Siblings and Leaving the Parental Home*

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January 20, 2026

Abstract

We investigate whether the decision of young adults on when to leave the parental home is influenced by the number of siblings they have, in the context of European countries over the last seventy years. Using data from two large surveys and exploiting random variation in sibship size induced by twin births, we identify the causal effect of having an extra sibling on the timing of home-leaving. We find that one additional sibling speeds up the transition to independent living by roughly six months. We provide evidence that our results directly stem from a decrease in the value of inter-generational co-residence implied by having an extra sibling.

Keywords: parental home, inter-generational co-residence, youth emancipation, siblings.

JEL classification numbers: D10, J11, J12, J13

*We are grateful for the valuable comments by Thomas Crossley, Alice Dominici, Andrea Ichino, and Alessandro Tarozzi. We thank participants to the 2022 Naples School of Economics and Finance PhD and Post-Doctoral Workshop, the 2022 Postgraduate Population Studies Conference, the 2023 PopDays conference, the 2023 PAA Annual Meeting, and EUI working groups for helpful feedback.

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1 Introduction

The proportion of young adults living in the parental home has been rising across the world over the starting years of the twenty-first century (Esteve and Reher, 2021). Ranging from the US and Canada to Latin America and Mediterranean Europe, individuals from recent cohorts left their parental homes much later than those from previous generations did. As the timing of home-leaving has been found to be a relevant determinant of many life-course outcomes (ranging from fertility choices to geographic mobility and wages), a vast literature emerged trying to understand the causes of the observed postponement of home-leaving: notable explanations include the spread of unstable employment (Becker et al., 2010), the postponement of union formation (Mazurik, Knudson, and Tanaka, 2020) and shifts in cultural norms (Giuliano, 2007).

In this paper, we study an additional channel that might have driven the increase in inter-generational co-residence[1]: past fertility trends and their effect on family structure. Due to the plunge in fertility rates that occurred in the second half of the last century, youths born after the seventies have, on average, fewer siblings than individuals from previous cohorts. We ask whether the number of siblings one grows up with (*sibship size*) has a causal effect on home-leaving patterns. Growing up in larger families (which were more prevalent before the seventies) could either speed up or slow down the home-leaving process, depending on two sets of channels. First, the number of siblings one grows up with may influence the value of living in the parental home, ultimately affecting the child's decision on the optimal timing of home exit. To start with, having an extra sibling could make the parental home more crowded, thereby increasing privacy costs associated with co-residence and making it a less appealing option. At the same time, if ties among siblings are strong, having an additional sibling could increase the benefit of living in the parental household, leading to a delayed exit. Secondly, having an extra sibling could affect life-course trajectories due to parental choices, via a resource dilution mechanism à la Becker (1973), with ambiguous effects. On the one hand, lower parental investment per child may lead to worse labor market opportunities and a slower exit from the parental household; on the other, it could result in a shorter transition to adulthood as a consequence of lower educational achievement and an anticipated entry into the labor market.

We combine data from two different cross-national studies, the *Survey of Health, Ageing and Retirement* (SHARE) and the *Generations and Gender Survey* (GGS), obtaining detailed information

[1] Throughout the paper, we define the *home-leaving age* or *nest-leaving age* as the age at which an individual leaves the parental home for good to start living independently. When we refer to *co-residence* or *inter-generational co-residence*, we define it as the individual residing in the parental home, as opposed to having left to live independently.

on family structure and on the timing of nest-leaving by adult children for a large sample of European households over many decades. The European setting is interesting because both the levels and the time trends of inter-generational co-residence rates are highly heterogeneous, with Mediterranean countries experiencing both the highest rates of young adults living with their parents and the steepest increases over time. We exploit random variation in the number of siblings induced by twin births at the second parity to identify the causal effect of having two siblings (as opposed to one) on the home-leaving patterns of firstborn children. We combine an instrumental variables approach with survival analysis to estimate the causal effect of an additional sibling on age profiles of home-leaving and on the average exit age.

We find that having an additional sibling speeds up the home-leaving of firstborn children, decreasing their expected exit age by approximately six months, according to our preferred specifications. Results are similar using information from either dataset and are robust to functional form assumptions regarding the survival function. In a heterogeneity analysis exercise, we show similar and precisely estimated effects on average leaving-home age in Nordic and Western countries, while estimates for Mediterranean countries are rather imprecise. Mediterranean countries have experienced a sharper decline in fertility and a larger rise in inter-generational co-residence. These patterns make it tempting to view the similar point estimate as evidence that sibship-size changes explain part of the divergence in inter-generational cohabitation trends that we observe in Europe. However, the estimate is too imprecise to support such an interpretation, and we cannot draw clear conclusions about the role of trends in household size in explaining these divergences.

Exploiting data on the educational achievement of SHARE respondents, we provide evidence that rules out resource dilution as the main mechanism. More precisely, (i) individuals with additional siblings induced by a twin birth are not statistically less likely to hold a tertiary degree and (ii) despite not displaying the same home-leaving patterns in terms of hazard rates, individuals who hold a tertiary degree leave at the same age as those who do not, on average. Our results are consistent with an extra sibling leading to a fall in the value of co-residing with parents. We test this interpretation by performing heterogeneity analyses with respect to the age distance between the first and second parity, exploiting age differences as a proxy for *siblings' ties*.[2] We find that the speeding-up effect of an extra sibling is the strongest when age differences

[2]Several papers in demography and sociology find that age differences are a relevant predictor of ties among siblings in terms of contact frequency, emotional closeness, and practical support. Smaller age gaps are systematically associated with stronger ties (see, for example, Voorpostel et al. (2007) and Tanskanen and Rotkirch (2019)).

are the largest. We argue that this is consistent with the idea that the *crowded nest* effect resulting from a rise in sibship size is increasingly offset by the direct benefit of living with siblings as ties get stronger. We show this to be consistent for different countries and for men and women separately.

Delayed home-leaving has been found to be a determinant of birth postponements, eventually leading to low fertility (Giuliano, 2007). Besides its effects on birth rates, the evidence on the implications of a late transition to adulthood is mixed: on one hand, it is associated with reduced degrees of geographic mobility (Becker et al., 2010) and lower incomes (Billari and Tabellini, 2011); on the other, studies show that in some countries inter-generational co-residence is positively associated with happiness levels of both parents and children (Manacorda and Moretti, 2006) and that a longer stay in the parental household allows young adults to insure against unemployment shocks (Kaplan, 2012).

As these results showed the relevance of youth nest-leaving choices, a vast literature emerged aimed at investigating their determinants, to understand the causes of such widespread increases in co-residence rates. Existing papers have shown that the timing of youth home-leaving is affected by many factors, including parental resources (Manacorda and Moretti, 2006; Angelini and Laferrère, 2013; Stella, 2017; Angelini, Bertoni, and Weber, 2022), youths' labor market opportunities (Aassve, Billari, and Ongaro, 2001), housing prices (Modena and Rondinelli, 2012) and credit constraints (Fogli, 2004). Recent research has shown that rising co-residence rates are the consequence of structural changes such as labor market uncertainty and the spread of unstable employment (Becker et al., 2010), increasing debt holdings (Dettling and Hsu, 2018), the postponement of union formation (Mazurik, Knudson, and Tanaka, 2020), rising house prices relative to income (Cooper and Luengo-Prado, 2018; Rosenzweig and Zhang, 2019) and shifts in cultural norms (Giuliano, 2007).

Some studies have instead focused on the effect that different characteristics of the family of origin have on the decision to move out for independent living: examples include the presence of step-parents or step-siblings (Mitchell, Wister, and Burch, 1989; Goldscheider and Goldscheider, 1998) and the quality of parent-child and marital relations (Ward and Spitze, 2007). In this paper, we causally study the role played in home-leaving decisions by a relevant characteristic of the family of origin: the number of siblings an individual grows up with. Previous studies (Gierveld, Liefbroer, and Beekink, 1991; Ward and Spitze, 2007; Chiuri and Del Boca, 2010) only include the number of siblings as an explanatory variable in their empirical models, obtaining mixed results. Other papers study the impact of siblings' choices on nest-leaving decisions: examples include Aparicio-Fenoll and Oppedisano (2016) and Her, Vergauwen, and Mortelmans (2022). However, there are no studies whose main focus is to establish the effect of sibship size on the timing of youth emancipation, exploiting and discussing reasonable sources of exogenous variation.

Our main contribution is to evaluate the causal effect of an important characteristic of the family of origin, namely the number of siblings one grows up with, on the home-leaving decisions of daughters and sons; therefore, we contribute to the literature that studies the determinants of nest-leaving choices. We rely on survival analysis and estimate the *speed* at which the individuals leave the nest, which is both our main goal and a novel approach: most of the previous literature, in fact, focuses on the probability of living with parents in a particular moment of time, without backing out the change in the average age at which individuals leave their parental home. Exceptions are Angelini, Bertoni, and Weber (2022) and Angelini and Laferrère (2013), who study the role of parental resources and focus on cohorts old enough that virtually all individuals have already left the parental home. By design, their samples are not affected by censoring. Our study on sibship size, in contrast, tackles censoring directly, which allows us to extend the analysis to more recent cohorts. The relevance of this micro-level determinant may hint toward a potential macro-level relationship between two demographic variables: past fertility rates and current rates of inter-generational co-residence.

2 Data

We use information from two different data sources: the first seven waves of the *Survey of Health, Ageing and Retirement in Europe* (SHARE), and the first two waves of the *Generations and Gender Survey* (GGS). SHARE is a large-scale, cross-national longitudinal study representative of the population aged 50 or older (and their partners) in 27 European countries. The survey contains a wide array of information on the life course of around 140,000 respondents. Similarly, GGS is a longitudinal survey containing information on around 200,000 adults coming from 19 European countries. Starting from 2004, both surveys collect information on individuals' working status, partnership, and fertility, among others.

These sources are particularly suitable for our study as they contain detailed information on all the respondents' children: in particular, in both surveys, respondents are asked about the year in which each of their children moved out of the parental house.[3] Interviewers are explicitly asked to instruct respondents to refer only to the last move-out episode, in case an adult child returned home after a failed emancipation attempt (boomeranging). Moreover, temporary departures from the parental home (for example, for a study period or military service) do not count as leaving-home episodes. Family structure is the second key ingredient for our analysis. This is strictly related to our identification strategy, which relies on twin births. For individuals interviewed in SHARE, we define twins as individuals born in the same year and from the same mother. In GGS, the respondents are asked to indicate the month of birth of their children, allowing us to define twins as siblings born in the same month-year combination and from the same mother.

[Table 1]

Sample restrictions. Throughout the analysis, we impose some restrictions on our sample motivated by our identification strategy. First, we select couples without foster, adopted, or step-children to avoid misclassifying twins. Since the analysis is carried out by exploiting twin births at the second parity, it is common in studies leveraging the same source of variation to restrict the analysis to non-twin children. Therefore, the population of interest is based on non-twin firstborns in families with at least two children. Ideally, to relate our findings to broader demographic trends, we would have liked to test our theory by comparing families with one versus two children. In response to this limitation, our analysis will be confined to the effect of an additional sibling at the smallest parity that our strategy enables us to consider. Moreover, as we focus on an outcome (youth home-leaving) usually determined in early adulthood, we exclude individuals under 16. Finally, we exclude respondents whose reported nest-leaving age is below 14 or above 40, as their decision to move out was likely determined by peculiar circumstances.

As just mentioned, the population of interest is restricted to firstborn children. When evaluating the external validity of the findings, it is important to account for how this subgroup differs from the broader population of children. A large literature (see Härkönen and Santacroce (2024) for a review) documents systematic birth order effects: relative to later-born siblings, firstborns exhibit advantages across several domains, including labor market outcomes (e.g.,

[3] We exploit the panel nature of SHARE to get the most up-to-date information on each child, eliciting the home-leaving details from the last wave in which each family appears. For instance, if a family is observed both in 2007 and in 2010, we could note that children still living with their parents in 2007 moved out before 2010. In those cases, the repeated observations enable us to obtain additional information on the timing of home-leaving.

earnings and occupational status), cognitive ability, and educational attainment. However, extending our findings to demographic trends requires asking whether firstborns in two-child families differ systematically from only-children. Credibly addressing this comparison is challenging. In a recent study by Ilciukas et al. (2025), the authors exploit quasi-random success in vitro fertilization (IVF) treatments in Denmark to compare firstborns who remain only children with those who gain a sibling. They find that the two groups are very similar in school test scores and personality traits. This suggests that differences across children of different parities may matter more than whether firstborns grow up with a sibling or not.

Summary statistics. In Table 1, we show summary statistics for our sample, separately for GGS, SHARE, and different country groups. In both datasets, the sample is balanced in terms of gender, and the average number of siblings is around one and a half. The probability of experiencing a twin birth at the second parity is around 1.3 percent in SHARE and 1.1 in GGS. Around 86 percent of individuals left the parental home in SHARE, while 65 percent did so in GGS. The difference is mostly attributable to GGS respondents being from more recent cohorts and more likely to be younger. Finally, the average age at which individuals leave the parental home is 23. In Figure 1, we zoom in on home-leaving age by displaying its distribution, together with the distribution of age conditional on not having left the parental home.

[Figure 1]

For a subset of individuals, we exploit retrospective data provided by their parents on family characteristics before birth. For individuals interviewed in SHARE, we use data on the parental labor market, education, and housing provided in SHARELIFE - a detailed retrospective survey on people's life histories. We can retrieve complete information on 45% of SHARE respondents. We complement it with data on the education of grandparents for GGS respondents (around 62%). We make use of this additional information to corroborate the randomness assumption of the twin births instrument and to test the robustness of our results. There is evidence that the probability of having twins increases with the mother's age, and that twin births are also correlated with maternal education (Rosenzweig and Zhang, 2009). This correlation arises because women with higher education tend to have children at older ages. Since maternal education could therefore act as a confounder, it is important that we observe this information in SHARE, while in GGS we rely on the available proxy based on grandparents' education.

3 Empirical strategy

As our outcome of interest is the timing of a life-course event, we make use of survival analysis as the main modeling tool. Survival analysis provides a suitable framework to deal with censored observations, which constitute a sizable portion of our sample. Censoring may happen for two reasons: either (i) an adult child has not moved out yet when the parent is interviewed (*right-censoring*), or (ii) the adult child has already moved out when the interview takes place, but the responding parent does not precisely remember (or refuses to share) the year in which her child moved out (*left-censoring*).[4]

Estimands. Let a denote age and n denote the number of siblings an individual has. The survival function $S(a|n)$ describes the probability that an individual with n siblings is still living in the parental home at age a, i.e.,

$$S(a \mid n) = \Pr(T > a \mid n), \tag{1}$$

with the random variable T denoting the home-leaving age. We are interested in the effect of n on home-leaving profiles: our object of interest is the function

$$\frac{\partial S(a \mid n)}{\partial n} \tag{2}$$

i.e., the derivative of the survival function with respect to the number of siblings n. The second estimand of interest is the implied change in the average age at which individuals leave their parental home brought by an additional sibling. Let $P(a, n)$ be the probability of leaving home at age a for an individual with n siblings. We have that

$$P(a \mid n) = S(a \mid n) - S(a + 1 \mid n)$$

Define now as $E(n)$ the average home-leaving age for an individual with n siblings. Let $\mathcal{A} = \{\underline{a}, \underline{a} + 1, ..., \overline{a} - 1, \overline{a}\}$ denote the set of ages at which it is possible to experience the event. Under the assumption that $S(\underline{a}) = 1$ and $S(\overline{a}) = 0$ (everybody leaves home after age $\underline{a} - 1$ and before

[4]For SHARE respondents, when information on co-residence status is missing, we are able to derive it by exploiting a key variable available in the data: geographical proximity between the responding parent and each child. The *Proximity* variable can take several values (*In the same household*, *In the same building*, *Less than 1km away*, ... , *More than 500km away*). If it takes a value different from *In the same household*, we code the individual as having left the parental home. We cannot retrieve complete information on past leaving-home decisions for 6% of the observations, which we define as *left-censored*.

age $\overline{a}+1$) we have that:

$$E(n)=\sum_{a\in\mathcal{A}}P(a\mid n)\cdot a.$$

Our second estimand of interest is therefore defined as

$$\frac{\partial E(n)}{\partial n}=\sum_{a\in\mathcal{A}}\frac{\partial P(a\mid n)}{\partial n}\cdot a=\sum_{a\in\mathcal{A}}\left(\frac{\partial S(a\mid n)}{\partial n}-\frac{\partial S(a+1\mid n)}{\partial n}\right)\cdot a \tag{3}$$

Estimation. Let $Age_{i,t}$ denote the age of individual i at time t and define $S_{i,t}=\mathbb{1}\left(T_i>Age_{i,t}\right)$, a dummy variable equal to one if individual i is still living with her parents at age $Age_{i,t}$ and equal to zero otherwise. Moreover, let $NSiblings_i$ represent the number of siblings that individual i has. A natural empirical counterpart of (1) is the model

$$S_{i,t}=\sum_{j=0}^{k}\alpha_j Age_{i,t}^{j}+\sum_{j=0}^{k}\beta_j(NSiblings_i\times Age_{i,t}^{j})+\gamma\mathbf{X}_{i,t}+\epsilon_{i,t}, \tag{4}$$

where $\mathbf{X}_{i,t}$ is a vector of controls. Model (4) is therefore a standard discrete-time event history model where we model the survival $S_{i,t}$ instead of the hazard $h_{i,t}$[5] and we allow baseline survival probabilities to depend on a kth order polynomial of age: we adopt a flexible specification in which the effect of an additional sibling can vary at different stages of the life cycle. Given specification (4), a natural estimator of (2) is given by

$$\widehat{\frac{\partial S(a\mid n)}{\partial n}}=\sum_{j=0}^{k}\hat{\beta}_j a^j. \tag{5}$$

After estimating $\{\widehat{\partial S(a\mid n)/\partial n}\}_{a=\underline{a},\dots,\overline{a}}$, it is straightforward to obtain $\widehat{\frac{\partial E(n)}{\partial n}}$ via (3). Clearly, a major concern related to the estimation of such a model is the endogeneity of family size. Families with a different number of children are likely to differ in unobserved characteristics we cannot control for, which could lead to biased estimates for the causal effect of an extra sibling.

[Table 2]

[5]This enables us to exploit, at least partially, left-censored histories, i.e. observations related to those young adults that we know had left home *by age* a, even though we cannot tell exactly when. For these individuals, $S_{i,t}$ is going to be equal to zero for all ages larger than or equal to their age when their parents were interviewed. We further depart from the most common approach by using a linear probability model instead of a logistic or a complementary log-log specification. As better detailed in the next paragraph, we do this in order to be able to use a two-stage least squares approach (unfeasible in a nonlinear setting) to address endogeneity of our variable of interest.

Identification. We estimate β_j using an instrumental variable approach and a Two Stage Least Squares (2SLS) estimation strategy, instrumenting $NSiblings$ with the variable $Twins_i$, an indicator equal to one if the parents of individual i experience a twin birth at their second parity, zero otherwise. In Table 2, we show estimates from the first-stage equation, that is, the effect of a twin birth at second parity on the number of siblings. The effect varies across country groups, and is weaker in Southern countries; moreover, it is slightly larger for individuals interviewed in GGS, mostly because it surveys more recent cohorts.[6]

Two features of our instrument $Twins$ are particularly attractive for this setting. First, it generates quasi-random variation in the number of children that operates almost entirely at the margin from two to three children. Figure A.3 documents this by comparing the probability of having three, four, five, or six children for families with and without a twin birth at second parity. Among families who reached two children, the probability of progressing to a third child is roughly 35% in the non-twin group, while it mechanically jumps to 100% for families experiencing twins. In contrast, the probabilities of having a fourth or higher-order child are smaller and are only weakly affected by twins. Hence, the instrument identifies an average causal effect that is primarily computed over families whose fertility increases from two to three children due to the twin shock. Second, the local average treatment effect (LATE) for the compliers coincides with the average effect on the non-treated (ATNT), where the treatment is having an additional child.[7] The ATNT, defined for a very large group of individuals in this setting (around 99% of families do not have twins), is arguably close to the average treatment effect (ATE).

In Table A.1 we show average differences between firstborns from families that experienced a twin birth at second parity and firstborns from those that did not, in terms of predetermined characteristics. We notice that, not surprisingly, mothers with twins are older on average. Several studies have documented that the likelihood of twin birth is increasing in maternal age. Moreover, we notice that twin births are less likely to be registered in Southern and Eastern countries in our sample.[8] For SHARE respondents, we exploit information on the labor market,

[6]The estimates are slightly larger compared to Black, Devereux, and Salvanes (2005) and Angrist, Lavy, and Schlosser (2010), but comparable to other studies such as Angrist and Evans (1998).

[7]The result follows the reasonable assumption of one-sided perfect compliance as a consequence of the absence of never-takers (see Angrist, Lavy, and Schlosser (2010) for a discussion). In this setting, never-takers are defined as families who decide to have a child and, despite a twin pregnancy, decide to have only one child.

[8]This is especially true for the GGS sample, where we define a twin birth if children are born from the same mother, the same year-month combination. This alleviates potential concerns related to misclassifications of twins induced by higher fertility rates in these country groups. Moreover, it is in line with official statistics about the birth of twins in Europe.

educational outcomes, and living arrangements (years of employment, years of education, type of employment, house ownership, and whether it was located in a rural area), which we measure one year before the birth of the firstborn. All variables are balanced across the two groups, with the notable exception of maternal education. Consistent with recent findings on the relationship between maternal education and twin birth (Bhalotra and Clarke, 2019), we find that mothers of twins have more years of education. For GGS respondents, we do not observe differential educational outcomes of grandparents, which are broadly categorized as low, medium, and high.[9]

Even if exogenous, a shock in the number of siblings induced by twin birth may be problematic when the objective is to study the sole role of sibship size. Twins often possess unique traits that could impact our findings beyond adding another sibling to the household. This concern, commonly referred to as a violation of the exclusion restriction, generally relies on the idea that parents may differentially invest time or resources in twin children as compared to non-twins (Rosenzweig and Zhang, 2009). In the results section, we provide and discuss evidence that such dynamics are not relevant in our context.

4 Results

[Figure 2]

In Figure 2 we plot the marginal effect of sibship size on the probability of still living with parents by age a (equation (2)). Having an additional sibling speeds up the emancipation process. In terms of magnitude, the effect is estimated to be the largest around 23 years old, when children are around seven percentage points (p.p.) less likely to still be living with their parents if they have an extra sibling. As described in the previous section, estimating differences in the survival function allows us to recover the change in the probability of leaving the parental home at age a and, consequently, the change in the average age at which individuals with different numbers of siblings leave home. The estimates shown in Table 3 suggest that having an additional sibling decreases the home-leaving age by approximately ten months. Once we account for country-fixed effects (column (2)), the effect shrinks to six months and remains statistically

[9] Although twins are more likely to be born due to in vitro fertilization (IVF), and mothers who use it may be selectively different, this is unlikely to be an issue in our sample. IVF was first accessible in the early 1980s, and in Table A.2 we show that roughly 32% of our sample consists of families with births at second parity after the 1980s. Moreover, the share of births due to IVF before 1990 was close to zero in almost every country in Europe.

different from zero at conventional levels.[10]

We test the robustness of the result by (i) estimating the model separately for the two datasets (columns (3) and (6) in Table 3), (ii) including sets of controls, and (iii) different parametric assumptions regarding the survival function. As shown in columns (3) and (6), the estimated effects for individuals in GGS and SHARE are both negative (8 and 5 months, respectively). The similarity of the estimates across the two data sources is noteworthy given the differences between the GGS and SHARE samples. GGS covers more recent birth cohorts, provides a larger sample size, and allows for a more precise identification of twin births based on month–year information. SHARE, by contrast, covers older cohorts and identifies twins using only the year of birth, but it offers richer retrospective information on predetermined parental characteristics.[11]

To understand whether predetermined parental characteristics are relevant in explaining the estimated effect, we first estimate the same model for the subsample of individuals for whom we gathered information on parental characteristics, summarized in Table A.1 (columns (4) and (7)). The effect remains similar in magnitude for this population, and once we control for these characteristics in the specification, the effect remains essentially unchanged (columns (5) and (8)). This attenuates the potential omitted variable bias concern related to mothers of twins being different along some relevant dimension. This is particularly true for their years of education, measured in SHARE, which is arguably a relevant characteristic for their children's life trajectories, and the only variable where we see significant statistical differences between the two groups of mothers. Moreover, we relax our parametric assumption on the survival function by estimating a linear probability model, using $S_{i,t}$ as the outcome, separately for each age a. The outcome of the exercise is shown in Figure A.1 and Table A.3, and displays a very similar pattern compared to our main result presented in Figure 2 and Table 3. Finally, we also compare the IV estimates obtained from our linear probability model with the corresponding control–function

[10]Given the inherent difficulty of using analytical standard errors, as $\widehat{\partial E(n)/\partial n}$ is a function of random variables and we are not able to reliably estimate their covariance matrix, we use bootstrap standard errors by randomly resampling within countries. We use 500 simulations, but the estimates of the standard errors do not significantly change if we increase the number of bootstrap samples.

[11]A potential concern with pooling SHARE and GGS data is that differences in measurement protocols could create inconsistencies when combining the datasets, possibly inducing some bias. However, the covariates included in our baseline specification (country fixed effects, child gender, year of birth, and maternal age at birth) are standard demographic variables that are measured consistently across both surveys. Regarding the identification of twins, which differs across the two datasets, in Table A.4 we re-estimate the model on the pooled sample after redefining the GGS twin variable to mimic the SHARE definition (using the "same year" criterion). The resulting estimates remain essentially unchanged compared to the baseline results of Table 3. This exercise alleviates concerns about issues related to pooling datasets when drawing average conclusions.

estimates based on a logit specification.[12] As in the non-parametric exercise, both models are estimated separately for each age. Figure A.2 shows the resulting age-specific effects and shows that the logit control–function estimates closely resemble the IV estimates from the linear model, indicating that our conclusions are not sensitive to the choice between a linear and a nonlinear specification for the survival probability.

[Table 3]

Comparing OLS and IV. As a descriptive complement to the IV analysis, we report OLS (Ordinary Least Squares) estimates on the same sample and model specification. As shown in Table A.5, OLS and IV estimates deliver qualitatively similar results: in both cases, having an additional sibling is associated with an earlier transition out of the parental home. While OLS estimates should not be given a causal interpretation, they display a qualitative pattern similar to the IV results.

Table A.6 reports OLS estimates for a broader population that includes only children and firstborns from smaller families. All sample restrictions described in Section 3 are maintained, with the exception of the restriction on the number of siblings, which is relaxed to include all firstborns. These results indicate that only children tend to leave the parental home later than firstborns with one sibling (Panel A), and that a similar pattern holds when comparing them with firstborns with two siblings (Panel B). The estimated effects are larger than the corresponding OLS estimates for firstborns in our analytical sample. While purely descriptive, the consistency in the direction of the estimates suggests that the relationship between sibship size and home-leaving documented in the IV analysis may plausibly extend to the only-child margin, providing suggestive evidence in support of external validity.

4.1 Heterogeneity

We concentrate on two dimensions of heterogeneity: gender of the firstborn and country groups. We focus on the entire sample, and use a specification in line with the one delivering results we showed in column (2) of Table 3, as country fixed effects are the only variables that affect our estimates once we include them in the model.

[12]The control–function estimates proceed by first estimating the same first-stage equation for $NSiblings_i$ used in the 2SLS approach described in Section 3 (Table 2). The residual from this first-stage regression is then included linearly as an additional control in (4), which is re-estimated separately for each age a using a logit specification for $S_{i,t}$. In this formulation, the outcome equation is assumed to have an error term that follows a logistic distribution, so that model (4) can be estimated by maximum likelihood. We rely on 2SLS rather than a control–function approach because the latter requires stronger functional–form assumptions in non-linear models (Wooldridge, 2015).

Country groups. Inter-generational cohabitation rates differ significantly across the Euro area, with the highest rates found in Mediterranean countries and the lowest in Nordic ones. The same pattern also emerges in our sample, as we can see from country-group differences in the survival functions displayed in the left panel of Figure A.4 and in the average ages at home-leaving reported in Table 1. Heterogeneous responses by country groups are of interest not only because of this reason but also because of the different trends in cohabitation rates across those regions. Using data provided by Eurostat on the percentage of young adults living with their parents, in Figure A.8, we plot their time series evolution for different European countries. Regions characterized by a slow transition to adulthood, such as Southern Europe, are experiencing even more delayed home-leaving patterns over time. In contrast, in Nordic countries, where most young adults leave their parental home early, co-residence rates have been more stable over time. In other words, the gap in home-leaving patterns across European regions is widening. Several papers have suggested reasons why cohabitation rates in Europe are diverging, proposing, among others, cultural (Giuliano, 2007) and economic (Becker et al., 2010) determinants. Another explanation may be found in fertility trends. In Figure A.9 we plot trends in Total Fertility Rates (TFR) for a subset of countries, using again data provided by Eurostat.[13] Comparing Figure A.8 and Figure A.9, it can be noticed that across Europe, the regions that experienced an increase in the share of young adults living with their parents are those where fertility fell more sharply in the last decades of the twentieth century.

[Table 4]

In Table 4, we show the estimated effect of an additional sibling induced by twin birth in different country groups. It seems to be imprecisely estimated closer to zero for Eastern countries while close in magnitude for the other country groups to the average effect. Although this suggests that differential fertility trends—particularly the steeper fertility decline in Mediterranean countries—may partially explain the divergences in emancipation patterns across Europe, we cannot reject the null hypothesis that the effect is zero in Mediterranean countries.[14] As a result, we cannot draw conclusions about whether the decline in TFR, and thus in the number

[13]Until the mid-seventies, the decrease in fertility was stronger for Western and Northern countries, with the other two regions experiencing little to no plunge in TFR. As fertility kept falling in South and East Europe throughout the 1985-2000 period, TFRs in these country groups ultimately fell below those prevailing in Northern and Western countries. Therefore, countries in which cohabitation rates are increasing are the same ones in which the number of siblings is decreasing the most with the two pairs of regions stabilizing on different levels from the early 2000s.

[14]It is worth noting that the impact picks up around the average home-leaving age (see Figure A.6). In Northern and Western countries, it is stronger in the early twenties, while in the Mediterranean, around 28. This evidence suggests that decisions about leaving home are more likely to be influenced by the sibship size around the average age at which individuals decide to live on their own.

of siblings, accounts for part of the differential trends in home-leaving decisions in European countries.[15]

Gender. Differences in home-leaving ages between women and men have been shown to exist in several countries. This holds true for our analytical sample as well. In the right panel of Figure A.4, we plot survival functions by gender for our population of interest. As we can see, women are more likely to leave the parental home faster, both on average and within country groups (see Figure A.10). Differences in nest-leaving decisions by gender have also been shown to be related to different dynamics. Chiuri and Del Boca (2010) find that in Mediterranean countries, higher family resources predict a higher probability of cohabitation for sons but lower for daughters. Moreover, they argue that family structure is more important for daughters' decisions to leave their nest. On the other hand, focusing on returning home decisions in the UK, Stone, Berrington, and Falkingham (2014) find that women are less influenced by family and labor market shocks. In the Swedish setting, Nilsson and Strandh (1999) argue that despite the stronger link between men's home-leaving decisions and parental resources, women's decisions are more correlated with proxies for career concerns, strengthening the idea that the role of culture and family in home-leaving decisions is context-dependent. Analyzing the role of sibship size, in Figure A.5, we plot the marginal effect derived in equation (2) for men and women separately. Effect sizes seem to peak around the same age (4.5 p.p around 23 years old) and to be more persistent over the age profile for men. This translates into different impacts on the average home-leaving age (see Table 4). More precisely, having an additional sibling accelerates exit from the parental home by two additional months for men compared to women; in relative terms, the estimated effect for men is almost 70% larger than that for women. It is worth considering, however, that the estimated gender gap in the effect of an additional sibling on home-leaving ages is still relatively small when compared to the baseline difference in age at which men and women leave their nest.

4.2 Mechanisms

[Table 5]

We argue that there may be two sets of explanations for the delay in home-leaving induced by higher sibship size, which we broadly categorize into *resources* and *preferences*. Following the

[15] There could be heterogeneous results also within each country group, but because twin births are rare, estimating the effect separately for each country leads to noisy estimates. We nevertheless checked the country-specific results and did not find that any single country is driving the overall pattern. Across all countries, about 80% of the estimates are negative, and in the remaining cases the effect is simply too imprecise to interpret.

quality-quantity line of reasoning (Becker, 1973),[16] we expect families with a large number of children to invest less in the human capital of their offspring, with the effect of lower resources on the timing of home-leaving being not clear ex-ante. Another set of explanations concerns preferences for cohabitation. Individuals may face higher privacy costs arising from having an additional sibling, thus potentially anticipating the home-leaving decision.

For almost any respondent in SHARE, we have information on marital status, labor market outcomes, fertility, and education of their children at the moment of the interview. We test whether resource dilution is a mediator of the observed effect by estimating the effect of an additional sibling on educational attainment via 2SLS. As outcomes, we use indicators for having a high school diploma, a post-secondary or a tertiary degree. Results are shown in Table 5. The estimates suggest that firstborns with an additional sibling induced by twins born at second parity do not show statistically significant differences compared to those with one sibling in terms of these outcomes. Some studies have, however, argued about potential violation of the exclusion restriction concerning parental investments (Rosenzweig and Zhang, 2009). Twins may receive lower initial endowments at birth, leading to differing parental responses. We could, therefore, overstate (understate) the role of resources in our setting if parents compensate by investing more (less) in low-endowment children. In Figure A.7, we show survival functions by dividing our SHARE sample based on whether the individual obtained a tertiary degree. Despite the two functions not perfectly overlapping, the average home-leaving age is similar for both groups. This suggests that higher education decisions are not playing a major role in explaining the effect of sibship size, which is precisely estimated as negative and economically non-negligible in every specification we use when average home-leaving age is the outcome of interest. In fact, even if our findings on the relationship between sibship size and educational attainment may not be fully identified, the absence of a systematic association between educational attainment and leaving the parental home supports the conclusion that this channel is not a key mechanism behind our results.

[Table 6]

We then indirectly test whether the direct value of cohabiting with parents decreases when the number of siblings rises. Individuals trade off costs and benefits when deciding whether to keep co-residing with their parents or to move out. Costs associated with inter-generational

[16] The twin instrument has been broadly used to test the existence of a quality-quantity trade-off; see for example Black, Devereux, and Salvanes (2005), Angrist, Lavy, and Schlosser (2010) and Bagger et al. (2021).

co-residence are mainly *privacy costs*, which are potentially higher as the number of siblings grows and the parental home becomes a *crowded nest*. Benefits from staying are due to lower expenditures on food and housing, but also to the direct gain from living with siblings. We hypothesize that this latter benefit is increasing in the extent of ties among siblings, which we proxy with the age distance between the first and secondborn.[17] Heterogeneity along the age distance dimension allows us to indirectly test if a drop in the value of co-residing with parents when an extra sibling is around is a mechanism behind our estimated effects. Indeed, assuming that the change in the cost of co-residing from having one more sibling is constant, but that the benefit is decreasing in the age distance, we expect to see stronger negative effects on the average home-leaving ages when there is a wider age gap between first and secondborns. As we can see from Table 6, results are the strongest when siblings are born more than four years apart. This seems to be true for all the subgroups of interest, with the exception of Western countries, where the effect is similar for individuals with siblings closer in age (one or two years) and with more than four years apart. For Mediterranean countries, in which we could not detect a precise effect on average, we see significant changes in home-leaving patterns when focusing on families with siblings born more than four years apart, with an induced accelerated exit of around 10 months.[18]

Exclusion restriction and mechanisms. It is worth discussing that these mechanisms may spark speculation about potential violations of the exclusion restriction assumption: if twins are less likely to develop a good relationship with the older siblings compared to non-twins, then we may attribute part of the effect to this factor. We try to alleviate these concerns in two exercises. First, the SHARE dataset offers an opportunity to test this idea. In fact, respondents are asked about contact frequency with their children. Using these data, we test whether the presence of twins somehow affects ties in the family in two ways. Results are shown in Table A.8. In Panel A, we show differences between contact frequencies of firstborns from families with and

[17] Some papers investigating the role of sibship size have used spacing as a proxy for ties. See for example Black, Devereux, and Salvanes (2005) and Holmlund, Rainer, and Siedler (2013).

[18] Age distance may also proxy for other family-level characteristics, which could independently affect home-leaving decisions. To assess whether the observed heterogeneity by age distance reflects such alternative mechanisms, we re-estimate the specifications in Table 6 on the subsample of individuals for whom we observe predetermined parental characteristics, and explicitly control for them. The results, reported in Table A.10, suggest that parental characteristics are unlikely to be driving the heterogeneity, as they do not move our estimates once included in the model. To further assess alternative explanations, we examine whether sibling age distance is associated with other outcomes that could point to different channels. In particular, Table A.11 shows OLS coefficients for age distance and measures of contact frequency with parents, as well as for educational attainment. We find no systematic relationship, suggesting that the age-distance heterogeneity is unlikely to be driven by alternative mechanisms.

without twins. In Panel B, we show *within families* differences of contact frequencies between twins and non-twin siblings. Both results indicate that twins do not seem to alter the relationship between children and parents. Second, we use the sex composition of the twins as an indirect test of the idea that stronger bonds between the twins could weaken their relationship with the firstborn and, in turn, influence her home-leaving behavior. Since same-sex twins are more likely to develop close ties, we could expect that the estimated effect on emancipation age being concentrated among firstborns with same-sex twins may raise concerns about this violation. However, when comparing the effect for firstborn with same-sex and opposite-sex twins, we find no evidence that same-sex twins induce a stronger response in leaving the parental home (see Table A.9).

Another source of violation of the exclusion restriction relates to spacing. If two younger siblings are born very close in time, parents may face higher demands on their time and resources, which could lead the firstborn to leave home earlier. To explore this idea, we compare firstborns with two younger non-twin siblings in families where the second and third child are born only one year apart to those where they are spaced further apart, keeping the spacing between the first and second child fixed. This provides a useful analogue to the twin setting, as having two closely spaced younger siblings may create similar pressures. Results are shown in Figure A.11. We find no meaningful differences in home-leaving behavior between these groups, suggesting that birth spacing does not drive our results.

Identifying other potential violations of the exclusion restrictions that would significantly impact our analysis is challenging. As previously mentioned, most of the existing literature centers on the argument that twin births may not be suitable for testing resource dilution in certain contexts. However, we do not consider this a concern for our conclusions, given the lack of an association between educational attainment and the average age at which individuals leave the parental home. Other twin-related factors that have been highlighted in the literature—such as altered parenting styles or economies of scale in child-rearing (Farbmacher, Guber, and Vikström, 2018)—could theoretically influence our findings. However, these effects would likely manifest through differences in the relationships with parents or siblings. We provide two tests for these differences and show that these considerations are not relevant for our sample.

Quantifying the importance of these channels goes beyond the scope of the paper, given the inherent challenge of indirectly testing their existence through heterogeneity analysis. The primary insight from this section highlights how shifts in the value of cohabiting with parents and siblings induced by a fall in family size may have played a role in the increase in cohabitation rates.[19]

5 Conclusion

In this paper, we ask whether past fertility trends and their impact on family structure have an effect on the home-leaving choices of young individuals. Exploiting random variation in sibship size induced by twin births, we estimate the causal effect of having an additional sibling on the timing of home-leaving in Europe over the last six decades of the twentieth century, combining two datasets that contain rich information on family structure and nest-leaving choices of young adults. Our results indicate that having more siblings speeds up the transition to independent living.

We provide evidence that a resource dilution mechanism à la Becker (1973) is not likely to drive our results, and we interpret our findings as evidence that when the number of siblings increases, the value of co-residence with parents goes down. We indirectly test this mechanism by performing heterogeneity analysis with respect to the age distance between the first and second parity, which has been shown to be a proxy for *sibling ties*. Effects are stronger when siblings are born more years apart, consistent with individuals taking into account the costs and benefits of co-residing with parents when deciding on their living arrangements. Therefore, our findings indicate that having an extra sibling increases the costs of co-residence, possibly through a *crowded nest* effect: as the house gets more crowded, privacy costs rise.

It is important to consider the increasing prevalence of families with only one child when linking our micro-level evidence to the macro-level relationship between fertility and leaving home ages. Even though our empirical strategy only enables us to illustrate the effects of having two siblings instead of one, we conjecture that our estimates are informative about the effect of having one sibling instead of being an only child. Our main mechanism could still be in place,

[19]There may be other ways preferences for cohabitation have evolved. In adult age, parental care might also play a role: since firstborn children may free-ride on their younger siblings to take care of their old parents (Rainer and Siedler, 2009; Maruyama and Johar, 2017; Stern, 2021) a larger number of siblings might reduce the value from co-residence that stems from caregiving needs. Studies show that an additional sibling leads an individual to live further away from parents (Holmlund, Rainer, and Siedler, 2013), potentially leading to better labor market outcomes (Rainer and Siedler, 2009; Maruyama and Johar, 2017). In this framework, we could also expect an individual to leave her nest faster, being less likely to co-reside at older ages as a consequence of having an additional sibling. Our results are in line with the prediction of these papers. We do not emphasize this channel as it arguably plays a minor role in explaining the fall in cohabitation preferences.

arguably being more relevant, when comparing an only child with individuals with one or more siblings.

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Tables

Table 1: Summary statistics.

	(1) SHARE	(2) GGS	(3) Eastern	(4) Northern	(5) Southern	(6) Western
Female	0.497	0.486	0.493	0.494	0.478	0.493
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Birth year	1970.940	1975.843	1972.705	1973.637	1975.913	1973.279
	(11.238)	(11.650)	(10.621)	(11.573)	(13.114)	(11.854)
Age	38.430	31.450	35.199	34.411	33.035	34.967
	(10.608)	(10.505)	(10.897)	(11.172)	(10.954)	(11.271)
Left home	0.868	0.649	0.738	0.813	0.595	0.799
	(0.338)	(0.477)	(0.440)	(0.390)	(0.491)	(0.400)
Home-leaving age	23.282	23.164	23.935	21.298	25.626	22.706
	(4.435)	(4.147)	(4.348)	(3.503)	(4.680)	(3.866)
Number of siblings	1.610	1.522	1.492	1.556	1.509	1.665
	(0.925)	(0.852)	(0.865)	(0.831)	(0.855)	(0.949)
Twins	0.013	0.010	0.009	0.013	0.008	0.013
	(0.112)	(0.100)	(0.097)	(0.113)	(0.090)	(0.115)
Age distance to second born sibling	3.751	3.885	3.958	3.941	4.026	3.504
	(2.791)	(2.797)	(2.882)	(2.766)	(2.754)	(2.721)
Average parental age at birth	25.317	24.641	23.635	25.081	26.036	25.523
	(4.572)	(4.217)	(3.892)	(4.294)	(4.672)	(4.451)
Observations	42672	52441	29109	19738	16797	29469

Note: The table reports sample averages for our analytical sample. Standard deviation in parentheses. Northern countries include Sweden, Denmark, Estonia, Lithuania, and Norway. Southern countries include Italy, Spain, Greece, and Portugal. Central/ Eastern countries include the Czech Republic, Croatia, Hungary, Poland, Slovenia, Bulgaria, and Romania. Finally, Western countries include Belgium, Switzerland, Germany, France, Ireland, Luxembourg, and the Netherlands. Age refers to the unit's last observed time point, meaning the oldest age recorded in the sample.

Table 2: First stage: twin births and number of siblings.

	Dependent variable: Number of siblings						
	Full sample	SHARE	GGS	Eastern	Nordic	Mediterranean	Western
Twins	0.881***	0.935***	0.815***	0.835***	0.972***	0.863***	0.829***
	(0.025)	(0.039)	(0.031)	(0.043)	(0.053)	(0.063)	(0.044)
Baseline mean	1.552	1.599	1.514	1.484	1.543	1.502	1.654
Observations	95113	42672	52441	29109	19738	16797	29469

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
The Table shows estimates from the first-stage equation, i.e., the OLS coefficient from a model with $Twins$ as an explanatory variable and $Nsiblings$ as the outcome. The baseline mean reports the average number of siblings for individuals with no twins at second parity.

Table 3: Effect of an additional sibling on the expected home-leaving age.

	Full data		GGS			SHARE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{\partial E(n)/\partial n}$ (*years*)	-0.91^{***}	-0.52^{***}	-0.72^{***}	-0.74^{***}	-0.75^{***}	-0.37^{*}	-0.7^{**}	-0.72^{**}
	(0.18)	(0.16)	(0.25)	(0.28)	(0.29)	(0.22)	(0.32)	(0.31)
Country FE		✓	✓	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓	✓	✓
Birth year	✓	✓	✓	✓	✓	✓	✓	✓
Mother age at birth	✓	✓	✓	✓	✓	✓	✓	✓
Grandparents educ.					✓			
Parental covariates								✓
Observations	95113	95113	52441	33288	33288	42672	19946	19946

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Parental covariates include employment, educational attainment one year before having the first child, homeownership status, an urban/rural dummy, and home-leaving age. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four. Columns (1), (2),(3) and (6) show estimates for the full dataset, while the other columns show estimates for the subset of data for which we have full information for all children (i.e. no missing covariates).

Table 4: Effects on home-leaving ages by country group and gender.

	Country Group				Gender	
name	Central and Eastern	Northern	Southern	Western	Male	Female
$\widehat{\partial E(n)/\partial n}$(*years*)	-0.33	-0.55^{**}	-0.62	-0.55^{**}	-0.67^{***}	-0.38^{*}
	(0.38)	(0.25)	(0.65)	(0.27)	(0.22)	(0.24)
Observations	29109	19738	16797	29469	48448	46665

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
All specifications adjust for maternal age at birth and country fixed effects. We include a gender dummy in the first four specifications. The sample is composed of respondents from GGS and SHARE. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four.

Table 5: Effect of an additional sibling on educational attainment.

	Secondary	Post-Secondary	Tertiary
Number of siblings	0.003	-0.004	-0.010
	(0.016)	(0.023)	(0.022)
Baseline mean	0.83	0.40	0.36
Observations	42072	42072	42072

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
The table shows 2SLS estimates of the effect of sibship size (instrumented by twin birth at second parity) on the educational attainment of firstborn children. All outcomes are binary indicators. The sample is composed of respondents from SHARE. Standard errors are robust to heteroskedasticity.

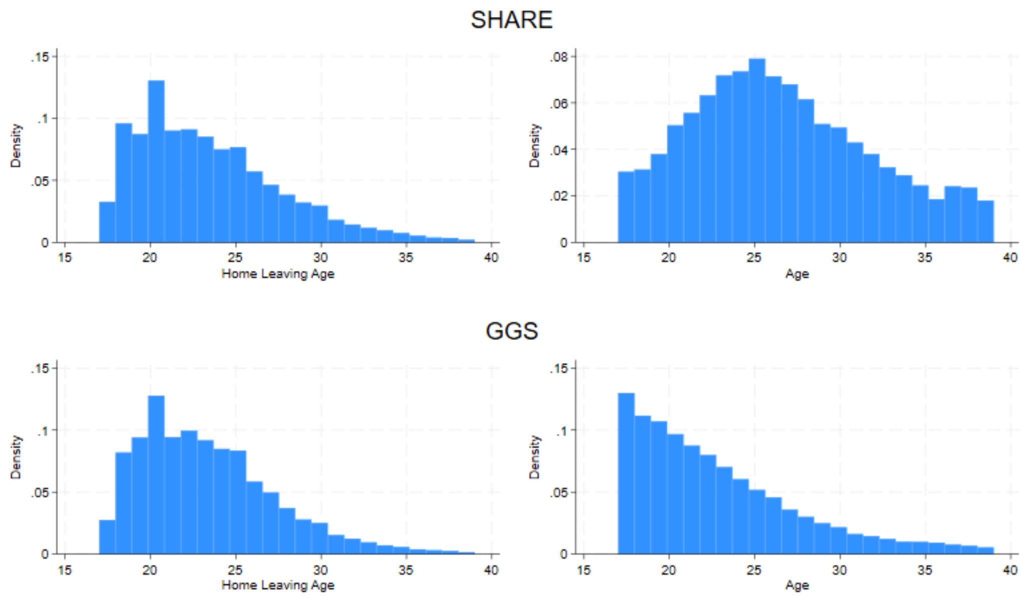
Table 6: Effects on home-leaving ages by age distance between first and second parity.

Age distance	$\widehat{\partial E(n)/\partial n}$						
	Full Sample	Men	Women	Central/Eastern	Northern	Southern	Western
$[1, 2]$	-0.45	-0.48	-0.44	0.45	-0.17	-0.54	-0.87^{**}
	(0.29)	(0.42)	(0.38)	(0.77)	(1.29)	(0.52)	(0.38)
$[3, 4]$	-0.04	-0.25	0.12	-0.35	0.11	-0.3	0.65
	(0.28)	(0.43)	(0.44)	(0.63)	(0.88)	(0.43)	(0.64)
≥ 5	-1.05^{***}	-1.41^{***}	-0.74^{**}	-0.98^{*}	-1.85^{*}	-0.88^{*}	-1.04^{**}
	(0.26)	(0.42)	(0.35)	(0.55)	(0.97)	(0.47)	(0.43)

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
All specifications adjust for maternal age at birth, country fixed effects and fixed effects for the year of birth of the individual. The sample is composed of respondents from GGS and SHARE. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four.

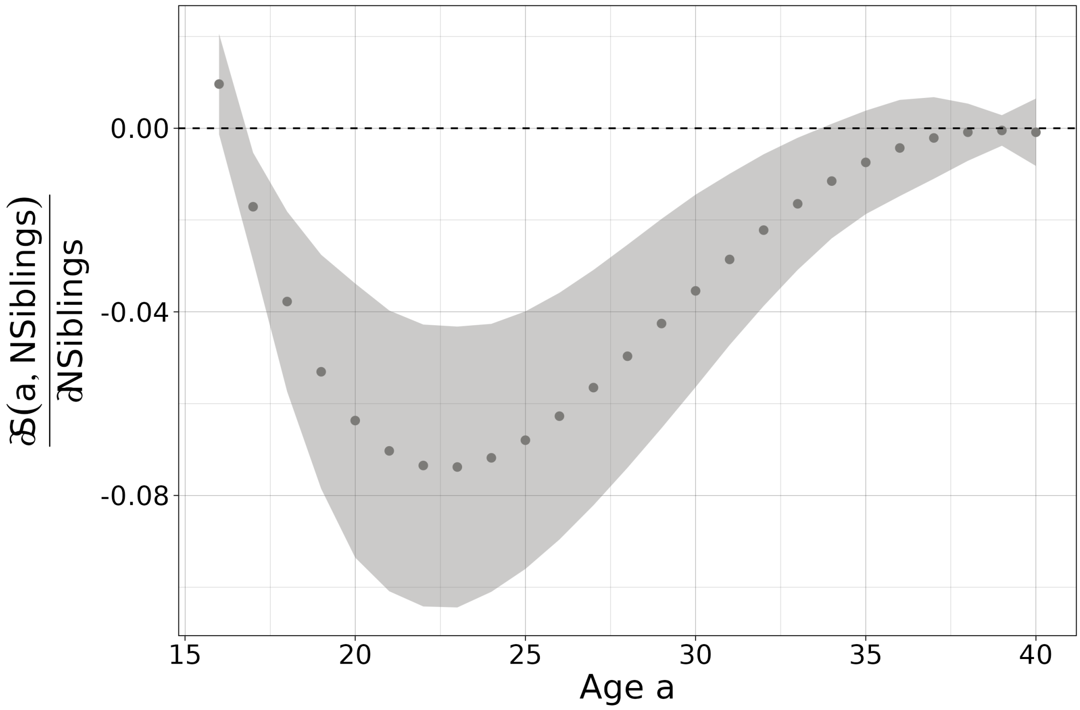
Figures

Figure 1: Home-leaving ages and age distribution of non-leavers.



Note: The two histograms on the left-hand side show the distribution of home-leaving ages for adult children who have already left the parental home. The histograms on the right-hand side display the age distribution for adult children who are still living with their parents when the survey takes place.

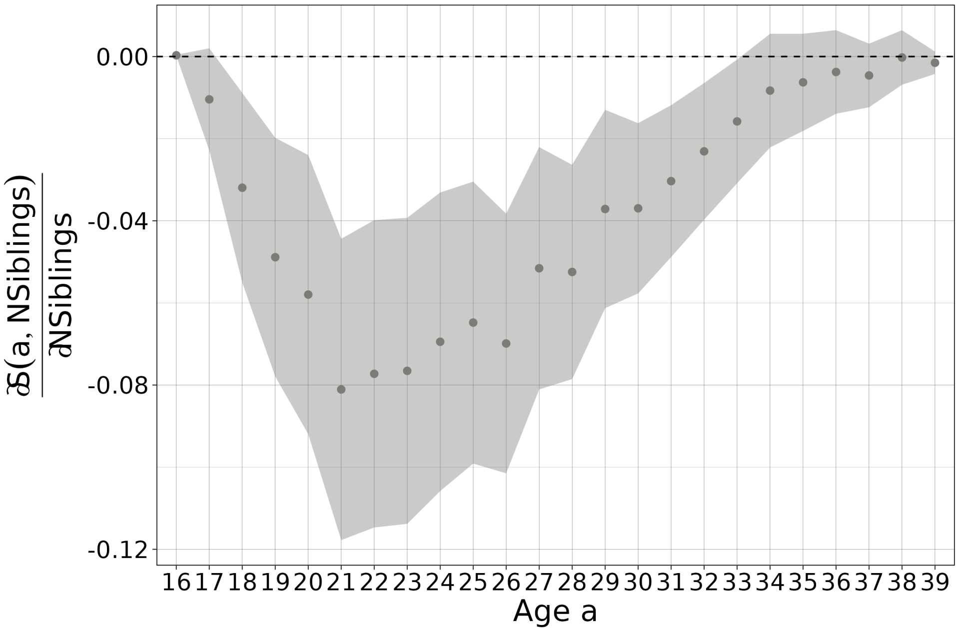
Figure 2: Marginal effects of an additional sibling on survival probabilities $S(a)$.



Note: The figure presents estimates of marginal effects as shown in equation (5), using a polynomial of degree four. The shaded area represents the 95% confidence level. The sample is composed of respondents from GGS and SHARE.

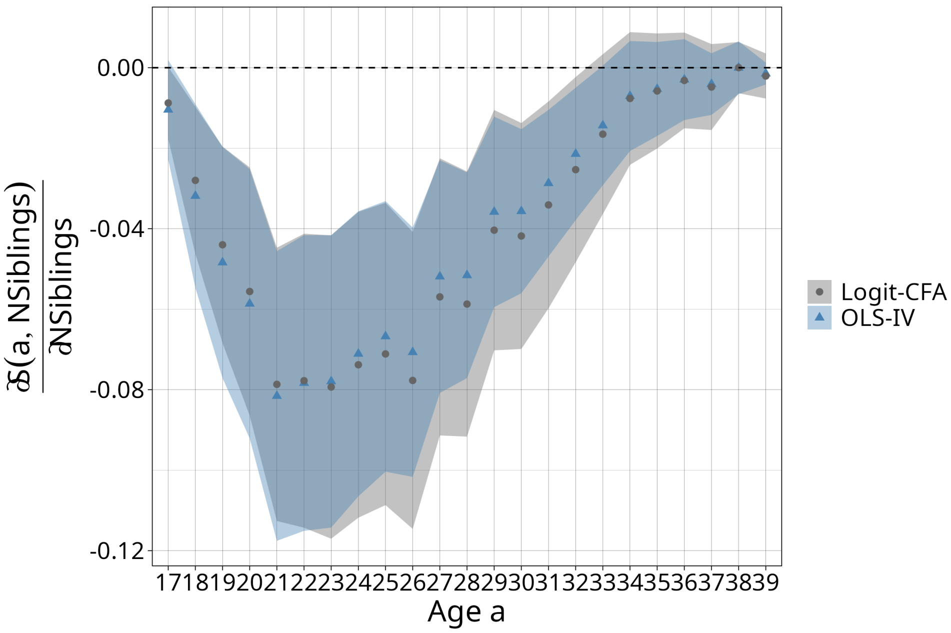
A Appendix - Additional Figures and Tables

Figure A.1: Marginal effect of an extra sibling, non-parametric.



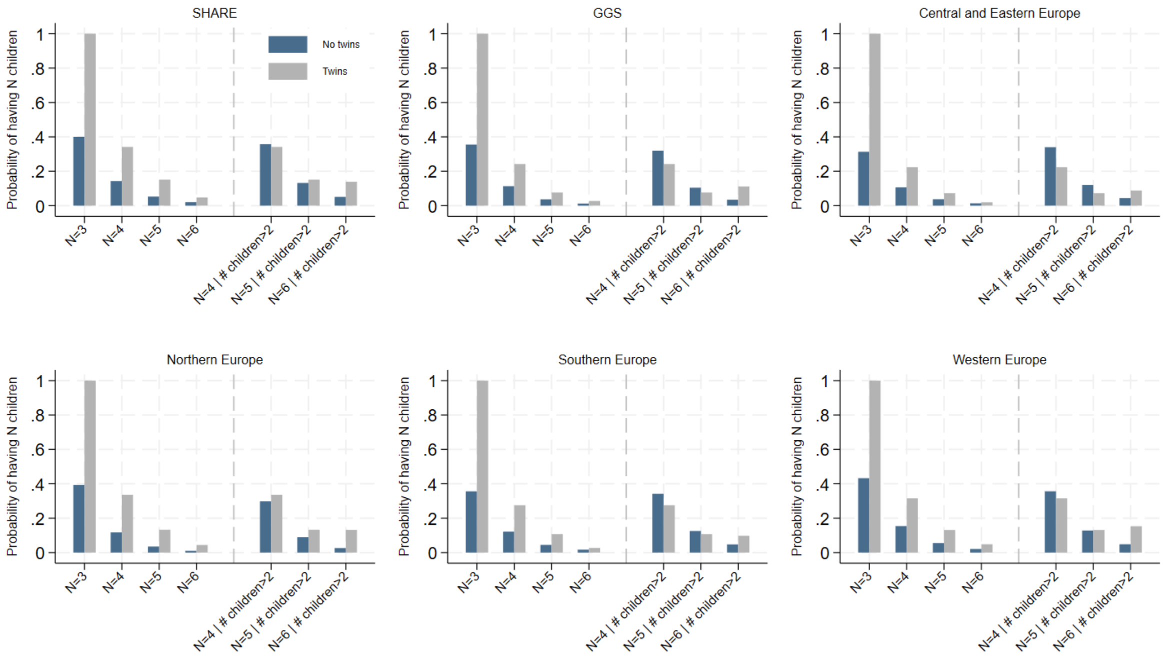
Note: The figure presents estimates of a linear probability model of living with parents by age a using twin birth at second parity as an instrument for the number of siblings. The model is estimated separately for every a. The sample is composed of respondents from GGS and SHARE.

Figure A.2: Marginal effect of an extra sibling, comparing IV and Control Function Approach.



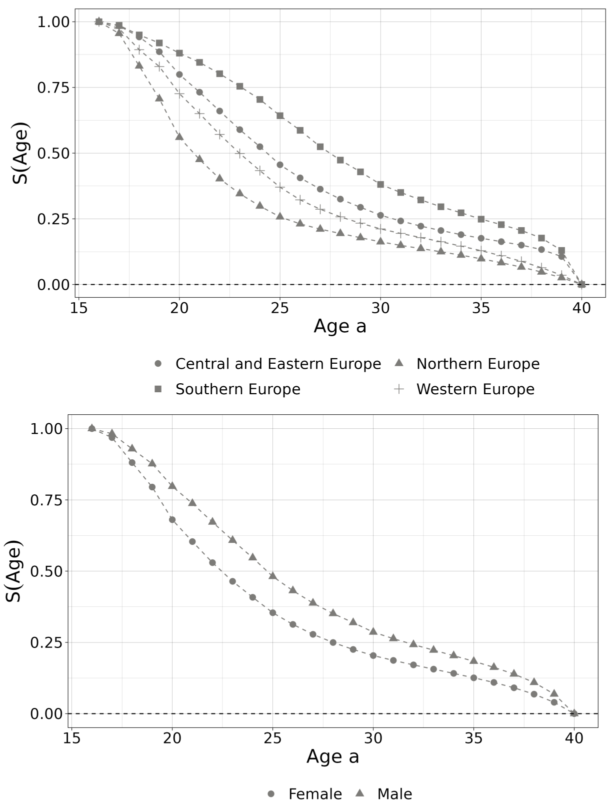
Note: The figure presents age-specific estimates of the effect of an additional sibling on the probability of living with parents. The sample is composed of respondents from GGS and SHARE. The blue curve reports IV estimates from a linear probability model using twin birth at second parity as an instrument; the model is estimated separately for each age a and adjusts for the mother's age at birth. The grey curve reports the corresponding control–function estimates obtained from a logit specification, also estimated separately by age.

Figure A.3: Change in the probability of having an n^{th} child in families with twins.



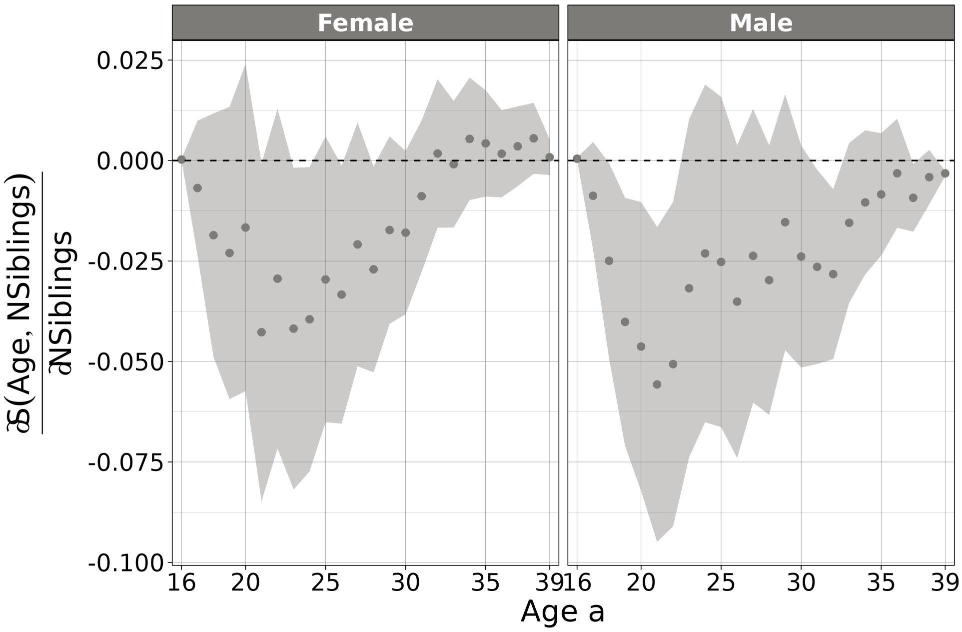
Note: The figure compares the probability of having a third, fourth, fifth, and sixth child for families with and without a twin birth at second parity. The first four set of bar reports unconditional probabilities measured over the entire sample. The second set restricts the sample to families who have already reached three children and shows, for this subgroup, the probability of having a fourth, fifth, or sixth child.

Figure A.4: Kaplan-Meier survival functions by country group (left) and gender (right).



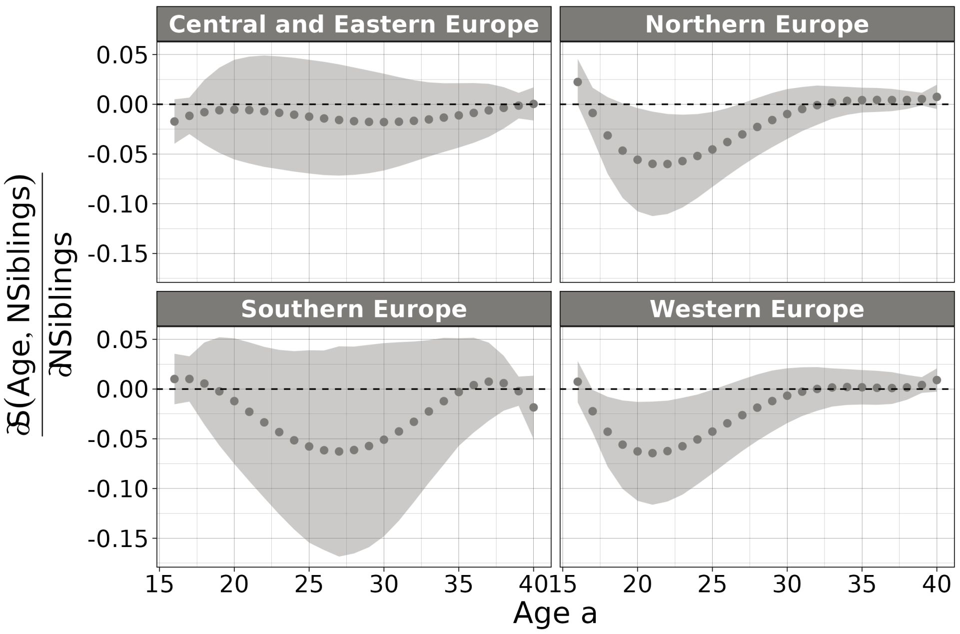
Note: The figure presents estimates of the proportion of individuals still living with parents by age a, by country group (left) and by gender (right). The sample is composed of respondents from GGS and SHARE.

Figure A.5: Marginal effect of an extra sibling by gender, non-parametric.



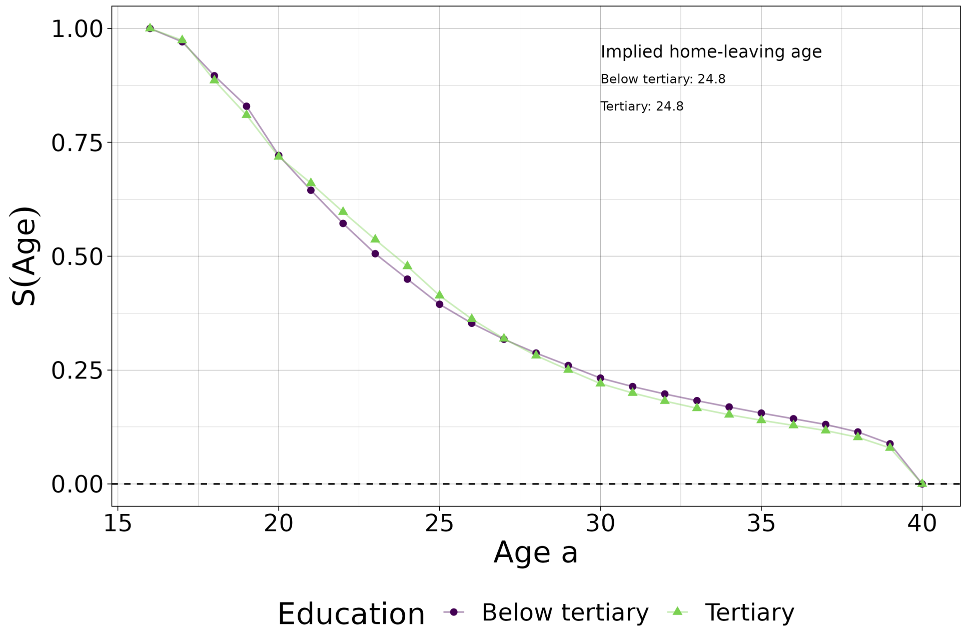
Note: The figure presents estimates of marginal effects as shown in equation (5) for men (right) and women (left). The sample is composed of respondents from GGS and SHARE.

Figure A.6: Marginal effect of an extra sibling by country group.



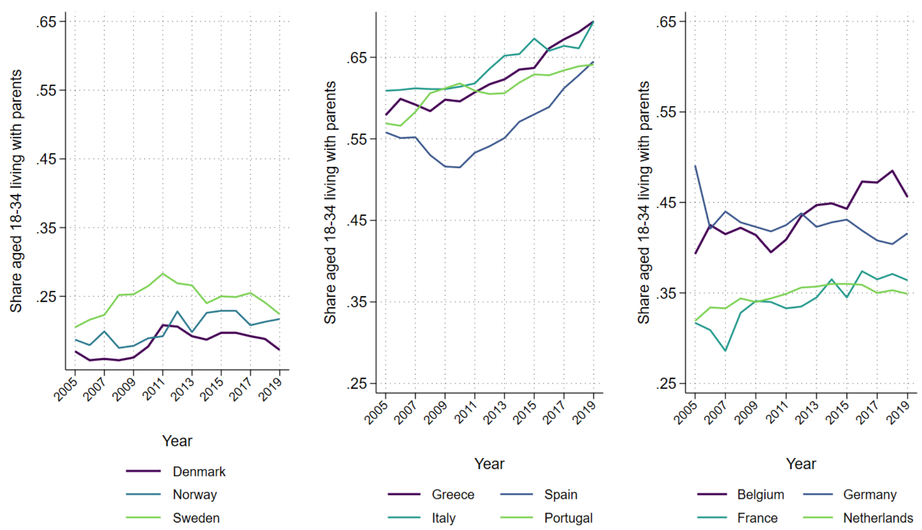
Note: The figure presents estimates of marginal effects as shown in equation (5) by country groups. The sample is composed of respondents from GGS and SHARE.

Figure A.7: Kaplan-Meier survival function by educational level.



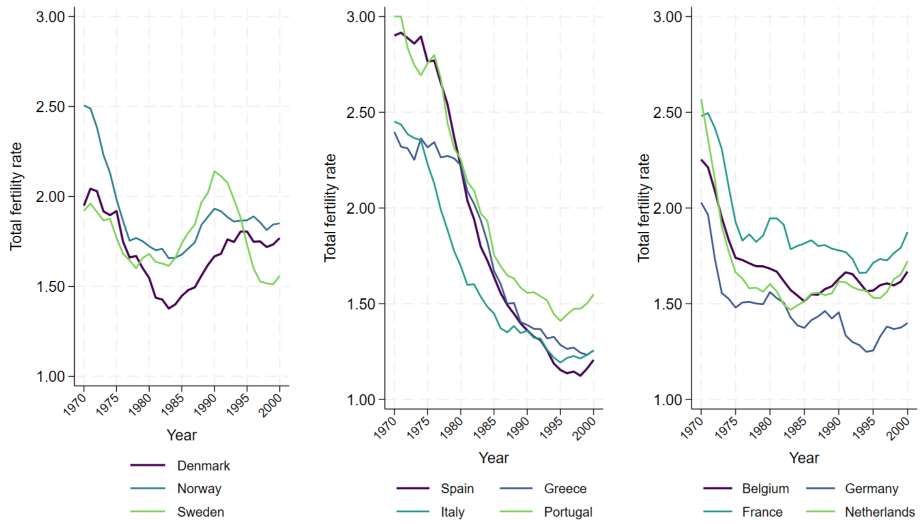
Note: The figure presents estimates of the proportion of individuals still living with parents by age a, by education level. The sample is composed of SHARE respondents.

Figure A.8: Rates of inter-generational co-residence by country.



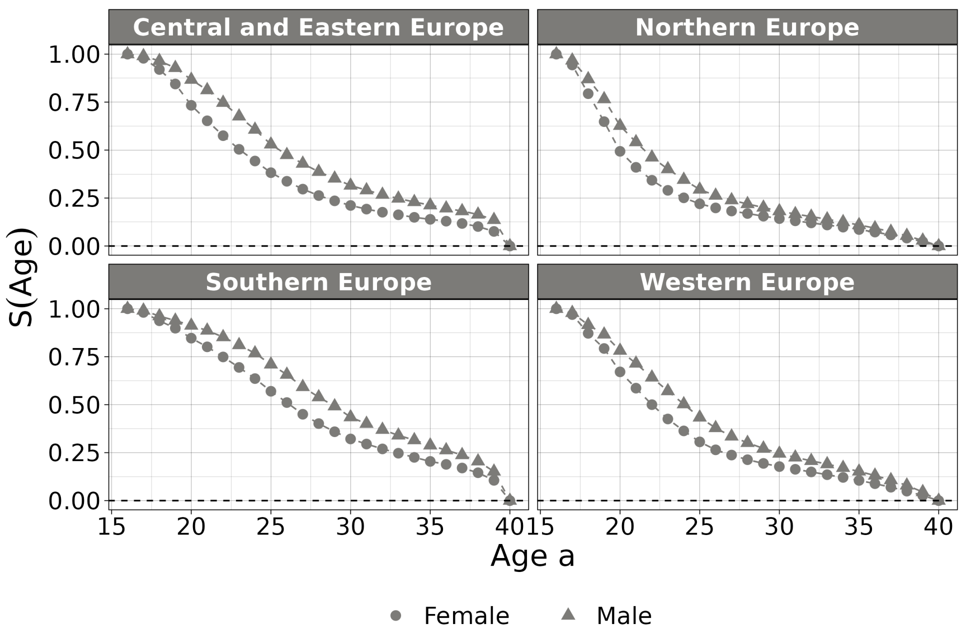
Note: The figure shows trends in the proportion of individuals aged between 18 and 34 still living with parents by country.
Source: European Statistics on Income and Living Conditions (EU-SILC).

Figure A.9: Fertility rates by country.



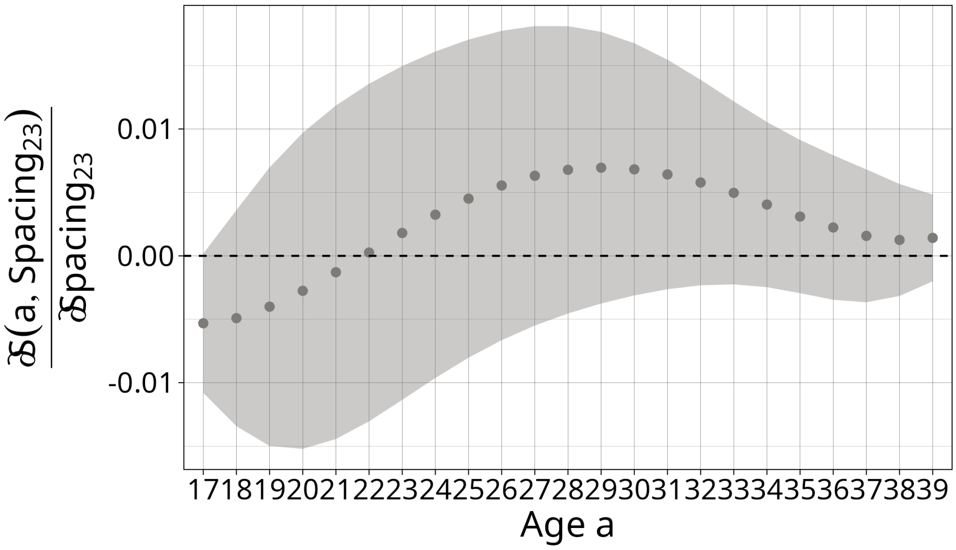
Note: The figure shows trends in the number of children per woman by country.
Source: Human Fertility Database.

Figure A.10: Kaplan-Meier survival functions by country group and gender.



Note: The figure presents estimates of the proportion of individuals still living with parents by age a, by country group, and gender. The sample is composed of respondents from GGS and SHARE.

Figure A.11: Difference in the probability of remaining in the parental home for firstborns by spacing between younger siblings.



Note: The figure plots the estimated difference in the probability that the firstborn still lives in the parental home between families where the second and third child are born one year apart and those where they are spaced further apart. Estimates are obtained from the following specification:

$$S_{i,t} = \sum_{j=0}^{4} \beta_j \left(dist_{23,i} \cdot Age_{i,t}^{j} \right) + \delta' X_i + \lambda_{dist12(i)} + \varepsilon_{i,t},$$

where $dist_{23,i}$ is an indicator equal to one if the age difference between the second and the third child is exactly one year, X_i denotes the set of control variables, and $\lambda_{dist12(i)}$ are fixed effects for the spacing between the first and second child. The sample, composed by respondents from SHARE and GGS, consists of firstborns with at least two younger non-twin siblings, $N = 34698$.

Table A.1: Balance table.

	$Twins = 0$	$Twins = 1$	Difference
		SHARE + GGS	
Age Mother at Birth	24.025	24.397	-0.372**
	(4.392)	(4.449)	(0.164)
Central and Eastern Europe	0.307	0.260	0.047***
	(0.461)	(0.439)	(0.014)
Northern Europe	0.207	0.241	-0.034***
	(0.405)	(0.428)	(0.013)
Southern Europe	0.177	0.129	0.048***
	(0.382)	(0.335)	(0.012)
Western Europe	0.309	0.371	-0.062***
	(0.462)	(0.483)	(0.014)
Observations	94050	1063	95113
		GGS	
Grandfather Education - High	0.065	0.064	0.001
	(0.246)	(0.245)	(0.013)
Grandfather Education - Medium	0.179	0.148	0.031
	(0.383)	(0.355)	(0.020)
Grandfather Education - Low	0.756	0.788	-0.032
	(0.429)	(0.409)	(0.023)
Grandmother Education - High	0.045	0.045	0.001
	(0.208)	(0.207)	(0.011)
Grandmother Education - Medium	0.246	0.217	0.029
	(0.431)	(0.413)	(0.023)
Grandmother Education - Low	0.693	0.713	-0.020
	(0.461)	(0.453)	(0.024)
Observations	32929	359	33288
		SHARE	
Parental Home Leaving Age	23.057	23.388	-0.331
	(4.871)	(5.604)	(0.311)
Years of Employment	7.387	6.996	0.391
	(4.560)	(4.304)	(0.290)
Years Unemployment	0.100	0.128	-0.028
	(0.779)	(0.836)	(0.050)
Owner House at Birth	0.450	0.468	-0.018
	(0.498)	(0.500)	(0.032)
Rural Area at Birth	0.560	0.540	0.020
	(0.496)	(0.499)	(0.032)
Employee at Birth	0.634	0.608	0.026
	(0.482)	(0.489)	(0.031)
Civil Servant at Birth	0.296	0.328	-0.032
	(0.457)	(0.470)	(0.029)
Years of Education	13.620	14.156	-0.536**
	4.089	4.468	(0.261)
In Education at Birth	0.055	0.068	-0.013
	(0.229)	(0.252)	(0.015)
Observations	19696	250	19946

Note: In the third column, the Table shows OLS coefficient from a model with $Twins$ as an explanatory outcome and predetermined characteristics as the outcome. Outcomes for the two groups are reported in the first and second columns, with standard deviations in parentheses.

Table A.2: Share of births and twins over time.

Birth decade	Share of births	Share of twins	Avg. maternal age at birth
Before 1940	0.002	0.007	22.3
[1940,1950)	0.019	0.010	22.5
[1950,1960)	0.107	0.010	23.1
[1960,1970)	0.228	0.010	23.4
[1970,1980)	0.310	0.012	23.5
[1980,1990)	0.256	0.011	24.6
[1990,2000)	0.070	0.014	27.1
2000 and after	0.007	0.006	29.3

Note: The table shows the share of twin births at second parity across birth decades and the corresponding distribution of these second births in our sample ("Share of births").

Table A.3: Effect of an additional sibling on the expected home-leaving age, non-parametric estimates.

	Full data		GGS			SHARE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{\partial E(n)/\partial n}$ (*years*)	-0.87^{***}	-0.46^{***}	-0.64^{**}	-0.66^{**}	-0.66^{**}	-0.33^{*}	-0.61^{**}	-0.64^{**}
	(0.16)	(0.16)	(0.25)	(0.27)	(0.28)	(0.2)	(0.29)	(0.3)
Country FE		✓	✓	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓	✓	✓
Birth year	✓	✓	✓	✓	✓	✓	✓	✓
Mother age at birth	✓	✓	✓	✓	✓	✓	✓	✓
Grandparents educ.					✓			
Parental covariates								✓
Observations	95113	95113	52441	33288	33288	42672	19946	19946

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Parental covariates include employment, educational attainment, homeownership status, an urban/rural dummy one year before having the first child, and home-leaving age. Standard errors are bootstrapped using 500 simulations. Columns (1), (2), (3) and (6) show estimates for the full dataset, while the other columns show estimates for the subset of data for which we have full information for all children (i.e. no missing covariates)

Table A.4: Effect of an additional sibling on the expected home-leaving age, homogeneous twins instrument definition.

	Full data		GGS			SHARE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{\partial E(n)}/\partial n$ (*years*)	-0.86^{***}	-0.56^{***}	-0.76^{***}	-0.82^{***}	-0.82^{***}	-0.37^{*}	-0.7^{**}	-0.72^{**}
	(0.16)	(0.16)	(0.23)	(0.25)	(0.26)	(0.23)	(0.32)	(0.31)
Country FE		✓	✓	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓	✓	✓
Birth year	✓	✓	✓	✓	✓	✓	✓	✓
Mother age at birth	✓	✓	✓	✓	✓	✓	✓	✓
Grandparents educ.					✓			
Parental covariates								✓
Observations	95113	95113	52441	33288	33288	42672	19946	19946

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Parental covariates include employment, educational attainment one year before having the first child, homeownership status, an urban/rural dummy, and home-leaving age. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four. Columns (1), (2),(3) and (6) show estimates for the full dataset, while the other columns show estimates for the subset of data for which we have full information for all children (i.e. no missing covariates).

Table A.5: Effect of an additional sibling on the expected home-leaving age - OLS

	Full data		GGS			SHARE		
	(1)	(2)	(1)	(2)	(3)	(1)	(2)	(3)
$\widehat{\partial E(n)}/\partial n$ (*years*)	-0.36^{***}	-0.29^{***}	-0.38^{***}	-0.33^{***}	-0.33^{***}	-0.2^{***}	-0.25^{***}	-0.25^{***}
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Country FE		✓	✓	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓	✓	✓
Birth year	✓	✓	✓	✓	✓	✓	✓	✓
Mother age at birth	✓	✓	✓	✓	✓	✓	✓	✓
Grandparents educ.					✓			
Parental covariates								✓
Observations	95113	95113	52441	33288	33288	42672	19946	19946

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Parental covariates include employment, educational attainment one year before having the first child, homeownership status, an urban/rural dummy, and home-leaving age. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four. Columns (1), (2),(3) and (6) show estimates for the full dataset, while the other columns show estimates for the subset of data for which we have full information for all children (i.e. no missing covariates).

Table A.6: Effect of an additional sibling on the expected home-leaving age - OLS

	Full data		GGS			SHARE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Only child vs first-born with one sibling								
$\widehat{\partial E(n)}/\partial n$ (*years*)	-0.98^{***}	-0.87^{***}	-1.17^{***}	-1.15^{***}	-1.16^{***}	-0.59^{***}	-0.59^{***}	-0.5^{***}
	(0.05)	(0.05)	(0.07)	(0.09)	(0.08)	(0.07)	(0.09)	(0.09)
Observations	88911	88911	50101	31583	31583	38810	18197	18197
Panel B: Only child vs first-born with two siblings								
$\widehat{\partial E(n)}/\partial n$ (*years*)	-0.87^{***}	-0.68^{***}	-0.88^{***}	-0.84^{***}	-0.84^{***}	-0.51^{***}	-0.48^{***}	-0.49^{***}
	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)
Country FE		✓	✓	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓	✓	✓
Birth year	✓	✓	✓	✓	✓	✓	✓	✓
Mother age at birth	✓	✓	✓	✓	✓	✓	✓	✓
Grandparents educ.					✓			
Parental covariates								✓
Observations	52696	52696	28783	17849	17849	23913	10826	10826

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Parental covariates include employment, educational attainment one year before having the first child, homeownership status, an urban/rural dummy, and home-leaving age. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four. Columns (1), (2),(3) and (6) show estimates for the full dataset, while the other columns show estimates for the subset of data for which we have full information for all children (i.e. no missing covariates). In Panel A, the population is restricted to only children and firstborns with only one sibling. In Panel B, the population is restricted to only children and firstborns with two siblings.

Table A.8: Twins and contact frequency with parents.

	Dependent variable: *High contact frequency*
Panel A: Twins and contact frequency with parents (only firstborn)	
Twins	-0.018
	(0.021)
Central and Eastern Europe	-0.162***
	(0.007)
Northern Europe	-0.259***
	(0.007)
Western Europe	-0.238***
	(0.006)
Year of Birth	0.009***
	(0.001)
Year of birth Parent	-0.003***
	(0.001)
# of Siblings	-0.053***
	(0.003)
Constant	-10.645***
	(0.425)
Observations	38453
Panel B: Twin vs non-twin differences in contact frequency with parents (only families with twins)	
Child is a Twin	-0.016
	(0.016)
Year of Birth	0.014***
	(0.002)
Observations	3166

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Panel A shows OLS estimates using a dummy variable defining *High contact frequency* with parents. The dummy takes value 1 if the parent responds either "Daily" or "Several times a week" when asked about the contact frequency with the child (roughly 60% of the sample). The sample is based on firstborns from SHARE with at least two siblings, and the variable $Twins$ takes value 1 if in the family there has been a twin birth. In Panel B we show *within familiy* differences in *High contact frequency* frequency between twin and non-twin children. The specification controls for family fixed effects.

Table A.9: Effects on home-leaving by the sex composition of twins.

	Opposite sex (1)	Same sex (2)
$\widehat{\partial E(n)/\partial n}$ (*years*)	-0.69^{**}	-0.41^{*}
	(0.27)	(0.21)
Observations	94338	94739

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
All specifications adjust for maternal age at birth, gender, and country fixed effects. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four. The sample is composed of respondents from GGS and SHARE. Column (1) restricts the sample to families with opposite-sex twins (male–female), while column (2) restricts the sample to families with same-sex twins (female–female or male–male).

Table A.10: Effects on home-leaving ages by spacing - adjusting for parental characteristics.

Age distance	$[1, 2]$		$[3, 4]$		≥ 5	
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{\partial E(n)/\partial n}$ (*years*)	-0.62	-0.62	-0.41	-0.39	-1.26^{***}	-1.24^{***}
	(0.41)	(0.41)	(0.34)	(0.34)	(0.32)	(0.32)
Country FE	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Birth year	✓	✓	✓	✓	✓	✓
Mother age at birth	✓	✓	✓	✓	✓	✓
Grandparents educ.		✓		✓		✓
Parental covariates		✓		✓		✓
Observations	20068	20068	18130	18130	14753	14753

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
All specifications adjust for maternal age at birth, gender, and country fixed effects. Standard errors are bootstrapped using 500 simulations. The polynomial function of age is of order four. The sample consists of observations for which parental characteristics are observed in either SHARE or GGS. When including parental and family covariates that are not jointly observed in SHARE and GGS, we follow a unified specification in which covariates are set to zero when not observed. Specifically, we include an indicator equal to one for observations from SHARE and assign zero to SHARE-specific covariates in GGS (and analogously for GGS-specific covariates in SHARE).

Table A.11: Association between age distance and education/-contact frequency with parents.

	High contact frequency	*Post-Secondary*	*Tertiary*
	(1)	(2)	(3)
Age distance	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)
Observations	38439	42056	42056

Note: $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$
The coefficients represent OLS estimates from three different models using *Age distance* as outcome, and controlling for maternal age at birth, gender, year-of-birth fixed effects, and country fixed effects. The sample is composed of respondents from SHARE. Robust standard errors in parentheses.