

Sibling Spillovers*

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Abstract

It is notoriously difficult to identify peer effects within the family, because of the common shocks and reflection problems. We make use of a novel identification strategy and unique data in order to gain some purchase on this problem. We employ data from the universe of children born in Florida between 1994 and 2002 and in Denmark between 1990 and 2001, which we match to school and medical records. To address the identification problem, we examine the effects of having a sibling with a disability. Utilizing three-plus-child families, we employ a differences-in-differences research design which makes use of the fact that birth order influences the amount of time that a child spends in early childhood with their siblings, disabled or not. We observe consistent evidence in both locations that the second child in a family is differentially affected when the third child is disabled. We also provide evidence suggesting that the sibling spillovers are working at least in part through the relative exposure to parental time and financial resources.

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1 Introduction

Brothers and sisters share a bond that is unlike any other relationship - they typically grow up in the same home, with the same parents and similar genetics, and experience life events together. It would not be surprising, then, to learn that siblings have important influences on children’s outcomes. This influence might manifest in multiple ways. Siblings might teach each other directly, and they might model both good and bad behaviors. And since siblings also typically grow up in the same household raised by the same parents, they share limited parental resources. Time, attention, and money directed towards one child may be time, attention, and money diverted from another. And, of course, thanks to these spillovers, policies that affect one child in a family may well have important effects on other children as well, suggesting a potential multiplier effect of a policy’s effects. They may also lead to underestimating these policy effects in models that utilize sibling fixed effects identification strategy through “control group contamination”.

Even though the question appears crucial from the policy perspective, there exists very little evidence regarding the causal role of siblings on children’s outcomes (Nicoletti and Rabe 2014; Yi et al. 2015; Breining et al. 2016b; Adhvaryu and Nyshadham 2016; Joensen and Nielsen 2017; Parman 2015; Qureshi 2016; Qureshi 2017). Because siblings share many common traits and experiences, it is difficult to identify the effect of one sibling on another. In many ways, siblings are the ultimate peers, and all of the difficulties associated with estimating peer effects (e.g. Manski 1993; 2000; Brock and Durlauf 2001; 2010; Epple and Romano 2010; Moffitt 1999) are strongly present. And it’s especially difficult to know how much of the cross-sibling correlations in economic outcomes (e.g., Björklund and Jäntti 2012; Mazumder 2008; Raaum et al. 2006; Solon 1999) are due to common shocks or sibling spillovers.

This paper makes use of population-level data from two contexts and a unique identification strategy to estimate one important element of cross-sibling correlations working through sibling spillovers. Our approach to addressing this difficult identification problem is very data-intensive. Because shocks to families are not randomly-distributed across families, we propose an identification strategy that looks *within* families, and compares two siblings, one of whom is likely more affected by a common sibling spillover than the other. We therefore consider families with *three or more children*, where a plausibly exogenous shock to family dynamics occurs in the case of the third child. Because the second child in the family is arguably more affected in early childhood by issues regarding the third child than the first child in the family would be, we can compare the outcomes of the second-born (more affected) sibling to those of the first-born (less affected) sibling in families where the third-born sibling presents issues versus those where the third-born sibling does not. In doing so, we eliminate all biases associated with unobserved time-invariant family characteristics, and isolate the effects of siblings using a differences-in-differences, within-family comparison.

This empirical approach allows us to conduct a variety of placebo exercises and falsification checks. For instance, we can compare first and second children’s birth outcomes in cases with and without a family shock occurring at the time of the third child to gauge whether families experiencing shocks are trending over time prior to the observed shock. And we can compare the

likelihood of first-born and second-born children having negative outcomes by age five in the case in which the spacing between the second-born and third-born is at least five years.

We use disability status as a vehicle through which we can study sibling spillovers. Specifically, we examine the effect of having a sibling with a disability on test scores of older children, measured when they are in elementary and secondary school. Children with disabilities require different kinds of investments of time, attention and financial resources from parents.¹ Any resulting diversion of these inputs may affect the development of other children in the family. At the same time, because siblings interact so frequently, disabled siblings may have a direct effect on children’s outcomes. This within household problem is also economically significant as millions of families are affected by disabilities of the type that we consider: In 2012-13, for instance, in the United States alone over 6.4 million children aged 3 to 21 (12.9 percent of all students) were supported under Part B of the Individuals with Disabilities Education Act (NCES 2016).²

This difference-in-differences strategy is necessary because first-born and second-born children may be different for reasons unrelated to exposure to younger siblings. There is a large literature on birth order effects, documenting differences in outcomes, with a particular focus on first-born children relative to others (e.g., Black et al. 2005; Booth and Kee 2009; Conley and Glauber 2006; Price 2008; Breining et al. 2016a). Our approach allows us to separate the birth order effect from the effect of having a disabled sibling. While this approach allows us to estimate the effects of differential exposure to a disabled sibling, we are unable to estimate the total effect of this exposure, since we cannot estimate a direct effect of the disabled sibling on the first-born child in the family.³ Therefore, we think of our results as lower-bound estimates of the magnitude of the effect of having a disabled younger sibling.

Because our research strategy is based on differential exposure to disabled younger siblings in early life, it is particularly important to observe disabilities that are noticeable to families early. We make use of administrative data that reveal when children first obtain services for their disabilities, and we concentrate on disabilities that are first identified in or before kindergarten – when children are just starting school. It is harder to separate the spillover effect from the common-shock effect for disabilities first diagnosed in the elementary school years because those disabilities are more likely to reflect unobserved family factors rather than a singular health or physical shock. Furthermore,

¹An extensive literature documents the effects of child disability on parents. The increased complexity of parenting children with disabilities affects parental labor supply and the likelihood that families rely on public assistance (Crowe 1993; Kimmel 1998; Noonan et al. 2005; Powers 2003; Salkever 1982; Deshpande 2016b). Having a disabled child affects the likelihood that parents divorce or live apart (Reichman et al. 2004; Kvist et al. 2013), decreases social participation (Seltzer et al. 2001), and reduces maternal health (Burton et al. 2008) and grandparental well-being (Mitchell 2007). In addition, a number of papers have documented the effects of childhood disability on one’s own human capital accumulation (e.g. Currie and Stabile 2006; Fletcher and Wolfe 2008) and the role of welfare transfers for disabled youth (Deshpande 2016a). Stabile and Allin (2012) identify a number of economic costs of childhood disability.

²A couple of recent papers - Fletcher et al. (2012), using PSID-CDS, and Breining (2014), using Danish registry data – use cousin comparisons to study the correlations between childhood disabilities and sibling outcomes. Cousin comparisons reduce the omitted variables bias associated with differences across extended families, but large within-extended-family differences may still remain.

³Any estimate of the general relationship between a disabled sibling and older siblings’ outcomes will combine the true spillover effect with unobserved differences between families with and without disabled children.

as an alternative to measuring the effects of disabilities, we also consider the differential spillover effects of the third-born child having problems at birth, such as congenital anomalies, abnormal conditions, low birth weight, prematurity, or very poor perinatal health. We find very similar results regardless of whether we study the spillover effects of the third-born child’s disability or of the child’s poor birth conditions. And we find evidence that our empirical design passes a wide variety of falsification checks and placebo tests, including the fact that there is no evidence of a pattern in birth outcomes across first and second-born siblings by third-born sibling disability status, and that first-born and second-born siblings are equally likely to be considered disabled by age five when the spacing between second and third-born siblings is at least five years.

We estimate these relationships using data from two data sources, drawn from two distinct locations: Florida and Denmark. In both cases, we make use of matched administrative datasets where we merge full birth cohorts with schooling data. The large sample sizes allow us to generate good statistical power while focusing on families with at least three children, a subset of which has third child with disabilities. The data also allow us to follow children, for whom we know birth outcomes and family structure, into early adolescence to measure cognitive development.

The Florida data include birth records for every birth in the state between 1994 and 2002, merged with schooling records for all children who attended public schools within the state. The key outcome we measure in Florida is the state’s criterion-referenced end-of-year standardized test, the Florida Comprehensive Assessment Test (FCAT), administered to children in grades 3-8 over the relevant time period (for more detail on these data, see [Figlio et al. 2013](#) and [Figlio et al. 2014](#)).⁴

The Danish data include birth records for every birth in the country between 1994 and 2001, merged with medical patient register and school records.⁵ Our main outcome of interest in Denmark is the cohort-standardized grade from the ninth grade exit exam (GPA). These exam results are based on assessments by the student’s teacher and by an external reviewer. While these exam scores do not capture exactly the same features as the Florida exams, they are widely interpreted to represent similar types of outcomes in the extant literature.

Including data from two very different contexts provides a number of advantages. For one, the United States and Denmark have very different educational systems, health care systems, and social welfare systems. To the degree to which we find comparable findings in these two quite different environments, this further increases our confidence that our results are externally valid and more generally reflect spillovers within the family, as opposed to reflecting something that is very context-specific. Second, disability is measured differently in the two contexts – on the education record in Florida and on the medical record in Denmark. This fact also aids in establishing the degree to which our findings are externally valid. Furthermore, having data from both settings provides greater opportunities to explore the mechanisms through which these finding might be operating, because the two countries have quite different population characteristics and different data strengths.

⁴We pooled a child’s FCAT observations across grades to reduce the potential consequences of measurement error. That said, our results are stable if we focus on a certain grade as opposed to pooling across multiple grades.

⁵Additionally, for children born between 1990 and 1993, we observe medical patient register and school records, but not birth outcomes.

For instance, in Denmark we are able to match children to parental labor market histories and sick leave, while in Florida, there is a much higher rate of single parenthood that we can study. Between the two sites, we are able to paint a more complete picture about the nature of the sibling spillovers that we seek to investigate.

We find evidence in both locations consistent with there being a sibling spillover. Specifically, we observe that, in both Denmark and Florida, student outcomes are differentially worse when a third-born sibling is disabled. In both contexts, the second-born child in a family has worse outcomes (test scores in Florida, grade point average in Denmark) than does his or her older sibling when the third-born sibling is disabled, relative to the case in which the third-born sibling is not disabled. The magnitude of these differences is policy relevant; for example, in Florida it is about half of the observed relationship between an extra year of maternal education and children’s test scores. These results are concentrated in cases in which the third child’s disability is observed early – and therefore, presumably, more likely to affect older siblings in early childhood. Furthermore, the results are more pronounced in the case of physical disabilities – which are likely to be more visible early – rather than cognitive or behavioral disabilities – which are less likely to be manifested and diagnosed early. On the other hand, we find no evidence of differences in birth outcomes across these same comparisons, suggesting that the differences that are present in schooling outcomes were not present before the youngest sibling was born.

As mentioned above, there are a number of potential mechanisms through which these sibling spillovers might occur. For one, siblings might have a direct effect on one another. But they also might have an indirect effect on one another through their effect on the allocation of parental resources – both of time and of money. Families with a child who requires additional attention, either in terms of time or in terms of costly services, will have fewer resources to share with their other children. We interrogate these possible pathways in a couple of different ways, and conclude that our results are consistent (though not definitely so) with a story of the spillovers operating in part through differences in parental allocations of time and financial resources.

2 Data and descriptive statistics

The Florida data in our analysis are based on all singletons born in Florida between 1994 and 2002 and subsequently educated in Florida public schools. For the purposes of this research, Florida’s education and health agencies matched children along three dimensions: first and last names, date of birth, and social security number. [Figlio et al. \(2013\)](#) describe details of the data and match quality; the match rate between birth and school records is nearly identical to that which would have been predicted using data from the American Community Survey. We observe sibling matches for the majority of the Florida data; [Figlio et al. \(2014\)](#) present the attributes of the sibling-matched data.

For Denmark, the key data source is the Danish Birth Register, which includes information on all individuals born in the period 1960-2010 from which we select a subsample of 1990 to 2001 births.

For each child, the data include information on exact date of birth and various birth outcomes.⁶ A unique identification number enables us to link generations, and therefore children can be linked to their parents and siblings. Given this structure of the data and access to the date of birth, we can measure each individual’s birth order and match the birth records to rich data from various administrative registers. This provides us with demographic characteristics of the parents such as age, educational attainment, labor market status, earnings, leave taking, and immigrant status.

In the two settings, we measure disability differently. In Florida, disabilities are measured on the school records, and are mutually exclusive categories.⁷ The most common disabilities observed by age five in Florida are speech impairment (48%), developmental delay (21%) and language impairment (17%). The most common disabilities first observed between ages five and ten in Florida are specific learning disabilities (46%), speech impairment (29%) and language impairment (9%). In Denmark, on the other hand, we observe disabilities recorded in the medical registries, and they are not mutually exclusive categories. The most common disabilities, among all children with any diagnoses, observed by age five in Denmark are congenital malformations and deformations of the musculoskeletal system (20%), congenital malformations of the circulatory system (10%), and congenital malformations of genital organs (9%), and the most common disabilities first observed between ages five and ten are behavioral and emotional disorders (29%), disorders of psychological development (14%) and congenital malformations of eye, ear, face and neck (8%).⁸ The fact that we observe different dimensions of disability in the different settings increases the potential for external validity of this study.

Our identification strategy requires us to observe the first several births in a family. Furthermore, because we want to have conditions as similar as possible between the first two births, we restrict the sample to the set of families for whom the father is the same in both cases. Since we make use of data from a longer range of birth years for Denmark than for Florida, we observe information on a larger number of families in Denmark than in Florida, despite the fact that Florida is more populous than Denmark.⁹ There are 80,879 Florida families for whom we observe the first two children, and of these, we observe three or more children in 9,987 families. In Denmark, we observe the first two children in 134,277 families and the first three children in 28,581 families. Of the three-child families, 2,483 Florida families and 1,882 Denmark families have a child observed with a disability by age five.

Table 1 presents descriptive statistics for our analysis population.¹⁰ The table is divided into two panels, the top showing descriptive statistics for the Florida sample and the bottom for the

⁶Birth outcomes in Denmark are only observed from 1994 forward.

⁷A child may have multiple disabilities and we observe all of these but there is always only one primary disability which we use in the analysis.

⁸Asthma is common among children, but we do not include this as a disability. Our results are robust to treating asthma as a disability.

⁹In Florida, we are restricted to only those families where we can observe the first three births during the nine-year birth records window, and we are further limited by the fact that siblings are only matched in a subset – albeit a large majority – of Florida counties. The United States also has a lower rate than Denmark of cases in which the father is the same for the first two births.

¹⁰The numbers of observations in our analyses sometimes differ from the numbers reported in this table, since we are missing data on particular outcome variables for some individuals.

Denmark sample. Column 1 shows descriptive statistics for all families with at least two children and the same father for the first two children, while column 2 shows descriptives for all families with at least three children and the same father for the first two children, a sample restriction we make to carry out our research design.

The Florida sample is demographically diverse. Twelve percent of the mothers in the Florida sample are African-American, and 23 percent are Hispanic.¹¹ Eight percent of sampled Florida mothers are high school dropouts, and 33 percent are college graduates. Note that the requirement that the first two children have the same father on the birth certificate makes this a more advantaged population than the overall population of Florida births.¹² The Danish sample is less racially and ethnically diverse than the Florida population, though 11 percent of the mothers in Denmark are immigrants. Denmark also has more high school dropout (22 percent) and fewer college graduate (24 percent) mothers in the analysis sample. Just as in Florida, the Danish estimation sample is comparatively privileged in terms of socio-economic background.¹³ The age at first birth, however, is quite similar on average: 25.8 in Florida versus 26.3 in Denmark.

The sample of families with at least three children is somewhat different from the sample of families with at least two children (conditional on the first two having the same father). In Florida, families with at least three children are more likely to have African-American mothers (17 percent versus 12 percent), but have similar shares of Hispanic mothers. In Denmark, families with three or more children are more likely to have immigrant mothers (16 percent versus 11 percent). In Florida, mothers with less education are more likely to have families with three or more children (11 percent versus 8 percent high school dropout mothers, 29 percent versus 33 percent college graduate mothers), though the pattern is reversed in Denmark (26 percent versus 24 percent college graduate mothers).

Partly because all Florida births in the data come from a nine-year window, the average spacing between first and third births in Florida (4.6 years) is shorter than in Denmark (5.9 years). Birth spacing between the first and second born is also somewhat wider in Denmark than Florida, and in both places families that go on to have a third child are also likely to have first and second born children who are born closer together. On average, among those with sibling sets observed in the data, second born children arrive when the first born is 2.7 years old in Florida and 2.9 years old in Denmark. Among families with at least three children, that birth spacing is 2.2 years in Florida and 2.4 years in Denmark.

Columns 3 and 4 of Table 1 compare families with a third child who is disabled to families with a third child who is not disabled. In Florida, mothers with a disabled third child are less likely be African-American or Hispanic, though in Denmark, there is no difference in the share of mothers who are immigrant across these groups. In both Florida and Denmark, mothers with a disabled third child are less educated on average than mothers with a non-disabled third child. Mothers with

¹¹African-American and Hispanic are not mutually-exclusive categories.

¹²Overall, 23 percent of all Florida births are to African-American mothers, 23 percent are to Hispanic mothers, 21 percent are to high school dropout mothers, and 20 percent are to college graduate mothers.

¹³Overall, 12 percent of all Danish births are to immigrant mothers; 26 percent are to high school dropout mothers, and 21 percent are to college graduate mothers.

a third disabled child are a bit younger when they give birth to their first child (23.5 versus 24.0 in Florida, 25.5 versus 25.7 in Denmark), and are less likely to be married at the time of birth, at least in Florida (70 percent versus 73 percent). These differences highlight that cross-family comparisons between children who have disabled siblings and those who do not may be confounded by differences in both observables and unobservables. This potential for confoundedness is what motivates our research design that relies on comparison within families and that controls for differences across families.

3 Differences-in-differences empirical strategy

Our research approach is to carry out a simple differences-in-differences design: We compare second versus first-born children, in families with disabled and non-disabled third children. The basic idea of the comparison is that in families with a disabled third-born child, second-born children spend a larger share of their early childhood exposed to a disabled sibling than first-borns do. However, first and second-born children may have different outcomes because of direct effects of birth order. We therefore subtract off the first- versus second-born difference measured among families that have non-disabled third-born children as a way of separating the birth-order effect from the effect of differences in exposure to the third-born sibling.

Put differently, the comparisons in Table 1 make it clear that families with disabled children are different in some ways from families with non-disabled children. This means that comparisons between children who have disabled siblings and children who do not have disabled siblings may be confounded by any differences in unobservables. Our research design accounts for those differences across families by making comparisons within families, across first- and second-born siblings.

Our main regression equation is the following:

$$Y_{if} = \alpha_f + \beta_1 M_{if} + \beta_2 D_f M_{if} + \beta_3 X_{if} + \varepsilon_{if} \quad i = 1, 2 \quad (1)$$

where Y denotes a student’s standardized test scores or grade point average (normalized to have mean zero and standard deviation one), D is an indicator variable taking the value one if the third child is observed disabled and zero otherwise (and is not actually identified itself, as it does not vary between the first and second siblings), and M is an indicator variable taking the value one if the individual is second born, henceforth “more exposed”, and zero if the individual is first born, or “less exposed”. The vector of covariates, X , includes indicator variables for year and month of birth as well as child gender. The index i indexes individuals and f indexes family, and α_f denotes the family fixed effect. Our main parameter of interest is β_2 , which represents the difference in achievement gaps for more exposed versus less exposed siblings in families with and without a disabled third child, in a model in which family fixed effects net out time invariant differences between families with and without a disabled third child.

The key identifying assumption in a difference-in-differences approach is that there can be no differential trends between the treatment and control group in the absence of treatment. One concern

is that, even before the third child is born, families with disabled third children may be trending in their risk of adverse outcomes in a different manner than are those without disabled third children. If children in families with disabled third children are becoming progressively comparatively less successful for reasons that are unrelated to the third child’s disability, this could manifest itself as if a disabled third child differentially affects the second-born sibling relative to the first-born sibling. We investigate this common trends assumption by studying birth weight and five-minute APGAR scores as outcomes of the analyses. While the identifying assumption cannot be tested directly, we would interpret potential “effects” on birth outcomes as an indication of confoundedness.

We estimate the main regression equation for the entire sample as well as for subgroups defined by immigrant status, family structure, education of mother, and type of disability. Furthermore, as another robustness check we replace M by the number of years of exposure, which means that we identify the effect of interest off variation in birth spacing instead of birth order. In Section 4.1 we further explore an alternative treatment variable based on health conditions observed at the time of birth, rather than measured by disability status, in order to exclude the possibility that our results are driven by endogenously-determined disability identification during childhood.

4 Results: Exposure to a disabled younger sibling

The results for our Florida sample are shown in Panel A of Table 2. Columns 1 and 2 show mean test scores for second- and first-born children, respectively, and column 3 shows the difference. The first and second rows show mean test scores for families with and without a disabled third-born child, respectively, and the third row shows the difference. We define disability based on the categorization in school records at kindergarten entry, around age five. As can be seen in the first and second row, first-born children score higher on standardized tests in third through eighth grade than do second-born children; this could be due to many reasons, such as differential exposure to undivided parental attention or longer exposure to parental influence, mentioned by the extensive birth order literature cited in the introduction. In families with disabled third-borns, the difference in test scores between first and second-borns is 0.112 standard deviations, and in families with non-disabled third-borns, the difference is 0.064 standard deviations. The difference between those two differences, 0.048 standard deviations ($p=0.036$), is our estimate of the effect of additional exposure to a disabled younger sibling on third through eighth grade test scores.¹⁴ Standard errors are adjusted for clustering at the family level.

¹⁴The difference in differences model is appealing because it transparently shows the variation that identifies the effect of exposure to a disabled sibling. By comparing first-borns to second-borns, however, it ignores some information that might be useful for measuring the sibling spillover. Namely, there is variation across families in the spacing between first and second-born children. In families where the first and second-born children are spaced farther apart, the difference in exposure to the third-born sibling is greater. To leverage this additional variation, we also estimate a model that includes family fixed effects, and an interaction between the difference in exposure to the third-child and an indicator for whether the family has a disabled third-born child. This model is identified largely by the same variation that identifies the difference-in-differences models shown in Table 2, but adds variation in exposure that comes from variation in birth spacing between first and second-borns across families. These results are presented in Table 3.

It is also interesting to note that children in families with disabled third-borns score lower than their counterparts in families with non-disabled third-borns. This pattern is apparent among both first and second-borns. First-borns in families with a disabled third-born sibling have test scores that are 0.040 standard deviations lower than first-borns in families with non-disabled third-born siblings. And second-borns in families with a disabled third-born sibling have test scores that are 0.089 standard deviations lower than second-borns in families with a non-disabled third-born sibling. The difference in those differences is also our estimate of the effect of additional exposure to a disabled younger sibling. It indicates that the gap between children from families with disabled and non-disabled younger siblings is greater among second-borns than among first-borns. This pattern is consistent with a causal effect of the additional exposure to a disabled child that second-borns experience relative to their older first-born siblings.

Panel B shows the parallel results for Denmark. Similar to the patterns shown for Florida, Danish first-borns score higher than second-borns, and Danish children in families with disabled siblings score lower than those with non-disabled siblings. The gap in grade point average between first-born and second-born children with disabled third-born siblings is 0.123 standard deviations. The gap in families with non-disabled third-born siblings is 0.089 standard deviations. The difference-in-differences estimate in Denmark is 0.034 standard deviations, somewhat smaller than the estimate in Florida, but still statistically significant ($p=0.073$). Smaller effects for Denmark may reflect the fact that the educational, health care, and social welfare systems are focused on alleviating the impact of disadvantages such as disabilities on the individual and the family.

Table 3 presents the same basic results but in a model with family fixed effects and controls for child gender and year and month of birth. Column 1 in row 1 of Table 3 presents the estimated effects of a third-born child identified as disabled by age five on a second-born sibling's outcomes relative to the first-born sibling's outcomes. These results are extremely similar to those presented in Table 2. We have also examined the effects of particular groups of disabilities, namely: physical, cognitive and behavioral. We find the largest deleterious effects in the case of physical disabilities.¹⁵ We also find larger estimates for reading in comparison to mathematics, consistent with the possibility that home production contributes more to reading than to mathematics performance, though we have no way of knowing for certain if this is the explanation for the stronger estimated effects on reading versus mathematics outcomes. We have also investigated, in Florida, where this analysis is possible, whether there are differences in outcomes across grade levels but find that the estimated effects are quite similar between primary and secondary grades, or between any two observed grades in the data. This could mean that the negative spillover effects of having a disabled younger sibling is largely determined in the years before third grade, but it is still of course possible that the negative spillover effects continue to change over time. Because our identification strategy exploits cross-sibling differences, it may be that once both siblings are school-aged, a disabled younger sibling has similar additional effects on both siblings. It is, of course, impossible for us to determine whether

¹⁵The estimate on the interactions of second-born and third-born being disabled by age five is -0.077 (standard error of 0.027) for physical disabilities and 0.012 (0.040) for combined cognitive and behavioral disabilities for Florida, and -0.037 (0.019) for physical disabilities and 0.031 (0.051) for cognitive/behavioral disabilities for Denmark.

the negative spillover grows only in the early years of life and then stays stable or if it continues to grow over time but not differentially between middle and eldest sibling, because of the within-family comparison that we use for identification.

In the subsequent two rows, we then augment the model to make the same comparisons in the case in which the third child is identified as disabled by age five versus the case in which the third child is identified as disabled at some point between ages five and ten. The purpose of this second specification is to gauge the degree to which relatively late-observed disabilities appear to differentially affect second-born versus first-born siblings. The answer appears to be that there's not much of a differential effect of late-observed disabilities. While the point estimates associated with early-observed disabilities are nearly identical regardless of whether we include or exclude an interaction between birth order and whether the third child was observed disabled between ages five and ten, the estimated differential for the late-observed group is a small fraction of that seen with the early-observed group, and far from statistical significance at conventional levels. For completeness, in the final row we also present results for cases in which the third-born child was observed disabled by age ten; this is simply a combination of those first observed disabled by age five and those first observed disabled between ages five and ten. As a consequence, it appears that the disabilities that have the biggest spillover, at least as identified using our particular strategy, are those that are observed early. This may be because these disabilities tend to be more severe, or at least more noticeable in very early childhood, and therefore more likely to have affected older siblings while they were comparatively young (Heckman 2006; Cunha and Heckman 2007).¹⁶ Column (3) repeats the above exercise but replaces the simple birth order comparison with variation leveraging differences in spacing between the first and second-born sibling. Panel B shows the parallel results for the Denmark sample, which albeit smaller are similar to patterns documented for Florida with the exception of lower precision in the estimates utilizing spacing between siblings.¹⁷

4.1 Testing for unconfoundedness

It may be the case that families with disabled third children are trending in a manner that is different from those without disabled third children. If children in families with disabled third children are becoming progressively comparatively less healthy or academically successful for reasons that are unrelated to the third child's disability, this could manifest itself as appearing as if a disabled third child differentially affects the second-born sibling relative to the first-born sibling.

While it is impossible to completely rule out this possibility, we can investigate whether these relationships are observed in terms of birth outcomes. Specifically, we consider four different birth outcomes observed in both Denmark and Florida: Birth weight in grams, log of birth weight,

¹⁶In the Danish data we can directly address disability severity by investigating hospitalization records. Those observed disabled by age five have twice the rate of inpatient admission, three times the rate of outpatient admission, and slightly more emergency room admissions as do those first observed disabled between ages five and ten.

¹⁷We have also conducted a nonparametric analysis of the estimated spillover effect based on different degrees of spacing between the second-born and third-born siblings. We do not have sufficient power to distinguish differential results by spacing, but the pattern of results suggests that the spillover might be larger for more closely-spaced siblings than for more distant-spaced siblings.

low birth weight (defined as birth weight less than 2500 grams), and five-minute APGAR scores (observed on a ten-point scale). Table 4 presents an analogous analysis to Table 3 – the coefficients on the interaction between the third sibling’s disability status by age five and second-born status in the family in a model with family fixed effects and indicators for gender, year and month of birth.¹⁸ (Note that in Florida we can observe a wide range of additional birth outcomes; in no instances do we find evidence that birth outcomes, however defined, are trending differently in families where the third child is disabled versus those where the third child is not disabled.) Table 4 has fewer observations in Denmark than previous tables, because we only observe birth outcomes for children born from 1994 forward. The results from Table 3 are similar when we restrict the Danish observations to be the same as those for which we can test for unconfoundedness in the birth outcomes observed in Denmark.

We observe a clear pattern of no apparent relationship between third-born disability and the differential birth outcomes of second versus first-born siblings. Consider, for example, the estimated birth weight relationship. While we find that birth weight is slightly smaller for second-born versus first-born siblings in the case of a disabled (observed by age five) third-born sibling, 17 grams in Florida or 11 grams in Denmark, these differences are tiny relative to the estimated effects of a disabled third-born sibling on differential test scores. If we apply the estimated relationships between birth weight and test scores reported by Figlio et al. (2014), the difference observed in birth weight in Table 4 would translate into test score differences of between 0.0009 and 0.0018 standard deviations, depending on the specification employed in Figlio et al.’s Table 2. Thus, the true relationship between birth weight and test scores would have to be 26 to 49 times larger in order to explain the test score differences we observe in Table 3 of this paper. The results for the other outcomes are equally minuscule. We therefore conclude that it is unlikely that pre-existing family health trends – at least not those observable at birth – are responsible for our differential academic outcomes.

As a further test, in columns (2) and (4) of Table 3 we repeat the analysis from columns (1) and (3), but also control for birth weight, one and five-minute APGAR scores, and clinical estimate of gestational age at birth.¹⁹ As can be seen in the table, the results are extremely similar to our baseline estimates. These findings provide additional evidence that factors that take place after birth and before the commencement of testing in third grade are responsible for the differences in birth order outcomes by third-born disability status. Sibling spillovers are a likely explanation.

We have also looked at other non-birth-outcome tests of unconfoundedness, and do not find statistically significant or large differences on other variables, and the patterns of (insignificant)

¹⁸In Table 4 we only present coefficients from the specification where the third child is observed disabled by age five. Our results are unchanged if we focus on the other two specifications from Table 3: third observed disabled by age five or between ages five and ten; or third observed disabled by age ten. The results are also invariant to defining exposure based on spacing between siblings rather than simple birth order. In Appendix Table A1 we further present differences-in-differences comparisons akin to Table 2 for birth weight and five-minute APGAR score.

¹⁹As mentioned above, we do not observe birth outcomes for Danish children born before 1994, so we set these missing outcomes to zero and include a dummy for missing birth outcome (birth cohorts 1990 to 1993). The results of all columns in Table 3 are essentially unchanged if we restrict the Danish sample to children for whom we observe both birth and later life outcomes.

differences do not move in consistent ways – a finding suggesting that there exists no particular pattern of differential “trending” between families that would have a disabled third child versus those who would not have a disabled third child. We find, for instance, that in Florida the difference between the second-born and the first-born siblings in the median income of the zip code at the time of birth is \$353 (standard error of 293), or 2.5 percent of a standard deviation, lower when the third child is disabled versus when the third child is not disabled, and the difference in first grade school quality, based on measures by the state of Florida²⁰, is 0.36 percentile points (standard error of 0.29), or 2.2 percent of a standard deviation lower. But on the other hand, Florida parents with a disabled third child are 1.3 percentage points (standard error of 1.0) more likely to get married between the first and second birth, relative to families without a disabled third child. In Denmark, families with a disabled third child are 0.3 percentage points (standard error of 0.5) less likely to be married or cohabiting at the time of the second birth than at the time of the first birth, relative to families without a disabled third child. The difference-in-differences in paternal income is small but statistically significant (-5223 Danish kroner, 3.3 percent of a standard deviation; standard error of 2007) but the difference-in-differences regarding maternal income is not statistically distinct from zero and is positively signed (809 kroner, 0.8 percent of a standard deviation; standard error of 1591).²¹ In sum, some patterns tend to have the first birth more advantaged in the case of a disabled third child, while other patterns tend to have the second birth more advantaged; none of the Florida patterns suggest statistically significant differences in these “pre-trends”; and the only statistically significant Danish difference is of such a small magnitude that it is highly unlikely to be driving the differences in sibling outcomes. Again, sibling spillovers are a more likely explanation for the differences in test scores apparent by third grade.

We can propose another strong test of unconfoundedness that we can carry out in Denmark but not in Florida due to data availability. We study the "effects" of a disabled third child on any interaction with hospitals of second-born versus first-born at age one and two – almost invariably *before the birth of the third child*. We have studied the "effects" on any contacts as well as contacts related to specific common diagnoses separately, and none of these are sizable or statistically significant. For instance, the “effect” on any interaction with hospital system in the first year of life is 1.1 percentage points (standard error of 2.9), and mean of the dependent variable in this regression is 25 percent. In addition, we have studied mother’s employment and interaction with hospital system when the second-born versus first-born are age 1 and 2, and again none of the "effects" are sizable or statistically significant. For example, when we measure maternal labor supply as an indicator for maternal employment at age two (mean of 69.8 percent) the “effect” is 0.2 percentage points (standard error of 1.1).

We can also propose another test of unconfoundedness that, this time, we can carry out in Florida but not in Denmark due to sample size constraints. Ideally, we would be able to study the “effects” of a disabled third child on the test score outcomes of second-born versus first-born children for families where the test score is measured *before the third-born child is born*. Unfortunately, that

²⁰See [Autor et al. \(2016b\)](#) for a description of the percentiled school quality measure.

²¹The results are very similar if we express these financial values in log kroner instead.

would require families to have birth spacing of at least eight years between the second-born and third-born children in the case of Florida (and almost twice that amount of spacing in the case of Denmark), and only a handful of Florida families have this birth spacing pattern. However, we can look at an intermediate outcome – whether the second-born child is more likely to be identified as disabled by age five when the spacing between the second and third-born children in the family is at least five years. In this case, we have 372 Florida families where we can observe an outcome for the second-born child *before the third-born child is born*.²² In this case, we find that second-born children who would eventually have disabled third-born siblings are slightly less likely to be identified as disabled before their third-born siblings are born. Consistent with a notion that unobserved trajectories are trivial and, to the extent that they exist, even work against our findings, second-born siblings with disabled third-born siblings are 0.8 percentage points less likely to be disabled than their first-born siblings, with a standard error of 5.3, relative to those without disabled third-born siblings, and the results are identical whether or not we control for birth outcomes. This test implies that our results are driven by actual sibling spillovers and not differential unobserved trajectories. But consistent with a story of differential spillovers to the second-born versus first-born child, when the spacing between the second-born and third-born is much smaller, then the second-born is more likely to be identified as disabled. This last result could be due to a disabled third-born child differentially weakening the second-born child, or potentially due to endogenous identification of the third-born sibling’s disability. The following analysis addresses this latter possibility, which remains a threat to identification.

First, we can eliminate the potential concern about co-diagnosis of siblings by estimating our models in the cases of families in which *neither* the first-born nor the second-born child is observed as disabled. In this case, we estimate that the (non-disabled) second-born child has scores that are 6.8 percent of a standard deviation lower (standard error of 3.1) in Florida and 4.3 percent of a standard deviation lower (standard error of 2.0) in Denmark than his/her (non-disabled) first-born sibling in the case in which the third-born sibling is observed disabled by age five. Again, the coefficients are nearly identical regardless of whether we include controls for birth outcomes. These estimates provide further evidence that our results are indeed due to sibling spillovers and not unobserved time-varying family factors.

Finally, because there is still the possibility that disability of the third child might be endogenously determined (if, say, the second-born sibling is performing poorly in school and that triggers increased vigilance and identification of the third-born as disabled), as an alternative to estimating the differential effects of third-sibling disability on second-born versus first-born siblings, we also consider the case in which the third-born sibling is observed with a significant issue at the time of birth as measured in the birth certificate – which we define as a congenital anomaly, an abnormal condition at birth, low birthweight (<2500g), prematurity (gestation less than or equal to 36 weeks),

²²We cannot apply this test in Denmark because there are only at most three families where the third-born child is disabled and the spacing between the second-born and third-born child is at least five years. (This is because our measures of disability in Denmark apply to smaller fractions of the population than do our measures of disability in Florida.) Danish law prohibits mentioning sample information if the sample size is three or less.

or poor health at birth (measured by five-minute APGAR scores below six on a ten-point scale). In Table 5, we present the results of this estimation.²³ We continue to find that if the third-born sibling has substantial issues at birth, the second-born sibling has worse schooling outcomes than does his/her first-born sibling. Second-born siblings score 4.5 percent of a standard deviation lower than do first-born siblings in Florida and 3.7 percent of a standard deviation lower in Denmark when the third-born sibling has substantial birth issues than when he/she does not. These results are again very similar when we include birth certificate controls for the first- and second-born children. This is consistent with our finding that the previously-mentioned results are disproportionately driven by physical disabilities that are likely observable to parents at a comparatively early age. Thanks to this alternative variation we are less concerned that our results are somehow driven by endogenous identification of disability of the third-born sibling.

We have also attempted, in the case of Florida, to stratify disabilities on the basis of severity. We do not have airtight ways of measuring disability severity, but can proxy for severity based on the degree to which the state department of education compensates school districts for a student in a specific disability category, or the fraction of students in that category who do not take the state assessment (as those who are physically or cognitively unable to take the assessment are exempt). We find that the estimated effects of disabled third siblings are most negative when the third child has a moderately severe disability, relative to mild disabilities or very severe disabilities – though we note that comparatively few children with very severe disabilities are observed in the data. We believe that this makes sense because children with the most severe disabilities receive additional services through the state’s Early Steps Program and other early childhood health and education support programs for disabled children, which might mitigate the effects of having a very disabled sibling, and those with mild disabilities might have less of an overall effect on the family. These different severity groups could therefore affect families differently due to how they influence parental time and financial resources available to older non-disabled siblings. In section 5 below we present some exploratory analysis that begins to investigate whether effects of disabled younger siblings on parental time and financial resources could be responsible for our results.

In summary, while the differentially stronger effects of a disabled third-child on second-child versus first-child outcomes are present across a variety of ways to measure third-child disability, we do not observe evidence that these differential effects are due to endogenously-determined disability identification. We do not observe evidence that our results are due to co-diagnosis of siblings; we do not observe evidence that second-born children had any different outcomes observed before the third child was born (whether birth outcomes or early health problems), relative to first-born siblings, depending on third-sibling disability status; and we find no evidence that families whose third-born children are disabled were on any different trajectories in well-being prior to that birth than were families whose third-born children are not disabled. We carry out these various balancing tests

²³In this analysis we are able to utilize full sample for Denmark because all third-born children are born in 1994 or later when we can observe complete birth records. In column (4) of Table 5 we use the same imputation strategy as in columns (2) and (4) of Table 3. The results are unchanged if we restrict the sample to only individuals born after 1994.

along a wide range of potential areas of imbalance (e.g. labor supply, income, marital stability, education, health care utilization or school quality) and consistently find no evidence whatsoever of differences across families with a disabled third-child versus those without a disabled third-child. Moreover, our results are concentrated in the types of disabilities – physical disabilities and those observed early in life – where we would most expect to see differential spillovers on second-born versus first-born siblings. In other work not reported here, we also observe no evidence that third-child disability status influences the probability of a fourth sibling being born, which could have been a different mechanism through which a second-born sibling would be differentially affected by a disabled third-born sibling. Therefore, we are as confident as we can be that our empirical results are not due to omitted variables that compromise the internal validity of our findings.

5 Heterogeneity: Understanding mechanisms

We have found a general pattern, present in both Denmark and Florida, that a disabled third sibling differentially affects the middle sibling relative to the oldest sibling. There are a number of plausible explanations for why this pattern would take place. For one, siblings who are more closely spaced in age are probably more likely to spend time together in mutual activities, so there are more opportunities for direct spillovers between the youngest sibling and the middle sibling than between the youngest sibling and the oldest sibling. But there are a set of indirect mechanisms through which this differential effect could take place as well. We think of these mechanisms broadly as involving time and money. On the time front, if a disabled younger sibling requires very time-intensive attention (e.g., additional medical visits, home-based therapies, and increased parental care) then there would be fewer opportunities for families to invest time in their older children. On the financial front, to the degree to which disabled siblings require costly therapies or equipment, or reduce parental labor supply (as is evidenced by the papers cited in footnote 2), there would be fewer financial resources available to invest in the older children. In both the financial and time resource case, the middle child should be more directly affected than the oldest child in the family, because the oldest child will necessarily be considerably older by the time the third sibling’s disability becomes apparent and the family begins to make accommodations.

While we have no direct way of investigating the potential roles of time and financial resources in explaining our patterns of results, we believe that heterogeneity analysis may be helpful. For Denmark, we supplement these heterogeneity analyses with descriptive evidence, informative about health care utilization and mothers’ use of the social services which may alleviate some of the constraints in terms of time and financial resources.²⁴

We begin by exploring the differential effects by maternal education, and divide the samples into three groups, based on whether the mother is a four-year college graduate (24 percent in Florida, 28 percent in Denmark), a high school graduate or someone with some postsecondary education (55 percent in Florida, 49 percent in Denmark), or a high school dropout (or basic school completion

²⁴We are unable to use the Danish Time Use Survey to directly address the time-constraint mechanism due to insufficient number of three-child families available in this data set.

only in Denmark; 21 percent in Florida, 23 percent in Denmark).²⁵ We suspect that the financial aspects of the sibling spillover may be more significant the less educated the mother, at least in the context of Florida with its less encompassing social safety net, given that many costs borne by families are similar across socio-economic status. On the time dimension, however, we suspect that the sibling spillover could have more substantial effects at the top of the SES distribution, given that highly-educated parents tend to spend considerably more time with their children than do less-educated parents (Guryan et al. 2008; Kalil et al. 2012), especially along quality-adjusted dimensions (Vinopal and Gershenson 2017).

Cultural and institutional factors might both play a role in determining which of these relationships may be more likely to dominate. The Danish and American social safety nets differ considerably, and the Danish welfare state offers heavily subsidized high-quality daycare including additional support for disabled children as well as income support programs for parents. According to the Law of Service, parents of severely disabled children can be employed by the municipality to care for their own children and can make claims for time lost if they are unable to work full time. Danish families have considerably more access to own sick leave, should the child disability affect parental mental health. We observe that mothers with a disabled third child are three percentage points less likely to work full time when the child is aged five and collect as much as 31% more in sickness benefits during the period from birth until the third child turns five (see Table A3 for these comparisons).²⁶ At the same time, they spend more time in doctor’s offices and hospitals with their third-born children. Furthermore, Denmark is a much smaller country (around the size of Maryland) where people live closer to their extended families. The differences in proximity between young families and grandparents across the settings is obvious when we consider that only 34% of mothers of Florida-born children (and 22% of college-graduate mothers) were even born in Florida themselves, while in Denmark, 38% of families (26% for college-graduate mothers) reside in the same *municipality*²⁷ as the child’s maternal grandparents. And these municipalities are very small – on average 169 mi², 17% of the size of the average *county* in Florida.²⁸ Metropolitan Copenhagen alone consists of 34 municipalities within 1,170 mi², representing 36% of the nation’s population, and the two largest Danish metropolitan areas combine to comprise 60% of the Danish population. New York’s metropolitan area, in contrast, accounts for just 6% of the population of the United States, and one would have to add up the 18 largest American metropolitan areas (everything from Tampa-St. Petersburg, FL and larger) to account for the same share of the American population as metropolitan Copenhagen represents.²⁹

²⁵We code maternal education as the level of maternal education at the time of the first sibling’s birth. We have also used it as an outcome and we do not observe any significant improvements in maternal education between first and second birth depending on the disability status of the third child. This provides another data point to our placebo analyses discussed in Section 4.1.

²⁶We do not find any differences in unemployment benefits utilization, and in fact mothers of disabled third-borns receive less leave benefits that are not directly related to their kids disability. We have also studied maternal labor supply and cohabitation status at age 10 as outcomes. The estimated effects are small and insignificant.

²⁷The country is divided in 98 municipalities ranging from 3.5 mi² to 600 mi² (see www.noegletal.dk).

²⁸In fact, 63% of the Danish population reside in municipalities smaller in area than the city of San Francisco, and 94% reside in municipalities smaller in area than New York City.

²⁹One would have to add up the 67 largest American metropolitan areas (everything from McAllen, TX and larger)

Table 6 presents differences in the estimated effects of a disabled younger sibling by maternal education levels in Florida and Denmark. We observe that the patterns in the estimated relationships diverge across the two settings. In the case of Florida, there is an apparent U-shaped pattern, with the most negative estimated spillovers occurring at the bottom of the SES distribution (where financial resources are more likely to play a role) and at the top of the SES distribution (where time resources are more likely to play a role). In the Danish case, there is no apparent negative spillover at the top of the SES distribution, but the negative spillover is present in the lower two groups of the SES distribution. It is important to note, however, that we cannot reject the null of equality of coefficients across SES groups at conventional levels of statistical significance (the p-value of test of equality is 0.14 in Denmark and 0.45 in Florida). Nonetheless, the patterns of these differences are still of interest.

While we cannot definitively point to mechanisms, we suspect that the different pattern of results may reflect the fact that different options are available to cope with a disabled child in the two contexts. Appendix Table A2 shows, in fact, that the negative effects in Denmark are driven by parents living in different municipalities than the maternal grandmothers at the time of birth of the third child, which is consistent with the idea that parental and grandparental time with children may be relatively substitutable. Another potential explanation for the lack of a negative effect at the top of the SES distribution in Denmark could be that these parents may be better at utilizing the public support available to cope with the challenges associated with raising a disabled child. We believe that these institutional differences explain why the magnitude of the overall effect is slightly smaller in Denmark compared to Florida, and why the pattern of heterogeneity across education of the mother varies across the two contexts.³⁰

We further attempt to unpack the spillover relationship by investigating the changing parental status of children in the data. Our sample consists of families where the father of the first two children is the same. However, the third - disabled or not - child may have a different father than the two older siblings.³¹ This may imply that more or less financial and time resources are available for the first and second born children depending on how the parents organize themselves. A different father might also mean that the father of the third sibling is absent from the family altogether, at which point a single mother might be particularly time-constrained in the case of a disabled young child.³² In practice, in Florida there is a much higher rate of the third child having a different father than the older two siblings than there is in Denmark; 13 percent of Florida families have a third-child father who is different (or absent), as compared with 6 percent in Denmark, conditional on the

to account for the same share of the American population as the two largest Danish metropolitan areas represent.

³⁰Stratification based on the immigrant status of the mother also supports this potential explanation. In both environments, we find estimates that are more negative (though not statistically significantly so) in the case of immigrant mothers than in the case of native-born mothers. In Florida, the differences-in-differences estimate is -0.066 (standard error of 0.066) for immigrant mothers and -0.042 (0.024) for native-born mothers. In Denmark, the differences-in-differences estimate is -0.119 (0.045) for immigrant mothers and -0.015 (0.020) for native-born mothers.

³¹There is negative relationship between third child disability and different or absent father indicator meaning that there are fewer disabled third-borns in households where father changes or does not claim paternity.

³²We define “absent fathers” as cases in which paternity is not claimed on the birth certificate; families where either of the first two children have “absent fathers” are excluded altogether from our analysis. See Autor et al. (2016a) for more details.

father of the first two siblings being the same. In Denmark, absent fathers are nearly nonexistent in these data, while five percent of Florida families with the same father for the first two children have absent fathers for the third sibling.

Table 7 presents a breakdown of estimated effects in Florida by three groups – same father for all three children; different father for the third child but paternity is claimed; and absent father for the third child. As can be seen in the table, the results are concentrated in cases in which the father is different for the third child than for the first two, or is absent altogether. These findings provide further evidence in support of a time (and, perhaps, financial) resources explanation as a possible reason that disabled third children negatively affect the middle siblings more than the oldest siblings in a family.³³

While families tend to be more stable in Denmark than they are in Florida, it is still possible to conduct some heterogeneity analysis along these lines in the Danish case. In Table 8 we stratify families based on whether the parents are still coresident when the third child is five years old; whether the parents are no longer coresident when the third child is five years old; or whether the father of the third child is different from the father of the first two siblings.³⁴ We find that in the Danish context there is no apparent negative effect of a disabled sibling on the differential outcomes of the older children in cases where the father of the third child differs from that of the first two. Interestingly, in the Danish environment, the results are concentrated entirely within intact families where the parents remain coresident for at least five years following the birth of the third child. In short, however, we view the available evidence as suggestive that the sibling spillovers are working at least in part through time and financial resource dimensions.

6 Conclusions

It is notoriously difficult to measure the magnitude of spillovers within a family because an exogenous shock to family interaction is required. We propose a strategy that allows us to causally identify a portion of the magnitude of a sibling spillover. Our insight is that the estimated effects of birth order should be different when the first and the second sibling are differentially exposed to a family shock. We carry this out by studying the case of a disabled third sibling. Using detailed population-level data on thousands of three-plus-child families in Denmark and Florida, we observe consistent evidence indicating that the second child in a family is more adversely affected, relative to the first-born child, when the third child is disabled, a result that survives a battery of falsification tests. The magnitude of these differences is policy relevant; for example, in Florida it is about half of the observed relationship between an extra year of maternal education and children’s test scores. We also provide some preliminary evidence that suggests that the sibling spillovers are working at least in part through the relative constraints on time and financial resources. This empirical approach could, of course, be applied to numerous other family shocks that affect one sibling differentially

³³Although statistically insignificant the estimates for families where father is the same across all three births are of similar magnitude as the average differences found for Denmark in Table 3.

³⁴These are not mutually exclusive conditions for Denmark but they are in the Florida data.

from another, and indeed, we find that our results are very similar if instead we study the differential effects of problems of the third-born child observed at the time of birth.

This research has direct implications not just for the peer effects literature, but also for a variety of discussions surrounding family policy. For example, discussions concerning human capital policy frequently consider what the benefits of interventions and resources may be for the children themselves, but are less likely to involve thinking about the spillover effects of interventions and resources for the family. Our results indicate that investments that ameliorate the effects of negative shocks to children likely have larger benefits than would be ascertained from considering the effects on the child alone. They also have also methodological consequences for the interpretation of estimates based on sibling fixed effects identification strategies where one sibling is treated and the other forms a control group, and suggest that these may be downward biased.

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Tables

Table 1: Descriptive statistics

	(1) 2+ children All	(2) 3+ children All	(3) 3+ children 3rd child observed disabled by age 5	(4) 3+ children 3rd child not observed disabled by age 5
<i>Panel A. Florida: Family characteristics</i>				
African-American	0.12 (0.32)	0.17 (0.38)	0.14 (0.35)	0.18 (0.38)
Hispanic	0.23 (0.42)	0.22 (0.42)	0.19 (0.39)	0.23 (0.42)
Mother HS dropout	0.08 (0.27)	0.11 (0.32)	0.14 (0.35)	0.10 (0.30)
Mother college graduate	0.33 (0.47)	0.29 (0.46)	0.26 (0.44)	0.30 (0.46)
Mother's age at first birth	25.76 (5.29)	23.84 (5.02)	23.51 (4.99)	23.95 (5.03)
Married at first birth	0.79 (0.41)	0.72 (0.45)	0.70 (0.46)	0.73 (0.45)
Spacing 1st to 2nd	2.66 (1.26)	2.15 (0.94)	2.11 (0.94)	2.16 (0.94)
Spacing 1st to 3rd	- -	4.64 (1.40)	4.46 (1.36)	4.70 (1.41)
Spacing 2nd to 3rd	- -	2.49 (1.15)	2.35 (1.08)	2.54 (1.17)
Number of families	80,879	9,987	2,483	7,504
<i>Panel B. Denmark: Family characteristics</i>				
Immigrant	0.11 (0.31)	0.16 (0.36)	0.17 (0.38)	0.16 (0.36)
Mother HS dropout (or basic school only)	0.22 (0.42)	0.21 (0.41)	0.24 (0.43)	0.21 (0.41)
Mother college graduate	0.24 (0.43)	0.26 (0.44)	0.24 (0.43)	0.26 (0.44)
Mother's age at first birth	26.26 (3.88)	25.65 (3.61)	25.47 (3.71)	25.67 (3.60)
Married/cohabiting at first birth	0.95 (0.21)	0.96 (0.19)	0.96 (0.21)	0.96 (0.19)
Spacing 1st to 2nd	2.91 (1.34)	2.40 (0.95)	2.40 (0.95)	2.40 (0.95)
Spacing 1st to 3rd	- -	5.92 (1.77)	5.92 (1.74)	5.92 (1.77)
Spacing 2nd to 3rd	- -	3.48 (1.56)	3.48 (1.57)	3.48 (1.55)
Number of families	134,277	28,581	1,882	26,699

Note: Only families with same father of first two children are included. The first column presents means in families with two or more children. The second column presents means in families with three or more children. The third column presents means in families with three or more children where the third child is observed disabled by age five. The fourth column presents means in families with three or more children where the third child is not observed disabled by age five.

Table 2: Difference-in-differences: Exposure to disabled younger sibling

	(1) More exposed (2nd)	(2) Less exposed (1st)	(3) Difference
<i>Panel A. Florida: Test scores</i>			
3rd observed disabled by age 5	0.156 (0.885)	0.268 (0.900)	-0.112*** (0.015)
3rd not observed disabled by age 5	0.244 (0.869)	0.309 (0.868)	-0.064*** (0.006)
Difference	-0.089*** (0.011)	-0.040*** (0.011)	-0.048** (0.023)
Number of observations (families)		104,130 (9,812)	
<i>Panel B. Denmark: 9th grade gpa</i>			
3rd observed disabled by age 5	-0.041 (0.821)	0.082 (0.802)	-0.123*** (0.026)
3rd not observed disabled by age 5	0.029 (0.806)	0.118 (0.813)	-0.089*** (0.007)
Difference	-0.070*** (0.019)	-0.036* (0.019)	-0.034* (0.019)
Number of observations		57,162 (28,581)	

Note: This table presents the results of difference-in-differences models with no covariates included. Sample means and standard deviations are reported in the first two rows of columns (1) and (2) for both Florida and Denmark. T-test differences with standard errors in the first two rows of column (3) and first two columns of the third row for both Florida and Denmark. Differences-in-differences estimates are presented in the bottom right corner for each location. Outcome variables combine math and reading assessments. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table 3: Main results: Effects of exposure to disabled younger sibling

	More exposed vs. less exposed		Exposure measured in years	
	(1)	(2)	(3)	(4)
<i>Panel A. Florida: Test scores</i>				
3rd observed disabled by age 5	-0.047** (0.023)	-0.047** (0.023)	-0.021** (0.010)	-0.020** (0.010)
3rd observed disabled by age 5	-0.046** (0.023)	-0.047** (0.023)	-0.020** (0.010)	-0.020** (0.010)
3rd observed disabled between age 5 and 10	0.008 (0.026)	0.005 (0.026)	0.007 (0.011)	0.005 (0.011)
3rd observed disabled by age 10	-0.023 (0.018)	-0.024 (0.018)	-0.008 (0.008)	-0.009 (0.008)
Controlling for birth outcomes		X		X
Mean of dependent variable			0.268	
Number of observations			104,130	
Number of families			9,812	
<i>Panel B. Denmark: 9th grade gpa</i>				
3rd observed disabled by age 5	-0.032* (0.018)	-0.030 (0.018)	-0.010 (0.007)	-0.009 (0.007)
3rd observed disabled by age 5	-0.033* (0.018)	-0.030* (0.018)	-0.010 (0.007)	-0.009 (0.007)
3rd observed disabled between age 5 and 10	-0.017 (0.024)	-0.016 (0.024)	-0.010 (0.009)	-0.009 (0.009)
3rd observed disabled by age 10	-0.027* (0.015)	-0.025* (0.015)	-0.010* (0.006)	-0.009* (0.006)
Controlling for birth outcomes		X		X
Mean of dependent variable			0.070	
Number of observations			57,162	
Number of families			28,581	

Note: This table presents the results of difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Each panel has 12 regressions: four for the third child being observed disabled by age five; four for the third child being observed disabled by either age five or between ages five and ten; and four for the third child being observed disabled by age ten. Outcome variables combine math and reading assessments. Birth outcomes controls in columns (2) and (4) include infant birth weight, one and five minutes APGAR scores and clinical estimate of gestational age. Birth outcomes are not available in Denmark for birth cohorts 1990 to 1993. For these observation we impute zero in place of a birth outcome control and additionally include and indicator variable for missing value. Standard errors are adjusted for clustering at the family level. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table 4: Testing unconfoundedness: Birth outcomes

	(1)	(2)	(3)	(4)
	Birth weight	Ln(birth weight)	Low birth weight	Apgar 5 score
<i>Panel A. Florida: Birth outcomes</i>				
3rd observed disabled by age 5	-16.591 (16.328)	-0.004 (0.006)	0.003 (0.008)	0.018 (0.020)
Mean of dependent variable	3371	8.11	0.044	8.98
Number of observations		19,624		
<i>Panel B. Denmark: Birth outcomes</i>				
3rd observed disabled by age 5	-10.922 (13.339)	-0.004 (0.004)	0.002 (0.006)	-0.026 (0.029)
Mean of dependent variable	3539	8.160	0.027	9.859
Number of observations		51,940		

Note: This table presents the results of difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Standard errors are adjusted for clustering at the family level. The Danish observations are smaller than in previous tables because we only observe birth outcomes for children born from 1994 forward, and thus for Denmark this table includes birth cohorts 1994 to 2001. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table 5: Using health issues at the time of birth as an alternative to measured disability

	(1)	(2)	(3)	(4)
	Florida		Denmark	
3rd observed with birth problems	-0.045* (0.025)	-0.041* (0.025)	-0.037** (0.017)	-0.032* (0.017)
Controlling for birth outcomes		X		X
Mean of dependent variable		0.268		0.070
Number of observations		104,130		57,162
Number of families		9,812		28,581

Note: Difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Outcome variables combine math and reading assessments. Treatment is defined as following indicators recorded on birth certificate: congenital anomaly or abnormal condition at birth or birth weight below 2500 grams or gestational age below 37 weeks or five minutes Apgar score below 6. Standard errors clustered at the family level. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table 6: Heterogeneity by maternal education

	(1) College graduate	(2) Complete HS or with some college	(3) HS dropout (or basic school only)
<i>Panel A. Florida: Test scores</i>			
3rd observed disabled by age 5	-0.078* (0.045)	-0.021 (0.030)	-0.080 (0.054)
Mean of dependent variable	0.814	0.217	-0.257
Number of observations	25,680	57,256	21,194
Number of families	2,387	5,373	2,052
<i>Panel B. Denmark: 9th grade gpa</i>			
3rd observed disabled by age 5	0.030 (0.036)	-0.058** (0.027)	-0.031 (0.040)
Mean of dependent variable	0.484	0.121	-0.391
Number of observations	14,706	25,616	12,206
Number of families	7,353	12,808	6,103

Note: This table presents the results of difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Maternal education is measured at the time of first birth. Outcome variables combine math and reading assessments. Standard errors are adjusted for clustering at the family level. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table 7: Heterogeneity by parental status of the third child: Florida

	(1) Same father of all three children	(2) Different father of third child	(3) Absent father of third child
3rd observed disabled by age 5	-0.028 (0.024)	-0.175** (0.087)	-0.218* (0.112)
Mean of dependent variable	0.324	0.032	-0.351
Number of observations	91,020	7,902	5,208
Number of families	8,571	731	510

Note: This table presents the results of difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Outcome variables combine math and reading assessments. Standard errors are adjusted for clustering at the family level. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table 8: Heterogeneity by parental status of the third child: Denmark

	(1) Parents together when third child is 5 years old	(2) Parents not together when third child is 5 years old	(3) Father of the third child is different
3rd observed disabled by age 5	-0.044** (0.020)	0.043 (0.048)	0.003 (0.081)
Mean of dependent variable	0.120	-0.244	-0.308
Number of observations	49,460	7,510	3,648
Number of families	24,730	3,755	1,824

Note: This table presents the results of difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Outcome variables combine math and reading assessments. Standard errors clustered at the family level. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Appendix Tables

Table A1: Difference-in-differences: Unconfoundedness

	(1)	(2)	(3)
	More exposed (2nd)	Less exposed (1st)	Difference
<i>Panel A. Florida</i>			
<i>Birth weight</i>			
3rd observed disabled by age 5	3411 (531)	3338 (564)	73*** (21)
3rd not observed disabled by age 5	3417 (501)	3324 (518)	93*** (8)
Difference	-6 (15)	14 (15)	-20 (17)
<i>Five minutes Apgar score</i>			
3rd observed disabled by age 5	9.00 (0.42)	8.96 (0.54)	0.04** (0.02)
3rd not observed disabled by age 5	8.99 (0.47)	8.97 (0.55)	0.02*** (0.01)
Difference	0.01 (0.01)	-0.01 (0.02)	0.02 (0.02)
Number of observations		19,624	
<i>Panel B. Denmark:</i>			
<i>Birth weight</i>			
3rd observed disabled by age 5	3600 (544)	3440 (532)	160*** (18)
3rd not observed disabled by age 5	3625 (512)	3456 (510)	169*** (5)
Difference	-25** (13)	-16 (13)	-9 (13)
<i>Five minutes Apgar score</i>			
3rd observed disabled by age 5	9.87 (0.82)	9.84 (0.82)	0.03 (0.03)
3rd not observed disabled by age 5	9.89 (0.75)	9.83 (0.82)	0.06*** (0.01)
Difference	-0.02 (0.02)	0.01 (0.02)	-0.03 (0.02)
Number of observations		51,940	

Note: Difference-in-differences models with no covariates included. Sample means and standard deviations are reported in the first two rows of columns (1) and (2) for both Florida and Denmark. T-test differences with standard errors in the first two rows of column (3) and first two columns of the third row for both Florida and Denmark for each outcome. Differences-in-differences estimates are presented in the bottom right corner for each location and outcome. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table A2: Differences in relationships, by access to grandparents in Denmark

	(1) Living in the same municipality as grandmother	(2) Living in a different municipality than grandmother
3rd observed disabled by age 5	0.025 (0.033)	-0.046* (0.026)
Mean of dependent variable	0.003	0.227
Number of observations	17,034	28,288
Number of families	8,517	14,144

Note: Difference-in-differences models with family fixed effects and controls for year and month of birth and the child's gender. Outcome variables combine math and reading assessments. We define the grandmother as the mother of the child's mother. We only consider families where the grandmother is not dead at the time of birth of the third-born, and thus assign grandparental living status based on third birth. Standard errors clustered at the family level. Estimates marked *** are significant at 1%, ** at 5%, * at 10%.

Table A3: Utilization of welfare and health care systems

	(1)	(2)		(3)	(4)	(5)	(6)		(7)
	Employed	Leave benefits	Unemployment benefits	Sickness benefits	Inpatient admissions	Outpatient admissions	Health care utilization (child)		ER admissions
3rd observed disabled by age 5	0.724 (0.447)	29351 (44565)	42544 (76641)	20390 (57585)	3.691 (4.566)	2.791 (2.731)			1.069 (1.494)
Observations	1,862		1,697			1,882			
3rd not observed disabled by age 5	0.753 (0.431)	33504 (47057)	42657 (78621)	15589 (48686)	1.556 (1.964)	0.472 (0.995)			0.776 (1.166)
Observations	25,996		23,705			26,699			
Difference (t-test)	-0.029*** (0.010)	-4153*** (1178)	-113 (1972)	4801*** (1240)	2.135*** (0.053)	2.319*** (0.028)			0.293*** (0.028)
Observations	27,858		25,402			28,581			

Note: This table presents utilization of welfare system by mothers and health care system by a third-born child in Denmark. Unit of analysis is a household. For all outcomes it shows means and standard deviations, measured for third birth, for families where the third child is disabled by age 5 and for families where the third child is not disabled by age 5. The last row presents difference in means and standard errors based on t-test. The outcome variables are: probability that mothers is employed full-time when third child is 5 years old (column 1), amount of leave benefits received (column 2), amount of unemployment benefits received (column 3), amount of sickness benefits received (column 4), number of inpatient admissions (column 5), number of outpatient admissions (column 6) and number of emergency room admissions (column 6). Columns (2) to (4) present cumulative amounts from one to five years after the birth of the third child in DKK. Leave benefits in column (2) include child care leave (one additional year on top of maternity leave), education leave (time off to improve one's education) and sabbatical (unrestricted one year off work). Columns (5) to (7) present count of the events by age five. Differences marked *** are significant at 1%, ** at 5%, * at 10%