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ORIGINAL PAPER

# The impact of family size and sibling structure on the great Mexico–USA migration

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## Abstract
We investigate the impact of fertility and demographic factors on the Great Mexico–USA immigration by assessing the causal effects of sibship size and structure on migration decisions within the household. We use a rich demographic survey on the population of Mexico and exploit presumably exogenous variation in family size induced by biological fertility and infertility shocks. We further exploit cross-sibling differences to identify the effects of birth order, siblings' sex, and siblings' ages on migration. We find that large families per se do not boost offspring's emigration. However, the likelihood of migrating is not equally distributed within a household. It is higher for sons and decreases sharply with birth order. The female migration disadvantage also varies with sibling composition by age and gender.

**Keywords** International migration · Mexico · Family size · Sibling structure

**JEL Classification** J13 · F22 · O15

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## 1 Introduction

Migration from poor to rich countries is a key human capital investment with a significant impact on the earnings and well-being of both migrants and family members left behind. Yet, international migration is largely a youth phenomenon, involving *upfront* monetary costs usually borne by individuals and their origin families. High fertility, which translates into a large number of siblings, may alter a household's ability to pay for this investment. This paper examines the role of the size and the structure of the childhood's household—in particular the role of siblings—on migration investment decisions. To this end, we assess whether sibship size has a causal impact on the individual probability to migrate abroad. We further assess whether sibling structure—birth order and sibling composition by age and gender—plays any role in the migration decision within a household.

High fertility and the resultant demographic pressure are generally recognized to be an important driver of international migration flows. Previous studies have shown that by increasing population cohort sizes, high fertility boosts emigration (Hanson and McIntosh 2010, 2016). The main mechanism addressed in the literature is shocks to labor supply, which deteriorate domestic labor market opportunities and increase the incentive for migrating abroad (Borjas 1987; Hanson 2004). These studies focus on the effect of high demographic pressure on the returns to migration, i.e., on general equilibrium effects in the labor market. This paper adds to that literature by empirically assessing the household-level effect of high fertility on international migration choices, focusing on the impact of "family size" (the number of siblings) and sibling structure. This is an important gap to fill in the literature, since migrants are typically young and male, come from high-fertility countries, and often leave siblings behind (Hatton and Williamson 2003).[1]

Our paper contrasts with previous studies that, in using an aggregate, or cohort-level, approach, are likely to confound the effect of high fertility and larger cohort sizes on returns to migration (general equilibrium effects) with its effect on the ability of households to invest in migration (household-level effect). Disentangling the two effects is important in order to better understand the implications of population growth for migration. Indeed, while a decline in fertility rates, and thus less pressure on the labor market, is likely to reduce migration, it can simultaneously change the allocation of resources among household members.[2]

High fertility may decrease migration at the household level by diluting the per capita resources available to pay for it or because more family-work is needed at home, such as caring for younger children (Becker and Lewis 1973). On the other hand, in order to keep pace with higher livelihood needs, large families may spur investment in the form of migrating household members. Importantly, given the

[1] To date, the household-level literature has mainly focused on the determinants of family migration investigating network effects (Winters et al. 2001; Stöhr 2015), the effect of the number of children on parental migration (Lindstrom and Saucedo 2007; Sarma and Parinduri 2015), and the effect of migration on fertility (Mayer and Riphahn 2000; Lindstrom and Saucedo 2002).

[2] The degree to which household structure, rather than the performance of the aggregate economy, influences intra-family resource allocation in developing contexts has been documented by several seminal studies such as Rosenzweig (1988) and Rosenzweig and Stark (1989).

costly and selective nature of international migration, household demographics may determine who is enabled to leave within the family (e.g., Chen 2006). This is because the returns to migration (and perhaps some of the costs) accrue over a period of time and may depend on a child's own characteristics as well as those of his/her siblings. Indeed, in the context of limited resources and high returns to the migration investment, siblings may become rivals and some children (for instance, girls, or later borns) may have fewer economic opportunities than their siblings (Garg and Morduch 1998; Black et al. 2005; Jayachandran and Kuziemko 2011).[3]

We address this question in the context of the Great Mexico–USA migration over the last two decades of the twentieth century. Mexico is a highly populated country that, during a demographic boom, experienced a period of mass migration to the USA, which gradually weakened over the years. As reported in several studies using decennial US Census throughout the twentieth century, Mexico–USA migration swelled in the 1970s and continued to grow in the 1980s and 1990s, ranging from 5.2% of Mexico's national population in 1990 to a peak of 10.2% in 2005 (Hanson and McIntosh 2010).[4] Importantly, emigration patterns differed by age and gender, with a dominant share of young males.[5] In 1970, Mexico's fertility rate stood at about seven children per woman (Cabrera 1994). The gradual spread of family planning practices contributed to a fertility transition and by 2005, the number of children per woman had declined to slightly more than two. Despite abundant evidence on the potentially significant implications of high fertility rates for child investment and economic outcomes, there is no systematic evidence on the impact of family size on migration decisions.

Using two waves of a large and nationally representative demographic household survey in Mexico, we focus on the determinants of migration for young adults aged 15 to 25. A crucial element in our dataset is the inclusion of information on

[3] A well-established theoretical literature in economics rationalizes a causal link between children's economic resources and their lifetime opportunities and adult outcomes (Becker and Tomes 1976; Thomas 1990). A related stream of empirical studies has investigated the role of family size, birth order, and sibling composition (by age and gender) on household investments in other forms of children's human capital, such as health and education (see Black et al. 2005; Jayachandran and Kuziemko 2011; Jayachandran and Pande 2017, among others). In general, findings point to the little role of family size on children's outcomes, while sibling structure and composition have more significant effects on offspring human capital investment.

[4] To put the Great Mexican migration in historical perspective, it is worth noting that "as a share of Mexico's national population, the number of Mexican immigrants living in the USA remained at 1.5% from 1960 to 1970, before rising to 3.3% in 1980, 5.2% in 1990, and 10.2% in 2005" (see Hanson and McIntosh 2010, p.1). In terms of absolute numbers, it is estimated that about 7 million Mexican immigrants entered the USA in the 1990s, 2.2 million did it legally while 4.8 million entered illegally (see Borjas and Katz 2007; Card and Lewis 2007). As a result, the Mexican-born population residing in the USA in 2000 was nearly 9.2 million, accounting for one-third of the US foreign-born population. Hence, the Mexican-born population of the late twentieth century appears historically unprecedented, being both numerically and proportionately larger than any other immigrant influx in the former and following century (Passel et al. 2012).

[5] Using population censuses, Hanson and McIntosh (2010) report (in Fig. 2) that a significant proportion of men in Mexico start migrating around age 15, with emigration increasing sharply until approximately age 30 and decreasing thereafter, presumably as a result of return migration. By contrast, there is less youth migration among Mexican women and migration rates are relatively stable over the course of their lives.

women's completed fertility history, and hence on the total number of biological siblings ever born into a family, along with individual migration histories. Access to such information is rare, and perhaps unique, in the migration literature, particularly in combination with a large sample of young adults and the availability of sources of plausibly exogenous variation in their mothers' fertility.[6]

We find little evidence that high fertility drives migration choices at the household level. The positive correlation between the number of siblings and migration vanishes when the potential endogeneity of sibship size is addressed by leveraging presumably exogenous variation in women's biological fertility and infertility shocks. However, the likelihood of migrating is not equally distributed across children within a family. We find that older siblings, especially firstborn sons, are more likely to migrate, while having more sisters than brothers may increase the chances of migration, particularly among females. This may be because investing in low parity children lengthens the period of time over which the family expects to reap the benefits of having a migrant child, hence maximizing net returns (for instance, older siblings "cater" for the younger siblings). In addition, a son may be more valuable to send abroad as a migrant than a daughter, given unequal labor market opportunities for different genders. Indeed, labor market returns for Mexican men in the USA were relatively high in the 1990s (for instance, in the agriculture sector) compared to women, who were more likely to be responsible for chores and family duties at home and also more exposed to (sexual) violence at the destination. Overall, our results on the effects of sibling composition highlight the importance of the migrant's family of origin in driving migration decisions and are consistent with a household's optimal migration model in which mobility is an investment in the human agent, but private costs and rewards involve both migrants and non-migrant household members (Sjaastad 1962; Stark 1991).

Our findings have implications for both policy makers and researchers. Many observers highlight the importance of demographic pressures in migration flows from today's developing countries, including Africa. As argued by Hanson and McIntosh (2016) while discussing the contribution of differentials in population growth to international migration in the long run, "the European immigration context today looks much like the United States did three decades ago" (p. 2). The main reason for this lies in the socio-demographic features of both source and host countries, namely low living standards and high population pressure in both Africa today and Mexico in the past, and high income and low fertility in both destinations (Europe and the USA). Our analysis provides evidence of the first-order effects of decreasing fertility on migration choices at the household level. In particular, we show that an exogenous decrease in the number of children does not necessarily change the number of migrants in a family. This finding suggests that fertility-reducing programs may have

[6]Costs and benefits of migration may be unevenly distributed across both families and siblings within family, and hence bias the results. Moreover, 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 parental 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.

little impact on migration decisions at the household level. Such programs have been endorsed in many developing countries as a policy response to the apparent vicious circle of high-fertility, poverty, and economic stagnation (Schultz 2008; Miller and Babiarz 2016). On the other hand, welfare policies such as pension schemes or old-age support measures may reduce the incentives that push a household member, typically the firstborn son, to migrate abroad. Finally, by showing that not all children in the household have the same likelihood of migrating, our results point to the existence of an intra-household selection process that may have important implications in terms of individual (child) welfare and income distribution.

The paper unfolds as follows. Section 2 presents the data and sample selection. The methodology and empirical strategy are described in Section 3. Section 4 presents our main results on sibship size effects on migration while Section 5 reports results on the role of birth-order, gender, and sibling composition. Finally, Section 6 summarizes our main findings and concludes.

## 2 Data and sample selection

This study uses data from the 1992 and 1997 waves of the *Encuesta Nacional de la Dinámica Demográfica* (ENADID), a cross-section survey conducted by the National Institute of Statistics and Geography (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 the 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 (including those not currently living in the household of origin).

The ENADID collects detailed information on fertility for all women aged 15 to 54 at the time of the survey. Women answer specific questions regarding the number of children they have given birth to, their gender and birth order, current and past contraceptive use, fertility preferences, as well as their socio-economic and marital status. Unlike other studies where fertility is measured as the number of co-resident children, thanks to this rich dataset, we are able to precisely measure the total number of biological children per woman, that is our key explanatory variable. Moreover, information in ENADID enables us to identify exogenous shocks to parental fertility induced by infertility episodes, in addition to miscarriage at first pregnancy (see Section 3.2 for more details). In line with the extant literature and the medical definition of infertility, namely the failure to conceive after a year of regular intercourse without contraception, we restrict our sample to the children of non-sterilized women who are not currently using contraception or who never did. In so doing, we identify women with infertility episodes as those who report not using contraception because of infertility problems (see Agüero and Marks 2011). This sample selection reduces measurement error in the definition of infertility as women can only be aware of an infertility condition if they do not use contraception.[7]

Our dataset allows us to define household members' international migration experience based on three separate questions: (i) whether there is any household member who migrated abroad (even temporarily) during the five years prior to the survey; (ii) whether any household member has ever worked in the USA or looked for a job while they were in the USA (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 international migration phenomenon.[8] Overall, in 1997 (1992) almost 18 (15) percent of households in Mexico reports 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, and we exclude children who experienced episodes of tied migration (with their parents) and those whose parents have an international migration experience. We do so for two main reasons. First, family and individual migration are inherently very different choices and our focus is on the latter. Second, we exclude parents with migration experiences because parental absence due to migration may affect fertility and hence generate a reverse causality problem.[9]

Figure 1 reports the incidence of non-tied migration by age and gender in Mexico showing that, overall, migrants are highly concentrated (over 70%) in the age range of 15 to 25. Throughout our analysis, we therefore restrict the sample to this age group. This is also consistent with the argument that Mexican youngsters finish compulsory schooling and can potentially enter the labor market at the age of 15, and that beyond the age of 25, they are more likely to make their own lives apart from their household of origin.

One limitation of the data is that, by requesting migration information only for children who are still considered household members, defined as those currently present or those who emigrated less than five years prior,[10] ENADID may introduce a potential sample selection bias if the children for whom we have information are more (or less) likely to come from larger families. We address this concern as follows. First, by focusing on migration outcomes in the age range 15–25, we lessen concerns

[7] We check the robustness of our results to this sample selection, though, by also including the children of sterilized women and those using contraception in our sample. We use miscarriage at first birth as a source of fertility variation in this sample and results (reported in Table 10 Appendix A.1) appear to be unaffected.

[8] Other papers on migration using the same data set are Hanson (2004) and Mckenzie and Rapoport (2007) among others.

[9] We check the robustness of our findings to the inclusion of tied-migrants in the sample (about 13 percent of the sample) or those for which parents had migration experiences, adding parents' migration status among the controls, in the analysis in Appendix A.4. The results in Table 13 are unaffected.

[10] It is worth recalling that ENADID also collects information on migration episodes for temporarily absent household members, as long as migration occurred in the five years before the survey. Thus, ENADID only lacks information on permanent or long-term migration for non-household members.

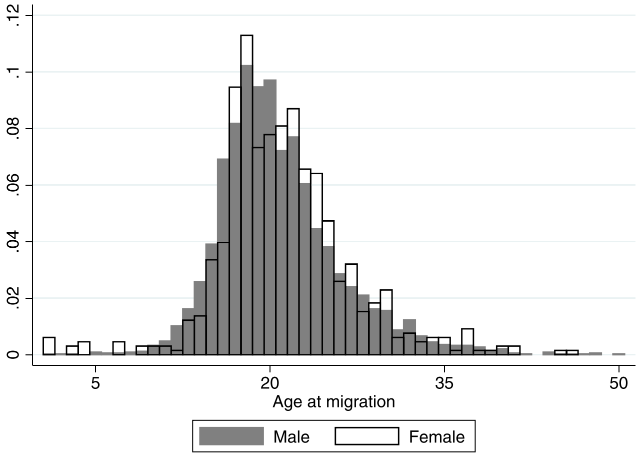


**Fig. 1** Distribution of Mexican individual (non-tied) migration by age and gender. Source: Our computations on ENADID, 1992 and 1997

of household partitioning. In fact, the average age at first marriage in Mexico during the 1990s was between 22 and 23 for females and about 25 for males (World Bank Gender Statistics).[11] Thus, we expect the majority of children for whom we miss information to be mostly young, married daughters who do not live in extended families.[12] Moreover, Mexico–USA migration during the 1990s was mostly of a temporary nature, with an average duration of about two years,[13] and most migrating children may still be considered as family members by their parents. Yet, if the probability of being observed in the data is correlated with family size, the estimated effect of family size on migration may still be biased. We further investigate this issue in Section 4.3.[14]

Our final estimation sample includes 26,743 children in the age range of 15 to 25, whose mothers are 45 years of age on average. The average birth spacing between the first and last child is 13 years, which is below the minimum age of the individuals we consider (15). This ensures that, on average, our measure of fertility can be interpreted as *completed fertility* at the moment of offspring's migration.[15] In other words, in our estimation sample the child migration decision occurs when all children are already born. This is important in order to avoid potential reverse causality issues related to child migration affecting their parents' fertility (for example, through remittances).

[11] http://databank.worldbank.org.

[12] This is the reason for the gender imbalance observed in our estimation sample (see Table 1).

[13] Our computation for migrants of all ages in ENADID.

[14] Moreover, in Appendix A, we run a series of robustness checks—that include sensitivity analyses on subsamples of sons (Table 11) and younger children (Table 12)—in order to show that our results do not suffer from sample selection bias induced by new household formation.

[15] Our sample does not include children whose mothers are older than 54 years of age (9 percent of the total population aged 15–25) since fertility information was not collected from them.

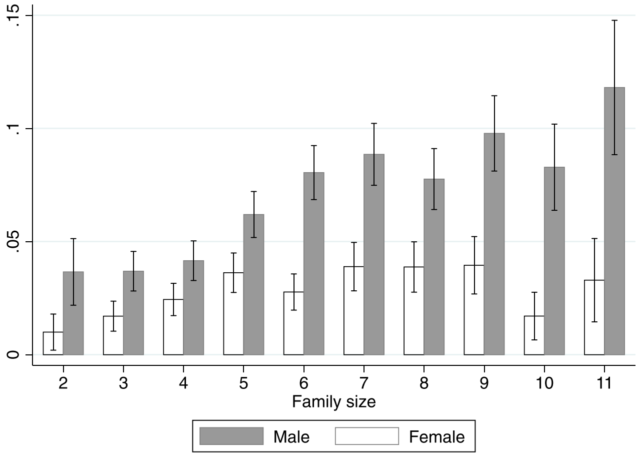


**Fig. 2** Migration rate by family size. Source: Our computations on ENADID, 1992 and 1997. The figure reports the share of migrants by family size and gender with 95% confidence intervals. Statistics are shown for the sample of individuals for which we have two or more siblings and we can include household fixed effects in the estimates

In our sample of individuals, 5.2 percent are migrants, with male and female migration rates of 7.07 and 2.92 percent, respectively. In Fig. 2, we plot the average migration rate of boys and girls in our sample by the number of children.[16] A positive association between sibship size and the migration of sons clearly emerges. Individual sample characteristics according to migration status are reported in Table 1. Migrants are mostly male (75 percent) and report significantly more brothers and sisters than non-migrants. Moreover, migrant children appear to be slightly older and live in less educated households than non-migrant children. All in all, Table 1 suggests that child migration may be more frequent in households that are less well-off, households that also have a higher number of children on average.

## 3 Empirical strategy and identification

### 3.1 Identification of sibship size and birth order effects

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, however, we need to control for the birth order of children (see, for instance, Black et al. 2005). Indeed, if parents have a preference for their firstborn children 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

[16]Since our models estimated at the individual level include household fixed effects, we can only focus on children coming from households with two or more children. Household-level estimates, including households with single children, are reported in Appendix E.

**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 |
| Number 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 |
| Father's age | 48.799 | 52.207 | 0.000 |
| Father's years of schooling | 4.931 | 3.789 | 0.059 |

Source: ENADID, 1992 and 1997. The estimation sample includes individuals aged 15–25 whose mothers are not using contraceptive methods. The sample comprises 1394 migrants and 25,349 non-migrant individuals

we 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 the outcomes of firstborns from families of different sizes, 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.

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

$$M_{ij} = \alpha_0 + \sum_{k=2}^{K} \boldsymbol{\alpha}_{1k} bo_{ijk} + \boldsymbol{\alpha}_2 \mathbf{X}_{ij} + u_j + \epsilon_{ij} \tag{1}$$

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 (top coded at 10 or more) and $k = 1$ (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).[17] $u_j$ is a family fixed effect, and $\epsilon_{ij}$ is an idiosyncratic error.[18]

The effect of sibship size is captured in Eq. 1 by the household 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. Systematic differences in ages between different parities, which are likely to affect migration choices, are controlled for by a quadratic polynomial in child age. Only within-family variation is exploited in these estimates, and the birth order effects are not contaminated by between-family differences 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, by computing the difference $\widehat{NM}_{ij} = M_{ij} - \sum_{k=1}^{K} \hat{\boldsymbol{\alpha}}_{1k} bo_{ijk}$ where $NM$ stands for "netted migration", and use this as the dependent variable in the second step.[19] Hence, the following equation is estimated:

$$\widehat{NM}_{ij} = \beta_0 + \beta_1 S_{ij} + \boldsymbol{\beta}_2 \mathbf{X}_{ij} + \boldsymbol{\beta}_3 \mathbf{W}_j + v_{ij} \tag{2}$$

where $S_{ij}$ is sibship size. The coefficient $\beta_1$ captures the effect on migration of being raised in a family with sibship size $S_{ij}$ for the average child in that family, 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 (chronic diseases), father's absence from the household (due to widowed and divorced single-mother families) and municipality fixed effects. The latter capture rural vs. urban residence along with many other factors related to different local cultural influences or socio-economic conditions such as access to contraception, water sanitation, quality of health care, distance from the US border, etc. Importantly, municipality indicators also capture the local population size which may be related to the demographic pressure on both local labor supply and emigration.[20] Since the dependent variable has been generated by a regression, standard errors are corrected by weighting the estimation with the inverse

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

[18] Another way to disentangle birth order and family size effects has been suggested by Booth and Kee (2009). They build a new birth order continuous index that purges family size from birth order and use this to test if siblings are assigned equal shares in the family's educational resources. Since we prefer to estimate birth order effects using dichotomous indicators, we follow the approach described in Bagger et al. (2013).

[19] Coefficients of all birth order indicators (including firstborns) are recovered using the method described in Suits (1984), whereby the coefficients on the dummy variables show the extent to which the behavior of each birth order deviates from the average behavior (of all birth orders).

[20] This is to say that our identification strategy is able to isolate the within-family dimension of the impact of fertility on migration from the general equilibrium effect of population size. In some more data-demanding specifications reported in Appendix B we also control for municipality-year fixed effects.

of the standard error of $\widehat{NM}_{ij}$ using weighted least squares (WLS).[21] Throughout, standard errors are clustered at the family level so as to account for potential error correlation across siblings. We also estimated models with heteroskedasticity-robust standard errors and the results hold.

If the number of children and investment in child out-migration are both outcomes over which parents exercise some choice, then the WLS estimate of the sibship size effect in Eq. 2 would provide spurious evidence. In other words, parental fertility may be endogenous with respect to offspring migration.

Hence, to clearly identify the relationship between sibship size and migration, an exogenous source of variation in family size is required. The ENADID allows us to identify self-reported infertility from specific questions. Similarly to Agüero and Marks (2008), we construct an indicator variable for infertility (i.e., the inability to conceive) that takes the value of one if a woman reports she is not currently using any contraception method (including natural ones) because of infertility, and zero otherwise. Two things are worth noting. First, the fact that a woman is not currently using contraception because of her inability to get pregnant does not imply that fertility impairments were also present during most part of her reproductive life. This means that we can observe large family sizes also for women reporting infertility problems. Hence, our indicator is closer to the medical definition of "secondary infertility" (a reduction in the ability to conceive or to carry a pregnancy to a live birth), than to "primary infertility" (the condition of women who never had a live birth). Second, in our sample of women who are not using contraception due to infertility, the largest share is represented by those who never used it (89%), while the share of women who stop using it because of infertility is relatively low (11%). 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.[22] These two shocks are used in an instrumental variable (IV) strategy (implemented with two-stage least squares, 2SLS).[23] The first-stage equation is

$$S_{ij} = \gamma_0 + \gamma_1 Z_j + \boldsymbol{\gamma}_2 \mathbf{X}_{ij} + \boldsymbol{\gamma}_3 \mathbf{W}_j + u_{ij} \tag{3}$$

[21] See, for instance, Lewis and Linzer (2005). We also run estimates using White robust standard errors and the results of the analysis are unaffected.

[22] Miscarriages or spontaneous abortions typically refer to any loss of pregnancy that occurs before the 20th week of pregnancy.

[23] Other studies have considered different instruments such as twin births (e.g., Rosenzweig and Wolpin 1980; Angrist and Evans 1998; Càceres-Delpiano 2006) and sibling-sex composition (e.g., Angrist and Evans 1998; Fitzsimons and Malde 2014). Those instruments, however, are not suitable either for our data or for the Mexican context. Twin births cannot be used because we do not have administrative data, and although we make use of a large survey, we observe twin births only in 1.3 percent of families in our estimation sample. Sibling-sex composition is not suitable to the Mexican context because, for its very nature, it is likely to affect the fertility of parents who desire a small number of children. The idea behind the instrument is indeed that parents have an extra child just because they are not happy with the gender of those they already have (i.e., the group of compliers). This typically happens in Mexico when early parities are all females because parents have a son bias. However, average family size in Mexico is very large in our estimation period, the probability of having at least one son is also high, hence the instrument is unlikely to be relevant for a large share of the population.

where $u_{ij}$ is an idiosyncratic error term and $Z_j$ is a dichotomous indicator for secondary infertility or miscarriage experienced by the mother of the potential migrant. The second-stage equation is

$$\widehat{NM}_{ij} = \beta_0 + \beta_1 \widehat{S}_{ij} + \boldsymbol{\beta}_2 \mathbf{X}_{ij} + \boldsymbol{\beta}_3 \mathbf{W}_j + v_{ij} \quad (4)$$

where everything is defined as for Eq. 2, with the exception of $\widehat{S}_{ij}$ which comes from the estimation of Eq. 3.

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. An alternative way to proceed is to estimate the migration equation using the household instead of the individual as the unit of analysis,[24] which enables us to retain in the estimation sample single-child households.[25] In so doing, we are able to check the robustness of our baseline estimates to changes in the estimation sample and the estimation strategy. Indeed, 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 instrumental variables procedure. Thus, we estimate a specification as follows:

$$m_j = \gamma_0 + \gamma_1 n_j + \boldsymbol{\gamma}_2 \mathbf{W}_j + v_j \quad (5)$$

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 $n_j$, defined as the total number of children in household $j$. The coefficient $\gamma_1$ captures the increase in the number of migrants associated with a unitary increase in family size. 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 2SLS in Appendix E.

### 3.2 Instruments' relevance, exogeneity, and exclusion restriction

For our identification strategy to be valid, the two instruments must satisfy three conditions—relevance, exogeneity, and the exclusion restriction assumption—which are discussed below.

#### 3.2.1 Relevance

Infertility or sub-fertility conditions have been already used in the economic literature to estimate the effect of the number of children and fertility timing on mothers' labor market outcomes in both advanced and developing countries (see, for instance, Agüero and Marks 2008; Schultz 2008; Agüero and Marks 2011). We leverage the same source of exogenous variation in sibship size to identify its causal effect on chil-

[24] More precisely, our unit of analysis are biological children in the same household.

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

**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 | | |

The table reports the incidence of fertility and infertility shocks in the estimation samples used in the individual-level (see Section 4.2) and the household-level analysis (see Appendix E), respectively

dren's migration. 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, 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 non-monotonic. More direct evidence on the instruments' relevance is reported in the 2SLS first stages.

### 3.2.2 Exogeneity

There is evidence that infertility is largely 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 US data, education, occupation, and race have been shown to be unrelated to impaired fecundity (Wilcox and Mosher 1993).

Also miscarriages have been used to identify fertility *tempo* and *quantum* effects on women's labor market outcomes (Hotz et al. 2005; Miller 2011; Bratti and Cavalli 2014). By their nature, miscarriages should have a negative effect on total fertility, and in our context on sibship size.[26] Their exogeneity is generally supported by the

[26]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.

medical literature. For example, a few papers using administrative data, in which rich labor market and health data are merged, show that in general miscarrying is not significantly associated with worse labor market outcomes (such as, work absences) before miscarriage (Karimi 2014; Markussen and Strøm 2015). 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.

For both biological shocks a potential threat to identification may come from mothers' general health conditions, as these conditions may affect both fertility and child out-migration. Moreover, ill health might also be related to high levels of alcohol or tobacco consumption that have been observed to correlate with miscarriage (Garcìa-Enguìanos et al. 2002) or to obesity, which might reduce fecundity (Gesink Law et al. 2007).[27] We seek to attenuate these concerns by including controls for mothers' chronic illness and disabilities in our estimation models, as women who heavily consume alcohol, tobacco, drugs or who are obese are also more likely to have developed chronic conditions.

The number of miscarriages generally increases with the number of pregnancies (which depends 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).

A possible way to support the exogeneity assumption is to regress women's characteristics on the biological shocks and show that the latter are not statistically significant predictors of the former (see, for instance, Agüero and Marks 2011). However, these tests are informative only if women's predetermined characteristics are considered as dependent variables. Unfortunately, given the cross-sectional nature of the data, most women's characteristics provided in ENADID are measured at the time of the survey and may be considered as outcome variables (e.g., parents' marital status, income, labor market participation). Regressing them on the biological shocks would be equivalent to running reduced form models for the effect of fertility (level or timing) on such variables. In fact, we might expect significant coefficients for many of them, as fertility is likely to affect marriage duration and dissolution (Kjaer et al. 2014; Bellido et al. 2016), earnings (Miller 2011; Lundborg et al. 2017) or labor supply (Angrist and Evans 1998; Agüero and Marks 2008), just to mention some. That said, we can implement such tests for a subsample of women, namely those still living with their mothers.[28] For these women, we test whether infertility and fertility shocks are systematically associated with their mothers' (or parents') background characteristics, which are predetermined to maternal biological shocks. This test is possible thanks to the ENADID large sample size, unique features of ENADID demographic data that allow us to match women's shocks with their parents'

[27] Other behavioral factors mentioned in Garcìa-Enguìanos et al. (2002) are caffeine, drug consumption, and induced abortions.

[28] That is those for which we have parents' characteristics.

**Table 3** Correlations between infertility shocks and women's background variables

| Dependent variable | (1) Infertility | (2) Miscarriage at first pregnancy |
|---|---|---|
| Mother's age | 0.009 | 0.013 |
| | (0.006) | (0.010) |
| Mother's age squared | −0.000 | −0.000 |
| | (0.000) | (0.000) |
| Mother's age at first pregnancy | −0.000 | 0.001 |
| | (0.000) | (0.001) |
| Mother's years of schooling | 0.001 | 0.000 |
| | (0.001) | (0.001) |
| Single mother | −0.006 | −0.005 |
| | (0.006) | (0.013) |
| Mother chronic illness | −0.026[c] | 0.027 |
| | (0.008) | (0.032) |
| Father's age | −0.001 | −0.009 |
| | (0.005) | (0.013) |
| Father's age squared | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Father's years of schooling | 0.001 | 0.001 |
| | (0.001) | (0.001) |
| Individual (quadratic) Age | Yes | Yes |
| Individual's year of birth indicators | Yes | Yes |
| Mother's cohort dummies | Yes | Yes |
| Father's cohort dummies | Yes | Yes |
| Municipality indicators | Yes | Yes |
| Observations | 4,268 | 4,268 |
| R-squared | 0.101 | 0.059 |

The dependent variable is a dichotomous indicator of the woman having experienced an infertility episode (column 1) or miscarriage at first birth (column 2). The model is estimated using OLS. The estimation sample includes women who had at least one pregnancy and who are considered members of their mother's household in ENADID's 1992 and 1997 waves. Robust standard errors in parentheses. [a,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

characteristics and institutional features of Mexico where living in extended households is not rare.[29] We estimate a woman's likelihood to have sub-fertility episodes or miscarriage at first pregnancy as a function of parental background variables, while controlling for women's age, birth cohort dummies and municipality indicators. Despite the limitations of this exercise, since women cohabiting with their mothers cannot be considered as a random sample, Table 3 reports regression coefficients for both shocks, which are consistent with the literature and support the argument that infertility or sub-fertility conditions are randomly assigned and independent of the characteristics of women's family of origin.

[29] Alternatively, the literature has been using (rare) long-spanning longitudinal data, which allow to link the childhood (background) family characteristics to grown-up children's outcomes (Joffe and Barnes 2000).

Finally, as a last check of exogeneity of fertility and infertility shocks, for each child we compute the average biological shock at the level of the mother's municipality of residence (excluding his / her mother's own shock) and regress the individual shocks on these averages. If infertility shocks are as good as randomly assigned to women, we expect these municipality averages not to be significant predictors of individual shocks. Indeed, municipality averages may capture factors such as local sanitation and health conditions, social norms towards voluntary abortion or access to illegal abortion, all factors that may also be correlated with children's attitudes towards migration, hence undermining our identification strategy (see Fletcher and Wolfe 2009). The coefficient of the regression of individual infertility on municipality average infertility is 0.097 ($p$-value = 0.36), and the one of miscarriage at first pregnancy on the municipality average of the same variable is 0.064 ($p$-value = 0.62).[30] In both cases, these results show that biological shocks are unlikely to be driven by time-varying municipality-level unobservables.

### 3.2.3 Exclusion restriction

For our instruments to be valid, in addition to exogeneity, they have to satisfy the exclusion restriction assumption, such that 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 confounding factors and for those that may be affected by the instruments while also having a direct effect on children's migration. Among these variables, we include the mother's age, age at first pregnancy, education, marital status and the husband's characteristics (age, education, and absence). In particular, while parental education may directly influence fertility, it also acts as a proxy for household well-being and poverty. Yet, in a set of robustness checks we include additional controls for household economic conditions, namely municipality by year (1992 or 1997) fixed effects and municipality by parental education fixed effects (see Appendix B).

A threat to the exclusion restriction assumption comes from the fact that miscarriage is a stressful event impacting negatively on women's mental well-being. In principle, this may impair children's geographical mobility for two reasons. The shock may create an emotional bond inducing children to stay close to their mothers, or emotionally distressed mothers may need more support at home, in both cases negatively affecting children's likelihood of migration. Unfortunately, ENADID does not provide information on respondents' mental health status and we cannot directly control it. At the same time, this threat is less relevant in our case. Indeed, we only exploit miscarriage at first pregnancy and since migration is measured for children aged 15 or more, such negative psychological shocks on mothers must persist for more than 15 years and to several childbirths to bias our results. However, an empirical check of

[30] Standard errors are clustered by municipality. Only children with mothers living in municipalities for which there are at least ten women in our baseline estimation sample are included in these regressions.

the existence of direct effects of mother's emotional distress on children's migration is reported in Appendix D and shows the lack of such effects.[31]

#### 3.2.4 Measurement error

There is a potential issue of measurement error with the miscarriage instrument, since women may be unaware of miscarriages if they happen very early in the pregnancy,[32] may fail to recall them (this may hold especially for older women, although mothers in our sample are not older than 54) or just avoid reporting them as they are painful events. Misreporting may affect the strength 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 polynomial in maternal age). Finally, as it was formulated in the ENADID, the question does not distinguish between voluntary and involuntary abortions. Thus, some of the reported abortions may be actually voluntary, even though induced abortion was illegal and Mexico had the strictest anti-abortion legislation in Latin America during the period under consideration. For women who voluntary had 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 (information on religion is available in the 1997 wave only): incidence of abortion is 4.6 percent in the former group and 4.8 percent in the latter.[33]

## 4 Results on family size

### 4.1 First-step estimation of birth order effects

We start by estimating the impact of birth order on individual migration controlling for household fixed effects, as specified in Eq. 1. The within-family estimator sweeps out all parental- and family-level heterogeneity, including sibship size. Moreover,

[31] We test for the potential direct effects of miscarriage on child migration, via the emotional distress that a traumatic event such as miscarriage can cause to the mother, drawing on the work of van den Berg et al. (2017). The authors show that a child's death represents one of the largest losses that an individual can face and has adverse effects on parents' labor income, employment status, marital status and hospitalization. Similarly, we include child death and the duration of the pregnancy that ended in a miscarriage or a stillbirth as controls in the child migration equation, but we do not find 'grief' effects on migration. This analysis is reported in Appendix D (Table 20).

[32] In this case, however, the effect on completed fertility is probably negligible.

[33] 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), $\beta_{1,OLS} = \beta_1 + \mathrm{Cov}(S, v)/\mathrm{Var}(S)$ while $\beta_{1,IV} = \beta_1 + \mathrm{Cov}(Z, v)/\mathrm{Cov}(Z, S)$, where $\mathrm{Cov}(Z, S) < 0$ and $\mathrm{sign}(\mathrm{Cov}(S, v)) = -\mathrm{sign}(\mathrm{Cov}(Z, v))$. 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.

family fixed effects account for omitted family-specific unobservable factors simultaneously affecting fertility and child migration. The first column of Table 4 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's birth cohort dummies (one for each year of birth).[34] 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 household 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. Column 2 shows that the effect is non-linear and starts to be economically meaningful from children of birth order 3, who are 2.1 p.p. less likely to migrate than firstborns. Although this appears to be a small effect in absolute value, it represents an approximately 40 percent decrease in the probability of 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). We also estimated (1) by allowing interactions between gender and birth order indicators, but interaction terms are never statistically significant (see Section 5 for discussion). Thus, we use the specification without gender interactions to implement the two-step procedure as described above.

### 4.2 Sibship size effect: WLS and 2SLS results at the individual level

By applying the two-step procedure, we now turn to the estimation of the sibship size effects. We report the WLS estimates as a benchmark model, where the dependent variable is "netted migration" (see Section 3.1).[35] The number of siblings is tallied as the number of currently living biological brothers and sisters of each child.[36] The first column of Table 5 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 in the first step, 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). In column 3,

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

[35]The inverse of the standard errors of "netted migration" are used as weights.

[36]Those currently deceased are excluded from our definition of siblings. This is done for two reasons: (i) 70 percent of deceased children in our sample died before the first year of life, 90% of them before the second one; (ii) the focus of our analysis is not on very young children so that we need to take into account siblings who actually "had enough time" to compete over household resources, and exclude accordingly infant deaths. In Appendix C (Tables 16–19) we report robustness checks related to concerns about the endogeneity of our definition of sibship size and birth order and estimate models based on ever-born children, i.e., currently alive or deceased, and the results do not change.

**Table 4** Birth order effects on child's migration status

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Female | −0.036[c] | −0.035[c] | −0.032[c] | −0.031[c] |
| | (0.003) | (0.003) | (0.006) | (0.007) |
| Birth order | −0.019[c] | | −0.019[c] | |
| | (0.003) | | (0.003) | |
| Birth order 2 | | −0.002 | | 0.002 |
| | | (0.005) | | (0.006) |
| Birth order 3 | | −0.021[c] | | −0.023[c] |
| | | (0.007) | | (0.008) |
| Birth order 4 | | −0.038[c] | | −0.034[c] |
| | | (0.010) | | (0.011) |
| Birth order 5 | | −0.068[c] | | −0.070[c] |
| | | (0.013) | | (0.014) |
| Birth order 6 | | −0.086[c] | | −0.077[c] |
| | | (0.016) | | (0.017) |
| Birth order 7 | | −0.112[c] | | −0.103[c] |
| | | (0.019) | | (0.020) |
| Birth order 8 | | −0.136[c] | | −0.140[c] |
| | | (0.022) | | (0.023) |
| Birth order 9 | | −0.161[c] | | −0.166[c] |
| | | (0.026) | | (0.028) |
| Birth order 10+ | | −0.199[c] | | −0.188[c] |
| | | (0.030) | | (0.033) |
| Birth order × female | | | −0.001 | |
| | | | (0.001) | |
| 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) |

**Table 4** (continued)

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Birth order 10+ × female | | | | −0.022 |
| | | | | (0.027) |
| Age | $0.020^{b}$ | $0.021^{b}$ | $0.020^{b}$ | $0.021^{b}$ |
| | (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 |
| Household 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 |

The dependent variable is a dichotomous indicator of the child's migration status. The model is estimated using OLS. Sibship size is absorbed by household fixed effects. Standard errors clustered at the household level in parentheses. $^{a,b,c}$Statistical significance at 10, 5, and 1 percent level, respectively

we estimate the same model as above by allowing for differential effects by child gender. The significant negative coefficient for the interaction term indicates that females' likelihood to migrate increases less due to sibship size with respect to males. Specifically, one extra sibling raises 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 geographical-level controls in order to account for potential confounding factors of the relationship between family size and offspring's migration. Specifically, in columns 4 and 5, 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.[37] In columns 6 and 7, we further add municipality fixed effects that, conditional on family size, control for population size along with many other local factors related to different cultural or economic conditions, which may have an effect on fertility and migration (for instance, employment rates, migration intensity, access to contraception, social services, etc.). Overall, the sibship size effect is essentially unchanged when we control for all of the aforementioned factors, and the same holds for the differential effect by gender.

Yet, as noted in the methodological section, the coefficients on sibship size reported in Table 5 are still likely to be biased, even when a rich set of demographic and economic controls is included. This is so as fertility may be endogenous with respect to child out-migration. Thus, we employ an IV approach and exploit the

[37] 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 paternal information.

**Table 5** Sibship size effect on child's "netted migration" status: WLS estimates

| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Number of siblings | 0.011[c] | 0.011[c] | 0.014[c] | 0.010[c] | 0.013[c] | 0.010[c] | 0.013[c] |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Number of siblings | | | −0.008[c] | | −0.007[c] | | −0.006[c] |
| × Female | | | (0.001) | | (0.001) | | (0.001) |
| Female | | −0.038[c] | −0.036[c] | −0.033[c] | −0.031[c] | −0.033[c] | −0.031[c] |
| | | (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 |

The dependent variable is "netted migration" (see Section 3). 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,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

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 6 we present 2SLS estimates and the two-step methodology, as outlined above, to estimate Eq. 4. In column 1, we instrument sibship size with an indicator variable for infertility shocks taking value one if the woman declares she is not using contraception because she is infertile. 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 using both instruments in an overidentified 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, children whose mothers experienced an infertility shock have a reduction in their sibship size of nearly 0.5 ($t = -5.2$) with an $F$-statistic of 26.9 (column 1). The negative impact of miscarriage on sibship size 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).[38] The sibship size effects estimated using 2SLS

[38]The Hansen $J$-statistic does not reject the "validity" of the instruments (i.e., orthogonality to the error term and correct exclusion from the main equation) in the overidentified model.

**Table 6** Sibship size effect on child's "netted migration" 2SLS estimates

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Second stage | | | |
| Number of siblings | 0.004 | −0.018 | −0.005 |
| | (0.014) | (0.023) | (0.012) |
| Female | −0.033[c] | −0.033[c] | −0.033[c] |
| | (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—number of siblings | | | |
| Infertility | −0.494[c] | | −0.491[c] |
| | (0.095) | | (0.095) |
| Miscarriage | | −0.437[c] | −0.433[c] |
| | | (0.10) | (0.10) |
| Female | −0.032 | −0.032 | −0.033 |
| | (0.024) | (0.024) | (0.024) |
| 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 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. The list of control variables is the same as in Table 5. Standard errors clustered at the household level in parentheses. $p$-values are reported in brackets. [a,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

are always economically small. In the models using miscarriage, infertility and both instruments the effect of increasing sibship size by one changes the likelihood of migration by 0.4, −1.8, and −0.5 percentage points, respectively. Although, due to our sample size, these estimates are not "precisely estimated zeros" (i.e., very small statistically significant coefficients) they are zeros in economic terms. The lack of significance does not appear to be due to imprecision related to a weak instruments problem. For all models, the Anderson-Rubin $F$-statistic (robust to weak instruments) cannot reject that the coefficient on the instrument is zero in the reduced form. Interestingly, the point estimate of the effect of sibship size on child migration obtained with the miscarriage instrument (which might include voluntary abortions) is lower than the one obtained with the infertility instrument, which we consider to be less affected by endogeneity issues, and much lower than the OLS estimate, a fact that is inconsistent with the premise that induced abortions include a substantial share of total abortions (see Section 3.2).

Even though in all specifications we control for parental education, in Appendix B (Tables 14 and 15) we show that our results are robust to the inclusion of a number of additional controls for household economic conditions, namely municipality by time fixed effects and municipality by parent's education fixed effects.

The two instruments we used may act on different compliers. We expect miscarriage at first pregnancy to hit especially women with very high desired fertility. Indeed, in Mexico in the 1990s, there was generally a long time span between a woman's first pregnancy and the end of her fertile life, during which women could reach their desired number of children. Only women who desired a very large family size were prevented from attaining their target by a miscarriage experienced at first pregnancy. By contrast, compliers with the infertility instrument may be in principle more evenly distributed across different desired family sizes. Since ENADID data do not provide the exact timing of the infertility shock, we are unable to check the age it occurs and the family size margin the instrument is mainly relevant for. Yet, Table 2 suggests that infertility is more prevalent in (ex-post) smaller families. Accordingly, finding (in Table 6) similar results using instruments with potentially different compliers is reassuring in terms of the external validity of our estimates.

In Table 7 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.[39] Results do not point to any significant difference in the impact of sibiship size between 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 hypotheses of sibship size not affecting child migration.

Overall, findings in this section point to the negligible role of family size on children's migration outcomes. The comparison between the OLS and the IV estimates indicates an upward bias in the former. According to our estimates, the correlation between family size and child migration observed in the data is driven by unobservable variables which make some families more prone to both have more children and more migrant children. Such variables may include, for instance, risk aversion and preferences for income diversification. Indeed, higher fertility may be a way for parents to diversify the sources of old-age income support and child migration a way to spatially diversify household production.

As outlined in Section 3.1, household-level estimates enable us to include single-child households in the estimation sample and use a standard 2SLS procedure. They can be therefore considered as a robustness check to changing the composition of the sample and the estimation strategy. The results are reported in Appendix E (Tables 21 and 22) and confirm those of the individual-level analysis.

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

**Table 7** Child gender and sibship size effect on child's "netted migration" status: 2SLS estimates

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Second stage | | | |
| Number of siblings | 0.005 | −0.065 | −0.007 |
| | (0.016) | (0.048) | (0.015) |
| Number of siblings × Female | −0.005 | 0.112 | 0.005 |
| | (0.013) | (0.079) | (0.013) |
| Female | −0.032[c] | −0.064[c] | −0.034[c] |
| | (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—number of siblings | | | |
| Infertility | −0.567[c] | | −0.564[c] |
| | (0.109) | | (0.108) |
| Infertility × female | 0.168 | | 0.169 |
| | (0.115) | | (0.115) |
| Miscarriage | | −0.453[c] | −0.450[c] |
| | | (0.117) | (0.117) |
| Miscarriage × female | | 0.037 | 0.038 |
| | | (0.106) | (0.105) |
| Female | −0.041 | −0.033 | −0.043[a] |
| | (0.025) | (0.025) | (0.025) |
| Angrist-Pischke $F$-statistic instrument(s) | 28.62 | 11.98 | 15.68 |
| | | | |
| First stage—number of siblings × female | | | |
| Infertility | 0.125[c] | | 0.125[c] |
| | (0.038) | | (0.038) |
| Infertility × female | −0.694[c] | | −0.691[c] |
| | (0.131) | | (0.131) |
| Miscarriage | | −0.067 | −0.068 |
| | | (0.044) | (0.043) |
| Miscarriage × female | | −0.261[b] | −0.254[a] |
| | | (0.131) | (0.130) |
| Female | 0.292[c] | 0.269[c] | 0.303[c] |
| | (0.03) | (0.03) | (0.031) |
| Angrist-Pischke $F$-statistic instrument(s) | 26.93 | 4.27 | 13.83 |
| Individual's controls | Yes | Yes | Yes |
| Mother's controls | Yes | Yes | Yes |

**Table 7** (continued)

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Father's controls | Yes | Yes | Yes |
| Municipality indicators | Yes | Yes | Yes |
| Observations | 26,743 | 26,743 | 26,743 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. The list of control variables is the same as in Table 5. Standard errors clustered at the household level in parentheses. *p*-values are reported in brackets. [a,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

### 4.3 Sample selection bias from new household formation

In this section, we carry out a direct check for the potential sample selection bias induced by new household formation. We are able to analyze migration decision only of children who are considered as members of the household, that is children who are present or emigrated less than five years before. If a new household formation (when the child leaves the parental home and starts living alone or forming his/her own family) is associated with the number of siblings, our estimates would not be representative of the effect of family size on migration for the whole population of children but only for children cohabiting with their parents or who are current household members. To check if this is the case, we estimate a LPM in which the dependent variable is a dichotomous indicator for a child's not being observed in the household ("absent child"). Even for the "absent children" some individual-level information can be recovered, namely gender, birth order, and age, from the mother's fertility history, so that we can include in the estimated equation exactly the same controls as in the migration equation. First, in column 1 of Table 8, we estimate a reduced form LPM in which we include all the controls of the migration equation except sibship size, but we also include the two excluded instruments used for sibship size (miscarriage and infertility conditions). We interpret the test for the joint significance of the coefficients on the two instruments as evidence about the role of family size on a child's choice to permanently leave his/her origin household. Finding statistically significant coefficients on the instruments would generate concerns that estimates could be affected by a sample selection bias. We do a similar exercise in column 2 of Table 8 in which we use 2SLS, and test for the significance of family size on the "absent child" equation. In both cases, we can reject the null hypothesis that family size is a significant driver of a child's probability of being included in the sample used to estimate the decision to migrate abroad.

## 5 Gender and sibling composition

### 5.1 Migration outcomes as a function of gender and birth order

In columns 1 and 2 of Table 4 (see Section 4.1), we show that an individual's probability to migrate decreases with birth order. While this is consistent with a household

**Table 8** The effect of family size on being an "absent child"

| Variables | (1) OLS | (2) 2SLS |
|---|---|---|
| Infertility | −0.013 | |
| | (0.010) | |
| Miscarriage | 0.002 | |
| | (0.011) | |
| Number of siblings | | 0.015 |
| | | (0.015) |
| $F$-statistic instruments$^{(a)}$ | 0.838 | |
| IV: | – | Overidentified |
| Anderson-Rubin $F$-statistic | | 0.838 |
| | | [0.433] |
| Hansen $J$-statistic | | 0.875 |
| | | [0.350] |
| Individual's controls | Yes | Yes |
| Mother's controls | Yes | Yes |
| Father's controls | Yes | Yes |
| Municipality indicators | Yes | Yes |
| Observations | 34,852 | 34,852 |

The dependent variable is a dichotomous indicator of the child's not being observed in the household of origin ("absent child"). The model is estimated using OLS in column 1 and two-stage least squares in column 2. The list of control variables is the same as in Table 5. Standard errors clustered at the household level in parentheses. $^{(a)}$ $F$-statistic for the joint test that infertility and miscarriage are zero in the "absent child" equation. $^{a,b,c}$Statistical significance at 10, 5, and 1 percent level, respectively

optimal migration model where family's returns from migration decrease with child parity, in this and the reminder sections of the paper we present more compelling tests on whether migration chances are distributed unevenly—by age and gender—across children within the same family. Indeed, within a household resources allocation framework, low-parity children may be more likely to migrate because the family has more time to reap the benefits of migration. However, it may still be argued that first-born children are better off with respect to other forms of human capital investments as well. For example, earlier parities may have benefited from higher pre-natal or post-natal parental investments, having shared household resources with fewer siblings, and this may affect the returns to migration. Thus, here we explore the gendered pattern of migration in order to test the hypothesis of the low-parity advantage.

In columns 3 and 4 of Table 4, we report results by adding interaction effects between birth order and gender to the models.[40] The interactions of being female with birth order dummies are not statistically significant, suggesting that the birth-order

[40] As our two-step procedure relies on household 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.

gradient in child migration is not statistically different between boys and girls. Yet, the latter holds for all parities but for firstborns: in column 2, 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 but a firstborn daughter is significantly less likely to migrate than a firstborn son by about 3 p.p. (which means a reduction in the probability of migration of roughly 60 percent at the sample average migration rate). Finding a significant effect on the interaction between first-parity and gender suggests that it is unlikely that parents decide to invest in the migration of the firstborns only, irrespective of gender (firstborn bias). Also, gender turns out to be a significant factor, and in particular (firstborn) boys may have higher migration returns than their female peers. This is consistent with a male-dominated Mexican migration phenomenon, as shown by different data (e.g., Cerrutti and Massey 2001). Yet, while parental investment in (low-parity) boys is still a rational choice when returns to migration in the US labor market are higher (or moving costs are lower) for boys than for girls, these findings are also consistent with the argument that parents may just value (low parity) sons more than daughters (preference for sons). Hence, in the next and last sub-section, we estimate the same migration equations as above by allowing for a separate effect of sibling composition from the individual gender and birth-order variables. If migration choices are driven by birth-parity or son preference, we should find no separate effect of sibling composition. On the contrary, a significant effect of sibling composition variables on the likelihood to migrate points to the existence of an intra-household migration selection process within which some children may have systematically more chances to migrate than others.

### 5.2 Migration outcomes as a function of gender and sibling-sex composition

Our estimates so far show that gender is a robust predictor of migration in Mexican families and, *ceteris paribus*, boys—especially firstborns—are systematically more likely to migrate abroad than girls. In practice, this means that if migration is costly and not all children are in the position to migrate, a pro-eldest-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 older sisters than brothers (Garg and Morduch 1998). Thus, in order to explore the scope of sibling rivalry by age and gender, we test how sibling composition influences child migration by running two sets of regressions as reported in Table 9. 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, (a quadratic polynomial in) 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, consistently with a household-level migration strategy.

**Table 9** Siblings' composition effect on child's migration status: OLS estimates

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Number of older brothers | −0.014[c] | −0.014[c] | −0.017[c] | −0.016[c] |
| | (0.004) | (0.004) | (0.004) | (0.005) |
| Female | −0.028[c] | −0.026[c] | −0.022[c] | −0.016[c] |
| | (0.003) | (0.005) | (0.005) | (0.006) |
| Number of 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[b] |
| | | | | (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 |
| Household 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 |

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

Moreover, the sibling composition effect does not differ significantly by the gender of the child, suggesting that older siblings' sex composition equally matters for boys and girls (column 2).

Yet, we further exploit the gendered migration pattern and the fact that siblings are likely to migrate in order of birth to test whether females pay a toll for higher migration returns for boys. We do so by including a control for having a next-born brother in the household 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.[41] Results in columns 3 and 4 in Table 9 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 sibling composition by gender and age plays a significant role in the determinants

[41] A similar empirical strategy is employed by Vogl (2013) to study sibling rivalry over arranged marriages in South Asia.

of migration decisions within the household. More specifically, from our results, it seems that a daughter with a next-born brother is less likely to migrate than a girl with a next-born sister. In other words, when parents face the costly decision whether to send a daughter abroad, they seem to prefer to invest in the migration of her next-born brother (if there is one). Similarly, by allowing children's agency, it may be that young women are less likely to offer themselves as migrants if they have a next-born brother.

All in all, our results are consistent with an optimal household migration strategy where private costs and returns of migration are shared among all siblings. Indeed, a low-parity Mexican boy in the 90s may be more valuable to send as a household migrant abroad than a girl. 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 male-dominated pattern of Mexico-USA 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 of origin. Migration is largely a youth phenomenon occurring in households that seldom dispatch all or most 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 an offspring migrating. Yet, in relatively poor contexts, parents are likely to depend on their grown-up children for the provision of care and income, and migration opportunities can significantly contribute to the living arrangements of elderly parents.

We use data on teenagers and young adults from a rich household survey to examine the causal effects of sibship size, birth order and sibling composition on migration outcomes in Mexico. Mexican migration, mainly to the USA, is an enduring flow that accounts for one-third of total US immigration and one-tenth of the entire population born in Mexico. Importantly, migration patterns in the 90s differed by age and gender, with a significant fraction of Mexican males migrating between the ages of 15 and 25.

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 no evidence that larger families have a causal impact on migration. The positive link between family size and migration breaks down when potential endogeneity 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. Older siblings, especially firstborn males, are more likely to migrate, while having relatively more older brothers than sisters systematically decreases the likelihood of migration for all children. Yet, girls, but not boys, are less likely to migrate when their next parity is a male.

Our findings are consistent with an optimal household migration model in which parents maximize returns to migration when deciding on whether to have one or more of their children move abroad. Large family size per se does not constitute a significant push factor in the migration choice, while gender, birth order, and sibling composition have much more influence on the migration outcome. In particular, in resource-scarce contexts, out-migration of girls 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.

These results contribute to the migration literature by shedding new light on the significant role of both the family dimension and demographic factors in the migration decision problem. Labor mobility, especially from poor to rich settings, is one of the most important ways through which young adults can expand their productivity and earning potentials. The type of family-based migration that occurred from Mexico to the USA during the 1990s is of considerable and growing importance for many of today's developing countries (such as, Asia and Africa) where both migration and fertility rates are substantial (e.g., Hatton and Williamson 2003). Despite the easily observable association between high fertility rates and migration, we provide evidence that large families are unlikely to be a systematic driver of migration at the household level.

Understanding the link between fertility and migration is also 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 (China and India, the world's two most populous countries, have experimented with different family planning policies to control family size, for instance). Yet, although our empirical findings do not point to a causal link between family size and migration, they hint to the fact that parental returns from offspring migration may play a role in lifetime fertility choices. Moreover, by showing that not all children within the family have the same chances to migrate, our findings point to the existence of an optimal intra-household selection process into migration. This is so as in contexts of scarce resources and weak formal safety nets, children may be a key social security valve for parents such that high migration opportunities to rich countries may increase the value of children (some more than others, specifically, low-parity sons). Hence, effective family welfare measures or the development of credit and insurance markets may lead to a reduction in both migration and fertility, and perhaps less gender inequality.

Eventually, it is worth noting that our analysis seeks to address endogeneity concerns related to fertility resorting to women's sub-fertility conditions and miscarriage events, and we have reported several pieces of evidence consistent with the validity of the instruments we used. Moreover, quite reassuringly, the evidence based on different instruments, which act on different compliers, and on an over-identified model, all lead to the same conclusion. Yet, there might some remaining concerns that our instruments are not completely exogenous, or have direct effects on child migration. For this reason, given the paucity of the research investigating the causal effects of family size on children's migration, it would be important to build more evidence, on other countries and / or using other sources of presumably exogenous variation in fertility (such as, fertility control programs for which a treatment and a comparison group can be clearly identified) to assess the external validity of our findings.

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### Compliance with Ethical Standards

**Conflict of interests** All authors declare that they have no conflict of interest.

## Appendix A: Robustness checks

As described in Section 2, we restrict our sample to children of mothers for whom we have information on arguably exogenous variation in fertility (i.e., miscarriage and infertility shocks). Here, we run the same analysis on the full sample of women, including children of sterilized women and of women using contraceptives, in order to address potential concerns related to sample selection. Moreover, ENADID provides information on the migration status only for children cohabiting with their parents and for those temporarily absent but still considered as household members. In order to lessen the concerns with the potential selection bias this may introduce, we make a number of further sensitivity checks by changing the composition of the estimation sample.

First, Section A.1 reports both WLS and IV estimates on the full sample, the one including children of sterilized women and of women using contraceptives, by using miscarriage at first birth as instrument. Table 10 shows that the estimated effect of sibship size is −0.024 (s.d. = 0.017) very close to our baseline estimate of −0.018 (s.d. = 0.023) in Table 6.

In Section A.2, we run a sample sensitivity check by focusing on the male (sons) subsample, since according to the data boys tend to marry and hence leave their parents' household later compared to girls. In Section A.3, we focus on a sample of individuals aged 15 to 20 as a further robustness check: only few individuals are expected to be out of their origin household in this age group. Moreover, since we are able to recover migration patterns of all individuals who left in the five years prior to the survey, our measure of migration is very precise (very few individuals leave alone before age 15 and can be considered as permanent migrants) at the cost of a smaller sample size. In both cases, the estimation results are very similar to those commented in the main text, although some coefficients are less precisely estimated.

Finally, Section A.4 addresses the biases potentially generated by the exclusion from the estimation sample of parents migrated in the past and episodes of children's tied migration. We include in the estimation sample children with parents who ever migrated abroad, adding in the regressions an extra control (in the form of a dichotomous indicator) for parental migration. Table 13 confirms the statistically insignificant effect of sibship size on child migration when endogeneity is addressed.

### A.1 Sample including sterilized women and those using contraceptives

**Table 10** Sibship size effect on child's "netted migration" status: WLS and 2SLS estimates

| Variables | (1) WLS | (2) 2SLS |
|---|---|---|
| Number of siblings | 0.005[c] | −0.024 |
| | (0.001) | (0.017) |
| Female | −0.031[c] | −0.031[c] |
| | (0.002) | (0.002) |
| First stage—number of siblings | | |
| Miscarriage | | −0.421[c] |
| | | (0.084) |
| Angrist-Pischke $F$-statistic instrument(s) | | 25.33 |
| Individual's controls | Yes | Yes |
| Mother's controls | Yes | Yes |
| Father's controls | Yes | Yes |
| Municipality indicators | Yes | Yes |
| Observations | 40,008 | 40,008 |

The sample includes also children of sterilized women and women using contraceptives. The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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,b,c]Satistical significance at 10, 5, and 1 percent level, respectively

## A.2 Sons

**Table 11** Sibship size effect on sons' "netted migration" status: WLS and 2SLS estimates

| Variables | (1) WLS | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Number of siblings | 0.012[c] | 0.004 | −0.051 | −0.016 |
| | (0.001) | (0.019) | (0.036) | (0.017) |
| IV: | — | Infertility | Miscarriage | Overidentified |
| Anderson-Rubin $F$-statistic | | 0.036 | 2.567 | 1.315 |
| | | [0.850] | [0.109] | [0.268] |
| Hansen $J$-statistic | | | | 2.175 |
| | | | | [0.140] |
| First stage—number of siblings | | | | |
| Infertility | | −0.549[c] | | −0.547[c] |
| | | (0.110) | | (0.109) |
| Miscarriage | | | −0.441[c] | −0.438[c] |
| | | | (0.117) | (0.117) |
| Angrist-Pischke $F$-statistic instrument(s) | | 25.13 | 14.19 | 20.38 |
| Individual's controls | Yes | Yes | Yes | Yes |
| Mother's controls | Yes | Yes | Yes | Yes |
| Father's controls | Yes | Yes | Yes | Yes |
| Municipality indicators | Yes | Yes | Yes | Yes |
| Observations | 14,777 | 14,777 | 14,777 | 14,777 |
| R-squared | 0.242 | | | |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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,b,c]Sstatistical significance at 10, 5, and 1 percent level, respectively

### A.3 Age group 15–20

**Table 12** Sibship size effect on children's age 15–20 "netted migration" status: WLS and 2SLS estimates

| Variables | (1) WLS | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Second stage | | | | |
| Number of siblings | 0.005[c] | 0.005 | −0.012 | −0.002 |
| | (0.001) | (0.014) | (0.020) | (0.011) |
| Female | −0.024[c] | −0.024[c] | −0.025[c] | −0.025[c] |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| IV: | — | Infertility | Miscarriage | Overidentified |
| Anderson-Rubin $F$-statistic | | 0.146 | 0.377 | 0.257 |
| | | [0.702] | [0.539] | [0.773] |
| Hansen $J$-statistic | | | | 0.515 |
| | | | | [0.473] |
| First stage—number of siblings | | | | |
| Infertility | | −0.455[c] | | −0.452[c] |
| | | (0.102) | | (0.102) |
| Miscarriage | | | −0.412[c] | −0.408[c] |
| | | | (0.105) | (0.105) |
| Angrist-Pischke $F$-statistic instrument(s) | | 19.78 | 15.43 | 17.82 |
| Individual's controls | Yes | Yes | Yes | Yes |
| Mother's controls | Yes | Yes | Yes | Yes |
| Father's controls | Yes | Yes | Yes | Yes |
| Municipality indicators | Yes | Yes | Yes | Yes |
| Observations | 18,707 | 18,707 | 18,707 | 18,707 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

## A.4 Tied and parents' migration

**Table 13** Sibship size effect on child's "netted migration" WLS and 2SLS estimates

| Variables | (1) WLS | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Number of siblings | 0.012[c] | −0.000 | −0.002 | −0.001 |
| | (0.001) | (0.013) | (0.019) | (0.011) |
| IV: | — | Infertility | Miscarriage | Overidentified |
| Anderson-Rubin $F$-statistic | | 0.001 | 0.013 | 0.007 |
| | | [0.979] | [0.908] | [0.993] |
| Hansen $J$-statistic | | | | 0.006 |
| | | | | [0.936] |
| First stage—number of siblings | | | | |
| Infertility | | −0.566[c] | | −0.560[c] |
| | | (0.089) | | (0.088) |
| Miscarriage | | | −0.489[c] | −0.482[c] |
| | | | (0.092) | (0.092) |
| Angrist-Pischke $F$-statistic instrument(s) | | 40.66 | 28.24 | 35.06 |
| Individual's controls | Yes | Yes | Yes | Yes |
| Mother's controls | Yes | Yes | Yes | Yes |
| Father's controls | Yes | Yes | Yes | Yes |
| Municipality indicators | Yes | Yes | Yes | Yes |
| Observations | 30,977 | 30,977 | 30,977 | 30,977 |

The dependent variable is "netted migration" (see Section 3). Compared to the estimates in Tables 5 and 6 in the main text, we retain in the estimation sample also children that experienced tied migration with their parents, or whose parents had previous migration experiences. Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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, being single and a dummy for past migration experiences; father's controls include decade of birth indicators, age and age squared, years of schooling and a dummy for past migration experiences. Standard errors clustered at the household level in parentheses. $p$-values are reported in brackets. [a,b,c]Statistical significance at 10, 5 and 1 percent level, respectively

## Appendix B: Poverty and further identification threats

In developing countries women's infertility conditions may partly depend on material poverty, which affects women's health. Failing to control for economic conditions may represent a threat to our IV estimates because poverty is also likely to affect children's migration status. In the baseline estimates of Section 4.2, we took into account this potential threat by including some strong correlates of individual or household poverty, such as parents' educational levels, age and municipality fixed effects. In this section, we run supplementary checks by estimating models including municipality by (ENADID) wave fixed effects and municipality by parent's education fixed effects (years of education of the most educated parent, either the mother of the father, are interacted with municipality indicators). We report both OLS and 2SLS estimates.

Table 14 shows that the OLS estimates are not sensitive to the inclusion of additional proxies for poverty and family wealth, suggesting that the income and wealth channels are unlikely to be the main sources of the OLS upward bias.

2SLS results are reported in columns 1 and 2 of Table 15, respectively. They also serve as checks of potential concerns related to the miscarriage at first pregnancy instrument, which may also be affected by women's living standards. The results confirm the robustness of our 2SLS estimates of family size effects to including alternative proxies of household poverty.

**Table 14** Robustness of WLS estimates of the effect of sibship size on child's "netted migration" status to various proxies of poverty

| Variables | (1) | (2) |
|---|---|---|
| Number of siblings | 0.006[c] | 0.006[c] |
| | (0.001) | (0.001) |
| Municipality × wave indicators | Yes | No |
| Municipality × parents' education indicators | No | Yes |
| Individual's controls | Yes | Yes |
| Mother's controls | Yes | Yes |
| Father's controls | Yes | Yes |
| Municipality indicators | Yes | Yes |
| Observations | 26,743 | 26,743 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

**Table 15** Robustness of 2SLS estimates of the effect of sibship size on child's "netted migration" status to various proxies of poverty

| Variables | (1) | (2) |
|---|---|---|
| Second stage | | |
| Number of siblings | −0.009 | −0.009 |
| | (0.011) | (0.011) |
| IV: | Overidentified | Overidentified |
| Anderson-Rubin $F$-statistic | 0.666 | 0.603 |
| | [0.514] | [0.547] |
| Hansen $J$-statistic | 0.827 | 0.822 |
| | [0.363] | [0.365] |
| First stage—number of siblings | | |
| Infertility | −0.552[c] | −0.546[c] |
| | (0.092) | (0.089) |
| Miscarriage | −0.470[c] | −0.439[c] |
| | (0.096) | (0.095) |
| Angrist-Pischke $F$-statistic instrument(s) | 30.83 | 30.05 |
| Municipality × wave indicators | Yes | No |
| Municipality × parents' education indicators | No | Yes |
| Individual's controls | Yes | Yes |
| Mother's controls | Yes | Yes |
| Father's controls | Yes | Yes |
| Municipality indicators | Yes | Yes |
| Observations | 26,743 | 26,743 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

## Appendix C: Sibship size including deceased children

**Table 16** Birth order effects on child's "netted migration" status

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Female | −0.035[c] | −0.035[c] | −0.031[c] | −0.031[c] |
| | (0.003) | (0.003) | (0.005) | (0.007) |
| Birth order | −0.017[c] | | −0.016[c] | |
| | (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[a] | | −0.016[a] |
| | | (0.007) | | (0.008) |
| Birth order 4 | | −0.031[c] | | −0.030[c] |
| | | (0.010) | | (0.011) |
| Birth order 5 | | −0.057[c] | | −0.055[c] |
| | | (0.012) | | (0.013) |
| Birth order 6 | | −0.076[c] | | −0.070[c] |
| | | (0.015) | | (0.016) |
| Birth order 7 | | −0.091[c] | | −0.081[c] |
| | | (0.017) | | (0.018) |
| Birth order 8 | | −0.120[c] | | −0.116[c] |
| | | (0.020) | | (0.022) |
| Birth order 9 | | −0.133[c] | | −0.143[c] |
| | | (0.023) | | (0.025) |
| Birth order 10+ | | −0.164[c] | | −0.158[c] |
| | | (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 order 6 × female | | | | −0.013 |
| | | | | (0.012) |
| Birth order 7 × female | | | | −0.023[a] |
| | | | | (0.014) |
| Birth order 8 × female | | | | −0.007 |
| | | | | (0.016) |

**Table 16** (continued)

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Birth order 9 × female | | | | 0.022 |
| | | | | (0.019) |
| Birth order 10+ × female | | | | −0.012 |
| | | | | (0.019) |
| Age | 0.022$^{b}$ | 0.022$^{b}$ | 0.022$^{b}$ | 0.022$^{b}$ |
| | (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 |
| Household 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 |

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

**Table 17** Sibship size effect on child's "netted migration" status: WLS estimates

| Variables | (1) | (2) |
|---|---|---|
| Number of siblings | 0.010$^{c}$ | 0.013$^{c}$ |
| | (0.001) | (0.001) |
| Number of siblings × female | | −0.007$^{c}$ |
| | | (0.001) |
| Female | −0.032$^{c}$ | −0.030$^{c}$ |
| | (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 |

The dependent variable is "netted migration" (see Section 3). The model is estimated using WLS (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,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

**Table 18** Sibship size effect on child's "netted migration" status: 2SLS estimates

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Second stage | | | |
| Number of siblings | 0.002 | −0.015 | −0.005 |
| | (0.014) | (0.024) | (0.013) |
| Female | −0.032[c] | −0.033[c] | −0.032[c] |
| | (0.003) | (0.003) | (0.003) |
| IV: | Infertility | Miscarriage | Overidentified |
| Anderson-Rubin $F$-statistic | 0.023 | 0.433 | 0.232 |
| | [0.880] | [0.510] | [0.793] |
| Hansen $J$-statistic | | | 0.419 |
| | | | [0.517] |
| First stage—number of siblings | | | |
| Infertility | −0.475[c] | | −0.472[c] |
| | (0.095) | | (0.095) |
| Miscarriage | | −0.411[c] | −0.407[c] |
| | | (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 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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,b,c] Statistical significance at 10, 5, and 1 percent level, respectively

**Table 19** Child gender and sibship size effect on child's "netted migration" status: 2SLS estimates

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Second stage | | | |
| Number of siblings | 0.002 | −0.065 | −0.006 |
| | (0.016) | (0.057) | (0.015) |
| Number of siblings × female | −0.001 | 0.105 | 0.007 |
| | (0.013) | (0.084) | (0.013) |
| Female | −0.032[c] | −0.059[c] | −0.034[c] |
| | (0.004) | (0.022) | (0.004) |
| | | | |
| IV: | Infertility | Miscarriage | Overidentified |
| Anderson-Rubin $F$-statistic | 0.012 | 1.577 | 0.797 |
| | [0.989] | [0.207] | [0.527] |
| Hansen $J$-statistic | | | 2.925 |
| | | | [0.232] |
| | | | |
| First stage—number of siblings | | | |
| Infertility | −0.557[c] | | −0.555[c] |
| | (0.107) | | (0.107) |
| Infertility × female | 0.168 | | 0.190[a] |
| | (0.115) | | (0.114) |
| Miscarriage | | −0.390[c] | −0.387[c] |
| | | (0.113) | (0.113) |
| Miscarriage × female | | 0.046 | −0.046 |
| | | (0.106) | (0.102) |
| Angrist-Pischke $F$-statistic instrument(s) | 31.59 | 5.74 | 19.49 |
| | | | |
| First stage—number of siblings × female | | | |
| Infertility | 0.135[c] | | 0.136[c] |
| | (0.039) | | (0.038) |
| Infertility × female | −0.689[c] | | −0.686[c] |
| | (0.134) | | (0.134) |
| Miscarriage | | −0.066 | −0.068 |
| | | (0.044) | (0.044) |
| Miscarriage × female | | −0.287[b] | −0.280[b] |
| | | (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 |

**Table 19** (continued)

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Municipality indicators | Yes | Yes | Yes |
| Observations | 26,743 | 26,743 | 26,743 |

The dependent variable is "netted migration" (see Section 3). Observations are weighted by the inverse of the standard error of "netted migration" because the dependent variable is estimated. 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. $^{a,b,c}$Statistical significance at 10, 5, and 1 percent level, respectively

## Appendix D: Pyschological effects of miscarriage

As already mentioned in Section 3.2, miscarriage may be a traumatic event, creating a special bond between a mother and her children or a higher need for care, which may reduce the likelihood of offspring migration. Although we do not have measures of mother's mental health, in this section we seek to shed light on this issue.

In a recent paper, van den Berg et al. (2017) show that a child's death represents one the largest losses that an individual can face and has adverse effects on parents' labor income, employment status, marital status and hospitalization. Based on that paper, we assume that a child death should produce more negative psychological effects on mothers than a miscarriage. We also assume that a miscarriage later in the pregnancy should produce more emotional distress than an early miscarriage (i.e., the intensity of the child-mother bond depends on the duration of the interrupted pregnancy). We test for "grief" effects by including in the 2SLS regressions an indicator variable for child death and, alternatively, the duration of the interrupted pregnancy because of stillbirth as control variables.[42] The coefficients on both variables, which are reported in columns 1, 2, and 4 to 7 of Table 20, respectively, are not statistically significant. Finally, in column 3 of Table 20, we leverage on the fact that we have two excluded instruments and run an overidentification test, which is based on the validity of the infertility instrument. In particular, we include the miscarriage indicator only in the second stage of a just-identified model. The coefficient on miscarriage is not statistically significant in the second stage, and suggests that it does not have a direct effect on child migration over and above the effect on family size, identified by infertility shocks. All these checks suggest that the direct effect of miscarriage on child migration is not a major issue in our analysis.

[42] In line with the medical definition, stillbirth episodes are different from miscarriages: the former refer to a loss between the sixth and the ninth month, while the latter to a loss during the first five months of pregnancy.

**Table 20** Threats to identification: effect of "grief" on child's "netted migration" status

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Instrument | Infertility | | | Miscarriage | | Overidentified model | |
| Number of children | 0.004 | 0.004 | 0.004 | −0.018 | −0.018 | −0.005 | −0.005 |
| | (0.014) | (0.014) | (0.014) | (0.023) | (0.022) | (0.012) | (0.012) |
| Female | −0.033[c] | −0.033[c] | −0.033[c] | −0.033[c] | −0.033[c] | −0.033[c] | −0.033[c] |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Child death (dummy) | −0.001 | | | −0.002 | | −0.002 | |
| | (0.004) | | | (0.004) | | (0.004) | |
| Months of stillbirth | | −0.000 | | | −0.001 | | −0.000 |
| | | (0.000) | | | (0.001) | | (0.001) |
| Miscarriage | | | 0.010 | | | | |
| | | | (0.011) | | | | |
| Individual's controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mother's controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Father's controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Municipality indicators | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 26,743 | 26,743 | 26,743 | 26,743 | 26,743 | 26,743 | 26,743 |

The dependent variable is "netted migration" (see Section 3). In this table, we investigate the psychological costs of miscarriage. In column 1, we add an indicator for a child death; in column 2, we add the duration of stillbirth; and in column 3, we leverage the fact that we have two instruments and use a just-identified model in which miscarriage is included in the second stage of 2SLS and infertility status is used as the excluded instrument. [a,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

## Appendix E: Household-level estimates

Results of the household level estimates are reported in Table 21. Column 1 shows that a unit increase in the number of children is associated with an average increase in the number of migrants of 0.02 ($t = 12.3$).

Column 2 reports the 2SLS 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. Indeed, women with only one child are those who may have suffered from more severe sub-fertility conditions and for whom the instrument is likely to be stronger (see Table 2 in the main text). In spite of the higher strength of the instrument, the second stage does not show any evidence of a statistically significant effect of fertility on migration. Column 3 reports the 2SLS 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

**Table 21** Family size effect on the number of migrants: household-level estimates

| Variables | (1)<br>OLS | (2)<br>2SLS | (3)<br>2SLS | (4)<br>2SLS |
|---|---|---|---|---|
| Second stage | | | | |
| Number of children | 0.020[c] | −0.004 | −0.031 | −0.011 |
| | (0.002) | (0.015) | (0.031) | (0.014) |
| IV: | — | Infertility | Miscarriage | Overidentified |
| Anderson-Rubin $F$-statistic | | 0.078 | 1.050 | 0.553 |
| | | [0.780] | [0.306] | [0.575] |
| Hansen $J$-statistic | | | | 0.657 |
| | | | | [0.418] |
| First stage—number of children | | | | |
| Infertility | | −0.753[c] | | −0.750[c] |
| | | (0.062) | | (0.062) |
| Miscarriage | | | −0.476[c] | −0.469[c] |
| | | | (0.071) | (0.071) |
| Angrist-Pischke $F$-statistic instrument(s) | | 145.4 | 45.05 | 96.20 |
| 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 |

The dependent variable is the total number of children in the household who ever migrated. 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. [a,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

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. In Table 22, we report the estimates of the same model as above while using an indicator for the household having at least one migrant child as dependent variable and results do not change.

These findings are consistent with those reported in Section 4.2, pointing to a positive correlation between family size and migration, but excluding a 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 estimate because of endogeneity, suggesting that families more likely to send young migrants abroad tend to also have more children.

**Table 22** Family size effect on having at least a migrant child: household-level estimates

| Variables | (1) OLS | (2) 2SLS | (3) 2SLS | (4) 2SLS |
|---|---|---|---|---|
| Second stage | | | | |
| Number of children | 0.012[c] | 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—number of children | | | | |
| Infertility | | −0.753[c] | | −0.750[c] |
| | | (0.062 ) | | (0.062) |
| Miscarriage | | | −0.476[c] | −0.469[c] |
| | | | (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 |

The dependent variable is a dummy for the household having at least one migrant child. The list of control variables is the same as in Table 21. $p$-values are reported in brackets. [a,b,c]Statistical significance at 10, 5, and 1 percent level, respectively

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