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Later-borns Don't Give Up: The Temporary Effects of Birth Order on European Earnings

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Abstract The existing empirical evidence on the effects of birth order on wages does not distinguish between temporary and permanent effects. Using data from 11 European countries for males born between 1935 and 1956, we show that firstborns enjoy on average a 13.7 % premium in their entry wage compared with later-borns. This advantage, however, is short-lived and disappears 10 years after labor market entry. Although firstborns start with a better job, partially because of their higher education, later-borns quickly catch up by switching earlier and more frequently to better-paying jobs. We argue that a key factor driving our findings is that later-borns have lower risk aversion than firstborns.

Keywords Birth order · Earnings · Risk aversion · Europe

Introduction

Firstborns typically earn higher wages. According to Ruth Mantell of *The Wall Street Journal* (2011), firstborns are ". . . the most likely to earn six figures and hold up a top executive position among workers with siblings. . . ." Firstborns also have lower mortality risk, as documented by Barclay and Kolk (2015). In their influential work in this area, Black et al. (2005; BDS, hereafter), used Norwegian registry data and showed that being firstborn increases education by around 0.7 years of schooling.

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Depending on the order of birth, BDS also estimated that later-born males (females) earn between 1.2 % (4.2 %) and 3.3 % (21.1 %) less than male (female) firstborns.

Is the earnings premium enjoyed by firstborns temporary or permanent? Do later-borns catch up with their older siblings, or does the premium persist or even widen over time? In most cases, the existing empirical literature cannot answer these questions because the estimated premium is based on current rather than lifetime wages. However, this premium is permanent only in the special case when earnings profiles are parallel with respect to birth order. In general, disentangling temporary from permanent wage effects requires information on earnings at different points of the life cycle as well as on lifetime earnings (see, e.g., Tamborini et al. 2015).

In this article, we address these questions by studying the effects of birth order on life cycle earnings in a sample of 4,270 males born between 1935 and 1956, and residing in one of the following European countries: Austria, Belgium, the Czech Republic, Denmark, France, Germany, Italy, the Netherlands, Spain, Sweden, and Switzerland.

We consider several measures of real annual earnings: the entry wage, or the initial wage in the first job; the wages 5, 10, 15, and 25 years after labor market entry, defined as the time since the first job was started; and the current or last wage, defined either as the wage in the job currently held if still active at age 50 or older, or as the wage in the last job before retiring. We also add a measure of lifetime earnings, or the discounted value of the stream of earnings from age 10 to retirement. We show that firstborns enjoy in their first job a 13.7 % wage premium over later-borns. This advantage, however, declines sharply after five years and is completely gone 10 years after labor market entry. Because the initial wage gains are quickly lost and later-borns start working earlier than firstborns, it is not surprising that being a firstborn has no statistically significant effect on lifetime earnings.

The temporary advantage enjoyed by firstborns implies that birth order has a positive effect on earnings growth, measured as wages t years after labor market entry minus the entry wage. Importantly, we find that this effect remains even after educational attainment is controlled for, suggesting that differences in education between firstborns and later-borns are not sufficient to explain the observed differences in wages over the life cycle. We also find that education negatively affects earnings growth—a result consistent both with the learning model by Altonji and Pierret (2001) and with the human capital model, provided that education and experience are substitutes in the production of skills.

We document that temporary birth order effects are closely associated with differences in job-to-job mobility after labor market entry. On the one hand, firstborns find better initial jobs: not only do they earn more, but they are also more likely than later-borns to be employed in white-collar and in public-sector jobs as well as stay on their initial jobs longer. On the other hand, later-borns start with poorer matches but change jobs swiftly, quickly catching up with firstborns by virtue of job mobility.

To illustrate the effects of mobility, we compare the expected (log) wages of firstborns and later-borns 10 years after labor market entry and find that they are quite similar. These wages can be expressed as the weighted average of (log) wages for those still in the first job and (log) wages for those in other jobs, using as weights the probability of being in the first job. Firstborns who are still in their first job 10 years after labor market entry retain a 10.1 % advantage over the earnings of later-borns in their first job. This advantage, however, is compensated by the fact that, 10 years after entry, later-borns have an 11.7 % (or 7.1 percentage points) higher probability of being already in their second or third job, which pays higher earnings than the first job.

Drawing on a vast literature in psychology (see, e.g., Sulloway 2007) and using our own evidence in support, we argue that firstborns differ from later-borns both because they have higher education and because they are less likely to engage in risky behaviors (Wang et al. 2009). Better education may partly explain why firstborns start with a better job, and the higher propensity to take risks helps explain why later-borns incur higher turnover (Allen et al. 2005) and enjoy higher wage growth than firstborns (Shaw 1996).

By focusing on outcomes that relate birth order to population quality (education) and by taking a life cycle approach that highlights the importance of the age structure, our analysis has implications both for demography and for economics.

Review of the Literature

The relationship between birth order and individual outcomes has been investigated in demography, sociology, psychology, and economics for several years. The effects of birth order on educational attainment have been widely studied. In addition to the contribution by BDS, negative effects of birth order have also been found in recent research by Bagger et al. (2013) for Denmark, by Björklund and Jäntti (2012) for Sweden, and by Kantarevic and Mechoulan (2006) and De Haan (2010) for the United States.

Several studies examining the effects of birth order on earnings are based on U.S. data. The earlier evidence suggests that the estimated effects tend to be small or negligible. Behrman and Taubman (1986), for instance, using U.S. data for young adults, found differences by birth order in both schooling and log earnings after adjusting for age or work experience. The effects on earnings, however, became statistically insignificant when they included controls for observed childhood family background characteristics.

Olneck and Bills (1979) examined the effect of birth order and family size on childhood test scores and adult levels of education, occupation, and wages, finding a negligible influence of birth order on all measures of achievement. Kessler (1991) used data from the U.S. National Longitudinal Survey of Youth to examine the effect of birth order and family size on individual behavior until early adulthood. He found that neither birth order nor childhood family size significantly influences the level or growth rate of wages for individuals aged 14–22, 18–26, and 22–30.

More sizable effects are found in the recent literature. Kantarevic and Mechoulan (2006) used a sample aged 25–89 from the Childbirth and Adoption History File (CAHF), which is a special supplemental file of the U.S. Panel Study on Income Dynamics (PSID); they found that when they omitted age of the mother at birth from the vector of covariates, birth order had no statistically significant effect on earnings. When they included age, however, they reported that firstborns garner 6.3 % higher earnings than later-borns. The statistical significance of the estimated effect falls from 5 % to 10 % when the father's education and the age of the father at childbirth are added to the covariates.

Björklund and Jäntti (2012) used Swedish registry data to study the impact of birth order on education and long-term earnings. Using the well-known result that long-term earnings are approximated by earnings between ages 31 and 40 (Haider and Solon 2006), they found that the firstborn child attains 0.2 years of additional education and has approximately 0.25 % higher long-run earnings than other siblings. These are small effects, at least compared with those obtained by BDS and reported in the Introduction.

After examining other outcomes, Björklund and Jäntti concluded that birth order is not a major source of the family impact on economic outcomes and thus not a major source of inequality of opportunity. In contrast, De Haan et al. (2014) found that birth order affects early outcomes in Ecuador.

With the exception of the study by Björklund and Jäntti (2012), the studies reviewed in this section examined current rather than lifetime earnings and focused on how birth order affects individual wages, thereby ignoring its effects on experience (or age) wage *profiles*. Therefore, they cannot inform whether later-borns catch up, totally or partially, or even overtake their better-educated firstborns who may start with a higher wage; neither can they inform whether the initial gap widens with labor market experience.

Data

Our research requires data on individual earnings at different points in the life cycle. In this article, we use the Survey of Health, Ageing and Retirement in Europe (SHARE), which is a multidisciplinary and cross-national European data set containing current and retrospective information on labor market activity, retirement, health, and socioeconomic status (SES) for more than 25,000 individuals aged 50 or older. We draw our data from the first three waves of the survey, and in particular the third wave, SHARELIFE, which contains detailed retrospective data on life histories and labor market histories. We exclude females because their labor force participation is often lower and less continuous than that of males. We also exclude the self-employed (as did Murphy and Welch (1990)), as well as people aged 50 and older who either worked fewer than 5 years or started working before age 10 or after age 35 (10 individuals). In SHARELIFE, survey participants are asked to report the amount they were paid monthly after taxes each time they started an employment spell; the survey also includes information on the start and end dates of each employment spell. They are also asked the monthly net wage in their current job (if still working) and the monthly net wage at the end of the main job in their career (if already retired). For wages and other benefits to be comparable across time and country, we transform them into 2006 Euros using purchasing power parity (PPP) exchange rates and consumer price index (CPI) indices.

We use these rich data, which include the entry wage (the initial wage in the first job) and the current or last wage,[1] to construct estimates of individuals' lifetime earnings and earnings after 5, 10, 15, and 25 years in the labor market entry. We define lifetime earnings (or permanent income) as the income flowing from the asset value of working at age 10. The construction of this variable and of wages at different ages is described in detail both in Brunello et al. (2015: appendix A) and in Weiss (2012).

In short, for those who have had only one job in their working life (more than 20 % of the sample), we interpolate between the entry wage and the last (or current) wage.

[1] Because information on the first wage is missing for about 25 % of the individuals in our final sample, we use predictive mean matching to impute missing data. Single-imputation predictive mean matching replaces a missing value with the observed value for which the predicted value is the closest to that of the missing value. When we use multiple imputations, we randomly draw five plausible values from the pool of the 10 closest neighbors in terms of the predicted wage. See Weiss (2012) for details. The percentage of missing values is very similar among firstborns (23.2 %) and later-borns (24.3 %). As shown in the Results section, our results do not depend on imputations or on the use of single versus multiple imputations.

For those who have had more than one job, we observe the entry wage in each job as well as the current or last wage. For this second group, we regress current wages on labor market experience; a rich set of controls, which includes education, occupation, sector of activity, cohort and country effects, and economic conditions at age 10; and the interactions of these controls with experience. We then use the estimated coefficients and the entry wage in each job to generate both the final wage in the job and within-job earnings growth.[2] With this information in hand, we compute annual wages after 5, 10, 15, and 25 years in the labor market and the discounted value of earnings at age 10, using a 2 % real interest rate for the discount factor.[3]

Our data set has the advantage that it covers 11 European countries, and the potential drawback that it uses long recall data. These data are subject to measurement error, possibly not of the classical type. Importantly, Bingley and Martinello (2014), using Danish administrative register information drawn from tax reports and civil registries to validate SHARE data on earnings, showed that measurement error for annual income in these data is classical. This error is partly averaged out when we use lifetime earnings.

Validation studies have also found that recall bias is not severe in SHARELIFE data, arguably because of the state-of-the-art elicitation methods used: interviewers help respondents to locate events along the time line, starting from domains that are more easily remembered, and then ask respondents progressively more details about those events.[4] Reassuringly, Brunello et al. (2015) used these data to evaluate the long-run returns to education and found that their estimates are quite similar to those obtained by Bhuller et al. (2014) using Norwegian administrative data.

Our final sample consists of 4,270 males born in the period 1935–1956 and residing in Austria, Belgium, the Czech Republic, Denmark, France, Germany, Italy, the Netherlands, Spain, Sweden, and Switzerland.[5] Although the first two waves of the survey have information on order of birth ("Were you the oldest child, the youngest child, or somewhere in-between?"), Wave 3 has data on individual and household conditions at age 10. We measure gross family size, which includes siblings and other members, with the question, "Including yourself, how many people lived in your household at this accommodation when you were ten?"[6] To estimate net family size, or the number of siblings, we use the answers to the question, "Who lived in the

[2] Brunello et al. (2015) showed that estimates are broadly unaffected when they replace labor market experience with age and exclude education in the wage regressions used to generate both the end wage in each job and within-job earnings growth for individuals who have had more than one job.

[3] Haider and Solon (2006), Böhlmark and Lindquist (2006), Brenner (2010), and Brunello et al. (2015) also assumed a constant real interest rate of 2 % to construct a measure of lifetime income. Bhuller et al. (2014) instead used an interest rate of 2.3 %. We also experiment with a 3 % discount rate with no qualitative change of results. We are grateful to Christoph Weiss for providing the codes required to compute earnings profiles and lifetime earnings from the third wave of the survey SHARE.

[4] Brunello et al. (2015) validated this procedure by comparing predicted and actual wages in the German Socio-Economic Panel (SOEP) and found that predictions based on the methodology suggested in the text are quite accurate.

[5] By selecting only individuals born from 1935 onward, we reduce the role of survivorship bias (see Modin 2002) and recall bias for older workers, reduce the weight of imputation, and also ensure that no individual in our sample entered the labor market before World War II.

[6] Of course, household size at age 10 is less correlated with order of birth than is household size at birth. For the small minority of individuals for which this information was not available—around 2 % of our sample—we reconstruct sibship size using information on the number of siblings alive at the time of the first SHARE interview.

household when ten?"; we subtract other members (parents, grandparents, and other relatives) from gross family size. In our data, the average household size at age 10 is 5.44 members, and the average number of siblings is 3.34.[7] Compared with the distribution of siblings in the Norwegian sample used by BDS, our sample comprises households with a higher number of siblings, which may partly reflect both the different sample period—with individuals born in 1935–1956 in our sample and in 1912–1984 in the BDS sample—and the fact that our sample includes also Southern European countries, where the number of siblings is typically higher (2.90 in Sweden and 3.88 in Spain).

The third wave of SHARE also contains a wealth of data on household and individual conditions at age 10. We define the vector **X** as comprising the following covariates: whether the household was located in a rural area or a village; dummy variables for the profession of the main breadwinner; a dummy variable for the presence of hunger episodes before age 15; a dummy variable indicating whether parents smoked, drank heavily, or had mental health problems during childhood; a dummy variable equal to 1 if one parent died before age 35; and dummy variables for the presence of parents, grandparents, or foster parents in the household.[8]

Unfortunately, our information on the age of the parent at birth is available only for those parents who were still alive at the time of the interview. We check whether omitting this critical piece of information significantly affects our estimates by running our regressions with and without the age of the mother at birth in the subsample where this measure is available. As reported later, our evidence suggests that omitting maternal age at birth does not qualitatively affect our estimates.

Table 1 shows the summary statistics of the main variables used in this study, separately by order of birth (firstborns and later-borns). We report that firstborns are, on average, better educated than later-borns (respectively, 12.59 vs. 11.49 years of schooling, and 35.8 % vs. 28.2 % with at least postsecondary education), start working later (at age 19.6 versus 18.6), and have a substantially higher entry wage (11,765 real Euros versus 10,576, a 11.2 % premium). This premium declines with the second and third job and with experience in the labor market, settling at close to 3.3 % in the current or last wage (23,555 vs. 22,787). Firstborns have fewer siblings (1.51 vs. 2.91) than later-borns. Furthermore, the households where firstborns lived at age 10 were more likely to be located in urban areas and to have a white-collar breadwinner, indicating that household wealth was also higher.

Empirical Methodology

Our main interest in the empirical analysis is to understand how the effect of birth order on wages varies over the life cycle. For this purpose, we estimate both a standard log-

[7] We recode the number of siblings so that the top category is 10 or more.

[8] We exclude information such as the number of books in the household and housing facilities at age 10 because they could be affected by birth order, as suggested by De Haan et al. (2014).

earnings model and a model in which the dependent variable is earnings growth relative to the initial wage. We start by estimating the following linear regression model:

$$\ln w_{it} = \alpha + \beta O_i + \gamma F_j + \delta \mathbf{X}_i + \mu_s + \mu_c + \varepsilon_{it}, \tag{1}$$

where the subscripts i, j, and t are for individuals, households, and time; w is annual real earnings; O is a dummy variable equal to 1 if the individual is firstborn and 0 otherwise;[9] F is the number of siblings in the household when the individual was aged 10; the vector $\mathbf{X}$ is described in the previous section; and μ_c and μ_s are cohort and country fixed effects. The error term, ε_{it}, can be decomposed as $\varepsilon_{it}=\lambda_j+\eta_i+\nu_{it}$, where λ_j and η_i are family and individual fixed effects, and v is random noise. Because we are interested in the effects of being firstborn on earnings at different points of the life cycle, we use as dependent variables (in logs) the entry wage; the wage 5, 10, 15, and 25 years after labor market entry[10]; the current or last wage (if retired); and lifetime earnings.

As discussed by Bagger et al. (2013), family size can be viewed as the outcome of intertemporal utility maximization by altruistic parents, and the family fixed effect λ_j as a function of parental spending and preferences, partly unobserved by the analyst. Parental choice implies that family size is a function of the family unobserved traits embedded in λ_j, or $F_j=F(\lambda_j)$. Because parents typically choose size and individual investment in human capital, which affects earnings, the family fixed effect λ_j influences individual outcomes directly. On the other hand, although the order of birth may be considered to be randomly assigned within a given family in the absence of firstborn-specific genes,[11] this is less clear-cut when variation between families is also used, as in our study. As shown in Table 1, firstborn individuals more frequently belong to smaller families, and smaller families not only fare typically better financially but may also devote more time and economic resources to each child (the quality-quantity trade-off discussed by Becker and Lewis 1973). Because family size depends on both observable and unobservable parental traits that may also be related to earnings capacity, the omission of some of these traits in Eq. (1) biases the estimated coefficient of family size and, as a consequence, contaminates the estimates of birth order effects.

BDS addressed this problem by using two approaches. With the first approach, they relied on selection on observables, including a rich set of covariates describing economic and social conditions of families, in the hope that this set captures the family fixed effect. In the second approach, they used family fixed effects, thereby focusing on within-family variation in educational outcomes. We capture some household traits by

[9] As in BDS, we treat children without siblings as firstborns. However, a sensitivity analysis that excluded single children yielded very similar results. Results are not shown but are available from the authors upon request.

[10] We obtain equivalent results when we consider earnings at age 25, 30, 40, and 50, but we prefer to use time spent in the labor market instead of age because age of entrance in the labor market could be endogenous.

[11] BDS argued that ". . . in general, there are no genes for being a firstborn or a later-born so it is unlikely that the birth order effects we find have genetic or biological causes. . . ." (p. 20). De Haan et al. (2014) have recently questioned this assumption on the grounds that later-borns may face higher prenatal environmental risks because of increased levels of maternal antibody, which attack the development of the brain *in utero*.

Table 1 Summary statistics, by birth order

	Oldest Sibling			Other Sibling		
	Mean	SD	Number of Obs.	Mean	SD	Number of Obs.
Entry Wage	11,764.6	11,924.2	1,748	10,576.2	13,063.9	2,522
Entry Wage in Second Job	18,124.8	17,140.1	1,275	16,358.6	15,476.7	1,979
Entry Wage in Third Job	22,307.8	19,460.5	849	20,554.4	16,686.9	1,363
Wage 5 Years After Labor Market Entry	15,091.5	14,691.5	1,698	14,105.7	15,048.0	2,433
Wage 10 Years After Labor Market Entry	18,372.7	16,143.1	1,731	18,039.1	16,614.4	2,485
Wage 15 Years After Labor Market Entry	20,648.0	17,579.1	1,738	19,792.7	16,131.3	2,499
Wage 25 Years After Labor Market Entry	22,763.1	16,506.3	1,714	21,836.0	16,054.6	2,467
Current Or Last Wage	23,555.6	15,168.6	1,748	22,799.1	15,179.3	2,522
Lifetime Earnings Net of Pensions	8,851.3	5,582.6	1,748	8,689.3	5,485.5	2,522
Age When First Job Started	19.558	4.064	1,748	18.565	3.985	2,522
Not Employed 5 Years After Labor Market Entry	0.029	0.167	1,748	0.035	0.185	2,522
Not Employed 10 Years After Labor Market Entry	0.010	0.098	1,748	0.015	0.120	2,522
Not Employed 15 Years After Labor Market Entry	0.006	0.075	1,748	0.009	0.095	2,522
Not Employed 25 Years After Labor Market Entry	0.019	0.138	1,748	0.022	0.146	2,522
Age When Last Job Ended	58.156	4.425	1,748	57.792	4.397	2,522
Only Child	0.240	0.427	1,747	0	0	2,522
Number of Siblings (including the interviewee)	2.514	1.456	1,748	3.913	1.941	2,522
Mother in the House at 10	0.965	0.184	1,748	0.972	0.165	2,522
Father in the House at 10	0.914	0.281	1,748	0.930	0.255	2,522
Foster Mother in the House at 10	0.021	0.142	1,748	0.011	0.105	2,522
Foster Father in the House at 10	0.032	0.176	1,748	0.017	0.128	2,522
Grandparents in the House at 10	0.147	0.354	1,748	0.106	0.308	2,522
Other Relatives in the House at 10	0.059	0.236	1,748	0.048	0.215	2,522
Other Nonrelatives in the House at 10	0.016	0.126	1,748	0.022	0.146	2,522
Hunger Episodes Before Age 15	0.031	0.175	1,748	0.041	0.199	2,522

Table 1 (continued)

	Oldest Sibling			Other Sibling		
	Mean	SD	Number of Obs.	Mean	SD	Number of Obs.
Parents Smoke, Drank, or Had Mental Problems	0.692	0.462	1,748	0.700	0.458	2,522
At Least One Parent Died Before Turning 35	0.038	0.192	1,748	0.017	0.129	2,522
Breadwinner at 10 Is Blue Collar	0.661	0.473	1,748	0.722	0.448	2,522
Lived In Rural Area	0.378	0.485	1,748	0.439	0.496	2,522
% With Postsecondary or Higher Education	0.358	0.479	1,748	0.282	0.450	2,522
Years of Education	12.586	4.091	1,748	11.491	4.246	2,522
Age of Mother at Birth	23.865	3.954	762	27.666	4.709	691

Source: SHARE survey Waves 1, 2, and 3.

conditioning our estimates on the covariates included in vector **X**. When these effects are netted out and we estimate Eq. (1) by ordinary least squares (OLS), the bias in the estimated coefficient of birth order is

$$\beta_{OLS} = \beta + \frac{\operatorname{cov}(O_i, F_j)}{\operatorname{var}(O_i)}(\gamma - \gamma_{OLS}) + \frac{1}{\alpha}\frac{\operatorname{cov}(O_i, F_j)}{\operatorname{var}(O_i)} + \frac{\operatorname{cov}(O_i, \eta_i)}{\operatorname{var}(O_i)}, \quad (2)$$

where we assume that $F(\lambda_j)=\alpha\lambda_j$. Furthermore, $\operatorname{cov}(O_i,\eta_i)=0$ in the absence of firstborn-specific genes. We conclude from this that the bias in Eq. (2) is driven both by the negative correlation between order of birth and family size, $\operatorname{cov}(O_i,F_j)$, and by the OLS bias in estimated family size effects, $(\gamma_{OLS}-\gamma)$.

The use of family fixed effects in Eq. (1) would produce consistent estimates of birth order effects. However, because we do not observe multiple members within the same original family in our data, this option is precluded. As an alternative strategy, we estimate separate regressions by family size—as done, for instance, by BDS. We show that the qualitative results based on these estimates are broadly unaffected when we pool different family sizes, suggesting that the bias induced by pooling has relatively small effects on the coefficient of interest, which measures the effects of birth order on labor market outcomes. Reassuringly for our estimation strategy, BDS found that birth order effects on educational attainment are rather homogeneous across families of different sizes and that their estimates do not vary much when family fixed effects are added to tease out unobservable family characteristics.

Notice that empirical strategies that rely on family fixed effects are not entirely free of problems. To see why, consider that within a given family, firstborn and later-born children usually belong to different birth cohorts and therefore tend to face different macroeconomic and labor market conditions at several key moments of their lives. For example, Oreopoulos et al. (2012) found negative effects of graduating during a recession on employment and earnings, especially in the short run. This may confound the effect of birth order on earnings.

When the effect of birth order on earnings is constant over the life cycle, the key parameter β is also constant and earnings *growth* is independent of birth order. In this case, the estimated effect is permanent. However, when the effect declines or increases with labor market experience, earnings growth is either a negative or a positive function of birth order. In either case, the birth order effect can include both a temporary and a permanent component. Because we have measures of real annual earnings at different points of an individual's working life as well as a measure of lifetime earnings, we can study the effects of birth order on earnings growth.

To illustrate, suppose that firstborns have a higher initial wage in their first job than later-borns, and also assume that we can observe the wage of both groups 25 years after labor market entry. We can then estimate

$$\begin{aligned}\ln W_{i25} - \ln W_{iF} &= (\alpha_{25} - \alpha_F) + (\beta_{25} - \beta_F)O_i + (\gamma_{25} - \gamma_F)F_j \\ &\quad + (\delta_{25} - \delta_F)\mathbf{X}_i + \phi_s + \phi_c + (v_{i25} - v_{iF}),\end{aligned} \tag{3}$$

where the subscripts F and 25 are for the entry wage and the wage 25 years after entry, and the parameters ϕ are country and cohort effects. This approach has the advantage that it differences out both family and individual fixed effects, λ_j and η_i. Thus, we can consistently estimate whether the earnings gap between firstborns and later-borns is constant or varies over the life cycle even when family fixed effects cannot be used. Assuming that $\beta_F > 0$, the estimation of Eq. (3) allows us to evaluate whether the positive effect of birth order on earnings persists ($\beta_{25} - \beta_F = 0$), increases ($\beta_{25} - \beta_F > 0$), or declines ($\beta_{25} - \beta_F < 0$) over time.

Results

Table 2 shows the estimated effect of the dummy variable "oldest child" on educational attainment, both by family size (two, three, and four siblings) and by all family sizes pooled.[12] In panel A of the table, we use a discrete variable equal to 1 for education below the International Standard Classification of Education (ISCED) level 2, equal to 2 for ISCED 2 and 3 (secondary education), and equal to 3 for postsecondary and tertiary studies (ISCED 4 or higher). In the table, we report the marginal effects of order of birth on the probability of having at least postsecondary education, estimated using an ordered probit model.

In panel B of Table 2, we use the number of years of schooling, which facilitates the comparison of our results with those obtained by BDS and others. We find a positive and statistically significant effect that ranges between 4.8 and 5.4 percentage points in the probability of having at least postsecondary education, and between 0.645 and 0.745 years of education, similar to the average effect estimated by BDS for Norway (0.656)[13] but much higher than the effect estimated by Björklund and Jäntti (2012) for Sweden (0.248).

[12] Similar to Price (2008), we stop at a family size of four siblings because sample sizes for households with a higher number of siblings would be very small.

[13] This effect is computed as the arithmetic mean of the effect of being the 2nd to the 10th child (Black et al. 2005: table 8).

Table 2 Birth order effects on education, by number of siblings: Marginal effects on the probability of having at least a postsecondary education from an ordered probit model (panel A) and on the number of years of schooling from a linear regression model (panel B)

	Two Siblings	Three Siblings	Four Siblings	All Siblings
A. Probability of Having Postsecondary Education (ordered probit)				
Oldest child	0.048**	0.051*	0.048†	0.054**
	(0.020)	(0.023)	(0.023)	(0.012)
Number of siblings	—	—	—	–0.011**
				(0.003)
Number of Observations	1,310	1,019	647	4,270
Pseudo-R^2	0.121	0.149	0.150	0.128
B. Number of Years of Schooling (linear regression)				
Oldest child	0.745**	0.690**	0.701*	0.645**
	(0.205)	(0.248)	(0.336)	(0.123)
Number of siblings	—	—	—	–0.122**
				(0.033)
Number of observations	1,310	1,019	647	4,270
R^2	.243	.253	.319	.252

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at age 10; grandparents in the house at age 10; other relatives in the house at 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Robust standard errors are shown in parentheses.

†$p < .10$; *$p < .05$; **$p < .01$

In Table 3, we report the estimated effect of birth order both on the entry wage and on the current or last wage, separately by number of siblings (two, three, or four siblings) and by all different family sizes pooled, after controlling for sibship size. The table is organized in eight columns, four for each definition of earnings. We find that the dummy variable "oldest child" has a positive, sizable, and statistically significant effect on the entry wage. Depending on the number of siblings, our estimates suggest that at labor market entry, firstborns earn approximately 13.7 % to 18.8 % more than later-borns—a substantial amount. Yet, this gain disappears by around age 50, when the wage in the current or last job is measured.

Table 3 also shows that our qualitative results are not affected if we pool families with different numbers of siblings. For instance, we estimate on the pooled sample that firstborns enjoy a 13.7 % premium with respect to later-borns in their entry wage and no premium at all in their current wage. Thus, we will focus the presentation of our results in the rest of this section on the sample that pools all family sizes.[14]

[14] Detailed results by family size are available from the authors upon request.

Table 3 Birth order effects on real earnings, by family size and pooling sizes: Dependent variable is log real wage

	Entry Wage 2 Siblings	Entry Wage 3 Siblings	Entry Wage 4 Siblings	Entry Wage All Siblings	Wage in Current or Last Job 2 Siblings	Wage in Current or Last Job 3 Siblings	Wage in Current or Last Job 4 Siblings	Wage in Current or Last Job All Siblings
Oldest Child	0.137*	0.146*	0.188†	0.137**	0.005	–0.019	0.029	–0.011
	(0.056)	(0.064)	(0.098)	(0.033)	(0.031)	(0.039)	(0.057)	(0.020)
Number of Siblings	—	—	—	–0.032**	—	—	—	–0.021**
				(0.009)				(0.005)
Observations	1,310	1,019	647	4,270	1,310	1,019	647	4,270
R^2	.236	.255	.250	.233	.250	.272	.227	.210

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at age 10; grandparents in the house at age 10; other relatives in the house at age 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Robust standard errors are shown in parentheses.

† $p < .10$; * $p < .05$; ** $p < .01$

Table 4 Birth order effects on earnings: Without imputation, with single imputation, and with multiple imputation

	Entry Wage No Imputation	Entry Wage With Single Imputation	Entry Wage With Multiple Imputation
Oldest Child	0.129**	0.137**	0.113**
	(0.040)	(0.033)	(0.040)
Number of Siblings	–0.034**	–0.032**	–0.034**
	(0.011)	(0.009)	(0.010)
Number of Observations	3,254	4,270	4,270

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at age 10; grandparents in the house at age 10; other relatives in the house at age 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Multiple imputations are obtained by drawing five random wages from the pool of 10 closest neighbors in terms of the predicted wage. Robust standard errors are shown in parentheses.

**$p < .01$

Because several entry wages in our data have been imputed, one may worry that our imputation method drives our results for these wages. We address this concern in two ways. First, we compare in Table 4 the estimated birth order effects on entry wages with and without imputation, showing that results are very similar. The marginal effect of birth order is slightly smaller without imputed data than with them (12.9 % vs. 13.7 %), but the difference is not statistically significant. Second, we also show in Table 4 that both the magnitude and the precision of our estimated birth order effects vary little when we use single versus multiple imputations: the latter estimate is somewhat smaller than the former (11.3 % vs. 13.7 %), and both are statistically significant at the 1 % level of confidence.

An additional source of concern is that the estimates in Table 3 do not control for the age of the mother at birth. According to Kantarevic and Mechoulan (2006), this can affect our estimates because parents of firstborns are likely to be younger than parents of later-borns. Unfortunately, our data include information on the age of parents at birth only for the interviewed individuals whose parents were still alive at the time of the survey. Given that the survey focuses on individuals aged 50+, this is only a minority of the original sample. Nonetheless, for this smaller sample, we can compare estimates with and without controlling for the age of the mother at birth. As reported in Table S1 in Online Resource 1, including the age of the mother at birth as an additional covariate in the regressions has no relevant effect on our estimates.

In Table 5, we look at earnings measured at different points of the lifecycle (5, 10, 15, and 25 years after labor market entry) as well as at lifetime earnings, and confirm that order of birth matters only at labor market entry.[15] Because we do not detect any

[15] Using a higher real interest rate (3 %) to compute lifetime earnings does not alter qualitatively the effect of the order of birth, which is estimated at –0.003 (standard error = 0.019). Table S2 in Online Resource 1 shows that our results are robust to controlling for the prevailing macroeconomic conditions in the country of the respondent at the time when the earnings are measured, using GDP growth. In this case, we cluster standard errors at the country-by-year level. Data on GDP growth by country and year are from the Maddison Tables (http://www.ggdc.net/maddison/oriindex.htm).

Table 5 Birth order effects on earnings over the life cycle, pooling family sizes

	Entry Wage	Wage 5 Years Later	Wage 10 Years Later	Wage 15 Years Later	Wage 25 Years Later	Lifetime Earnings
Oldest Child	0.137**	0.047	–0.013	0.012	0.016	0.000
	(0.033)	(0.031)	(0.027)	(0.026)	(0.023)	(0.019)
Number of Siblings	–0.032**	–0.020*	–0.026**	–0.016*	–0.016*	–0.015**
	(0.009)	(0.009)	(0.008)	(0.007)	(0.006)	(0.006)
Number of Observations	4,270	4,131	4,216	4,237	4,181	4,270
R^2	.233	.249	.247	.228	.213	.267

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at age 10; grandparents in the house at age 10; other relatives in the house at age 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Robust standard errors are shown in parentheses.

$*p < .05$ $**p < .01$

statistically significant effect of birth order on lifetime earnings, we conclude that its effect on the entry wage is entirely temporary.

As further support to this conclusion, Table 6 presents the estimated effects of birth order on earnings growth over the life cycle, measured alternatively as the difference between earnings after 5, 10, 15, and 25 years in the labor market, current earnings, and the entry wage. By differencing individual wages over the life cycle, we can purge our estimates from the influence of unobserved fixed family and individual effects on earnings. In all cases, the estimated coefficient associated with being firstborn is negative

Table 6 Birth order effects on wage growth over the life cycle

	Wage 5 Years After Entry – Entry Wage	Wage 10 Years After Entry – Entry Wage	Wage 15 Years After Entry – Entry Wage	Wage 25 Years After Entry – Entry Wage	Wage in Current or Last Job – Entry Wage
Oldest Child	–0.094**	–0.144**	–0.122**	–0.121**	–0.147**
	(0.026)	(0.032)	(0.034)	(0.035)	(0.036)
Number of Siblings	0.012*	0.006	0.018*	0.015	0.011
	(0.007)	(0.009)	(0.010)	(0.010)	(0.010)
Number of Observations	4,131	4,216	4,237	4,181	4,270
R^2	.027	.055	.069	.091	.137

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at 10; grandparents in the house at age 10; other relatives in the house at age 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Robust standard errors are shown in parentheses.

$*p < .05$ $**p < .01$

and statistically significant, ranging from –9.4 % to –14.7 %. This finding confirms that although firstborns may have an early advantage, later-borns quickly catch up.

We investigate whether the birth order effect disappears when we control for differences in educational attainment by adding years of schooling as an additional covariate in the earnings growth regressions, where the fixed individual and family effects that correlate with educational levels have been removed. Table S3 in Online Resource 1 shows that education attracts a negative and statistically significant coefficient, and that the effect of birth order remains even after we condition on education, albeit with a lower absolute value. This finding suggests that education is not the only mediator of the effects of birth order on earnings.

Finally, we consider the effects of birth order both on the age in the first job and on the probability of being without a job at different points in the life cycle (see Table S4 in Online Resource 1). We estimate that firstborns start working about 0.7 years later than later-borns, consistent with the fact that they complete around 0.7 more years of education. However, we fail to find effects on employment 5, 10, 15, and 25 years after labor market entry.

Discussion

We have found that firstborns have higher earnings than later-borns in their first job. This initial advantage, however, is temporary and declines with labor market experience. To explain our results, it is useful to briefly describe the labor market careers of firstborns and later-borns and to highlight the importance of labor mobility in the process of catching up of the latter with the former. Table 7 shows that firstborns are less mobile: compared with later-borns, firstborns are 4.1 percentage points less likely to have more than a single job in their careers and are more likely to be employed in their first job as white-collar workers or as employees in the public sector.[16] These jobs are typically more stable than private sector jobs (see Clark and Postel-Vinay 2009), and in some countries, they are also associated to milder age earnings profiles.[17]

We define stayers and movers as those who are still in their first job 10 years after entry in the labor market and those who have moved to new jobs by that time. We estimate that in our data, the probability of being a stayer is 4.4 percentage points lower for later-borns than for firstborns.[18] For both, the

[16] Because 10.2 % and 8.7 % of later-borns are white-collar workers and workers in the public sector, respectively, the estimated percentage difference is equivalent to a 25.5 and a 35 % gap. Our results are qualitatively unaltered when we add education as an additional control.

[17] Cappellari (2002) and Hartog and Oosterbeek (1993) showed that age earnings profiles are steeper in the private sector in Italy and the Netherlands, respectively. Conversely, Dustmann and Van Soest (1998) showed that profiles are steeper in the German public sector, and Disney et al. (2009) presented mixed evidence for the United Kingdom. Following Zajonc (1976), we speculate that firstborns may have had to share with parents the responsibility of raising younger siblings. This could have induced them to invest effort and parental networks to locate a good and stable first job and to keep it longer. In support of this view, Table S5 in Online Resource 1 shows that the probability of a firstborn landing a white-collar or a public-sector job as a first job increases with the number of siblings.

[18] Table S6 in Online Resource 1 compares stayers and movers after 10 and 15 years in the labor market. The table shows that stayers (more likely to be firstborns) start their second job (if ever) at an average age of 39, more than 17 years later than movers.

Table 7 Birth order, number of jobs held, and type of first job

	Had More Than One Job	First Job Was Full-Time	First Job Was White Collar	First Job Was in Public Sector
Oldest Child	–0.041**	–0.003	0.030**	0.036**
	(0.014)	(0.006)	(0.010)	(0.010)
Number of Siblings	0.001	0.002	–0.003	–0.001
	(0.004)	(0.002)	(0.003)	(0.003)
Number of Observations	4,270	4,270	4,265	4,270
R^2	.064	.029	.117	.061

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at age 10; grandparents in the house at age 10; other relatives in the house at age 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Robust standard errors are shown in parentheses.

$^{**}p < .01$

average log wage after 10 years in the labor market can be written as $\log W_{10} = p_{10} \times \log W^S_{10} + (1 - p_{10}) \times \log W^M_{10}$, where the superscripts S and M are for stayers and movers, and p_{10} is the probability of being still in the first job after 10 years in the labor market.

For firstborns, we calculate that the log wage after 10 years in the labor market is equal to 9.469 = 0.399 × 9.229 + 0.601 × 9.632. For later-borns, it is equal to 9.442 = 0.328 × 9.128 + 0.672 × 9.599. Therefore, the average wage earned by firstborns after 10 years in the labor market is only about 2.7 % higher than the wage earned by later-borns (9.469 – 9.442 = 0.027, not statistically significant), in spite of the fact that firstborn stayers earn, on average, 10.1 % more than later-born stayers (9.229 – 9.128 = 0.101, statistically significant at the 5 % level). Because the average wage of movers differs by only 3.3 % (9.632 – 9.599 = 0.033, not statistically significant), the catching up by later-borns that we observe in our data is explained by their higher turnover probability (0.672 – 0.601 = 0.071, statistically significant at the 5 % level).[19] We conclude that firstborns start with a good match—sometimes a white-collar or a public job—and stay in this match for a relatively long period. Later-borns instead struggle from initial low wages to higher wages by moving quickly to new, higher-paying jobs.

Why do we observe these differences in labor market turnover? An important reason is education: because firstborns are better educated, they are more likely to locate a good initial job. An additional candidate factor, we believe, is that later-borns are more willing than firstborns to engage in risky behavior and change employers more frequently.

Allen et al. (2005) showed that the relationship between turnover intentions and turnover is stronger for those with lower risk aversion. In support of this view, the

[19] We find similar results 15 years after entry in the labor market. We also examine whether being firstborn had any effect on experiencing unemployment but find no evidence that this is the case.

psychological literature has pointed out that later-borns tend to be more rebellious and reckless than firstborns,[20] who instead have a tendency to be more conscientious and self-disciplined (Sulloway 2007). Psychologists explain these differences by referring to the fact that although firstborns are endowed with higher parental resources (see Lehmann et al. (2013) and Hotz and Pantano (2015)), later-borns are put under greater pressure to obtain the same returns from more limited resources and thus need to take riskier moves (Wang et al. 2009).

To verify whether later-borns are less risk-averse than firstborns, we use principal component analysis to extract the latent variable ρ from the vector **Γ**, which includes five indicators of risk attitudes available in our data: whether the individual has ever bought private retirement accounts or life insurance packages; body mass index (BMI); and smoking and drinking habits. Smoking habits are captured by a dummy variable indicating whether the individual has ever smoked, and drinking habits by a dummy variable indicating whether the individual drinks alcohol on a daily basis. Because this variable increases with risky health behaviors and decreases with the willingness to buy insurance and retirement accounts, we interpret it as a measure of the propensity to take risks.

We regress ρ on birth order and the other covariates and report our estimates in Table 8. We find that the effect of being firstborn on the willingness to take risks is negative and statistically significant, independently of whether we control for the mediating role of education. Unless the distribution of wage offers differs substantially between firstborns and later-borns, higher risk aversion implies not only that firstborns are less mobile but also that their initial earnings advantage with respect to later-borns declines over time, given that later-borns move more frequently to higher paying jobs than firstborns.[21]

We next augment the human capital model so that it can account for our findings,[22] as given by the following augmented Mincer earnings function:

$$\ln w_{it} = a + b_1 S_i + c_1 x_{it} + b_2 S_i x_{it} + d_1 R_i + d_2 R_i x_{it} + f\mathbf{X}_i + \lambda_j + \eta_i + \varepsilon_{it}, \quad (4)$$

where S, R, and x are, respectively, education, risk-taking attitudes, and potential labor market experience. Bonin et al. (2007) and Hartog et al. (2003) have shown that wages

[20] In his extensive monograph "Born to Rebel," Sulloway (1996) showed descriptive evidence that firstborns have always been more prone to support the status quo and that later-borns have been more willing to challenge it. In addition, later-borns are more likely to play risky sports than firstborns (Nisbett 1968) and when playing the same sport are more likely to carry out riskier moves (Sulloway and Zweigenhaft 2010). Other research has shown that being a later-born positively affects the number of times a college student was arrested (Zweigenhaft and Von Ammon 2000). These findings mirror general beliefs about personality traits of first and later-borns (Herrera et al. 2003). Finally, Healey and Ellis (2007) found differences in conscientiousness and openness to new experiences across birth order by using family fixed-effect models.

[21] Let us define w_0, w_1, U', U'', and RR, respectively, as the current wage, the wage offer, the first and second derivatives of the utility function, and the index of relative risk aversion. Using a second-order Taylor approximation of $U(w_1)$ around w_0, we see that a wage offer is accepted and mobility occurs when

$$\frac{w_1 - w_0}{w_0} > -\frac{2U'(w_0)}{U''(w_0)w_0} = \frac{2}{RR}.$$

If the distribution of expected wage gains $\frac{w_1 - w_0}{w_0}$ is not too different for firstborns and later-borns, both turnover and observed earnings growth are lower for the former, who have higher values of RR.

[22] The learning model could also be augmented by positing that birth order captures other individual attitudes and noncognitive skills accumulated before schooling (see Cunha and Heckman 2007; Heckman et al. 2006). However, this extension would require that employers could observe birth order, which seems unlikely in the presence of rules prohibiting discrimination.

Table 8 Birth order, education, and the propensity to take risks (without (1) and with (2) controls for years of schooling)

	Risk Propensity	
	(1)	(2)
Oldest Child	–0.071*	–0.057*
	(0.028)	(0.028)
Years of Schooling		–0.021**
		(0.003)
Number of Siblings	0.007	0.004
	(0.007)	(0.007)
Number of Observations	3,922	3,922
R^2	.189	.197

Notes: All regressions include dummy variables for cohort; country; mother in the house at age 10; father in the house at age 10; foster mother in the house at age 10; foster father in the house at age 10; grandparents in the house at age 10; other relatives in the house at age 10; hunger episodes by age 15; parents smoked, drank, or had mental problems; at least one parent died by age 35; breadwinner occupation at age 10; and lived in rural area at age 10. Robust standard errors are shown in parentheses.

$*p < .05$ $**p < .01$

are increasing in risk-taking attitudes ($d_1 > 0$). We have shown that firstborns are more risk-averse than later-borns, implying that

$$R_i = r_0 - r_1 O_i + z_i, \tag{5}$$

where O_i is a dummy variable for being firstborn, and $r_1 > 0$. Placing Eq. (5) into Eq. (4) yields

$$\begin{aligned}\ln w_{it} = (a + r_0 d_1) + b_1 S_i + (c_1 + r_0 d_2) x_{it} + b_2 S_i x_{it} + d_1 r_1 O_i - d_2 r_1 O_i x_{it} \\ + f\mathbf{X}_i + \lambda_j + \eta_i + \varepsilon_{it},\end{aligned}$$

and by taking first differences, we obtain

$$\Delta \ln w_{it} = (c_1 + r_0 d_2) + b_2 S_i - d_2 r_1 O_i + \Delta\varepsilon_{it}. \tag{6}$$

Using data from the Survey of Consumer Finances, Shaw (1996) found that wage growth is positively correlated with preferences for risk-taking. In our setup, this implies that $d_2 > 0$ and that firstborns have lower earnings growth, which is in line with our finding. We also notice that the entry wage is the wage at zero labor market experience ($t = 0$), so that

$$\ln w_{i0} = (a + r_0 d_1) + b_1 S_i - d_1 r_1 O_i + f\mathbf{X}_i + \lambda_j + \eta_i + \varepsilon_{i0}, \tag{7}$$

Because education is higher among firstborns, they have higher early wages if the positive effect on earnings of their higher education more than compensates the negative effect of being less willing to take risks.[23]

Conclusions

Although empirical evidence suggests that birth order affects wages, less is known about whether and to what extent this effect is temporary or permanent. We used a sample of 4,270 European males born between 1935 and 1956 to study how the effects of birth order on earnings vary over the life cycle. We found that firstborns earn, on average, a 13.7 % premium in their entry wage. This advantage, however, fades with labor market experience, disappearing 5 to 10 years after entry in the labor market. We also found that in our sample of Europeans aged 50+, being a firstborn has no statistically significant effect on current earnings or earnings in the last job. The temporary nature of the earnings premium and the fact that firstborns enter the labor market later than later-borns explains why we found no effect of birth order on lifetime earnings.

We investigated whether the order of birth affects earnings growth, measured at different points of the working life cycle, and found a negative effect. We interpreted this result by noticing that firstborns have both higher education and higher risk-aversion than later-borns. Using these facts, for which we find support both in this article and in the economic and psychological literature, we argue that the observed patterns of earnings can be explained by differences in labor turnover. On the one hand, better education is a key reason why firstborns start with a better match. On the other hand, the higher propensity to take risks helps in explaining why later-borns change jobs more frequently and enjoy higher wage growth than firstborns.

Compared with the rest of the literature, this article emphasizes the importance of using a life cycle approach in the study of the effects of birth order on earnings. This approach allows us to distinguish between temporary and permanent effects, unlike cross-sectional studies that consider a single point in the individual earnings profile. Because we showed that the effect of birth order varies along the life cycle, choosing a single point in this cycle is likely to yield a misleading view of the relationship between birth order and earnings.

Our study also has implications for the use of birth order as an instrument for education. This empirical strategy has been previously used to estimate the causal effect of education on earnings (BDS) and health (Mazzonna 2013), and the intergenerational effect of parental education on children's schooling (Havari and Savegnago 2013). The exclusion restriction associated to this strategy is that birth order affects earnings, health, or children's schooling only by affecting education. Our finding that birth order affects risk aversion questions the validity of this restriction given that risk aversion also affects earnings and occupational choice (for a theoretical analysis, see Hogan and Walker (2007); for empirical evidence, see Belzil and Leonardi (2007) and Bonin et al. (2007)).

Hence, the reasons for the effect of birth order on risk-taking behavior warrant further exploration. Unfortunately, our data do not allow us to shed more light on this issue. The studies by Hotz and Pantano (2015) and Lehmann et al. (2013), reporting

[23] To see this, define $S_i = \pi_0 + \pi_1 O_i + \xi_i$ and substitute this in Eq. (7). We obtain that the marginal effect of being firstborn on earnings is $b_1\pi_1 - d_1 r_1$.

that firstborns in the United States grow up in a more stringent disciplinary environment and are exposed to a more stimulated cognitive environment early in life, suggest a promising avenue of future inquiry.

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