Family Size, Sibling Rivalry and Migration: Evidence from Mexico*

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Abstract

This paper examines the effects of family size and demographic structure on offspring's international migration. We use rich survey data from Mexico to estimate the impact of sibship size, birth order and sibling composition on teenagers' and young adults' migration outcomes. We find little evidence that high fertility drives migration. The positive correlation between sibship size and migration disappears when endogeneity of family size is addressed using biological fertility (miscarriages) and infertility shocks. Yet, the chances to migrate are not equally distributed across children within the family. Older siblings, especially firstborn males, are more likely to migrate, while having more sisters than brothers may increase the chances of migration, particularly among girls. [*JEL codes:* J13 F22 O15.]

Keywords: International Migration, Mexico, Family Size, Birth Order, Sibling Composition.

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1 Introduction

Migration from poor to rich countries is one of the most important ways through which workers can increase their income opportunities as well as their families' welfare back home (Chen et al. 2003, Kennan and Walker 2011, Clemens 2011). A key feature of migration is that it mainly involves young adults who are more likely to have a positive net expected return to migration due to their longer remaining life expectancy (Sjaastad 1962). According to recent UN figures, worldwide international migrants aged 15 to 24 account for 12.5 per cent of total migrants worldwide, and when migrants between the ages of 25 and 34 are added, young migrants represent over 30 per cent of the total (UNDESA 2011). The proportion of youth migrants is much higher in developing countries than it is in advanced countries and it more than doubles if we consider internal migrants as well (UN 2013).

Given the profitable nature of labor mobility, which involves both the (young) migrant and her origin family, an extensive literature on the determinants of migration has emphasized the important role of household (along with individual) factors in the migration decision (e.g. Rosenzweig and Stark 1989, Stark 1991). Indeed, in many developing countries, labor migration is a family strategy to diversify income sources, improve earning potentials and increase household security through remittances (e.g. Stark and Bloom 1985, Yang 2008, Antman 2012).

As a result, family migration strategies in developing contexts may involve the costly parental decision to dispatch one of the children to work in a different city or abroad, and to invest in a potentially remitting child (Lucas and Stark 1985, Jensen and Miller 2011). However, parents face a number of trade-offs when allocating resources across their children, due to either limited household resources or (perceived) different returns on the migration investment (e.g., son bias).[1] This may generate resource dilution effects in large families or competition (rivalry) among siblings from the same household (Garg and Morduch 1998, Black et al. 2005). Although the individual determinants of migration have already been extensively studied, far less is known about the role of the size and the structure of the origin household — in particular the role of siblings — on migration investment decisions. This is

[1] A well-established theoretical literature in economics rationalizes a causal link running from children's economic resources to their lifetime opportunities and their adult outcomes (Becker and Tomes 1976, Schultz 1990, Thomas 1990)

a surprising gap given the popular view that migrants come from high-fertility countries and typically leave behind several household members who oftentimes are siblings (Hatton and Williamson 1998).

To the best of our knowledge, this is the first paper to assess the causal effect of demographic characteristics of one's childhood household, i.e. sibship size, birth order and composition of siblings (by gender and age), on the likelihood to migrate abroad.[2] We address this question in the context of the Mexico-U.S. mass migration in the 1990s. Mexico is one of the largest migrant-sending and remittance-recipient countries worldwide, with a migration wave that swelled in the 1970s and kept growing in the 1980s and 1990s, ranging from 5.2 percent of Mexico's national population in 1990 to a peak of 10.2 per cent in 2005 (Hanson and McIntosh 2010). According to the Mexico Population Census, during the 1990s alone, 9 percent of Mexicans aged 16 to 25 (based on age in 1990) migrated to the United States. A distinguishing feature of last century Mexico-U.S. migration is that most migrants typically have low levels of education and many of them have their first U.S. jobs in seasonal agriculture (Martin 1993).[3] According to U.S. Census data, in 1990 70.4 percent of Mexican immigrant men were high-school dropouts, compared to 12.9 percent of the male native-born working population and 21 percent of non-Mexican immigrant working men (Borjas and Katz 2005). Yet, the American Dream creates opportunities for upward mobility such that Mexican immigrants enjoy income gains with respect to their counterparts living in Mexico, and family members at home share in these gains through remittances (Hanson 2004, Ozden and Schiff 2006, Rosenzweig 2007, Clemens et al. 2010). Importantly, emigration rates differ by age and gender. Using Mexico population censuses Hanson and McIntosh (2010) report that a significant fraction of males migrates by age 16 with emigration increasing sharply until approximately age 30 and decreasing thereafter, presumably as a result of return migration. By contrast, for females there is less emigration by age 16, with subsequent rates being relatively stable over the course of their lives.[4]

Moreover, the wave of Mexican migration in the 1990s crosses over a demographic boom

[2] Several studies document sibship size and birth order effects in outcomes as varied as schooling, height and IQ (see Black et al. 2005, Angrist et al. 2010, Pande 2003, Jayachandran and Pande 2015, among others).

[3] U.S. policy supported the recruitment of rural Mexicans under bilateral agreement between the 1940s and the 1960s (e.g., the Bracero Program) but most of the 20th century Mexican migrants arrived and were employed outside guestworker programs (Martin 1993).

[4] See Figure 2 reported in Hanson and McIntosh (2010).

that petered out years later. Mexico's birth rate stood at about seven children per mother in 1970. The gradual spread of family planning practices contributed to impelling the fertility transition in the country where, by 2005, the number of children per woman declined to slightly more than two (Cabrera 1994).[5] Yet, despite the abundant evidence on the potentially significant implications of high fertility rates for child investments and economic outcomes, the existing literature provides scant rigorous analysis of the link between family size and the international migration of offspring.[6]

By using two waves of a large and nationally-representative demographic household survey, we focus on the determinants of migration of Mexican adolescents and young adults in the age range 15 to 25. Our large dataset allows us to overcome the limitations of small samples of children, and it includes detailed information on fertility histories, infant and general mortality. Importantly, it allows us to address the potential endogeneity of parental fertility choices which arises from the fact that families who choose to have more children may also be those who value child out-migration more. This may be the case because, in a context such as Mexico with weak institutions and (credit or insurance) market imperfections, children may be viewed as a means of acquiring old-age security and support (Becker 1960, Cigno 1993).[7] Thus, the lure of international migration from Mexico to the U.S. may increase the likelihood of upstream transfers from children to parents, and hence raise the economic returns of high fertility for parents (Stark 1981). We address this endogeneity issue by exploiting exogenous variation in family size induced by either infertility shocks or miscarriage at first pregnancy (Agüero and Marks 2008, Miller 2011). We further investigate birth order, sibling-sex and sibling-age composition effects on migration by using family fixed effects, i.e. by exploiting between siblings variation only. This is important in order to shed light on the intra-household selection process into migration, which has important implications for child welfare, gender disparities and the ultimate impact on origin families

[5]In 1974 a new population policy was designed in Mexico with the aim of reducing population growth and promoting development. The new institutional structure established to ensure policy implementation (the National Population Council- CONAPO) has expanded geographically and socially over time (Zuniga Herrera 2008).

[6]In what follows we use 'family size', i.e. the number of children, and 'sibship size', i.e. the number of siblings, interchangeably: the former takes the point of view of parents, whereas the latter takes the perspective of children.

[7]We use data on young adults in Mexico in the mid-1990s whereby fertility decisions of their mothers were made across the 1970s and 1980s. At that time, the country was classified as a developing poor economy and the lack of markets or institutions was more likely to be mitigated by the family than is currently the case in Mexico.

(Chen 2006, Mourard 2015).

We find no evidence that high fertility drives migration choices at the household level. The positive correlation between fertility and migration disappears when the potential endogeneity of sibship size is addressed. On the other hand, the chances to migrate are not equally distributed across children within the same family. Older siblings, especially firstborn males, are more likely to migrate, while having more sisters than brothers may increase the chances of migration, especially among females. Results are robust to several changes in both the estimation sample and the estimation strategy.

These findings have relevant implications. First, our analysis can contribute to explaining the impact on migration of fertility-reducing programs —such as investments in family planning, sex and reproductive health— which have been endorsed in many developing countries as a policy response to the apparent vicious circle of high-fertility, poverty and economic stagnation (Miller and Babiarz 2014, Schultz 2008). Some of these programs have been implemented in high fertility societies with significant out-migration rates, such as Mexico, but little is known about the (intended and unintended) consequences of the former on the latter. By observing a positive association between fertility and economic migration, implications may be drawn that smaller families may lead to lower rates of mobility. Yet, we provide little evidence that the causal relationship goes in this direction. Second, our empirical findings hint to the fact that parental investment in offspring's migration may matter for dynamic fertility decisions in contexts of poor resources and high emigration opportunities, i.e. parents may take into account their offspring's future migration opportunities when making their fertility choices. The reason is that, in developing settings, the offspring are the primary caretakers of parents and they may do so by providing support to their origin family through emigration and spatial diversification in residential location.

The paper unfolds as follows. Section 2 describes the link between household structure and migration as considered by the related literature on human capital investment. Section 3 presents the data and sample selection. The methodology and empirical strategy is described in Section 4. Section 5 presents our main results on birth order and sibship size effects on migration, and some evidence on siblings' composition effects. Finally, Section 6 summarizes our main findings and concludes.

2 Related literature

Standard economic theory conceives labor migration as an investment in human capital whereby relocation requires up-front resources followed by a positive payout in the future (Sjaastad 1962, Schultz 1972, Dustmann and Glitz 2011). Positive returns on migration, which are higher for young people, are conceived in terms of both migrants' earnings and remittances sent back home (Stark and Bloom 1985, Yang 2008, Amuedo-Dorantes and Pozo 2011). Indeed, people decide to migrate because they expect their own or their family's payoff to be higher in terms of a different and higher profile of earnings, quality of life, health or security or they do so because migration mitigates the risks and household portfolios at origin (Chen et al. 2003, Kennan and Walker 2011, Clemens 2011). Recent evidence shows that – after controlling for self-selection– workers who move from a poor to a rich country can experience immediate, lasting, and very likely increases in earnings, even for performing exactly the same tasks (Gibson and McKenzie 2012, Ashenfelter 2012).[8] Beyond income gains for migrants, cross-border migration typically brings additional liquidity to the family members left behind through remittances, which significantly support consumption and investment decisions, in addition to the management of risk and credit constraints in the household of origin.

Given the key economic role played by migrants' remittances, especially in developing contexts, several contributions in the migration literature point to the household as the main unit for migration choices (Rosenzweig 1988, Stark 1991, Ghatak and Price 1996). The core feature of this collective decision-making framework is that the family aims at maximizing household income and therefore can make the costly decision to dispatch one (or more) young member to work in foreign labor markets in order to receive remittances (Stark and Bloom 1985). Thus, in the absence of well-functioning credit or insurance markets, migration can be a household investment strategy whereby one or more members are assigned to work in the local economy while others are sent abroad to act as a source of insurance

[8] By combining household data in Mexico with U.S. and Mexico population censuses, and controlling for self-selection on observables and unobservables in migration, recent works estimate that yearly income gains to Mexico-U.S. migration are around 6,700 to 8,000 U.S. dollars (Hanson 2004, Clemens et al. 2010). Moreover, according to the 2009 poll by the Pew Research Center, one third of all Mexicans would move to the U.S. if they could do so, and half of these potential Mexican migrants report to be prepared to move illegally to the U.S. According to more than 55 percent of those polled in Mexico, Mexicans who move to the U.S. have a better life despite well-known hardships, whereas less than 15 percent report that life is worse in the U.S.

or financial enhancement. Empirical evidence on the implications of migration as a family security strategy in developing countries is abundant (see Ratha et al. 2011, for a review). For instance, Rapoport and Docquier (2006) survey the different motives for remittances sent by migrants, which are also found to be used as a form of support for the elderly (see also Clemens et al. 2014). By using data from Mexico, Antman (2012) shows that children migrated to the U.S. (strategically) provide financial contributions to the health care of their parents (see also Stöhr 2015). At the same time though, little evidence exists on the degree to which the family environment — in particular family size and composition — affects children's out-migration decisions.

The link between the household structure and parents' investments in the human capital of their children has received substantive attention in the household economics literature. Theoretical models of fertility choices have been widely influenced by the argument of the 'quantity-quality (Q-Q) trade-off'. The Q-Q model treats the quantity and quality of children in a similar fashion as other consumption goods in the household so that, in the absence of parental discrimination between children, there is a trade-off between child 'quality' (or outcomes) and the number of children within a family (Becker 1960, Becker and Lewis 1973, Becker and Tomes 1976). However, in many of today's developing countries (as well as in rich countries around the time of their industrial revolution) parents have often used their children as a substitute for missing institutions and markets, notably social security in old age (e.g., Nugent 1985, Cigno 1993, Ray 1998).[9] According to this framework — known as the 'old-age security hypothesis' — on top of the consumption-good aspect of children, fertility choices are influenced by the child role of investment-good or household asset. Children embody income-earning possibilities for both themselves and their parents, which may be the reason why, in poor contexts (i.e. with weak formal markets and social safety-nets) people generally choose to invest in their future in the form of children (Duflo and Banerjee 2011). The traditional system of family arrangement, though, may have important consequences on economic choices and offspring's outcomes (Platteau 1991).

Although an extensive empirical literature provides evidence on the role of household size and composition in parental investments in other forms of children's human capital,

[9] Recent contributions on contemporary developed societies show that when pensions and income from retirement decrease, the old-age security motive matters for fertility decisions even in these settings (see Gábos et al. 2009, Billari and Galasso 2014).

such as education and health (Garg and Morduch 1998, Black et al. 2005), within-family considerations have been less analyzed in the context of migration decisions.[10] Yet, if migration is costly and migrants move at a relatively young age, it is plausible that migration is the result of family decision-making in which parents decide on their children's relocation (potentially retaining some control over their children's earnings as well), or that children are influenced by their family background (e.g., household characteristics, number of siblings) when deciding to move. Thus, in families with limited resources and more than one child to raise, greater sibship size may *negatively* affect child out-migration through a resource dilution effect (i.e. a smaller share of resources per child) or because more family-work is needed at home, e.g., care for younger children (Becker and Lewis 1973, Giles and Mu 2007). On the other hand, larger families may increase the pressure of the family hierarchy, the dependency ratio and the amount of disposable resources to support family members. Hence, a reallocation of resources from children to parents may become necessary so that young household members are dispatched abroad in order to send remittances or offer potential support back home. In particular, if children contribute to family income either through child-labor, economic diversification or parental-care, then a larger number of siblings may have a *positive* effect on the out-migration of one (or more) of them (Brezis and Ferreira 2014, Stöhr 2015). A similar positive association between sibship size and migration may be observed if a larger number of siblings allows some children to move far from their families and help them with financial transfers, as they can count on other siblings for the provision of elderly care (i.e. time inputs) to their parents (Antman 2012). The relative strength of these competing forces is ultimately an empirical question. This is what we turn to in the following sections.

3 Data and sample selection

This study uses data from the 1992 and 1997 waves of the *Encuesta Nacional de la Dinámica Demográfica* (ENADID), conducted by the National Institute of Statistics and Geography

[10] Findings on the impact of family size on child outcomes are mixed. Early results tended to predominantly show that children from larger families have worse outcomes, especially in terms of human capital investment and earnings (Rosenzweig and Wolpin 1980, Hanushek 1992, Parish and Willis 1993). However, after controlling for the endogeneity of fertility, in more recent papers family size does not turn out to adversely affect child outcomes (see Black et al. 2005, Angrist et al. 2010, Fitzsimons and Malde 2014, among others).

(INEGI) in Mexico. Each ENADID's wave surveys more than 50,000 households from all over the country and is representative of the Mexican population. The dataset is very rich and unique, collecting comprehensive information on women's fertility as well as migration history of all household members, in addition to standard socio-economic characteristics. Importantly, by using detailed demographic information on age (month and year of birth) and gender of individuals in the same household with the same mother, we are able to identify all biological families in the sample and recover complete information on the number and gender of all siblings (also those not currently living in the household of origin).

The ENADID allows us to define household members' international migration experience based on three separate questions, i.e. (i) whether there is any household member (even temporarily absent) who migrated abroad during the five years prior to the survey; (ii) whether any household member has ever worked in or looked for work in the United States (and the year in which this occurred); and (iii) whether the respondent reports a period of residence abroad at any point in time prior to the survey. The use of these three different sources of information for migration episodes ensures that we are able to capture a relevant part of the phenomenon.[11] Overall, in 1997 (1992) almost 18 (15) percent of households in Mexico report having a member who migrated abroad.

Since we are interested in the effect of family size on parental investment in offspring's migration, we define individual migration episodes as *non-tied* migration, i.e. we exclude from the sample children who experienced episodes of migration joint with their parents and those whose parents have an international migration experience.[12] Figure 1 reports the incidence of non-tied migration by age and gender in Mexico showing that, overall, migrants are massively concentrated (more than 70%) in the age range of 15 to 25. Hence, throughout our analysis we restrict the sample to individuals aged 15 to 25. This is also consistent with the argument that Mexican youngsters finish compulsory schooling and potentially enter the

[11]By containing information on migrants who have either returned to Mexico, or who have at least one household member remaining in Mexico, excluding households that have migrated abroad in their entirety, the ENADID tends to under represent permanent tied migrants (see also Hanson 2004, Mckenzie and Rapoport 2007). However, the latter form of potential selection is of little concern to us since our main outcome of interest is the effect of family size on parental investment in children's migration, so that we do need to exclude 'family migration' and focus on households left behind by one or more migrant member.

[12]Yet, we investigated the robustness of our findings to the inclusion of tied-migrants as well (9 percent of the sample), including parents' migration status among the controls (results available upon request). In their study of Mexican migration to the U.S., Cerrutti and Massey (2001) report that nearly half of all male migrants leave to the U.S. before having or without a wife or a parent.

labor market at the age of 15, whereas beyond the age of 25 they are more likely to make their own lives apart from the origin family.[13]

[Figure 1 about here]

The ENADID further collects detailed information on fertility for all women aged 15 to 54 at the time of the survey. Women answer specific questions on the number of children ever born, their gender and birth order, current and past contraceptive use, fertility preferences, and their socioeconomic and marital status. Such information allow us to construct our key explanatory variable, that is the total number of biological siblings of each individual in the sample. Moreover, it enables us to identify parental exogenous shocks to fertility induced by self-reported infertility episodes and miscarriage at first pregnancy (see Section 4.2 for more details). In line with the medical definition of infertility [14] and with the literature (e.g., Agüero and Marks 2011), we restrict our sample to children of non-sterilized women who are not currently using contraceptives or who never have (about 80 per cent of the original sample).

Our final estimation sample comprises 26,743 children in the age range of 15 to 25.[15] In our sample of individuals, 5.2 percent are migrants with male and female migration rates equaling 7.07 and 2.92 percent, respectively. In Figure 2 we plot the average migration rate of the boys and girls in our sample by sibship size. A positive association between sibship size and migration for sons clearly emerges. Individual sample characteristics are reported in Table 1 according to the migration status. Migrants are mostly males (75 percent) and they report significantly more brothers and sisters than non-migrants. Moreover, migrant children appear to be slightly older and live in less educated but richer (in terms of income) households than non-migrant children.

In Figure 3 we plot the ratio of migrant children in the household by family size, in the sample of households with at least one migrant child (against the distribution of migrant

[13] Yet, our findings are also robust to the sample cut on individuals aged 15 to 35. Results are available upon request.

[14] The medical literature defines infertility as the failure to conceive after a year of regular intercourse without contraception.

[15] Mothers of individuals in our estimation sample have an average age of 45 years old. The average birth spacing between the first and the last child in our estimation sample is 13 years, which is below the minimum age of individuals we consider (15). Our sample of children does not include those with mothers older than 54 years of age (9 percent of the total population aged 15-25) since fertility information was not collected from them.

households in the population). The plot shows a negative association between the child migrant ratio and sibship size, which means that all households, regardless of size, hardly have more than one young family member who migrated abroad (the average number of young migrant members per household is 1.14 in the sample of households with migrants). This is suggestive of an intra-household selection process into offspring's migration which we further explore through inferential analysis in the following sections.

[Table 1 about here]

[Figure 2 and Figure 3 about here]

4 Empirical strategy and identification

4.1 Sibship size and birth order effects

In our analysis we are interested in the effects of sibship size and composition on an individual's likelihood to migrate. In order to estimate the effect of sibship size, though, we need to control for the birth order of children (see, for instance Black et al. 2005). Indeed, if parents have a preference for the first children they have, and invest comparatively more resources in them, then a spurious negative correlation between sibship size and human capital investments may emerge simply because in larger families we also find children with higher birth orders. In other words, the two variables of birth order and sibship size are highly correlated. In particular, although one can assess the effect of family size on firstborns by looking at firstborns' outcomes in families of different size, it is not possible to examine, for instance, the outcome of a fourth-born child when sibship size changes from two to three, given that fourth born children are only found in larger families.

Recently, Bagger et al. (2013) have proposed a theoretically-grounded methodology to disentangle the two effects. We draw on their study to employ a two-step estimation strategy. In a first step we estimate the following regression using OLS:

$$M_{ij} = \alpha_0 + \sum_{k=2}^{K} \alpha_{1k} bo_{ijk} + \alpha_2 \mathbf{X}_{ij} + u_j + \varepsilon_{ij} \tag{1}$$

where the outcome variable M_{ij} pertains to the migration status of child i in household j and is a dichotomous indicator of either current or past migration experiences abroad. bo_{ijk} is a dichotomous indicator for the child being of birth order $k = 2, ..K$ where K is the maximum birth order of children in our sample and $k = 1$ (i.e. firstborn) is the reference group; $\mathbf{X}_{ij}$ is a vector of individual covariates including child gender, age, age squared and cohort indicators (one for each year of birth).[16] u_j is a family fixed effect, and ε_{ij} an idiosyncratic error.

The effect of sibship size is captured in equation (1) by the family fixed effects, which control for any (observed and unobserved) difference between families. The birth order fixed effects capture the differences in the probability of migration between children of different orders within the same family. Only within-family variation is exploited in these estimates, and the birth order effects are not contaminated by between-family variation in family sizes, i.e. the fact that children in larger families also have higher average birth orders.

In the second step, we subtract the birth order effects from the dependent variable, i.e. we compute the difference $\widehat{NM}_{ij} = M_{ij} - \sum_{k=1}^{K} \hat{\alpha}_{1k} bo_{ijk}$ where NM stands for 'netted migration,' and use it as the dependent variable in the second step.[17] Hence, the following equation is estimated:

$$\widehat{NM}_{ij} = \beta_0 + \beta_1 S_{ij} + \beta_2 \mathbf{X}_{ij} + \beta_3 \mathbf{W}_j + v_{ij} \tag{2}$$

where S_{ij} is sibship size. The coefficient β_1 captures the effect on migration of being raised in a family with sibship size S_{ij} for the 'average child' in that family, i.e. regardless of his/her birth order. $\mathbf{X}_{ij}$ is a vector of individual covariates defined as above and $\mathbf{W}_j$ includes family background characteristics such as the mother's and father's age and age squared, and the mother's and father's years of completed education. In some specifications, we also control for maternal health, the father's absence from the household (i.e. widowed and divorced single-mother families) and municipality fixed effects (which also capture the rural vs. urban residence along with many other factors related to different local cultural or economic conditions, access to contraception, etc.). Since the dependent variable has been generated by a regression, standard errors are corrected by weighting the estimation with the inverse of the standard error of $\widehat{NM}_{ij}$.[18] We estimate equation (2) by using either

[16]We can include a control for both age and birth cohort indicators because we use two cross-section surveys.

[17]Coefficients of all birth order indicators (including firstborns) are recovered using the method described in Suits (1984).

[18]See, for instance Lewis and Linzer (2005). We also run estimates using OLS and White robust standard

Weighted Least Squares (WLS) or Two-stage Least Squares (2SLS) (See the next Section.) Throughout, standard errors are clustered at the household level as to account for potential error correlation across siblings.

4.2 The sibship size effect: Identification strategy

If the number of children and investment in child out-migration are both outcomes over which parents exercise some choice, then the estimate of the sibship size effect in equation (2) provides spurious evidence. In other words, parental fertility may be endogenous with respect to children's migration. It is plausible, for instance, that the opportunity to send some children abroad modifies parents' fertility choices. In developing countries, children are a valuable asset for parents and a source of old-age support. If offspring's migration opportunities are not equally distributed across families, it may be the case that households with lower migration costs or higher benefits for their members will also decide to have more children. Alternatively, unobservable parental preferences for children and old-age support through migration may positively co-vary. Stark (1981) and Williamson (1990), for instance, postulate that heterogeneity in parents' preferences for childbearing and for migration are systematically related, and in a context such as Mexico where migration cum remittances is an essential lifeline to households of origin, they are generally positively related. In both of these cases, the positive association observed between fertility and child out-migration is likely to overstate the true causal relationship. This pattern of heterogeneity of preferences or migration costs may lead to a larger positive association between fertility and child out-migration than would be observed if there were fertility changes due to exogenous shocks.

Hence, to clearly identify the relationship between sibship size and migration, a presumably exogenous source of variation in family size is required. The ENADID allows us to identify self-reported infertility from specific questions posed to non-sterilized women who have never used contraceptive methods or who are not currently using them. More specifically, we construct an indicator variable for infertility shocks that takes the value of one if a woman declares she never used contraception or she has stopped using the previous method because of infertility episodes ('infertility shock') and zero otherwise (Agüero and Marks

errors, and the results on the effect of sibship size are virtually the same.

2008).[19] The ENADID also enables us to build a second indicator variable that equals one if a woman experienced a miscarriage at first pregnancy ('fertility shock') and zero otherwise. For our identification strategy to be valid, the two instruments must satisfy three conditions — i.e. exogeneity, relevance and the exclusion restriction assumption — which we discuss below.

Infertility or subfertility conditions have already been used in the economic literature to estimate the effect of the number of children and fertility timing on mothers' labor market outcomes (see, for instance, Agüero and Marks 2008; 2011, Schultz 2008). There is evidence that infertility is virtually random, i.e. it is independent of the background characteristics of infertile women. For example, variables such as the father's social status and parity have been shown to be unrelated to observed heterogeneity in fertility (Joffe and Barnes 2000). In an article summarizing the epidemiological literature regarding the role of lifestyle factors (cigarette smoking, alcohol and caffeine consumption, exercise, BMI, and drug use) in female infertility, Buck et al. (1997) conclude that few risk factors have been assessed or identified for secondary infertility. In addition, using U.S. data, education, occupation, and race have been shown to be unrelated to impaired fecundity (Wilcox and Mosher 1993). By using data on a large set of developing countries, Agüero and Marks (2011) present evidence that infertility is generally uncorrelated with the background characteristics of women, with a few exceptions such as women's education and rural residence (which will be controlled for in our models).

Also miscarriages and stillbirths have been used to identify fertility *tempo* and *quantum* effects on women's labor market outcomes, mainly in advanced countries (Hotz et al. 2005, Miller 2011, Bratti and Cavalli 2014). Their exogeneity is generally supported by the medical literature. For example, a few papers using administrative data, in which rich labor market and health data are merged, show that miscarrying is not generally significantly associated with worse labor market outcomes (e.g., work absences) before miscarriage (Karimi 2014, Markussen and Strøm 2015). Miscarriage or spontaneous abortion typically refers to any loss of pregnancy that occurs before the 20th week of pregnancy. By nature, miscarriages

[19] Such shocks may only be temporary or have emerged relatively recently. This means that even subfertile women may have large families. The data do not provide information on the age when these problems first presented.

should have a negative effect on total fertility, and in our context on sibship size.[20] Only two etiological factors for miscarriage are recognized by different authors in the obstetrics literature, i.e. uterine malformations and the presence of balanced chromosomal rearrangements in parents (Plouffe et al. 1992). The latter though, are unlikely to be correlated with women's attitudes towards offspring's migration. The number of miscarriages and stillbirths generally increases with the number of pregnancies, which depend in turn on desired fertility, and this could potentially generate a spurious positive correlation between the number of miscarriages and observed fertility. For this reason, we consider only miscarriages that occurred at the first pregnancy (Miller 2011). There is a potential issue of measurement error with this instrument, since women may be unaware of miscarriages or, especially the case with older women, may fail to recall them. Misreporting may generally affect the power of the instrument, but we do not expect any specific pattern of correlation between it and parents' attitudes towards child out-migration conditional on the observables (including a quadratic in the mother's age). Finally, as it was formulated in the ENADID, the question does not distinguish between voluntary and involuntary abortions. Thus, it may be the case that some of the reported abortions were actually voluntary, even though induced abortion was illegal and Mexico had the strictest anti-abortion legislation in Latin America during the period under consideration.[21]

For our instruments to be valid, in addition to exogeneity, they have to satisfy the exclusion restriction assumption, i.e. fertility and infertility shocks must have an impact on children's migration only through sibship size. For this reason, in the child migration equation we control for many variables that may act as a confounding factor and those that may be affected by the shocks while having a direct effect on children's migration. Among

[20] According to Bongaarts and Potter (1983) overall spontaneous loss rates are about 20 percent of recognized pregnancies (i.e. one out of five). Casterline (1989) stresses that in most societies pregnancy losses produce a reduction of fertility of 5-10% from the levels expected in the absence of miscarriages and stillbirths.

[21] For women who voluntary have an abortion, the instrument would be endogenous. However, there is no evident sign in our data that a relevant share of the recorded abortions could be voluntary. For instance, Catholic women in our sample do not tend to abort significantly less than other women (this check can be done only for the 1997 wave, which includes information on religion): for the first group the incidence of abortion is 4.6 percent and for the second group is 4.8 percent. In case the instrument is substantially contaminated by voluntary abortions, we would expect IV estimates to be biased in the same direction as OLS. Indeed, omitting subscripts and in the models without controls, if we define as $M = \beta_0 + \beta_1 S + v$ the migration equation, where M and S are child migration status and sibship size, respectively, and $S = \gamma_0 + \gamma_1 Z + u$ the sibship size equation (the first stage) and Z the instrument (abortion), $\beta_{1,OLS} = \beta_1 + Cov(S,v)/Var(S)$ while $\beta_{1,IV} = \beta_1 + Cov(Z,v)/Cov(Z,S)$, where $Cov(Z,S) < 0$ and $sign(Cov(S,v)) = -sign(Cov(Z,v))$. In case, for instance, unobserved mother's total desired fertility is positively correlated with children's migration and a substantial share of abortions are voluntary, both OLS and IV will be similarly upward biased.

these variables, we include the mother's age, age at first pregnancy, education, chronic illness/disability, marital status and the husband's characteristics (age, education and absence).

Table 2 reports the incidence of infertility and miscarriage shocks in our (individual and household-level) estimation samples. Data clearly show a monotonic negative association between infertility and sibship size. For instance, while 13.4 percent and 11.4 percent of women with family sizes equal to one or two (i.e. sibship sizes zero or one), respectively, have experienced an infertility condition, the incidence falls to 3.5 percent for women with seven children or more. A negative relationship also emerges between miscarriage and sibship size, although it is not monotonic. In Figures 4 and 5 we report a preliminary visual representation of the relevance of our instruments (more compelling evidence is provided by the first-stage of the IVs reported in Section 5). In particular, we use ENADID data to plot the average number of live births by women's age and infertility shock and miscarriage status. [22] Figure 4 shows that women who ever experienced an infertility condition generally have a lower number of children, and those differences in fertility tend to increase with age. Similarly, Figure 5 displays a negative association between miscarriage at first pregnancy and the total number of live births. Both figures suggest that our instruments are relevant. They also suggest that, although the shocks we consider have a negative impact on family size, overall Mexican women were able to achieve a generally high fertility rate by the end of their fecund life span. This is due to the fact that exogenous infertility shocks, as defined in this and related papers, clearly affect the number of children a woman can have but they also may be temporary (i.e. secondary infertility) or treatable so that fertility may eventually be restored.

[Table 2 about here]

[Figure 4 and 5 about here]

[22] Older women belong to earlier birth cohorts, whose fertility is likely to be higher. For women below 45 years of age, instead, fertility may not be completed.

5 Results

5.1 Birth order effects

We start by estimating the impact of birth order on individual migration, as specified in equation (1), controlling for family fixed effects. The within-family estimator sweeps out all parental- and family-level heterogeneity, including completed family size. Moreover, family fixed effects account for omitted family-specific unobservable factors simultaneously affecting fertility and child migration. The first column of Table 3 reports estimates with a linear specification of birth order on the full sample, whereas in column (2) we allow for a more flexible specification by adding birth-order-specific dichotomous indicators. Regressions control for individual age and gender plus child cohort dummies (one for each year of birth).[23] Indeed, child age is correlated with birth order and it is also likely to have a (non-linear) relationship with migration (which is why we include the age quadratic term).

First, in column (1) we observe that, after controlling for family fixed effects, birth order and individual characteristics, females are significantly less likely to migrate than males by 3.6 percentage points (p.p.). Moreover, the birth order point estimate is negative and statistically significant. The effect starts to be economically significant from children of birth order 3, who are about 2.1 p.p. less likely to migrate than firstborns (column 2). Although this appears to be a small effect in absolute value, it represents an approximately 40 percent decrease in migration at the sample average (5.2 percent migration rate). The coefficients for the following birth orders are larger in absolute value and peak for birth orders 9 and 10 or more (-16.6 and -20 p.p. respectively).

In columns (3) and (4) we estimate the same regressions as above by adding interaction effects between birth order and gender to the models.[24] We observe a negative birth order gradient for boys (the coefficients on the third and higher parities are negative and significant), which is consistent with the average results above. The interactions of being female with birth order dummies are not statistically significant, suggesting that the birth-order gra-

[23] By including child age and cohort dummies, with household fixed effects, we are also de facto controlling for birth spacing between siblings.

[24] As our two-step procedure relies on family fixed effects, when estimating separate regressions by gender only families with at least two sons and at least two daughters can be included in the estimates for males and females, respectively. In order to avoid such a sample selection, we rather adopt a pooled estimation including interaction effects with gender.

dient in child migration is not statistically different between boys and girls. Yet, the latter holds for all parities but for firstborns: in column (4) the female main effect shows that female firstborns are significantly less likely to migrate than male firstborns. Overall, these estimates suggest that the chances of migration are not equally distributed across children within the same family. Low-parity children are in general more likely to migrate and this may be explained by the fact that, if migration is also a household-level investment strategy, the family will have more time to reap the benefits of migration. Yet, from our fidings a firstborn daughter is significantly less likely to migrate than a firstborn son by 3 p.p., which means a reduction in the probability of migration of roughly 60 percent at the sample average migration rate. We further explore these gendered effects in light of potential parental preferences below in Section 5.4.

[Table 3 about here]

5.2 Sibship size effect: WLS and 2SLS results at the individual-level

In this Section, we turn to the estimation of the sibship size effects. By applying the two-step procedure described above, we start by reporting WLS estimates as a benchmark model, where the dependent variable is 'netted migration' (see Section 4.1).[25] The number of siblings is tallied as the number of currently living biological brothers and sisters of each child.[26] The first column of Table 4 reports WLS results for a linear specification including sibship size. The highly significant coefficient implies that, on average and after controlling for birth order effects, adding one sibling is associated with a 1.1 p.p. higher likelihood of migrating for young adults (+17 percent at the sample mean). The same effect holds once we include individual level controls, namely child gender, age, age squared and years of birth indicators (column 2). When we allow for differential effects by child gender (column 3), the significant negative coefficient for the interaction term indicates that the female likelihood to migrate increases less due to sibship size than for males. Specifically, one extra sibling raises

[25]The inverse of the standard errors of 'netted migration' are used as weights.

[26]Those currently deceased are excluded from our definition of siblings. This is so because of two reasons: (i) 70 per cent of deceased children in our sample died before the first year of life, 90 per cent of them before the second one; (ii) the focus of our analysis is not on young children so that we need to take into account siblings who actually 'had enough time' to both receive and compete over household resources, so that can exclude infant deaths. In Appendix, we report robustness checks related to concerns about the endogeneity of our definition of sibship size and birth order based on ever-born children, i.e. currently alive or deceased.

the migration probability more for sons than for daughters by 0.8 p.p. In columns (4) to (7), we run the same regressions above while adding further parental, household and aggregate-level controls in order to account for potential confounding factors of the relationship between family size and offspring's migration. Specifically, in column (3) and (4) we include parental covariates, which may predict completed fertility and affect child migration, namely mother's years of birth indicators, age at first pregnancy, chronic illness, single status (i.e. widow, divorced, single de facto), father's decade of birth indicators, mothers' and father's (quadratic) age and years of schooling.[27] In column (5) and (6) we further add municipality fixed effects that, conditional on family size, control for rural vs. urban residence along with many other local factors related to different cultural or economic conditions, which may have an effect on fertility and migration (e.g., employment rates, migration intensity, access to contraception, social services etc). Overall, the sibiship size effect is essentially unchanged when we control for all of the aforementioned factors, and the same holds for the differential effect by gender.

[Table 4 about here]

Yet, as noted in the methodological section, the coefficients on sibship size reported in Table 4 are still likely to be biased, even when including a rich set of demographic and economic controls. This is so as fertility may be endogenous with respect to child out-migration. Thus we employ an IV approach and exploit the arguably exogenous fertility variation generated by episodes of infertility and miscarriage. Since these events can vary the actual family size from the desired one, we use infertility shocks and miscarriage at first pregnancy to identify the effect of sibship size on child out-migration. In Table 5 we present two-stage least squares (2SLS) estimates using a linear version of our 'saturated' specification (with controls) and the two-step methodology, as outlined above, to estimate equation (2). In column (1) we instrument sibship size with an indicator variable for infertility shocks taking value one if the woman declares she never used or she stopped using contraception because of infertility. In column (2), instead, we report results using a woman's experience of miscarriage on her first pregnancy as an instrument. Eventually, in column (3) we present results

[27]We are de facto also controlling for mother's age at delivery, which is a linear combination of child's age and mother's age. As far as parental controls are concerned, we have more missing information for fathers than it is the case for mothers. As to keep the sample size constant, we further include a dummy variable for missing information of fathers.

using both instruments in an over-identified equation model. Throughout all models, the first stage results point to a strong and highly significant relationship between infertility /fertility shocks and completed fertility. In particular, women who experienced an infertility shock have a reduction in their number of children of nearly 0.5 ($t = -5.2$) with an $F-$statistic of 26.9 (column 1). The negative impact of miscarriage on completed fertility is similar in magnitude (-0.437) with an $F-$statistic of 19.13 (column 2). Also the $F-$statistic of the joint significance of the instruments in the over-identified model is as high as 23.37 (column 3). The sibship size effects estimated using 2SLS are always small and statistically insignificant at standard confidence levels. For all models, the Anderson-Rubin $F-$statistic cannot reject that the coefficient of the instrument is zero in the reduced form, and the Hansen $J-$statistics confirms the validity of the instruments in the overidentified model. Interestingly, the point estimate of the effect of sibship size on child migration obtained with the abortion instrument (which might include voluntary abortions) is lower than the one obtained with the infertility instrument, which we consider to be much less (or not) affected by endogeneity issues, and much lower than the OLS estimate, a fact that is inconsistent with the premise that induced abortions comprise a substantial share of total abortions (cf. footnote 21).

[Table 5 about here]

In Table 6 we report results of the same 2SLS regressions as above while testing the sibship size differential effect by gender in the pooled sample with interaction terms.[28] Results do not point to any significant difference in the impact of sibiship size bewteen boys and girls, as it turns out to be insignificant for both (columns 1-3). When using miscarriage as an instrument, though, we cannot draw strong conclusions as the $F-$statistic for the interacted endogenous variable is rather low (4.27, column 2). However, even in this case the Anderson-Rubin $F-$statistic confirms that we cannot reject the hypothesis of sibship size not affecting child migration.

[Table 6 about here]

Overall, findings in this section point to the little role of family size on children's migration outcomes. This evidence is not in line with the popular view that high-fertility in

[28] The interaction effect sibship size*female is instrumented using the interaction *instrument**female, where the *instrument* is infertility or miscarriage depending on the specification.

developing countries is a major cause of international emigration: according to our estimates this correlation is driven by unobservable variables which make some families more prone to both have more children and send some of them abroad.

5.3 Robustness checks: Household-level estimates

In this section, we estimate the migration equation while using the household instead of the individual as the unit of analysis.[29] In so doing we are able to check the robustness of our baseline family size effect to changing the estimation sample and strategy. Indeed, the two-step procedure reported above is based on household fixed effects and therefore can only be applied to households with more than one child in the full sample. By contrast, while focusing on the total number of migrants in the household as a function of total fertility, we do not need to control for birth order effects and we can use a standard IV procedure. As a consequence, household-level regressions allow us to include also one-child households in the sample.[30] Thus, we estimate a specification as follows:

$$m_j = \gamma_0 + \gamma_1 n_j + \gamma_2 \mathbf{W}_j + v_j \qquad (3)$$

where the dependent variable is the number of children in the age range 15-25 who ever migrated in household j and the independent variable of interest is n_j, i.e. the total number of children in household j. The coefficient γ_1 captures the increase in the number of migrants associated with a unitary increase in the number of children. Like in the child-level estimates, $\mathbf{W}_j$ includes family background characteristics such as the mother's and the father's age, age squared, and years of completed education, mother's age at first pregnancy, an indicator for the father not being in the household and municipality fixed effects; v_j is an household-level error term. This specification is estimated both with OLS and with IVs (namely two-stage least squares).

[Table 7 about here]

Results are reported in Table 7. Column (1) shows that a unit increase in the number of

[29] More precisely, our unit of analysis is the biological family.

[30] Thus, in these estimates we also exploit individuals who do not have siblings, and look at whether they are more (less) likely to migrate than individuals with siblings.

children is associated with an average increase in the number of migrants in the household of 0.012 ($t = 11.1$). Computed at the average number of child migrants per household in the sample (0.075), this corresponds to a 16 percent increase. Column (2) reports the IV estimate using the infertility instrument. The first stage shows a reduction of -0.753 ($t = -12.1$) in the total number of children per woman who experienced an infertility shock, with an $F-$statistic of 145.4. The first-stage coefficient is a bit higher in magnitude than the one obtained in the child-level estimates (-0.5), probably because of the inclusion of one-child households in the estimation. In spite of the strength of the instrument, the second stage does not show any evidence of a positive effect of fertility on migration: the coefficient on the number of children turns out to be negative and statistically insignificant. Column (3) reports the IV results using the variation in the number of children generated by miscarriage. Also in this case the first-stage coefficient is highly statistically significant and negative, with an $F-$statistic of about 45. The negative impact of miscarriage on total fertility is smaller than the one exerted by infertility, yet it is quite large and precisely estimated, i.e. -0.476 ($t = -6.7$). Like for the previous instrument, also in this case no significant effect is detected in the second stage. The same happens in the overidentified model in column (4).

The household-level estimates in this section confirm the results of Section 5.2 of a positive correlation between family size and migration, but of no causal effect of the former on the latter. Also in this case, as with individual-level estimates, the larger magnitude of OLS estimates relative to the IV ones points to an upward biased estimation because of endogeneity, i.e. families more likely to send young migrants abroad tend to have more children.

5.4 Sibling gender composition

Our estimates so far show that gender is a robust predictor of migration and, *ceteris paribus*, boys – especially firstborns – are systematically more likely to migrate to the U.S. than girls in Mexican families. This points to a migration male-dominated phenomenon (e.g., Cerrutti and Massey 2001) that may be explained by (perceived) higher migration returns for boys (due to either higher expected wages abroad than at home or by lower moving costs for males with respect to females) or by a pure parental preference for sons. In practice, if migration is costly and not all children are in the position to migrate, a pro-son migration bias may lead to a situation in which children compete for household resources in order to migrate and such 'rivalry' can yield gains to having relatively more sisters than brothers (Garg and Morduch 1998). Thus, in order to explore the scope of sibling rivalry by gender, we test how sibling composition influences child migration investment by running two sets of regressions as reported in Table 8. First, we estimate migration equations on the full sample of children as a function of the number of their older brothers, while controlling for both family and birth order fixed effects (i.e., conditioning on the number of both siblings and older siblings), child gender, (quadratic) age and cohort dummies. Results in column (1) show that, *ceteris paribus*, having an older brother (sister) instead of an older sister (brother) decreases (increases) the migration probability by 1.4 p.p. ($t = 3.6$). This result points to a significant role of the gender and age composition of siblings in children's migration outcomes, which does not differ significantly by the child's gender (column 2).

[Table 8 about here]

Yet, we further exploit the gendered migration pattern and the fact that siblings are likely to migrate in order of birth (with higher parities being less likely to migrate, as shown by our former estimates in section 5.1) to test the hypothesis of parental son preference. We do so by including a control for having a next-born brother in the family fixed effects regressions on the pooled-sample (with and without interactive effects), as above. If a child has at least one younger sibling, the gender of his/her next-born sibling is random and a comparison of children with next-born brothers with children with next-born sisters, while controlling for older siblings composition, can identify the effect of the sibling's gender.[31] Results in columns (3) and (4) in Table 8 show that, conditional on older siblings' composition, having a next-born brother does not play any role for sons, but reduces the likelihood to migrate for girls with respect to boys by 1.2 p.p. ($t = -2$). This result suggests that when parents decide the level of investment in their children's out-migration, the siblings' composition by gender and age matters. More specifically, from our results it seems that a daughter with a next-born brother may be less likely to migrate than a girl with a next-born sister. In other words, when parents face the decision whether to send a daughter abroad, they seem to prefer to invest

[31] A similar empirical strategy has been used in Vogl (2013) to study sibling rivalry over arranged marriages in South Asia.

in the migration of her next-born brother. These results are in line with other evidence from developing countries that, when there are high returns to investing in the human capital of children but resources are limited, children may become rivals (even in the absence of any explicit strategic behavior on the part of any family member) and typically girls turn out to be disadvantaged when they compete with boys (Dunn and Plomin 1990, Kristin and Anne 1994, Morduch 2000). Indeed, our findings are suggestive that children, especially girls, with relatively more brothers than sisters are less likely to migrate abroad than their peers.

These results, combined with the birth order effects reported above, are consistent with the argument that a low-parity Mexican boy may be more valuable to send as a migrant abroad than a girl. Indeed, labor market returns for Mexican boys in the U.S. were relatively higher in the 1990s (e.g., in the farm sector). In addition, the opportunity cost of sending girls abroad may be higher because they usually take care of chores and family duties at home or are in charge of being close to parents in their elderly age. Hence, social norms or practices combined with market returns on the migration investment may explain the pro-male biased pattern of mass Mexico-U.S. migration and document, similarly to other developing contexts, that young females tend to have less access to human capital investment and enhancing economic opportunities than it is the case for males.

6 Conclusions

In this paper we provide novel and rigorous evidence on the extent to which international labor mobility is affected by the demographic characteristics of the migrant's household. Migration is largely a youth phenomenon that occurs in households that never dispatch all of their children to work abroad. With capital market imperfections and high migration costs, the 'resource dilution' hypothesis predicts that a larger sibship size will decrease the chances of offspring's migration. Yet, in relatively poor contexts, parents are likely to depend on their grown children for the provision of care and income, and high rates of migration can significantly contribute to the living arrangements of elderly parents.

We use a rich household-survey dataset on teenagers and young adults to examine the causal effects of sibship size, birth order and sibling composition on migration outcomes in Mexico. Mexican migration, mainly to the U.S., is an enduring flow that account for one third of total U.S. immigration and one-tenth of the entire population born in Mexico. Importantly, migration patterns differ by age and gender, with a significant fraction of Mexican males migrating between the ages of 15 and 30.

We focus on the determinants of adolescents' and young adults' migration in Mexico. Our large dataset allows us to overcome the limitations of small samples of children, and it includes detailed information on both women's fertility and the migration histories of household members'. We find little evidence that fertility has a causal impact on migration. The positive correlation between fertility and migration disappears when the potential endogeneity of sibship size is addressed using biological fertility and infertility shocks. On the other hand, we find differences in the chances of migration between siblings within the same family (sibling rivalry). Older siblings, especially firstborn males, are more likely to migrate, while having relatively more sisters than brothers systematically increases the likelihood to migrate. Moreover, girls are less likely to migrate when their next parity is a male. This is consistent with the argument that, in resource-scarce contexts, girls' migration can be viewed as less economically rewarding and more socially costly to parents, with the result that boys end up having more economic opportunities than girls, even through migration.

Our findings contribute to the migration literature by shedding new light on the role of the family in determining international migration choices. Labor mobility, especially from poor to rich settings, is one of the most important ways through which young adults can expand their human capital and earning potentials. The type of family-based migration from Mexico to the U.S. during the 1990s is of substantial and growing importance for many other developing countries (e.g., in Asia and Africa) that are currently affected by both high fertility rates and international migration (e.g., Hatton and Williamson 2003). Despite the easily observable association between fertility rates and migration, we provide evidence that large families are unlikely to be a systematic driver of migration. This finding is in line with recent evidence showing that high fertility in developing contexts is not necessarily bad for children's economic outcomes (e.g., Qian 2009). In terms of policy, understanding the link between fertility and migration is especially relevant today, since many governments in developing countries have attempted to curb population growth as a means of increasing the average human capital investment and possibly reducing migration (e.g., China and India, the world's two most populous countries, have experimented with different family planning policies to control family size). Yet, although our empirical findings do not point to a causal link between fertility and migration, they hint to the fact that parental investment in offspring's migration may matter for lifetime fertility choices. This is so as in a context of poor resources and weak institutional safety nets, children may be a key social security valve for parents such that high migration opportunities to rich countries increase the value of having children. Hence, effective safety and welfare measures (such as old age pensions) or even the development of credit and insurance markets may lead to a reduction in both migration and fertility, and also perhaps a lesser gender gap.

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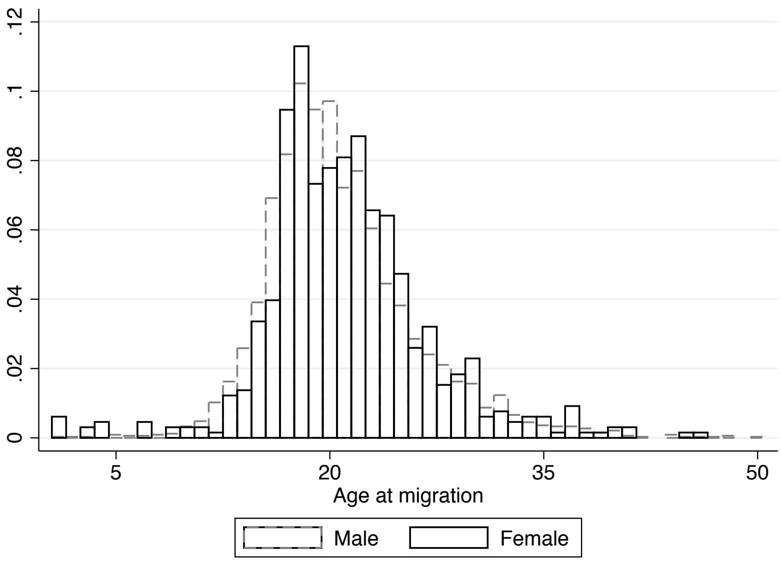
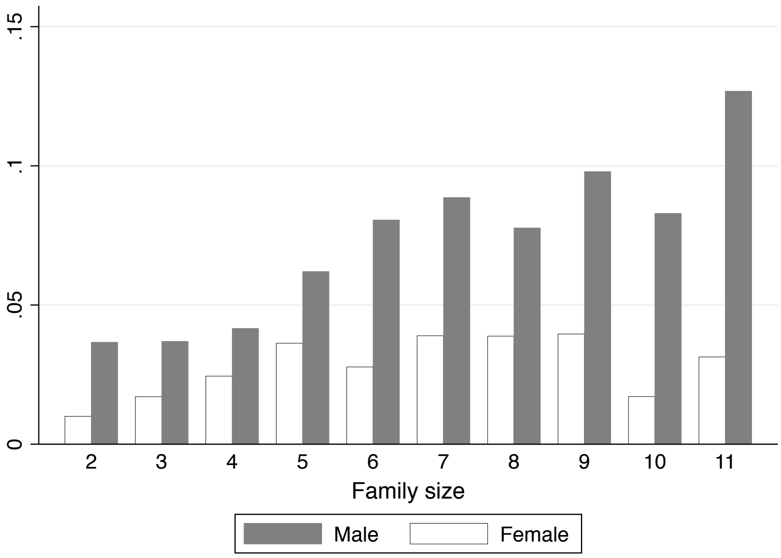
Figure 1: Mexican individual (non-tied) migration by age and gender



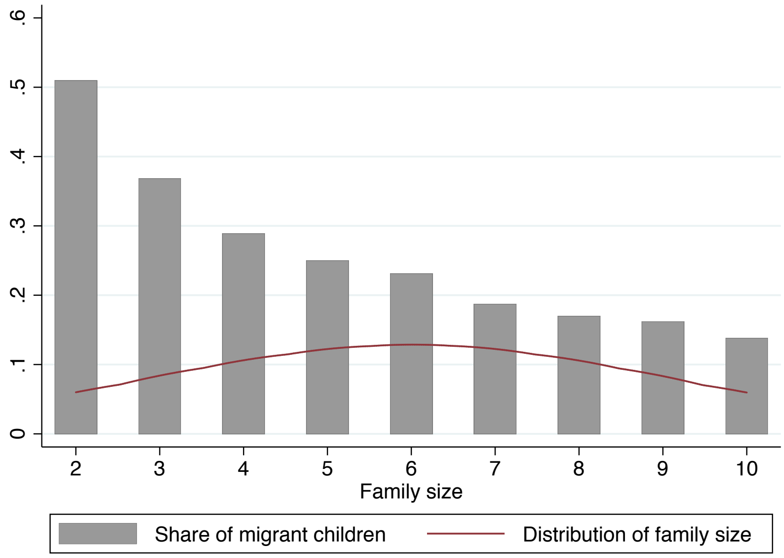
Source: Our computations on ENADID, 1992 and 1997.

Figure 2: Individual migration rate by family size



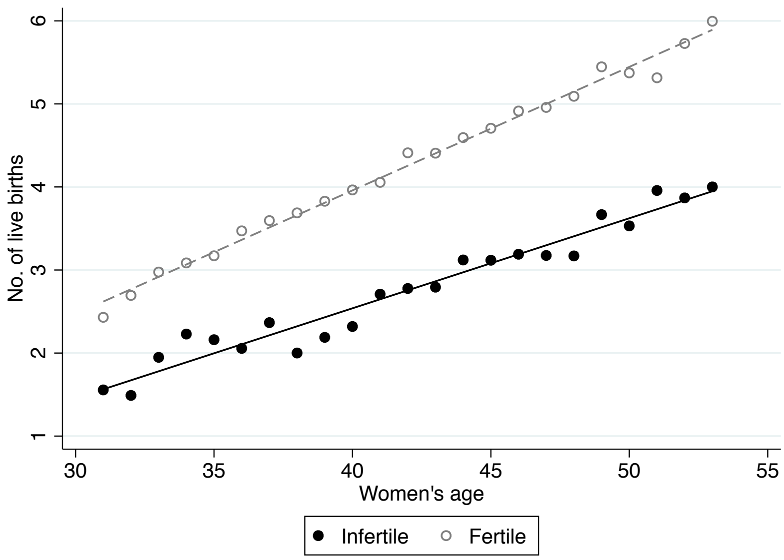
Source: Our computations on ENADID, 1992 and 1997.

Figure 3: Ratio of migrant children by family size



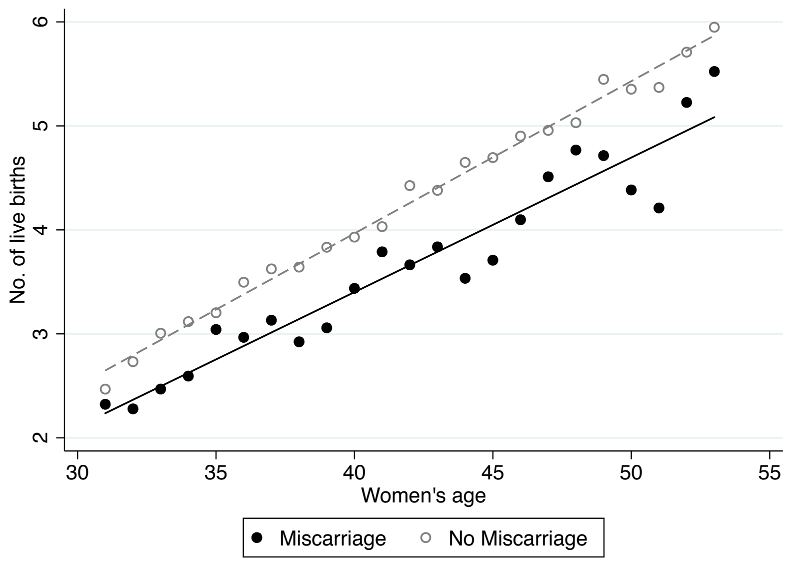
Source: Our computations on ENADID, 1992 and 1997 (subsample of households with migrants).

Figure 4: Average number of children by women's infertility shock status



Note. Source: ENADID, 1992 and 1997. The figure reports the average number of live births by women's infertility shock status and age. Regression lines are super-imposed to the cross-plot.

Figure 5: Average number of children by women's miscarriage at first pregnancy



Note. Source: ENADID, 1992 and 1997. The figure reports the average number of live births by miscarriage at first pregnancy and age. Regression lines are super-imposed to the cross-plot.

Table 1: Sample characteristics by migration status

	Non-migrants (A)	Migrants (B)	$p-$values (A)-(B)
Individual-level characteristics			
Age	18.878	20.982	0.000
Female	0.458	0.250	0.000
N. of siblings	5.071	5.869	0.000
Birth order 1	0.181	0.192	0.300
Birth order 2	0.231	0.225	0.555
Birth order 3	0.178	0.178	0.978
Birth order 4	0.137	0.154	0.077
Birth order 5	0.102	0.102	0.993
Birth order 6	0.071	0.073	0.781
Birth order 7	0.046	0.041	0.343
Birth order 8	0.028	0.021	0.100
Birth order 9	0.014	0.009	0.121
Birth order 10+	0.011	0.006	0.107
Household-level characteristics			
Mother's age	44.612	46.171	0.000
Mother's age at first pregnancy	20.030	19.699	0.182
Mother's years of schooling	4.091	3.452	0.010
Mother chronic illness	0.023	0.008	0.131
Single mother	0.185	0.188	0.896
Mother's labor income	600.403	862.540	0.037
Father's age	48.799	52.207	0.000
Father's years of schooling	4.931	3.789	0.059

Note. Source: ENADID, 1992 and 1997. The estimation sample includes individuals aged 15–25 whose mothers are not using contraceptive methods. The sample comprises 1,394 migrants and 25,349 non-migrant individuals.

Table 2: Incidence of fertility and infertility shocks by sibship size

Individual sample				Household sample			
		Incidence of shock (%)				Incidence of shock (%)	
sibship size	%	infertility	miscarriage	sibship size	%	infertility	miscarriage
				0	3.69	13.37	5.05
1	4.59	11.56	6.03	1	9.84	11.43	6.80
2	12.16	8.33	5.38	2	16.88	7.48	5.20
3	14.20	5.45	4.06	3	15.96	5.16	4.02
4	14.68	5.12	4.05	4	13.66	4.30	3.86
5	13.54	4.00	5.55	5	11.20	3.73	4.71
6+	40.82	3.94	3.68	6+	28.76	3.51	3.44
	100.00				100.00		

Note. The table reports the incidence of fertility and infertility shocks in the estimation samples used in the individual-level (see Section 5.2) and the household-level analysis (see Section 5.3), respectively.

Table 3: Birth order effects

Variables	(1)	(2)	(3)	(4)
female	-0.036***	-0.035***	-0.032***	-0.031***
	(0.003)	(0.003)	(0.006)	(0.007)
birth order	-0.019***		-0.019***	
	(0.003)		(0.003)	
birth order × female			-0.001	
			(0.001)	
birth order 2		-0.002		0.002
		(0.005)		(0.006)
birth order 3		-0.021***		-0.023***
		(0.007)		(0.008)
birth order 4		-0.038***		-0.034***
		(0.010)		(0.011)
birth order 5		-0.068***		-0.070***
		(0.013)		(0.014)
birth order 6		-0.086***		-0.077***
		(0.016)		(0.017)
birth order 7		-0.112***		-0.103***
		(0.019)		(0.020)
birth order 8		-0.136***		-0.140***
		(0.022)		(0.023)
birth order 9		-0.161***		-0.166***
		(0.026)		(0.028)
birth order 10+		-0.199***		-0.188***
		(0.030)		(0.033)
birth order 2, female				-0.011
				(0.009)
birth order 3, female				0.005
				(0.010)
birth order 4, female				-0.010
				(0.010)
birth order 5, female				0.006
				(0.011)
birth order 6, female				-0.018
				(0.012)
birth order 7, female				-0.017
				(0.015)
birth order 8, female				0.010
				(0.018)
birth order 9, female				0.012
				(0.024)
birth order 10+, female				-0.022
				(0.027)
age	0.020**	0.021**	0.020**	0.021**
	(0.009)	(0.009)	(0.009)	(0.009)
age squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Year of birth indicators	YES	YES	YES	YES
Family fixed effects	YES	YES	YES	YES
Observations	26,743	26,743	26,743	26,743
R-squared	0.050	0.052	0.050	0.053

Note. The dependent variable is a dichotomous indicator of the child's migration status. The model is estimated using OLS. Sibship size is absorbed by family fixed effects. Standard errors clustered at the household level in parentheses. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table 4: Sibship size effect: WLS estimates

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
N. siblings	0.011***	0.011***	0.014***	0.010***	0.013***	0.010***	0.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N. siblings $\times$ female[(a)]			-0.008***		-0.007***		-0.006***
			(0.001)		(0.001)		(0.001)
female		-0.038***	-0.036***	-0.033***	-0.031***	-0.033***	-0.031***
		(0.003)	(0.006)	(0.003)	(0.006)	(0.003)	(0.003)
Individual's controls	NO	YES	YES	YES	YES	YES	YES
Mother's controls	NO	NO	NO	YES	YES	YES	YES
Father's controls	NO	NO	NO	YES	YES	YES	YES
Municipality indicators	NO	NO	NO	NO	NO	YES	YES
Observations	26,743	26,743	26,743	26,743	26,743	26,743	26,743
R-squared	0.013	0.054	0.055	0.177	0.178	0.202	0.203

Note. The dependent variable is *netted migration* (see Section 4). The model is estimated using Weighted Least Squares (weights are the inverse of the standard errors of *netted migration*). Individual's controls include year of birth indicators, age, age squared; mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. Standard errors clustered at the household level in parentheses. [(a)] The number of siblings is demeaned before taking the interaction. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table 5: Sibliship size effect: 2SLS estimates

Variables	(1)	(2)	(3)
Second stage			
N. siblings	0.004	-0.018	-0.005
	(0.014)	(0.023)	(0.012)
female	-0.033***	-0.033***	-0.033***
	(0.003)	(0.003)	(0.003)
IV:	infertility	miscarriage	overidentified
Anderson-Rubin $F-$statistic	0.073	0.686	0.389
	[0.787]	[0.407]	[0.678]
Hansen $J-$statistic			0.737
			[0.391]
First stage — N. siblings			
infertility	-0.494***		-0.491***
	(0.095)		(0.095)
miscarriage		-0.437***	-0.433***
		(0.10)	(0.10)
Angrist-Pischke $F-$statistic instrument(s)	26.90	19.13	23.37
Individual's controls	YES	YES	YES
Mother's controls	YES	YES	YES
Father's controls	YES	YES	YES
Municipality indicators	YES	YES	YES
Observations	26,743	26,743	26,743

Note. The dependent variable is *netted migration* (see Section 4). Observations are weighted by the inverse of the standard error of *netted migration*. Individual's controls include year of birth indicators, age, age squared; mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. Standard errors clustered at the household level in parentheses. $P-$values are reported in brackets. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table 6: Child gender and sibship size effect: 2SLS estimates

Variables	(1)	(2)	(3)
Second stage			
N. siblings	0.005	-0.065	-0.007
	(0.016)	(0.048)	(0.015)
N. siblings $\times$ female$^{(a)}$	-0.005	0.112	0.005
	(0.013)	(0.079)	(0.013)
female	-0.032***	-0.064***	-0.034***
	(0.004)	(0.022)	(0.005)
IV:	infertility	miscarriage	overidentified
Anderson-Rubin $F-$statistic	0.074	2.210	1.150
	[0.928]	[0.110]	[0.331]
Hansen $J-$statistic			4.399
			[0.111]
First stage — N. siblings			
infertility	-0.567***		-0.564***
	(0.109)		(0.108)
infertility $\times$ female	0.168		0.169
	(0.115)		(0.115)
miscarriage		-0.453***	-0.450***
		(0.117)	(0.117)
miscarriage $\times$ female		0.037	0.038
		(0.106)	(0.105)
Angrist-Pischke $F-$statistic instrument(s)	28.62	11.98	15.68
First stage — N. siblings $\times$ female			
infertility	0.125***		0.125***
	(0.038)		(0.038)
infertility $\times$ female	-0.694***		-0.691***
	(0.131)		(0.131)
miscarriage		-0.067	-0.068
		(0.044)	(0.043)
miscarriage $\times$ female		-0.261**	-0.254*
		(0.131)	(0.130)
Angrist-Pischke $F-$statistic instrument(s)	26.93	4.27	13.83
Individual's controls	YES	YES	YES
Mother's controls	YES	YES	YES
Father's controls	YES	YES	YES
Municipality indicators	YES	YES	YES
Observations	26,743	26,743	26,743

Note. The dependent variable is *netted migration* (see Section 4). Observations are weighted by the inverse of the standard error of *netted migration*. Individual's controls include year of birth indicators, age, age squared; mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. Standard errors clustered at the household level in parentheses. $P-$values are reported in brackets. $^{(a)}$ The number of siblings is demeaned before taking the interaction. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table 7: Family size effect: Household-level estimates

Variables	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
Second stage				
N. children	0.012***	0.001	-0.016	-0.003
	(0.001)	(0.010)	(0.020)	(0.009)
IV:	—	infertility	miscarriage	overidentified
Anderson-Rubin $F-$statistic		0.008	0.640	0.324
		[0.928]	[0.424]	[0.723]
Hansen $J-$statistic				0.580
				[0.446]
First stage — N. children				
infertility		- 0.753***		-0.750***
		(0.062)		(0.062)
miscarriage			-0.476***	-0.469***
			(0.071)	(0.071)
Angrist-Pischke $F-$statistic instrument(s)		145.42	44.95	96.17
Mother's controls	YES	YES	YES	YES
Father's controls	YES	YES	YES	YES
Municipality indicators	YES	YES	YES	YES
Observations	18,217	18,217	18,217	18,217

Note. The dependent variable is a dummy for the child's migration status. Mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. $P-$values are reported in brackets. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table 8: Siblings' composition effect: OLS estimates

Variables	(1)	(2)	(3)	(4)
N. older brothers	-0.014***	-0.014***	-0.017***	-0.016***
	(0.004)	(0.004)	(0.004)	(0.005)
female	-0.028***	-0.026***	-0.022***	-0.016***
	(0.003)	(0.005)	(0.005)	(0.006)
N. older brothers × female		-0.002	-0.002	-0.003
		(0.002)	(0.002)	(0.002)
Next brother			-0.005	0.001
			(0.004)	(0.004)
Next brother × female				-0.012**
				(0.006)
Age, age squared	YES	YES	YES	YES
Birth order fixed effects	YES	YES	YES	YES
Year of birth indicators	YES	YES	YES	YES
Family fixed effects	YES	YES	YES	YES
Observations	26,743	26,743	26,743	26,743
Number of hid	10,139	10,139	10,139	10,139
R-squared	0.053	0.053	0.053	0.053

Note. The dependent variable is a dichotomous indicator of the child's migration status. The model is estimated using OLS. Sibship size is absorbed by family fixed effects. Standard errors clustered at the household level in parentheses. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

A Appendix: Sibship size including deceased children

Table A1: Birth order effects

Variables	(1)	(2)	(3)	(4)
female	-0.035***	-0.035***	-0.031***	-0.031***
	(0.003)	(0.003)	(0.005)	(0.007)
birth order	-0.017***		-0.016***	
	(0.003)		(0.003)	
birth order×female			-0.001	
			(0.001)	
birth order 2		-0.003		0.001
		(0.005)		(0.006)
birth order 3		-0.013*		-0.016*
		(0.007)		(0.008)
birth order 4		-0.031***		-0.030***
		(0.010)		(0.011)
birth order 5		-0.057***		-0.055***
		(0.012)		(0.013)
birth order 6		-0.076***		-0.070***
		(0.015)		(0.016)
birth order 7		-0.091***		-0.081***
		(0.017)		(0.018)
birth order 8		-0.120***		-0.116***
		(0.020)		(0.022)
birth order 9		-0.133***		-0.143***
		(0.023)		(0.025)
birth order 10+		-0.164***		-0.158***
		(0.027)		(0.029)
birth order 2, female				-0.010
				(0.010)
birth order 3, female				0.006
				(0.010)
birth order 4, female				-0.002
				(0.011)
birth order 5, female				-0.003
				(0.011)
birth order6, female				-0.013
				(0.012)
birth order 7, female				-0.023*
				(0.014)
birth order 8, female				-0.007
				(0.016)
birth order 9, female				0.022
				(0.019)
birth order 10+, female				-0.012
				(0.019)
age	0.022**	0.022**	0.022**	0.022**
	(0.009)	(0.009)	(0.009)	(0.009)
age squared	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Year of birth indicators	YES	YES	YES	YES
Family fixed effects	YES	YES	YES	YES
Observations	26,743	26,743	26,743	26,743
Number of households	10,139	10,139	10,139	10,139
R-squared	0.050	0.052	0.050	0.052

Note. The dependent variable is a dichotomous indicator of the child's migration status. The model is estimated using OLS. Sibship size is absorbed by family fixed effects. Standard errors clustered at the household level in parentheses. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table A2: Sibship size effect: WLS estimates

Variables	(1)	(2))
N. siblings	0.010***	0.013***
	(0.001)	(0.001)
N. siblings $\times$ female[a]		-0.007***
		(0.001)
female	-0.032***	-0.030***
	(0.003)	(0.003)
Individual's controls	YES	YES
Mother's controls	YES	YES
Father's controls	YES	YES
Municipality indicators	YES	
Weighted	YES	YES
Observations	26,743	26,743
R-squared	0.204	0.206

Note. The dependent variable is *netted migration* (see Section 4). The model is estimated using Weighted Least Squares (the weights are the inverse of the standard errors of *netted migration*). Individual's controls include year of birth indicators, age, age squared; mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. Standard errors clustered at the household level in parentheses. [a] The number of siblings is demeaned before taking the interaction. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table A3: Sibship size effect: 2SLS estimates

Variables	(1)	(2)	(3)
Second stage			
N. siblings	0.002	-0.015	-0.005
	(0.014)	(0.024)	(0.013)
female	-0.032***	-0.033***	-0.032***
	(0.003)	(0.003)	(0.003)
IV:	infertility	miscarriage	overidentified
Anderson-Rubin $F-$statistic	0.0229	0.433	0.232
	[0.880]	[0.510]	[0.793]
Hansen $J-$statistic			0.419
			[0.517]
First stage — N. siblings			
infertility	-0.475***		-0.472***
	(0.095)		(0.095)
miscarriage		-0.411***	-0.407***
		(0.10)	(0.10)
Angrist-Pischke $F-$statistic instrument(s)	25.01	17.78	21.70
Individual's controls	YES	YES	YES
Mother's controls	YES	YES	YES
Father's controls	YES	YES	YES
Municipality indicators	YES	YES	YES
Observations	26,743	26,743	26,743

Note. The dependent variable is *netted migration* (see Section 4). Observations are weighted by the inverse of the standard error of *netted migration*. Individual's controls include year of birth indicators, age, age squared; mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. Standard errors clustered at the household level in parentheses. $P-$values are reported in brackets. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.

Table A4: Child gender and sibliship size effect: 2SLS estimates

Variables	(1)	(2)	(3)
Second stage			
N. siblings	0.002	-0.065	-0.006
	(0.016)	(0.057)	(0.015)
N. siblings $\times$ female$^{(a)}$	-0.001	0.105	0.007
	(0.013)	(0.084)	(0.013)
female	-0.032***	-0.059***	-0.034***
	(0.004)	(0.022)	(0.004)
IV:	infertility	miscarriage	overidentified
Anderson-Rubin $F-$statistic	0.0115	1.577	0.797
	[0.989]	[0.207]	[0.527]
Hansen $J-$statistic			2.925
			[0.232]
First stage — N. siblings			
infertility	-0.557***		-0.555***
	(0.107)		(0.107)
infertility $\times$ female	0.168		0.190*
	(0.115)		(0.114)
miscarriage		-0.390***	-0.387***
		(0.113)	(0.113)
miscarriage $\times$ female		0.046	-0.046
		(0.106)	(0.102)
Angrist-Pischke $F-$statistic instrument(s)	31.59	5.74	19.49
First stage — N. siblings $\times$ female			
infertility	0.135***		0.136***
	(0.039)		(0.038)
infertility $\times$ female	-0.689***		-0.686***
	(0.134)		(0.134)
miscarriage		-0.066	-0.068
		(0.044)	(0.044)
miscarriage $\times$ female		-0.287**	-0.280**
		(0.131)	(0.130)
Angrist-Pischke $F-$statistic instrument(s)	40.51	4.55	17.22
Individual's controls	YES	YES	YES
Mother's controls	YES	YES	YES
Father's controls	YES	YES	YES
Municipality indicators	YES	YES	YES
Observations	26,743	26,743	26,743

Note. The dependent variable is *netted migration* (see Section 4). Observations are weighted by the inverse of the standard error of *netted migration*. Individual's controls include year of birth indicators, age, age squared; mother's controls include year of birth indicators, age and age squared, age at first pregnancy, years of schooling, indicators for mother's chronic illness and being single; father's controls include decade of birth indicators, age and age squared, years of schooling. Standard errors clustered at the household level in parentheses. $P-$values are reported in brackets. $^{(a)}$ The number of siblings is demeaned before taking the interaction. *,** and *** denote statistical significance at 10, 5 and 1 percent level, respectively.