Family Size and Child Migration: Do Daughters Face Greater Trade-Offs than Sons?*

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Abstract

Daughters may be less likely to migrate with parents because they tend to have more siblings in societies with strong son preference. Exploiting exogenous variation in twinning, we find that a one unit increase in family size decreases the probability that a daughter migrates by 12.5 percentage points—with stronger negative associations when migration restrictions are more stringent—but has negligible effects on sons in China. The results are indicative of family size trade-offs in a novel aspect of parental investment and highlight the need for gender-neutral relaxation of migration constraints to mitigate the gendered family size trade-offs.

Keywords: Child migration, family size trade-offs, son preference, parental investment

JEL Codes: D13, J13, J16, O15

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1 Introduction

Around 763 million or 12% of the world's population live in their home country but outside of their hometown (UNESCO, 2019). Whereas adult internal migration has been touted as beneficial for development (Meng, 2012), a darker issue arises in terms of what migrant parents do with their children. Around 138 million or 46 percent of children have migrant parents in China and among such children, around half are left-behind in their hometowns—to be taken care of by extended family—while the rest migrate with parents (UNICEF et al., 2023). Alongside such issue, son preference often results in larger sibship sizes for daughters as parents practice son preferring fertility stopping rule (Basu and De Jong, 2010). Combining these two considerations, this study posits that daughters will be less likely to migrate with parents compared to sons because girls tend to have more siblings than boys. Our research investigation highlights the unexplored interaction between two mechanisms driving gendered family size trade-offs in child migration: the indirect effect stemming from the fact that daughters tend to have more siblings and the presence of migration restrictions as additional constraints.

In this study, we investigate the effects of family size on child migration in China, a country that contains nearly one-fifth of the world's population and that has massive rural-to-urban (RU) migration. Child migrants have now reached 71 millions and make up 24% of all children in China (UNICEF et al., 2023). The potential benefits of child RU migration are twofold. First, children could enjoy better education and healthcare since urban areas tend to be more prosperous than rural areas (Huang and Zhang, 2023). Second, young migrant children can benefit from parental care, which may improve their human capital (Heckman, 2006; Yum, 2023). There is indeed extensive evidence that left-behind children tend to have lower cognitive and health outcomes compared to those who do not suffer from parental absence (Yue et al., 2020; Zhang et al., 2014). Moreover, migrant children tend to have better educational and health outcomes compared to their left-behind counterparts (e.g., see Appendix Table A1 and Zhang and Zheng (2022); Zheng et al. (2022, 2023)). The literature further documents that those who move to better neighborhoods tend to experience higher human capital gains (Chetty and Hendren, 2018; Huang and Zhang, 2023). Hence, child migration could be regarded as an important form of parental investment, and one that captures both money and time dimensions.

Whereas parents may wish to invest in their children, they may face important trade-offs, especially when they have many children. The quantity-quality (QQ) trade-off theory predicts a negative relationship between sibship size and child quality outcomes (Becker and Lewis, 1973). Nevertheless, there is mixed empirical evidence on such phenomenon, with past studies finding negative, near-zero, and even positive associations (Angrist et al., 2010; Bagger et al., 2021; Black et al., 2005; Guo et al., 2020; Li et al., 2008; Liu, 2014; Tan, 2019; Qian, 2018). Such mixed results are potentially driven by the fact that quality outcomes—such as education attainment—depend not only on parental investments but also on other inputs such as a child's own effort (Mogstad and Torsvik, 2023). Specifically, family size can potentially decrease direct parental investment in children due to parental resource constraints and increase a child's own effort due to sibling competition. The net effect on child quality that is often identified in past literature, may be mixed because family size affects parental investments and other inputs in different ways.[1]

This study proposes a framework for parental investment in child migration and estimates the causal effects of family size on a *novel* dimension and *direct* measure of parental investment. Despite a vast literature documenting gender biases in the level of parental investments—with sons receiving higher inputs in many societies (Jayachandran, 2015; Jayachandran and Pande, 2017), there is a lacuna in terms of how the *responsiveness* of parental investments to family size may differ by the child's gender. Given its discrete nature, the level of parental investments in sons' and daughters' migration may be similar, especially in smaller families. Specifically, migrant parents with fewer children may be better able to bring all of their offspring to cities, irrespective of the children's gender. Conversely, migrant parents with larger families may be unable to bring all children to cities and thus choose to leave some or all of their children behind. It follows that girls' migration may be at-risk in a setting with strong son preference, as girls tend to have more siblings.

We use data from the 2016 China Migrants Dynamic Survey (CMDS), a nationwide dataset of domestic migrants that contains rich information on migrant parents and their children. The main analyses focus on rural families with at least one migrant parent, two or more children, and all children aged 12 or below; we also perform sensitivity analyses on alternative samples. Note that rural households were bounded only at the third birth under China's fertility planning policies such that the majority of rural households tend to have multiple children in our setting (Guo et al., 2020).[2] The vast majority of households (around 90%) in the main sample have two migrant parents while around 70% of sons and daughters migrate with parents.

Estimating how sibship size may affect the probability that a child migrates is challenging due to the potential endogeneity of family size. For example, parents who prefer a more cohesive family may choose to have fewer children and also be more likely to migrate with all children. Since rural families were constrained only at the third birth, we exploit exogenous variation in twinning at the second birth to instrument for family size. Conditional on maternal conditions such as age at birth, the occurrence of twins births is generally regarded as a natural experiment (Farbmacher et al., 2018; Bhalotra and Clarke, 2020). We also perform a battery of sensitivity analyses includ-

[1] The literature also argues that positive empirical associations could be driven by economies of scale (Qian, 2018) or complementarities (Mogstad and Wiswall, 2016).

[2] Rural households faced more relaxed restrictions under the one child policy as early as the 1980s (Zhang, 2017). Using census data, Guo et al. (2020) find that the proportion of rural mothers with at least two children is quite stable and above 80% across cohorts for rural mothers born between 1940 and 1960.

ing correcting for sample selection (Heckman, 1979), restricting the sample to firstborn children, accounting for selection on unobservables (Oster, 2019), relaxing the exclusion restriction (Nevo and Rosen, 2012), and extrapolating policy relevant effects (Brinch et al., 2017).

We find that the probability that a child migrates decreases by 7.9 percentage points when the child has an additional sibling. Nevertheless, this masks an important heterogeneity by gender. Further analyses reveal that the probability that a daughter migrates decreases by 12.5 percentage points when she has one more sibling, whereas the effect of sibship size on boys is negligible. Gender differences in the trade-offs between family size and child migration are also present only for primary school age children, but not for preschool age children. Moreover, the negative family size trade-offs are mostly driven by families who migrated to prefectures with more stringent migration policies. The findings are consistent with the existence of family size trade-offs with parental investment in child migration, where such trade-offs are much greater for girls who are of primary school age and whose parents may be more constrained due to stringent migration policies.

Our study has several highlights. First, it is the first to examine family size trade-offs with child migration, a direct measure of parental investment in children, and one that captures both money and time investment.[3] Second, it highlights additional constraints that generate family size trade-offs: migration restrictions discourage larger families from migrating with children. Third, as daughters tend to have a greater number of siblings, they take the brunt of the family size trade-offs. Our study thus generates an interesting policy implication from the interaction between the perpetuation of the (indirect) female disadvantage for daughters born in larger families and the level of difficulty in accessing facilities in destination cities. In particular, gender-neutral policies—such as the relaxation of migration restrictions or the provision of education subsidies—may help mitigate gendered trade-offs by encouraging larger families to migrate with children.

2 Conceptual Framework

2.1 Rural-to-Urban Migration in China

There are around 376 million internal migrants in China, corresponding to nearly half of the world's population of intra-national migrants (UNESCO, 2019; UNICEF et al., 2023). RU migration used to be very restrictive prior to the 1980s due to the *Hukou* system, whereby households registered in a rural area were prohibited to reside, work, or access education and health facilities

[3]While a few studies have used expenditure on education as a direct measure of investment to test for QQ trade-offs (Chen, 2020; Dang and Rogers, 2016), we are not aware of any equivalent study on time investments. A handful of studies have looked into the interactions between family size and the migration decision of adults or teenage children in the context of their own labor supply and remittances (upstream transfers) (Bratti et al., 2020; Zhao and Zhong, 2019). In contrast, in our context, the migration of young children is predominantly driven by their parents' own migration decision and investment in the relatively young children (downstream transfers).

in urban areas (An et al., 2024). In response to increased demand for labor, such restrictions were substantially relaxed since the 1980s, giving rise to huge waves of RU migration since the 1990s. Such migration was predominantly driven by a desire to access a wider range of consumption goods, receive higher wages, and seek better social mobility opportunities (Lin et al., 2021; Meng and Yamauchi, 2017; Wang et al., 2019).

China sought to promote the education of migrant children in cities from the late 1990s (UNICEF, 2018). However, local governments were instructed to implement a differentiated education system for migrant children without local-*Hukou*, who were segregated in different classes and whose parents had to pay hefty extra school fees. Migrant children who did not get into public schools had to attend lower quality migrant schools or receive informal education. Better access to education was provided in response to the UN Millennium Development Goals in the 2000s. Specifically, the 2006 amendment to the *Compulsory Education Law* emphasized that host city governments and public schools had to ensure that migrant children get the compulsory education of nine years, free of discriminatory and restrictive practices (UNICEF et al., 2023).

Recent evidence indicates that the majority of migrant children ($\geq 75\%$) are now enrolled in public schools in most provinces (Huang and Zhang, 2023). Using a sub-survey from the 2010 wave of the CMDS, we also find that 68.24%, 9.70%, and 22.06% of migrant children were enrolled in public, migrant, and private schools, respectively.[4] Moreover, school fees became more affordable. RU migrant students were eligible for exemption of tuition fees for compulsory education as early as 2008 (State Council of the People's Republic of China, 2008). Nevertheless, given potential policy lags, we calculate the average annual school fees per migrant child in non-first-tier cities using the 2010 sub-survey of the CMDS. The average annual school fees range from 656 to 1597 RMB or 6.7% to 11.6% of migrant household income, among migrant students enrolled in public or migrant schools, which is consistent with Heckman (2005). Therefore, per-child school fees may be considered affordable although the total fees faced by families with many children may still take up a substantial portion of household income.

2.2 A Model of Parental Investment in Child Migration

There is abundant evidence that sons benefit from higher *levels* of investments than daughters (Jayachandran, 2015). Similarly, boys are more likely to be brought to cities than girls in China (Huang et al., 2024; Lin et al., 2021; Gao et al., 2023). Nevertheless, son preference may also indirectly affect parental investment, as it usually exhibits in son-preferring fertility stopping rule (Chen et al., 2019; Jayachandran and Pande, 2017). There may thus be differences in the *responsiveness* of child migration to family size by child gender. Specifically, daughters tend to have more siblings

[4] The sub-survey included Beijing, Zhengzhou, Chengdu, Suzhou, Zhongshan, and Hanchen (8,200 observations).

compared to sons (Basu and De Jong, 2010). As migration restrictions discourage larger families from migrating with children, daughters are at greater risk of being left-behind. The interaction between migration restrictions and son-preferring family size generates an interesting policy implication: gender neutral relaxation of migrant children's restrictions, such as education subsidies, may help mitigate gendered family size trade-offs by encouraging larger families to migrate with children. We formalize these ideas in a stylized model of son preference over child quantity (but not quality) in Appendix A.[5] The main predictions of the models are:

Prediction 1 (Gender.) *Conditional on family size, sons and daughters have equal likelihood of migrating with parents.*

Prediction 2 (Family size.) *An increase in family size will decrease the probability that a child migrates with parents.*

Prediction 3 (Family size and gender.) *The responsiveness of child migration to family size will be greater for daughters than for sons.*

Prediction 4 (Migration restrictions.) *Children whose parents migrated to more strict cities have a lower likelihood of migrating with parents.*

Prediction 5 (Family size and migration restrictions.) *The gendered family size trade-offs are exacerbated in the presence of more stringent migration restrictions.*

The intuition behind the predictions is illustrated in Figure 1. We show that there exists a threshold income above which parents migrate with children and below which parents leave children behind. The threshold income, $Y_T(n, \gamma_C)$, is a function of the number of children, n, and the stringency of migration restrictions, γ_C, in city C. Let γ_S (γ_R) denote the higher (lower) education cost in stringent (relaxed) destinations. We confirm that education costs per child are higher in stringent cities (RMB 1,613) compared to relaxed cities (RMB 984) from the 2010 sub-survey of the CMDS ($p < .01$).

We show in Appendix A that $Y_T(n, \gamma_C)$ is the same for all children of a given family size, irrespective of child gender (Prediction 1). This is because parents equally care about sons' and daughters' human capital. Conversely, as $Y_T(n, \gamma_C)$ increases in n, larger families are more likely to leave children behind compared to smaller families (Prediction 2). Moreover, as daughters tend to belong to larger families (due to son-preferring fertility stopping rule), they will be more likely to be left-behind compared to sons when there is an increase in family size as $Y_T(n, \gamma_C)$ increases at an

[5] In Appendix A, we present an alternative model where parents directly value investment in sons more than investment in daughters (son preference over quality and not quantity). Such model predicts gender differences in the *levels* of child migration for a given n. We fail to find empirical support for this in Sections 3 and 4.

increasing rate with n (Prediction 3).[6] Furthermore, destinations with higher education costs tend to have a higher income threshold, suggesting that parents who migrate to more strict destinations may be more likely to leave children behind (Prediction 4), and even more so when they have larger families as the marginal rate at which $Y_T(n, \gamma_C)$ increases in n rises with γ_C, such that the gendered family size trade-offs are exacerbated (Prediction 5).

3 Data and Empirical Strategy

3.1 Data and Sample

The CMDS is a nationally representative repeated cross-sectional dataset of domestic migrants conducted from 2009 to 2018 by the National Health Commission (Wang et al., 2021). We focus on the 2016 wave, which is the latest accessible wave that contains the richest information including each child's migration status as well as birth year and month. The 2016 wave targets domestic migrants who were aged 15 or above, with non-local *Hukou*, and who stayed in the destination city for at least one month. It covers 169,000 households in all provinces in mainland China.

Our sample selection is detailed in Appendix Figure A2. Since we are interested in RU migration in multiple children families, we focus on households with rural *Hukou* and with two children or more, yielding 51,650 households. Note that rural families in our setting were restricted only from the third birth (see Footnote 2). The majority (57.5%) of households have at least two children in our sample. We control for potential selection on family size in sensitivity analyses (Heckman, 1979).[7] We further drop households with children older than 12, capturing households with pre-school (0-5) and primary school (6-12) age children, which leaves us with 19,661 households. Children aged 12 and below would have been born after the nationwide ban on ultrasound prenatal screening in 2004 (Sun and Zhao, 2016), which mitigates potential concerns about sex or twin selection.[8] We also report separate analyses for the subsamples of pre-school and primary school age children, as well as additional analyses on older children aged 13-18.[9] After excluding a handful

[6] The predictions could still hold with linear or concave education costs (and thus linear or concave threshold income functions), depending on the income distribution (see Appendix Figure A1).

[7] Excluding single-child households is common in twin-based studies (Bagger et al., 2021; Black et al., 2005) as fertility preferences are more likely to be similar among households with two or more children. We conducted sensitivity analyses (a) on the sample of all children including those from single-child households, where the presence of first-born twins serves as the instrument, and (b) using the sub-sample of children where the oldest or youngest in the family is aged above 6 as such families are likely to have completed their fertility (Huang et al., 2021). The results and inferences were robust to these sensitivity checks.

[8] The Law of Population and Family Planning was implemented from September 2002 to April 2004, while the *Guan Ai Nv Hai Xing Dong* Program was put in place in 2003.

[9] The migration decision for older children may potentially be confounded by their own labor supply or by incentives to take the college entrance exam, known as the *Gaokao*. The *Gaokao* can only be taken in the province of origin and since different provinces use different syllabuses, migrant children have greater incentives to enroll in middle or

of households with deceased children or missing values, we reshape the household sample at the child level to get one observation per child but several observations per household. This yields a child-level sample of 40,145 children, which we use to construct twin indicators at the family level. Subsequently, we drop twin children from the child sample while retaining the indicators for the presence of twins in the family for the remaining children.[10]

Our analytic sample comprises of 38,829 children from 18,864 households. We present descriptive statistics from the analytic child-level sample in Table 1. Approximately 50% of children are male while the average family size is 2.11 children. 53% of children are of primary school age and the vast majority, 90%, of children have two migrant parents.

Child migration. 69.5% and 69.1% of sons and daughters, respectively, migrate with parents, suggesting that male and female children have similar migration probabilities. However, there is substantial variation in these proportions by family size. 89.7% (10.3%) of children were in families with two (three or more) children. In families with two children, the proportion of sons and daughters who migrate are very similar in magnitudes and not statistically different, at 69.7-69.9%. Conversely, in families with three or more children, the proportion of sons who migrate is higher at 66.7% compared to the 63.2% of migrant daughters ($p < .05$). Two observations arise from comparing smaller to larger families. First, children belonging to larger families have a lower probability of migrating compared to children from smaller families, irrespective of child gender ($p < .05$). Second, the within-gender difference in migration probability (between families with 2 children and families with 3 or more children) is around twice larger for daughters than for sons, suggesting that daughters are more likely to be left-behind compared to sons in larger families.

Daughters also tend to have more siblings. 18.7% (18.1%) of children are from only-son (only-daughter) households with 2.0 (2.1) children on average, and 63.1% of children come from mixed-gender households with 1.04 sons and 1.11 daughters. On average, daughters' families are larger with 2.13 children compared to an average of 2.09 children in sons' families ($p < .01$). The generally larger family size for daughters compared to sons is consistent with son-preferring fertility stopping rule (Basu and De Jong, 2010).

Additionally, 64% (25.4%) of children are from household that migrate with all (no) children. Around 10.6% of children belong to households with partial child migration, where some children migrate while others are left-behind. Among such families, around 52.4% (47.6%) of sons (daughters) are brought to an urban area. The gender difference is statistically significant ($p < .01$)

high school in the region of origin of their *Hukou* (Chen and Feng, 2017; UNESCO, 2019).

[10] The twining rate in our sample—defined as the number of twins over the total number of children in all (including single child) households—is 1.71% (1,316 twins out of 76,911 children), which is similar to previous studies (Li et al., 2008). We exclude twins because they are arguably different from singletons (Bagger et al., 2021).

and is driven predominantly by mixed-sex households.[11] This is consistent with son preference in mixed-gender families with partial child migration. Nevertheless, we note that children from such families make up a relatively small proportion, 6.6%, of the child-level sample.[12]

3.2 Empirical Specifications

The first and second stages of the two-stage least squares (2SLS) model are, respectively, given by:

$$FS_{jp} = \alpha_1 + \alpha_2 Twin_{jp} + \alpha_3 SA_{ijp} + \alpha_4 RelBO_{ijp} + \alpha_5 Son_{ijp} + \mathbf{X}'_{ijp}\alpha_6 + \mu_p + \eta_{ijp}. \tag{1}$$

$$M_{ijp} = \beta_1 + \beta_2 \widetilde{FS}_{jp} + \beta_3 SA_{ijp} + \beta_4 RelBO_{ijp} + \beta_5 Son_{ijp} + \mathbf{X}'_{ijp}\beta_6 + \mu_p + \varepsilon_{ijp}. \tag{2}$$

Our main outcome of interest, M_{ijp} is an indicator that takes unity if child i in household j located in destination province p migrated, and zero otherwise. Our main predictor is family size (i.e., the number of children), FS_{jp}. We control for an indicator of whether the child is of primary school age SA_{ijp}, relative birth order, $RelBO_{ijp} = \frac{(BO_{ijp}-1)}{(FS_{jp}-1)}$, where BO_{ijp} is the raw birth order such that 0 (1) denotes the first (last) born in the family (Zhang et al., 2023), and an indicator of whether the child is male, Son_{ijp}. We also perform interaction analyses using child gender (see Appendix Section B.1) to capture gender-differentiated effects. $\mathbf{X}_{ijp}$ is a vector of additional covariates: second order polynomial in child and parental ages, maternal age at second birth, parental education indicators for middle school, high school, and college and above, both parents migrant (yes/no), any grandparent migrant (yes/no), duration of migration in years, willing to settle down (yes/no), and indicators for within-province and within-prefecture migration. We also control for province fixed effects, μ_p. The error term, ε_{ijp}, is clustered at the household level.[13]

Our main instrumental variable (IV), $Twin_{jp}$, is an indicator that takes unity if the family had twins at the second parity and 0 otherwise. 0.41% of children in the analytic sample have second born twins in their family.[14] Our IV strategy identifies the weighted average causal response from compliers, that is, families induced by twinning to switch from having n to at least $n+1$ children, for any $n = 2,3,4$ (Angrist et al., 2010). As documented above, those with a rural *Hukou* were restricted only at the third birth (see Footnote 2). If all couples desire at least two children—as is often the case in rural areas (Zhuang et al., 2020)—then the increase in fertility due to twins at

[11] Among partial child migrant households, 50.3% (50%) of sons (daughters) from only-son (only-daughter) households migrate, while 53.7% (46.2%) of sons (daughters) from mixed-gender families migrate.

[12] The vast majority, 94%, of children in households with partial child migration have two migrant parents. Sensitivity analyses dropping children in partial parent or child migrant families indicate that our results are quantitatively robust, suggesting that partial migration is unlikely to be a major driver in our setting.

[13] Results were robust to dropping the indicators of both parents or any grandparent migrant, to controlling for either or both of destination and origin province fixed effects, and to clustering standard errors at the prefecture level.

[14] The twinning rate of second-born children is 0.40% as per the definition in Footnote 10.

second birth would be orthogonal to unobserved parental preferences and constraints. Conversely, twins at higher parities may be systematically correlated with fertility as extra pregnancies are choices while firstborn twins would tend to have a lower proportion of compliers given that most rural households desire at least two children. In sensitivity analyses, we use an indicator for the presence of twins at any parity as an alternative IV and find the results to be very similar.[15]

Although twinning is often regarded as good as random (Bagger et al., 2021; Black et al., 2005; Chen et al., 2019; Guo et al., 2020; Oliveira, 2016), there may still be some threats to identification. First, the presence of twins may cause parents to reallocate resources across siblings due to the shorter birth spacing and low birth endowment of twins (Black et al., 2005; Rosenzweig and Zhang, 2009). If the presence of twins leads to greater (lower) resources being allocated towards singletons, then negative family size effects from 2SLS would be underestimated (overestimated). Second, unobserved maternal conditions may correlate with both the occurrence of twins and child outcomes (Bhalotra and Clarke, 2019, 2020). Third, there may be selection into twinning, although, as discussed in Section 3, this is unlikely in our context as all children in our sample were born after the nationwide prohibition of ultra-sound prenatal screening.

Given these concerns, we conduct balance checks by regressing our $Twin_{jp}$ indicator on several pre-determined household characteristics (Appendix Table A2). All coefficients are statistically insignificant at the 10% level. We further compare the socio-economic characteristics of families with and without second-born twins (Appendix Table A3). Besides maternal middle schooling, all other characteristics are relatively well-balanced, being either very close in magnitudes and/or not statistically significantly different between the two types of families. Thus, twin families seem to be similar in observable characteristics compared to non-twin families.

We additionally report the reduced-form ordinary least squares (OLS) estimates of the IVs and partial validity checks—where we control for both $Twin$ and FS—in Appendix Table A4. When family size is not controlled for, there is a negative and significant association between $Twin$ and the probability that a child migrates. However, the coefficient of $Twin$ become statistically insignificant at the 5% level once family size is controlled for, suggesting that the correlations between family size and child migration have been absorbed by the family size covariate so that there is limited remaining associations between the IVs and child migration.[16] Hence, the exclusion restriction cannot be invalidated. To further mitigate concerns about the twin instruments, we derive bounds for family size effects using two separate methods. First, by assuming that selection on

[15]The twinning rate at any parity is 1.71% as per the definition in Footnote 10. The proportion of children in the analytic child-level sample, who have a twin in their families is 0.63%.

[16]Whereas twinning seems marginally significant in one of the specifications, a limitation of this test is that if the coefficient of the instrument is significant, then we cannot know whether this is due to the violation of exclusion restriction or due to the harmless correlation built by controlling for the collider variable (i.e., family size) when the exclusion restriction is valid (Morgan and Winship, 2014).

unobservables is proportional to selection on observables (Oster, 2019), and second, by relaxing the exclusion restriction (Nevo and Rosen, 2012).

Finally, similar to prior studies that used twinning as an instrument, we identify a Local Average Treatment Effect (LATE) from compliers (Bagger et al., 2021; Black et al., 2005; Chen et al., 2019; Oliveira, 2016). To shed light on the external validity of our estimates, we extrapolate to estimate average treatment effects (ATE) below (Brinch et al., 2017).

4 Results

4.1 Main Results

Table 2 presents OLS estimates in Columns (1) and (3), and 2SLS estimates in Columns (2) and (4). Focusing on our preferred 2SLS specification, we find evidence of positive and statistically significant associations between the presence of twins at second birth, $Twin_{jp}$, and family size, FS_{jp} from the first-stage regressions. The Kleibergen-Paap Wald rk F-statistics are reasonably large and greater than the rule-of-thumb thresholds of 10 and 104.7 (Lee et al., 2022), suggestive of strong instruments. The Kleibergen-Paap *rk* LM test also always rejects the null hypothesis of underidentification while the Anderson-Rubin χ^2 test rejects the null hypothesis that the coefficients of the endogeneous variables are jointly equal to zero.

Turning to the second stage estimates, we note that child gender per se does not seem to have any effects on child migration in Column (2). Whereas Column (4) seems to indicate that Son_{ijp} has a negative coefficient, the marginal effect (ME) is still not statistically significantly different from zero. To see this, using the notation from Appendix eq. (A3), the ME of being a son rather than a daughter is given by $\gamma_3\overline{FS}_{jp} + \gamma_5\overline{SA}_{ijp} + \gamma_7\overline{RelBO}_{ijp} + \gamma_8$, where $\overline{var}$ denotes the average. The ME is -0.002 and statistically insignificant. Conditional on family size, we thus do not find evidence of gender disparity in child migration, which is consistent with Prediction 1. Conversely, having one more sibling is associated with a 7.9 pp decrease in the probability that a child migrates ($p < .05$) in Column (2), which indicates the presence of a trade-off between family size and child migration consistent with Prediction 2. The negative family size effects seem driven by daughters in Column (4). A unit increase in family size leads to a 12.5 pp decrease in the probability that a daughter migrates ($p < .05$) but has no effect on sons. The gender difference in family size effects is 11.1 pp ($p < .10$). Consistent with Prediction 3, the results indicate that daughters' migration are more likely to be traded-off when there is an increase in sibship size.

Interestingly, we also find that children who are of primary school age are more likely to migrate with parents compared to children who are of pre-school age, especially sons. These results are consistent with the fact that parents may be more willing to invest in school age children in terms of child migration. We additionally extend our sample to include older children and conduct separate analyses on the subsamples of children aged 0-5 (pre-school age), 6-12 (primary school age), and 13-18 (middle school age and above) in Appendix Table A5. The results confirm that the negative family size effects on daughters' migration are the most relevant for primary school age children. Finally, we find that the youngest child is less likely to migrate than the oldest child, especially the youngest daughter. This is consistent with evidence of lower parental investments in children at higher parities (Bagger et al., 2021; Black et al., 2005; Zhang et al., 2023).

4.2 Robustness Checks

Our results are robust to a battery of sensitivity analyses, which are summarized here and detailed in Appendix B.2.

First, we *bound family size effects* by (i) accounting for selection on unobservables using selection on observables (Oster, 2019) and (ii) relaxing the exclusion restriction to allow for partial correlation between *Twin* and ε (Nevo and Rosen, 2012). From Figure 2, the bounds on family size effects confirm our inferences on gendered family size trade-offs.

Second, we *address sample selection* concerns by (i) performing sample selection correction using the gender composition of firstborn twins as an instrument (Farbmacher et al., 2018; Heckman, 1979), (ii) re-estimating family size effects on the subsample of firstborn children, whose gender is arguably random (Ebenstein, 2010; Sun and Zhao, 2016), and (iii) focusing on the subsample of migrant children born outside of the destination city. From Appendix Tables A6 and A7, the results are robust to the sample selection corrections and subsample analyses.

Third, we explore several *alternative econometric models*: (i) two-step estimation whereby we predict child migration net of birth order effects using family fixed effects models in the first step, and subsequently apply 2SLS using the net predicted value from the first step as outcome in the second step (Bagger et al., 2021), and (ii) apply control function approach to take into account the discrete nature of child migration (Wooldridge, 2010). From Appendix Table A8, the results are once again robust.

Finally, we *extrapolate from the LATE to estimate ATE*, and find the effects to be comparable (Brinch et al., 2017).

4.3 Stringency of *Hukou* Policies

To assess whether migration restrictions matter for family size trade-offs, we compile the composite *Hukou* index constructed by Zhang and Lu (2019) for 2014–2016. The index is multi-dimensional and comprises of four sub-indices that measure the difficulty in obtaining a local

Hukou through investment, housing property purchase, high-end and ordinary employment (An et al., 2024; Gao et al., 2023). The composite index takes a non-negative value, and a higher value reflects greater difficulty for migrants to obtain a local *Hukou*. We utilize the median value of the index as the cutoff to split the sample into strict and relaxed prefectures.

From Table 3, the proportion of children who migrate to prefectures with more strict *Hukou* policies is lower than the proportion of children who migrate to the relaxed prefectures, which is consistent with Prediction 4. Moreover, from Columns (2) and (5), there are clear family size trade-offs with child migration, especially for daughters, in prefectures with more stringent *Hukou* policies. Conversely, there is no statistically significant evidence of family size trade-offs in prefectures with more relaxed *Hukou* policies. Moreover, from Column (6), there is a significant difference in family size effects on daughters' migration between relaxed and strict prefectures ($p < .05$), which is consistent with Prediction 5. Thus, the strictness of *Hukou* policies reinforces gendered family size trade-offs with child migration. This result is consistent with the fact that countries with greater access to education (e.g., Norway) tend to have insignificant or modest fertility-human capital trade-offs (Doepke et al., 2023).

5 Conclusion

Our study demonstrates the existence of important trade-offs between the migration of daughters (but not sons) and sibship size. We do not find supporting evidence that gender *per se* affects the levels of child migration. Rather, migrant parents with smaller families are more likely to bring all children to cities while those with larger families are more likely to leave all children behind. As daughters tend to have more siblings, they are more likely to be left behind, suggesting that son preference has an *indirect* effect on daughters' migration through family size. The gendered family size trade-offs are particular pertinent in the presence of restrictive migrant policies. Ironically, cities with more stringent migration restrictions tend to have better education quality (Huang and Zhang, 2023), suggesting that migrant children may lose out on important human capital gains due to restrictive access to public schools. Our study has the potential to generate important implications on the interactions between sibship size and parental investments in children, in a setting with relatively strict migrant policies and where sons are strongly favored. Specifically, better access to quality education by migrant children may help reduce gendered family size trade-offs by encouraging larger families to migrate with children.

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Tables and Figures

Table 1: Demographic and Socio-Economic Characteristics of Children

	Overall		Daughters		Sons	
	Mean/Prop.	S.D.	Mean/Prop.	S.D.	Mean/Prop.	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Child characteristics						
Prop. child migration	0.69	0.46	0.691	0.46	0.695	0.46
No. of children	2.11	0.34	2.13	0.37	2.09	0.31
Prop. male	0.50	0.50	0.00	0.00	1.00	0.00
Prop. primary school age	0.53	0.50	0.57	0.50	0.50	0.50
Child age	5.95	3.35	6.21	3.36	5.70	3.33
Relative birth order	0.50	0.49	0.40	0.48	0.59	0.49
Panel B: Parental characteristics						
Maternal age at second birth	27.24	4.02	27.34	4.03	27.13	4.01
Maternal age	31.35	4.20	31.36	4.25	31.33	4.16
Paternal age	33.40	4.53	33.46	4.61	33.33	4.46
Maternal education is middle school	0.64	0.48	0.63	0.48	0.64	0.48
Maternal education is high school	0.18	0.39	0.19	0.39	0.18	0.38
Maternal education is college and above	0.04	0.20	0.05	0.21	0.04	0.20
Paternal education is middle school	0.63	0.48	0.63	0.48	0.63	0.48
Paternal education is high school	0.21	0.41	0.21	0.41	0.21	0.40
Paternal education is college and above	0.06	0.23	0.06	0.24	0.06	0.23
Panel C: Household characteristics						
Prop. with both parents migrant	0.90	0.30	0.90	0.31	0.90	0.30
Prop. with grandparents migrant	0.06	0.23	0.06	0.23	0.06	0.24
Duration of migration in years	5.22	4.39	5.22	4.40	5.22	4.38
Prop. willing to settle	0.61	0.49	0.61	0.49	0.61	0.49
Prop. inter-province migration	0.53	0.50	0.53	0.50	0.54	0.50
Prop. inter-prefecture migration	0.30	0.46	0.30	0.46	0.30	0.46
Prop. twins at second parity in family	0.004	0.06	0.005	0.07	0.003	0.06
No. of observations	38,829		19,553		19,276	

Notes: Data is from the 2016 China Migrants Dynamic Survey (CMDS). Means or proportions and standard deviations are reported.

Table 2: Effect of Family Size on Child Migration

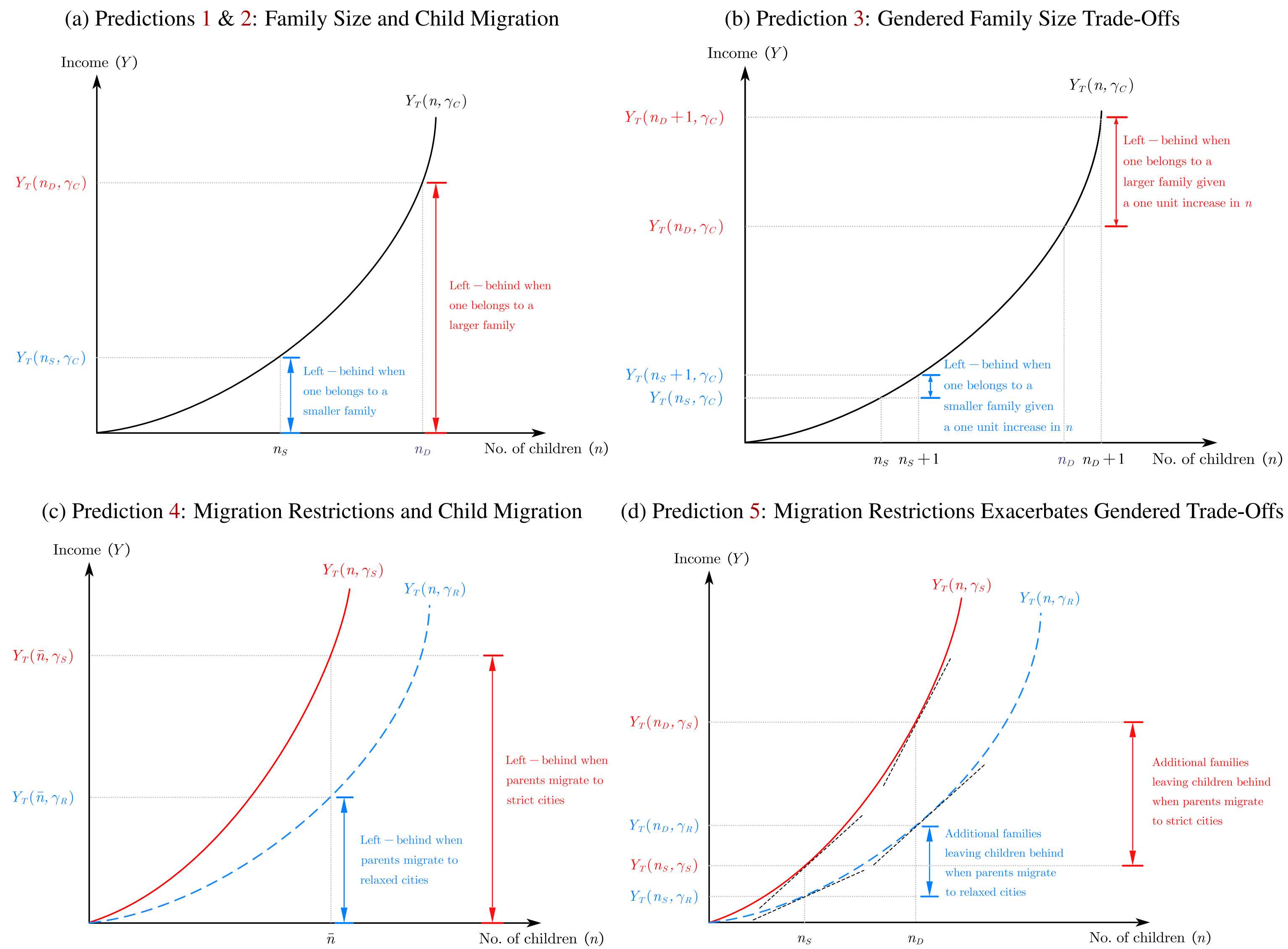
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
No. of children	-0.025***	-0.079**	-0.031***	-0.125**
	(0.009)	(0.036)	(0.011)	(0.051)
No. of children × Son			0.020*	0.111*
			(0.010)	(0.066)
Primary school age	0.032***	0.032***	0.013	0.018**
	(0.007)	(0.007)	(0.008)	(0.009)
Primary school age × Son			0.038***	0.030***
			(0.009)	(0.010)
Relative birth order	-0.017***	-0.031***	-0.030***	-0.042***
	(0.005)	(0.011)	(0.008)	(0.011)
Relative birth order × Son			0.027***	0.024**
			(0.010)	(0.010)
Son	-0.000	-0.002	-0.075***	-0.265*
	(0.004)	(0.004)	(0.024)	(0.137)
Marginal effects for sons				
No. of children			-0.012	-0.013
			(0.011)	(0.044)
Primary school age			0.051***	0.048***
			(0.008)	(0.009)
Relative birth order			-0.003	-0.018
			(0.007)	(0.012)
First-stage coefficients				
$FS : Twins$		0.999***		0.960***
		(0.038)		(0.043)
$FS : Twins \times Son$				0.103**
				(0.048)
$FS \times Son : Twins$				-0.009
				(0.010)
$FS \times Son : Twins \times Son$				1.078***
				(0.052)
Kleibergen-Paap Wald rk F-statistic		706.753		242.008
Kleibergen-Paap rk LM p-value		0.000		0.000
Anderson-Rubin χ^2 p-value		0.026		0.044
Sample mean of the outcome variable	0.693	0.693	0.693	0.693
R^2	0.289	0.287	0.289	0.286
Obs.	38,829	38,829	38,829	38,829

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). All the controls listed in Section 3 are included in the models above. Standard errors clustered at the household level are in the parentheses.
***p<.01, **p<.05, *p<.10.

Table 3: Heterogeneities of Stringentness of *Hukou* Policies in Destination

	Relaxed	Strict	(1)-(2)	Relaxed	Strict	(4)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)
No. of children	-0.029	-0.171***	0.142	-0.004	-0.263***	0.259**
	(0.057)	(0.066)	(0.087)	(0.082)	(0.090)	(0.118)
No. of children × Son				-0.055	0.240*	-0.295*
				(0.103)	(0.130)	(0.173)
Primary school age	0.035***	0.036***	-0.001	0.017	0.018	-0.002
	(0.012)	(0.013)	(0.018)	(0.014)	(0.016)	(0.022)
Primary school age × Son				0.036**	0.034*	0.002
				(0.017)	(0.018)	(0.023)
Relative birth order	-0.020	-0.044**	0.024	-0.032*	-0.054***	0.021
	(0.016)	(0.018)	(0.026)	(0.017)	(0.020)	(0.029)
Relative birth order × Son				0.023	0.026	-0.004
				(0.017)	(0.020)	(0.026)
Son	0.005	-0.010	0.016*	0.091	-0.542**	0.633*
	(0.006)	(0.007)	(0.009)	(0.214)	(0.268)	(0.359)
Marginal effects for sons						
No. of children				-0.059	-0.023	-0.036
				(0.068)	(0.090)	(0.127)
Primary school age				0.052***	0.052***	0.000
				(0.014)	(0.016)	(0.020)
Relative birth order				-0.010	-0.027	0.018
				(0.019)	(0.020)	(0.029)
Kleibergen-Paap Wald rk F-statistic	227.142	1180.129		75.153	836.816	
Kleibergen-Paap rk LM p-value	0.000	0.000		0.000	0.000	
Anderson-Rubin χ^2 p-value	0.612	0.009		0.683	0.011	
Sample mean of the outcome variable	0.743	0.620		0.743	0.620	
R^2	0.314	0.237		0.314	0.235	
Obs.	13,021	12,795		13,021	12,795	

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). As the index is only available for 120 prefectural cities, not every city in the CMDS matches with a prefectural index. Hence, only matched observations are kept, resulting in a smaller sample: 25,816 observations, which account for roughly 66.49% of the main analytical sample. All the controls listed in Section 3 are included in the models above. Standard errors are clustered at the household level in Columns (1), (2), (4) and (5). Standard errors in Column (3) and (6) computed by block-bootstrapping at the household level with 200 repetitions are in the parentheses.
***p<.01, **p<.05, *p<.10.



(a) Predictions 1 & 2: Family Size and Child Migration

(b) Prediction 3: Gendered Family Size Trade-Offs

(c) Prediction 4: Migration Restrictions and Child Migration

(d) Prediction 5: Migration Restrictions Exacerbates Gendered Trade-Offs

Figure 1: Model 1 – Threshold Incomes when Girls Have More Siblings than Boys

Notes: The figure illustrates the threshold incomes above which parents will migrate with children and below which parents will leave children behind. n_S denote the family size of sons (or smaller families) and n_D denotes the family size of daughters (or larger families). $Y_T(n, \gamma_C)$ denotes the threshold for destination city C, where γ_S and γ_R denote strict and relaxed cities, respectively.

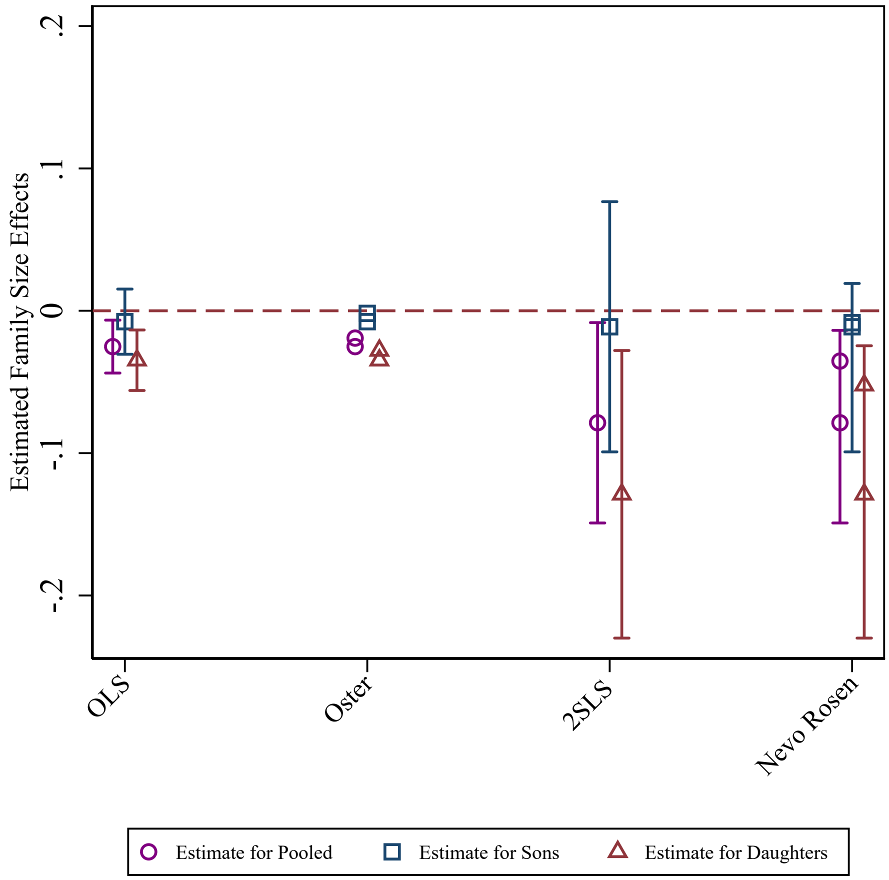


Figure 2: Bounding Family Size Effects

Notes: The figure plots the estimates of family size effects based on the pooled sample, the sub-sample of sons, and the sub-sample of daughters, respectively. All the controls listed in Section 3 are included. Standard errors are clustered at the household level. 95% confidence intervals are reported.

Online Appendix

Appendix A Parsimonious Models of Child Migration

We present two models of parental investment in child migration. In the first and main model, parents value sons' and daughters' quality equally but son preference drives larger sibship sizes for daughters such that it *indirectly* affects investment. This model predicts gender differences in the responsiveness of child migration to family size but not in the levels of child migration for a given sibship size. Given the indirect effect, gender-neutral education subsidies may help mitigate gender differences in family size trade-offs. In the second model, parents value sons' quality more than daughters' quality such that son preference *directly* affects investment. That model predicts gender differences in both the responsiveness of child migration to family size and in the levels of child migration for a given sibship size. Given the direct effect, female-targeted education subsidies may help mitigate gender differences in family size trade-offs. We do not find evidence that supports gender differences in the levels of investment in our empirical analyses, suggesting that the indirect effect captured by the first model may be more pertinent in our context.

A.1 Son Preference on Quantity: Indirect Effect

Suppose that a migrant parent has exogenous income Y and n children. The parent chooses family consumption c and average investment in a child's education quality e. Let child human capital be:

$$h(e) = \theta_0 + \theta_1 e.$$

θ_0 may be interpreted as the human capital of a left-behind child who benefits from free schooling of quality $e = 0$ in the rural hometown. θ_1 captures the returns to investment when a child migrates to the city and thus needs to pay higher schooling fees for higher quality education $e > 0$.

We assume that education cost per unit of quality, $\gamma_C(n)$ in city C, is convex in n: $\gamma_C'(n) > 0$ and $\gamma_C''(n) > 0$. This is based on the fact that it is harder for larger families to enroll all of their children in public schools due to capacity constraints. Thus, some children may go to the more expensive and lower quality migrant or private schools (Chen and Feng, 2017), which makes it costlier to achieve a given quality of education for all children.[A1] Consistent with Section 2, we further assume that education is more expensive in stringent (S) compared to relaxed (S) cities:

[A1] From the sub-survey of 2010 CMDS, parents of migrant children in public (migrant or private) schools spent RMB 780.886 (RMB 2,525.659) per child on tuition. The difference is statistically significant at the 1% level. For anecdotal evidence, see https://www.theguardian.com/world/2010/mar/15/china-migrant-workers-children-education and https://www.cq.gov.cn/hdjl/cqhlwdc/lyxd1/202310/t20231027_12485462.html.

$\gamma_S(n) > \gamma_R(n)$, such that per-quality education cost increases more steeply with n in strict than in relaxed cities. Note that the model predictions could also hold if we have a linear or concave (instead of convex) cost function, depending on the income distribution (e.g., see Footnote A2).

The parent maximizes utility over consumption and overall human capital of children:

$$ln(c) + \alpha ln(nh),$$

subject to the budget constraint:

$$c + \gamma_C(n)e = Y.$$

Solving the model, education quality of a child is given by:

$$e = \max\left\{0, \frac{1}{1+\alpha}\left[\frac{\alpha Y}{\gamma_C(n)} - \frac{\theta_0}{\theta_1}\right]\right\}.$$

There is thus a threshold income level—denoted as $Y_T(n,\gamma_C) = Y_T(n,\gamma_C(n))$ for simplicity—above which parents migrate with children and invest positively in their education quality, $e > 0$:

$$Y_T(n,\gamma_C) = \frac{1}{\alpha}\frac{\theta_0}{\theta_1}\gamma_C(n).$$

As Y_T is independent of child gender, Prediction 1 is obvious: conditional on the same n, sons and daughters have equal likelihood of migrating. Moreover, the threshold income increases in n, generating Prediction 2 (see Figure 1a). We next show that the responsiveness of child migration to family size is greater for daughters than for sons (Prediction 3). First, we build on Basu and De Jong (2010) to define son-preferring fertility stopping rule and get the following lemma:

Definition A1 (Son-preferring fertility stopping rule.) *Couples continue childbearing until they attain a desired target number of sons or hit a ceiling for the maximum number of children.*

Lemma A1 (Daughters have more siblings.) *The expected number of siblings for female children is greater than expected number of siblings for male children.*

The numerical proof for Lemma A1 can be found in Basu and De Jong (2010). We further show that this holds in our analytic sample in Section 3. Second, define family sizes n_S for small families and n_D for large families: $n_S < n_D$, and note that the threshold income increases at an accelerating rate due to convexity: $\frac{d^2Y_T(n,\gamma_C)}{dn^2} = \frac{1}{\alpha}\frac{\theta_0}{\theta_1}\gamma_C''(n) > 0$. It follows that Y_T increases by a greater amount for larger families than for smaller families when n increase by one unit (see Figure 1b):

$$\left.\frac{dY_T(n,\gamma_C)}{dn}\right|_{n_S} < \left.\frac{dY_T(n,\gamma_C)}{dn}\right|_{n_D} \Rightarrow \left.\frac{dY_T(n,\gamma_C)}{dn}\right|_{\text{sons}} < \left.\frac{dY_T(n,\gamma_C)}{dn}\right|_{\text{daughters}}. \tag{A1}$$

Thus, sons would be less likely to be left-behind compared to daughters when n increases.[A2]

It is also straightforward to see that, for a given n, the threshold income is higher in cities with more stringent migration restrictions compared to cities with more relaxed migration restrictions:

$$Y_T(n,\gamma_S) = \frac{1}{\alpha}\frac{\theta_0}{\theta_1}\gamma_S(n) > \frac{1}{\alpha}\frac{\theta_0}{\theta_1}\gamma_R(n) = Y_T(n,\gamma_R).$$

Thus, the probability that children are left behind when parents migrate to a stringent city is greater than when parents migrate to a relaxed city, generating Prediction 4 (see Figure 1c). Finally, the rate at which the threshold income increases in n is greater for more stringent cities: $\frac{dY_T(n,\gamma_S)}{dn} = \frac{1}{\alpha}\frac{\theta_0}{\theta_1}\gamma_S'(n) > \frac{1}{\alpha}\frac{\theta_0}{\theta_1}\gamma_R'(n) = \frac{dY_T(n,\gamma_R)}{dn}$. This implies that the probability that a child migrates will decrease to a greater extent when there is an increase in n, when the parent migrates to a strict city compared to a relaxed city. Moreover, as shown above, the rate at which the threshold income increases in n is greater for larger than for smaller family sizes. As daughters tend to have more siblings, it follows that the gendered family size trade-offs are exacerbated in the presence of more stringent migration restrictions, giving rise to Prediction 5 (see Figure 1d).

A.2 Son Preference on Quality: Direct Effect

We now modify the model to allow the parent to have different valuations for sons' and daughters' human capital. Let e_g and $h_g = \theta_0 + \theta_1 e_g$, respectively, denote investment in education quality and human capital of a child of gender $g = S, D$. Assume further that the proportion of sons in the family is $q = 0.5$ as we observe in our data (see Table 1). Son preference is captured by the fact that the parent puts a greater weight on sons than on daughters: $\alpha > 1-\alpha$.

The parent maximizes utility over consumption and human capital of sons and daughters:

$$ln(c) + \alpha ln(qnh_S) + (1-\alpha)ln((1-q)nh_D),$$

subject to the budget constraint:

$$c + \gamma_C(n)qe_S + \gamma_C(n)(1-q)e_D = Y.$$

[A2] Specifically, eq. (A1) is a sufficient condition when the distribution of income is uniform or as in Figure A1a. Eq. (A1) is not a necessary condition though. For example, with a linear education cost function, we instead have an equal increase in Y_T for small and large families when n increases by one unit (i.e., replace the inequalities in (A1) with equalities). Nevertheless, larger families still face higher income thresholds than smaller families: $Y_T(n_D,\gamma) > Y_T(n_S,\gamma)$. Thus, even if Y_T increases by the same amount for both types of families, the fall in the proportion of larger families who can migrate with children can still be larger than smaller families. Thus, daughters may be more likely to be left-behind when there is an increase in family size as daughters tend to have more siblings. See Figures A1b and A1c for illustrations with linear and concave costs. Figure A1d provides an illustration whereby the threshold incomes are the same for all families, but income distributions differ by family size.

Solving the model, the human capital of sons and daughters are, respectively, given by:

$$h_S = \frac{\alpha}{\gamma_C(n)}[\theta_1 Y + \gamma_C(n)\theta_0],$$

$$h_D = \frac{1-\alpha}{\gamma_C(n)}[\theta_1 Y + \gamma_C(n)\theta_0].$$

It follows that there are threshold income levels above which parents migrate with sons ($e_S > 0$ or $h_S > \theta_0$) and daughters ($e_D > 0$ or $h_D > \theta_0$), respectively:

$$Y_S = \frac{(1-\alpha)\theta_0}{\alpha\theta_1}\gamma_C(n),$$

$$Y_D = \frac{\alpha\theta_0}{(1-\alpha)\theta_1}\gamma_C(n).$$

Whereas Predictions 2 to 5 carry over in this model, Prediction 1 does not carry over. Specifically, since $1-\alpha < \alpha$, we have $Y_S < Y_D$ for a given n. Thus, son preference on child quality implies that parents will be more likely to migrate with sons than with daughters, even if n were the same. We do not find support for this in our analytic sample (see Sections 3 and 4).

A.3 Policy Implications

The two models generate different policy implications. In the first model of son preference on child *quantity*, gender-neutral education subsidies may help mitigate the gendered family size effects. By reducing $\gamma_C(n)$, the slope of $Y_T(n, \gamma_C)$ decreases such that the gap in the threshold incomes between smaller and larger families also decreases (see Figure 1d). This implies that whereas the relaxation of migration restrictions in the form of gender-neutral education subsidies encourages both small and larger families to migrate with children, such incentives are stronger for larger than for smaller families. As daughters tend to belong to larger families, they experience a much larger decrease in the probability that they would be left-behind when migration restrictions are relaxed.

In the second model of son preference on child *quality*, the family size trade-offs arise due to the direct effect of son preference on child quality. Thus, this model predicts that even within the same family, parents may migrate with sons but not daughters. In such case, gender-targeted education subsidies that reduce $\gamma_C(n)$ only for daughters may help mitigate the gendered family size effects. Should we observe the prevalence of partial child migration, where families with mixed-gender children are more likely to bring sons but not daughters to cities, the gendered policy implication would hold. However, in our analytic sample, we find that most families tend to migrate either with all or no children (see Section 3 for a discussion) so that the gender neutral policy from the first model seems more pertinent in our context.

Appendix B Additional Analyses

B.1 Interaction Analyses by Child Gender

We extend the 2SLS model to allow for heterogeneous effects by child gender:

$$\begin{aligned} K_{jp} = \quad & \delta_1 + \delta_2 Twin_{jp} + \delta_3 Twin_{jp} \times Son_{ijp} + \delta_4 SA_{ijp} + \delta_5 SA_{ijp} \times Son_{ijp} \\ & + \delta_6 RelBO_{ijp} + \delta_7 RelBO_{ijp} \times Son_{ijp} + \delta_8 Son_{ijt} + \mathbf{X}'_{ijp}\delta_9 + \mu_p + \eta_{ijp}. \end{aligned} \tag{A2}$$

$$\begin{aligned} M_{ijp} = \quad & \gamma_1 + \gamma_2 \widetilde{FS}_{jp} + \gamma_3 \widetilde{FS_{jp} \times Son}_{ijp} + \gamma_4 SA_{ijp} + \gamma_5 SA_{ijp} \times Son_{ijp} \\ & + \gamma_6 RelBO_{ijp} + \gamma_7 RelBO_{ijp} \times Son_{ijp} + \gamma_8 Son_{ijt} + \mathbf{X}'_{ijp}\gamma_9 + \mu_p + \varepsilon_{ijp}. \end{aligned} \tag{A3}$$

$K_{jp} = \{FS_{jp}, FS_{jp} \times Son_{ijp}\}$ whereby separate first-stage regressions are run with FS_{jp} and $(FS_{jp} \times Son_{ijp})$ as dependent variables. From model (A3), the marginal effect (ME) of family size on daughters (sons) is given by γ_2 $(\gamma_2 + \gamma_3)$. Similarly, γ_4 $(\gamma_4 + \gamma_5)$ captures the ME of being of primary school age for a daughter (son). Finally, the ME of relative birth order for daughters (sons) is given by γ_6 $(\gamma_6 + \gamma_7)$. Including interaction terms between child gender and all controls—equivalent to sub-sample regressions by child gender as in Figure 2—yields very similar results.

B.2 Robustness Checks

B.2.1 Bounding Family Size Effects

Accounting for selection on unobservables. Consider our OLS model:

$$M_{ijp} = \beta_1 + \beta_2 FS_{jp} + \beta_3 SA_{ijp} + \beta_4 RelBO_{ijp} + \beta_5 Son_{ijp} + \mathbf{X}'_{ijp}\beta_6 + \mu_p + \varepsilon_{ijp}. \tag{A4}$$

We follow Oster (2019) to bound the family size effects using selection on observed variables as a guide to selection on unobserved variables in model (A4). This method builds on the observation that omitted variable bias is often deemed to be limited if a coefficient is stable when observed controls are added in a regression. Following Oster (2019), we assume that (i) there is equal selection on observables and unobservables, and (ii) the attainable R^2 in a regression with a full set of (observable and unobservable) controls, R_{max}, is equal to 1.3 times the estimated R^2 from a regression with observable controls only, $\hat{R}_{OLS}$, as per specification (A4). Under these two assumptions, the identified set lies between the OLS estimate from (A4), $\hat{\beta}_2^{OLS}$, and the bias-adjusted estimate, $\beta_2^* = \hat{\beta}_2^{OLS} - \left[\hat{\beta}_2^0 - \hat{\beta}_2^{OLS}\right] \frac{R_{max} - \hat{R}_{OLS}}{\hat{R}_{OLS} - \hat{R}_0}$, where $\hat{\beta}_2^0$ and $\hat{R}_0$ are, respectively, the estimated treatment effect and R^2 from a regression of child migration on sibship size without any controls. Figure 2 plots OLS estimates as well as the bounds à la Oster (2019). The identified sets of β_2 look fairly tight and are consistent with our inferences on gendered family size trade-offs.

Relaxing the exclusion restriction. We now relax the assumption that $Twin_{jp} \perp \varepsilon_{ijp}$ in 2SLS model (1)-(2) and estimate bounds on family size effects to provide more compelling evidence of family size trade-offs on child migration. In particular, we follow Nevo and Rosen (2012) to allow the IV to be correlated with the error term in the second stage regression. Under the relatively weak assumptions that (i) the correlation between the instrument and ε_{ijp} has the same sign as the correlation between the endogenous regressor and ε_{ijp} and (ii) the instrument is less endogenous than the endogenous regressor, we can derive informative bounds for the causal effects of family size on child migration based on set identification. In our context, we argue that FS_{jp} and ε_{ijp} could be positively correlated because parents with a strong preference for old age support from children could potentially choose to have more children and also be more likely to migrate with children. This is consistent with the seemingly upward bias from OLS estimates compared to 2SLS estimates in Table 2. Conversely, we believe that $Twin_{jp}$ and ε_{ijp} could be negatively correlated because the presence of $Twin_{jp}$ in the family imposes simultaneous and greater demands on parental time, which may result in a lower ability to cater for and thus migrate with other children. To satisfy the first assumption (i), we specify our instrument as $-Twin_{jp}$. In that case, there is also a negative correlation between FS_{jp} and $-Twin_{jp}$ as can be inferred from our first stage regression coefficients reported in Table 2. Nevo and Rosen (2012) show that in such situation, the family size effect would be bounded by the 2SLS and OLS estimates: $\hat{\beta}_2^{2SLS} \leq \beta_2 \leq \hat{\beta}_2^{OLS}$. If the second assumption (ii) additionally holds, then we can obtain tighter bounds: $\hat{\beta}_2^{2SLS} \leq \beta_2 \leq \hat{\beta}_2^{NR}$, where $\hat{\beta}_2^{NR}$ is a 2SLS estimator that employs $NR = \sigma_{FS}Twin_{jp} - \sigma_{Twin}FS_{jp}$ as an IV, and σ_{var} denotes the standard deviation of $var = Twin_{jp}, FS_{jp}$. Figure 2 reports the point estimates and 95% confidence intervals from 2SLS estimates as well as the Nevo and Rosen (2012) bounds. The presence of negative family size effects on child migration are confirmed for daughters but not for sons.

B.2.2 Sample Selection

Sample selection correction. We apply Heckman type selection correction to account for potential selection into families with two or more children. Consider a Probit model for family size:

$$\mathbb{P}(\mathbb{1}\left\{FS_{jp} \geqslant 2\right\} = 1) = \Phi(\rho_1 + \rho_2 \bar{z}_{jp} + \rho_3 SA_{ijp} + \rho_4 Son_{ijp} + \mathbf{X}'_{ijp}\rho_5 + \mu_p), \tag{A5}$$

where $\bar{z}_{jp} \equiv Twin_{jp}^{1same} - Twin_{jp}^{1opp}$ is an IV for the selection equation. $Twin_{jp}^{1same}$ and $Twin_{jp}^{1opp}$ are indicators for same-sex and opposite-sex firstborn twins, respectively. $\bar{z}_{jp}$ is -1 for opposite-sex and 1 for same-sex firstborn twins, and 0 for non-firstborn-twin households. We leverage the nature of the three-valued instrument to avoid the perfect-prediction issue that arises in one-child households when using a classical twin indicator in a Probit model. The inverse Mills ratio, $\lambda(\hat{\Delta})$, is predicted from the family size equation (A5) and inserted as an additional control in our 2SLS estimation.

We estimate the entire system of equations using the IV-Heckit model and block-bootstrap the whole process at household level with 200 repetitions to compute standard errors. As shown in the first two columns of Table A6, the results are nearly the same as those in Table 2.

Firstborn children. As per Angrist et al. (2010), the estimated effects for lower parity children may be more precise than for higher parity children, because the outcomes of the last-born child possibly come from an endogenously selected sample. We thus follow Black et al. (2005) and employ a sample truncation specification to re-estimate the family size effects on the sample of firstborn children (48% of children). This set of sensitivity analyses also helps mitigate potential concerns on the endogeneity of child gender since firstborn children's gender are generally believed to be random (Ebenstein, 2010; Sun and Zhao, 2016). From Columns (1) and (2) of Table A7, the family size effects are similar to those in Table 2. The effects are also robust to controlling for the inverse Mills ratio to account for potential sample selection (Columns (3) and (4) of Table A6).

Children born outside of the destination city. Child migration captures both children who migrated by travelling from a rural to an urban area with the parents and children who were born to migrant parents in the destination cities. We refine the sample by considering only children who were born outside the destination city (68.4% of children). From Columns (3) and (4) of Table A7, the family size effects are once again comparable to those in Table 2.

B.2.3 Alternative Specifications

Two-step estimation. It is well-known that birth order configuation tends to be jointly determined with family size. We thus perform an additional robustness check to identify birth order effects separately from family size effects using a two-step estimation approach (Bagger et al., 2021; Bratti et al., 2020). In the first step, we use mother fixed effect (MFE) models—that exploit within-family variation—to identify birth order effects. We then predict child migration net of birth order effects. In the second step, we apply 2SLS but using the predicted child migration net of birth order effects as outcome variable. As the outcome variable is now predicted from a regression, we block-bootstrap the standard errors, clustered at the household level. From Table A8, the results and inferences from the two-step estimation are similar to those in Table 2.

Control function approach. We now take the discrete nature of migration into account and estimate marginal effects using Probit models and a control function approach (Wooldridge, 2010). From Columns (5) and (6) of Table A8, the ME are very similar to those from the linear models, which boosts confidence in the robustness of the 2SLS estimates.

B.2.4 Estimating Average Treatment Effects

We follow Brinch et al. (2017) to estimate MTEs and extrapolate to obtain ATEs. As the MTE framework builds on binary treatment, we start by replicating our 2SLS model (1)-(2) by replacing FS with D^{FS}, an indicator that takes unity if the family has three or more children and 0 if the family has two children, and perform subsample analyses for sons and daughters separately. The inferences using D^{FS} as treatment (Table A9) are similar to those that use FS (Table 2).

Next, we reframe the econometric model as a generalized Roy model:

$$M_{ijp} = (1 - D^{FS}_{jp})M^0_{ijp} + D^{FS}_{jp}M^1_{ijp}, \tag{A6}$$

$$M^t_{ijp} = \mathbf{X}'\iota^{\mathbf{t}} + U^t_{ijp}, \text{ where } t = 0, 1, \tag{A7}$$

$$D^{FS}_{jp} = \mathbb{1}\{v(\mathbf{X}, \mathbf{Z_{jp}}) > V^D_{ijp}\}, \tag{A8}$$

where $\mathbf{X}$ is augmented to include all covariates and $\mathbf{Z_{jp}}$ are the IVs. The conditional expectations of M^t_{ijp} for $t = 0, 1$ are defined:

$$\mathbb{E}(M^t_{ijp}|\mathbf{X} = \mathbf{x}, V^D_{ijp} = c) = \mathbf{x}'\iota^{\mathbf{t}} + \mathbb{E}(U^t_{ijp}|V^D_{ijp} = c) \equiv \mathbf{x}'\iota^{\mathbf{t}} + k_t(c).$$

The MTE can then be expressed as a function of $\mathbf{x}$ and $k_t(c)$:

$$\Delta^{MTE}(\mathbf{x}, c) = \mathbb{E}(M^1_{ijp}|\mathbf{X} = \mathbf{x}, V^D_{ijp} = c) - \mathbb{E}(M^0_{ijp}|\mathbf{X} = \mathbf{x}, V^D_{ijp} = c) = \mathbf{x}'(\iota^{\mathbf{1}} - \iota^{\mathbf{0}}) + k_1(c) - k_0(c).$$

The MTE can be interpreted as the expected treatment effects for individuals who are indifferent between having at least three children and having two, with $\mathbf{X} = \mathbf{x}$ and $\mathbb{P}(D^{FS}_{ijp} = 1|\mathbf{X}, \mathbf{Z_{jp}}) = c$. Following the literature, we assume that $k_t(c) = \alpha_t\left(c - \frac{1}{2}\right)$, where $k_t(c)$ is a linear function of c and α_t, and may be interpreted as the coefficient of an "inverse Mills ratio" type expression in the canonical Heckit model. As it is challenging to identify MTE with *Twin* as the only IV (there are no never-takers as families with twins at second birth have at least three kids), we use an indicator for having the same gender for the first two births as an additional IV to *Twin* (see Table A9).

Figure A3 plots the estimated MTEs for sons and daughters. In our context, the MTEs capture the average effects of being born in a household with three or more children for daughters or sons on the margin between migrating and being left-behind. These margins correspond to percentiles of the distribution of the unobserved resistance, V^D_{ijp}. Following Mogstad and Torgovitsky (2018), we next express the ATEs as a weighted average of MTEs, assigning the same weight on the MTE at each value of V^D_{ijp}. The ATE for sons is negligible (-0.006, p-value>0.1) while the ATE for daughters is -0.111 ($p < .05$), which is consistent with gendered family size trade-offs.

Appendix C Additional Tables and Figures

Table A1: Educational Performance and Health Status: Migrant vs. left-behind children

	Pooled Sample		Girls		Boys	
	Left-Behind	Migrant	Left-Behind	Migrant	Left-Behind	Migrant
Panel A: Selected test score and health outcomes						
Chinese test score (standardized)	69.45	71.07*	72.49	74.00*	66.93	68.41*
	(10.07)	(9.34)	(8.62)	(8.15)	(10.32)	(9.53)
Math test score (standardized)	69.39	70.78*	69.81	71.21*	69.12	70.49 *
	(10.27)	(9.50)	(9.94)	(9.06)	(10.47)	(9.82)
English test score (standardized)	69.30	70.41*	72.55	73.36*	66.60	67.80*
	(10.25)	(9.55)	(8.68)	(8.35)	(10.60)	(9.75)
BMI	18.68	18.96*	18.54	18.68	18.81	19.22*
	(3.18)	(3.42)	(2.80)	(2.90)	(3.49)	(3.84)
Ever hospitalized last year	0.09	0.07*	0.09	0.06*	0.09	0.08
	(0.29)	(0.25)	(0.29)	(0.25)	(0.29)	(0.27)
Overall health status (1:worst; 5:best)	3.93	4.12*	3.87	4.09*	3.98	4.16*
	(0.91)	(0.90)	(0.91)	(0.88)	(0.91)	(0.91)
Shortsighted	0.54	0.62*	0.60	0.68*	0.49	0.57*
	(0.50)	(0.48)	(0.49)	(0.47)	(0.50)	(0.49)
Grit index	9.50	9.49	9.94	9.86	9.14	9.18
	(2.65)	(2.72)	(2.27)	(2.54)	(2.87)	(2.80)
Noncognitive skill	11.77	11.95*	11.84	11.99	11.75	11.94
	(3.32)	(3.40)	(2.93)	(3.19)	(3.59)	(3.53)
Depression index	10.67	10.18*	10.95	10.18*	10.40	10.21
	(4.33)	(4.45)	(4.17)	(4.04)	(4.42)	(4.76)
Panel B: Demographics						
Age	13.61	13.43*	13.57	13.39*	13.64	13.47*
	(1.28)	(1.23)	(1.27)	(1.23)	(1.29)	(1.23)
Single child in the family	0.34	0.33	0.32	0.28*	0.37	0.39
	(0.47)	(0.47)	(0.47)	(0.45)	(0.48)	(0.49)
Boy	0.53	0.52				
	(0.50)	(0.50)				
Mother's education attainment	3.45	3.50	3.48	3.53	3.43	3.49
	(1.83)	(1.80)	(1.86)	(1.78)	(1.82)	(1.82)
Father's education attainment	3.78	4.03*	3.84	4.07*	3.75	3.99*
	(1.81)	(1.89)	(1.85)	(1.90)	(1.78)	(1.89)
Family income status	2.84	3.07*	2.83	3.07*	2.86	3.06*
	(0.60)	(0.47)	(0.58)	(0.46)	(0.62)	(0.49)
Observations	4,400	2,888	2,020	1,360	2,301	1,490

Notes: Data from the China Education Panel Study (2013-2014). Unweighted means and standard deviations (in parentheses) are presented. Mother's and father's education attainment: 1: no schooling; 2: elementary; 3: junior high; 4: technical school; 5: vocational high school; 6: senior high; 7: junior college; 8: bachelor degree; 9: above bachelor degree. Family income status: 1: very poor; 2: somewhat poor; 3: moderate; 4: somewhat rich; 5:very rich. A handful of observations were missing from some of the variables including child gender. Migrant children are defined as those without local *Hukou*; Left-behind children defined as those living without at least one of their parents. * indicates that the difference between left-behind and migration children is statistically significantly different at the 5% level.

Table A2: Balance Check: Do Pre-Determined Characteristics Predict the Instrument?

	Dummy for twinning at second parity					
	(1)	(2)	(3)	(4)	(5)	(6)
Maternal age at second birth	0.00003	-0.00024	-0.00021	-0.00022	-0.00024	-0.00023
	(0.00008)	(0.00017)	(0.00017)	(0.00017)	(0.00017)	(0.00017)
Maternal age square		-0.00001	-0.00000	-0.00000	-0.00000	-0.00000
		(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Maternal age		0.00116*	0.00034	0.00037	0.00037	0.00038
		(0.00062)	(0.00085)	(0.00086)	(0.00086)	(0.00087)
Paternal age square			-0.00002	-0.00002	-0.00002	-0.00002
			(0.00001)	(0.00001)	(0.00001)	(0.00001)
Paternal age			0.00126	0.00128	0.00123	0.00130
			(0.00083)	(0.00083)	(0.00083)	(0.00082)
Dummy for maternal education is middle school				-0.00196	-0.00233	-0.00230
				(0.00124)	(0.00147)	(0.00147)
Dummy for maternal education is high school				-0.00029	-0.00121	-0.00119
				(0.00144)	(0.00179)	(0.00179)
Dummy for maternal education is college and above				-0.00045	-0.00180	-0.00182
				(0.00206)	(0.00216)	(0.00216)
Dummy for paternal education is middle school					0.00069	0.00065
					(0.00158)	(0.00158)
Dummy for paternal education is high school					0.00160	0.00161
					(0.00184)	(0.00183)
Dummy for paternal education is college and above					0.00215	0.00219
					(0.00222)	(0.00222)
Dummy for both parents migrant						0.00064
						(0.00109)
Dummy for grandparent migrant						0.00210
						(0.00166)
Dummy for inter-province migration						0.00013
						(0.00103)
Dummy for inter-prefecture migration						-0.00133
						(0.00105)
Duration of migration in years						0.00006
						(0.00009)
Dummy for willing to settle						-0.00004
						(0.00070)
Sample mean of the outcome variable†	0.004	0.004	0.004	0.004	0.004	0.004
R^2	0.00000	0.00022	0.00030	0.00047	0.00052	0.00071
Obs.	38,829	38,829	38,829	38,829	38,829	38,829

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). Standard errors clustered at the household level are in the parentheses.
†The sample mean is the proportion of singleton children who had second-born twin siblings.
***p<.01, **p<.05, *p<.10.

Table A3: Balance Check by Twinning at Second Parity at Household Level

	Twin Families		Non-Twin Families		Tests of Differences	
	Mean/Prop.	S.D.	Mean/Prop.	S.D.	(1)−(3)	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)
Maternal age at second birth	27.50	3.83	27.30	4.02	0.21	0.53
Paternal age	34.21	4.11	33.37	4.55	0.84**	0.02
Maternal age	31.97	3.98	31.33	4.21	0.65*	0.06
Prop. with both parents migrant	0.92	0.28	0.90	0.3	0.02	0.50
Maternal education is middle school	0.55	0.50	0.63	0.48	-0.09**	0.03
Maternal education is high school	0.22	0.42	0.18	0.39	0.04	0.23
Maternal education is college and above	0.05	0.22	0.04	0.21	0.01	0.66
Prop. with grandparents migrant	0.08	0.28	0.06	0.23	0.03	0.15
Paternal education is middle school	0.58	0.50	0.63	0.48	-0.05	0.16
Paternal education is high school	0.25	0.43	0.21	0.41	0.04	0.24
Paternal education is college and above	0.08	0.27	0.06	0.24	0.02	0.32
Prop. inter-province migration	0.58	0.49	0.54	0.50	0.05	0.25
Prop. inter-prefecture migration	0.25	0.43	0.30	0.46	-0.05	0.16
Duration of migration in years	5.82	4.96	5.22	4.38	0.60*	0.09
Prop. willing to settle	0.63	0.48	0.61	0.49	0.02	0.57

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). The analyses are conducted at household level. We refer to households with and without second-born twins to as twin and non-twin families, respectively. No. of twin families = 153, and no. of non-twin families = 18,711.
***p<.01, **p<.05, *p<.10.

Table A4: Reduced-Form Regressions and Partial Instrument Validity Test

	Reduced-Form Regressions		Partial Instrument Validity Test	
	Baseline	Interaction	Baseline	Interaction
	(1)	(2)	(3)	(4)
Twin	-0.079**	-0.121**	-0.056	-0.094*
	(0.035)	(0.048)	(0.037)	(0.050)
Twin × Son		0.107		0.092
		(0.067)		(0.068)
No. of children			-0.023**	-0.028**
			(0.010)	(0.011)
No. of children × Son				0.017
				(0.011)
Primary school age	0.032***	0.012	0.032***	0.013
	(0.007)	(0.008)	(0.007)	(0.008)
Primary school age × Son		0.040***		0.038***
		(0.009)		(0.009)
Relative birth order	-0.011*	-0.026***	-0.017***	-0.031***
	(0.006)	(0.008)	(0.005)	(0.008)
Relative birth order × Son		0.029***		0.028***
		(0.010)		(0.010)
Son	0.001	-0.036***	-0.000	-0.070***
	(0.004)	(0.009)	(0.004)	(0.024)
R^2	0.288	0.289	0.289	0.289
Obs.	38,829	38,829	38,829	38,829

Notes: Data from the 2016 China Migrants Dynamic Survey (CMDS). All the controls listed in Section 3 are included in the models above. Standard errors clustered at the household level are in the parentheses.
***p<.01, **p<.05, *p<.10.

Table A5: Effect of Family Size on Child Migration for Different Age Groups

	Age 0-5		Age 6-12		Age 13-18	
	(1)	(2)	(3)	(4)	(5)	(6)
No. of children	0.022	-0.052	-0.103***	-0.140**	0.025	0.018
	(0.083)	(0.116)	(0.039)	(0.055)	(0.043)	(0.053)
No. of children × Son		0.166		0.092		0.022
		(0.133)		(0.076)		(0.087)
Relative birth order	-0.012	-0.009	-0.020**	-0.027**	-0.020	-0.005
	(0.047)	(0.045)	(0.010)	(0.013)	(0.013)	(0.019)
Relative birth order × Son		0.022		0.010		-0.023
		(0.015)		(0.015)		(0.023)
Son	-0.009*	-0.373	0.004	-0.194	0.037***	-0.007
	(0.006)	(0.278)	(0.006)	(0.161)	(0.009)	(0.187)
Marginal effects for sons						
No. of children		0.113		-0.047		0.040
		(0.088)		(0.053)		(0.070)
Relative birth order		0.012		-0.017		-0.027*
		(0.043)		(0.013)		(0.016)
Kleibergen-Paap Wald rk F-statistic	363.368	95.547	756.104	190.202	852.281	277.073
Kleibergen-Paap rk LM p-value	0.000	0.000	0.000	0.000	0.000	0.000
Anderson-Rubin χ^2 p-value	0.790	0.368	0.008	0.023	0.563	0.808
Sample mean of the outcome variable	0.713	0.713	0.675	0.675	0.618	0.618
R^2	0.303	0.300	0.283	0.282	0.283	0.283
Obs.	18,063	18,063	20,766	20,766	13,097	13,097

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). The 0-5 and 6-12 age groups are from our main analytical child-level sample, where children are from households without children aged above 12. The 13-18 age group belongs to households without any child older than 18. Similar results can be obtained from (a) subsample regressions on children from households consisting of children aged 0-5 only, 6-12 only, and 13-18 only, and (b) from subsample regressions on children aged 0-5, 6-12, and 13-18 from households with all children aged below 18. All the controls listed in Section 3 are included in the models above. Standard errors clustered at the household level are in the parentheses.
***p<.01, **p<.05, *p<.10.

Table A6: Sample Selection Correction à la Heckman (1979)

	Whole Sample		Firstborn Sample	
	Baseline	Interaction	Baseline	Interaction
	(1)	(2)	(3)	(4)
No. of children	-0.078**	-0.124**	-0.094***	-0.131***
	(0.037)	(0.051)	(0.036)	(0.048)
No. of children × Son		0.111*		0.097
		(0.066)		(0.067)
Primary school age	0.032***	-0.001	-0.019	-0.056***
	(0.007)	(0.013)	(0.016)	(0.021)
Primary school age × Son		0.065***		0.075***
		(0.021)		(0.029)
Relative birth order	-0.031***	-0.041***		
	(0.011)	(0.012)		
Relative birth order × Son		0.022**		
		(0.010)		
Son	0.031**	-0.257*	0.045**	-0.216
	(0.012)	(0.138)	(0.020)	(0.137)
Inverse Mills Ratio	-0.216***	-0.151**	-0.318***	-0.224**
	(0.073)	(0.073)	(0.116)	(0.099)
Marginal effects for sons				
No. of children		-0.013		-0.033
		(0.047)		(0.049)
Primary school age		0.064***		0.019
		(0.013)		(0.022)
Relative birth order		-0.019		
		(0.012)		
Kleibergen-Paap Wald rk F-statistic	705.799	241.399	1882.452	553.902
Kleibergen-Paap rk LM p-value	0.000	0.000	0.000	0.000
Anderson-Rubin χ^2 p-value	0.028	0.047	0.008	0.021
Sample mean of the outcome | Selection	0.693	0.693	0.680	0.680
R^2	0.288	0.287	0.291	0.290
Obs. of the outcome equation	38,829	38,829	18,804	18,804

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). All the controls listed in Section 3 are included in the models above. Standard errors computed by block-bootstrapping at the household level with 200 repetitions are in the parentheses. All regressions above are based on the IV-Heckit model discussed in Section B.2.2.
***p<.01, **p<.05, *p<.10.

Table A7: Sample Restrictions

	Firstborn Children		Children Born Elsewhere	
	Baseline	Interaction	Baseline	Interaction
	(1)	(2)	(3)	(4)
No. of children	-0.095***	-0.132***	-0.088**	-0.162***
	(0.035)	(0.049)	(0.042)	(0.058)
No. of children × Son		0.098		0.175**
		(0.068)		(0.075)
Primary school age	-0.019	-0.027*	0.015	0.001
	(0.014)	(0.015)	(0.010)	(0.011)
Primary school age × Son		0.022		0.031**
		(0.015)		(0.013)
Relative birth order			-0.043***	-0.054***
			(0.012)	(0.013)
Relative birth order × Son				0.022*
				(0.013)
Son	-0.006	-0.226	0.003	-0.397**
	(0.006)	(0.138)	(0.005)	(0.156)
Marginal effects for sons				
No. of children		-0.034		0.013
		(0.047)		(0.051)
Primary school age		-0.005		0.032***
		(0.017)		(0.011)
Relative birth order				-0.032**
				(0.013)
Kleibergen-Paap Wald rk F-statistic	1889.853	555.945	478.986	159.288
Kleibergen-Paap rk LM p-value	0.000	0.000	0.000	0.000
Anderson-Rubin χ^2 p-value	0.007	0.019	0.032	0.016
Sample mean of the outcome variable	0.680	0.680	0.596	0.596
R^2	0.291	0.290	0.278	0.276
Obs.	18,804	18,804	26,570	26,570

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). All the controls listed in Section 3 are included in the models above. Standard errors clustered at the household level are in the parentheses.
$^{***}p$<.01, $^{**}p$<.05, $^{*}p$<.10.

Table A8: Alternative Specifications: Two-Step Estimation and Control Function Approach

	Mother Fixed Effect		Two-Step Estimation		Control Function Approach	
	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
	(1)	(2)	(3)	(4)	(5)	(6)
No. of children			-0.075**	-0.119***	-0.073**	-0.106**
			(0.030)	(0.046)	(0.032)	(0.042)
No. of children × Son				0.107*		0.089
				(0.064)		(0.061)
Primary school age	0.023***	0.011*	0.032***	0.022**	0.034***	0.020**
	(0.005)	(0.006)	(0.007)	(0.008)	(0.007)	(0.009)
Primary school age × Son		0.023***		0.022**		0.029***
		(0.007)		(0.009)		(0.010)
Relative birth order	-0.006	-0.011*			-0.030***	-0.040***
	(0.005)	(0.006)			(0.010)	(0.011)
Relative birth order × Son		0.009				0.022**
		(0.008)				(0.010)
Son	0.000	-0.017**	-0.005	-0.246*	-0.002	-0.216*
	(0.003)	(0.007)	(0.004)	(0.134)	(0.004)	(0.127)
Marginal effects for sons						
No. of children				-0.012		-0.017
				(0.040)		(0.046)
Primary school age		0.034***		0.043***		0.049***
		(0.006)		(0.008)		(0.008)
Relative birth order		-0.002				-0.018*
		(0.006)				(0.011)
Kleibergen-Paap Wald rk F-statistic			788.129	238.388		
Kleibergen-Paap rk LM p-value			0.000	0.000		
Anderson-Rubin χ^2 p-value			0.028	0.051		
Sample mean of the outcome variable	0.693	0.693	0.696	0.696	0.693	0.693
R^2 (Pseudo R^2)	0.011	0.012	0.288	0.287	0.259	0.259
Obs.	38,829	38,829	38,829	38,829	38,829	38,829

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). All the controls listed in Section 3 are included in the models above. Two-step estimation comprise of mother fixed effects models in the first step – Columns (1) and (2) – and 2SLS in the second step – Columns (3) and (4). Marginal effects are reported for the control function approach, which is estimated from IV-Probit models – Columns (5) and (6). The standard errors in Columns (1), (2), (5) and (6) are clustered at the household level while standard errors in Columns (3) and (4) are block-bootstrapped at the family level with 200 repetitions.
***p<.01, **p<.05, *p<.10.

Table A9: Family Size Effects with Binary Treatment

	Subsample of Sons			Subsample of Daughters		
	Both IV	Twin IV Only	Same-sex IV Only	Both IV	Twin IV Only	Same-sex IV Only
	(1)	(2)	(3)	(4)	(5)	(6)
Second-stage coefficients:						
𝟙(Have three or more children)	-0.038	-0.013	-0.179	-0.109***	-0.144**	-0.087**
	(0.049)	(0.052)	(0.148)	(0.034)	(0.056)	(0.043)
First-stage coefficients:						
Twins	0.918***	0.923***		0.845***	0.861***	
	(0.027)	(0.027)		(0.022)	(0.020)	
Same sex	0.045***		0.046***	0.153***		0.155***
	(0.005)		(0.005)	(0.006)		(0.006)
Kleibergen-Paap Wald rk F-statistic	615.689	1159.208	97.903	1169.191	1874.624	628.230
Kleibergen-Paap rk LM p-value	0.000	0.000	0.000	0.000	0.000	0.000
Anderson-Rubin χ^2 p-value	0.463	0.802	0.224	0.005	0.011	0.046
Hansen J statistics p-value	0.287			0.424		
R^2	0.285	0.285	0.277	0.293	0.291	0.294
Obs.	19,276	19,276	19,276	19,553	19,553	19,553

Notes: Data is from 2016 China Migrants Dynamic Survey (CMDS). All the controls listed in Section 3 are included in the models above. We conduct subsample regressions for sons and daughters with different instrument(s) and a binary family size measure. In Columns (1) & (4), both twinning at second parity and having first two children with the same sex serve as instruments, whereas twinning at second parity serves as the only instrument in the Columns (2) & (5) and having first two children with the same sex as the only instrument in the Columns (4) & (6). The standard errors are clustered at the household level.
***p<.01, **p<.05, *p<.10.

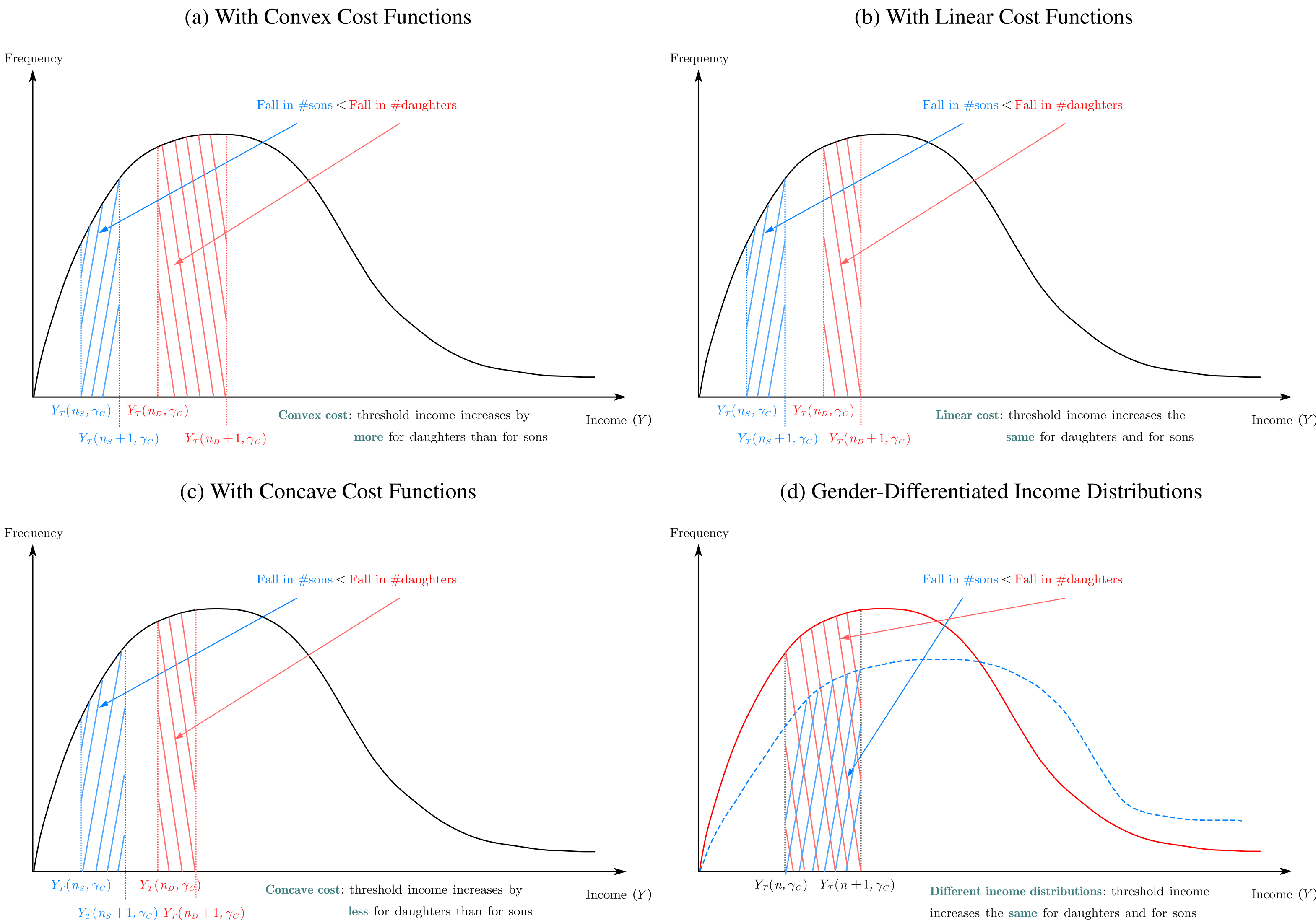


Figure A1: Alternative Education Cost Functions

Notes: Panels (a), (b), and (c) illustrate how changes in the threshold incomes—due to an increase in family size by one unit—for respectively, convex, linear, and concave education cost functions, could lead to greater fall in larger families (n_D) compared to smaller families (n_S) migrating with children, and thus a greater fall in daughters migrating compared to sons. Panel (d) illustrates the case where the threshold incomes are the same for different family sizes but where the income distribution differs by family size.

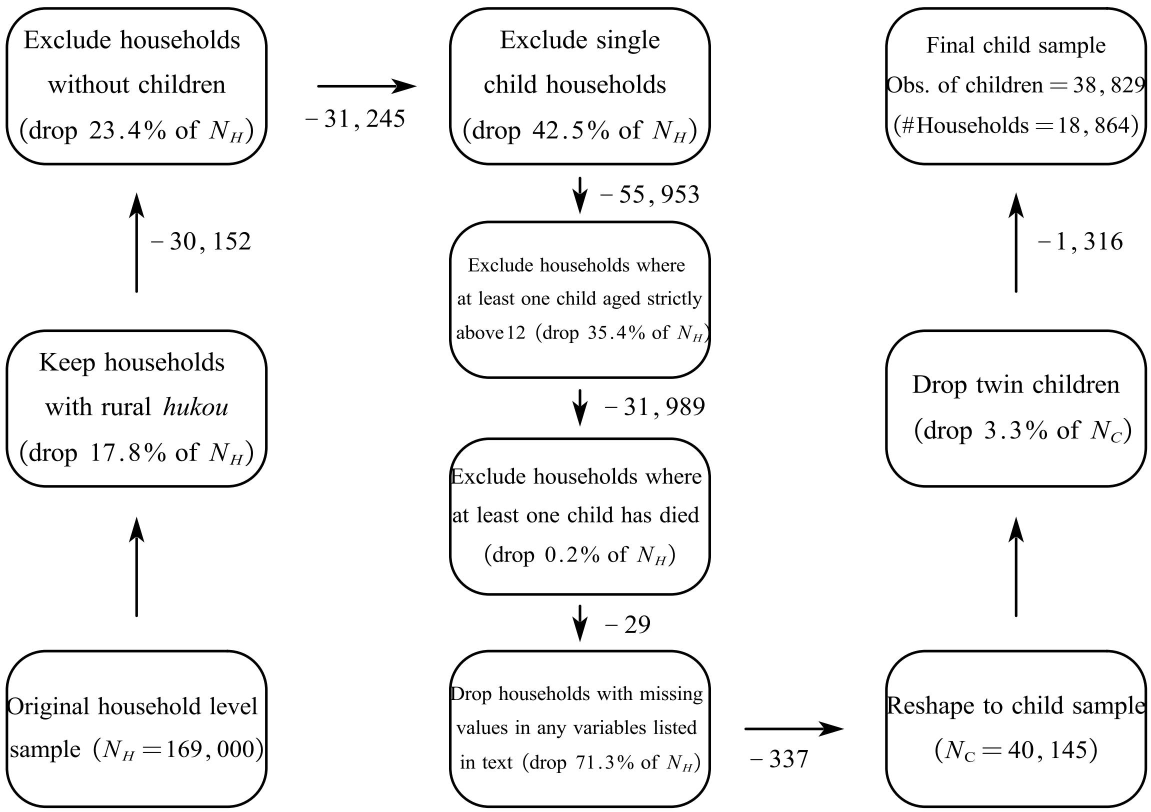


Figure A2: Sample Selection

Notes: There are some overlaps between the different restrictions such that the sum of the excluded proportions may not necessarily add up to the final proportion of observations dropped. All of the proportions are computed when the specific restriction is *solely* imposed on the original household or child level sample (i.e., N_H or N_C).

(a) Marginal Treatment Effects for Sons

(b) Marginal Treatment Effects for Daughters

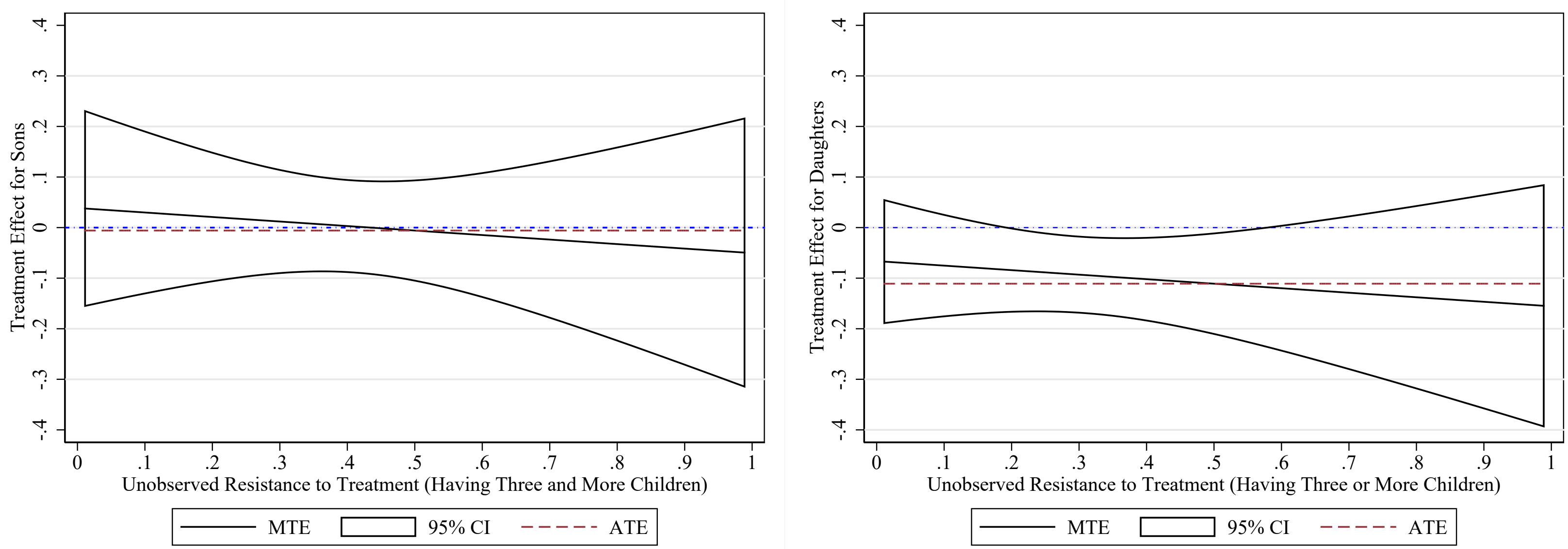


Figure A3: Marginal Treatment Effects

Notes: The figure plots the MTEs from the sub-samples of sons and daughters. All the controls listed in Section 3 are included. The MTE estimation is based on a separate estimation procedure using a linear polynomial specification following Brinch et al. (2017). The horizontal axis in Panel (a) and (b) is the predicted probability of having three or more children after residualizing out all covariates including province fixed effects, that is, V_{ijp}^{D} in our notation. The vertical axis in Panel (a) and (b) is the MTE of having three and more children (rather than two) on the probability of child migration for sons or daughters. Following Mogstad and Torgovitsky (2018), we compute ATE as a weighted average of MTE. All estimations were done via the estimation program provided in Andresen (2018).

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