Unpacking the birth order effects

PRELIMINARY DRAFT

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Abstract

A considerable number of studies have found negative effects of birth order on a range of individual outcomes, from earnings and employment in adulthood to cognitive and non-cognitive skills throughout childhood and adolescence. Nevertheless, studies in developing countries exhibit positive effects, suggesting that birth order estimates may be highly context-specific. Moreover, recent evidence on the strong causal effect of sibling spillovers implies that well-established birth order estimates are likely biased. This paper is the first to estimate birth order parameters on educational attainment after accounting for sibling spillovers. We exploit rich data sources in two different settings–Germany and Egypt–that allow us to link individuals to their parents and siblings. Using a variant of the standard linear-in-means model of peer effects, we find strong negative effects of increasing birth order on years of schooling. However, we show that the marginal effects of birth order are substantially attenuated once sibling interactions are accounted for. We find a negative spillover effect which points towards a preference for non-conformity (i.e. sibling rivalry) among siblings.

Keywords—birth order; sibling spillovers; years of schooling; family size; gender ...

1 Introduction

There is little doubt that the family environment is an important determinant of a child's cognitive and non-cognitive development as well as her long-term economic outcomes. However, within the same family, siblings' outcomes can be vastly different. Over the last couple of decades, the economic literature has been exploring birth order effects as one particular mechanism that leads to a systematic divergence in the outcomes of siblings within the same household. In their seminal work, Black et al. (2005) documented that later-born children perform significantly worse in terms of their education and labor market outcomes relative to their older siblings. The presence of such negative birth order effects has been later confirmed by a number of studies, as, for example, in the development of cognitive (Booth & Kee, 2009; Conley & Glauber, 2006) and non-cognitive skills (Black et al., 2018), and even in criminal behavior (Breining et al., 2020).

A theoretical explanation for these negative birth order effects can be found in the quantity-quality tradeoff (Becker, 1960; Becker & Lewis, 1973; Becker & Tomes, 1976), where the rising marginal cost of quality with respect to quantity (i.e., with each additional child, it is costlier to sustain the same level of quality among children) incentivizes parents to invest less time and fewer resources in later-born children (Galor & Weil, 2000; Rosenzweig & Wolpin, 1980).[1] Empirically there is an abundance of evidence that first-born children receive greater temporal investments (Price, 2008), higher financial transfers (De Haan, 2010; Ginja et al., 2017), more supervision (Averett et al., 2011), and better prenatal inputs such as breastfeeding and abstention from alcohol (Lehmann et al., 2018), thus underpinning the various mechanisms through which birth order effects are likely to emerge.

The negative sign of birth order effects, however, may not be as universal as previously thought. In Ecuador, De Haan et al. (2014) show that, in addition to receiving less education and parental quality time, first-born children are also more likely to participate in child labor. The fact that these effects are larger in low-income households and are reversed or muted in higher-income households implies that parents treat their older children differently (at least in part) to ease financial constraints.

[1] This argument has been questioned in the literature (Angrist et al., 2010; Black et al., 2005; Mogstad & Wiswall, 2016; Rosenzweig & Zhang, 2009). For example, Black et al. (2005) find that family size and child outcomes are not negatively correlated once birth order is accounted for.

Ejrnæs and Pörtner (2004) also find positive birth order effects, which are especially pronounced in low-educated families, in the Philippines. They interpret these results by providing a theoretical explanation where families implement an optimal stopping rule such that the last child has a favorable family environment. This implies that both the direction and magnitude of birth order estimates may be highly context-specific.

A different strand of literature has recently emerged that provides evidence of sibling spillovers following the extensive literature of peer effects in classrooms (Sacerdote, 2011). In particular, sibling spillover effects have been found in school enrollment and achievement, choice of high-school track, college institution and major (see Dahl et al. (2020) and Altmejd et al. (2021) for a review). However, to the best of our knowledge, the literature has so far ignored the interaction between birth order and siblings' spillover effects. As we argue below, to the extent that sibling spillover effects are present, the estimated birth order effects may be significantly biased.

This paper is the first to provide estimates of the birth order effects accounting for sibling interactions. We focus on two different settings, Germany and Egypt, in an effort to assess the generality of our results. The primary outcome variable is years of schooling. The focus on the years of schooling as opposed to other education-related outcomes is particularly relevant when attempting to isolate the effect of birth order on human capital formation. Since only a fraction of the population attends college/post-secondary education or chooses a major, we are able to capture the impact of birth order on a wider population.

The magnitude and sign of sibling spillover effects on educational outcomes, among other things, is interesting in itself. In theory, younger siblings can either look up to older siblings and benefit from their experiences or attempt to differentiate themselves due to sibling rivalry; research in the psychology literature provides support for both theories (see McHale et al. (2013) for an overview). Empirically, studies on schooling achievement present a positive spillover effect (Gurantz et al., 2020; Joensen & Nielsen, 2018; Nicoletti & Rabe, 2018; Qureshi, 2018), while studies on the choice of college institution and major provide mixed evidence (Altmejd et al., 2021; Dahl et al., 2020). [2]

[2] Altmejd et al. (2021) show that in Chile, Croatia and Sweden, "Younger siblings follow their older siblings to the same college and college major combination even when the target program has lower expected earnings,

The present paper contributes to both the literature of sibling spillovers and the literature of birth order effects. In order to study the latter in the presence of sibling interactions we employ an econometric model where individuals' outcomes depend on their own characteristics as well as their siblings' outcomes. Implicit in our formulation is that individuals interact with, and therefore exert some effect on all their siblings. This is a major departure from the literature on sibling spillovers where it is usually maintained that the direction of the effect is from older to younger siblings. [3] Using a variant of the linear-in-means model studied by Lee (2007) and Davezies et al. (2009), we attempt to distinguish between the direct effect of birth order from indirect effects arising from the reciprocal relationships between siblings' outcomes. [4] These reciprocal relationships may, of course, arise due to correlated unobserved characteristics, and we take this into account by including household-specific fixed effects.

Our findings can be summarized as follows: Increasing birth order has a negative effect on years of schooling, and this is true for both Germany and Egypt. We proceed to decompose ("unpack") the total effect of birth order into a direct effect and an indirect effect that is due to sibling interactions. The latter effect is found to be negative which, in the simple theoretical framework discussed in Blume et al. (2015) and Kline and Tamer (2020), points towards a preference for non-conformity among siblings. This in turn implies that the coefficient on birth order, which would be incorrectly interpreted as a marginal effect ignoring sibling interactions, is overstated.

2 Econometric Model

The specification of the econometric model follows a linear-in-means model of peer effects that relates the outcome y_{ig} of individual $i = 1, ..., m_g$ in household $g = 1, ..., N$ of size $m_g = 2, ..., M$ to

peer quality, and retention rates. If, however, the older sibling drops out of college, this eliminates any spillover effect, suggesting that older siblings' experiences in college matter." Likewise, using Swedish data, Dahl et al. (2020) that younger siblings are more likely to choose the major of an older sibling or parent, but these choices can either strengthen or weaken gender stereotypes.

[3] The only exceptions we can find are studies exploiting the one-child policy in China, which have shown that first-born children benefit from having younger siblings by positioning themselves as role models (Cameron et al., 2013; Qian, 2009).

[4] For the theoretical foundations of the linear-in-means model see, for example, the overviews in Blume et al. (2015) and Kline and Tamer (2020).

a $(k \times 1)$ vector of individual-specific exogenous covariates, $x'_{ig} = \left(x'_{1,ig}, x'_{2,ig}\right)$, whose components are k_1 and k_2-vectors respectively, and the outcomes of other members of the household, y_{jg}, $j \neq i$, according to

$$y_{ig} = x'_{1,ig}\beta_1 + x'_{2,ig}\beta_{2,m_g} + \frac{\gamma}{m_g - 1}\sum_{j \neq i} y_{jg} + \eta_g + \varepsilon_{ig} \tag{1}$$

where

$$\mathrm{E}\left[\varepsilon_{ig} \mid x_{1g}, ..., x_{m_g g}, \eta_g\right] = 0 \text{ for all } i, g \tag{2}$$

The model's unobservables are decomposed into household-specific, η_g, and individual and household specific, ε_{ig}, components. The unknown parameters $\beta = \left(\beta_1, \beta_{2,m_g}\right)$ are partitioned depending on whether or not they vary across households of different sizes. Sibling interactions are accommodated by including the mean outcome of all other siblings. Implicit in our formulation is that individuals interact with, and therefore exert some effect on, all their siblings. This is a first departure from the literature on sibling spillovers where it is usually maintained that the direction of the effect is from older to younger siblings (Joensen & Nielsen, 2018; Nicoletti & Rabe, 2019). In Manski (1993) terms, the parameter γ captures the endogenous effect. Contextual effects, whereby an individual's outcome depends on the average of the observable explanatory variables of her siblings, are not considered in our initial treatment.

We note here that the linear-in-means econometric model can be interpreted as the equilibrium of a complete information game with quadratic utility functions (Kline & Tamer, 2020). The complete information assumption, by which the individual's type – which is partially observed by the econometrician – is commonly known among group members, is more likely to hold in a small group interaction setting, and hence seems natural in our application. A typical specification of the utility function in this case includes a quadratic term measuring deviations of the individual's outcome from the mean outcomes of the siblings. The γ parameter or some known function thereof then quantifies the individual's preference to either conform to $(\gamma > 0)$ or deviate from $(\gamma < 0)$ the mean outcome.

We may write our model in matrix form by stacking the m_g equations of siblings in each household

g as

$$Y_g = X_{1,g}\beta_1 + X_{2,g}\beta_{2,m_g} + \gamma W_{m_g} Y_g + l_{m_g}\eta_g + \varepsilon_g \tag{3}$$

Here, $W_{m_g} = 1/\left(m_g - 1\right)\left(l_{m_g} l'_{m_g} - I_{m_g}\right)$ denotes the interaction matrix, which is assumed fixed, where l_{m_g} is an m_g-vector of ones and I_{m_g} the identity matrix. Taking into account the quasi-panel nature of our model, we offer here an illustration of the identification challenges associated with (3). Fixing m_g to some $m \geq 2$, and provided that $\gamma \neq 1 - m$ and $\gamma \neq 1$, we may write the reduced form of (3) as

$$Y_g = \left(I_m - \gamma W_m\right)^{-1}\left(X_g\beta + l_{m_g}\eta_g + \varepsilon_g\right) \tag{4}$$

where,

$$\left(I_m - \gamma W_m\right)^{-1} = \frac{1}{1 + \gamma/\left(m-1\right)}\left[I_m + \frac{\gamma}{\left(1-\gamma\right)\left(m-1\right)} l_m l'_m\right] \tag{5}$$

The right-hand-side variables in (4) are decomposed into idiosyncratic terms and terms corresponding to the aggregate characteristics of the group. Recovering β and γ in this case is analogous to identifying the parameters of time-invariant explanatory variables in the standard panel fixed effects regression. This is similar to the observation of Graham and Hahn (2005) in a variant of the model considered here, where the mean group outcome, excluding the own outcome, is replaced by the expectation of the outcome of the entire group. In the absence of additional assumptions on the relationship between the explanatory variables and the composite error term, one possible source of identification relies upon the availability of instruments for the group-invariant regressors, in the spirit of Hausman and Taylor (1981).

Before turning to our identification strategy, we describe the effect of the presence of sibling interactions ($\gamma \neq 0$) on the sign and magnitude of the main parameter of interest, β. Applying the within transformation to eliminate the fixed effects, the second term in (5) is also eliminated and the only identifiable object becomes the reduced form parameter $\beta\left(m-1\right)/\left(m-1+\gamma\right)$. We note that ignoring the presence of the sibling effect leads to either overstatement or understatement of β, depending on the sign and magnitude of γ. By the same argument, the within transformed error variance respectively increases or decreases by a factor of $1/\left(1+\gamma/\left(m-1\right)\right)^2$ according to whether

γ is negative or positive (Glaeser et al., 1996; Lee, 2007).

Identification is possible in the linear-in-means model if there is sufficient variation in group sizes as shown by Lee (2007). Davezies et al. (2009) extend the identification analysis in a slightly different framework which we adopt in this paper. We provide a rough sketch of the main argument here: The reduced form parameters, treated as functions of the group size, m_g, are one-to-one mappings. Given that we observe at least two different group sizes, and as long as $k_1 > 0$ and β_1 contains at least one non-zero element, (β_1, γ) are identified. Given γ, one can recover the remaining parameters, β_{2,m_g} for each m_g. Some comments are in order here. First, the γ parameter along with β_1 are assumed to be common across all group sizes which can be rather restrictive. However, the identification strategy is extended in a straightforward manner to allow some of the parameters of the regressors to vary across different group sizes. This is particularly important in our application since our focus is on birth order effects. For example, in the equation for a household of size $m_g = 2$, the coefficient on a birth order of three is necessarily equal to 0, while it may be nonzero in the equation for for a household of size $m_g = 3$. It follows that $\beta_{2,2} \neq \beta_{2,3}$. Second, we often do not observe all siblings in the household. To account for this we additionally assume that the sample selection mechanism does not depend on the unobservable (no selectivity bias). However, it can be arbitrarily related to the family fixed effect. An additional requirement is that m_g be observed.

3 Data

3.1 GSOEP

The data for our analysis are drawn from the German Socioeconomic Panel (GSOEP), a continuing annual longitudinal survey of individuals in private households in Germany.[5] We focus on individuals that are over 23 years of age, whose parents (either the mother or the father), as well as one or more

[5]The survey was initiated in 1984 in the Federal Republic of Germany. As of June 1990, following the German reunification on the 3rd of October 1990, an additional Sample of East Germans was incorporated in the survey. The GSOEP contains data on seven distinct sub-samples (A-G), covering approximately 30,000 individuals living in 11,000 households, each of which was drawn in a different multi-step random-sampling process (Goebel et al., 2019).

siblings, were present at least once throughout the sample period. Information on individuals is typically available in multiple waves, and, in this case, information are extracted for the latest year of observation. This, in principle, ensures first that we have completed family size and second that individuals have completed their education. We do not require all siblings to be observed as long as information on the total number of siblings is available. Still, families with five or more children are highly infrequent and hence are excluded from our sample.

Birth order is not directly reported in the GSOEP. In most cases we observe all siblings and therefore we can infer birth order by the individual's year of birth relative to that of the siblings. For the remaining cases, we construct birth order indicators using information on the fertility histories provided by the parents. Years of schooling are assigned according to the highest degree ever obtained. Appendix Table 5 reports the number of siblings and relative frequencies observed by sibship size and country. For example, in Germany, 1059 individuals are observed in three-sibling families, but for 80.7% of these families, all siblings are observed in the data. For the remaining households, only two of the three siblings are observed. As expected, the probability of observing all siblings decreases with sibship size. Thus, we observe all siblings for about 88% ((1694+855+313)/3260) of individuals in the German sample.

3.2 ELMPs

The data used in our analysis come from four waves-1998,2006, 2012 and 2018-of the Egyptian Labor Market Panel survey (ELMPs) which includes information on exact date of birth and birth order among siblings, together with rich information on educational attainment and labor market outcomes. A unique individual identifier allows us to track individuals across waves, and, at the same time, enables us to link individuals to their parents and siblings provided that the family has resided in the same household at least once throughout the sample period.[6] Due to our sample selection criteria, this matching is predominately performed in the early waves. In particular, we

[6]Linking comes from two sources. Individuals report the within household person number that identifies their parents. If the latter is missing, we link children to their parents based on relationship to household head.

focus on individuals between the ages of 18 and 59, with one or more siblings, who have completed their education at the time of the survey. We do not require all siblings to be observed as long as information on the total number of siblings is available. Families with six or more children are highly infrequent and hence are excluded from our sample.

The main variable of interest, birth order, comes from two sources. First, it is self-reported by individuals and second it is reported by their mothers. Specifically, the mother is asked to provide information on the month and year of birth of all her children. To construct the birth order indicators we subsequently match those to the year of birth reported by the individual. Our main analysis uses self-reported birth order. If the latter is missing, we instead use the information provided by the mother. It is less likely to link individuals to siblings and parents for the Egyptian data relative to the German data. As shown in Appendix Table 6, unlike the German case, the probability of observing all the siblings in the Egyptian sample drops sharply with sibship size. Overall, we observe all siblings for only about 37% of individuals in the Egyptian sample.

3.3 Descriptive Statistics

Tables 1 displays the summary statistics for key variables in the two data sets. The average age of individuals in the German sample is about 40 and 46% are female. In contrast, the average age in Egypt is 26 years old and women consist of 41% in the sample. The under-representation of women is likely to due to more women dropping out of the sample and forming their own households at a younger age; this may also explain the disparity in the under-representation of women between the two countries. The majority of individuals in the German sample have completed high school and fathers are more educated than mothers. In the Egyptian data, the average years of schooling is about 11 and there are considerable differences in educational attainment between fathers and mothers.

To examine these variables by birth order and sibship size, Table 2 displays summary statistics for education, the share of individuals with more than a high school diploma, and education by gender. Two trends stand out. First, in both countries, the decline in the average years of schooling by sibship size and by birth order are more or less equivalent. For example, according to col(2), 37% of first-born children and 18% of fourth-born children in Germany have vocational or college training, amounting to a disparity of about 19 percentage points on average between the first and fourth child. Likewise, the average share of post-high school graduates for siblings in two-children families is 16 percentage points higher than that for siblings in four-children families. The corresponding figures for Egypt are 23 and 25 percentage points. Second, the educational attainment of boys is more sensitive to birth order effects and sibship size in Germany while the opposite holds true for Egypt.

Table 1: Summary Statistics

	Mean	SD	Mean	SD
	Germany		Egypt	
Age	40.51	8.88	26.41	4.63
Female	0.46	0.50	0.41	0.49
Education	12.68	2.73	10.99	4.19
Mother's Age in 2016/18	66.85	9.49	52.75	6.55
Father's Age in 2016/18	69.97	9.96	58.99	6.75
Mother % Less Than HS	0.48	0.50	0.74	0.44
Mother % HS	0.42	0.49	0.20	0.40
Mother % More Than HS	0.10	0.29	0.06	0.24
Father % Less Than HS	0.41	0.49	0.64	0.48
Father % HS	0.42	0.49	0.22	0.42
Father % More Than HS	0.16	0.37	0.13	0.34
N	3260		3196	

Notes: Education is measured in years. HS refers to High School Education.

Table 2: Average Education by Birth Order and Number of Siblings

	N	Education	% with > HS	Education Male	Education Female
	(1)	(2)	(3)	(4)	(5)
			Panel A: Germany		
			Birth Order		
1	1336	13.04	0.37	12.85	13.28
		(2.87)	(0.48)	(2.84)	(2.88)
2	1391	12.62	0.29	12.44	12.81
		(2.60)	(0.45)	(2.60)	(2.58)
3	435	12.06	0.22	12.15	11.95
		(2.51)	(0.41)	(2.59)	(2.40)
4	98	11.42	0.18	11.05	12.00
		(2.76)	(0.39)	(2.66)	(2.85)
			Sibship Size		
2	1694	13.11	0.36	12.96	13.28
		(2.68)	(0.48)	(2.69)	(2.65)
3	1059	12.46	0.28	12.34	12.60
		(2.69)	(0.45)	(2.67)	(2.71)
4	507	11.72	0.20	11.55	11.96
		(2.69)	(0.40)	(2.64)	(2.75)
			Panel B: Egypt		
			Birth Order		
1	952	11.67	0.41	11.47	12.01
		(3.84)	(0.49)	(3.75)	(3.96)
2	1075	11.11	0.34	10.66	11.73
		(4.19)	(0.47)	(4.16)	(4.16)
3	698	10.47	0.26	10.13	10.94
		(4.31)	(0.44)	(4.14)	(4.50)
4	349	10.28	0.26	10.46	10.05
		(4.45)	(0.44)	(4.27)	(4.69)
5	122	9.79	0.18	9.66	9.93
		(4.43)	(0.39)	(4.10)	(4.81)
			Sibship Size		
2	536	12.27	0.47	12.05	12.67
		(3.54)	(0.50)	(3.32)	(3.88)
3	962	11.27	0.35	10.68	12.15
		(4.12)	(0.48)	(4.22)	(3.80)
4	973	10.81	0.30	10.54	11.17
		(4.18)	(0.46)	(3.99)	(4.38)
5	725	9.94	0.22	10.05	9.79
		(4.45)	(0.41)	(4.29)	(4.66)

Notes: Education is measured in years. HS refers to High School Education.

4 Results

Table 3 presents our estimates of the birth order effects on years of schooling for Germany and Egypt (Panels A and B respectively) across different specifications. Column (1) presents the OLS estimates controlling for family size (one indicator for each family size), parents' education (three indicators relative to high-school education, in particular, less than high-school, high-school and more than high-school), an indicator for sex and cohort effects. Due to data limitations, year of birth is aggregated to 4-year and 10-year birth cohorts in Germany and Egypt respectively. For Germany, we consider families of up to four children, while for Egypt families of size of up to five children are additionally included in our sample. In our initial specifications, sibling interactions are not accounted for. The effects are negative and of similar magnitude in both countries, however, only the coefficients of birth orders two and three are statistically significant.

Column (2) reports the results after controlling for household fixed-effects. In both Germany and Egypt, second-born children are expected to receive about one-half of a year of education less than their older sibling. For Egypt, the magnitude slightly increases for later-born children while in Germany, the magnitude more than doubles for the fourth-born child. Comparing columns (1) and (2) we see that the OLS estimates are attenuated relative to the fixed-effects estimates. This points towards unobserved heterogeneity at the household level that is unaccounted for in our specification in (1).

The next three columns (columns (3) through (6)) relax the assumption maintained in the previous specifications that the same parameters apply to all household sizes and present fixed-effects estimates of the model separately for each household size. The results imply that birth order effects are more pronounced in larger households.

In column (7) we present estimates for the linear-in-means specification that explicitly allows for sibling interactions. In other words, we assume that our data are generated according to (4). The sibling interactions are quantified by the parameter γ that appears in the first row of Table 3. Before discussing the results, we provide a brief overview of the estimation methodology. In a first step, we obtain estimates of the reduced form parameters, $\beta\,(m_g - 1)\,/\,(m_g - 1 + \gamma)$, reported in columns (3) to (6). The structural parameters, (γ, β), are then obtained in a second step using an efficient minimum distance estimator.[7] Two things stand out. First, the γ parameter is negative in both countries. Interpretation of γ is however complicated by the fact that sibling outcomes are endogenous and simultaneously determined. Following the analysis of Kline and Tamer (2014), γ corresponds to the marginal effect of $\frac{1}{m_g-1}\sum_{j\neq i}\mathrm{E}\big[y_{jg} \mid X_g, \eta_g\big]$. As the latter changes by one unit, the individual's average educational attainment changes by γ units as a "partial" effect through the social interaction process. Second, we obtain negative estimates for the β parameters, which we refer to as the direct effects of birth order. We observe that these are larger for birth orders of three and four for Germany compared to Egypt. Note, however, that these β parameters do not correspond to marginal effects. See our discussion below.

The results of the linear-in-means specification in column (7) can be contrasted with those obtained in our previous specifications. We first note that the fixed-effects estimates reported in column (2) correspond to the case $\gamma = 0$ which is however rejected in our linear-in-means specifications in both countries. It follows that, given that more than one household size is observed, the model in column (2) is misspecified. As already discussed, if $\gamma \neq 0$ the per-household size estimates of columns (3) to (6) correspond to the reduced form parameters. Failing to acknowledge this, one would misinterpret these as estimates of the marginal effects of birth order.

[7] Table 3 also reports the test statistic of a test of overidentifying restrictions along with the p-values corresponding to the null hypothesis that the reduced form parameters are equal to the functions of the true parameters, β, γ, implied by our model. The test fails to reject the null in both the German and the Egyptian sample.

Table 3: Birth order and endogenous effects

	OLS	Fixed-effects	Fixed-effects (by size)				Minimum Distance
			$m_g = 2$	$m_g = 3$	$m_g = 4$	$m_g = 5$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Germany						
γ							-0.599***
							(0.176)
Second child	-0.420***	-0.462***	-0.447***	-0.303*	-0.674**		-0.218**
	(0.09)	(0.09)	(0.13)	(0.15)	(0.26)		(0.091)
Third child	-0.432***	-0.760***		-0.747***	-1.166***		-0.427***
	(0.14)	(0.17)		(0.27)	(0.36)		(0.140)
Fourth child	-0.442	-1.103***			-1.688***		-0.637***
	(0.29)	(0.29)			(0.52)		(0.225)
J-test							15.33
							[0.88]
N	3157	3260	1694	1059	507		3260
	Panel B: Egypt						
γ							-0.558**
							(0.233)
Second child	-0.419***	-0.478***	-0.579***	-0.435*	-0.379	-1.151***	-0.272**
	(0.14)	((0.13))	(0.21)	(0.23)	(0.29)	(0.38)	(0.126)
Third child	-0.526***	-0.627****		-0.582*	-0.191	-1.007**	-0.384**
	(0.18)	(0.19)		(0.30)	(0.35)	(0.49)	(0.177)
Fourth child	-0.327	-0.630**			-0.557	-0.761	-0.526**
	(0.29)	(0.29)			(0.43)	(0.61)	(0.267)
Fifth child	-0.203	-0.551				-0.800	-0.645
	(0.48)	(0.48)				(0.68)	(0.427)
J-test							20.81
							[0.11]
N	2667	3196	536	962	973	725	3196

Notes: Asterisks indicate statistical significance at the 10%(*), 5%(**) and 1%(***) level. Standard errors (in parentheses) allow for correlation of errors within households. All regressions include indicators for year of birth and sex. The specifications estimated by OLS additionally include indicators for sibship size and parents education. P-values are in brackets.

Although direct effects are interesting in their own regard, one would typically like to know what is the total effect on the outcome of interest caused by a change in one of the model's exogenous regressors and this is what we consider next. Since our focus is on the discrete birth order indicators, our exposition follows along these lines. Under (2), using the expressions in (4) and (5), we obtain

$$\mathrm{E}\left[y_{ig} \mid X_g, \eta_g, m\right] = x_{ig}'\beta \frac{1}{1+\gamma/\left(m-1\right)} + \sum_{j=1}^{m} x_{jg}'\beta \frac{\gamma}{\left(\gamma+m-1\right)\left(1-\gamma\right)} + \alpha_g \tag{6}$$

$$= x_{ig}'\beta\left(1-\frac{\gamma^2}{\left(\gamma+m-1\right)\left(\gamma-1\right)}\right) + \sum_{j\neq i} x_{jg}'\beta \frac{\gamma}{\left(\gamma+m-1\right)\left(1-\gamma\right)} + \alpha_g \tag{7}$$

$$\equiv h_g\left(x_{ig}, x_{-ig}, m, \eta\right) \tag{8}$$

where $\alpha_g = \left(I_m - \gamma W_m\right)^{-1}\eta_g$ and x_{ig}, x_{-ig} denote, respectively, the own and other sibling exogenous regressors. The birth order marginal effects across the different household sizes can be computed as

$$h_g\left(1, x_{-ig}, m_g, \eta\right) - h_g\left(0, x_{-ig}, m_g, \eta\right) = \beta\left(1-\frac{\gamma^2}{\left(\gamma+m_g-1\right)\left(\gamma-1\right)}\right) \tag{9}$$

The second term in the above expression is always greater than unity and not symmetric with respect to the sign of γ. This should not come as a surprise since, in the presence of sibling interactions, the effect of varying one of the exogenous regressors enters directly through its effect on the individual's own-outcome as well as indirectly through its effect on the outcomes of her siblings. Table 4 presents the estimates of the birth order marginal effects using the structural parameter estimates reported in Table 3, column (7). These should be compared to columns (3)-(5), for Germany, and (3)-(6), for Egypt, in Table 3. We see that the bias in the birth order marginal effects is non-negligible. For example, looking at four-children households in Germany, the estimated effects are, approximately, 2.8, 2.5 and 2.4 times higher, for birth orders two, three and four respectively, compared to the actual ones reported in column (3) of Table 4.

Table 4: Marginal Effects

Household Size	$m_g = 2$	$m_g = 3$	$m_g = 4$	$m_g = 5$
	(1)	(2)	(3)	(4)
	Panel A: Germany			
Second child	-0.34***	-0.253***	-0.238***	
	(0.071)	(0.088)	(0.09)	
Third child		-0.496***	-0.467***	
		(0.138)	(0.139)	
Fourth child			-0.697***	
			(0.231)	
	Panel B: Egypt			
Second child	-0.395***	-0.31***	-0.294**	-0.288**
	(0.105)	(0.119)	(0.123)	(0.124)
Third child		-0.438**	-0.416**	-0.407**
		(0.177)	(0.177)	(0.177)
Fourth child			-0.569**	-0.556**
			(0.275)	(0.273)
Fifth child				-0.683
				(0.441)

Notes: Asterisks indicate statistical significance at the 10%(*), 5%(**) and 1%(***) level. Standard errors (in parentheses) are obtained using the delta method and allow for correlation of errors within households.

5 Conclusion

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Appendix A Tables

Table 5: Relative Frequencies of Observed No. of Siblings (Germany)

Observed Actual	2	3	4	Total
2	1694 (100.0)	0 (0.0)	0 (0.0)	1694
3	204 (19.3)	855 (80.7)	0 (0.0)	1059
4	86 (17.0)	108 (21.3)	313 (61.7)	507
Total	1984 (60.9)	963 (29.5)	313 (9.6)	3260

Notes: Rows sum to 100%

Table 6: Relative Frequencies of Observed No. of Siblings (Egypt)

Observed Actual	2	3	4	5	Total
2	536 (100.0)	0 (0.0)	0 (0.0)	0 (0.0)	536
3	561 (58.3)	401 (41.7)	0 (0.0)	0 (0.0)	962
4	423 (43.5)	360 (37.0)	190 (19.5)	0 (0.0)	973
5	304 (41.9)	237 (32.7)	133 (18.3)	51 (7.0)	725
Total	1824 (57.1)	998 (31.2)	323 (10.1)	51 (1.6)	3196 (100.0)

Notes: Rows sum to 100%