

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% 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 (Almond et al., 2019; Jayachandran and Kuziemko, 2011). 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 Around 90% of households have two migrant parents while 70% of sons and daughters migrate. The vast majority, 89.4%, of families migrate with either all or no children, girls tend to have more siblings than boys, and larger families are more likely to leave children behind.

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

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

²Rural households faced more relaxed restrictions under the one-child policy (OCP) 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. The OCP was eliminated in 2016 and by 2021 all families in China were allowed to have three children.

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 including 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 effects for a more general population (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.

The remainder of this paper is structured as follows. Section 2 documents the institutional background and discusses related literature as well as key predictions from our model. Section 3 introduces the data, the main twin identification strategy, and empirical models. Section 4 presents the main results on gendered family size trade-offs with child migration and a battery of sensitivity analyses. Section 5 explores how migration restrictions may matter for family size effects. Section 6 discusses how gender-neutral policies may help mitigate gendered family size trade-offs and

³While 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).

offers concluding remarks.

2 Conceptual Framework

2.1 Rural-to-Urban Migration in China

Migration restrictions. 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 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 migration from rural areas to cities since the 1990s. Such RU 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 RU 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).

In recent years, the average quality of migrant schools improved dramatically thanks to government subsidies, although this appears to be specific to cities with sufficiently large financial support (Chen and Feng, 2017). Recent evidence further indicates that the majority of migrant children are now enrolled in public schools. For instance, Huang and Zhang (2023) show that public school enrollment rate of migrant children is greater than 75% in most provinces. 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). In some first-tier cities, such as Shanghai, all public and migrant schools stopped charging tuition fees from 2008 (Chen and Feng, 2017). Nevertheless, given that there

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

could be some 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), who shows that the fraction of tuition fees per child ranges from 0.7% to 14.8% of rural household income in China. 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.

Child migration. Given better access to urban public schools, it is unsurprising that child migration is also substantial at 71 millions or 24% of all children in China (UNICEF et al., 2023). The strong cultural value placed on education has led to the widespread belief among most Chinese parents that providing a better education for their children is a crucial and rationalized goal. Parental perceptions about child well-being is indeed a significant push factor in child migration (UNICEF, 2017). There is also evidence that higher income workers place greater weights on educational and medical resources when choosing destination cities (Wang and Chen, 2019) while migrant parents may also choose their cities of destination based on social mobility opportunities (Wang et al., 2019). Better neighborhoods in turn, can affect intergenerational mobility (Mogstad and Torsvik, 2023).⁵

The children of RU migrants can have access to better educational and health resources compared to their rural counterparts since urban areas tend to be more prosperous than rural areas. The opportunity to gain access to better facilities coupled with the fact that RU migration tends to be long-term—at least 4-5 years for the majority of migrants (Wang et al., 2019)—can help improve migrant children’s human capital (Lin et al., 2021; Wang and Chen, 2019). Chetty and Hendren (2018) documents that children who move at birth to a better neighborhood, tend to have higher income in the longer-term compared to their counterparts who remain in their original neighborhoods or relocate later in life. Meanwhile, Huang and Zhang (2023) find that increased enrollment in urban public schools by migrant children can lead to non-trivial human capital gains, especially for those who come from smaller cities. Using data from the China Education Panel Study (2013-2014) we also find that migrant children tend to have higher standardized test scores and better health outcomes compared to left-behind children (Appendix Table A1).

Moreover, migrant children may benefit from care from their own parents, which can be an important determinant of their future human capital (Del Boca et al., 2014; Yum, 2023). Parental

⁵Migration has long been associated with higher human capital accumulation in a more global context (Dustmann and Glitz, 2011; Dustmann et al., 2010). Whereas children who migrate internationally with parents may fall behind in school due to language barriers (Dustmann and Glitz, 2011), this is not a concern for children who migrate internally as there are no language barriers. Even among those who migrate internationally, migrant children from Confucian societies that value education have been found to eventually outperform natives in the UK (Dustmann et al., 2010).

absence is known to be detrimental to left-behind children and those aged below 12 tend to experience greater adverse effects from parental migration compared to their older counterparts in China (Yue et al., 2020; Zhang et al., 2014). The benefits of child RU migration may thus be particularly salient for younger children who get to migrate with their parents (Heckman, 2006).

2.2 Family Size and Parental Investments in Children

Family size trade-offs. Despite the potential benefits from child migration, parents may be resource constrained and unable to migrate with children. QQ trade-offs posit that an increase in the quantity of children would lead to lower investment in the quality of an average child (Becker and Lewis, 1973). The literature has found evidence of negative associations between family size and child quality as measured by education and health outcomes (Bagger et al., 2021; Li et al., 2008; Liu, 2014; Tan, 2019). Nevertheless, family size effects may be very small (Angrist et al., 2010; Black et al., 2005) or even positive (Guo et al., 2020; Qian, 2018).

We study the effects of family size on a novel dimension and direct measure of parental investment in children. Whereas past literature focus on quality outcomes (e.g., education attainment), child migration could be perceived as parental investment that contributes to child quality. Child quality outcomes may be driven not just by parental investments but also by other factors such as a child's own effort (Mogstad and Torsvik, 2023). Sibship size may have opposite effects on parental investment and child effort: while parents who have more children may invest less in each child, children with many siblings may invest more in themselves due to stronger sibling rivalry. The net effect of family size on child quality may thus be small or even positive, not because parental investments are not negatively affected by family size but due to the mediating effects of a child's own effort. While a few studies have used expenditure on education as a direct measure of parental investment to test for QQ trade-offs (Chen, 2020; Dang and Rogers, 2016), we are not aware of any empirical studies that has tested the presence of a link between sibship size and time investments from parents. The latter has been found to be far more effective than in generating child human capital, especially when children are young (Del Boca et al., 2014; Yum, 2023). Our study thus offers a set of new insights on family size trade-offs with child RU migration, which captures both money and time dimensions. To the best of our knowledge, this is the first paper that studies the trade-offs between family size and migration of preschool and primary school age children.

Family size and child gender. There is abundant evidence that parents tend to directly invest more in sons than in daughters such that the *levels* of investment differ in many societies (Jayachandran, 2015; Jayachandran and Pande, 2017). Similarly, there is evidence that boys are more likely to be brought to cities and that parents tend to have higher expectations for boys than for

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, according to the gender of the child.⁶

Specifically, daughters tend to have more siblings compared to sons (Almond et al., 2019; Basu and De Jong, 2010; Jayachandran and Kuziemko, 2011). 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: although there may be gender-differentiated family size trade-offs, gender neutral relaxation of migrant children’s education restrictions—for example, in the form of education subsidies—may be desirable. Specifically, relaxing migration restrictions may encourage larger families to migrate with their children, thereby, mitigating the gendered trade-offs.

2.3 A Model of Parental Investment in Child Migration

We formalize the above ideas in a stylized model of son preference over child quantity (but not quality) in Appendix A. In the main model of son preference over quantity, we show that even if parents were to equally care about sons and daughters—such that male and female children with the same number of siblings are equally likely to migrate with parents, daughters may still be more likely to be left-behind by virtue of them being born in larger families compared to sons. Thus, son preference has an indirect effect on parental investment in daughters’ migration due to their larger sibship size.⁷ The main predictions of the models are:

Prediction 1 (Gender.) *Conditional on the same 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.*

⁶The findings from the literature on gender differences in the effect of family size on child quality tend to be mixed (Chen, 2020; Chen et al., 2019; Li et al., 2008). As discussed above, child outcomes may capture not only parental investments in children but also other potential mediators, which may contribute to the mixed findings.

⁷In 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.

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 increasing rate with n (Prediction 3).⁸ 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

We use data from the China Migrants Dynamic Survey (CMDS) conducted by the Migrant Population Service Center of the National Health Commission. The CMDS adopts a stratified, multi-stage probability-proportional-to-size sampling method, resulting in a large-scale, nationally representative, and random sample of domestic migrants. The survey has been conducted annually from 2009 to 2018, resulting in a series of repeated cross-sections (Wang et al., 2021). The CMDS provides rich data on the demographic and socio-economic characteristics of migrant respondents, their spouses (if any), and their children. 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

⁸The 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).

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 as of May 2016, when the survey was conducted. It covers $N_H = 169,000$ households in total (including those without children) in all provinces in mainland China and excludes special administrative regions.

Our sample selection is detailed in Appendix Figure A2. Since we are interested in RU migration in multiple children families, we drop households with urban *Hukou* (17.8% of N_H), childless households (23.4% of N_H), and single child households (42.5% of N_H). This leaves us with 51,650 rural migrant households with at least two children in the sample. Note that under China’s family planning policies, rural families in our setting were restricted only from the third birth onward (see Footnote 2). The majority (57.5%) of households with a rural migrant parent have at least two children in the CMDS. To address potential selection on family size, we employ selection correction methods à la Heckman (1979) in sensitivity analyses and find that the results are robust.⁹

We further drop households with any child strictly older than 12 (35.4% of N_H), thereby capturing households with only pre-school (0-5) and primary school (6-12) age children, which leaves us with 19,661 households. As documented in Section 2, left-behind children aged below 12 tend to be the most vulnerable to the adverse effects stemming from parental migration, while that age group tends to experience the highest returns to education and parental time investment. Moreover, 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.¹⁰ We, therefore, focus our analyses on the migration of children aged 0-12. For the sake of comparison, we also report separate analyses for the subsamples of pre-school and primary school age children, as well as additional analyses on the sample of older children aged 13 to 18.¹¹

After excluding a handful of households with deceased children or missing values, we reshape the household-level sample at the child level to get one observation per child but several observations per household. This gives us 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-level sample while retaining the indicators for the presence of twins in the family for the remaining

⁹Excluding 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.

¹⁰The Law of Population and Family Planning was implemented from September 2002 to April 2004, while the *Guan Ai Nv Hai Xing Dong* Program, which aimed to alleviate son-biased parental behaviors, was put in place in 2003.

¹¹We note that the migration decision for older children may potentially be confounded by their own labor supply decisions 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 high school in the region of origin of their *Hukou* (Chen and Feng, 2017; UNESCO, 2019).

non-twin children.¹² We exclude twin children because twins are arguably different from singletons due to the lack of birth spacing between twins (Bagger et al., 2021).

Our analytic sample comprises of 38,829 children from 18,864 households. We present descriptive statistics for demographic and socioeconomic characteristics from the analytic child-level sample in the Table 1. Approximately 50% of children are male while the average family size is 2.11 children. Children are on average 5.95 years old and 53% of them are of primary school age. Mothers and fathers are in their early thirties on average and 63-64% of children have parents with middle school education. The vast majority, 90%, of children have two migrant parents who migrated to the same destination city while 6% had grandparents (and/or great-grandparents) who migrated with the parents. 53% of parental migration is inter-province while 30% of parental migration is inter-prefecture. The average duration of migration of parents is around 5.22 years and 61% would be willing to settle in the destination city.

Child migration. We now turn to some descriptive statistics on child migration from the analytic child-level sample. 69.5% of sons and 69.1% of daughters migrate with parents, suggesting that male and female children have similar migration probabilities on average. However, there is substantial variation in these proportions by family size. 89.7% of children were in families with two children and 10.3% of children were in families with three or more children. In families with two children, the proportion of sons and daughters who migrate are very similar and not statistically significantly different: 69.9% of sons and 69.7% of daughters migrate with parents. Conversely, in families with three or more children, the proportion of sons who migrate is 66.7% while the proportion of daughters who migrate is 63.2%. The gender difference is statistically significant at the 5% level. Two observations arise from comparing families with two children and families with three or more children. 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 come from larger families. In the analytic child-level sample, 18.7% of children are from only-son households with 2.0 sons on average, 18.1% of children are from only-daughter households with 2.1 daughters on average, and 63.1% of children come from mixed-gender households with 1.04 sons and 1.11 daughters on average. Overall, daughters' families have 2.13 children while sons' families have 2.09 children on average. The difference in daughters' and sons' sibship sizes is statistically significant at the 1% level. The generally larger family size for

¹² The twinning rate in our sample—defined as the number of twin children over the total number of children in all households (including single child households)—is 1.71% (1,316 twin children out of 76,911 children), which is quite similar to that reported in previous studies (Li et al., 2008).

daughters compared to sons is consistent with son-prefering fertility stopping rule where families tend to have more children as they attempt to get a son (Almond et al., 2019; Jayachandran and Kuziemko, 2011).

Additionally, 64% of children are from a household that migrates with all children while 25.4% of children are from a household without any migrant child. Around 10.6% of children come from a household with partial child migration, where some children migrate while others are left-behind. Among children from partial migrant households, around 52.4% and 47.6% of sons and daughters are brought to an urban area, respectively. The difference between sons and daughters is statistically significant at the 1% level and is driven predominantly by mixed-sex households.¹³ This is consistent with son preference in mixed-gender families with partial child migration, whereby parents are more inclined to invest in sons than in daughters. Nevertheless, we note that children from such families make up a relatively small proportion, 6.6%, of the child-level sample.¹⁴

Overall, the descriptive statistics suggest that there may be a negative correlation between the probability that a child migrates and family size. Moreover, since daughters are more likely to come from larger families and larger families are more inclined to leave some or all of their children behind, the negative family size-trade offs may be stronger for daughters. These align with our predictions from Section 2. We next outline the empirical strategy to identify the causal effects of family size on sons' and daughters' migration.

3.2 Empirical Specifications

Baseline specification. The baseline ordinary least squares (OLS) specification is given by:

$$M_{ijp} = \beta_1 + \beta_2 FS_{jp} + \beta_3 SA_{ijp} + \beta_4 RelBO_{ijp} + \beta_5 Son_{ijp} + \mathbf{X}'_{ijp} \beta_6 + \mu_p + \varepsilon_{ijp}, \quad (1)$$

where M_{ijp} is dummy variable that takes unity if child i in household j located in destination province p migrated with parents, and zero otherwise. The error term ε_{ijp} is clustered at the household level to account for potential correlation across children in the same family.

In line with previous literature (Angrist et al., 2010; Bagger et al., 2021; Black et al., 2005; Li et al., 2008; Rosenzweig and Wolpin, 1980), we define family size, FS_{jp} , as the number of children in a family. We also include an indicator of whether the child is of primary school age SA_{ijp} (on top of controlling for child age as a continuous measure in \mathbf{X}_{ijp}). We further control for relative

¹³ Among households with partial child migration, the proportion of migrating sons from only-son households is 50.3% while the proportion of migrating daughters from only-daughter households is 50%. Conversely, the proportions of sons or daughters who migrate are 53.7% and 46.2%, respectively, for mixed-gender families.

¹⁴ 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.

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). We also include an indicator of whether the child is male, Son_{ijp} .

\mathbf{X}_{ijp} is a vector of additional control variables including child and family level characteristics: second order polynomial of child age, maternal age at second birth, second order polynomial in maternal age, second order polynomial in paternal age, maternal education indicators for middle school, high school, and college and above, paternal education indicators for middle school, high school, and college and above, both parents migrant (yes/no), any grandparent migrant (yes/no), duration of migration for the main parent respondent in years, willing to settle down (yes/no), indicator for within-province migration, and indicator for within-prefecture migration. We also include province fixed effects, μ_p .¹⁵

A major identification challenge relates to the potential endogeneity of family size. For example, parents who have a preference for family cohesion may want to have fewer children and also be more willing to migrate with children. Thus, a negative association between the number of children and child migration may potentially be driven by unobserved family preferences, such that OLS estimates of family size effects, β_2 , would be biased downwards (i.e., the magnitudes of negative family size effects would be larger). Alternatively, parents who expect children to provide old age support may choose to have a larger family and also be more willing to migrate with children. In this case, OLS estimates of family size effects, β_2 , would be biased upwards (i.e., the magnitudes of negative family size effects would be smaller).

Instrumental variable strategy. To identify the effects of family size on child migration, one needs an exogenous variation in fertility, which does not alter child migration status. The occurrence of twins is usually regarded as good as random and has often been used as an instrumental variable (IV) for family size in published literature (Bagger et al., 2021; Black et al., 2005; Chen et al., 2019; Oliveira, 2016). We therefore use twinning at the second parity as an IV for the number of children and apply two-stage least squares (2SLS).

The first stage of 2SLS for the baseline model is given by:

$$FS_{jp} = \alpha_1 + \alpha_2 Twin_{jp} + \alpha_3 SA_{ijp} + \alpha_4 RelBO_{ijp} + \alpha_5 Son_{ijp} + \mathbf{X}_{ijp}' \alpha_6 + \mu_p + \eta_{ijp}, \quad (2)$$

where $Twin_{jp}$ 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.¹⁶

The second stage uses the predicted \widetilde{FS}_{jp} from the first stage and is given by:

¹⁵Results 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.

¹⁶The twinning rate of second-born children is 0.40% as per the definition in Footnote 12.

$$M_{ijp} = \beta_1 + \beta_2 \widetilde{FS}_{jp} + \beta_3 SA_{ijp} + \beta_4 RelBO_{ijp} + \beta_5 Son_{ijp} + \mathbf{X}'_{ijp} \beta_6 + \mu_p + \varepsilon_{ijp}. \quad (3)$$

Our twin IV strategy identifies the weighted average causal response from compliers, that is, families induced by twinning to switch from having fewer than 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). Thus, 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 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 also use an indicator for the presence of twins at any parity as an alternative IV and find the results to be very similar.¹⁷

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 $Twin_{jp}$ and FS_{jp} —in Appendix Table A4. When family size is not controlled for, there is a negative and significant association between $Twin_{jp}$ and the probability that a child migrates. However, the coefficient of $Twin_{jp}$ become statistically insignificant at the 5% level once family size is controlled for, suggesting that the corre-

¹⁷The twinning rate at any parity is 1.71% as per the definition in Footnote 12. The proportion of children in the analytic child-level sample, who have a twin in their families is 0.63%. Results are available upon request.

lations 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.¹⁸ 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 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).

Interaction analyses by child gender. We extend the 2SLS model to allow for heterogeneous effects of family size, schooling age, and birth order, by child gender. The first stage is given by:

$$K_{jp} = \delta_1 + \delta_2 Twin_{jp} + \delta_3 Twin_{jp} \times Son_{ijp} + \delta_4 SA_{ijp} + \delta_5 SA_{ijp} \times Son_{ijp} + \delta_6 RelBO_{ijp} + \delta_7 RelBO_{ijp} \times Son_{ijp} + \delta_8 Son_{ijt} + \mathbf{X}'_{ijp} \delta_9 + \mu_p + \eta_{ijp}, \quad (4)$$

where $K_{jp} = \{FS_{jp}, FS_{jp} \times Son_{ijp}\}$. Separate first-stage regressions are run with FS_{jp} and $(FS_{jp} \times Son_{ijp})$ as dependent variables. The second stage is given by:

$$M_{ijp} = \gamma_1 + \gamma_2 \widetilde{FS_{jp}} + \gamma_3 \widetilde{FS_{jp} \times Son_{ijp}} + \gamma_4 SA_{ijp} + \gamma_5 SA_{ijp} \times Son_{ijp} + \gamma_6 RelBO_{ijp} + \gamma_7 RelBO_{ijp} \times Son_{ijp} + \gamma_8 Son_{ijt} + \mathbf{X}'_{ijp} \gamma_9 + \mu_p + \varepsilon_{ijp}. \quad (5)$$

From model (5), the marginal effect (ME) of family size on daughters is given by γ_2 while the marginal effects of family size on sons is given by $(\gamma_2 + \gamma_3)$. Similarly, γ_4 captures the marginal effect of being of primary school age for a daughter while $(\gamma_4 + \gamma_5)$ captures the marginal effect of being of primary school age for a son. Finally, the marginal effect of relative birth order for daughters is given by γ_6 while the marginal effect of relative birth order for sons is given by $(\gamma_6 + \gamma_7)$. In sensitivity analyses, we also estimate non-linear models using control function approach and adjust the marginal effects accordingly. Note that we only include the three interaction terms in our main specification, but including interaction terms between child gender and all controls—equivalent to sub-sample regressions by child gender as depicted in Figure 2—yields very similar results.

¹⁸Whereas 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).

4 Results

4.1 Gender and Family Size Trade-offs in Child Migration

The baseline results are presented in Table 2. OLS results in Columns (1) and (3) suggest that there is no economically or statistically significant association between child gender and the probability that the child migrates. Conversely, from Column (1) there is a negative association of 2.5 percentage points between the number of children and the probability that a child migrates ($p < .01$). From Column (3), this negative association seems to be predominantly driven by daughters. In particular, having one more sibling is associated with a 3.1 percentage points decrease in the probability of migration for daughters ($p < .01$) but with an insignificant 1.2 percentage points decrease for sons. Nevertheless, we reckon that the OLS estimates may suffer from the endogeneity issues discussed above and thus focus on the 2SLS estimates.

Columns (2) and (4) of Table 2 report the coefficients from the first stage regressions (2) and (4), respectively. The positive associations between the presence of twins at second birth, $Twin_{jp}$, and family size, FS_{jp} , are statistically significant at the 1% level, which is consistent with the fact that family size increases upon the occurrence of twins. Similarly, the Kleibergen-Paap Wald rk F -statistics are reasonably large and greater than the rule-of-thumb thresholds, that is, 10 and 104.7 (Lee et al., 2022), suggesting that the instruments are not weak. The Kleibergen-Paap rk LM test also always strongly rejects the null hypothesis of underidentification while the Anderson-Rubin χ^2 test—which is robust to weak instruments—strongly rejects the null hypothesis that the coefficients of the endogenous variables (family size FS_{jp} and its interaction with Son_{ijp}) are jointly equal to zero. Altogether, the tests provide greater confidence in our twin-based instruments.

Gender. Whereas the coefficient of Son from the second stage of the 2SLS in Column (4), seems to indicate that Son_{ijp} has a negative coefficient, the marginal effect of being a son rather than a daughter is still not statistically significantly different from zero. To see this, using the notation from equation (5), 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 value of variable $var = FS_{jp}, SA_{ijp}, RelBO_{ijp}$. The ME of being a son instead of a daughter is -0.002 and is statistically insignificant at the 10% level. Conditional on family size, we thus do not find evidence of gender disparity in terms of the average levels of child migration, which is consistent with Prediction 1.

Family size. From the second stage of the 2SLS model, reported in Column (2) of Table 2, having one more sibling is associated with a 7.9 percentage points decrease in the probability that a child migrates ($p < .05$). This is consistent with Prediction 2 and indicates the presence of a trade-off between family size and child migration. Compared to the 2SLS estimates, OLS estimates seem to

be biased upwards (i.e., the magnitude of the negative family size effect is smaller for OLS than for 2SLS), which is consistent with the presence of unobserved factors that may potentially drive both family size and child migration. As discussed in Section 3, one such factor may be parental desire to receive old age support from children, which may incentivize parents to have more children and be more likely to migrate with children.

Family size and gender. Similar to the inferences from OLS, the negative family size effects from 2SLS seem to be driven predominantly by daughters. From Column (4) of Table 2, a unit increase in family size leads to a 12.5 percentage points decrease in the probability that a daughter migrates ($p < .05$). Conversely, a unit increase in family size has no effect on sons both in terms of magnitude and in terms of statistical insignificance. The gender difference in family size effects on child migration is 11.1 percentage points ($p < .10$). 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.

4.2 Do Schooling Age and Birth Order Matter?

Schooling age. We examine the effects of schooling age on the probability that a child migrates. From Column (2) of Table 2, children who are of primary school age are 3.2 percentage points more likely to migrate with parents compared to children who are of pre-school age. Moreover, from Column (4), such effect seems to be more pertinent for sons than for daughters: sons of primary school age are 4.8 percentage points more likely to migrate than sons of pre-school age ($p < .01$) while daughters of primary school age are only 1.8 percentage points more likely to migrate than daughters of pre-school age ($p < .05$). The difference in the schooling age effects between sons and daughters is statistically significant at the 1% level. The results are consistent with the fact that parents may be more willing to invest in school age children—especially sons—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 Table 3. The analyses from the extended sample confirm that the negative family size effects on daughters' migration are the most relevant for primary school age children. Conversely, the migration of older children aged may be clouded by their own labor supply decision or by barriers to higher education (see Footnote 11).

Birth order. We next turn to the effects of birth order on the probability that a child migrates. From Column (2) of Table 2, we find that the youngest child is less likely to migrate than the oldest child. This is consistent with evidence of lower parental investments in children or lower quality

children at higher parities (Bagger et al., 2021; Black et al., 2005; Zhang et al., 2023). Moreover, in Column (4), there is evidence that the negative association between relative birth order and child migration is predominantly driven by daughters. The probability that a daughter migrates decreases by 4.2 percentage points when relative birth order increases by one unit ($p < .01$) while the probability that a son migrates decreases by 1.8 percentage points when relative birth order increases by one unit. The latter effect is not statistically significant at the 10% level. Our findings that youngest daughters tend to be less likely to migrate and therefore more likely to be left-behind is consistent with the existence of son preference complemented by family size trade-offs in China.

4.3 Robustness Checks

We now discuss several sensitivity analyses including (i) bounding family size effects: accounting for selection on unobservables using selection on observables and relaxing the exclusion restriction, (ii) sample selection: Heckman selection correction, limiting the sample to firstborn children, and to children who were born outside of the current place of residence, (iii) additional specifications: two-step estimation and control function approach, and (iv) extrapolating from LATE to estimate ATE.

4.3.1 Bounding Family Size Effects

Accounting for selection on unobservables using selection on observables. We follow Oster (2019) to bound the family size effects using selection on observed variables as a guide to selection on unobserved variables in the OLS model (1). 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. To derive the bias-adjusted treatment effect, we adopt two recommended assumptions from Oster (2019). The first assumption is that there is equal selection on observables and unobservables, that is, the observables and unobservables are equally related to the treatment, which is sibship size in our case. The second assumption is that 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 (1). Under these two assumptions, the identified set lies between the OLS estimate from (1), $\hat{\beta}_2^{OLS}$, 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). From the figure, the identified sets of β_2 are fairly tight and are consistent with our inference that while the probability that a daughter migrates is negatively associated with sibship size, the association between the probability that a son migrates and family size is very small and close to zero.

Relaxing the exclusion restriction. For the $Twin_{jp}$ instrument to be valid in the 2SLS model (2)-(3), it has to satisfy the exclusion restriction: $Twin_{jp} \perp \varepsilon_{ijp}$. However, the exclusion restriction may be violated because twins have lower average birth weight than singletons (Rosenzweig and Zhang, 2009). Parents may thus reallocate resources from twins to singletons, which would dampen the (negative) associations between family size and parental investments in children. We thus relax the assumption that $Twin_{jp} \perp \varepsilon_{ijp}$ and estimate bounds on family size effects to provide more compelling evidence of family size trade-offs on child migration (Bhalotra and Clarke, 2020). In particular, we follow the method proposed by Nevo and Rosen (2012), which allows for 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 the 95% confidence intervals from OLS and 2SLS estimates. From the figure, the presence of negative family size effects on child migration are confirmed for daughters but not for sons. Note that Nevo and Rosen (2012) argue that the effects are always bounded by $\hat{\beta}_2^{OLS}$, $\hat{\beta}_2^{2SLS}$ or $\hat{\beta}_2^{NR}$. Given that all estimates including the 95% confidence intervals are always negative for daughters, the above conjectures on the direction of the various correlations effectively do not matter for the purpose of confirming the presence of negative family size effects for daughters. Our results and inference that family size trade-offs matter for daughters but not for sons are thus robust to the relaxation of the exclusion restriction.

4.3.2 Sample Selection

Sample selection correction. As discussed in Section 3, we limit our analytical sample to families with at least two children. Although rural families in our setting were restricted only on the third birth onward, there may be some sample selection. For instance, those with a firstborn daughter may be more likely to choose to give birth to a second child compared to those with a firstborn son. To address such concern, we perform sample selection correction à la Heckman (1979) using the gender composition of firstborn twins as an instrument (Farbmacher et al., 2018). The detailed empirical strategy is elaborated in Appendix Section B.1. From Columns (1) and (2) of Appendix Table A5, the results are robust to controlling for an inverse Mills ratio in our analyses.

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 come from an endogenously selected sample if fertility is endogenous. We thus follow Black et al. (2005) and employ a sample truncation specification to re-estimate the family size effects. In our context, we re-estimate the 2SLS models on the sample of firstborn 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 Appendix Table A6, the family size effects are similar to those from the main models in Columns (2) and (4) of 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 A5).

Children born outside of the destination city. Our measure of child migration, M_{ijp} , 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. Note that although children may be born in cities, migrant parents would still need to register the children in the village of origin such that the child would have the same rural *Hukou* as the parents (An et al., 2024). We refine the sample by considering only children who were born outside the destination city (68.4% of children in the main sample). From Columns (3) and (4) of Appendix Table A6, the family size effects are once again comparable to those from the main models in Columns (2) and (4) of Table 2.

4.3.3 Alternative Specifications

Two-step estimation. It is well-known that birth order configuration 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. Note that family size and other household level predictors (including unobserved preferences) would be differenced out in the MFE models. We then predict child migration net of birth order effects. In the second step, we apply 2SLS as described above 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 Appendix Table A7, the results and inferences from the two-step estimation are similar to those from the main specification 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 Appendix Table A7, the marginal effects are very similar to those from the linear models, which boosts confidence in the robustness of the 2SLS estimates.

4.3.4 Moving Beyond LATE and Estimating ATE

Finally, as mentioned above, our estimates recover the LATE from the relatively small set of compliers. As a guide to the relevance of our results to a more general population, we extrapolate from the LATE to estimate ATE (Brinch et al., 2017). See Appendix Section B.2 for the methodology. The ATE for sons is negligible (-0.006 , $p > 0.1$) while the ATE for daughters is -0.111 ($p < .05$), which is once again consistent with gendered family size trade-offs.

5 Does the Stringency of *Hukou* Policies Matter?

Family size trade-offs often occur due to tight budget constraints for poorer families (Becker and Lewis, 1973; Rosenzweig and Wolpin, 1980). The presence of additional constraints in the form of migration restrictions—due to for example, a lack of access to facilities in cities arising from the *Hukou* system—may exacerbate matters. 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* by investment in each prefectural city, housing property purchase, high-end employment, 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 composite measure as the cutoff to split the sample into strict prefectures and relaxed prefectures to perform heterogeneity analyses using 2SLS.¹⁹

¹⁹Utilizing the national mean of the index as the cutoff yielded similar results. The results were also robust to controlling for Heckman style inverse Mills ratios to account for potential selection into destination cities. To do so,

From Table 4, 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).²⁰

6 Conclusion

This study is the first to examine the trade-offs between family size and a novel and direct measure of parental investment in children: child migration, which entails both time and money dimensions. We highlight an interesting interaction between the indirect effect of son preference on daughters' migration and the presence of migration restrictions as additional constraints that result in family size trade-offs. The salience of such interaction suggests that gender-neutral relaxation of migration policies may help mitigate gendered trade-offs in child migration.

Our findings demonstrate the existence of important trade-offs between the migration of daughters and sibship size. A one unit increase in the number of children in the family leads to a 12.5 percentage points decrease in the probability that a daughter migrates. Conversely, we do not find evidence of any trade-off between the probability that a son migrates and sibship size. Moreover, primary school age children—especially sons—are more likely to migrate compared to preschool age sons, suggesting that investment in young children's schooling may be a driving force behind child migration. Family size trade-offs for daughters are also more pertinent in provinces that have more stringent restrictions on migrants, highlighting the role of such restrictions as additional

we used the interaction between the share of migrants from the same province and the schooling year of the main respondent, as an additional instrument for the selection equation. Results are available upon request.

²⁰Rural migrant families in relaxed cities may potentially be a selected sample. In particular, given that we restrict the sample to those with a rural *Hukou*, we may miss out on those who changed their *Hukou* from rural to urban. Although the process of changing one's *Hukou* is usually costly and still intensively restricted (Gao et al., 2023; Wang and Chen, 2019), those who migrated to relaxed areas may be better able to change their *Hukou* status, especially if the parents are of higher ability. If that is the case, then the remaining RU migrants from relaxed cities are more likely to be of lower income (An et al., 2024). However, in such a scenario, we would expect family size trade-offs to be even more pertinent in relaxed cities because such parents are likely to be more resource constrained. Given that we observe the opposite, we do not believe that changes in *Hukou* status are driving the near null effects in relaxed cities but rather that family size trade-offs are more pertinent in strict cities due to more restrictive access to facilities.

constraints that parents face when deciding whether to bring their children to cities with them.

The family size trade-offs for daughters arise primarily from the fact that girls tend to come from larger families (possibly due to son-preferring fertility stopping rule) rather than from allocative bias (where girls are explicitly discriminated against within the sibship). In fact, we do not find any evidence that gender *per se* affect the levels of child migration. Rather, parents with smaller families are more likely to bring all children with them to cities while parents with larger families are more likely to leave all children behind. As daughters tend to have more siblings, they are thus 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 particularly 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. In a setting where the marginal returns to female investment are higher than those of male investment (Ho, 2019; Schultz, 2002), reducing family size-trade offs for daughters can in turn, help improve family welfare and boost economy growth. Improved access to education by migrant children is even more salient in the light of the recent relaxation of fertility restrictions in China (see Footnote 2). If such relaxation leads to a greater number of children without a corresponding relaxation of migrant restrictions, there may be more children, especially daughters, left-behind. Our study thus highlights the importance of gender-neutral policies that relax migration restrictions, such as education subsidies, so that migrant children may stay with their parents in cities and access better quality education.

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Tables

Table 1: Demographic and Socio-Economic Characteristics of Children

	Overall		Daughters		Sons	
	Mean/Prop. (1)	S.D. (2)	Mean/Prop. (3)	S.D. (4)	Mean/Prop. (5)	S.D. (6)
<i>Panel A: Child characteristics</i>						
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
<i>Panel B: Parental characteristics</i>						
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
<i>Panel C: Household characteristics</i>						
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 (1)	2SLS (2)	OLS (3)	2SLS (4)
No. of children	-0.025*** (0.009)	-0.079** (0.036)	-0.031*** (0.011)	-0.125** (0.051)
No. of children \times Son			0.020* (0.010)	0.111* (0.066)
Primary school age	0.032*** (0.007)	0.032*** (0.007)	0.013 (0.008)	0.018** (0.009)
Primary school age \times Son			0.038*** (0.009)	0.030*** (0.010)
Relative birth order	-0.017*** (0.005)	-0.031*** (0.011)	-0.030*** (0.008)	-0.042*** (0.011)
Relative birth order \times Son			0.027*** (0.010)	0.024** (0.010)
Son	-0.000 (0.004)	-0.002 (0.004)	-0.075*** (0.024)	-0.265* (0.137)
<i>Marginal effects for sons</i>				
No. of children			-0.012 (0.011)	-0.013 (0.044)
Primary school age			0.051*** (0.008)	0.048*** (0.009)
Relative birth order			-0.003 (0.007)	-0.018 (0.012)
<i>First-stage coefficients</i>				
<i>FS : Twins</i>		0.999*** (0.038)		0.960*** (0.043)
<i>FS : Twins \times Son</i>				0.103** (0.048)
<i>FS \times Son : Twins</i>				-0.009 (0.010)
<i>FS \times Son : Twins \times Son</i>				1.078*** (0.052)
Kleibergen-Paap Wald rk <i>F</i> -statistic		706.753		242.008
Kleibergen-Paap rk LM <i>p</i> -value		0.000		0.000
Anderson-Rubin χ^2 <i>p</i> -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: 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 (0.083)	-0.052 (0.116)	-0.103*** (0.039)	-0.140** (0.055)	0.025 (0.043)	0.018 (0.053)
No. of children \times Son		0.166 (0.133)		0.092 (0.076)		0.022 (0.087)
Relative birth order	-0.012 (0.047)	-0.009 (0.045)	-0.020** (0.010)	-0.027** (0.013)	-0.020 (0.013)	-0.005 (0.019)
Relative birth order \times Son		0.022 (0.015)		0.010 (0.015)		-0.023 (0.023)
Son	-0.009* (0.006)	-0.373 (0.278)	0.004 (0.006)	-0.194 (0.161)	0.037*** (0.009)	-0.007 (0.187)
<i>Marginal effects for sons</i>						
No. of children		0.113 (0.088)		-0.047 (0.053)		0.040 (0.070)
Relative birth order		0.012 (0.043)		-0.017 (0.013)		-0.027* (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 4: Heterogeneities of Stringentness of *Hukou* Policies in Destination

	Relaxed (1)	Strict (2)	(1)-(2) (3)	Relaxed (4)	Strict (5)	(4)-(5) (6)
No. of children	-0.029 (0.057)	-0.171*** (0.066)	0.142 (0.087)	-0.004 (0.082)	-0.263*** (0.090)	0.259** (0.118)
No. of children \times Son				-0.055 (0.103)	0.240* (0.130)	-0.295* (0.173)
Primary school age	0.035*** (0.012)	0.036*** (0.013)	-0.001 (0.018)	0.017 (0.014)	0.018 (0.016)	-0.002 (0.022)
Primary school age \times Son				0.036** (0.017)	0.034* (0.018)	0.002 (0.023)
Relative birth order	-0.020 (0.016)	-0.044** (0.018)	0.024 (0.026)	-0.032* (0.017)	-0.054*** (0.020)	0.021 (0.029)
Relative birth order \times Son				0.023 (0.017)	0.026 (0.020)	-0.004 (0.026)
Son	0.005 (0.006)	-0.010 (0.007)	0.016* (0.009)	0.091 (0.214)	-0.542** (0.268)	0.633* (0.359)
<i>Marginal effects for sons</i>						
No. of children				-0.059 (0.068)	-0.023 (0.090)	-0.036 (0.127)
Primary school age				0.052*** (0.014)	0.052*** (0.016)	0.000 (0.020)
Relative birth order				-0.010 (0.019)	-0.027 (0.020)	0.018 (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.

*** $p < .01$, ** $p < .05$, * $p < .10$.

Figures

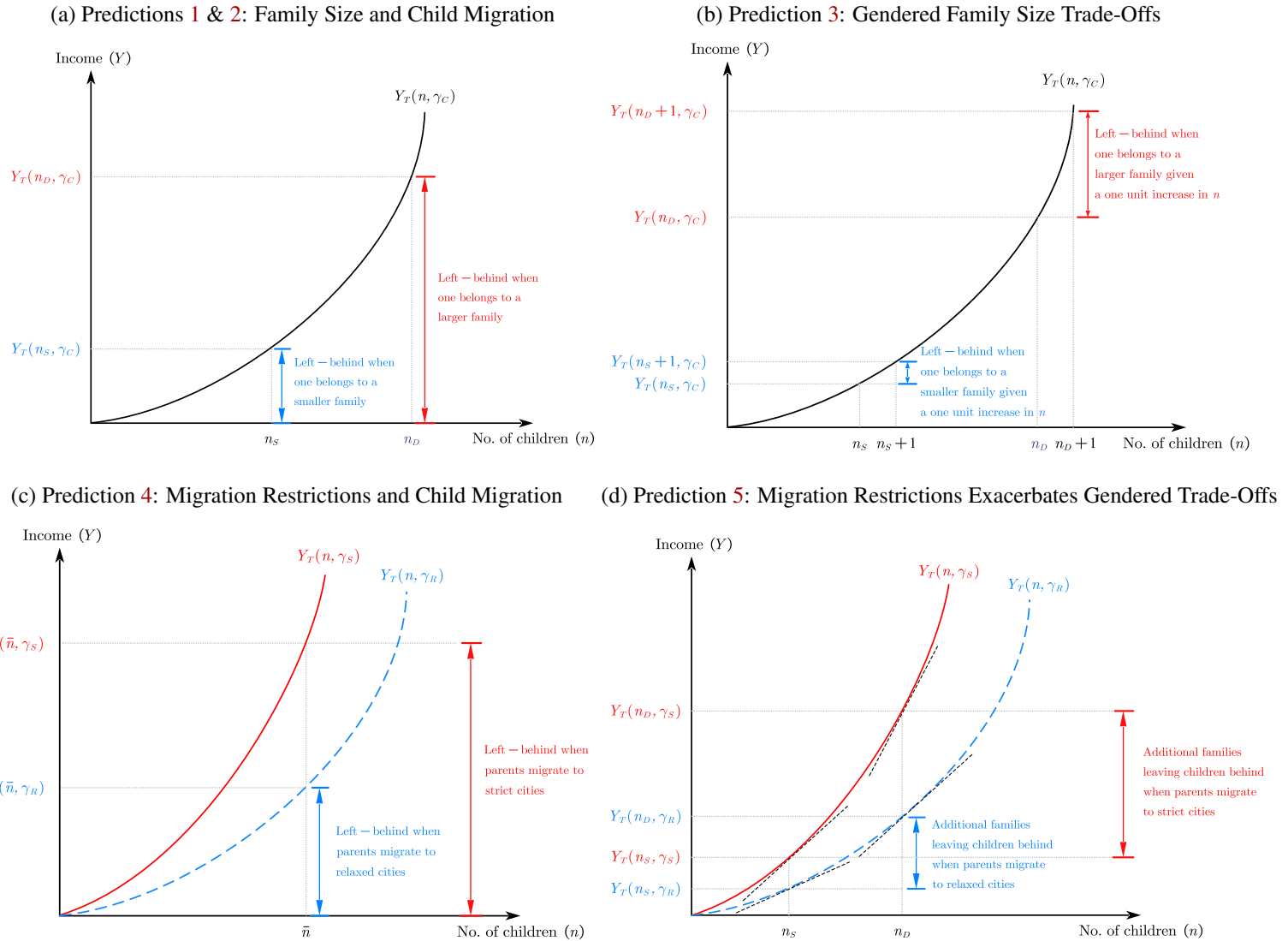


Figure 1: Model 1 – Threshold Incomes when Girls Have More Siblings than Boys

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

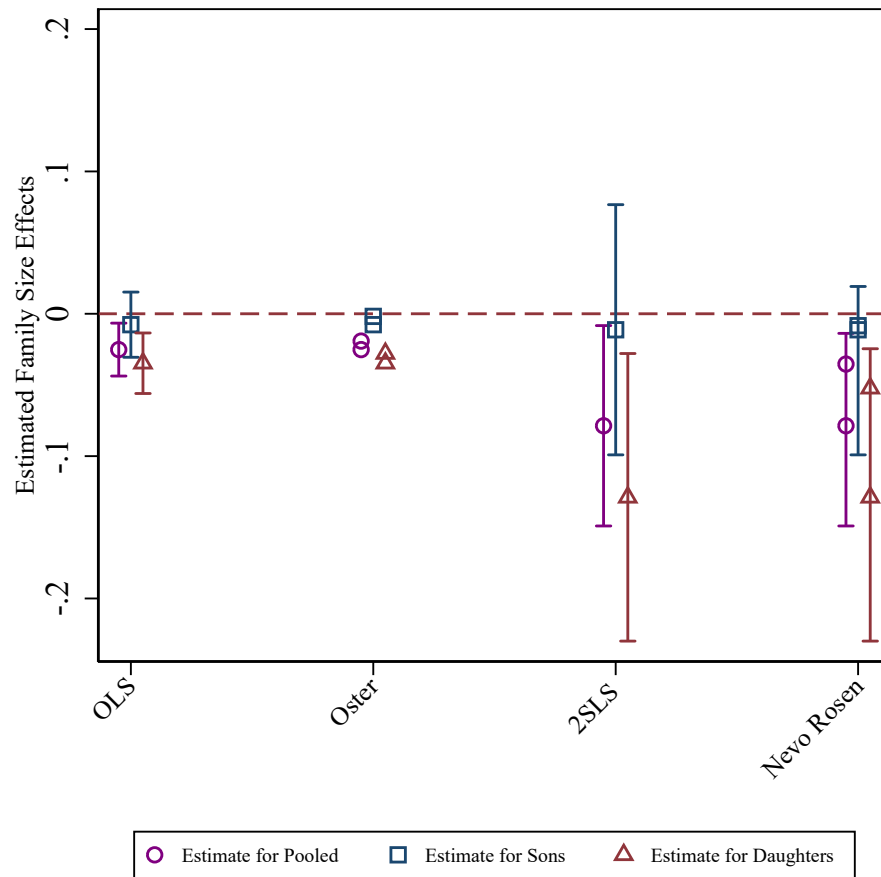


Figure 2: Bounding Family Size Effects

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 (R) 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: $\frac{d^2 Y_T(n, \gamma_C)}{dn^2} = \frac{1}{\alpha} \frac{\theta_0}{\theta_1} \gamma_C''(n) > 0$. 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):

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

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:

$$\ln(c) + \alpha \ln(qn h_S) + (1 - \alpha) \ln((1 - q)n h_D),$$

subject to the budget constraint:

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

^{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 Sample Selection Correction

Under son-preferring fertility stopping rule, it is likely that households with two or more children are self-selected. In particular, parents with a firstborn daughter may be more likely to have a second child in an attempt to get a son compared to parents with a firstborn son. Hence, our results may reflect the family size effects of households with stronger son preference, which could be different from the population. We apply Heckman type selection correction to account for such possibility.

The empirical specifications include a Probit model for family size:

$$\mathbb{P}(\mathbb{1}\{FS_{jp} \geq 2\} = 1) = \Phi(\rho_1 + \rho_2 \bar{z}_{jp} + \rho_3 SA_{ijp} + \rho_4 Son_{ijp} + \mathbf{X}'_{ijp} \rho_5 + \mu_p), \quad (\text{A2})$$

where $\bar{z}_{jp} \equiv Twin_{jp}^{lsame} - Twin_{jp}^{lopp}$ is our instrument for the selection equation. $Twin_{jp}^{lsame}$ and $Twin_{jp}^{lopp}$ are indicators for same-sex and opposite-sex firstborn twins, respectively. \bar{z}_{jp} takes a value -1 for all opposite-sex firstborn twins, a value of 1 for same-sex firstborn twins, and a value of 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 (A2) and inserted as an additional control in our 2SLS estimation described in Section 3. 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 A5, the results are nearly the same as those in Table 2. The negative and statistically significant coefficients of the inverse Mills ratios imply that the average migration probability of a child from a household with two or more children is lower than that of a randomly selected child from non-childless households. This is consistent to our fertility-child-migration trade-off story, as non-childless households also include smaller one-child families. Nevertheless, our family size trade-off estimates are robust to such selection.

B.2 Moving Beyond LATE and Estimating ATE

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 (2)-(3) 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 A8) are similar to those that use FS (Table 2).

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

$$M_{ijp} = (1 - D_{jp}^{FS})M_{ijp}^0 + D_{jp}^{FS}M_{ijp}^1, \quad (A3)$$

$$M_{ijp}^t = \mathbf{X}'\mathbf{t}^t + U_{ijp}^t, \text{ where } t = 0, 1, \quad (A4)$$

$$D_{jp}^{FS} = \mathbb{1}\{v(\mathbf{X}, \mathbf{Z}_{jp}) > V_{ijp}^D\}, \quad (A5)$$

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

$$\mathbb{E}(M_{ijp}^t | \mathbf{X} = \mathbf{x}, V_{ijp}^D = c) = \mathbf{x}'\mathbf{t}^t + \mathbb{E}(U_{ijp}^t | V_{ijp}^D = c) \equiv \mathbf{x}'\mathbf{t}^t + k_t(c).$$

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

$$\Delta^{MTE}(\mathbf{x}, c) = \mathbb{E}(M_{ijp}^1 | \mathbf{X} = \mathbf{x}, V_{ijp}^D = c) - \mathbb{E}(M_{ijp}^0 | \mathbf{X} = \mathbf{x}, V_{ijp}^D = c) = \mathbf{x}'(\mathbf{t}^1 - \mathbf{t}^0) + k_1(c) - k_0(c).$$

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 $\mathbb{P}(D_{ijp}^{FS} = 1 | \mathbf{X}, \mathbf{Z}_{jp}) = c$. Following the literature, we assume that $k_t(c) = \alpha_t(c - \frac{1}{2})$, 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 $Twin_{jp}$ 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 $Twin_{jp}$ (see Table A8).

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_{ijp}^D . Following Andresen (2018) and 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_{ijp}^D . The ATE for sons is negligible (-0.006, $p > 0.1$) 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
<i>Panel A: Selected test score and health outcomes</i>						
Chinese test score (standardized)	69.45 (10.07)	71.07* (9.34)	72.49 (8.62)	74.00* (8.15)	66.93 (10.32)	68.41* (9.53)
Math test score (standardized)	69.39 (10.27)	70.78* (9.50)	69.81 (9.94)	71.21* (9.06)	69.12 (10.47)	70.49* (9.82)
English test score (standardized)	69.30 (10.25)	70.41* (9.55)	72.55 (8.68)	73.36* (8.35)	66.60 (10.60)	67.80* (9.75)
BMI	18.68 (3.18)	18.96* (3.42)	18.54 (2.80)	18.68 (2.90)	18.81 (3.49)	19.22* (3.84)
Ever hospitalized last year	0.09 (0.29)	0.07* (0.25)	0.09 (0.29)	0.06* (0.25)	0.09 (0.29)	0.08 (0.27)
Overall health status (1:worst; 5:best)	3.93 (0.91)	4.12* (0.90)	3.87 (0.91)	4.09* (0.88)	3.98 (0.91)	4.16* (0.91)
Shortsighted	0.54 (0.50)	0.62* (0.48)	0.60 (0.49)	0.68* (0.47)	0.49 (0.50)	0.57* (0.49)
Grit index	9.50 (2.65)	9.49 (2.72)	9.94 (2.27)	9.86 (2.54)	9.14 (2.87)	9.18 (2.80)
Noncognitive skill	11.77 (3.32)	11.95* (3.40)	11.84 (2.93)	11.99 (3.19)	11.75 (3.59)	11.94 (3.53)
Depression index	10.67 (4.33)	10.18* (4.45)	10.95 (4.17)	10.18* (4.04)	10.40 (4.42)	10.21 (4.76)
<i>Panel B: Demographics</i>						
Age	13.61 (1.28)	13.43* (1.23)	13.57 (1.27)	13.39* (1.23)	13.64 (1.29)	13.47* (1.23)
Single child in the family	0.34 (0.47)	0.33 (0.47)	0.32 (0.47)	0.28* (0.45)	0.37 (0.48)	0.39 (0.49)
Boy	0.53 (0.50)	0.52 (0.50)				
Mother's education attainment	3.45 (1.83)	3.50 (1.80)	3.48 (1.86)	3.53 (1.78)	3.43 (1.82)	3.49 (1.82)
Father's education attainment	3.78 (1.81)	4.03* (1.89)	3.84 (1.85)	4.07* (1.90)	3.75 (1.78)	3.99* (1.89)
Family income status	2.84 (0.60)	3.07* (0.47)	2.83 (0.58)	3.07* (0.46)	2.86 (0.62)	3.06* (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 (0.00008)	-0.00024 (0.00017)	-0.00021 (0.00017)	-0.00022 (0.00017)	-0.00024 (0.00017)	-0.00023 (0.00017)
Maternal age square		-0.00001 (0.00001)	-0.00000 (0.00001)	-0.00000 (0.00001)	-0.00000 (0.00001)	-0.00000 (0.00001)
Maternal age		0.00116* (0.00062)	0.00034 (0.00085)	0.00037 (0.00086)	0.00037 (0.00086)	0.00038 (0.00087)
Paternal age square			-0.00002 (0.00001)	-0.00002 (0.00001)	-0.00002 (0.00001)	-0.00002 (0.00001)
Paternal age			0.00126 (0.00083)	0.00128 (0.00083)	0.00123 (0.00083)	0.00130 (0.00082)
Dummy for maternal education is middle school				-0.00196 (0.00124)	-0.00233 (0.00147)	-0.00230 (0.00147)
Dummy for maternal education is high school				-0.00029 (0.00144)	-0.00121 (0.00179)	-0.00119 (0.00179)
Dummy for maternal education is college and above				-0.00045 (0.00206)	-0.00180 (0.00216)	-0.00182 (0.00216)
Dummy for paternal education is middle school					0.00069 (0.00158)	0.00065 (0.00158)
Dummy for paternal education is high school					0.00160 (0.00184)	0.00161 (0.00183)
Dummy for paternal education is college and above					0.00215 (0.00222)	0.00219 (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. (1)	S.D. (2)	Mean/Prop. (3)	S.D. (4)	(1)–(3) (5)	<i>p</i> -value (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 (1)	Interaction (2)	Baseline (3)	Interaction (4)
Twin	-0.079** (0.035)	-0.121** (0.048)	-0.056 (0.037)	-0.094* (0.050)
Twin \times Son		0.107 (0.067)		0.092 (0.068)
No. of children			-0.023** (0.010)	-0.028** (0.011)
No. of children \times Son				0.017 (0.011)
Primary school age	0.032*** (0.007)	0.012 (0.008)	0.032*** (0.007)	0.013 (0.008)
Primary school age \times Son		0.040*** (0.009)		0.038*** (0.009)
Relative birth order	-0.011* (0.006)	-0.026*** (0.008)	-0.017*** (0.005)	-0.031*** (0.008)
Relative birth order \times Son		0.029*** (0.010)		0.028*** (0.010)
Son	0.001 (0.004)	-0.036*** (0.009)	-0.000 (0.004)	-0.070*** (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: Sample Selection Correction à la [Heckman \(1979\)](#)

	Whole Sample		Firstborn Sample	
	Baseline (1)	Interaction (2)	Baseline (3)	Interaction (4)
No. of children	-0.078** (0.037)	-0.124** (0.051)	-0.094*** (0.036)	-0.131*** (0.048)
No. of children \times Son		0.111* (0.066)		0.097 (0.067)
Primary school age	0.032*** (0.007)	-0.001 (0.013)	-0.019 (0.016)	-0.056*** (0.021)
Primary school age \times Son		0.065*** (0.021)		0.075*** (0.029)
Relative birth order	-0.031*** (0.011)	-0.041*** (0.012)		
Relative birth order \times Son		0.022** (0.010)		
Son	0.031** (0.012)	-0.257* (0.138)	0.045** (0.020)	-0.216 (0.137)
Inverse Mills Ratio	-0.216*** (0.073)	-0.151** (0.073)	-0.318*** (0.116)	-0.224** (0.099)
<i>Marginal effects for sons</i>				
No. of children		-0.013 (0.047)		-0.033 (0.049)
Primary school age		0.064*** (0.013)		0.019 (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.1.

*** $p < .01$, ** $p < .05$, * $p < .10$.

Table A6: Sample Restrictions

	Firstborn Children		Children Born Elsewhere	
	Baseline (1)	Interaction (2)	Baseline (3)	Interaction (4)
No. of children	-0.095*** (0.035)	-0.132*** (0.049)	-0.088** (0.042)	-0.162*** (0.058)
No. of children \times Son		0.098 (0.068)		0.175** (0.075)
Primary school age	-0.019 (0.014)	-0.027* (0.015)	0.015 (0.010)	0.001 (0.011)
Primary school age \times Son		0.022 (0.015)		0.031** (0.013)
Relative birth order			-0.043*** (0.012)	-0.054*** (0.013)
Relative birth order \times Son				0.022* (0.013)
Son	-0.006 (0.006)	-0.226 (0.138)	0.003 (0.005)	-0.397** (0.156)
<i>Marginal effects for sons</i>				
No. of children		-0.034 (0.047)		0.013 (0.051)
Primary school age		-0.005 (0.017)		0.032*** (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 A7: Alternative Specifications: Two-Step Estimation and Control Function Approach

	Mother Fixed Effect		Two-Step Estimation		Control Function Approach	
	Baseline (1)	Interaction (2)	Baseline (3)	Interaction (4)	Baseline (5)	Interaction (6)
No. of children			-0.075** (0.030)	-0.119*** (0.046)	-0.073** (0.032)	-0.106** (0.042)
No. of children \times Son				0.107* (0.064)		0.089 (0.061)
Primary school age	0.023*** (0.005)	0.011* (0.006)	0.032*** (0.007)	0.022** (0.008)	0.034*** (0.007)	0.020** (0.009)
Primary school age \times Son		0.023*** (0.007)		0.022** (0.009)		0.029*** (0.010)
Relative birth order	-0.006 (0.005)	-0.011* (0.006)			-0.030*** (0.010)	-0.040*** (0.011)
Relative birth order \times Son		0.009 (0.008)				0.022** (0.010)
Son	0.000 (0.003)	-0.017** (0.007)	-0.005 (0.004)	-0.246* (0.134)	-0.002 (0.004)	-0.216* (0.127)
<i>Marginal effects for sons</i>						
No. of children				-0.012 (0.040)		-0.017 (0.046)
Primary school age		0.034*** (0.006)		0.043*** (0.008)		0.049*** (0.008)
Relative birth order		-0.002 (0.006)				-0.018* (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 A8: Family Size Effects with Binary Treatment

	Subsample of Sons			Subsample of Daughters		
	Both IV (1)	Twin IV Only (2)	Same-sex IV Only (3)	Both IV (4)	Twin IV Only (5)	Same-sex IV Only (6)
<i>Second-stage coefficients:</i>						
1(Have three or more children)	-0.038 (0.049)	-0.013 (0.052)	-0.179 (0.148)	-0.109*** (0.034)	-0.144** (0.056)	-0.087** (0.043)
<i>First-stage coefficients:</i>						
Twins	0.918*** (0.027)	0.923*** (0.027)		0.845*** (0.022)	0.861*** (0.020)	
Same sex	0.045*** (0.005)		0.046*** (0.005)	0.153*** (0.006)		0.155*** (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.

*** $p < .01$, ** $p < .05$, * $p < .10$.

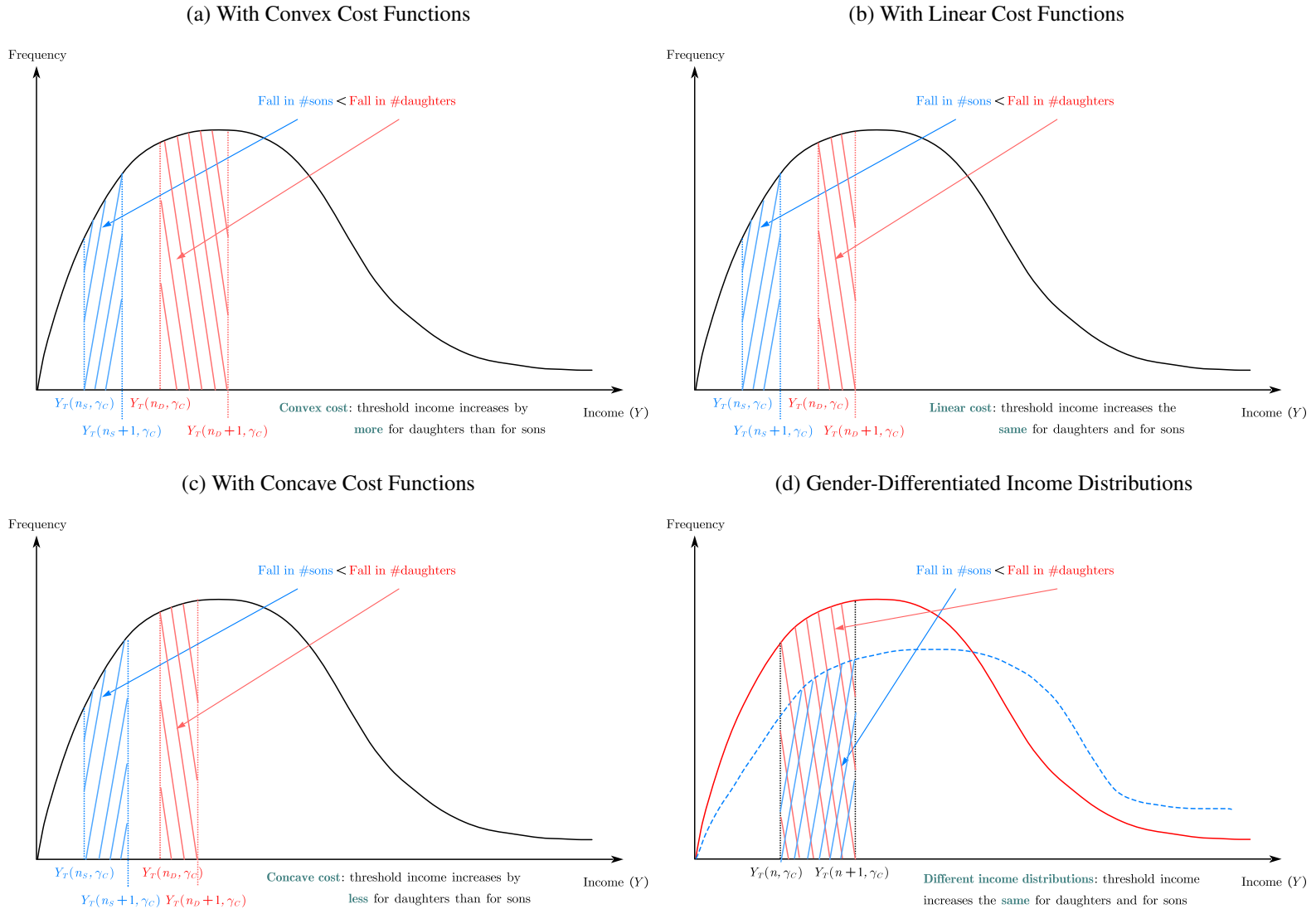


Figure A1: Alternative Education Cost Functions

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.

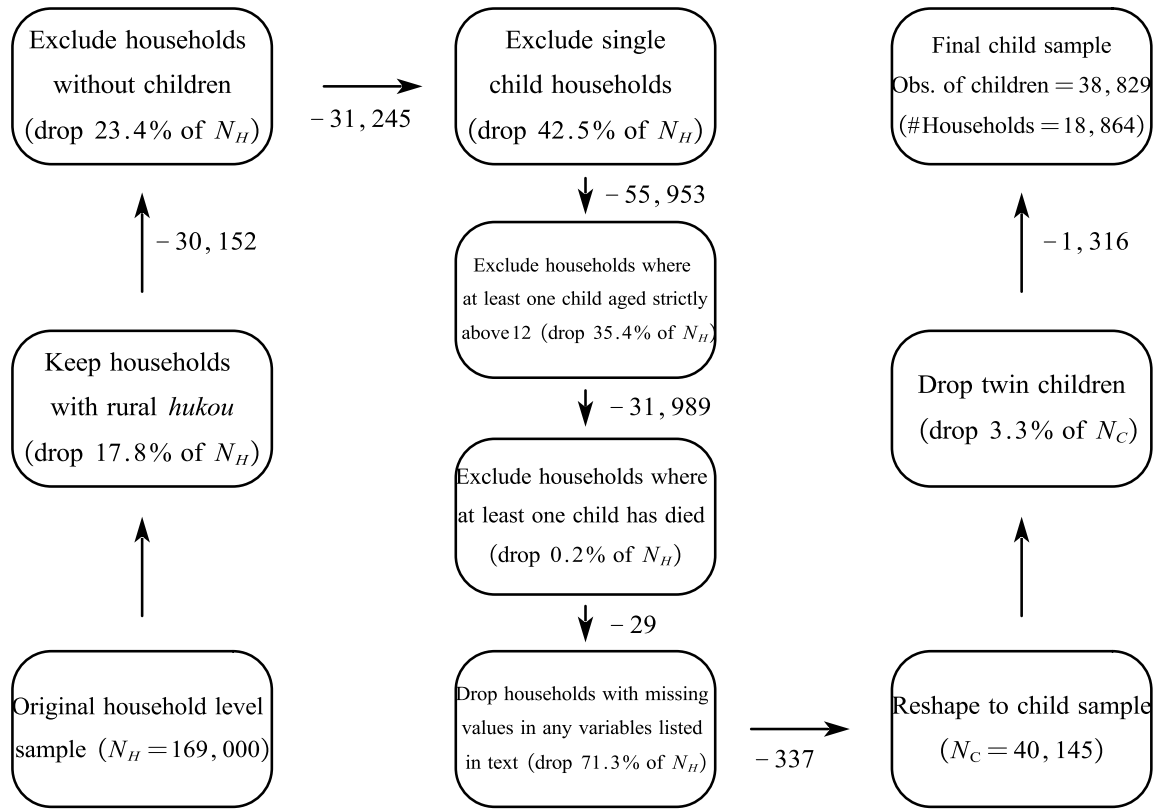


Figure A2: Sample Selection

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).

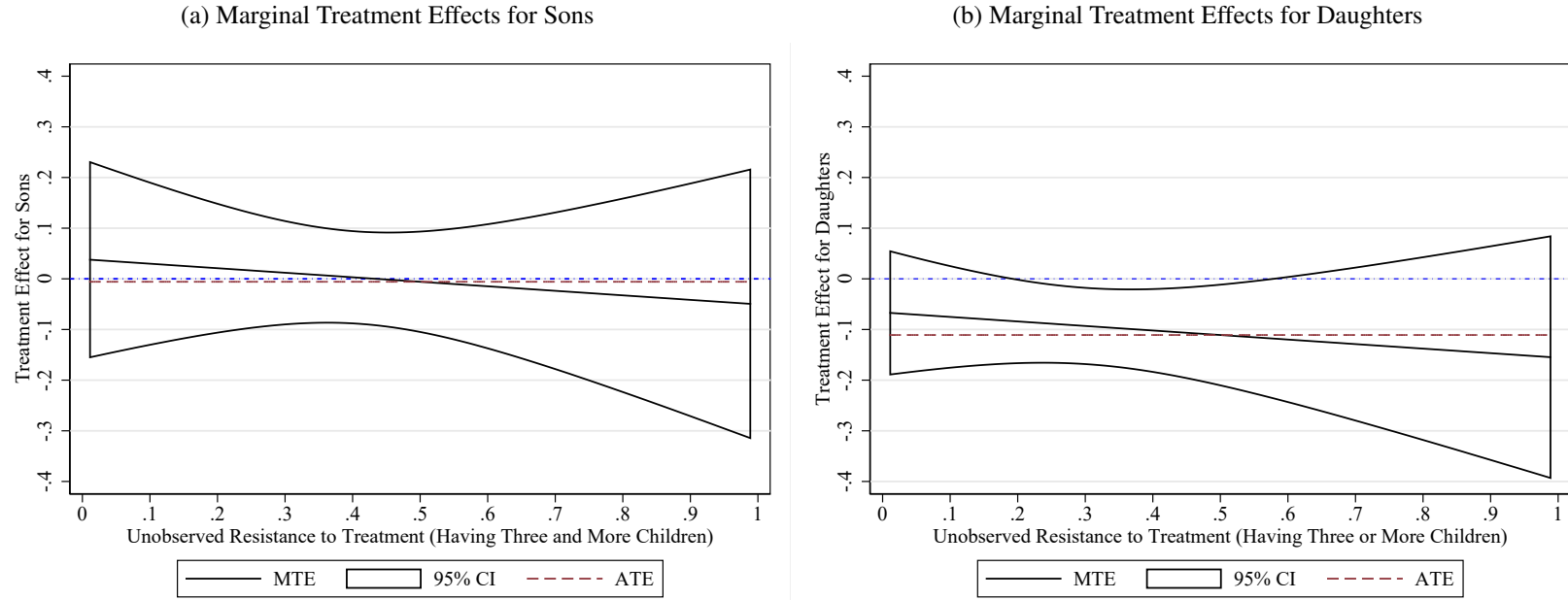


Figure A3: Marginal Treatment Effects

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 Andresen (2018) and 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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