is violated for the FBG instrument, we move to the sensitivity analysis of the OLS estimates.[14]

Column (4) of Table 2 assess how much selection on unobservables relative to selection on observable should be to account for the entire impact under the assumption of $R_{max} = min\{1.3\tilde{R}, 1\}$.[15] Altonji et al. (2005) suggest that a ratio (δ) above 1 can be viewed as robust. Only for three outcomes—currently enrolled, years of schooling, and height-for-age—δ >1. It is worth noting that the OLS estimate does not suggest any trade-off in the probability of taking private coaching. Column (5) reports the δ's under Altonji et al. (2005) assumption of $R_{max} = 1$. Altonji et al. (2005) assumption of $R_{max} = 1$ is the most restrictive as it is unlikely because of measurement errors in the outcome variables. Moreover, given the low $\tilde{R}$ for many measures, $R_{max} = 1$ can be considered implausible. Not surprisingly, under this restrictive assumption, δ>1 holds only for years of schooling.

Column (6) of Table 2 provides the bounds under the assumption of $R_{max} = min\{1.3\tilde{R}, 1\}$: As evident from the Table, except for three measures—current enrollment, years of schooling and height-for-age—the bounds include zero. For private coaching outcomes, the bound are right of zero precluding trade-off. Thus we find robust evidence of trade-off in three outcomes—currently enrolled, years of schooling, and height-for-age assuming $R_{max} = 1.3\tilde{R}$. Although, $R_{max} = 1.3\tilde{R}$, allows 90 percent of published randomized control results to survive (as reported in Oster, 2016), it remains somewhat arbitrary cut-off. Hence, in column (7) we also report the value of R_{max} at which each of results fail (the trade-off disappears). For two outcomes—years of schooling and height-for-age—since the increase in R^2 strengthens the trade-off, the magnitude of β increases as we increase R^2 above $\tilde{R}$. For the rest of the outcome measures, the values of R_{max} at which the sign of flips are low suggesting that even

[14] Conley et al. (2012) derives bounds for the IV estimates when the instrument is plausible but fails the exclusion restriction. They show that the bounds for the IV estimates are most informative when the instruments are strong. Although, the FBG instrument is strong in our case as indicated by first stage estimates (Table A2), to derive the IV bounds, further assumptions about relationship between instrument and outcome are needed.

[15] The stata command psacalc (Oster, 2016b) is used for the calculations.

if the unobservables play a small role in explaining the outcome, the results may not be robust.

Many studies on Q-Q focus on rural areas only as the households in rural areas are more credit constrained. Kugler and Kumar (2017) finds a greater impact in rural India compared to urban India. Similarly, Li et al. (2008) report that trade-off was more evident in rural parts of China and was negligible in urban areas. In Table 3 we present similar results as Table 2 but our sample is restricted to rural areas only. Similar to Table 2, the OLS results suggest trade-off in all outcomes except private coaching. Moreover, the IV results flips the sign for all outcomes except two—years of schooling and height-for-age. However, the IV estimates for these two are also statistically insignificant. Assuming $R_{max} = 1.3\tilde{R}$, the evidence of trade-off seems more robust in rural areas. The $\delta > 1$ for all outcomes except the reading and math test score. The bounds exclude zero for all outcomes except test scores suggesting that robust trade-offs in ever enrolled, current enrolled, years of schooling, private school attendance, expenditures on education and coaching, and height-for-age. Under stricter condition $R_{max} = 1$, only trade-offs in years of schooling and height-for-age remains robust. It should be worth mentioning that Oster suggests $R_{max} = 1.3\tilde{R}$, as 90 percent of randomized results published in top journals survive, while only 45 percent of non-random results published in top journals survive. Hence, one can argue that R_{max} is already strict and higher value of R_{max} especially equal to 1 lead to over adjustment. For example, Oster (2016) report that at $R_{max} = 1$, only 42 percent of randomized published results in top journals and 9 percent of published non-randomized results in top journals survive. Hence, based on $R_{max} = 1.3\tilde{R}$, we conclude that the evidence of trade-offs is more robust in rural areas.

5 Conclusion

In this paper, we address the Quantity-Quality trade-off in the context of India. Our interest variables consist not only the outcome measures such as ever enrolled, current attendance, years of schooling, math and reading test scores, and height-for-age but also measures that capture investment in child such as private school attendance, private coaching attendance, expenditure on private coaching and education. Using the Oster (2016) bounds, we find robust trade-offs at all India level in only 3 out of 10 measures considered--- enrollment, years of schooling, and height-for-age. However, we find more robust trade-offs in rural areas. Trade-offs appear in ever enrolled, private school attendance, expenditure on education and private coaching in addition to the trade-offs in the 3 measures for all India sample.

Although, the bounding approach does not provide us point estimates, it is quite useful to assess whether the estimates are robust to omitted variable bias. Especially, in our case where the interest also lies in determining the sign of the coefficient to establish trade-off. Our findings suggest that the policies to reduce family size is still relevant in India, especially in rural areas. Reduced family size can potentially increase parental investment in children's human capital in rural areas. These trade-offs off course do not capture the macro benefits of lower population growth through polices to control family size, i.e. a country can educate each worker better when there are fewer workers given the scarcity of resources. Furthermore, the trade-off in years of education and enrollment in rural areas suggest that public provision of education need strengthening to mitigate the adverse impacts of larger families.

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Table 1: Summary statistics of sample children aged 6-18

	(1)	(2)	(3)
	Mean	SD	N
Outcome Variables			
Ever attended school (1/0)	0.97	0.18	37,756
Currently enrolled in school (1/0)	0.89	0.31	37,756
Years of schooling	5.11	3.45	37,754
Attend private school (1/0)	0.29	0.45	37,764
Private coaching (1/0)	0.37	0.48	37,764
Log of education expenditure	6.30	2.99	37,764
Log of private coaching expenditure	1.69	3.12	37,764
Standardized reading score	-0.07	1.01	9,401
Standardized math score	-0.06	1.00	9,364
Height-for-age Z-score	-1.59	1.95	31,128
Explanatory Variables			
Child is girl (1/0)	0.48	0.50	37,764
Child-Age	11.75	3.61	37,764
Child-Age squared	151.16	86.35	37,764
Child's birth order-2	0.31	0.46	37,764
Child's birth order-3	0.19	0.39	37,764
Child's birth order-4	0.10	0.30	37,764
Child's brder-5 or more	0.08	0.27	37,764
Other Backward Castes	0.36	0.48	37,764
Scheduled Castes	0.23	0.42	37,764
Scheduled Tribes	0.08	0.27	37,764
Muslim	0.15	0.35	37,764
log of per capita income	9.40	0.97	37,301
Joint-two mother observations from same Household	0.03	0.16	37,764
Mother's age	36.12	5.74	37,764
Mother's age squared	1337.25	419.72	37,764
Mother height in cm	151.19	8.23	36,966
Father's age	41.12	6.52	37,764
Father's age squared	1732.99	550.94	37,764
Household main income source-Cultivation (1/0)	0.25	0.43	37,764
Household main income source-Salaried (1/0)	0.16	0.36	37,764
Household holds below poverty line card (1/0)	0.37	0.48	37,764
Household own TV (1/0)	0.59	0.49	37,764
Household own land (1/0)	0.48	0.50	37,764
Urban	0.31	0.46	37,764

Note: SD: Standard Deviation. Survey weights are used.

Table 2: Q-Q Trade-offs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	No controls	Controls	IV	δ for β^{*}=0; $R_{max} = 1.3\breve{R}$	δ for β^{*}=0; R_{max}=1	Oster's bound, δ =1, $R_{max} = 1.3\breve{R}$	Max R_{max} for $\beta < 0$ under $\delta = 1$
Ever attended school (1/0)	-0.014***	-0.009***	0.026*	0.904	0.028	[-0.009, 0.003]	0.108
	(0.001)	(0.002)	(0.015)				
R-squared	0.015	0.085	0.052				
Currently enrolled (1/0)	-0.036***	-0.031***	0.005	1.146	0.093	[-0.031, -0.013]	0.259
	(0.002)	(0.003)	(0.024)				
R-squared	0.034	0.192	0.180				
Years of schooling completed	-0.187***	-0.230***	-0.000	2.370	2.211	[-0.320, -0.230]	NA
	(0.018)	(0.021)	(0.161)				
R-squared	0.007	0.757	0.753				
Attend private school (1/0)	-0.047***	-0.031***	0.095**	0.938	0.124	[-0.031, 0.005]	0.381
	(0.003)	(0.003)	(0.038)				
R-squared	0.027	0.297	0.230				
Private coaching (1/0)	-0.006**	0.003	0.013	-0.424	-0.036	[0.003, 0.021]	NA
	(0.003)	(0.004)	(0.041)				
R-squared	0.000	0.221	0.221				
Log of education expenditure	-0.491***	-0.368***	0.366	0.934	0.113	[-0.368, 0.086]	0.333
	(0.020)	(0.025)	(0.232)				
R-squared	0.068	0.261	0.214				
Log of private coaching expenditure	-0.326***	-0.228***	0.284	0.982	0.132	[-0.228, 0.010]	0.388
	(0.018)	(0.025)	(0.246)				
R-squared	0.028	0.300	0.277				
Standardized reading score	-0.149***	-0.109***	0.165	0.786	0.103	[-0.109, 0.137]	0.353
	(0.012)	(0.016)	(0.171)				
Observations	9,401	9,401	9,401				
R-squared	0.054	0.287	0.248				
Standardized math score	-0.168***	-0.105***	0.115	0.653	0.093	[-0.105, 0.200]	0.364
	(0.011)	(0.015)	(0.172)				
Observations	9,364	9,364	9,364				
R-squared	0.070	0.307	0.274				
Height-for-age Z-score	-0.040***	-0.058***	-0.011	3.164	0.201	[-0.096, -0.058]	NA
	(0.012)	(0.016)	(1.188)				
Observations	31,128	31,128	31,128				
R-squared	0.001	0.173	0.172				

Note: Standard errors in parentheses are calculated taking survey design into account and clustered at the mother level. The coefficients refer to the coefficients of family size. The number of observations in each regression except for the last three dependent variables is 37,764. Control variables include district fixed effects, age and age squared of child, child's gender, indicators for birth order of child (second, third, fourth and fifth), indicator if two mothers reside in same household, indicators for Other Backward Castes, Scheduled Castes, Scheduled Tribes and Muslim, log of per capita income, age and age squared of mother, mother's height, age and age squared of father, indicators for household main income source being cultivation or salary, indicator for household having below poverty line status, and urban dummy. In column (3), sex of first born child is used as an instrument for family size. Sample is restricted to children aged 6-18 for all outcomes, except for reading and math test, the sample of which is restricted children aged 8-11 as tests were administered to only for 8-11 age group children. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Q-Q trade-off, rural sample

Dependent Variable	(1) No controls	(2) Controls	(3) **IV**	(4) δ for β^*=0; $R_{max} = 1.3\breve{R}$	(5) δ for β^*=0; R_{max}=1	(6) Oster's bound, δ =1, $R_{max} = 1.3\breve{R}$	(7) Max R_{max} for $\beta < 0$ under $\delta = 1$
Ever attended school (1/0)	-0.013***	-0.010***	0.023	1.042	0.035	[-0.010; -0.000]	0.121
	(0.002)	(0.002)	(0.019)				
R-squared	0.012	0.093	0.068				
Currently enrolled (1/0)	-0.031***	-0.027***	0.004	1.330	0.108	[-0.027; -0.017]	0.279
	(0.003)	(0.004)	(0.030)				
R-squared	0.023	0.198	0.190				
Years of schooling completed	-0.174***	-0.201***	-0.152	2.320	1.918	[-0.251; -0.201]	NA
	(0.022)	(0.025)	(0.196)				
R-squared	0.007	0.733	0.733				
Attend private school (1/0)	-0.023***	-0.026***	0.088**	2.114	0.214	[-0.031; -0.026]	NA
	(0.004)	(0.004)	(0.044)				
R-squared	0.008	0.245	0.172				
Private coaching (1/0)	-0.001	0.001	0.020	1.309	-0.132	[0.001; 0.004]	NA
	(0.004)	(0.005)	(0.050)				
R-squared	0.000	0.252	0.250				
Log of education expenditure	-0.362***	-0.315***	0.297	1.245	0.127	[-0.314; -0.185]	0.318
	(0.024)	(0.029)	(0.276)				
R-squared	0.040	0.230	0.190				
Log of private coaching	-0.222***	-0.209***	0.308	1.581	0.239	[-0.208; -0.181]	0.480
expenditure	(0.022)	(0.029)	(0.283)				
R-squared	0.016	0.324	0.293				
Standardized reading score	-0.130***	-0.088***	0.293	0.778	0.980	[-0.088; 0.091]	0.350
	(0.015)	(0.019)	(0.235)				
Observations	6,444	6,444	6,444				
R-squared	0.041	0.285	0.187				
Standardized math score	-0.137***	-0.094***	0.188	0.776	0.100	[-0.094; 0.104]	0.331
	(0.013)	(0.018)	(0.232)				
Observations	6,419	6,419	6,419				
R-squared	0.049	0.287	0.229				
Height-for-age Z-score	-0.023	-0.063***	-0.099	74.690	5.125	[-0.132; -0.063]	NA
	(0.016)	(0.019)	(0.221)				
Observations	21,165	21,165	21,165				
R-squared	0.000	0.184	0.183				

Note: Standard errors in parentheses are calculated taking survey design into account and clustered at the mother level. The coefficients refer to the coefficients of family size. The number of observations in each regression except for the last three dependent variables is 25,568. Control variables include district fixed effects, age and age squared of child, child's gender, indicators for birth order of child (second, third, fourth and fifth), indicator if two mothers reside in same household, indicators for Other Backward Castes, Scheduled Castes, Scheduled Tribes and Muslim, log of per capita income, age and age squared of mother, mother's height, age and age squared of father, indicators for household main income source being cultivation or salary, and indicator for household having below poverty line status. In column (3), sex of first born child is used as an instrument for family size. Sample is restricted to children aged 6-18 for all outcomes, except for reading and math test, the sample of which is restricted children aged 8-11 as tests were administered to only for 8-11 age group children. *** p<0.01, ** p<0.05, * p<0.1.

Table A1: Correlation between first born girl and household characteristics (mother-level)

Dependent variable: First born is girl	
Other Backward Castes	-0.005
	(0.011)
Scheduled Castes	-0.017
	(0.011)
Scheduled Tribes	-0.004
	(0.019)
Muslim	-0.010
	(0.016)
Log of per capita income	-0.029***
	(0.005)
Joint-two mother observations from same Household	-0.014
	(0.022)
Mother's age	-0.018**
	(0.009)
Mother's age squared	0.000***
	(0.000)
Mother height in cm	0.001
	(0.000)
Father's age	0.004
	(0.007)
Father's age squared	-0.000
	(0.000)
Household main income source-Cultivation (1/0)	0.004
	(0.012)
Household main income source-Salaried (1/0)	0.001
	(0.012)
Household holds below poverty line card (1/0)	-0.031***
	(0.009)
Household own TV (1/0)	-0.019**
	(0.009)
Household own land (1/0)	0.007
	(0.011)
Urban	0.017
	(0.013)
Constant	0.893***
	(0.157)
Observations	18,935
R-squared	0.025

Note: Each observation represents a mother. Standard errors in parentheses are calculated taking survey design into account and clustered at district level. The model also include district fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Q-Q trade-offs. IV method

Dependent variables	(1) Ever attended school (1/0)	(2) Currently enrolled in school (1/0)	(3) Years of schooling	(4) Attend private school (1/0)	(5) Private coaching (1/0)	(6) Log of education expenditure	(7) log of private coaching expenditure	(8) Standardized reading score	(9) Standardized math score	(10) Height-for-age Z-score
First stage										
First born girl	0.195***	0.195***	0.195***	0.195***	0.195***	0.195***	0.195***	0.166***	0.166***	0.206***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.027)	(0.027)	(0.022)
Weak identification test										
Cragg-Donald Wald F statistic	364.57	364.57	364.43	365.00	365.00	365.00	365.00	76.90	75.71	333.13
Kleibergen-Paap Wald rk F statistic	88.08	88.08	88.04	88.17	88.17	88.17	88.17	37.57	36.91	85.17
2 SLS Results										
Family Size	0.026	0.005	-0.001	0.095**	0.013	0.338	0.285	37.570	0.116	-0.013
	(0.015)	(0.024)	(0.161)	(0.038)	(0.042)	(0.232)	(0.246)	(0.172)	(0.173)	(0.188)
Observations	37756	37756	37754	37764	36,960	37764	37764	9401	9364	31128
R-squared	0.053	0.181	0.753	0.231	0.222	0.214	0.277	0.249	0.274	0.173

Note: Standard errors in parentheses are calculated taking survey design into account and clustered at the mother level. Control variables include district fixed effects, age and age squared of child, child's gender, indicators for birth order of child (second, third, fourth and fifth), indicator if two mothers reside in same household, indicators for Other Backward Castes, Scheduled Castes, Scheduled Tribes and Muslim, log of per capita income, age and age squared of mother, mother's height, age and age squared of father, indicators for household main income source being cultivation or salary, indicator for household having below poverty line status, and urban dummy. Sample is restricted to children aged 6-18 for all outcomes, except for reading and math test. Reading and Math test score sample consist of children aged 8-11, as tests were administered to only for 8-11 age group children. *** p<0.01, ** p<0.05, * p<0.1.