Family Matters: Essays on the Economics of Childhood

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[*]Luckily, for me, this works well with the H for Helmut too.

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Preface

Childhood is a formative period that shapes our identity, social relationships, and future labor market outcomes. Experiences during this time play a crucial role in influencing who we become and the paths we take in life. A key consensus from both research and policy is that *family matters.*

Family background has a profound impact on children's future earnings and well-being. In OECD countries, individuals who experience disadvantaged childhoods earn approximately 20 percent less per year and report worse health (Clarke et al. 2022). This effect can operate through multiple channels, such as the neighborhoods children grow up in (Chetty et al. 2016), the schools they attend (Björklund and Salvanes 2011), and the time and resources parents can invest in their human capital (Becker and Tomes 1979; Mogstad and Torsvik 2023). Beyond financial and educational resources, parents also transmit cultural ideals and values, further influencing children's development (Fernández and Fogli 2009; Tungodden and Willén 2023). Even maternal life events during pregnancy, such as stress or trauma, have been shown to have lasting effects on children after birth (Aizer et al. 2016; Currie et al. 2022).

Family matters—but families are also changing. One key shift is driven by migration, with children growing up at the intersection of their parents' heritage and the society in which they are raised. These families have been central to policy debates in Europe and the U.S., as the academic underachievement of migrant children is often linked to their parents' background characteristics and level of integration (Dustmann and Glitz 2011). Another major shift involves changing cohabitation patterns, with more children growing up in households affected by parental separation or divorce. This significantly affects parental roles and the course of childhood after divorce (Amato 2010).

In this dissertation, I examine how policies and life events influence these new types of families and, in turn, impact children's educational trajectories and well-being. It consists of three stand-alone chapters that can be read independently. The following provides a brief summary of each.

Chapter 1—*In the Shadow of Brothers*—examines how family, cultural background, and education interact to shape children's academic performance, with a particular focus on second-generation migrant girls. The analysis reveals that cultural son-preference within the family significantly influences girls' academic success.

This study takes a new approach by exploring family influences through a widely implemented education policy: school entry rules that set a cutoff date, causing children to start school at different ages. Entering school later generally provides a "maturity advantage" that boosts academic success and can also positively influence younger siblings through a "role-model effect" (Bedard and Dhuey 2006; Fredriksson and Öckert 2014; Karbownik and Özek 2021). However, these policies also affect the time siblings spend together before school, a period when parental investments are crucial. If traditional gender norms influence these investments, both the maturity advantage and role-model effects may differ for migrant girls depending on whether they have a brother.

I analyze this policy in Sweden using high-quality administrative data, which allows me to examine its effects on siblings in both migrant and native families. In Sweden, children enter public education the year they turn seven, creating a natural cutoff at January 1^{st}. I exploit this cutoff in a Regression Discontinuity Design (RDD) to establish causality.

Initial analysis confirms that late school entry improves the end-of-compulsory school grades for children in Sweden on average. However, my main analysis reveals that these average effects mask substantial heterogeneities when taking family structure into account. First, I examine how late school entry affects first-born migrant girls', comparing those with a younger brother versus a younger sister. Second, I analyze sibling spillovers by studying how having an older sibling who enters school late affects younger sisters in migrant families. I compare these effects with those for boys and native children.

The main analysis presents new and important findings into how family structure influences the effects of late school entry on second-generation migrant girls. First, the maturity advantage of late school entry exists for second-generation migrant girls only if they have a sister—having a brother offsets this benefit. Second, I find negative spillover effects on the grades of second-generation migrant girls with an older brother who entered school late. This finding sharply contrasts with the positive role-model effect commonly reported in the literature.

These findings are unique to second-generation migrant girls. For second-generation migrant boys and native children, the effects align with previous studies. A significant part of the chapter is devoted to exploring the underlying mechanisms. I present evidence that gender bias in parental investments within migrant families plays a key role in driving the effects.

Chapter 2—*Life After Divorce: Effects of Joint Custody on Parents and Children*—is based on joint work, examining the causal effects of joint versus sole custody on children and parents. While many Western countries have shifted toward promoting joint custody, evidence on its impact remains scarce. To the best of our knowledge, we are the first to establish causality in this context. We achieve this by leveraging a sample of families who experienced custody disputes in Swedish district courts. We exploit that cases were randomly assigned to judges with varying tendencies to grant joint custody. Our findings show that joint custody benefits children and fathers but has no significant impact on mothers' outcomes.

Parents play a crucial role in children's lives, providing support and resources that shape their social and academic development. Today, many children live with separated or divorced parents, making custody arrangements— which determine where a child lives and parents' involvement—especially important. Historically, courts favored mothers for sole custody. In recent decades, however, there has been a policy shift toward joint custody, giving both parents decision-making power over their children's education and well-being. This raises a key question: Do joint custody arrangements lead to better outcomes for families?

Despite its policy relevance, causal evidence remains scarce, primarily due to selection bias. More cooperative and advantaged parents are more likely to choose joint custody, potentially confounding its effects (Bauserman 2012). Additionally, studying custody arrangements is challenging, as datasets may record divorces but lack details on post-divorce custody decisions.

We address these challenges by analyzing joint custody rulings in Swedish custody disputes. A key contribution is our extensive collection of custody rulings, covering nearly all cases from 1992 to 2021, from which we construct a dataset linking court-ordered decisions with rich administrative data. Our empirical strategy exploits the random assignment of cases to judges who vary in their propensity to assign joint custody. We use this variation to implement an instrumental variables (IV) approach. We validate the random assignment by showing that while case characteristics strongly correlate with custody rulings, they do not predict a judge's propensity to award joint custody. However, judge propensity significantly influences case outcomes.

We reveal compelling evidence of the benefits of joint custody. Children's educational outcomes improve significantly, while their mental health is unaffected. Yet the most pronounced effects are for fathers: Joint custody improves earnings and reduces reliance on antidepressants and anti-anxiety medication. Notably, in most cases, a sole custody ruling results in fathers losing access to their child. In contrast, joint custody has no significant impact on mothers' earnings or mental health. These results suggest that joint custody benefits fathers by keeping them actively involved in their child's life, whereas for mothers, custody type has little effect on their economic situation or well-being.

Chapter 3—*Linguistic Distance and the Gender Gap in Education*—examines how gender differences in language acquisition shape educational outcomes among migrant children. While the academic underperformance of migrant children relative to natives is widely discussed, migrant girls often surpass migrant boys and, in many cases, even native boys. The reasons behind this pattern remain largely unexplored. Existing research has primarily linked the migrant-gender gap in education to cultural proximity in gender norms, highlighting the disadvantages faced by girls from more traditional backgrounds (Fernández and Fogli 2009; Bergvall 2022). However, this does not explain why, on average, migrant girls perform remarkably well. This chapter highlights an overlooked factor: girls' advantage in early language learning.

This study is the first to examine how linguistic distance between a migrant's origin and host country shapes the gender gap in academic performance. Little is known about how linguistic distance affects migrant children's education. If linguistic distance mainly reflects cultural proximity, girls from culturally distant origins may face greater disadvantages, as suggested by previous research. But if it captures language learning difficulty, they could outperform boys, since studies show girls acquire language more easily early in life (Van Der Slik et al. 2015; Rinaldi et al. 2023). As a result, they may find it easier to learn a second language, even when their first language is very different from the host-country language.

To investigate this, I use an epidemiological approach to analyze how linguistic distance affects the migrant-gender gap in education. I leverage data from the Programme for International Student Assessment (PISA) test, which measures 15-year-olds' cognitive skills worldwide. The dataset's detailed information on students' migration histories enables a comprehensive analysis, incorporating country-by-origin fixed effects.

I present novel evidence that first-generation migrant girls acquire a second language more easily than boys, improving their academic performance in both reading and math. However, linguistic distance does not significantly influence the gender gap for second-generation migrants, likely because they are exposed to the host-country language from birth. In contrast, first-generation children must learn the language later, often after starting school. Girls appear to have an advantage when second-language acquisition requires active learning during childhood.

To further explore this, I replicate the analysis using a gender inequality measure for host and origin countries, a common approach in the literature on cultural proximity. The findings indicate that while both linguistic distance and cultural gender norms reflect cultural proximity, they affect the academic gender gap differently across migrant generations. Linguistic distance, in particular, captures girls' advantage in language learning after arrival in the host country. These insights have important policy implications for the role of language acquisition in shaping migrant children's educational outcomes.

Chapter 1

In the Shadow of Brothers*

Unintended Impacts of a School Entry Policy on Migrant Girls

Abstract: Gender bias in parental investments can render otherwise beneficial educational policies ineffective for girls or even lead to unintended negative consequences. In light of this, I examine how Sweden's school entry policy interacts with family structure to shape the educational outcomes of second-generation migrant girls. Using a regression discontinuity design on high-quality administrative data, I first assess the direct effects of late school entry, showing that it benefits migrant girls with younger sisters but not those with younger brothers. Furthermore, by investigating sibling spillover effects from an older sibling's late school entry, I demonstrate that spending more time at home with an older brother who enters school late has a strong negative effect on the educational outcomes of younger sisters. I propose a simple theory to explain these results, highlighting gender bias in parental preferences as a key factor. Supporting this interpretation, I present evidence showing that these negative impacts are specific to migrant girls, with neither migrant boys nor native children experiencing similar effects. Moreover, the effects are more pronounced in migrant families with traditional backgrounds and are also reflected in mothers' labor supply decisions when sons, rather than daughters, enter school late.

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1.1 Introduction

Understanding how children develop human capital is crucial, as it significantly influences intergenerational mobility, inequality, and economic growth. This process depends on two key factors: how parents invest time and resources in their children, and how these investments interact with education policy (Becker 1981). Minority groups, such as children from migrant backgrounds, may face unique challenges because education systems are often designed around the parenting and investment norms of majority groups (Dustmann and Glitz 2011). When minorities have different parental investment norms, educational policies that work well for majority-group children may be less effective—or may even have unintended consequences—for children from minority backgrounds.

A primary concern in this context is the influx of migrant families from gender-traditional origins to more gender-egalitarian Western societies. Research indicates that migrants from traditional cultures tend to uphold these norms in the host country (Blau et al. 2013; Giavazzi et al. 2019). Migrant parents' cultural origin can therefore play a significant role in determining the outcomes of their children, particularly for girls. Sons are often perceived as benefiting more from education in the labor market due to household specialization in their future families (Almond et al. 2013; Anukriti et al. 2022). Meanwhile, daughters might be viewed as gaining less from education, especially in cultures that prioritize early marriage or domestic roles for women (Fernández and Fogli 2009; Dahl et al. 2022). With finite resources and a desire to maximize children's future earnings, parents may invest more in their sons' education than in their daughters' (Butcher and Case 1994; Mitrut and Wolff 2014).

The differential treatment of sons and daughters within migrant families could have implications for how educational policies impact migrant girls. Parents are key mediators between education policies and children's learning, providing supplemental time investments and resources that help unpack the benefits of such policies (Currie and Almond 2011; Greaves et al. 2023). From a theoretical perspective, when education policies and parental investments are complements in a child's skill production, gender bias in parental human capital investments can lead to two challenges. First, if a migrant girl is the focus of a policy, its effectiveness might be compromised by the presence of brothers because they may limit the parental support she receives. Second, if a brother is the focus of a policy, a negative spillover effect could occur, with parents diverting resources from the girl to her brother.

Against this backdrop, I examine how education policy and family structure interact to shape migrant girls' educational outcomes. Policies governing school entry age provide an interesting testing ground to explore this issue. Studies show that starting school at an older age usually provides a "maturity advantage" that boosts children's academic success (e.g. Bedard and Dhuey 2006; Dhuey et al. 2019; Cook and Kang 2020). This benefit can spill over to younger siblings, improving their educational outcomes as well due to a "role-model effect" (Karbownik and Özek 2021). However, these rules introduce exogenous variation not only in a child's school-starting age but also in the time siblings spend together at home before school entry. During this time parental investments play a significant role in children's human capital development. When traditional gender norms promote bias in parental investments, both the maturity advantage and the role-model spillover effect could play out differently for migrant girls depending on whether they grow up with a brother or not.

For identification, I leverage Sweden's school entry policy in a regression discontinuity design (RDD). In Sweden, children enter public education in the year the turn seven, creating a natural cutoff date of January 1^{st}. This means that children born in late December and early January enter school one year apart. All education systems have school-entry cutoff rules, but how strict the rules are is at the hand of policy makers, with some countries allowing more flexibility than others (Puhani and Weber 2007; Oosterbeek et al. 2021). In Sweden, however, the rule allows minimal discretion for parents to delay or advance their child's school entry, and nearly all children start on time. This lends credibility to the regression discontinuity design. It does also, however, offer little flexibility to families where children might benefit from entering a year early or late.

I perform two sets of analyses to explore how family structure influences the impact of Sweden's school entry policy on girls in migrant families, where cultural norms may promote parental gender bias. First, I examine the direct effect of entering school late on first-born girls' school performance and how this varies by the gender of their younger sibling. Specifically, I compare school outcomes of first-born girls who are either born before or after the cutoff and who have a second-born sister or brother at home. Second, I analyze sibling spillovers by assessing how having an older brother or sister who enters school late affects younger sisters' school performance. Here I compare school outcomes of migrant girls whose older brother or sister is born either before or after the cutoff. I then contrast these effects by replicating the analysis for boys in migrant families and for native children.

The data is drawn from Swedish administrative school, tax, and population records. A key advantage of using this data is its capacity to link all siblings and families in Sweden, while also identifying their migrant backgrounds through detailed information on parental origins. The main sample consists of all second-generation migrant siblings born in Sweden to two non-Nordic parents between 1988–2004.[1] The school outcomes measuring student performance include GPA, Swedish and math grade when the student

[1] Including only siblings born in Sweden is crucial for the analysis, as it ensures accurate birthdates and allows me to test for any birth-manipulation around the cutoff. Including first-generation migrants would skew the sample, leading to an over-representation of January 1^{st} birthdates, often assigned to undocumented migrants in Sweden.

leaves compulsory school outcomes, at 15/16 years old. I also consider whether the student was enrolled in the regular Swedish track as opposed to the "Swedish as a Second Language" track at the start of their final year of compulsory school.

I start off the analysis by first establishing a benchmark for how the policy impacts native and second-generation migrant girls and boys in general, without taking family structure into account. In line with previous literature, I find that entering school late has positive impacts on students' school performance relative to early starters. Estimating the sibling spillovers when the oldest enters school late without taking the gender composition of siblings into account reveals that there are no significant spillovers on the younger sibling's school outcomes.

Two key findings of this study reveal that these average effects mask substantial heterogeneities once taking family structure into account. The first key finding is that the maturity advantage of late school entry is completely offset for second-generation migrant girls if they remain at home for longer with a younger brother. However, the effect remains positive and statistically significant for first-born girls with a younger sister. These differing effects are observed only for second-generation migrant girls. For second-generation migrant boys the effect of entering school late on their school performance remains unchanged, regardless of whether they stay home with a sister or brother.

The second key finding is that spending extended time at home with an oldest brother who enters school late has lasting negative spillover effects on second-generation migrant girls' human capital formation. If a girl has an oldest brother who enters school late, her grade-point average at the end of compulsory schooling decreases by 17 percent of a standard deviation. Conversely, additional time spent at home with an oldest sister who enters school late has slightly positive effects on a younger sister's academic performance. Second-generation migrant boys' school outcomes, on the other hand, remain unaffected when the oldest sibling enters school late, regardless of their sibling's gender.

To rationalize these findings, I propose a simple household maximization model in which parents invest in their children's human capital. A crucial aspect of this model is that it accommodates biased parental preferences, meaning parents may prioritize the human capital of one child over another. I utilize the model to examine how parents adjust their investments in their children when one sibling enters school late due to being born after the cutoff. Comparative statics indicate that my main empirical findings—i.e., both the direct and sibling spillover effects of late school entry—can be accounted for by either biases in parental preferences or technological differences in children's human capital production functions.

An ideal test to rule out the technology-based explanation would consist of (i) identifying a group of families whose human capital production processes are identical to those of migrant families but without gender-biased preferences, and (ii) showing that girls in these families do not experience any unintended consequences from late school entry. To approximate this ideal, I re-estimate my regression models for native families with socioeconomic backgrounds similar to those of migrant families. The results show that in these families, both boys and girls benefit equally from late school entry, regardless of their siblings' gender, and no significant spillover effects are observed for either gender. To the extent that native families with socioeconomic backgrounds akin to those of migrant families likely face similar human capital production technologies, these results suggest that gender biases in parental preferences are driving my findings.

Two additional tests support this interpretation. First, I test for effect heterogeneity by the Gender-Inequality Index (GII) of the parent's origin country.[2] The effects of remaining at home with a brother due to the cutoff may differ for girls in families that come from more or less gender-traditional cultures. Second, I estimate the impact of having a child born after the cutoff on native or migrant mothers' earnings and employment at the end of the year the child turns seven.[3] The gender differences in effects of late school entry on second-generation migrant children suggest that migrant mothers may respond differently based on whether their sons or daughters enter school late.

The first test shows that remaining at home with a brother has more significant negative influences on girls' school outcomes in families from more gender-traditional backgrounds. This suggests that parents who originate from these countries are less likely to prioritize investments to daughters and sons equally, and more likely to divert investments from a girl to a brother as long as both remain at home.

The second test reveals that late school entry affects the earnings and employment of migrant and native mothers differently. For native mothers, late school entry of a child, regardless of gender, leads to significant positive impacts on labor market outcomes. In contrast, migrant mothers are significantly more likely to work full-time and earn higher wages when their daughters start school late, whereas having a son enter school late has a smaller impact and does not result in higher wages. Similar to how girls' school outcomes in migrant families are uniquely influenced by having a brother who starts school late, their mothers' labor market outcomes are also impacted. This further suggests that gender-biased preferences may shape outcomes within migrant families.

This study makes a novel contribution to the growing literature on sibling spillovers. In recent years economists have taken an interest in the influence that siblings have on each other. Policies which improve one sibling's school performance generally have positive spillovers on younger siblings' skill formation (Qureshi 2018; Nicoletti and Rabe 2019; Karbownik and Özek 2021; Figlio et al. 2023; Zang et al. 2023). Older siblings' educational decisions such as major choice largely influence their younger siblings' choices (Altmejd et al. 2021).[4] Recent papers have also focused on the heterogeneous spillover

[2] GII is provided by the United Nations Development Program (UNDP) and is a composite metric based on nationwide measures of reproductive health, female empowerment and the labor market.

[3] At this time, all children are in school, regardless of birthdate in relation to the cutoff date.

[4] Sibling health and school performance are also positively correlated, where negative shocks to

effects by the gender composition of siblings. Growing up in families that exhibit son preference reduces girls' performance in math (Dossi et al. 2021), and growing up with a twin-brother reduces girls' self-assessed math ability (Adamecz-Völgyi et al. 2023). I demonstrate that a positive educational shock to one child's human capital formation actually can negatively impact their siblings' human capital when parents likely hold biased preferences among their children. Migrant families provide an ideal study group, as siblings raised together in the host country may still experience parental investments influenced by gender norms from their country of origin.

This study ties to the literature on the role of cultural background on girls and women with migrant backgrounds. Most research has focused on labor market outcomes and fertility decisions in adulthood (Fernández and Fogli 2009; Blau et al. 2013; Kleven 2022), but there is also evidence that it impacts the gender gap in school performance (Nollenberger et al. 2016). Bergvall (2022) investigates the interaction between cultural norms and neighborhood for migrant siblings in Sweden, and finds that the sibling gender gap in math widens with stronger cultural gender norms. Other studies have looked at how policy changes and religious beliefs influence migrant parents' investments in their daughters (Mitrut and Wolff 2014; Dahl et al. 2022). Evidence suggests that migrant girls may benefit more than boys from early external investments, such as access to daycare (Drange and Telle 2015; Corazzini et al. 2021). Additionally, research shows that there is son preference in fertility decisions among migrants in North America (Almond and Edlund 2008; Almond et al. 2013; Blau et al. 2020) and Sweden (Mussino et al. 2019). The literature has mostly studied the direct effects of culture on female migrants' outcomes. My study contributes with novel evidence on the indirect effects of culture on migrant girls during childhood—a critical period when positive support is much needed. These indirect effects have enduring consequences for their human capital development and future labor market opportunities.

This study also extends the school-starting-age literature. Studies focusing on native children have found positive effects of being older at school start both in the short-run (e.g. Bedard and Dhuey 2006; Mühlenweg and Puhani 2010; Dhuey et al. 2019) and to some extent in the long-run (Angrist and Krueger 1991; Black et al. 2011; Fredriksson and Öckert 2014). Papers studying heterogeneous effects have found mixed results for the effects by gender (Datar 2006; McEwan and Shapiro 2008; Puhani and Weber 2007; Cook and Kang 2020), socioeconomic status (Elder and Lubotsky 2009; Fredriksson and Öckert 2014; Suziedelyte and Zhu 2015), and ethnicity (Leuven et al. 2010; Cook and Kang 2020). I contribute to this literature by studying the effects of late school entry in migrant families using detailed Swedish administrative data. This allows me to

one child negatively impact siblings' school performance (Fletcher et al. 2012; Black et al. 2021) while positive health shocks and medical treatment has positive spillovers on siblings (Bharadwaj et al. 2018; Daysal et al. 2022).

define migrant families by the parents' country of origin rather than by using a proxy, for example a dummy for whether a student speaks another language at home.

The remainder of this chapter is structured as follows: Section 1.2 provides insight into the institutional setting. Section 1.3 and 1.4 describes data and method. Section 1.5 presents the results. Section 1.6 presents a conceptual framework and several empirical tests of the mechanisms. Section 1.7 offers concluding remarks.

1.2 Institutional Setting

Sweden has a long tradition of hosting migrants. The number of children with migrant backgrounds has increased steadily over time, reaching around 24 percent in 2018 (Statistics Sweden 2020). These children are more likely to grow up in low income households with parents who have at most a secondary-level education.

Education is compulsory for nine years and publicly funded. Students attend their local public school or can opt out to a voucher-funded independent school, or a public school in a different neighborhood. After graduating from 9th grade, almost all students move on to upper-secondary education which includes academically oriented programs, vocational programs and preparatory programs for students ineligible for the academic and vocational programs.

Children in Sweden should start first grade in the year the turn seven.[5] Although school officials could allow children to start one year earlier or later at the request from parents, the cutoff in school admission is generally strictly followed. Among school starters in the 2002–2004 birth cohorts, 99 percent of both natives and migrant students started school on time, in the year they turned seven.[6] It is also uncommon that children face retention during school – there is no formal criteria for 'passing' a grade and to move up to the next year group. However, since 1998 there are formal eligibility criteria for upper-secondary education, consisting of a lower performance threshold (passing mathematics, Swedish and English).[7] Students who do not meet the eligibility criteria typically do not re-take year nine—instead they participate in a preparatory upper-secondary program. However, it is not impossible to skip or retake a grade in the Swedish system, nor is it impossible to delay or anticipate school start.

Compulsory school is comprehensive and all students follow the same curriculum. The only tracking is for migrant students who are deemed not able to follow the standard Swedish curriculum. They are offered an alternative course plan in the Swedish subject

[5] A year of "pre-school class" has been offered to six-year-olds since 1998. The children often attend the pre-school class in the same building as the elementary school children, though it is considered its own school form. Between 1998-2018 the pre-school class was not mandatory. In 2018 the pre-school class became mandatory and thus extended compulsory schooling to ten years.

[6] I report numbers for these specific cohorts since I can observe their first-grade enrollment in the dataset 'Elevregistret'.

[7] Stricter criteria introduced in 2011.

("Swedish as a second language"), which is adapted to their level. Students who follow the alternative course plan are graded according to the criteria of the "second language" course track, but these students take the same centralized exams as the students on the standard course track.

1.3 Data

The study uses administrative data from Sweden, including school records for all children matched with population and employment data from several registers covering the population.[8] I link all siblings by their parents' unique ID numbers in the data and define their migrant status based on detailed information on the family's migration background.

The main sample consists of all children born in Sweden to two foreign-born non-Nordic parents in 1988–2003. I observe their school outcomes in the year they graduated from compulsory school, at age 15/16, between 2004–2019. This amounts to 148,257 second-generation migrant students.[9] For analysis I include those born within a two month-window around the January 1^{st} cutoff, which includes 48,031 second-generation migrant students.

I only include families where all siblings were born in Sweden. This ensures that I observe all children's exact date of birth. This is important since it allows me to test the validity of the empirical strategy. Furthermore, there is an over-subscription of first-generation migrants born on January 1^{st} since it is commonly registered as the birthdate for those who arrive in Sweden without proper documentation. I also exclude all children of Nordic migrants due to the close proximity in language, social institutions and culture among the Nordic countries.[10]

The main outcomes include the students' final grade point average (GPA), and their Swedish and math grades at the end of compulsory school.[11] The final GPA is particularly important as it determines the upper-secondary school track the student can enter. While the Swedish grade reflects language proficiency, the math grade captures cognitive skills that depend less on language. All grade outcomes are standardized to have a mean of zero and a standard deviation of one.[12] As an additional outcome, I consider whether the student was enrolled in the regular Swedish track or the "Swedish as a Second Language"

[8] This includes the 'Multiple Generations' population register, education records for the students' final year of compulsory school including final grades, and the longitudinal 'LOUISE' database on education, income and occupation.

[9] Compared to 425,398 native students.

[10] The Nordic countries include Denmark, Norway and Finland. Among the excluded observations, the vast majority of children have parents who migrated from Finland. Although Finnish and Swedish have very different language structures, Finland has a minority of Swedish-Finns, making it likely that migrants from Finland have close ties to Sweden.

[11] The grades are set by the students' own teachers but are in part based on standardized testing.

[12] Outcomes for students enrolled in secondary Swedish subject are standardized within its own distribution.

track at the start of their final year of compulsory school.

The data contains detailed information on parents' background. Importantly, I observe what country group the parents migrated from and what year they arrived in Sweden.[13] The data also includes detailed information on parents' education and earnings measured after the child is born (and birthdate is realized), but before the child enters school.[14] These measures are mainly used for balance tests to validate the empirical strategy.

1.3.1 Summary Statistics

This section provides summary statistics which compares (i) outcomes and family characteristics for second-generation migrant girls and boys, (ii) indicators of socioeconomic status in migrant and native families, and (iii) fertility patterns among migrants and natives.

Second-generation migrant girls and boys. Table 1.1 presents summary statistics for the full sample of second-generation migrant girls and boys. Panel A compares mean values of girls and boys' education. Girls' mean GPA, math and Swedish grades at the end of compulsory school are higher than boys'. The t-test statistics provided in column (5) show that these differences are significant on a 1-percent level. Girls are also significantly more likely to be enrolled in the regular Swedish track at the beginning of 9th grade. This also makes them more likely to matriculate in to upper-secondary education.[15] Around 75 percent of second-generation migrant children attend a public school in their final year of compulsory schooling. Migrant boys are significantly more likely to be enrolled in an independent school in their final year of compulsory education.

Panel B in Table 1.1 shows summary statistics for second-generation migrant girls and boys' family characteristics. The only significant difference in parents' backgrounds is the father's average income when the child is of daycare age, a difference of 1,723 SEK, around 170 EUR, per year.

Migrant and natives' socioeconomic status. Figure 1.1 provides descriptive statistics of differences in migrant and native families' socioeconomic backgrounds. Graph (a) provides a density plot which shows migrant and native families' average annual incomes when the child is of daycare age. Migrant families have markedly lower annual incomes

[13] Country groups are predefined in the data and consists of countries with close geographical and cultural identities. All country groups are listed in Appendix Table A1.

[14] Ideally, parental background should be measured before the child's birth, but the parental background data for the oldest cohorts in my sample is limited and prevents me from constructing time-consistent measures of pre-birth socioeconomic status. Mother's and father's years of schooling is measured when the child is 5 years old, and parental earnings are the mean of labor earnings when the child is aged 5–7. I percentile rank parental earnings within the birth cohort of the child to facilitate comparisons of status across birth years.

[15] The fact that second-generation migrant girls do better in school than boys is in line with findings for all Nordic countries (Broström and Jansson 2022).

Table 1.1: Summary Statistics for Second-Generation Migrant Children

	Boys		Girls		
	(1) mean	(2) sd	(3) mean	(4) sd	(5) **t-test**
A. School outcomes					
Standardized GPA	-0.320	1.050	0.020	1.042	-0.339***
Standardized Swedish grade	-0.447	0.907	0.009	0.985	-0.456***
Standardized math grade	-0.234	0.978	-0.202	0.979	-0.032***
Enrolled standard track (Swedish)	0.647	0.478	0.700	0.458	-0.052***
Enrolled public school final year	0.784	0.411	0.766	0.423	0.018***
Enrolled any upper-secondary	0.863	0.344	0.868	0.339	-0.004**
Enrolled academic upper-secondary	0.701	0.458	0.748	0.434	-0.047***
B. Family characteristics					
Mother's average annual income age 3-5	72,779	91,058	72,957	90,955	-177
Father's average annual income age 3-5	140,214	150,090	141,938	157,431	-1723*
Mother's years of education age 3-5	10.872	2.655	10.881	2.685	-0.008
Father's years of education age 3-5	11.281	2.652	11.309	2.671	-0.028
Age of mother at first birth	25.887	5.132	25.941	5.167	-0.055
Mother time in Swe before birth	5.920	5.747	5.949	5.736	-.029
Living in low-share migrant neighborhood age 3	0.507	0.500	0.505	0.500	.001

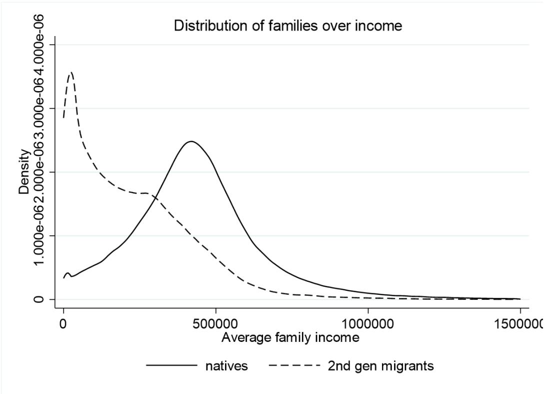
Notes: This table provides summary statistics for girls and boys born in Sweden between 1988–2003 to two non-Nordic migrant parents. The sample includes those within the bandwidth of estimation, which are those born between November-February each years. School outcomes are measured when children ended compulsory-school between 2004–2019. All grade outcomes have been standardized to have a mean of zero with a standard deviation of one. Family characteristics are measured when children are age 3–5 or at birth. Column (5) provides two-sample t-tests with equal variances where *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

than native families. Graph (b) provides a possible channel for this by showing that native mothers earn significantly more than migrant mothers when the child is of daycare age. In fact, the large peak at zero for migrant mothers indicates that many work very little or not at all before the child enters school. Lastly, Graph (c) shows that the vast majority of migrant mothers comes from countries with low to medium human development, measured by the Human Development Index (HDI).[16] These includes all African countries, and countries in the Middle East such as Iran, Iraq and Turkey. Children with mothers from countries with "high" and "very high" HDI make up a minority, though this group includes larger migrant-origin countries such as Chile, Poland, and countries from former Yugoslavia.

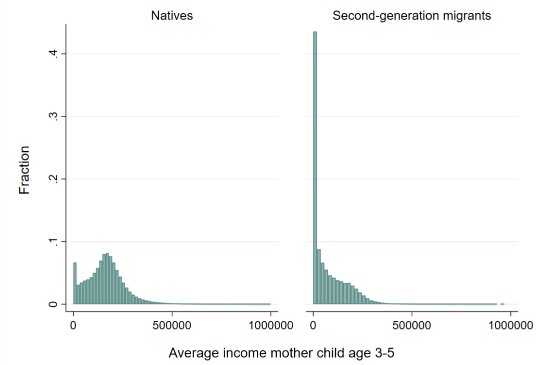
The fact that second-generation migrant children grow up in families with low socioeconomic status and where mothers work very little has two major implications. First, it reduces the likelihood that they grow up with a mother who is integrated into the labor force and society as a whole. Second, since daycare access hinges on parental employment in many Swedish municipalities, it also increases the likelihood that the child spends a significant amount of time at home before school start. In my data however I cannot observe whether children actually attended daycare before entering public education.

[16] Provided by the UNDP. The HDI classifications "low"–"very high" are defined by the UNDP. Appendix Table A1 provides more information on the classifications of all country groups in the sample.

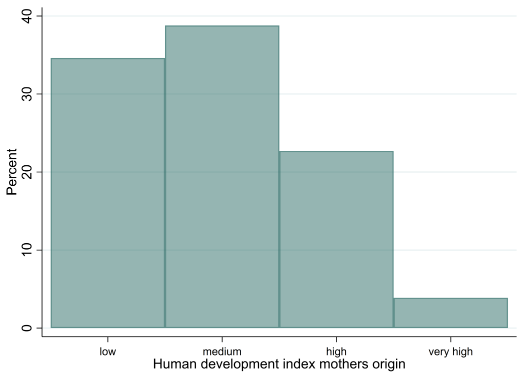
Figure 1.1: Migrant and Native Families' Socioeconomic Backgrounds



(a) Density annual family incomes



(b) Differences in mother earnings



(c) Human development of mother's origin

Notes: Graph (a) shows the average annual family incomes for migrant and native families when child is age 3–5. Migrant families include those with children born in Sweden to parents born in non-Nordic countries. Graph (b) shows the fraction of mothers with average incomes between 0–1,000,000 SEK for native and second-generation migrant children. Incomes are calculated by taking the average annual earnings of the mother when the child is of daycare age (3–5 years old). Graph (c) shows the sample of second-generation migrant students divided by the human development index (HDI) of their mother's origin. HDI is the average over the 1988–2003 sample birth-period divided into UNDP categories: low < 0.550, medium 0.550–0.699, high 0.700–0.799, very high ≤ 0.800. For reference Sweden's average HDI was 0.900 during the sample period.

Fertility and son-preference. Migrant and native families can exhibit different fertility patterns (Mussino et al. 2019). Appendix Figure A1 shows patterns of son preference in migrant families that is not found in native families. Graph (a) plots the confidence intervals for the average number of siblings of first-born girls and boys in native migrant families. First-born girls have almost one (0.8) more sibling than first-born boys. The fact that the confidence intervals are not overlapping indicate that this difference is statistically significant. There is no evidence of son preference in native families. On the contrary, first-born boys are significantly more likely to have more siblings than first-born girls, though the difference in absolute numbers is small. Graph (b) plots the confidence intervals for the average number of years between the births of the first and second-born child, so-called birth spacing. If migrant parents have a daughter first, they have their second child 7 months earlier on average than if they would have a first-born son. In native families there is no significant difference in birth spacing for families with first-born sons or daughters.

In light of this, I drop observations where the birth spacing is more than 6 years between the first and second born sibling when estimating the effects of late entry on the oldest's grade outcomes. In these families, the oldest will already have received treatment before the next sibling is born.

1.4 Method

1.4.1 Empirical Strategy

The aim of this study is to evaluate how family structure can influence the impacts of late school entry for girls in migrant families where parents may exhibit gender bias. All children start school in the year they turn seven, which means that January 1^{st} is the cutoff in school admission and that those born in the early spring will be older at school entry than those born a few months before, in the late fall.

Although most children adhere to the rule, some children may be allowed to start school early based on their anticipated academic ability and school readiness, while other children may be held back one year. Due to this potential endogeneity issue I use "expected" school starting age given by date of birth.

Following previous literature, I employ a regression discontinuity design (RDD) which considers children born just around the cutoff (Black et al. 2011; Fredriksson and Öckert 2014; Karbownik and Özek 2021). Their birth year and school starting age is as good as random, under the assumption that parents cannot exactly plan the conception and birthdate of their child.

The first part of the analysis measures the direct effect of late school entry by comparing outcomes of migrant girls born before or after the cutoff. Estimating the following reduced-form regression for the direct effects on first-born sibling i:

$$y_{ic} = \alpha + \beta_1 Late_i + \beta_2 f(r_i) + \beta_3 (Late_i \times r_i) + \beta_4 NS_i + \theta_c + \epsilon_{it} \tag{1.1}$$

where y_{ic} refers to the school outcomes of first-born child i in birth cohort c measured at the end of compulsory school, at age 15/16. The treatment variable *Late* is a dummy for whether the first-born has a birthdate just after the cutoff, thus expected to start school one year older, and β_1 is the coefficient of interest.[17] $f(r_i)$ represents a linear control function in the baseline analysis, where r_i is the running variable, the child's birthdate, normalized around the January 1^{st} cutoff. θ_c is a birth-cohort fixed effect. The baseline

[17] The school-entry age literature identifies three key age effects: absolute age at school start, relative age among classmates, and age at test day. While some studies decompose these effects (Black et al. 2011; Fredriksson and Öckert 2014; Cascio and Schanzenbach 2016), this chapter focuses on policy effects on migrant girls in different family structures.

specification includes a bandwidth of two months around the cutoff and the birth cohort is defined within this window. NS_i is a control variable for the number of siblings. This linear estimation includes a triangular kernel for weighting observations inside the window around the cutoff.

The second part of the analysis measures the spillover effects of the policy, by comparing the outcomes of migrant girls whose oldest sibling is born either before or after the cutoff. This can be evaluated by regressing a dummy for whether oldest sibling i enters school late on younger sibling *j's* grade outcomes:

$$y_{jc} = \alpha + \beta_1 Late_i + \beta_2 f(r_i) + \beta_3 (Late_i \times r_i) + \beta_4 NS_i + \theta_c + \epsilon_{jc} \tag{1.2}$$

The right-hand side of the eq. 1.2 is almost identical to the one above, but the outcome variable y_{jc} now refers to the younger sister j's school outcomes, measured at age 15/16, while c still refers to the oldest sibling's birth-cohort.

For both sets of analysis, I run the regressions separately for all combinations of sibling-gender pairs to examine how girls are impacted by remaining at home with either a sister or brother. This means that for the direct effects I split the sample of first-born girls by whether they have a second-born sister or brother. For the sibling spillover analysis I split the sample of younger sisters by whether they have an oldest sister or brother. The analysis is replicated for second-generation migrant boys and natives for comparison.

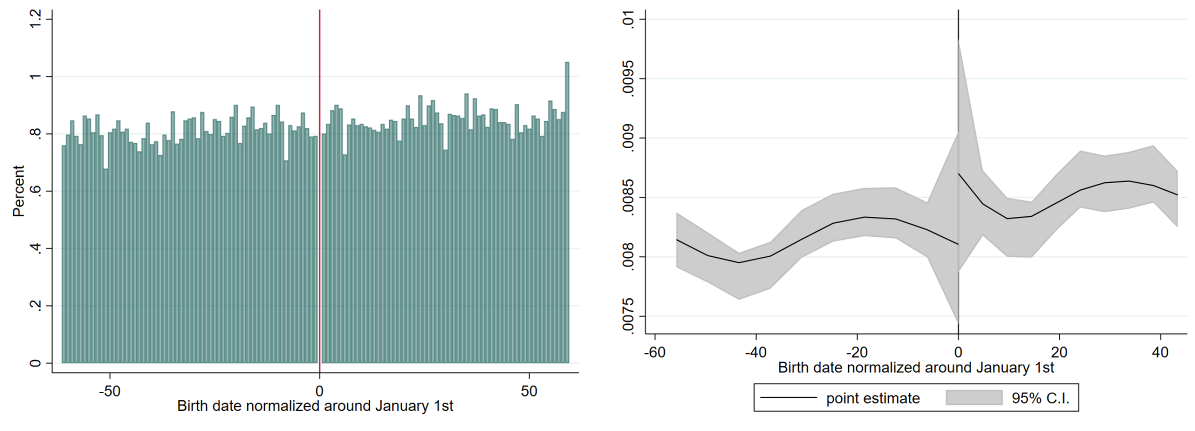
The bandwidth selected for the main analysis is +/- 60 days around the January 1^{st} cutoff. The standard errors are robust to heteroskedasticity. I do not cluster the standard errors, since the running variable (birth month) is discrete and only takes a limited number of values (Kolesár and Rothe 2018). In section 1.5.5 I show that the results are robust to multiple bandwidths, clustered standard errors, and alternative functional forms. It includes a data-driven bandwidth selection which yields marginal-squared-error optimal bandwidths within 48-67 days from the cutoff. This lends support to my choice of 60 days.

1.4.2 Specification Checks

The empirical strategy relies on the assumption that births are random around the cutoff, and that parents cannot perfectly manipulate the birthdate of their children. I perform several tests to see whether there is systematic selection of births on the right-hand side of the cutoff.

Figure 1.2 Graph (a) shows the distribution of births of second-generation migrants across the January 1st cutoff, which is normalized to 0 on the X-axis. The births are evenly distributed across dates, and I do not observe any significant peaks in birthdates after the cutoff. Graph (b) presents a manipulation test as described in Cattaneo et al.

Figure 1.2: Specification Checks



(a) Distribution of births

(b) Density test

Notes: Graph (a) shows the percent of second-generation migrant children born each day between November 1^{st} and February 28^{th}. Leap-year births are recoded to February 28^{th}. Graph (b) shows the density discontinuity of the running variable (birthdate) at the January 1^{st} cutoff. This test uses local-polynomial density estimators as explained in .

(2018) for any discontinuity in density at the cutoff. It shows no signs of manipulation of the running variable.

Appendix Table A2 presents the results of placebo regressions examining the effects of having a first-born child born after the cutoff on parental background characteristics. Since no estimates are statistically significant there is no evidence of selection into January-February births by migrant parents with relatively higher socioeconomic status.[18]

A potential threat to the sibling spillover analysis could be selection of both siblings into the same birth month. I run estimations to see whether the younger sibling's birthdate is continuous over the cutoff in oldest sibling's birthdate. Appendix Table A3 shows that having an oldest sibling who is born after the January 1^{st} cutoff does not predict a younger sibling being born after the cutoff. The only statistically significant connection between older and younger sibling's birthdates is that a boy is 7 percentage points *less* likely to be born after the cutoff if their oldest brother is.

One might worry that any gender differences in school-starting age effects stem from gender differences in first stage: the effect of birthdate on school starting age. A limitation in the data is that I only observe the students at the end of compulsory school. In another dataset I have available, 'Elevregistret', I can follow the 2002–2004 birth cohorts from first to ninth grade. A limitation here is that the data only includes a fraction of the students in my main sample, and I only observe their birth month. Nevertheless, I use this data to test the first stage for the 2002–2004 cohorts.

Appendix Table A4 columns (1)–(2) show that the first stage for absolute school-

[18] For native children the story is different. In fact, none of these specification tests hold when performed using the full sample of native children born between 1988–2003. I discuss the implications of this in Appendix A.3.

starting age is very similar for second generation migrant girls and boys both in terms of significance and magnitude. If we can extrapolate from this data to the additional cohorts in the main sample, there should not be gender differences in the first stage driving any gender differences in the main results. Appendix Figure A2 shows that girls born in January are more likely to start first grade one year earlier which leads to lower relative age for January-borns. Boys born in December are more likely to start school one year later which leads to a higher relative age for December-borns. This means that for both girls and boys, the jump at the cutoff is attenuated by January-born girls who are among the youngest in class, and December-born boys who are among the oldest.

For the main sample I can estimate whether being born close to cutoff impacts the likelihood that students are older or younger than 15 years at the beginning of ninth grade. This way I can assess whether there is any systematic gender differences in grade retention. I show the results of this regression for boys and girls in columns (3)–(6) in Appendix Table A4. Both girls and boys born after the January 1^{st} cutoff are less likely to be old for their grade and more likely to be young for their grade. Girls are slightly more likely to be both young or old for their grade compared to boys, but the difference is negligent.

1.5 Results

This section presents the results in the following order. I first establish a benchmark without accounting for family structure. Then, I examine the direct effects on second-generation migrant girls with a younger sibling at home, followed by the spillover effects on those whose oldest sibling enters school late. I continue by comparing these results to second-generation migrant boys. Lastly, I provide several robustness checks.

1.5.1 Benchmark: Average Effects of Late School Entry

The main objective of this study is to understand how family structure influences the impact of late school entry on girls' human capital formation in families where cultural norms may promote gender bias. To understand the main results it is useful to know something about the average impact of late school entry, without taking family structure into account. The standard school-starting-age paper would estimate the effect of being born after the cutoff on grade outcomes. This is essentially the regression equation 1.1 in Section 1.4.1, excluding the control for number of siblings. I run these regressions separately for second-generation migrant girls and boys as well as girls and boys from native families with comparable socioeconomic status.[19]

[19] The native sample consists of those who come from families with low socioeconomic status, i.e. those whose annual family incomes are at the 20th percentile or below. The point estimates are almost identical as for the full native student sample. See Appendix A.3 for more information.

The point estimates are presented in Graph (a) in Appendix Figure A4. Entering school late has positive impacts on the school outcomes of both second-generation migrant and native girls and boys, which is in line with the previous literature. The effect magnitudes for second-generation migrant girls are comparable to those of natives. The point estimates for second-generation migrant boys are closer to zero and the effect on the probability to be enrolled in the standard Swedish track is even statistically insignificant.[20]

Graph (b) in Appendix Figure A4 presents the results of estimating sibling spillovers on the school outcomes of younger sisters and brothers, without considering the gender of the oldest sibling. For both natives and second-generation migrants, having an oldest sibling who enters school late has no statistically significant effect on their school performance. Thus when the gender of the oldest is not taken into account, there seems to be no significant sibling spillovers of the policy.[21]

The important takeaway here is that if we would only evaluate the policy without taking family structure and the gender-composition into account, we would seriously miss the mark for how the policy impacts girls in migrant families. Once I run the analysis based on sibling-gender, the effects drastically change for second-generation migrant girls. But for second-generation migrant boys and for natives, the results will remain the same as in Appendix Figure A4. Family structure thus has an immense importance for how the policy influence second-generation migrant girls' human capital formation.

1.5.2 Main Results: Direct Effects

I now move on to the first set of my main results, which are derived from analyzing the direct effects of late school entry for second-generation migrant girls who are first-born in their families. I estimate these effects separately for those who have a second-born brother or sister.

Panel A. of Figure 1.3 provides a graphical presentation of the direct effects of late school entry for first-born girls in two graphs. The X-axis defines the running variable, the birthdate of first-born girls, binned to five days and normalized around the January 1^{st} cutoff, which is indicated by the red vertical line. The Y-axis displays first born girls' GPA outcome, standardized to a mean of zero and a standard deviation of one.

[20] The estimated effects of late school entry for students in Sweden are comparable to other studies that look at medium-run outcomes. Dhuey et al. (2019) finds that late school entry increases test scores with 15.8 percent of a standard deviation in 8th grade among students in Florida, the United States. Puhani and Weber (2007) finds an effect size of 40 percent of a standard deviation on fourth-grade test scores for German students. Similarly to my findings, they find that the effect sizes for migrant girls are comparable to natives, while the effects for migrant boys are smaller and less precise.

[21] The results for both migrant families and native families with low socioeconomic status differ from the findings of the two previous studies. These studies find large and positive spillover effects on younger siblings when the oldest enters school late, both in terms of socioeconomic status (Karbownik and Özek 2021) and minority status (Zang et al. 2023).

Graph (a) in Figure 1.3 reveals a significant positive discontinuity at the cutoff for first-born girls with a second-born younger sister. This means that these girls' GPA improves when they start school late. Panel A. in Table 1.2 provides point estimates for all outcomes. Columns (1)–(4) show that the maturity advantage of starting school late is positive and statistically significant for girls with a younger sister across all outcomes except for math grades, where the effect is statistically insignificant. It increases their GPA by 24.8 percent of a standard deviation, their Swedish grade by 25.6 percent of a standard deviation and the probability to be enrolled in the standard Swedish track by 15 percentage points.[22]

In contrast, Graph (b) in Figure 1.3 shows a much smaller positive discontinuity for first-born girls with a second-born younger brother. Columns (5)–(8) in Panel A. of Table 1.2 reveal that all effects for girls with a second-born younger brother are statistically insignificant and about half in size to the effects for those with a younger sister. This suggests that the maturity advantage of late school entry is offset for girls who remain at home with a second-born younger brother, but remains positive and significant for those with a younger sister.

1.5.3 Main Results: Sibling Spillover Effects

The second set of main results comes from the sibling spillover analysis, in which I estimate the impact of an oldest sibling entering school late—due to being born after the cutoff—on the school performance of second-generation migrant girls. Here, I run separate regressions for younger sisters with either a first-born brother or sister.

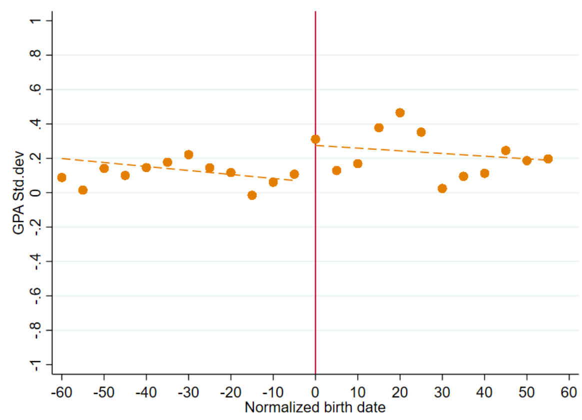
Panel B. of Figure 1.3 provides a graphical presentation of the spillover effects of having a sister or brother who enters school late on second-generation migrant girls' end-of-compulsory school GPA. Here, the X-axis displays the birthdate of the oldest sibling, while the Y-axis displays the GPA of the younger sister.

Graph (c) in Figure 1.3 shows the discontinuity in GPA for girls with oldest sisters, which is positive. From column (1) in Panel B. of Table 1.2 we see that having an oldest sister who entered school late due to the cutoff increases younger sisters' GPA by 13 percent of a standard deviation. The point estimate is however imprecisely measured. This is also true for the positive effect of 8.9 percent of a standard deviation on Swedish grade in Column (2). The point estimates presented in columns (3) and (4) show that the effects on math grades and probability to be enrolled in the standard Swedish track are positive and statistically significant, at 14 and 12 percent of a standard deviation

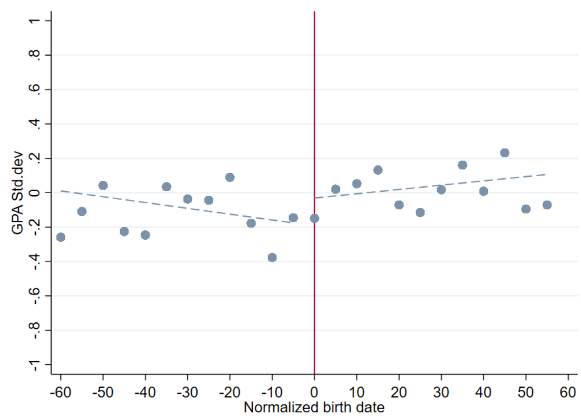
[22] Late school entry increases the GPA of first-born girls with a second-born younger sister with 24.8 percent of a standard deviation, which is large compared to the effect for the full population of second-generation migrant girls, of around 16 percent of a standard deviation. In general, the effects for first-born girls with a younger sister are around 40 percent larger than the effect on second-generation migrant girls overall. Part of the reason for the large effect sizes is the small sample size, which may be important to keep this in mind when interpreting the estimates in Table 1.2.

Figure 1.3: Graphical Presentation of Main Results

A. Direct Effects of Late School Entry First-Born Girls

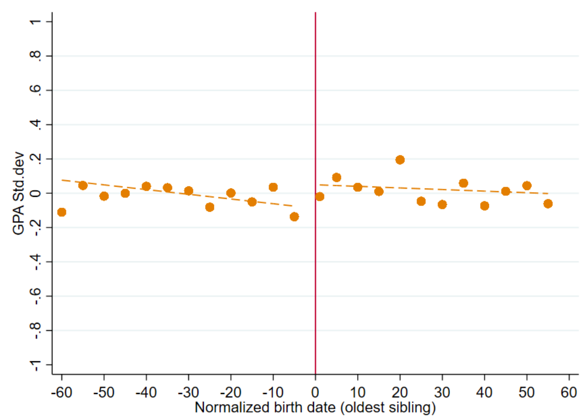


(a) Girls with second-born younger sister

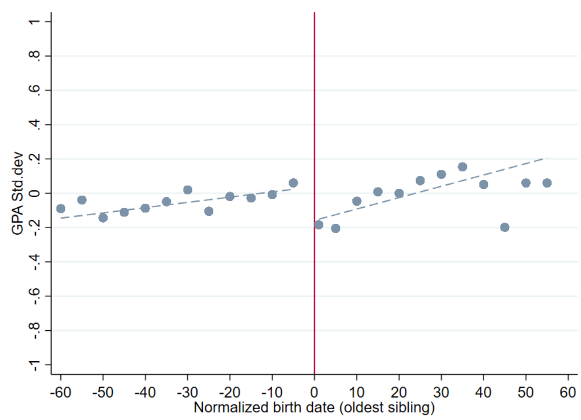


(b) Girls with second-born younger brother

B. Sibling Spillovers on Younger Sisters' GPA



(c) Girls with oldest sister



(d) Girls with oldest brother

Notes: The figure graphically presents the results from the RDD analysis. Panel A. shows the direct effects of late school entry for first-born girls with a younger sister or brother. Panel B. shows the spillovers of having an older sister or brother entering late on GPA for younger sisters. GPA is standardized to have a mean of zero and a standard-deviation of one. Estimation includes a linear control function and a 60-day bandwidth around the cutoff. The X-axis displays the running variable birthdate, normalized to zero around the cutoff, represented by the vertical red line. In Panel A, it represents first-born girls' birthdates, while in Panel B it represents the birthdates of younger sisters' oldest sibling. The sample consists of siblings born in Sweden to two non-Nordic parents between 1988–2003. Each orange or blue dot represents the average for a five-day birth bin.

respectively.

Remarkably, Graph (d) in Figure 1.3 shows that there is a stark negative effect of having an oldest brother who entered school late on younger sisters' GPA. Columns (5)–(8) in Panel B. of Table 1.2 show that the negative effect is visible across all outcomes for younger sisters. Their GPA is reduced by 17 percent of a standard deviation when they had an oldest brother who entered school late. It also reduces the likelihood that younger sisters are enrolled in the regular Swedish track by 7.4 percentage points. These effects are statistically significant on the five-percent level. The effects on younger sisters'

Table 1.2: Second-Generation Migrant Girls

A. Direct Effects First-born Girls	With Younger Sister				With Younger Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	0.248**	0.256***	0.163	0.152***	0.142	0.121	-0.050	0.001
	(0.097)	(0.097)	(0.104)	(0.047)	(0.139)	(0.127)	(0.126)	(0.064)
Observations	1,979	1,989	1,989	1,912	1,179	1,191	1,191	1,143
R-squared	0.045	0.052	0.031	0.042	0.039	0.039	0.030	0.054
Outcome mean	0.171	0.0880	-0.0462	0.708	-0.0451	-0.0754	-0.228	0.626

B. Sibling Spillovers Younger Sisters	With Oldest Sister				With Oldest Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	0.130	0.089	0.140*	0.121***	-0.170**	-0.103	-0.066	-0.074**
	(0.085)	(0.079)	(0.078)	(0.036)	(0.079)	(0.074)	(0.073)	(0.035)
Observations	3,254	3,287	3,287	3,250	3,122	3,147	3,147	3,119
R-squared	0.042	0.040	0.038	0.080	0.044	0.047	0.034	0.075
Outcome mean	-0.00143	-0.00935	-0.214	0.672	-0.0278	-0.0386	-0.224	0.681

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the direct effect and spillovers of entering school late on end-of-compulsory school outcomes for girls in migrant families. Panel A presents the direct effects of entering late on own outcomes. Panel B presents the spillover effects of having an oldest sister or brother enter late. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. For the estimations this sample is divided by the gender of the sibling. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the boy was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

Swedish and math grades are also negative at 10 and 6 percent of a standard deviation, but have weak power.

These effects can be compared to findings from two American studies on sibling spillovers related to school entry cutoffs. Karbownik and Özek (2021) reports a positive spillover effect of 14.8 percent of a standard deviation on younger siblings' test scores when an older sibling enters school late, specifically in families with low socioeconomic status. Similarly, Zang et al. (2023) finds that spillover effects of an older sibling entering school late range from 3.6 to 6.5 percent of a standard deviation in younger siblings' reading and math scores in families with low socioeconomic status.[23] The effect magnitudes for younger sisters in migrant families align with those in the two studies. However, unlike previous findings, the maturity advantage for the oldest sibling does not consistently generate positive spillovers for second-generation migrant girls but depends on the sibling's gender.

[23] Studies on the spillover effects of various educational or health shocks to one sibling show impacts on sibling school performance ranging from 1 to 15 percent of a standard deviation (Breining 2014; Qureshi 2018; Nicoletti and Rabe 2019; Black et al. 2021).

Table 1.3: Second-Generation Migrant Boys

A. Direct Effects First-born Boys	With Younger Sister				With Younger Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	0.052	0.064	0.054	-0.017	-0.031	-0.078	-0.077	0.164**
	(0.107)	(0.090)	(0.102)	(0.046)	(0.141)	(0.123)	(0.127)	(0.064)
Observations	1,962	1,988	1,988	1,907	1,186	1,194	1,194	1,153
R-squared	0.047	0.032	0.029	0.051	0.024	0.041	0.022	0.053
Outcome mean	-0.215	-0.367	-0.103	0.673	-0.325	-0.445	-0.213	0.567

B. Sibling spillovers Younger Brothers	With Oldest Sister				With Oldest Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	-0.075	0.001	-0.126*	0.052	-0.072	0.045	-0.066	-0.034
	(0.079)	(0.068)	(0.071)	(0.036)	(0.078)	(0.069)	(0.073)	(0.038)
Observations	3,286	3,331	3,331	3,301	3,103	3,130	3,130	3,104
R-squared	0.055	0.058	0.046	0.083	0.036	0.040	0.032	0.048
Outcome mean	-0.350	-0.489	-0.240	0.632	-0.336	-0.483	-0.232	0.632

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the direct effect and spillovers of entering school late on end-of-compulsory school outcomes for boys in migrant families. Panel A presents the direct effects of entering late on own outcomes. Panel B presents the spillover effects of having an oldest sister or brother enter late. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. For the estimations this sample is divided by the gender of the sibling. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the boy was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

1.5.4 Comparison to Second-Generation Migrant Boys

The main findings show that second-generation migrant girls' human capital formation is hampered by prolonged time at home with a brother, regardless of birth-order. What if children in migrant families are always at a disadvantage if they have to stay at home with siblings for an extended period. Parents who are constrained in time and resources might for example always prioritize investments to their first-born child, which could have negative implications for a younger sibling of any gender. To determine whether the results are driven by gender bias or a more general constraint in parental investments, I compare the effects for second-generation migrant girls with those for second-generation migrant boys.

Table 1.3 presents the results. Panel A shows the direct effects of entering school late for first-born boys. The effects are statistically insignificant and close to zero for most outcomes. Only the effect of late school entry on enrollment in the standard Swedish track is positive and statistically significant on the five-percent level for those with younger brothers.

Panel B shows the sibling spillover effects for younger brothers. The effects are mostly negative but relatively small and statistically insignificant for almost all outcomes. The only significant effect is a reduction in boys' math grades of 12.6 percent of a standard deviation when oldest sisters enters late. In all, the human capital formation of second-generation migrant boys does not seem to be impacted by remaining at home with an older or younger sibling due to the cutoff.

1.5.5 Robustness Checks

This section provides a discussion on the robustness of the main findings for second-generation migrant girls. Appendix Figure A5 presents a number of tests. All point estimates in orange represent girls with sisters, and all point estimates in blue represent girls with brothers. Each figure includes the point estimates presented in main results section, which are referred to as the baseline estimates.

First, I check whether the results are robust to clustering the standard errors on the birth-date of the oldest sibling, or adding controls for parent characteristics. Graphs (a) and (b) show the results for the direct effects and sibling spillovers. Clustering the standard errors on the running variable has been a common practice in the earlier regression discontinuity design literature, but Kolesár and Rothe (2018) shows that these perform relatively worse when the running variable is discrete. I find no visible differences in effects when using clustered or robust standard errors. The controls include parents' level of education when the child is 5 years old, and parents' average earnings when the child is age 5–7, percentile ranked within the birth cohort of the child for comparability. Overall, the point estimates remain the same as the baseline specification.

Second, I check the robustness of the main findings when varying the functional form. Graphs (c) and (d) present the results for the direct effects and sibling spillovers when estimated with a linear (the baseline) or a quadratic control function. Adding a second-order polynomial slightly enlarge a few of the estimates. Furthermore, the effects of having an oldest brother on younger sisters' Swedish and math grades turn weakly significant. Nevertheless, most point estimates do neither change in statistical significance nor sign.

Third, I reproduce the main results using different bandwidths. Graphs (e) and (f) show the point estimates when adding +/- 5 days around the January 1^{st} cutoff. For both the direct effects and sibling spillovers the estimates are somewhat unstable for bandwidths below 30 days, likely due to the very small sample sizes. After including approximately 50 days around the cutoff the estimates stabilize. I also reproduce the main results using a data-driven bandwidth selection developed by Calonico et al. (2017). This approach automatically chooses the marginal-squared-error optimal bandwidth for each outcome variable. Appendix Table A5 presents the results. Both the direct effects and the sibling spillovers are robust to the use of bandwidths selected with the data driven approach. For all outcomes, the optimal bandwidth is around 60 days, which lends support for the choice of a two-month bandwidth.

Lastly, to ensure that the results for second-generation migrant students are driven by the school-entry age of the oldest child, not vice versa, I run placebo regressions for the effects of the younger siblings school starting age on the older sibling's school outcomes. The results are presented in Appendix Figure A3. There are no statistically significant impacts of a younger sibling's school entry age on the oldest sibling's grade outcomes.

The robustness checks for second-generation migrant boys are presented in Appendix Figure A6 and the results using data driven bandwidth selection in Appendix Table A9. Overall, the main results are robust to all tests and remain mostly statistically insignificant and close to zero.

1.6 Mechanisms

The results thus far suggest that Sweden's school-entry policy has unintended consequences for girls in migrant families, particularly when it results in them spending more time at home with their brothers. In this section, I propose gender bias in parental preferences as a key driver of these outcomes. I begin by outlining a simple theory that emphasizes the role of gender biased preferences in explaining the findings and, based on this theory, design additional empirical tests to further support this explanation.

1.6.1 Conceptual Framework

In this section, I introduce a simple conceptual framework to examine how late school entry for one child in a family affects the human capital of both that child and their sibling (Becker 1981; Currie and Almond 2011; Karbownik and Özek 2021). Consider a family with one parent and two children (i = 1, 2), where the parent is altruistic and values the children's lifetime earnings. These earnings are determined by the children's human capital, which is developed during childhood. For simplicity, I assume a single time period. The human capital production function for sibling i is expressed as follows:

$$H_i = f^i(I_i, \sigma_1, \sigma_2) \tag{1.3}$$

where I_i represents parental investment, and σ_1 and σ_2 are the school starting ages of each child. The production function of child $i's$ human capital, $f^i(\cdot)$, is assumed to be strictly increasing in all of its arguments and strictly concave in I_i. Additionally, I assume a higher school starting age increases the marginal return to parental investments, meaning that I_i and σ_i complement each other in the production of human capital. This assumption captures the notion that if a child enters school later, they will require greater parental investment beforehand to fully benefit from late school entry.

The parent derives utility from their children's human capital, subject to a budget constraint:

$$U = U(\gamma_1 H_1, \gamma_2 H_2) \quad \text{s.t} \qquad y = I_1 + I_2. \tag{1.4}$$

The utility function is assumed to be strictly increasing and concave in its arguments. The parameters $\gamma_1 = \gamma$ and $\gamma_2 = 1 - \gamma$ represent the parent's preferences for each child's human capital. I will refer to the parent's preferences as *neutral* if they value both children's human capital equally ($\gamma_1 = \gamma_2$). In contrast, the parent is considered to have *biased* preferences if they place greater value on one child's human capital over the other ($\gamma_1 > \gamma_2$ or $\gamma_1 < \gamma_2$).

By using the budget constraint to express I_2 in terms of I_1 and substituting it into the utility function, I transform this constrained optimization problem into an unconstrained one with respect to I_1. The first-order condition that characterizes the optimal level of parental investment, $I_1^* = I_1^*(\sigma_1, \sigma_2, \gamma_1, \gamma_2, y)$, is given by:

$$G \equiv \gamma_1 U_1 f_1^1 - \gamma_2 U_2 f_1^2 = 0 \tag{1.5}$$

Here, U_i represents the partial derivative of $U(\cdot)$ with respect to H_i ($i = 1, 2$), while f_k is the derivative of $f^i(\cdot)$ with respect to kth argument ($k = 1, 2, 3$). The arguments of these functions are omitted for simplicity of exposition.

The goal is to analyze how an increase in the school starting age for child 1, σ_1, influences the human capital of both that child and their sibling, child 2, and to explore the role of parental investment adjustments in this process. Starting with the impact on the focal child ($i = 1$), the effect is given by:

$$\frac{\partial H_1^*}{\partial \sigma_1} = \underbrace{\frac{\partial f^1(I_1^*, \sigma_1, \sigma_2)}{\partial \sigma_1}}_{\substack{\text{maturity} \\ \text{effect}}} + \underbrace{\frac{\partial f^1(I_1^*, \sigma_1, \sigma_2)}{\partial I_1} \frac{\partial I_1^*}{\partial \sigma_1}}_{\substack{\text{investment} \\ \text{adjusmtent}}}. \tag{1.6}$$

The first term represents the direct impact of the policy, which can be interpreted as the maturity advantage effect of late school entry, an effect frequently emphasized in the empirical literature. The second term shows how this effect may be either amplified or reduced through adjustments in parental investments.

Next, the effect on the non-targeted child ($i = 2$) is described as:

$$\frac{\partial H_2^*}{\partial \sigma_1} = \frac{\partial f^2(I_2^*, \sigma_1, \sigma_2)}{\partial \sigma_1} + \frac{\partial f^2(I_2^*, \sigma_1, \sigma_2)}{\partial I_2} \frac{\partial I_2^*}{\partial \sigma_1}. \tag{1.7}$$

Using that $\partial I_2^*/\partial\sigma_1 = -\partial I_1^*/\partial\sigma_1$, the equation becomes:

$$\frac{\partial H_2^*}{\partial\sigma_1} = \underbrace{\frac{\partial f^2(I_2^*,\sigma_1,\sigma_2)}{\partial\sigma_1}}_{\substack{\text{role model}\\ \text{effect}}} - \underbrace{\frac{\partial f^2(I_2^*,\sigma_1,\sigma_2)}{\partial I_2}\frac{\partial I_1^*}{\partial\sigma_1}}_{\substack{\text{investment}\\ \text{spillover}}}. \tag{1.8}$$

In this case, the first term represents a role model effect, capturing the direct effect of child 1's late school entry on child 2's human capital. The second term accounts for the impact of parental investment spillovers, showing how adjustments in parental investment in response to changes in σ_1 affect the human capital of child 2.

The overall effect of the policy on both the focal child ($i = 1$) and the non-targeted child ($i = 2$) depends on the sign and relative size of the parental investment response, $\partial I_1^*/\partial\sigma_1$ (the comparative static result). In Appendix A.4, I formally derive $\partial I_1^*/\partial\sigma_1$, which yields the following insights. Suppose first that the production functions of the two children are symmetric, i.e., $f^i(\cdot) = f(\cdot)$. In this case there are three scenarios to consider. First, when the parent has neutral preferences ($\gamma_1 = \gamma_2$), the parental investment response to an increase in σ_1 is generally small, as two opposing forces balance out the parent's investment incentives. On one hand, due to the complementarity between parental investments and the school starting age in the human capital production function, the parent is incentivized to increase their investments in the focal child, holding everything else constant. However, since the parent is budget constrained, any increase in I_1 would require a reduction in I_2, which would decrease the non-targeted child's human capital and lower the parent's overall utility. This, in turn, counteracts the parent's incentive to raise their investment in the focal child. Hence, when the parent has neutral preferences, the two dominating factors which determine the policy's total effect are the maturity advantage effect for the focal child (first term in eq. 1.6) and the role model effect for the non-targeted child (first term in eq. 1.8).

Second, if the parent has biased preferences, there are two cases to consider. Let us begin by assuming the parent's preferences favor the focal child of the policy ($\gamma_1 > \gamma_2$). As previously noted, when σ_1 increases, the complementarity between I_1 and σ_1 in $f^1(\cdot)$ encourages the parent to increase their investment in the focal child of the policy, I_1. However, unlike in the case of neutral preferences discussed previously, this incentive is no longer offset by the corresponding decrease in I_2, which reduces the human capital of the non-targeted child, as this factor has less weight in the parent's utility function. Hence, when a parent favors the focal child, investments in that child will rise while investments in the non-targeted child will decline. Overall, the maturity advantage effect for the focal child will be amplified by the parental investment response, while the role model effect for the non-targeted child will be reduced by it. In scenarios where the role model effect is small, the total effect of the policy on the non-targeted child will be negative; specifically, their human capital will decline if the school entry age for their sibling is raised.

Next, let us consider the scenario in which the parent's preferences are biased in favor of the non-targeted child of the policy ($i = 2$). In this case, despite the complementarity between I_1 and σ_1 in $f^1(\cdot)$, the parent lacks strong incentives to increase their investment in the focal child, as the human capital of that child holds little weight in the parent's utility function. Nonetheless, the focal child's human capital still increases due the maturity advantage effect (first term in eq. 1.6). To counterbalance this, the parent will boost their investment in the non-targeted child, whose human capital has more weight in their utility function. As a result, when a parent favors the non-targeted child, investments in that child will rise while investments in the focal child will decline. Overall, the positive maturity advantage effect for the focal child will be offset by the parent's investment decisions, while the role model effect for the non-targeted child will be enhanced by this response.

The previous discussion assumes a uniform human capital production function $f(\cdot)$ for each child. However, when these production functions exhibit different technologies, the parent's investment response to an increase in σ_1 can become non-neutral (i.e, $\partial I_1^*/\partial\sigma_1 > 0$ or vice versa), even if their preferences are neutral. For example, if the focal child benefits from a substantial maturity advantage (first term in eq. 1.6), the parent might reduce investments in that child and increase investments in the non-targeted child to equalize the human capital across both children. In the following empirical analysis, I will introduce a test to distinguish between preference-based and technology-based explanations for my main findings.

In summary, when applied to the context of this study, the theory offers two possible explanations for the main findings—namely, the direct and sibling spillover effects, and how these differ based on the gender composition of siblings. The first explanation suggests that parents hold gender-biased preferences, placing higher value on sons' human capital than on daughters'. The second explanation is that the human capital production for sons and daughters follow different technologies. In the following sections, I present empirical tests to differentiate between these two explanations.

1.6.2 School-Entry Effects in Native Families

An ideal test for the preference-based explanation would involve (i) identifying a group of families with identical human capital production processes as migrant families but without gender-biased preferences, and (ii) demonstrating that girls in these families do not experience any unintended effects from late school entry. To approach this ideal test, I start by replicating the analysis for a sample of native families with comparable socioeconomic background.[24]

Panels A and B in Appendix Table A12 present the direct effects of entering school late for first-born girls and boys in the sample of native families with low socioeconomic status. There are no real differences in effects for first-born girls and boys by the gender of their second-born younger sibling. For both girls and boys, remaining at home with a second-born younger sister seems to have a marginal positive effect on their GPA and on boys' math grades. Remaining at home with a second-born younger brother has no statistically significant impact on girls or boys' school outcomes.[25]

Panels C and D in Appendix Table A12 provide the point estimates for the sibling spillover analysis for native siblings. All effects are close to zero and statistically insignificant. Hence having an oldest brother or sister who entered school late seems to not have any influence on younger siblings' school performance. Most importantly, there seem to be no real differences in magnitude or sign of the effects for younger sisters depending on the gender of the oldest sibling.[26]

Overall, first-born girls and boys in native families experience close to identical maturity advantages from late school entry, and there are no statistically significant spillover effects on their younger siblings. Importantly, the effects do not vary for girls by the gender of their sibling. This suggests that the main results are not driven by differences in production technologies among sisters and brothers in families with similar socioeconomic status, but rather by parents' preferences in investments to their daughters or sons.

1.6.3 Gender-Traditional Background

One way to test for gender bias in parental investments is to see whether there is heterogeneous effects depending on the parents' own cultural backgrounds. Traditional gender norms among migrants that stem from their home countries have proven remarkably persistent over time (Giavazzi et al. 2019). Parents from cultures with more conservative gender norms might be more prone to invest in their sons' education than their daughters'. If this is the case, we should see that remaining at home with brothers for longer should have more prominent negative influences on girls from these families.

[24] This includes families whose annual incomes are at the 20th percentile or below when the first-born child was do daycare age. For this sample, the specification checks shows no evidence of birth manipulation, and therefore the analysis provide unrepeatable estimates. Note that this is not the case for the total native student population, where the specification checks show signs of birth manipulation among high-earning and highly educated parents. See Appendix A.3 for details.

[25] The point estimates for the full native sample in Appendix Table A13 the direct effects on first-born girls and boys are all positive and statistically significant independent on their younger sibling's gender. If anything, it seems like the maturity advantage of late school entry is less evident for first-born girls and boys in families with low socio-economic status, but there are no gender differences in effects.

[26] The results for the full sample of natives are presented in Appendix Table A13, which also show no evidence of spillover effects from the timing of the oldest sibling's school entry. Interestingly, the absence of any sibling spillovers among low-SES natives is contrary to the findings from the two US studies, which argue that sibling spillovers are especially prominent in families where parents are constrained in their time and resources (Karbownik and Özek 2021; Zang et al. 2023).

For this test I split the sample by the Gender Inequality Index (GII) of the mother's origin country, which is computed by the United Nations Development Program (UNDP).[27] I run separate regressions for girls with mothers who originate from countries with a GII value above or below the mean. I equate originating from a country with a GII value above the mean with coming from a "more gender traditional" background.[28]

Appendix Table A6 presents the effects of late school entry for first-born girls in families from more and less traditional backgrounds. Panel A shows the point estimates for girls with a second-born younger sister. Here we see that the positive effects of late school entry are statistically significant for those from more gender traditional backgrounds. Panel B presents the results for first-born girls with a second-born younger brother, which are all statistically insignificant. Nevertheless, the effects for those from less gender traditional backgrounds are mostly positive. The effects for those from more gender traditional backgrounds are mostly negative.[29]

Appendix Table A7 presents the sibling spillovers in families from more or less gender traditional backgrounds. Panel A shows the effects for younger sisters of having an oldest sister who entered school late. The positive estimates only remain statistically significant for younger sisters in more gender traditional families. Panel B presents the effects for younger sisters of having an oldest brother who entered school late. Similarly, we observe that the negative impacts of having an older brother who enter school late are more prominent for girls from more gender traditional backgrounds.

Although the point estimates differ in significance to each other, they are not significantly different from each other. These test results therefore provide suggestive evidence that cultural backgrounds, which may influence gender-biased parental investments, are a key factor underlying the findings. The negative effects are more pronounced for girls who stay at home with brothers in families with more traditional values. On the other hand, the positive effects for two sisters who remain at home together are also primarily seen in those from more traditional backgrounds.

[27] The GII is a composite metric based on nationwide measures of reproductive health, employment, and the labormarket.

[28] I use the index values recorded during the same period as the sample birth cohorts, 1988–2003, to calculate an average index per country. Since I only observe the country group that the mother originates from in the administrative data, the index value used is the within-country group average weighted by the total migrant population from each country in Sweden during the study period. The GII value for each country group is provided in Appendix Table A1.

[29] Appendix Table A10 presents the results for boys split by more and less gender traditional backgrounds. For the vast majority the point estimates are small and statistically insignificant. There seem to be some evidence of negative effects of having an oldest sister who enters school late on younger brothers' GPA and math grades in less families from more gender traditional backgrounds. However, they are also more likely to be enrolled in the regular Swedish track when their sisters enters late.

1.6.4 Policy Impacts on Mothers' Labor Force Participation

Another test for gender bias in parental investments is to see whether having a daughter or son who is born after the cutoff impacts the parents' resource allocation. Previous research has shown that native mothers' labor market participation increases when their child enters school late (Landersø et al. 2017). The underlying idea is that being among the oldest at school entry eases their transition and frees up time and resources for the mother, leading her to go back to work. However, in families with gender bias, mothers may respond differently depending on whether it is her daughter or son who enters school late.

I estimate the effect of having a child who is born after the January 1^{st} cutoff on the mother's earning percentiles and the probability that she is employed.[30] The outcomes are measured by the end of the year the child turns seven years old, which is when the child attends first grade, regardless of entering school early or late.

Appendix Table A8 presents the results. Columns (1)–(2) show that having a daughter who enters late increase the mother's earnings by 2.4 percentiles and the probability to be employed by 5.8 percentage points. Both are statistically significant on the one-percent level. Columns (3)–(4) show that having a son who enters late increases the mother's employment with 3 percentage points, which is significant on the five-percent level, but this is not reflected in wages. These results can be compared to the effects for native mothers, presented in Appendix Table A14. Both for mothers in the low-SES and full native samples, there are no gender differences in the effects of having a child who enters school late on neither earnings nor employment.

In all, these findings suggest that gender bias within the family can cause parents to make sub-optimal economic choices. Since the 1971 tax reform which removed joint taxation of spouses, most women with children are working in Sweden (Selin 2014). Today, there is a notable difference in earnings between Swedish native and migrant mothers' earnings.[31] Migrant mothers differ from native mothers since they only improve their labor supply when they have daughters who are born after the January 1^{st} cutoff. Although parents may think that a mother's time with the son is an important investment, it is likely counter-intuitive. The positive income effect within the family that sons miss out on can improve the family's socioeconomic status and in turn the child's academic opportunities.[32]

[30] Employment is a dummy for whether the mother earns at least half of the median income for 45-year-old women in the labor force. This value was calculated using the average incomes of women aged 44–46 between 1985–2018, deflated to 2018 values. The threshold amounts to 123,766 SEK, which in 2018 was 12,599 EUR.

[31] See Figure 1.1.

[32] This can explain why there are smaller direct effects of late school entry for second-generation migrant boys than girls in the benchmark analysis in Section 1.5.1.

1.7 Conclusion

This study examines how a common education policy, determining the timing of school entry, and family structure interact to shape second-generation migrant girls' educational outcomes. It leverages the January 1^{st} cutoff in admission to public education in Sweden in a Regression-Discontinuity design. The previous literature has found that late school entry improves school outcomes. This study shows that the policy impacts for second-generation migrant girls drastically change depending on the gender of the sibling she remains at home with.

Second-generation migrant girls' school performance is hampered by extended time at home with a brother, regardless of birth order. Two key findings which supports this. First, the direct effects of late school entry only positively impacts the school outcomes of girls with a younger sister, but not those with a younger brother. Second, girls are negatively impacted by having an oldest brother who enters school late. The results are robust to a number of tests. This is striking compared to the results for migrant boys or native children, where the effects of late school entry are never influenced by sibling gender, and where there are no significant sibling spillovers from the policy.

This study highlights a unique disadvantage for girls in migrant families. Further analysis suggests that gender bias in parental investments is a key underlying factor. The presence of a brother at home may reduce the resources directed toward girls, potentially offsetting or even reversing the otherwise positive effects of late school entry.

There has been increasing concern about boys falling behind girls in school, with recent discussions in the U.S. suggesting holding boys back a grade (Reeves 2022). However, this study cautions that such approaches could have unintended negative spillover effects on girls, especially if parents hold traditional gender views. The Swedish school-entry cutoff is strict, and offers little flexibility for children who might benefit from entering school early or late. Other European countries, such as Germany, offer flexibility in school entry for children born in a time window around the cutoff date of admission. This could allow for more targeted information campaigns to parents, to provide them with the pros and cons of having their child enter school early or late. It could also include early testing of migrant children's proficiency in the host-country language as an indicator for whether they would benefit from early school entry. The weak maturity advantage of late school entry for migrant boys indicates that they might actually benefit from early school entry. This could, for example, increase boys' proficiency in the Swedish language. In turn, it could also positively influence their younger sisters' human capital formation.

An option to more flexibility in entry to public education is increasing migrant children's participation in early learning environments, by for example giving siblings in migrant families priority admission to daycare. Migrant girls may especially benefit from learning outside the home when they have younger brothers. Similarly, if brothers spent more time learning outside the home it could have a positive influence on their younger sisters. Apart from increasing early child care on the extensive margin, it may be important to also consider the intensive margin: providing mixed native-migrant peer groups and early language education would likely benefit migrant girls and boys.

More broadly, the findings offer insights into how education policies can have unintended consequences for children from minority backgrounds. This highlights the need for deeper insights into how population-wide policies impact children from minority families, where the dynamic complementarities between parental and public investments may differ significantly from those in majority families.

Chapter 2

Life After Divorce*

Effects of Joint Custody on Parents and Children

Abstract: Custody arrangements are crucial in determining how frequently and in what way children interact with each parent after a divorce, because these potentially translate to investments in children by both parents. This study explores the causal effects of joint versus sole custody on children's and parents' outcomes. We merge hand-collected data on nearly 100,000 custody cases with several registries from Statistics Sweden to create a comprehensive administrative data set containing various child and parental outcomes measures. To identify the casual effects of joint versus sole custody, we leverage the random assignment of custody cases to judges, along with systematic differences in judges' preferences for joint versus sole custody. We demonstrate that pre-determined characteristics of parents and children are uncorrelated with the preferences of the judge handling their case, whereas judges' preferences are highly predictive of the custody ruling in individual cases. The findings indicate that shared custody (i) has significant positive effects on children's educational outcomes, (ii) increases fathers' earnings and mental health, and (iii) shows no significant impact on mothers' well-being or economic situation.

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2.1 Introduction

Parents play a fundamental role in shaping a child's social and academic development, providing both emotional support and economic resources that influence long-term well-being. When a family undergoes divorce, custody arrangements become a first-order concern, determining not only where a child resides but also the extent of each parent's ongoing involvement in their upbringing. These arrangements can have far-reaching implications, not only for children's development but also for parents, affecting their caregiving responsibilities, concerns about economic stability, and overall well-being. By allocating time and responsibilities between parents, custody decisions may affect labor supply choices, mental health, and the degree of cooperation or conflict between former partners.

Historically, Western countries have favored sole maternal custody following divorce, reflecting traditional gender roles in caregiving. However, the past few decades have witnessed a significant shift toward joint custody arrangements, driven by changing societal norms and legal reforms emphasizing shared parental responsibilities. A key catalyst for this transition was the UN General Assembly (1989) Convention on the Rights of the Child, which reinforced the principle that both parents should contribute to a child's upbringing. In response, many countries—including the United States and several European nations—revised custody laws to facilitate joint custody arrangements, now widely viewed as the default in many jurisdictions (Nielsen 2011).

This policy shift raises important questions about the broader consequences of joint custody for both children and parents. Advocates contend that children benefit from maintaining close relationships with both parents, receiving greater emotional support and financial resources compared to those in sole custody arrangements. At the same time, joint custody may influence parental behavior, potentially increasing fathers' sense of responsibility while allowing mothers to maintain stronger labor market ties. However, critics argue that joint custody can introduce instability in children's lives, particularly when coordination between parents is difficult or when conflicts persist post-divorce (Bauserman 2002). The ongoing debate highlights a fundamental question: How do different custody arrangements shape outcomes for children and parents alike?

Despite the policy relevance of this question, causal evidence remains scarce. First, existing data sets often record whether parents divorced but lack detailed information on custody arrangements, making it difficult to study the effects of joint versus sole custody. Second, selection bias complicates causal inference: more cooperative parents, often with higher socioeconomic status, are more likely to choose joint custody, potentially benefiting their children through channels unrelated to the custody arrangement itself.

Against this backdrop, this study provides novel evidence on the causal impact of joint versus sole custody on parents and children. We address challenges related to data availability and selection bias by examining the effects of joint custody rulings following civil custody disputes in the Swedish district court system.

We construct a unique data set capturing the near universe of Swedish custody rulings from 1992 to 2021 to identify families' court-ordered custody arrangement. We obtained access to the digital copies of all Swedish custody and divorce rulings from 2010, when courts began digitizing their records, up until 2021, the final year in our sample. Before 2010, custody rulings were stored in physical archives. We scanned rulings from the years 1992 to 2010 from the eight largest district courts in Sweden. Through standard text processing methods, we extract the custody ruling, background characteristics of the case, judges, and the ID numbers of children and parents. This information has been combined with Swedish administrative records on personal background information, earnings, test scores, and health.

To address issues of selection, we leverage the random assignment of Swedish custody cases to judges who systematically differ in their propensities to assign joint versus sole custody. We construct a leave-out measure of judge propensity to assign joint custody, and use this as an instrument for the court-ordered custody arrangement in a two-stage Instrumental Variables (IV) design. We provide evidence supporting the random assignment of judges to cases by demonstrating that a wide range of predetermined parental and case characteristics (i) are strongly correlated with the custody ruling in a given case but (ii) do not predict the judge's propensity of awarding joint custody. Judge propensities, on the other hand, strongly predict the custody ruling in a case: a family has a 6.17 percentage point higher probability of being assigned joint custody when a judge with a 10 percentage point higher propensity for joint custody was assigned to their case.

Our findings highlight several key insights. First, joint custody has positive effects on children's educational outcomes but no impact on their mental health. Specifically, it improves children's scores on the first national standardized test taken after the custody ruling by 56 percent of a standard deviation, with the effect being strongest among children under the age of 12 at the time of the custody decision. Additionally, children in joint custody families are more likely to be enrolled in higher-quality primary schools. When parents make joint schooling decisions, it enhances school quality for their children in grade 3 by 1.04 standard deviations. In contrast, joint custody appears to have no measurable impact on children's mental health, as indicated by the use of antidepressants, anti-anxiety medications, and ADHD-related prescriptions.

Second, fathers with joint custody experience significant and lasting improvements in both earnings and mental health. Joint custody increases the likelihood of fathers earnings being above the 25th percentile or exceeding the median income four years after the ruling by roughly 40 percent relative to the corresponding means. Furthermore, fathers with joint custody are significantly less likely to take antidepressants or anti-anxiety medications, with these effects persisting for up to five years after the custody ruling. These results suggest that joint custody improves fathers well-being and motivates especially lower-earning fathers to increase their labor market participation. This is potentially driven by a sense of increased responsibility or financial necessity to support their children. For many fathers, being assigned joint custody allows them to maintain an active role in their child's life and continue sharing parental responsibilities.

Lastly, neither the earnings nor mental health of mothers are affected by being assigned joint custody. We find no statistically significant effects across any outcomes. However, mothers face a different counterfactual than fathers: when sole rather than joint custody is assigned, it is awarded to the mother in 79 percent of cases. For most mothers, the distinction between joint and sole custody primarily involves the level of involvement of their ex-partner rather than a fundamental change in their access to their child. While we do not observe the same positive effects for mothers as we do for fathers, we also do not find any negative effects. This suggests that whether or not they have joint custody has little bearing on mothers' well-being or economic situation.

Our study relates to several strands of literature. To date, all insights into the consequences of joint versus sole custody for children come exclusively from correlational studies. In a widely cited paper, Bauserman (2002) conducts a meta-analysis of 33 descriptive studies from psychology and sociology to examine whether children of divorce adjust better if their parents have joint legal and physical custody. The analysis shows that children in joint custody households are significantly better adjusted: they have a higher school attendance and higher cognitive ability (IQ), as well as better family relations, less behavioral problems, and higher self-esteem. Moreover, there is no evidence in support of the argument that joint-custody children are more likely to be exposed to parental conflict. In another descriptive paper, Amato et al. (2011) assess the notion that co-parenting—a 'good divorce'—protects children from negative consequences such as bad behavior or decreasing school grades. Their findings show that children in co-parenting families had the smallest number of behavior problems and the closest ties to their fathers. However, these children did not seem to have any significant advantage in school. This study extends this literature by providing the first causal estimates of joint versus sole custody arrangements, addressing the selection bias inherent in descriptive analyses.

A small body of work has exploited the staggered introduction of laws permitting joint custody across US states to estimate reduced-form effects on child and family outcomes (Nunley and Seals 2011; Halla 2013; Maiti 2015). Importantly, this literature does not estimate how divorcing families are affected by joint versus sole custody but rather asks whether exposure to joint custody legislation affects investments in children in intact families through a shift in intra-household bargaining power from mothers to fathers. The results support the idea that a switch from sole custody to joint custody regimes weakens the intra-household bargaining power of mothers and that this in turn lowers investments in children and reduces female labor force participation. Halla (2013) also finds that the adoption of laws permitting joint custody led to increasing marriage, divorce, and fertility rates in the US. In contrast to this literature, our study directly examines families going through divorce, focusing on how custody arrangements impact children's developmental outcomes and parental economic trajectories. Our study bridges the gap between broad legal reforms and the specific impacts of joint custody on post-divorce families.

More broadly, our study is related to a large and established literature examining how family disruptions during childhood affect children's later-life outcomes. Earlier work has established that living in a single-parent family during childhood is associated with lower educational attainment and more risky behaviors during adolescence (McLanahan and Sandefur 1994; Case et al. 2001; Ermisch et al. 2004; Gruber 2004). However, several studies have provided evidence that it is self-selection, rather than causation, driving this association (Björklund and Sundström 2006; Ermisch and Francesconi 2013). In another effort to explore the impact of growing up with only one parent, Adda et al. (2011) evaluate the consequences of parental death on children's long-term outcomes, using Swedish administrative data. They find that the effect of parental death on children's cognitive and non-cognitive future is negative but small. Bertrand and Pan (2013) investigate to what extent the family environment explains the gender gap in children's disruptive behavior. Their results show that boys are particularly worse off by not being brought up in an intact family with two biological parents. We add the first evidence of a critical dimension of family disruption - the impact of custody arrangements after divorce - that has received much less attention.

The remainder of this chapter is structured as follows: Section 2.2 outlines the institutional context. Section 2.3 details the data collection and dataset. Section 2.4 describes the empirical strategy, and Section 2.5 presents the results. Finally, Section 2.6 provides a conclusion.

2.2 Institutional Context

In this section, we outline the key aspects of child custody in Sweden relevant to our study. We also explain the regulations governing case assignment to judges and the extent of judicial autonomy in Swedish court proceedings.

2.2.1 Child Custody in Sweden

Under Swedish law, children have the right to care, security, and a good upbringing (SFS 1949:381). Legal custody is held by both parents, or one of them, unless the court assigns it to a specially appointed guardian. The custodian(s) are responsible for the child's personal welfare and must ensure that their needs for care, security, and upbringing are met. This also includes physical custody, meaning that the child should reside with the custodian(s). Both legal and physical custody remains in effect until the child turns 18.

At birth, a child is under the custody of both parents if they are married. If the parents are not married, the mother has sole custody until they apply for joint custody with the Swedish Tax Agency. In the event of a divorce or separation, custody remains joint by default, unless one parent starts the legal process of changing the custody arrangement. Divorces are granted by the district court, which is the first instance in Sweden's three-tiered court system. If children are involved, the court will require a six-month waiting period before they grant the divorce. Divorcing or separating parents who want to establish a new custody agreement must turn to their municipality of residence. The municipalities' social welfare boards have units dedicated to offering mediation and assisting parents in drafting a custody agreement. Once signed and approved by the social welfare board, the agreement becomes legally binding.

If parents cannot reach an agreement, they must bring the case to the district court with jurisdiction over the child's place of residence. Most commonly, parents disagree on how custody should be arranged, leading to the case being registered as a civil dispute (*tvistemål*). To initiate the dispute, one parent must file a formal custody petition outlining their preferred custody arrangement and the reasons behind their request. This parent becomes the plaintiff in the dispute, while the other parent becomes the defendant. When the petition is submitted to the district court, it is assigned a case number (a unique ID for the court and year) and a judge. Obtaining legal representation is not mandatory but common and individuals with low income may qualify for state-funded legal assistance.

Most custody cases are resolved at the district court level, as civil disputes impose strict limitations on the parties' ability to take their case to an appeals court, like introducing new evidence or circumstances (SFS 1949:381, 29 kap. 11 §, 29 kap. 11 §). Instead of appealing, parents can file a new petition to the district court to have the case reopened. In principle, a custody dispute over the same child can be repeatedly revisited until the child turns 18, as long as one parent continues to file petitions after each case is closed.

2.2.2 Judge Assignment in the Swedish Court System

There are 48 active district courts in Sweden, each covering a specific geographical area. Most courts are organized into departments (*avdelningar*), which are further divided into sections (*rotlar*). Smaller courts may be structured solely into sections. Each section consists of one judge, one clerk, and several administrators. District court judges are appointed for life and have the primary responsibility of adjudicating criminal cases and civil disputes. To qualify as a judge, an individual must hold a law degree and complete additional training within the court system.

Judges are assigned to cases based on the court regulation (*arbetsordning*), a publicly available document that outlines case allocation procedures and other court-specific guidelines (SFS 1996:381). According to the documents we obtained from the courts, the most common practice is to randomly assign incoming cases to a section. In larger courts, cases are sometimes randomly assigned to a department and then distributed by the head of the department to the sections.[1] While judicial specialization in criminal cases has become more common over time in these courts, civil cases continue to be randomly assigned across departments and sections.

2.2.3 Judges' Decision Making Power

Judges have significant autonomy in shaping both the case process and its outcome, but their decisions must always align with a fundamental principle of the Swedish judicial system: ruling in the child's best interest (SFS 1949:381, 6 kap, 6 kap). This requires judges to primarily consider the child's need for a close and positive relationship with both parents while also assessing potential risks, such as violence or unlawful abduction.

Initially, the judge calls the parents to a preliminary hearing, trying to reach an agreement. If no agreement is reached, the district court holds a main hearing, where the parents are called to testify. Sometimes, additional witnesses are called. In custody and divorce cases, the district court usually consists of one professional judge and two lay jurors.[2] After the hearing, the district court returns a verdict that includes a legally binding custody arrangement.

The court's primary objective is to encourage parents to reach an agreement before the case proceeds to a main hearing. Once an agreement is reached, it is formalized as a court ruling and signed by the judge. In our court data, the majority of cases are resolved in accordance with the parents' agreement without reaching a main hearing.

The judge can influence the process of the case by ordering specific interventions. One common measure is requiring parents to participate in mediation. Additionally, the judge can order the municipality to conduct a formal investigation into the child's well-being and living situation. These investigations are typically carried out by a social worker who interviews both parents and the child. As part of a comprehensive assessment, the social worker often issues a recommendation for either joint or sole custody. The judge has full discretion to rule independently in a main hearing, with no formal consequences for deviating from the social worker's recommendation.

[1] Out of the courts we have obtained data from, 21 courts sent their court regulations.

[2] Each district court maintains a large pool of appointed lay jurors (*nämndemän*), who serve a role similar to juries in the American and British legal systems. They are randomly assigned to cases and typically serve around 10 to 15 days per year. Any individual over the age of 18 is eligible to become a lay juror (Ahrsjö et al. 2024).

In a custody dispute, the judge is typically involved in facilitating an agreement between parents on one or more of the following matters: (i) legal custody, which grants a parent decision-making authority over the child (e.g., school and medical choices); (ii) physical custody, which determines where the child resides and for how long; and (iii) visitation rights, which regulate the child's contact with the non-custodial parent.

While these custody-related matters are central to most disputes, alimony plays a less prominent role in Sweden compared to other countries, such as the U.S. A key reason for this is that the Swedish Social Insurance Agency manages and enforces alimony payments from the non-custodial parent to the custodial parent. As a result, there is typically no need to involve the court to claim unpaid alimony. The amount is determined by the agency based on the parents' reported incomes and expenses and is usually below 2,000 SEK per month (less than 200 US Dollars) (SFS 1996:1030). Civil disputes over alimony are relatively rare and generally arise when a plaintiff seeks a significantly higher amount due to special circumstances or a non-custodial parent petitions to stop payments under exceptional conditions.

Physical custody of the child is often determined by the structure of legal custody. A parent granted sole legal custody typically also receives physical custody, meaning the child's primary residence is with them. When parents share joint legal custody, they may also share physical custody, allowing the child to alternate between homes, or one parent may still be granted sole physical custody.

It is common practice for the court to give significant weight to the child's preferred living arrangements once the child is considered old and mature enough, usually around the age of 12. However, since this practice is not legally mandated, the court retains the discretion to override the child's preference if there are concerns that the child has been coerced by a parent.

2.3 Data

Our analysis draws on a newly constructed data set of the near universe of Swedish custody cases from 1992 to 2021. We combine the court data with Statistics Sweden's administrative records on personal background information, earnings, national test scores, and health. This section describes the construction of the court data, its linkage with administrative records, and our main dependent and independent variables.

2.3.1 Court Data

Data Construction. We construct the court data from over 100,000 custody and divorce cases from 1992 until 2021. We obtained access to the digitally stored copies of nearly all district courts through a formal request issued to the courts in the spring of

2021.[3] Courts are responsible for archiving the rulings physically or digitally after they are issued. By 2010, all courts had switched to archiving cases digitally, which allowed us to gain access to PDF copies of all Swedish custody cases—70,000 documents—decided in 45 of the 48 district courts from 2010 to 2021.[4] We complement these court records with hand-collected files starting in 1992 from the eight largest district courts in several data collection trips between 2022–2023.[5] These courts allowed us to visit their physical archives where we scanned all custody rulings starting in 1992, adding an additional 30,000 documents to the data.

To transform the text of the custody rulings into a data set we develop an algorithm in Python that relies on regular expressions, keyword search, and the Swedish version of Google's natural language processing model BERT, trained by the National Library of Sweden. We capture (i) the personal ID numbers of parents and children involved in the case, which enables us to merge the custody ruling to their administrative records, (ii) the name of the judge issuing the ruling, and (iii) which parent is assigned legal custody, physical custody, or visitation rights and if each is decided on in the case. Each is essential for the empirical analysis. We also collect contextual information about the case, including initial plaints made by the parents, information on parents' lawyers, and which interventions where ordered by the judge in the case.

Sample Restrictions. To create our baseline sample, we exclude all cases where custody is assigned not to a parent but a legal guardian. We further exclude cases that do not include either legal custody or physical custody–these are rulings that just formalize the divorce, decide only alimony payments, or dismiss one of the parent's plaints so that the previous custody arrangement remains unchanged. Since we need parents' true personal ID numbers to link them to administrative registers, we remove cases where either parent's ID is not included in the ruling or where we cannot find the recorded ID in the administrative registers. We exclude roughly 100 cases where the mother was older than 45 years at first childbirth.

We exclude special cases where a parent who have recently migrated to Sweden with children wants sole custody from a parent who is abroad and/or is unreachable. These cases are special since there is only one outcome, which is to assign sole custody to the parent in Sweden.[6] These cases are either just two pages long ('rubber stamp cases'),

[3] Of the 48 active courts, Falu, Hälsingland, and Lycksele did not share documents. Some earlier courts in our sample no longer exist, having merged into larger courts: Handen, Huddinge, Jakobsberg, Roslagen, Simrishamn, Södra Roslag, Sollentuna, and Trelleborg. In total, we include 53 courts.

[4] Some courts started switching to digital archives before 2010, in which case we have access to these PDFs as well.

[5] Attunda, Gothenburg, Helsingborg, Hässleholm, Lund, Malmö, the National Archives in Lund, Stockholm, Uppsala, Varberg.

[6] Upon arriving in Sweden, children are initially registered under the custody of both parents. However, when the parent residing in Sweden needs to make education or healthcare decisions, sole custody simplifies the process. As a result, they may file for sole custody, often without the foreign-residing parent, the defendant, being present in the proceedings. The court automatically assigns the

which is the shortest case length since the formalities of listing all parties already takes up half of the first page, or it is explicitly listed that the defendant, the parent still abroad, is unreachable. We exclude these two-page and defendant unreachable cases to capture true custody disputes involving two parents.

Finally, we use only the first case and no reopened cases for our estimation and restrict the estimation sample to cases decided by judges who handle at least 50 other custody cases during our sample period.[7] Since we include court by year fixed effects in our regressions, cases in courts and years that only have one judge presiding are excluded as well. Appendix Table B1 shows how each sample restriction impact the number of cases included in the sample. The final estimation sample consists of 11,608 cases involving 17,948 children, 11,196 fathers, 11,220 mothers, and 159 judges.

Defining Joint Custody. We combine the three dimensions of custody a judge can rule on—legal custody, physical custody, and visitation rights—to construct our measure of joint custody. We define custody to be joint when physical custody and/or legal custody are shared, where the parent without physical custody may be assigned visitation rights in a weakened form of joint physical custody.[8] Joint custody is assigned in 37 percent of the cases in our estimation sample; sole custody is assigned in the remaining 63 percent of cases. In these cases, the mother received sole custody 79 percent of the time, while the father received sole custody of the child only in 21 percent of cases.

2.3.2 Outcomes Data

We link the court data with administrative records on demographic information, yearly income, medical prescriptions, and school performance for the full population provided by Statistics Sweden. The personal identifiers of parents, children, and judges that we extracted from the custody rulings were given a unique randomized ID with which we could merge information from administrative records to each party involved in a custody case.

Demographic information together with multi-generation registers provide information on individuals' age, country of origin, highest education, and family structure (number of siblings with birth dates, parents' marital- and cohabiting status). We merge this

defendant a legal representative, who attempts to make contact, often through an embassy. In many cases, these efforts are unsuccessful, and the defendant is deemed "unreachable".

[7] Reopened cases are cases where one parent issues a new plaint to the district court after the initial ruling to change parts of the custody ruling. As the verdict in this reopened case by definition depends on the initial ruling, we don't include these reopened cases in our IV estimation.

[8] When custody is assigned to be joint, both legal and physical custody are joint in one third of cases. Joint physical custody entails the child alternating between both parents' homes, most commonly spending one week with the mother and the next week with the father. In the remaining two thirds of cases, legal custody is shared but children don't go back an forth between parents. Instead, one parent is assigned sole physical custody and the other parent visitation rights, which often entails the child living with one parent during the week and staying with the other parent on weekends or every other weekend.

information to our court sample from 1990, two years before the first court case in our data, until 2022, the most recent year available. In a few court cases, the ID numbers of either a parent or a child are not included in the text of the custody ruling, either for security reasons or because parents asked for their child's ID number to be removed from the text. We find these missing ID numbers using the multi-generation registers.[9] We use data from crime registers containing convictions between 1985–2016 to control for parents' criminal status before the custody ruling.

Register data on earnings are available yearly for all Swedish residents from 1968–2022. To estimate parents' position in the national earnings distribution after a custody ruling, we calculate which age and gender-adjusted earnings percentile parents fall into each year for five years after the custody ruling. Specifically, we divide the earnings of all men and women of a certain age into quartiles (bottom 0–25%, 25% – 50%, 50% – 75%, and top 75% of earnings) and assign each individual the quartile group they belong to based on their earnings in that year. This means a 40-year-old father in our sample who earns above the median in 2015 does so relative to all 40-year-old men in Sweden with any positive earnings in 2015. Earnings are inflation-adjusted with 2020 as the base year and have been converted from Swedish Krona to US Dollars.

Health records come from administrative data on medication prescriptions from 2004–2022. The two main health outcomes we study are antidepressant and anti-anxiety prescriptions.[10] We identify if individuals have a prescription for each medication in a given year after the custody ruling. We also measure if individuals are on the medication continuously, that is, having a prescription for the medication for each year following the custody ruling.

In Sweden, students take national performance tests in grades 3, 6, and 9. To measure children's educational attainment we use register data on the performance on these tests, which is available from 2009–2023 for grade 3, from 2012–2023 for grade 6, and from 2003–2023 for grade 9 and includes scores or a pass/fail indication for each subject, a school identifier, school type, and school year in which the test was taken. We use data on all students in the different grades starting in 2012, when data on all scores are available, and standardize their math and Swedish scores within each year.[11] Since students don't take national tests each year, we don't have a measure of school performance each year for five years after the ruling as we do for health and earnings measures. Instead, we

[9] For children, we impute the IDs in the case of only children. For parents, we impute the ID of the other biological parent when we have the ID of the one parent and the child.

[10] We use ATC codes N06A for antidepressants, N05B and N05C for anti-anxiety medication, and N06B for ADHD medication.

[11] Scores are standardized to be mean 0, standard deviation 1. The tests have different sub-tests across years. We use grades on overall Swedish and math performance ("sv_provb" and "ma_provb") where available. Some years don't have a score on the overall grade but only scores for each sub-test. When this is the case we standardize the scores for each sub-test by year, take the mean of all sub-tests to get an overall score, and standardize the overall score again within the year.

Table 2.1: Descriptive Statistics of Individuals in Estimation Sample

	Mean	SD
Case specific characteristics		
Plaintiff is female	0.68	0.47
Child female	0.49	0.50
Mother age	36.42	6.90
Father age	40.45	7.98
Child age	7.92	4.14
Number of children in case	1.97	1.03
Parents cohabiting 2 years before ruling	0.36	0.47
Parental background characteristics		
Positive earnings 2 years before ruling, mother	0.90	0.30
Log(earnings) 2 years before ruling, mother	8.60	3.10
Positive earnings 2 years before ruling, father	0.89	0.31
Log(earnings) 2 years before ruling, father	8.95	3.22
Father education level: Compulsory	0.25	0.43
Father education level: High School	0.47	0.50
Father education level: College	0.23	0.42
Mother education level: Compulsory	0.24	0.43
Mother education level: High School	0.45	0.50
Mother education level: College	0.27	0.44
Child foreign born	0.08	0.27
Mother foreign born	0.42	0.49
Father foreign born	0.46	0.50
Crimes committed 2 years before ruling, father	0.47	0.49
Crimes committed 2 years before ruling, mother	0.21	0.39
Observations	17948	

Notes: This table shows descriptive statistics on child and parent characteristics. The estimation sample consists of children identified in the court data after applying relevant restrictions. Depicted are means and standard deviations of the balancing variables, which are included in the test for random assignment of custody cases to judges and all regressions. The data come from multiple administrative records provided by Statistics Sweden.

determine the first, second, and third tests taken after the custody ruling to track how the impact of joint versus sole custody on students' performance changes over time.

2.3.3 Summary Statistics

Table 2.1 provides descriptive statistics for the estimation sample. The first couple of rows present the means and standard deviations of key case-specific characteristics. In 68 percent of cases, the plaintiff is female, indicating that mothers most often submit petitions to the court to initiate civil disputes. Fathers are, on average, around 40 years old at the time of the case, mothers 36 years old, and the child a little under 8 years old. Each case involves an average of 1.97 children, meaning that civil custody disputes commonly concern siblings. In 36 percent of cases, parents were cohabiting two years before the ruling. The fact that the majority were not suggests that establishing a court-ordered custody arrangement takes time: Custody cases are usually resolved only after the divorce is finalized, which follows a six-month court-mandated probation period.

Mothers and fathers are similar in terms of earnings and years of education. Around 90 percent of both mothers and fathers were employed in some capacity (as indicated by having positive earnings) two years before the ruling. Log earnings, measured two years before the ruling, average 8.6 for mothers and 8.95 for fathers. Most commonly, both mothers and fathers have at most a high school education. Around 23 percent of fathers and 27 percent of mothers have attended college.

Certain features of the estimation sample are overrepresented compared to the total population. In approximately 45 percent of cases, both mothers and fathers are foreign-born, which is higher relative to the share of foreign-born individuals aged 30–49 in Sweden.[12] Additionally, 21 percent of mothers and 47 percent fathers who have been convicted of a criminal offense at any point in life, measured two years before the ruling. This can be any type of crime that shows up in our crime data. Some common ones are assault, crime against public order, drunk driving, intimidation, drug, fraud, traffic aggravation.

2.4 Empirical Strategy

We are interested in the effect of having joint custody after divorce on children's and parents' outcomes. The starting point is the following OLS regression:

$$Y_{it} = \beta Joint_{it} + X_{it}'\delta + \varepsilon_{it} \tag{2.1}$$

where β is the coefficient of interest, $Joint_{it}$ is an indicator for whether joint custody is assigned to the family post-divorce, as opposed to the one parent having sole custody, X_{it} is a vector of controls, and Y_{it} is the dependent variable of interest for family member i in time t. The causal interpretation of the OLS estimate is likely to suffer from selection bias, as more cooperative parents—often with higher socioeconomic status—are more likely to have joint custody. Their children may benefit from advantages unrelated to the custody arrangement itself, making it difficult to isolate its true effect.

To address this issue, we exploit the random assignment of judges to custody cases and the fact that judges systematically differ in their propensity to grant joint custody. This means that, within the same court and year, families with identical background characteristics may receive different custody outcomes simply because they were assigned

[12] According to publicly available population data from Statistics Sweden, the average share of foreign-born men and women in this age group was around 21–22 percent between 2000 and 2021.

different judges. We can utilize the random assignment of judges as an instrument in a two-stage Instrumental Variables (IV) design to estimate the causal effect of being assigned joint custody. The first stage regression can be written as:

$$Joint_{it} = \alpha Z_{ji} + X_{it}'\theta + \nu_{it} \tag{2.2}$$

where Z_{ji} denotes the propensity to assign joint custody of judge j who handled the case of family member i. The instrument Z_{ji} is a leave-out measure of the judge's propensity to assign joint custody based on all past and future custody rulings, excluding all rulings involving any family member of the focal case.[13]

The prediction of the treatment variable $Joint_{it}$ from the first stage regression is used as the regressor in the second stage regression in equation 2.1. Since the random assignment of judges happen within a court and year, both stages of regression include court-by-year fixed effects. We cluster the standard errors at the court case level, since one case can include multiple siblings who may all occur in our estimation sample.

2.4.1 Conditional Independence

Our research design relies on the random assignment of cases to judges. To assess this, Appendix Table B2 presents a balance test. Column (1) examines whether background characteristics, measured two years before the custody ruling, predict the probability of being assigned joint custody. Factors such as the mother being the plaintiff, the ages of the mother and child, criminal history, and the father's earnings and education are all significant predictors of the custody outcome, each reducing the likelihood of obtaining joint custody in court.

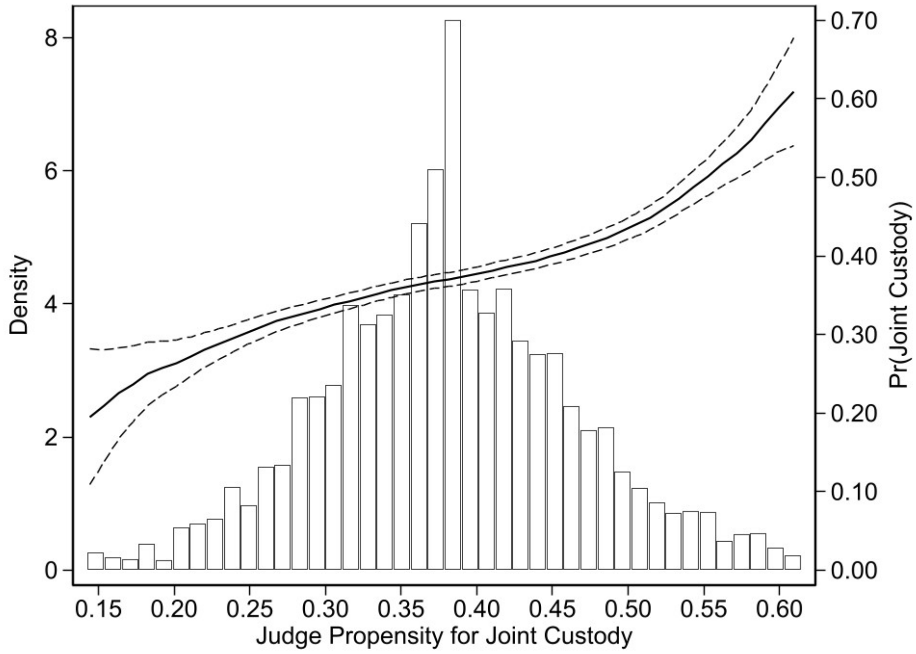
Next, we assess whether the same background characteristics predict judge propensity. Column (3) regresses judge propensity on these characteristics. Here, the estimates are closer to zero and jointly statistically insignificant (p-value = 0.8694), suggesting no systematic relationship between background characteristics and judge assignment. These findings confirm that while background characteristics strongly predict case outcomes, they are uncorrelated with our instrument, supporting the validity of the random assignment.

2.4.2 Instrument Relevance

The IV design depends not only on the random assignment of judges, but also on sufficient variation in judges' propensities to assign joint or sole custody. To assess this, the histogram in Figure 2.1 shows the distribution of judges propensities to assign joint

[13] This means we leave out the custody ruling of the child in question and their siblings in the focal case. Some parents are involved in multiple custody cases with different co-parents. We leave out the rulings issued in these cases for the child's half-siblings as well.

Figure 2.1: First-stage Graph of Joint Custody on Judge Propensity



Notes: This figure depicts the first-stage relationship between cases that where assigned joint custody and judge propensity. The estimation sample consists of children in the court data after applying relevant restrictions. The right Y-axis shows the probability of joint custody being assigned in the case, the X-axis shows the leave-out measure of judge propensity for joint custody. Plotted values are mean-standardized residuals from regressions on court-by-year fixed effects. The solid line shows a local linear first-stage regression of joint custody on judge propensity and court-by-year fixed effects. Dashed lines give the 95 percent confidence intervals. The histogram shows the density of judge propensity plotted on the left Y-axis. Top and bottom 1 percent of the distribution are excluded.

custody, controlling for fully interacted court and year dummies. The propensity scale is defined on the X-axis, where a higher value indicates a higher propensity to assign joint custody. The histogram shows substantial variation in judges' propensities to assign joint custody. The mean of the instrument is 0.37, with a standard deviation of 0.12. This means that judges, on average, assign joint custody in 37 percent of the cases they handle. Judges who are most likely to assign joint custody do so in around 60 percent of cases, while those who are least likely assign it in fewer than 15 percent.

Figure 2.1 also plots the probability that a family is assigned joint custody in the current case as a function of whether they are assigned to a judge that leans toward assigning joint or sole custody. The solid line plots estimates from a local linear regression, representing a flexible analog of the first stage in equation 2.2. The probability to be assigned joint custody is monotonically increasing in the judge's propensity of assigning joint custody. Appendix Table B3 presents the first-stage estimate from equation 2.2. The large F-statistics confirm the instrument's strength in predicting custody rulings. The significant coefficient in Column (2) shows that a family has a 6.17 percentage point higher probability of being assigned joint custody when a judge with a 10 percentage point higher propensity for joint custody was assigned to their case.

2.5 Results

This section presents the main findings from our IV design. First, we evaluate the impact of joint custody on children's school performance and mental health. Next, we assess outcomes for fathers and mothers, focusing on earnings and mental health.

2.5.1 Effect of Joint Custody on Children

Parental separation and the breakup of the family unit can be one of the most impactful events in a child's life. Joint custody may serve as a stabilizing factor during this disruptive period, enabling the child to maintain a strong emotional bond with both parents and turn to them for support and advice (Låftman et al. 2014). We estimate the impact of joint custody on children's educational achievement and find that children significantly benefit from joint custody.

Table 2.2 presents the effect of joint custody on children's performance on the first national test taken after the custody ruling. Column (1) shows the effect on average test score (Swedish and math scores combined). On average, joint custody leads to an improvement in student performance by 56 percent of a standard deviation. This is a large effect relative to the mean performance of children involved in a custody dispute, which is 0.287 of a standard deviation below the full population mean. The positive effect of joint custody on test scores is slightly larger for math scores (60.3 percent of a standard deviation, shown in column (3)) than for Swedish scores (51.8 percent of a standard deviation, shown in column (2)).

We examine heterogeneity based on the child's age at the time of the custody ruling and find young children to be most affected. Appendix Table B4 splits the estimates for test scores on the first national test taken after the ruling by student grade. For children who take the test at the end of grade 3, when they are on average 10 years old, joint custody improves test scores by 68.1 percent of a standard deviation. Children for whom the first test after the custody ruling takes place at the end of grade 6 are similarly affected in magnitude (66.3 percent of a standard deviation) but the effect is no longer statistically significant as the sample size decreases. Children in the Grade 3 estimation sample may have experienced the custody ruling at any age between 0 and 10, whereas those in the Grade 6 estimation sample are included only if the custody ruling occurred between third and sixth grade, typically between ages 10 and 13. The estimates for students in ninth grade, who are on average 16 years old, are much reduced and also not statistically significant. Our results suggest that young children benefit most from joint custody. This result is in line with younger children being more vulnerable and reliant on

Table 2.2: Effect of Joint Custody on Children's National Test Scores

	(1) First Test After Ruling Average Score	(2) First Test After Ruling Swedish Score	(3) First Test After Ruling Math Score
Joint Custody	.560**	.518*	.603**
	(.238)	(.265)	(.258)
Observations	8621	8621	8621
Mean of Dependent Variable	-.287	-.254	-.320

Notes: The table presents IV estimates of joint custody on children's national test scores at any point after the ruling. Average score refers to average of Swedish and Math scores. Only children who have both scores are included in the estimation. Mean of the Dependent Variable shows the sample mean of children included in the estimation sample, test scores were standardized (mean 0, SD 1) on the full population of test takers. All estimates control for variables listed in Appendix Table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

parental support and the greater impact of parental investments at an early age (Cunha and Heckman 2007).

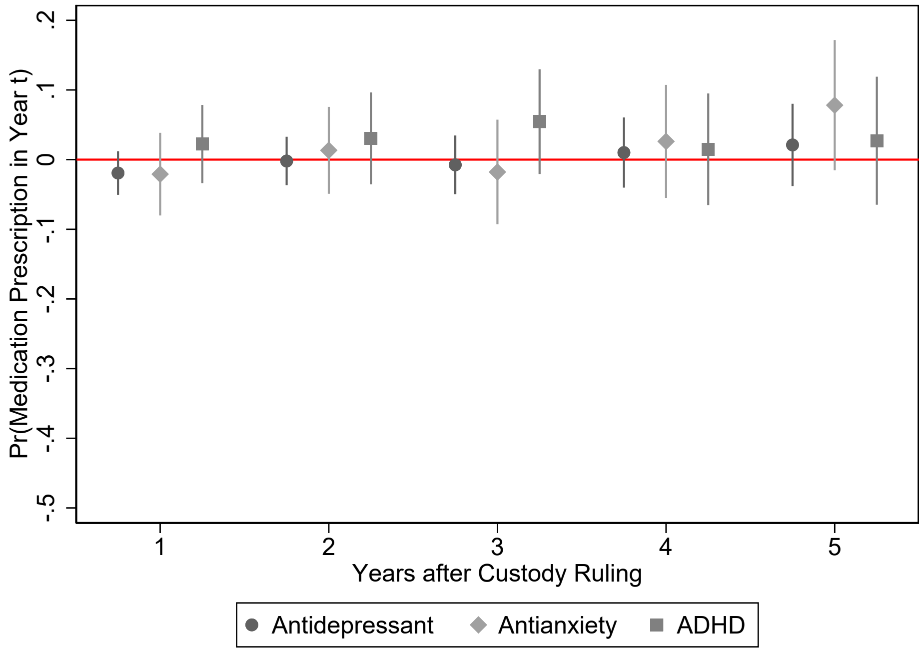
Children in a joint custody family also start at higher-quality primary schools. Appendix Table B5 shows the impact of joint custody on the quality of schools that children attend. Joint custody improves the quality of schools children attend in grade 3 by 0.368 points of our residual school quality measure, which represents a 1.04 standard deviation increase in school quality. In Sweden, parents can choose the school for their child and children most commonly switch school for junior high school, which goes from grade 6 to grade 9. The positive impact of joint custody on residual school quality in grade 3 suggests that joint custody allows parents, either through a heightened focus or through a wider network, to make a better choice for the school at which their child starts primary school. Again, young children are most affected. Once children switch to junior high school, the effect on residual school quality is no longer significant.

Next, we consider the effect of joint custody on children's mental health. We find little evidence for an impact of joint custody on using mental health medication for children. Figure 2.2 depicts this for the probability of being prescribed any antidepressant, anti-anxiety or ADHD-related medications. Panel (a) shows that the probability to be prescribed medication at any point five years after the ruling is close to zero and statistically insignificant. Panel (b) shows that the there is also no effect on the probability to have a continuous medical prescription during the five-year period.

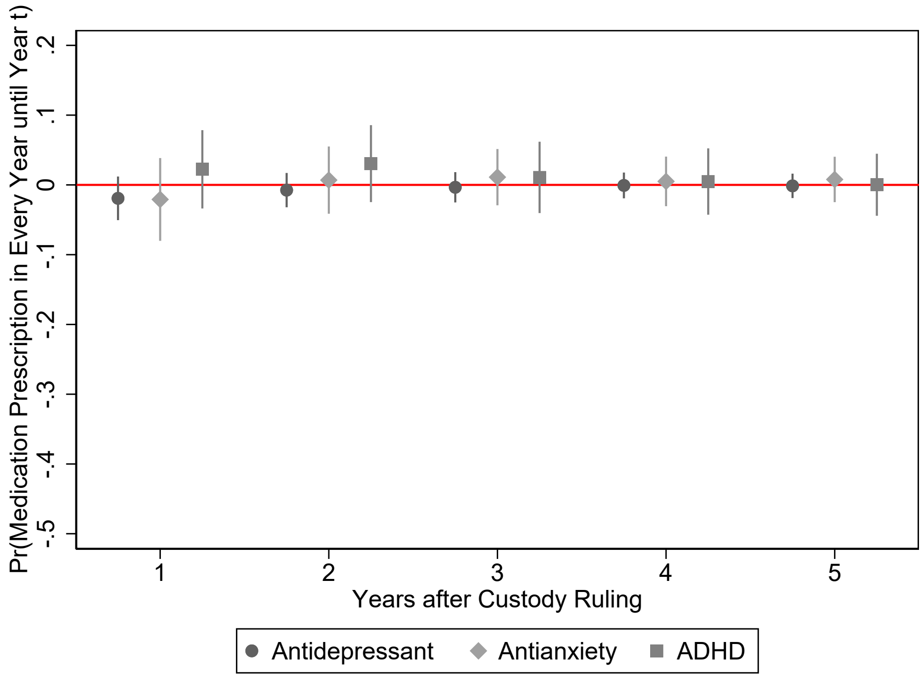
Appendix Table B6 presents the point estimates. While antidepressant and anti-anxiety medications relate to the most common mental health diagnoses in the general population, for children they are prescribed relatively rarely. Only 0.91 percent of children take antidepressants after a custody ruling, which increases to 3.09 percent for older children 5 years after the ruling but still remains low. For ADHD prescriptions, which are more common among children, we find a slight positive impact of joint custody, however the estimates are not statistically significant.

The suggestive evidence for a positive impact of joint custody on ADHD medication could be related to children displaying more disruptive behavior in class. However, it

Figure 2.2: Effect of Joint Custody on Children's Mental Health Medications



(a) Year-by-Year



(b) Continuous

Notes: Panel (a) depicts the effect of joint custody on the probability of children having a prescription at any point 1 to 5 years after the ruling. Panel (b) depicts the effect of joint custody on the probability of children having a continuous prescription for up to five years after the ruling. Symbols represent coefficients from the IV regression, which control for court-by-year fixed effect and all variables listed in Appendix Table B2. Standard errors are clustered on the case level. Lines depict 95% confidence intervals.

could also indicate a positive development in children's mental health if reflective of early diagnosis and health seeking behavior by the parents, which can significantly reduce the risk of developing a depression (Chang et al. 2016). Together with the positive impact that we find for joint custody on student's school performance, we interpret these suggestive results to indicate more attention on behavioral changes and in turn higher treatment rates of ADHD in children, rather than worsened mental health.

2.5.2 Effect of Joint Custody on Fathers

For fathers, a lot is at stake in a custody dispute. Joint custody allows them to continue sharing the responsibility for the child with the mother and remain involved in the child's live. In the counterfactual scenario of sole custody, they lose all access to their child in 79 percent of cases. We find strong and lasting earnings increases for fathers under joint custody.

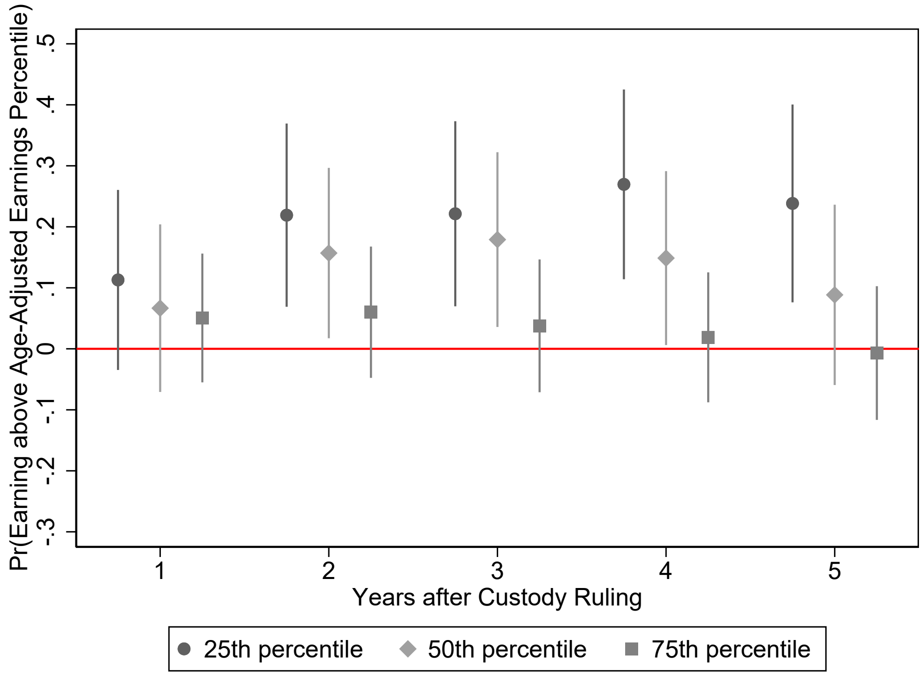
Figure 2.3 shows joint custody increases fathers' probability to earn above the 25th percentile and the median (relative to the population-wide age reference group) starting in the second year after the custody dispute and persisting until 5 years after the custody ruling.[14] Fathers are 21.9 percentage points more likely to earn above the 25th percentile in the second year after the custody ruling. The effect slightly increases in the third year to 22.1 percentage points, peaks at 27 percentage points in the fourth year, and remains strong at a 23.8 percentage point increase even in the fifth year after the custody ruling. The positive impact on earnings is economically significant: the estimates constitute an increase relative to the mean of 41.1 percent in year 2, 41.6 percent in year 3, 49.6 percent in year 4, and 43.8 percent in year 5.

Joint custody leads to strong and lasting earnings increases for lower earning fathers. Relative to sole custody, where most fathers lose access to their child, joint custody provides fathers with the continued responsibility and stability of maintaining a relationship with their child. This also implies a financial constraint: joint custody may induce fathers to consider providing for their child when they search for a new apartment post-divorce and when they assess new work opportunities.

In line with this interpretation, we find similar positive effects of joint custody on fathers' probability to earn above the median. In Figure 2.3 we see a positive but statistically insignificant effect in the first year after ruling before the estimates become strong and both economically and statistically significant. In year two, joint custody increases fathers' probability to earn above the median by 15.7 percentage points (54.5 percent increase relative to the mean). The effect peaks in year three at 17.9 percentage points (61.1 percent increase relative to the mean) and before leveling off in year four to 14.9 percentage points (50 percent increase relative to the mean). We find suggestive evidence

[14] Appendix Table B7 provides point estimates for the results.

Figure 2.3: Effect of Joint Custody on Fathers' Earnings



Notes: The figure depicts the effect of joint custody on probability that fathers' earnings are above the earnings percentile of the same gender and age reference groups in the full Swedish population 1–5 years after the ruling. Symbols represent coefficients from the IV regression, which control for court-by-year fixed effect and all variables listed in Appendix Table B2. Standard errors are clustered on the case level. Lines depict 95% confidence intervals.

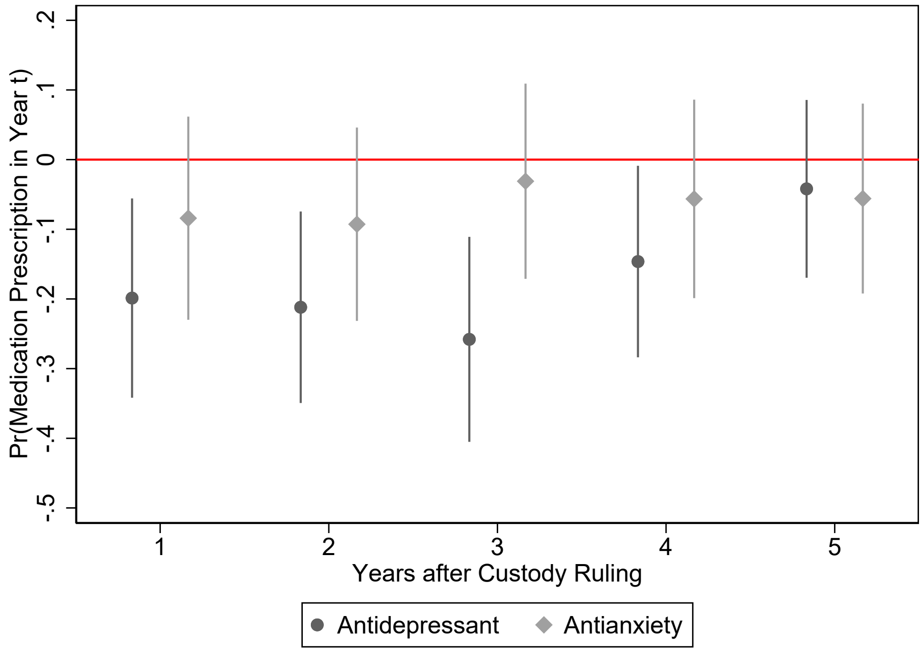
of a continued impact in year five, where the coefficient is still positive but no longer statistically significant.

For the highest income fathers, at the 75th percentile, we find little evidence that joint custody further improves earnings. The effect of joint custody on fathers' likelihood of reaching the top earnings quartile is marginally positive but not statistically significant in any of the five years following the custody ruling. The results suggest high-earning fathers are less affected by the financial responsibility that comes with obtaining joint custody for their child. The custody ruling, where fathers risk losing access to their child, can fundamentally reshape fathers' lives. While joint custody allows all fathers to remain involved in their child's development, the associated financial responsibilities impact lower-earning fathers most.

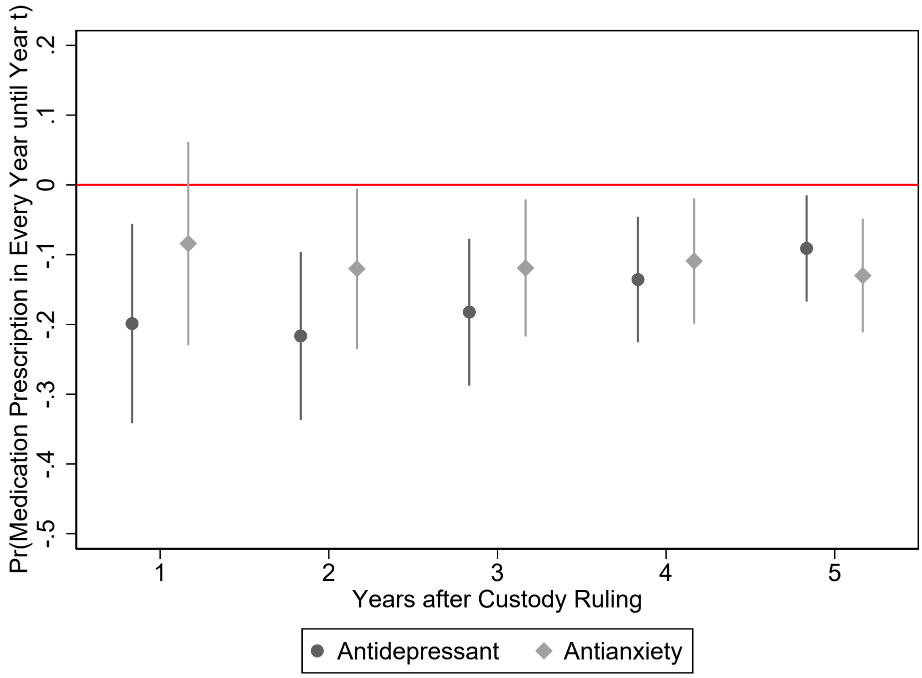
When sole custody implies losing access to their child for most fathers, joint custody can significantly impact fathers' well-being beyond its effect on earnings. We estimate the impact of joint custody on fathers' mental health to provide a holistic measure of well-being. Figure 2.4 depicts the results.[15] Panel (a) shows the effect of joint custody on the probability of fathers taking medications for mental health in any of the first

[15] Appendix Table B8 provides point estimates for the results.

Figure 2.4: Effect of Joint Custody on Fathers' Mental Health Medications



(a) Year-by-Year



(b) Continuous

Notes: Panel (a) depicts the effect of joint custody on the probability of children having a prescription at any point 1 to 5 years after the ruling. Panel (b) depicts the effect of joint custody on the probability of children having a continuous prescription for up to five years after the ruling. Symbols represent coefficients from the IV regression, which control for court-by-year fixed effect and all variables listed in Appendix Table B2. Standard errors are clustered on the case level. Lines depict 95% confidence intervals.

five years after the custody ruling. Joint custody decreases the probability of fathers taking antidepressant medication by 19.9 percentage points in the first year after the custody ruling. The effect increases to 21.2 percentage points in the second year and 25.8 percentage points in the third year, before leveling off again to 14.6 percentage points in the fourth year.

The significantly lower reliance on antidepressant and anti-anxiety medications among fathers with joint custody suggests that maintaining parental involvement after separation is crucial for fathers' mental well-being. Joint custody reduces the need for psychiatric medication immediately after the custody ruling and in the medium term.

Panel (b) in Figure 2.4 show the results from estimating the impact of joint custody on the most severe cases of depression and anxiety, by estimating the impact of joint custody on taking each drug for 2–5 continuous years. Joint custody decreases the probability of requiring continuous treatment for both depression and anxiety even more sharply. Fathers are less likely to take antidepressants for 2 (3, 4, 5) continuous years by 21.2 (18.2, 13.6, 9.1) percentage points. While the year-by-year results only showed suggestive evidence of a moderate effect of joint custody on prescriptions for anti-anxiety medication, the impact on the most severe cases is significant and strong. Fathers with joint custody are 12 (11.9, 10.9, 13) percentage points less likely to take anti-anxiety medication continuously for 2 (3, 4, 5) years after the custody ruling.

The sharp reduction in prescriptions for mental health medications, particularly for those requiring continuous, long-term treatment, indicates that maintaining parental involvement significantly alleviates psychological distress compared to the counterfactual fathers face, where most lose access to their child. Together with the improvement in earnings, our results show joint custody has pronounced positive impact on fathers' lives long-term.

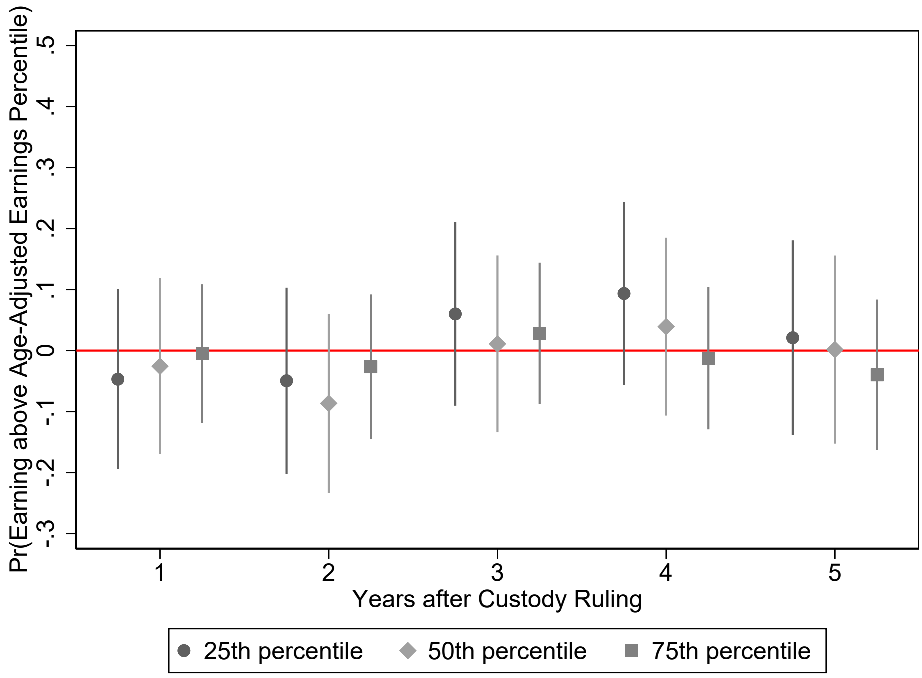
2.5.3 Effect of Joint Custody on Mothers

For most mothers, custody disputes carry different implications than for fathers: when sole custody is awarded, it is granted to the mother in 79 percent of cases. Hence, for mothers, the typical custody dispute determines the extent to which they engage with their child and ex-partner, rather than posing the risk of losing access to the child entirely.

We repeat the analysis of joint versus sole custody on mothers' earnings and find no significant effect. Figure 2.5 depicts the results.[16] The estimates for mothers' probability to earn above the 25th and 50th percentiles are negative for the first two years after the custody ruling before they turn slightly positive in years 3, 4, and 5 but are all statistically insignificant. Estimates for mothers' probability to earn above the 75th percentile remain close to zero and show no clear trend.

[16] Appendix Table B9 provides point estimates for the results.

Figure 2.5: Effect of Joint Custody on Mothers' Earnings



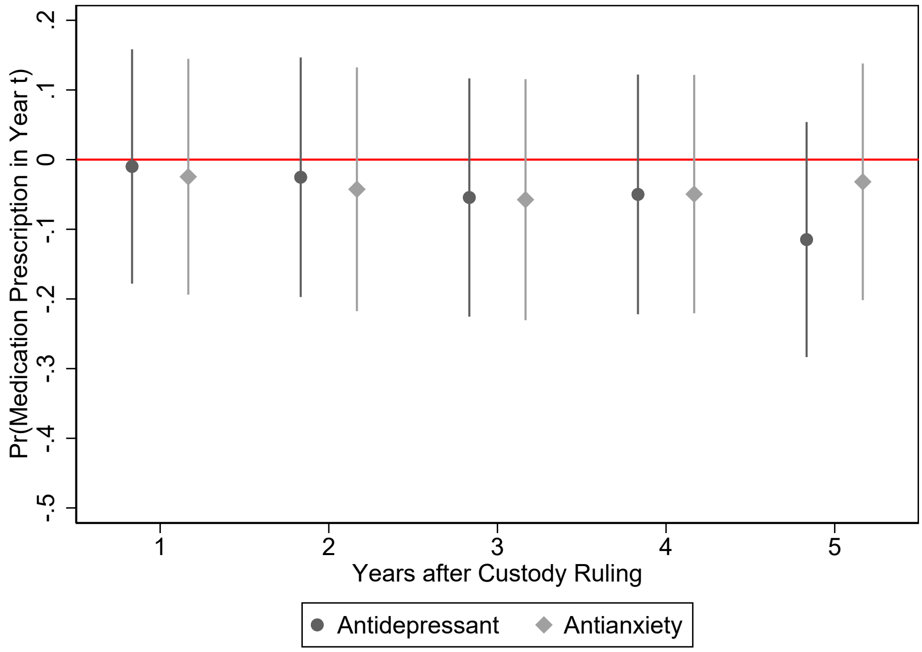
Notes: This figure depicts the effect of joint custody on probability that mothers' earnings are above the earnings percentile of the same gender and age reference groups in the full Swedish population 1–5 years after the ruling. Symbols represent coefficients from the IV regression, which control for court-by-year fixed effect and all variables listed in Appendix Table B2. Standard errors are clustered on the case level. Lines depict 95% confidence intervals.

We also consider the impact of joint custody on mothers' mental health. Again, we find little evidence that joint custody significantly impacts mothers' psychological distress compared to sole custody. Figure 2.6 depicts the effect of joint custody on mothers' probability of taking antidepressant or anxiety medication.[17] Panel (a) shows the effect on mothers' probability of taking medication in the first 5 years after the custody ruling. While not statistically significant, the estimates are consistently negative for both antidepressants and anti-anxiety medications across all years following the custody ruling. This provides some suggestive evidence that joint custody may reduce the likelihood of mothers taking mental health medication. Panel (b) shows the results for continuous mental health treatment to capture the impact on most severe psychological conditions. Also here, all estimates are statistically insignificant and close to zero.

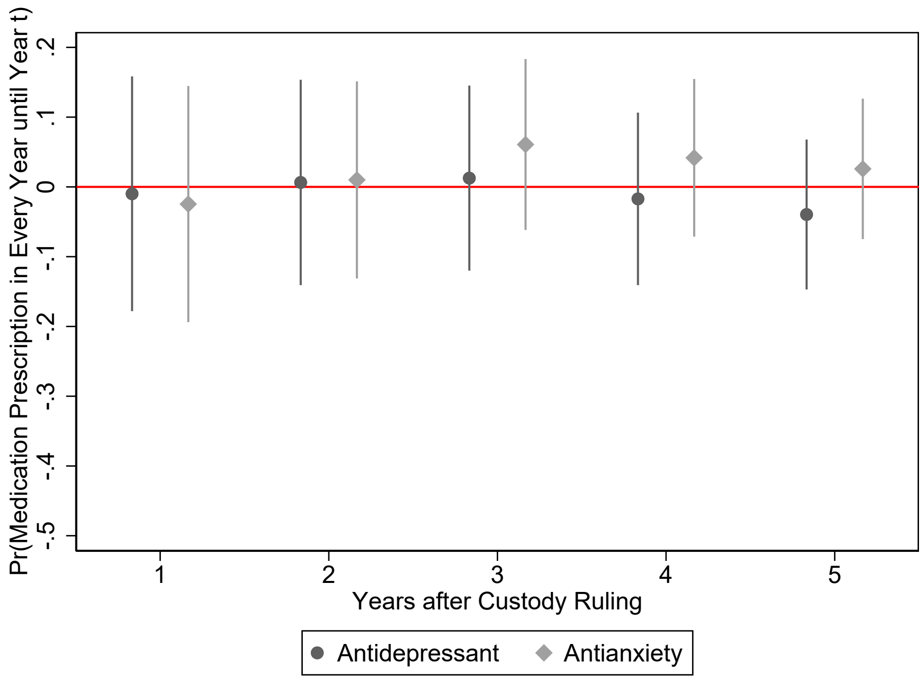
Although we do not observe the same positive effect of joint custody on mothers as we do for fathers, we also find no evidence of a negative impact. This may reflect that for most mothers, the difference between joint and sole custody is about the degree of involvement, rather than a question of access to the child.

[17] Appendix Table B10 provides point estimates.

Figure 2.6: Effect of Joint Custody on Mothers' Mental Health Medications



(a) Year-by-Year



(b) Continuous

Notes: Panel (a) depicts the effect of joint custody on the probability of children having a prescription at any point 1 to 5 years after the ruling. Panel (b) depicts the effect of joint custody on the probability of children having a continuous prescription for up to five years after the ruling. Symbols represent coefficients from the IV regression, which control for court-by-year fixed effect and all variables listed in Appendix Table B2. Standard errors are clustered on the case level. Lines depict 95% confidence intervals.

2.6 Conclusion

Custody arrangements play a crucial role in shaping parents' and children's lives post-divorce. While many Western countries have shifted toward promoting joint custody, little is known about the causal effects of joint versus sole custody due to limited data availability and selection bias.

We address these challenges by analyzing nearly all Swedish custody cases from 1992 to 2021, leveraging random assignment of cases to judges who systematically differ in their propensity to assign joint custody. We use a leave-out measure of judges' propensity scores to instrument court-ordered custody arrangements. The data includes hand-collected custody rulings from the Swedish district court system, combined with detailed administrative data on family background, education, earnings and health.

Our analysis reveals that joint custody improves children's standardized test scores and increases their likelihood of attending high-quality primary schools, without affecting mental health. However, the most pronounced effects are for fathers. Joint custody significantly improves their economic outcomes and reduces their likelihood of requiring antidepressants or anti-anxiety medication. In contrast, joint custody has no significant impact on mothers' earnings or mental health. These findings suggest that joint custody improves fathers' well-being and economic stability likely by enabling them to remain actively involved in their child's life. In 79 percent of sole custody rulings, father loose all access to their child. For mothers, the distinction between joint and sole custody primarily reflects changes in ex-partner involvement rather than access to the child. This does not seem to have any significant influence on her mental health or economic situation.

Chapter 3

Linguistic Distance and the Gender Gap in Education*

Abstract: This study examines how linguistic distance between host and origin languages influences the migrant gender gap in academic performance. Linguistic distance highlights an overlooked factor: girls' advantage in early language learning. Using cross-country PISA data and an epidemiological approach, I show that the gender gap in reading and math test scores widens in favor of girls among fifteen-year-old migrants from countries with less similar languages to the host country. Linguistic distance does not have an impact on the gender gap for second-generation migrants born in the host country. In contrast, cultural proximity in gender norms poses a significant disadvantage for second-generation migrant girls in terms of both reading and math scores. While distances in linguistic and cultural gender norms both reflect cultural proximity, they affect the academic gender gap of first- and second-generation migrant children differently.

* I thank Leonie Alewell, Niklas Boehm and Johanna Lehner for excellent research assistance.

3.1 Introduction

In most Western countries, migrant children perform below native students academically, posing a significant barrier to their economic and social integration (Dustmann et al. 2010). Notably, however, migrant girls tend to excel academically, often doing better in school than native boys (OECD 2021). Compared to migrant boys, migrant girls not only receive higher grades but also seem to benefit more from interventions aimed at improving migrants' academic performance (Hajj and Panizza 2009; Antman 2012; Drange and Telle 2015; Meschi and Pavese 2023).

Several studies have examined the migrant gender gap in education and the labor market by exploring the role of cultural origin, using measures of cultural proximity between migrants' origin and host countries (Fernández and Fogli 2009; Nollenberger et al. 2016; Hauge et al. 2023; Holmlund et al. 2023; Huber and Paule-Paludkiewicz 2024). For students from cultures with more traditional gender roles, the gender gap tends to widen or narrow in favor of boys, especially in math. This body of research enhances our understanding of how cultural backgrounds influence the experiences of migrant girls and women in the host country. However, it does not account for the policy-relevant observation that, on average, migrant girls tend to outperform migrant boys academically. Meanwhile, there is a notable lack of research on the underlying reasons for this gap.

This study is the first to examine how linguistic distance between a migrant's origin and host country affects the migrant gender gap in academic performance. The measure quantifies the similarity or difference between two languages, based on language family trees (Fearon 2003; Guarnieri and Tur-Prats 2023). Not only does it have the potential to reflect the cultural proximity between cultures, but also the difficulty for speakers of one language to become proficient in another (Isphording and Otten 2013; Adsera and Pytlikova 2015; Wong 2023).

We have a limited understanding of how linguistic distance affects the educational outcomes of migrant girls and boys. If linguistic distance merely reflects cultural proximity, one might expect, based on previous literature, that girls are more disadvantaged when coming from countries with greater cultural distance. However, if it primarily captures the difficulty of acquiring proficiency in the host-country language, girls could actually outperform boys. Research in both linguistics and economics suggests that girls have a relative edge in early language learning compared to boys (Van Der Slik et al. 2015; Rinaldi et al. 2023), which could make learning a second language with fewer linguistic similarities to their first language less challenging.

To investigate this, I apply an epidemiological approach which measures how the gender gap in academic performance widens or narrows by the linguistic distance between origin and host country languages. I use data from the 2003–2022 Program for International Student Assessment (PISA), which measures 15-year-olds' cognitive skills across the world. Importantly, this dataset includes detailed information on students and parents' migration histories. This allows me to analyze how linguistic distance influences the gender gap in PISA reading and math test scores for both first- and second-generation migrant children while accounting for a broad set of background characteristics. By adding country-by-origin fixed effects, the analysis compares the gender gap for girls and boys within a PISA test country who have migrated from the same origin country.

I present novel findings that girls have an advantage over boys in acquiring a second language after migration, which in turn enhances their academic performance. A one-standard deviation increase in linguistic distance widens the gender gap in favor of first-generation migrant girls by around 9 percent in both reading and math, after accounting for age at arrival. These effects remain robust to controlling for origin-country GDP, as well as parental and school characteristics. While linguistic distance poses challenges for skill development in the host-country education system, the results consistently demonstrate that girls navigate these challenges more effectively than boys. This advantage is evident not only in reading but also in math performance, despite math being less language-intensive.

In contrast, linguistic distance does not have a significant influence on the gender gap in test scores for second-generation migrants, who were born in the host country to two migrant parents. Hence, the influence of linguistic distance seems to weaken across generations, likely since second-generation migrants are exposed to the host-country language from birth. Meanwhile, first-generation migrant children must acquire the language at older ages, often after entering compulsory schooling. Girls appear to have an advantage when second-language acquisition requires more active learning during childhood.

To further support the role of linguistic distance in explaining these patterns, I compare it with other measures of cultural proximity. Specifically, I replicate the analysis using a measure of gender inequality in the host and origin countries, a standard approach in the literature on cultural norms and gender gaps in education. The results reveal that cultural proximity, when measured by gender norms, has the opposite effect on the migrant gender gap in two distinct ways. First, the measure does not influence the gender gap in reading and math scores for first-generation migrants but has a considerable impact on second-generation migrants. Second, greater differences in gender norms between the host and origin countries disadvantage girls, whereas greater linguistic distance, in contrast, benefits them.

These findings suggest that while both linguistic distance and cultural gender norms reflect aspects of cultural proximity, they shape the academic gender gap of first- and second-generation migrant children in distinct ways. Linguistic distance, in particular, appears to capture girls' advantage in early language acquisition. This is crucial for understanding why migrant girls tend to outperform boys academically and provides valuable policy insights into how language acquisition among migrant children shapes educational outcomes.

This study contributes to the literature on the role of language in migrants' academic and labor market outcomes. Proficiency in the host-country language is widely recognized as a key component of human capital and a crucial determinant of migrants' earnings (Dustmann and Van Soest 2002; Dustmann and Fabbri 2003; Bleakley and Chin 2004; Chiswick and Miller 2015). However, whether language proficiency affects men's and women's labor outcomes differently remains less clear. While Dustmann and Van Soest (2002) finds similar effects for both genders, Yao and Van Ours (2015) suggests that lower language proficiency disproportionately harms female migrants' wages. Investing in host-country language training has been shown to improve labor market outcomes across countries (Lochmann et al. 2019; Arendt 2022; Lang 2022; Heller and Mumma 2023; Foged et al. 2024). Proficiency in the origin-country language plays a crucial role in shaping ethnic enclaves, which, in turn, influences both employment opportunities (Edin et al. 2003) and access to institutional knowledge (Bertrand et al. 2000). However, living in an ethnic enclave can also hinder host-country language acquisition, particularly for female migrants (Laliberté 2019) and children of migrants (Danzer et al. 2022). Studies on migrant children's language proficiency often use the age at migration as a key factor in language acquisition (Bleakley and Chin 2004; Böhlmark 2008; Clarke and Isphording 2017), showing that younger arrivals tend to achieve better educational and health outcomes, largely due to stronger language proficiency. Additionally, migrant parents' language skills are a significant factor in shaping their children's proficiency in the host-country language (Casey and Dustmann 2008). This study contributes to the literature by providing new evidence on how gender differences in second-language acquisition shape the academic performance of migrant girls and boys.

My study is most closely related to the strand of literature on migrants and language that specifically examines linguistic distance between languages. Measures of linguistic distance are useful to assess the difficulty in acquiring proficiency in the host country's language (Chiswick and Miller 2005). Rather than relying on self-reported skills, studies in this field use fixed indices and linguistic metrics.[1] Linguistic distance has been measured in different ways. Chiswick and Miller (2005) uses American students' test scores in foreign languages as a proxy for the difficulty for foreigners to learn English. A more commonly used approach is based on linguistic trees (Fearon 2003; Guiso et al. 2009; Belot and Hatton 2012; Adsera and Pytlikova 2015; Guarnieri and Tur-Prats 2023).

[1] This takes care of the measurement error which arises from self-reported language proficiency, as individuals tend to overstate their language skills (Dustmann and Van Soest 2002).

Other studies use the Levenshtein distance, which measures differences between words in two languages based on edit distance (Isphording and Otten 2013; Adsera and Pytlikova 2015; Bousmah et al. 2021). Most studies focus on adult migrants, showing that greater linguistic distance hinders language acquisition and, in turn, reduces labor market outcomes (Isphording and Otten 2013; Isphording 2014; Strøm et al. 2018; Bousmah et al. 2021; Ghio et al. 2023; Wong 2023).Greater linguistic distance among workers can reduce firm productivity, though this effect diminishes as migrants gain host-country language proficiency (Dale-Olsen and Finseraas 2020). Linguistic distance also influences migrant selection, as migrants tend to move to countries with more linguistically similar languages (Adsera and Pytlikova 2015; Belot and Ederveen 2012; Belot and Hatton 2012; Bredtmann et al. 2020; Wang 2024).[2] This study makes a novel contribution to the literature by leveraging linguistic distance as a tool to explain why migrant girls outperform boys academically, highlighting their ability to navigate this challenge more easily and acquire a second language more effectively.

Lastly, my study contributes to the literature on cultural proximity and migrant girls' outcomes. In seminal work, Fernandez (2007) and Fernández and Fogli (2009) use the epidemiological approach to isolate cultural effects from institutions and markets, estimating the impact of cultural origin on migrant women's labor market outcomes.They use female labor force participation and attitudes in women's countries of ancestry as proxies for culture. Since then, numerous studies have shown that cultural gender norms shape migrant girls' educational and labor market performance, using various cultural proxies. Many rely on World Values Survey data (Nollenberger et al. 2016; Rodríguez-Planas and Nollenberger 2018; Bergvall 2022), while others use measures such as the prevalence of women in STEM fields or managerial roles (Aldén and Neuman 2022; Huber and Paule-Paludkiewicz 2024) or cultural indicators from social psychology (Holmlund et al. 2023). All find that second-generation migrant girls from more gender-traditional cultures perform worse in school—especially in math—relative to boys, compared to those from more gender-equal backgrounds. Linguistic distance is commonly defined as a measure of cultural proximity (Belot and Hatton 2012; Isphording and Otten 2013; Wang 2024). This study demonstrates that while linguistic distance and differences in gender norms reflect aspects of cultural proximity and may be correlated, they do not affect the gender gap in education among migrant children in the same way.

The remainder of this chapter is structured as follows: Section 3.2 introduces the measures of cultural proximity, describes the data, and presents summary statistics. Section 3.3 outlines the empirical strategy. Section 3.4 reports the main findings along with robustness checks, while Section 3.5 compares these findings to other measures of cultural proximity. Finally, Section 3.6 concludes.

[2] To address the issue of migrants' self-selection into host countries based on linguistic distance, I incorporate country-by-origin fixed effects.

3.2 Data

3.2.1 The Program for International Student Assessment (PISA)

The Program for International Student Assessment (PISA), administered by the OECD, evaluates students' abilities worldwide. Conducted every three years since 2003, the assessment targets a representative sample of 15-year-olds in each participating country. The primary goal is to benchmark student performance in reading, mathematics, and science using sophisticated survey and test designs. In addition to test scores, the dataset provides valuable information on family background and school characteristics, collected through student and school officials surveys.[3]

For this analysis, all children with migrant origins who participated in the 2003, 2009, 2012, 2015, 2018, or 2022 PISA waves are included.[4] This sample includes both first-generation migrants, who migrated to the test country at some point in their lives, and second-generation migrants, who were born in the test country to two foreign-born parents.

The test-taking countries, referred to as the migrant children's host countries, include all countries that participated in any of the PISA waves and provided data on students' exact countries of origin. The PISA data vary in the number of host countries that report on migrants' exact backgrounds. For example, some countries will only report origins by the continent instead of country. I drop host countries that do not include exact origins for first- and second-generation migrants. I also drop origin-by-host country groups with fewer than 15 observations.

Appendix Table C1 presents the sample sizes of first- and second-generation migrants within each host country. The sample includes 59 host countries, 100 origin countries for first-generation migrants, and 86 host countries for second-generation migrants. On average, each host country has 858 first-generation migrant students from 4.6 origin countries and 1,083 second-generation migrant students from 3.6 origin countries.

I use the PISA test scores in reading and math as outcomes. Reading scores reflect children's proficiency in the host-country language, as the test is administered in it. Math, while arguably less language-intensive, can still rely on some language skills. Thus, any effect on math scores may indicate how language proficiency spills over into other subjects.

It is important to handle PISA test scores correctly when using them as outcomes. Due to the PISA test's complex survey and test design, the data do not report standardized test scores. Instead, students answer a randomly assigned booklet of questions, and using a complex item-response theory model, a number of "test scores" is estimated for each student (Jerrim et al. 2017). These are commonly referred to as "plausible values" in the

[3] Most data in this study comes from the student questionnaire, which also contains the children's PISA test scores. Some school characteristics are used as controls in the analysis, and are collected from the school questionnaire, which is answered by the school officials.

[4] The 2006 PISA wave is excluded as it lacks the necessary information on migrant backgrounds.

PISA data. To interpret marginal effects on these scores, researchers must apply specific econometric methods and weights. I explain these steps in detail in Section 3.3.

3.2.2 Measures of Cultural Proximity

Linguistic Distance Index. The linguistic distance measure can be computed as an index based on language family trees in the Ethnologue database (Fearon 2003). Each language is cataloged in detail by its evolution, beginning with classification into language families, followed by its linguistic categories. A language family is a group of languages that share a common ancestry and can be represented as a tree, where each classification within the family is a node, and any shared classification corresponds to a shared node.

Following Putterman and Weil (2010) and Guarnieri and Tur-Prats (2023), I calculate the number of shared nodes between the host-country language and the origin-country language to obtain an index of linguistic distance that ranges between 0 and 1. The 'Linguistic Distance Index' (LDI) for host-country language i and origin-country language j is produced using the following formula:

$$LDI_{ij} = 1 - \frac{(Shared\ number\ of\ nodes\ between\ i\ and\ j)^{\lambda}}{number\ of\ nodes\ i\ + number\ of\ nodes\ j} \tag{3.1}$$

The parameter λ assigns greater weight to shared nodes that appear earlier in the language categorization and can range between 0 and 1. I set $\lambda = 1/2$ to ensure a concave weighting function (Fearon 2003). The index is constructed so that the linguistic distance between a host-country and origin-country language is 1 if they belong to different language families.

The index is computed for all languages globally and matched to PISA data based on each student's host and origin country. Some countries have multiple spoken languages. For host countries, I assign the language in which the PISA test was administered. For origin countries, I assign the most widely spoken language as identified in the Ethnologue database. The language is typically either the country's official language or its national identity language.

The linguistic distance index is assigned to first-generation migrant students based on their reported country of origin. To second-generation migrant students, I assign the linguistic distance index of their parents' origin. Over 90 percent of the second-generation students in the PISA data have parents from the same country. For students with parents from different countries, I choose the index value of the parent whose origin language is closest to the host-country language. This may be the language the children are more likely to speak at home.

Gender Inequality Index. I also add an alternative measure of cultural proximity to the PISA data: the 'Gender Inequality Index' (GII) from the United Nations Development Programme (UNDP). The GII is a country-level indicator of gender inequality, quantifying the extent to which disparities in gender roles result in a loss of achievement (UNDP 2025). It is based on three key dimensions: women's reproductive health, empowerment, and labor market participation. The index ranges from 0, indicating high gender equality, to 1, indicating severe gender disparity.

The GII has been collected for most countries from 1990–2022. I take the average GII value across all available years for each country to provide an overall measure of its gender inequality during this period. The GII is missing for a small number of countries included in the analysis, and for those, I use the average continent-wide GII value.[5] To assign the GII to first- and second-generation migrant students, I apply the same methods as described for the LDI above.

3.2.3 Summary Statistics

Table 3.1 presents the summary statistics for the estimation sample. Columns (1)–(3) report sample size, mean and standard deviation for all first-generation migrants, while columns (4)–(6) report the same for all second-generation migrants.

Age at arrival is only measured for first-generation migrants. They are on average 7.7 years old when they arrive in the host country, which in most countries is after the start of compulsory school (Bedard and Dhuey 2006).

First- and second-generation migrants are similar in terms of individual parental, and school characteristics. Both are on average around 15.8 years old when they take the PISA test, and have a balanced gender distribution. The parents' education is expressed in terms of ISCED scores, where a higher score reflects higher education (WorldBank 2025). Parents of first-generation migrants have, on average, an ISCED score '5' which means "short-cycle tertiary education". Parents of second-generation migrants have, on average, a score of "4.5", for which the lower bound is "post-secondary non-tertiary education". Around 80 percent of first- and second-generation migrants have parents who are employed in some capacity.

In terms of school characteristics, first- and second-generation migrant students attend schools with an average enrollment of approximately 850 students. About 85 percent are enrolled in public schools, and, on average, 85 percent of their teachers are certified. The student-teacher ratio is around 17 to 1. The school's location is defined by the surrounding community, averaging 3.3 on the PISA school questionnaire scale. This corresponds to towns with 15,000–100,000 inhabitants.[6]

[5] These countries are the Central African Republic, Palestine, and Liechtenstein.

[6] The PISA school questionnaire categorizes school locations as follows: 1 – rural area (0-–3,000 people), 2 – small town (3,000–15,000), 3 – town (15,000–100,000), 4 – city (100,000-–1,000,000), and

Table 3.1: Summary Statistics

	First Generation			Second Generation		
	(1) N	(2) Mean	(3) St Dev	(4) N	(5) Mean	(6) St Dev
Individual Characteristics						
Girl	48,073	0.48	0.50	59,590	0.50	0.50
Child age	48,073	15.80	0.29	59,590	15.78	0.29
Child age at arrival	48,073	7.69	4.79	–	–	–
GDP child origin country	48,073	14,653	15,615	–	–	–
GDP parent origin country	–	–	–	59,590	11,008	12,661
Parental Characteristics						
Education mothers	48,073	5.06	2.44	59,590	4.52	2.42
Education fathers	48,073	5.17	2.34	59,590	4.69	2.34
Working mothers	48,073	0.80	0.38	59,590	0.82	0.35
Working fathers	48,073	0.81	0.37	59,590	0.83	0.34
School Characteristics						
School size	48,073	877.71	578.88	59,590	813.41	525.65
Share certified teachers	48,073	0.85	0.26	59,590	0.85	0.24
Public school	48,073	0.82	0.37	59,590	0.85	0.33
Share female students	48,073	0.49	0.22	59,590	0.50	0.19
Students per teacher	48,073	17.04	39.64	59,590	16.73	21.22
School location	48,073	3.38	1.14	59,590	3.34	1.09
Linguistic Distance (LDI)						
LDI Child origin country	48,073	0.56	0.45	–	–	–
LDI Parent origin country	–	–	–	59,590	0.71	0.39
Gender Inequality Index (GII)						
GII Host country	47,554	0.22	0.15	59,372	0.18	0.12
GII Child origin country	48,073	0.35	0.19	–	–	–
GII Parent origin country	–	–	–	59,906	0.33	0.18
PISA Test Scores						
Reading Score	48,073	432.84	2.06	59,590	444.35	1.94
Math Score	48,073	439.85	1.75	59,590	447.51	1.66

Notes: This table provides summary statistics on the samples of first- and second-generation migrants from the 2003, 2009, 2012, 2015, 2018 and 2022 PISA tests. Child age refers to age at test. Age at arrival pertains to first-generation migrants' age when they settled in their host-country. Mothers and fathers' education is specified according to ISCED. Mothers and fathers' working are binary variables taking the value 1 if parents are in any type of employment. GDP refers to the average Gross Domestic Product of the origin of the child or the parents (measured within the sample period).

First- and second-generation migrants also originate from countries with similar levels of gender inequality. The Gender Inequality Index (GII) for both groups averages around 0.35, well below the global mean of approximately 0.6 (UNDP 2025). However, they now reside in host countries with significantly lower gender inequality, where the average GII is about 0.22 for first-generation migrants and 0.18 for second-generation migrants. This indicates that both first-generation migrants and the parents of second-generation migrants have moved to countries with comparatively lower gender inequality than their

5 – large city (1,000,000—10,000,000). In the 2022 data, an additional category 6 – mega city was introduced, which has been recoded as 5 for comparability.

countries of origin.

First- and second-generation migrants differ in the linguistic distance between their host and origin countries. On average, the Linguistic Distance Index (LDI) is 0.56 for first-generation migrants and 0.71 for second-generation migrants. This difference may be explained by the timing of migration, depending on whether parents moved before or after their child's birth. It appears that parents who migrated after their child's birth were more likely to choose a country with greater linguistic similarities, whereas those who migrated before childbirth were less influenced by linguistic distance.

Furthermore, second-generation migrant students tend to perform slightly better than first-generation migrant students in the PISA test. On average, their test scores in both reading and math are marginally higher. This pattern aligns with the expectation that earlier exposure to the host-country language and education system may contribute to improved academic outcomes. Second-generation students, having been born in the host country, face fewer language and adjustment barriers than first-generation migrants

Appendix Figure C1 presents the average gender gap in PISA test scores for first- and second-generation migrants across the host countries included in the study. Among first-generation migrant students, girls outperform boys in reading and math in almost all countries. For second-generation migrant students, the gender gap in reading remains in favor of girls, while the gender gap in math tends to favor boys in the majority of countries.

3.3 Empirical Strategy

To examine how linguistic distance between migrants' host and origin countries influences the gender gap in education, I use an epidemiological approach (Fernandez 2007; Fernández and Fogli 2009) and estimate the following model:

$$y_{sitj} = \beta_1 Girl_s + \beta_2 (Girl_s \times LDI_{ij}) + \beta_3 X'_{sitj} + \beta_4 (Girl_s \times X'_{sitj}) + \omega_t + \theta_i + \theta_j + (Girl_s \times \theta_i) + \epsilon_{sitj} \tag{3.2}$$

where y_{sitj} is the PISA test score for student s who lives in host country i at time t and is from origin country j. The binary variable $Girl_s$ equals one if the student is a girl and zero otherwise. LDI_{ij} is the index of linguistic distance between the student's host and origin country. The coefficient of interest is the interaction between the linguistic distance measure LDI_{ij} and the gender indicator $Girl_s$, which indicates whether the gender gap in test scores changes with linguistic distance between the origin and host country. X'_{sitj} is a vector of controls for multiple individual characteristics, which vary depending on the chosen specification but always include the student's age. The regression model also includes a number of fixed effects: ω_t for PISA wave, θ_i for the host country, and θ_j for the origin country. The gender indicator $Girl_s$ is interacted with θ_i to account for variation in gender gaps in education across host countries.

The epidemiological approach allows me to study the effects of linguistic distance on the migrant population in relative terms by isolating its effect from other factors that may impact the gender gap in education within the host country. To account for potential confounding variables, I include a set of controls—interacted with the gender indicator such as age at arrival, origin country GDP, parental and school characteristics.

The country-by-origin fixed effects account for observable and unobservable factors that vary across countries and migrant groups. For example, they deal with time-invariant reasons for certain migrants selecting into host countries where their language skills would benefit them.For example, migrants are more likely to move to a country with the same official language than to one with a more distant language (Adsera and Pytlikova 2015). The PISA-wave fixed effect isolates the effect of linguistic distance from factors that vary over time across PISA test-taking cohorts.

The model cannot fully account for migrant selection due to unobservable factors that vary across countries and over time. Recent studies employing the epidemiological approach with detailed administrative data on children address this by adding sibling fixed effects or leveraging reforms that introduce exogenous variation in neighborhood characteristics (Finseraas and Kotsadam 2017; Aldén and Neuman 2022; Bergvall 2022; Holmlund et al. 2023). However, the cross-country nature of my data, which includes limited family information beyond parental characteristics, prevents me from applying such methods. Instead, I follow previous studies applying the epidemiological approach to PISA data (Nollenberger et al. 2016; Rodríguez-Planas and Nollenberger 2018).

I take multiple measures to account for PISA's complex survey and test design in my estimation of Equation (3.2). First, I account for the fact that the PISA test scores are reported as plausible values obtained from Item Response Theory models. For example, it is not appropriate to use an average of these values as an outcome (Jerrim et al. 2017). Therefore, I use the Stata command 'pv', which is written specifically for estimation with plausible values as the outcome. This command allows me to run regression Equation (3.2) over all plausible values and then take the appropriate steps to generate the marginal effect (OECD 2009).

Second, the complex selection procedures of students across countries require that researchers use the replicate weights provided in the PISA data. These are derived from resampling methods similar to jackknife and bootstrapping techniques. By including these weights, I avoid the risk of underestimating or overestimating the amount of uncertainty due to sampling errors. The 'pv' command incorporates the replicate weights by adjusting the standard errors for stratification, following the OECD's recommended procedures.

Table 3.2: Linguistic Distance and the Gender Gap in PISA Scores

	Reading Test Scores					Math Test Scores				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. First-Generation Migrants										
Girl	177.463	134.833	139.507	121.743	138.424	124.302	98.675	100.567	87.398	93.898
	(111.668)	(110.530)	(110.445)	(109.027)	(106.221)	(100.151)	(100.637)	(101.075)	(99.875)	(99.166)
Girl × LDI	13.484***	12.748***	15.153***	13.590***	13.563***	8.837**	8.564**	9.538**	8.209*	7.507*
	(4.185)	(4.127)	(4.641)	(4.638)	(4.697)	(3.963)	(3.923)	(4.391)	(4.325)	(4.423)
Constant	82.495	113.361	108.977	95.933	62.910	150.884**	169.237**	167.135**	153.104**	129.151*
	(82.010)	(83.588)	(83.602)	(79.165)	(77.403)	(69.906)	(70.389)	(70.655)	(68.384)	(66.970)
Observations	48,073	48,073	48,073	48,073	48,073	48,073	48,073	48,073	48,073	48,073
R-squared	0.34308	0.36816	0.36838	0.39677	0.40849	0.38364	0.39620	0.39625	0.42204	0.43407
B. Second-Generation Migrants										
Girl	-28.712		-33.644	-31.899	-81.197	16.281		11.549	-4.349	-41.595
	(104.186)		(104.667)	(106.110)	(109.363)	(94.207)		(94.574)	(97.917)	(99.984)
Girl × LDI	-5.121		-2.016	-2.286	-1.509	0.178		2.950	2.747	4.021
	(3.481)		(3.787)	(3.734)	(3.882)	(3.316)		(3.391)	(3.423)	(3.395)
Constant	376.972*		373.871*	339.991	351.972	330.494		328.419	303.260	311.887
	(210.670)		(208.265)	(227.124)	(241.013)	(244.540)		(241.464)	(260.956)	(275.898)
Observations	59,590		59,590	59,590	59,590	59,590		59,590	59,590	59,590
R-squared	0.37925		0.37962	0.40589	0.41673	0.35474		0.35514	0.38160	0.39271
Age	X	X	X	X	X	X	X	X	X	X
Age at arrival		X	X	X	X		X	X	X	X
GDP origin country			X	X	X			X	X	X
Parental characteristics				X	X				X	X
School characteristics					X					X

Notes: Estimates from equation 3.2 measuring the relationship between linguistic distance (LDI) and the gender gap in test scores from the PISA test. Panel A. reports estimates for first-generation migrants, who migrated to the test-taking country. Panel B. reports estimates for second-generation migrants born in the test-taking country with foreign-born parents. Following OECD recommendations, standard errors are adjusted according to the FAY's BRR methodology using replicate weights. Covariates are interacted with the gender indicator 'Girl' and include controls for missing values. Parental characteristics include mothers' and fathers' years of education and employment status. School characteristics include number of students, share of certified teachers, an indicator for public school, the student-teacher ratio and type of neighborhood. *** p<0.01, ** p<0.05, * p<0.1.

3.4 Results

Table 3.2 presents the main results. Columns (1)–(5) report the effects on PISA reading test scores, while columns (6)–(10) show the effects on PISA math test scores. The key coefficient of interest is the interaction between the gender indicator 'Girl' and the Linguistic Distance Index (LDI). For first-generation migrants, this coefficient is positive and statistically significant across all outcomes and specifications.

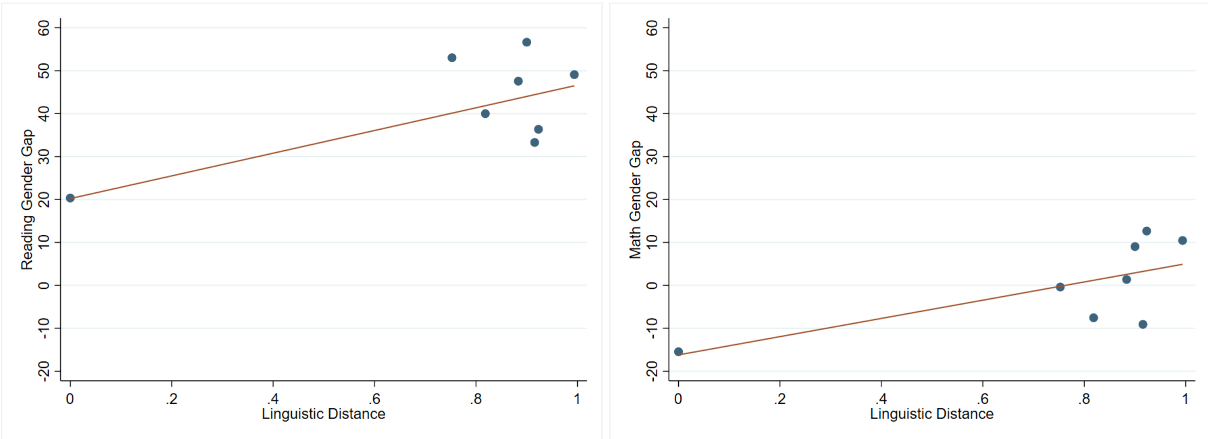
Starting with the baseline specification in column (1), which controls for the student's age at the time of testing, a one-standard-deviation increase in linguistic distance widens the gender gap in reading scores by 7.6 percent.[7] This effect is statistically significant at the one percent level. When controlling for age at arrival in column (2), the effect increases to 9.1 percent. I consider this the preferred specification for first-generation migrants, as previous research has shown that age at arrival plays a crucial role in shaping children's educational attainment and social integration (Bleakley and Chin 2004; Böhlmark 2008; Clarke and Isphording 2017).

Turning to the effect of linguistic distance on the gender gap in math scores in column (6), a one-standard-deviation increase in linguistic distance leads to a 7.1 percent widening of the gender gap in favor of girls. This effect is statistically significant at the five percent level. When accounting for age at arrival in column (7), the effect increases to 8.6 percent.

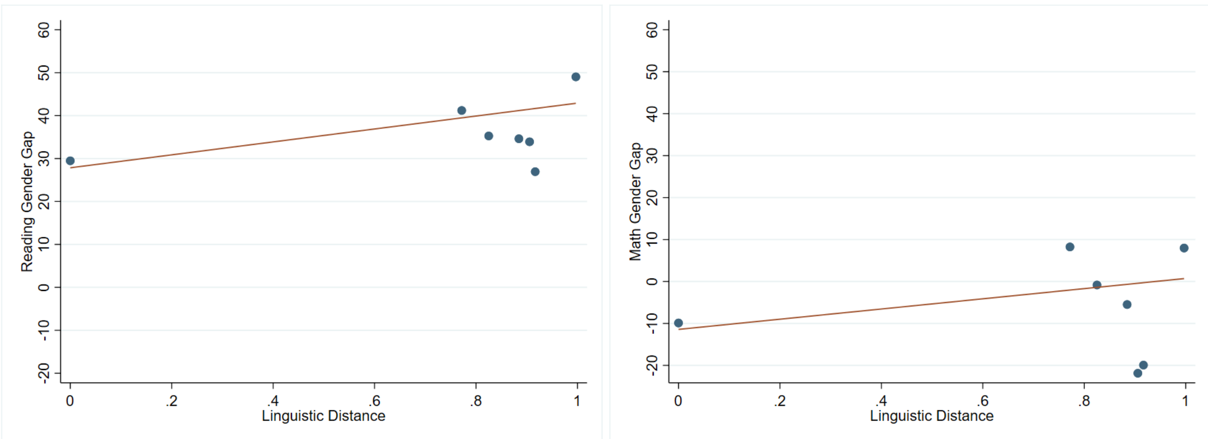
[7] Effect of interaction divided by the gender gap.

Figure 3.1: Graphical Presentation of Results

A. First-Generation Migrants



B. Second-Generation Migrants



Notes: The figure presents bin-scatter plots of the relationship between linguistic distance between host and origin country language on the gender gap in reading and math PISA test scores. For first-generation migrants, the linguistic distance index is calculated as the distance between the languages of the host country and their origin country. For second-generation migrants, the index is calculated between the languages of the host country and their parents' origin country.

These findings suggest that linguistic distance has the strongest impact on subjects that develop reading skills. Since math is generally less language-intensive, it is expected that the effect of linguistic distance would be weaker. Nevertheless, the results suggest that linguistic distance significantly benefits girls in acquiring math skills within the host-country education system.

The results for second-generation migrants are displayed in Panel B. of Table 3.2. For this group, there is no statistically significant relationship between linguistic distance and the gender gap in test scores. The estimates for the interaction terms columns (1) and (6), controlling for age at test, are statistically insignificant and comparably small. This suggests that the effect of linguistic distance on the gender gap in education is weaker among students with migrant backgrounds who have lived in the host country since birth.

Figure 3.1 presents binned scatter plots, which show the correlation between the migrant-gender gap in test scores and linguistic distance. There is a positive relationship between linguistic distance and the gender gap in both reading and math, meaning that girls' advantage over boys increases with linguistic distance. For first-generation migrants, the relationship is steeper in reading scores than in math scores, indicating a stronger correlational effect in terms of reading skills. For second-generation migrants, the correlational effects are also positive, yet show weaker relationships for both reading and math scores.

Overall, my findings show that greater linguistic distance between a student's first and second language gives girls a stronger advantage in both reading and math scores. The effect becomes stronger after controlling for age at migration. This indicates that children who migrate at an older age tend to face greater difficulties in acquiring the host-country language, and these challenges are more pronounced for boys than for girls. This suggests that girls have a relative advantage when second-language acquisition requires more active learning, particularly for those who migrate after starting school.

Consistent with this pattern, linguistic distance does not appear to significantly influence the gender gap in test scores for second-generation migrants, who are born in the host country. Since they are exposed to the host-country language from birth, boys are better able to catch up, and linguistic distance no longer presents a disproportionate challenge for them.

3.4.1 Robustness and Heterogeneity

To ensure that the results are not driven by potential confounding factors, I report the effects using several alternative model specifications in Table 3.2. These specifications incorporate different sets of covariates while consistently controlling for both age at test and age at arrival. Additionally, all covariates are interacted with the gender indicator 'Girl' to account for factors that may affect boys and girls differently.

First, I control for the origin country's log GDP per capita to separate the effect of linguistic distance from differences in economic development between host and origin countries. The corresponding coefficients are presented in columns (3) and (8) for reading and math scores, respectively. For first-generation migrants, controlling for origin-country GDP slightly increases the effect of linguistic distance on the gender gap in both reading and math.

Second, I control for parental characteristics to ensure that the results are not driven by differences in socioeconomic status. These controls include parental education levels measured using ISCED scores[8], along with indicators for maternal and paternal employment. The corresponding results are shown in columns (4) and (9) for reading and math scores, respectively. The estimates for first-generation migrants are robust to the

[8] A higher ISCED score indicates a higher level of completed education.

inclusion of these controls.

Third, I control for school characteristics to account for the possibility that girls and boys from different linguistic backgrounds attend different schools, which in turn affects the gender gap in their test scores. I include various measures of school quality: (i) school size, measured by the number of students; (ii) the share of certified teachers; (iii) whether the school is public or private; (iv) the student-teacher ratio; and (v) the type of neighborhood in which the school is located. The results in columns (5) and (10) indicate that the estimates for first-generation migrants remain robust after including these controls.

All coefficients for second-generation migrants remain statistically insignificant after including these controls. This suggests that linguistic distance plays a limited role in shaping the gender gap for second-generation students, even after controlling for the GDP of their origin country, as well as parental and school characteristics.

3.5 Comparison to Cultural Proximity in Gender Norms

The main results show that linguistic distance widens the gender gap in reading and math scores in favor of first-generation migrant girls, while having little impact on the gender gap for second-generation migrants. Previous studies have used an epidemiological approach to focus on the role of cultural proximity between host and origin countries in explaining the gender gap in education.

Among students from cultures with more traditional gender norms, second-generation migrant boys tend to have a larger advantage in educational outcomes (Fernandez 2007; Fernández and Fogli 2009; Nollenberger et al. 2016; Bergvall 2022; Holmlund et al. 2023). Linguistic distance is often considered a measure of cultural proximity, yet the findings of this study suggest that it influences the gender gap in migrant education differently than the gender norm measures used in previous research. These findings suggest that linguistic distance represents a distinct dimension of cultural proximity compared to gender norms, likely reflecting girls' advantage in second-language acquisition during childhood.

To verify that linguistic distance operates differently than conventional measures of cultural proximity, I replicate the analysis using the Gender Inequality Index (GII), which captures differences in gender norms between host and origin countries.[9]

By examining the impact of gender norm proximity within the same sample of students taking the PISA test, I can assess whether linguistic distance primarily reflects cultural

[9] The LDI and GII indices are weakly negatively correlated, with a correlation of -0.1958 for first-generation migrants and -0.1664 for second-generation migrants. This suggests that migrants from more gender-unequal origin countries tend to move to host countries with a smaller linguistic distance. One possible explanation is that they follow established migration patterns and settle in countries with well-established ethnic enclaves. However, the correlation remains weak.

Table 3.3: Gender Inequality Index and the Gender Gap in PISA Scores

	Reading Test Scores					Math Test Scores				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. First-Generation Migrants										
Girl	181.741	138.494	139.746	122.598	138.019	126.323	100.114	101.009	87.270	92.943
	(111.812)	(110.666)	(111.041)	(109.340)	(106.312)	(100.231)	(100.700)	(101.021)	(99.891)	(99.201)
Girl × GII	-15.504	-11.280	-8.521	-16.604	-27.065*	3.501	5.304	7.277	0.095	-4.197
	(12.048)	(12.047)	(14.207)	(14.331)	(15.685)	(10.350)	(10.555)	(12.703)	(12.885)	(13.678)
Constant	89.983	119.632	117.342	105.702	76.069	152.282**	170.243**	168.512**	156.402**	133.848**
	(81.777)	(83.272)	(83.537)	(79.276)	(77.288)	(69.700)	(70.136)	(70.508)	(68.443)	(66.927)
Observations	48,073	48,073	48,073	48,073	48,073	48,073	48,073	48,073	48,073	48,073
R-squared	0.34256	0.36766	0.36767	0.39628	0.40817	0.38331	0.39591	0.39592	0.42177	0.43387
B. Second-Generation Migrants										
Girl	-4.174		5.932	-0.160	-50.160	39.597		52.303	30.517	-8.662
	(105.898)		(104.467)	(106.182)	(109.411)	(96.351)		(95.734)	(99.011)	(101.154)
Girl × GII	-66.671***		-76.526***	-57.124***	-57.461***	-63.448***		-76.461***	-59.334***	-58.623***
	(9.897)		(12.329)	(12.146)	(12.506)	(8.888)		(10.892)	(10.794)	(10.980)
Constant	372.130*		375.543*	341.033	351.274	328.701		334.193	308.060	315.789
	(210.155)		(212.889)	(230.144)	(244.009)	(241.895)		(245.561)	(263.171)	(278.009)
Observations	59,590		59,590	59,590	59,590	59,590		59,590	59,590	59,590
R-squared	0.38152		0.38179	0.40688	0.41774	0.35750		0.35800	0.38307	0.39452
Age	X	X	X	X	X	X	X	X	X	X
Age at arrival		X	X	X	X		X	X	X	X
GDP origin country			X	X	X			X	X	X
Parental characteristics				X	X				X	X
School characteristics					X					X

Notes: Estimates from equation 3.2 measuring the relationship between the gender inequality index (GII) and the gender gap in test scores from the PISA test. Panel A. reports estimates for first-generation migrants, who migrated to the test-taking country. Panel B. reports estimates for second-generation migrants born in the test-taking country with foreign-born parents. Following OECD recommendations, standard errors are adjusted according to the FAY's BRR methodology using replicate weights. Covariates are interacted with the gender indicator 'Girl' and include controls for missing values. Parental characteristics include mothers' and fathers' years of education and employment status. School characteristics include number of students, share of certified teachers, an indicator for public school, the student-teacher ratio and type of neighborhood. *** p<0.01, ** p<0.05, * p<0.1.

proximity or captures gender differences in second-language acquisition among migrant children.

The results, presented in Table 3.3, reveal a contrasting pattern. For first-generation migrants (Panel A), higher origin-country gender inequality does not significantly affect the gender gap in reading or math scores. This means that girls and boys are not differently affected by coming from more gender-traditional origins when they themselves have migrated. However, for second-generation migrants (Panel B), higher GII levels are associated with a wider gender gap in favor of boys. The coefficients are robust to controlling for GDP, parental and school characteristics, and remain statistically significant on the one-percent level. The academic performance of girls whose parents migrated is thus negatively affected by their parents' gender-traditional origins. This is consistent with the findings of the previous literature.

This exercise demonstrates that while linguistic distance and the Gender Inequality Index both measure cultural proximity, they do not influence the gender gap in education among migrant children in the same way. This further reinforces the idea that linguistic distance captures girls' advantage in early language acquisition rather than other aspects of culture, such as norms. Girls outperform boys in second-language acquisition during childhood, particularly when they have migrated, and this effect is not driven by other aspects of cultural proximity.

3.6 Conclusion

This study is the first to explore how linguistic distance between migrant children's host- and origin-country languages influences the gender gap in academic performance. I construct a measure of linguistic distance based on shared nodes within language trees and add these to the PISA data on 15-year-olds' ability in math and reading. I leverage these data with an epidemiological approach that includes a rigorous battery of controls and country-by-origin fixed effects.

Linguistic distance has a large influence on the gender gap in first-generation migrants' education. In the preferred specification, controlling for age at arrival, a one-standard-deviation increase in linguistic distance widens the gender gap by 9 percent in reading scores and 8 percent in math scores, in favor of girls. These effects remain robust to controlling for origin-country GDP, parental and school characteristics. This suggests that a key reason migrant girls outperform migrant boys academically is their advantage in acquiring a second language during childhood. While linguistic distance is a challenge in the host country's education system, girls appear to navigate this barrier more effectively than boys.

Consistent with this pattern, I find that linguistic distance plays a smaller role in shaping the gender gap among second-generation migrants, who are exposed to the host-country language from birth. Although girls may have an initial advantage, boys are able to catch up more easily over time.

To assess whether linguistic distance reflects gender differences in language acquisition rather than culture, I compare its effect on the gender gap to other cultural proximity measures. Using gender inequality differences between host and origin countries, I find that cultural proximity affects second-generation but not first-generation migrants. Moreover, greater cultural gender norm differences disadvantage second-generation migrant girls. While both linguistic distance and cultural norms capture cultural proximity, they shape the academic gender gap differently. Linguistic distance, in particular, appears to reflect girls' advantage in early language acquisition.

Migration brings together children from linguistically diverse backgrounds, often requiring them to learn the host-country language while enrolled in the education system. This study provides novel insights into gender differences in migrant children's language acquisition. It shows that girls acquire the language more efficiently, while boys are more likely to struggle. For policymakers seeking to close this gap, targeted support to improve boys' language proficiency can promote equal educational opportunities for migrant children.

Appendices

Guide to the Appendices: The following appendices provide supplementary figures and tables for each chapter of this dissertation. Appendix A covers Chapter 1, with dedicated sections on second-generation migrant boys, natives, and the theoretical model. The other chapters have shorter appendices: Appendix B corresponds to Chapter 2, and Appendix C to Chapter 3.

A Appendix to Chapter 1: In the Shadow of Brothers

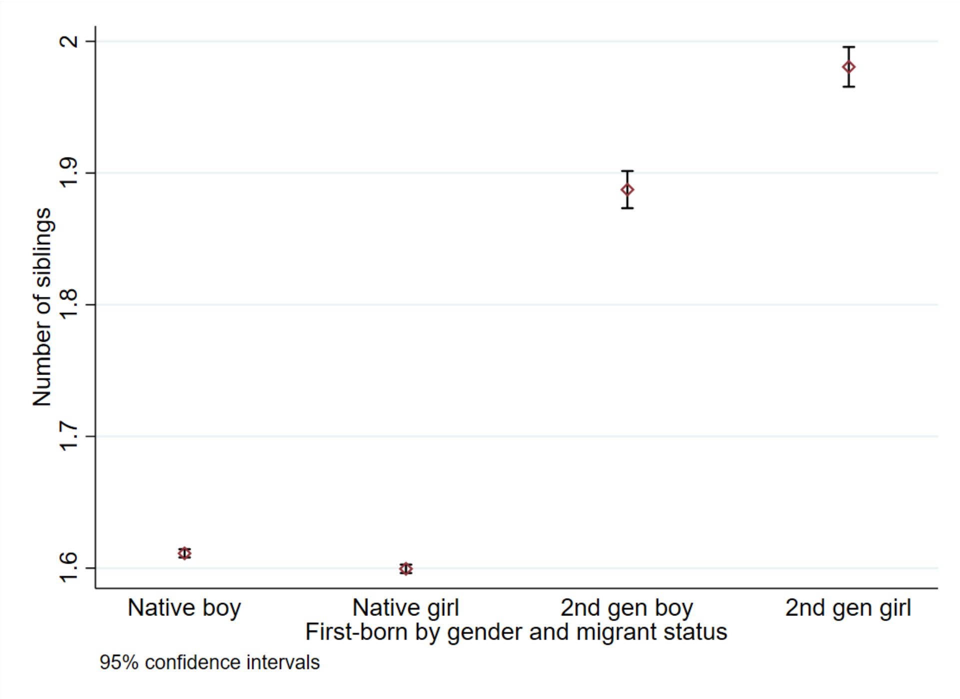
A.1 Figures and Tables

Table A1: Migrant Origin Countries

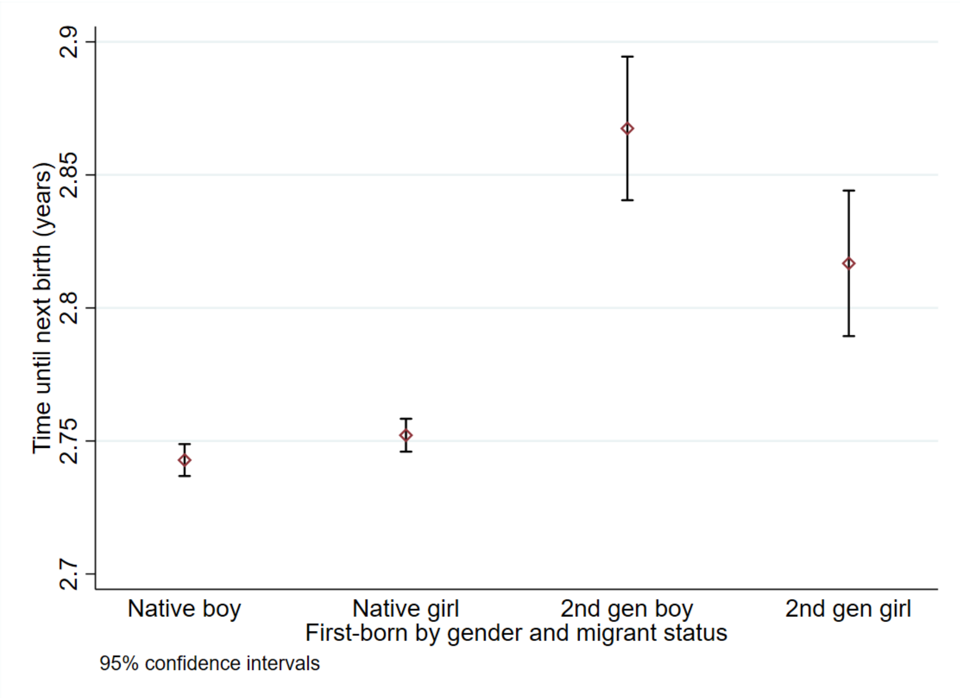
(1) ID	(2) Country group	(3) HDI value	(4) HDI class	(5) GII	(6) GII>m
00	Sweden	0.900	very high	0.055	no
29	Bosnia-Herzegovina	0.674	medium	0.326	no
30	Jugoslavia, Chroatia, North Macedonia, Slovenia	0.729	high	0.221	no
31	Gdansk, Poland	0.761	high	0.216	no
32	Ireland, Great Britian	0.837	very high	0.231	no
33	Germany	0.870	very high	0.142	no
34	Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain, Vatican State	0.799	very high	0.207	no
35	Estonia, Latvia, Lithuania	0.746	high	0.326	no
36	Albania, Armenia, Azerbadjan, Bulgaria, Georgia, Kazachstan, Kygyzstan, Moldavia, Romania, Russia, Tajikistan, Turkmenistan, Ukraine, Uzbekistan, Belarus	0.705	high	0.416	yes
37	Slovakia, Czhech Republic, Hungary	0.751	high	0.295	no
38	Andorra, Belgium, France, Lichtenstein, Luxembourg, Netherlands, Switzerland, Austria	0.856	very high	0.149	no
39	Canada, USA	0.885	very high	0.269	no
40	Antigua and Baruda, Bahamas, Barbados, Belize, Costa Rica, Cuba, Dominican Republic, El Salvador, Grenada, Guatemala, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, St Lucia, St Vincent, St Kitt and Nevis and Anguil, Trinidad and Tobago	0.624	medium	0.485	yes
41	Chile	0.740	high	0.463	yes
42	Argentina, Bolivia, Brasilien, Colombia, Ecuador, Guyana, Paraguay, Peru, Surinam, Uruguay, Venezuela	0.661	medium	0.515	yes
43	Dijibouti, Eritrera, Ethiopia, Somalia, Sudan	0.126	low	0.249	no
44	Algeria, Bahrain, Cyprus, Egypt, French Marocco, Arab Emirates, Gaza, Israel, Jemen, Jordan, Kuwait, Lebanon, Libya, Marocco, Palestina, Quatar, Saudi Arabia, South Jemen, Syria, Tunisia	0.417	low	0.357	yes
45	Angola, Benin, Botswana, Burkina Faso, Burundi, Central African Rep., Comoros, Equatorial Guinea, Ivory Coast, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Cameroon, Cap Verde, Kenya, Congo, Lesotho, Liberia, Madagaskar, Malawi, Mali, Mauritania, Mauritius, Mocambique, Namibia, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Swaziland (Eswatini), South Africa, Tanzania, Chad, Togo, Uganda, Dem rep of Congo, Zambia, Zanzibar, Zimbabwe	0.417	low	0.599	yes
46	Iran	0.658	medium	0.622	yes
47	Iraq	0.557	medium	0.686	yes
48	Turkey	0.641	medium	0.584	yes
49	Hong Kong, Japan, China, South Korea, North Korea	0.664	medium	0.231	no
50	Myanmar/Burma, Philippines, Indonesia, Laos, Malaysia, Singapore, Thailand, Vietnam	0.607	medium	0.414	yes
51	Afganistan, Bangladesh, Bhutan, Brunei, India, Kampuchea, Maldives, Mongolia, Nepal, Oman, Pakistan, Sikkim, Sri Lanka	0.438	low	0.435	yes
52	Australia, Fiji, Kiribati, Micronesia, Nauru, New Zeeland, Palau, Papa new Guinea, Salomon islands, Tonga, Vanutua, Samoan islands	0.875	very high	0.181	no

Notes: Country groups are specified in the data as countries with close cultural and geographical proximity. HDI refers to human development index, GII to gender inequality index, constructed by UNDP. A higher value indicates higher human development or higher gender equality, and should be interpreted as the within-country group mean weighted by the total migrant population from each country in Sweden during the study period.

Figure A1: Fertility Choices in Migrant and Native Families



(a) Number of siblings for first-born gender



(b) Birth spacing before first and second birth

Notes: Confidence-interval plots of the average number of siblings and birth spacing (in years) between first and second birth in families with either a first-born girl or boy. The estimates are displayed with 95 percent confidence intervals. Overlapping confidence intervals indicate insignificant estimates across sample groups. Families are included conditioning on having at least two children born between 1988–2003. Panel (a) shows the number of siblings by first-born girl and boy in native and migrant families. Panel (b) shows the birth space by first-born girl and boy in native and migrant families.

Table A2: Placebo Parents' Background Characteristics

	Fathers			Mothers		
	(1) Schooling (Years)	(2) Earnings (Percentiles)	(3) 10 Years or More in Swe	(4) Schooling (Years)	(5) Earnings (Percentiles)	(6) 10 Years or More in Swe
Late entry	-0.050	-0.417	0.015	-0.159	0.021	0.019
	(0.180)	(1.049)	(0.020)	(0.184)	(1.040)	(0.016)
Observations	9,242	9,242	9,242	9,242	9,242	9,242
R-squared	0.022	0.042	0.025	0.055	0.040	0.011
Outcome mean	10.53	23.15	0.242	10.02	22.24	0.131

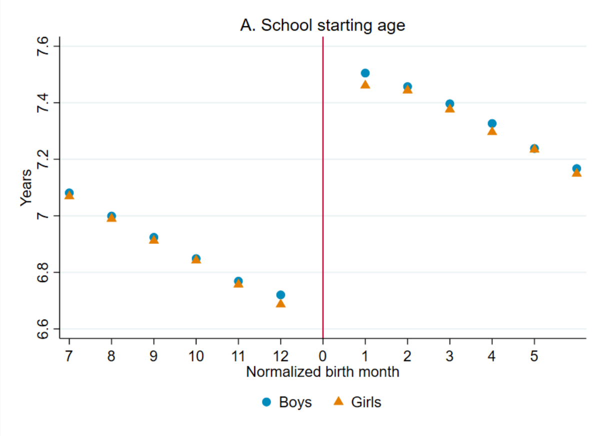
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents placebo regressions of first-born child being born after the January 1^{st} on parent's background characteristics. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of all first-born second-generation migrants born between 1988–2003. Late entry indicates that the first-born child in the family was born in Jan–Feb as opposed to Nov–Dec. Parent's schooling and earnings are measured after the child is born, at ages 3–5, but before the child enters school. The outcome "10-years or more in Swe" is a dummy which takes on the value 1 if the parent migrated to Sweden at least 10 years before the child was born. In the sample the median time in Sweden before the birth of the first child is six years for fathers and four years for mothers.

Table A3: Placebo Sibling Birthdates

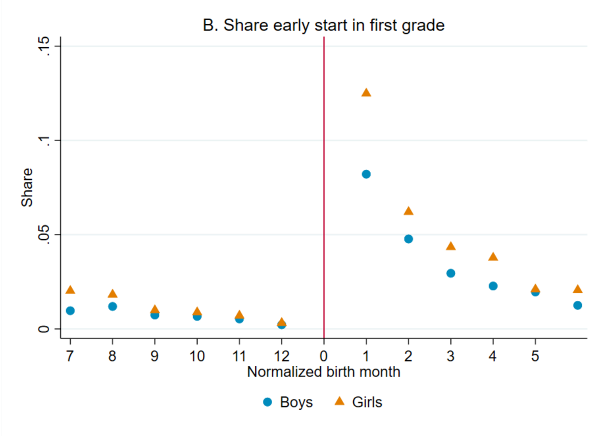
	First-Born Girls		First-Born Boys	
	(1) Younger Sister Jan–Feb	(2) Younger Brother Jan–Feb	(3) Younger Sister Jan–Feb	(4) Younger Brother Jan–Feb
Late entry	0.046	-0.007	-0.025	-0.070*
	(0.037)	(0.036)	(0.037)	(0.037)
Observations	3,743	3,856	3,583	3,614
R-squared	0.013	0.008	0.011	0.014
Outcome mean	-0.00143	-0.350	-0.0278	-0.336

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents placebo regressions of first-born child being born after the January 1^{st} on the likelihood that their younger sibling also is. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of all siblings born in Sweden to two non-Nordic migrants between 1988–2003. Late entry indicates that the first-born child in the family was born in Jan–Feb as opposed to Nov–Dec. The outcome "Jan–Feb" indicates that the younger sibling was born in these months instead of Nov–Dec.

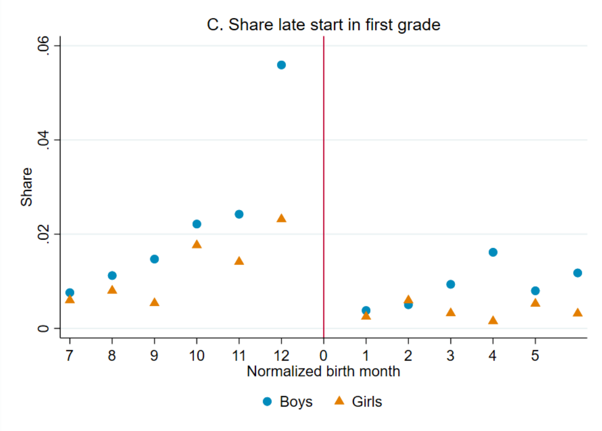
Figure A2: First Stage: Second-Generation Migrant Students



(a) Discontinuity in school entry age at cutoff



(b) Share of early starters by birth month



(c) Share of late starters by birth month

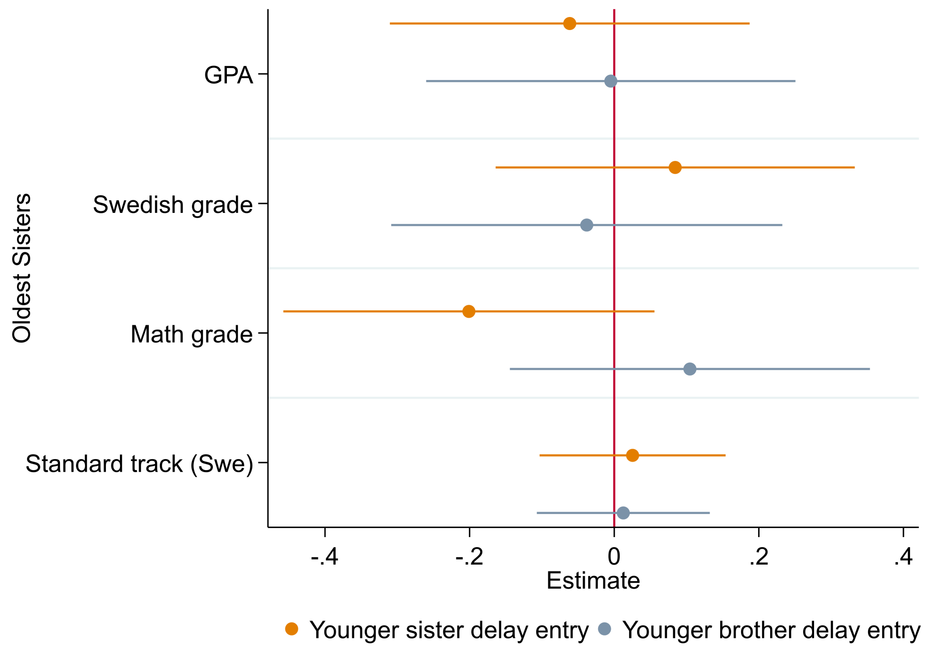
Notes: This figure presents an estimation of the first-stage, the effect of being born in January-February on starting first grade of compulsory school on time. The sample used for this estimation differs from the main sample: the data comes from "Elevregistret" and includes second-generation migrant students from the 2002/2003 and 2003/2004 birth cohorts. This data includes only birth month, and therefore the X-axis displays birth month normalized around the January 1^{st} cutoff. Panel A shows the discontinuity in school starting age over the cutoff, Panel B the discontinuity in the share of students that start at an earlier age than expected, and Panel C the discontinuity in the share of students that start later than expected (so-called red-shirting). Each dot represents the average for a 1-month birth bin for girls (orange) and boys (blue).

Table A4: First stage and Old or Young for Grade

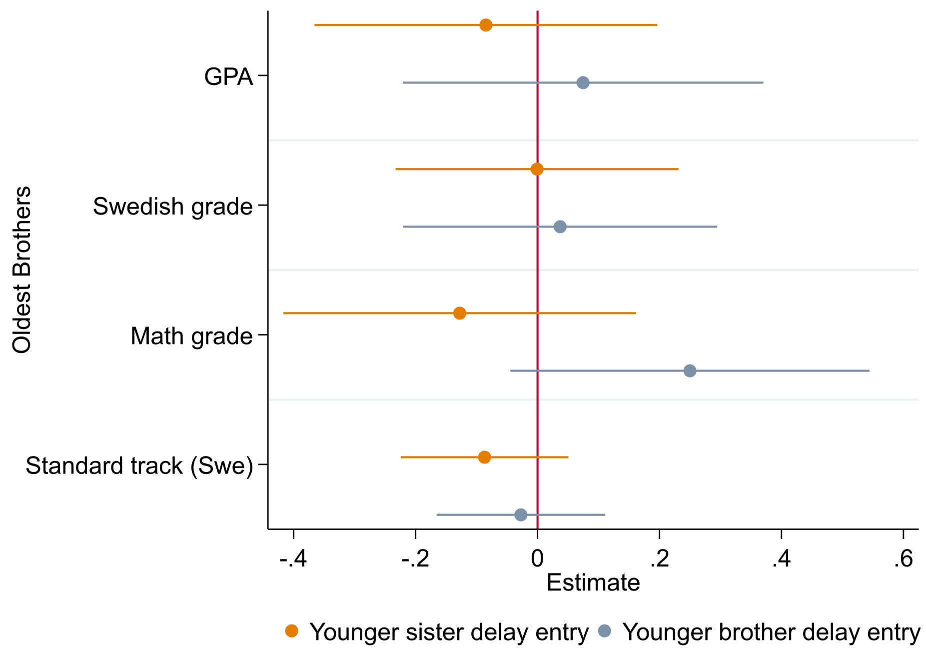
	First Stage		**Old or Young for Grade**			
	Age at School Entry First Grade		Older than 16 Ninth Grade		Younger than 15 Ninth Grade	
	(1) Girls	(2) Boys	(3) Girls	(4) Boys	(5) Girls	(6) Boys
Born Jan–Feb	0.784***	0.792***	-0.099***	0.178***	-0.147***	0.125***
	(0.012)	(0.013)	(0.007)	(0.008)	(0.009)	(0.007)
Elevregister dataset	X	X				
Main dataset			X	X	X	X
Observations	1,783	1,641	19,730	19,730	20,524	20,524
R-squared	0.687	0.713	0.032	0.070	0.048	0.053
Outcome mean	7.117	7.078	0.0408	0.0755	0.0686	0.0510

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents the first stage and being old/young for grade. First stage is the effect of being born after the January 1^{st} cutoff on starting first grade of compulsory school on time. Old/young for grade is the effect of being born after the cutoff on the probability to be older than 16 (old for grade) or younger than 15 (young for grade) at the start of the final year of compulsory school. Reduced-form RD regression with linear control function and a 60-day bandwidth around the January 1^{st} cutoff. The sample used to estimate the first stage differs from the main sample: the data comes from "Elevregistret" and includes second-generation migrant students from the 2002/2003 and 2003/2004 birth cohorts. This data includes only birth month. The sample used to estimate old or or young for grade is the same as the main data: children born in Sweden to two non-Nordic parents between 1988–2003.

Figure A3: Placebo: Sibling Spillovers on Oldest Sibling's outcomes



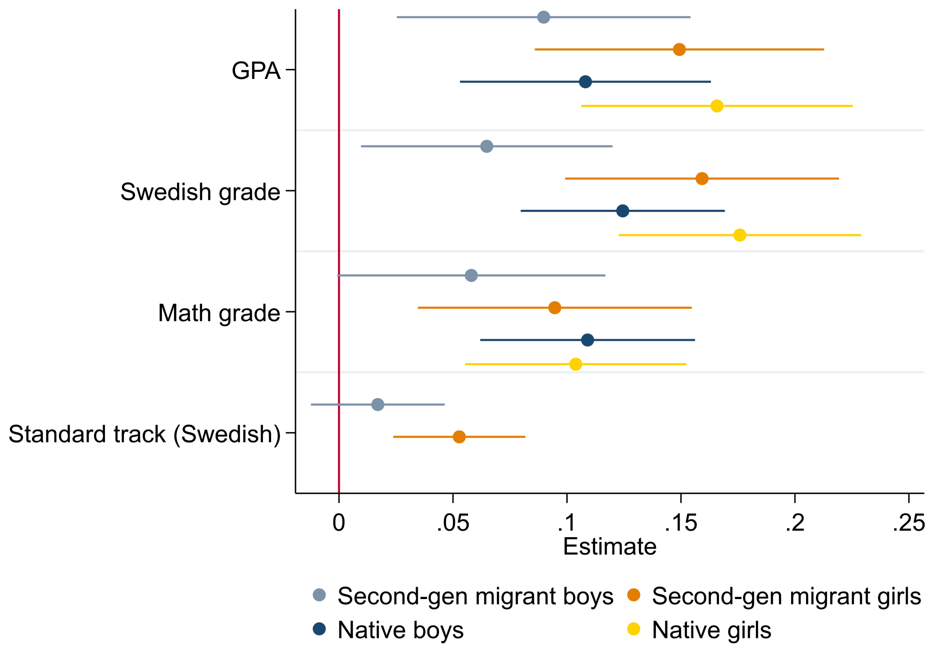
(a) Effects on Oldest Sister



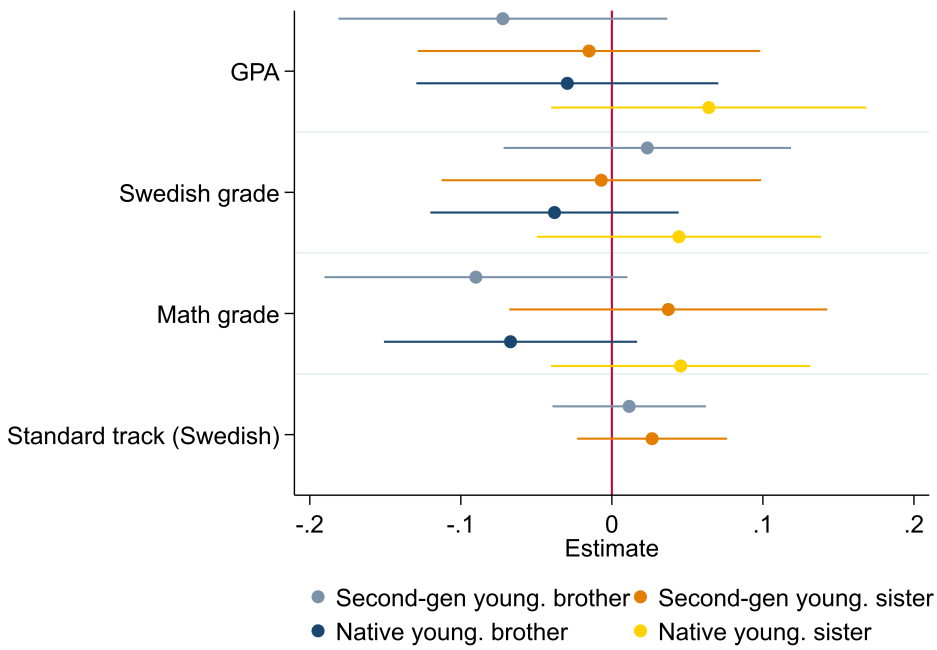
(b) Effects on Oldest Brother

Notes: This figure shows the results of placebo test estimating the spillover effects of having a younger sibling who enters school late on the oldest sibling's school outcomes. The plots show the coefficients from reduced-form RD regressions with a linear control function and a 60-day bandwidth around the January 1^{st} cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. Panel (a) shows the coefficients for oldest sisters who have a younger sister (orange) or brother (blue) who enters school late. Panel (b) shows the corresponding coefficients for an oldest brother.. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the younger sister was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

Figure A4: Benchmark: Policy Impacts Without Taking Family Structure into Account



(a) Direct effects of entering school late

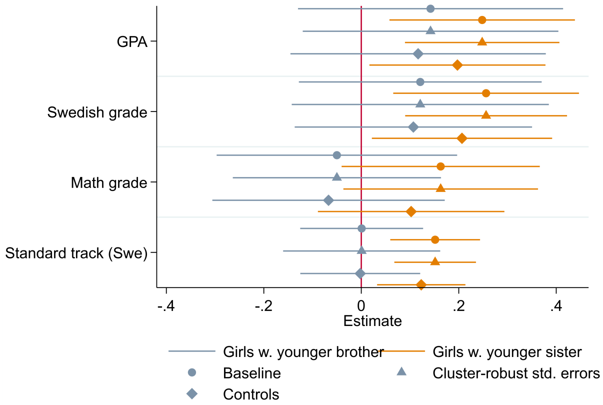


(b) Spillover effects oldest sibling entering school late

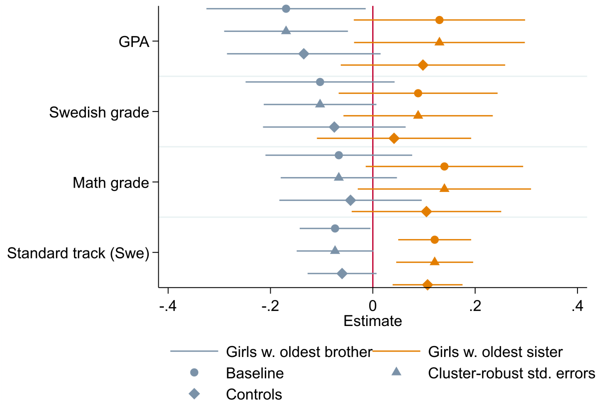
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. The plots show the coefficients from reduced-form RD regressions with a linear control function and a 60-day bandwidth around the January 1^{st} cutoff. Panel (a) shows the direct effects of starting school late on the student's own school outcomes. Panel (b) shows the spillover effects of having an oldest sibling start school late on the younger sibling's school outcomes. Second-generation migrant refers to those born in Sweden to two non-Nordic migrant parents. Native refers to those born to two Swedish-born parents in families where the average income is at the 20th percentile or below. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome 'Standard track (Swedish)' is only measured for migrants since only they are offered the secondary 'Swedish as a second language' track.

Figure A5: Robustness Checks

Clustering Standard Errors / Adding Controls

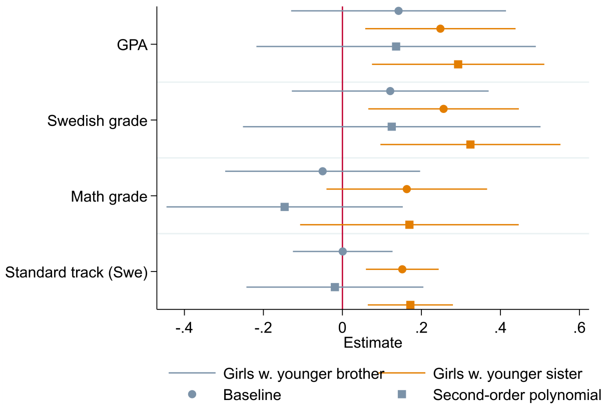


(a) Direct effects oldest sisters

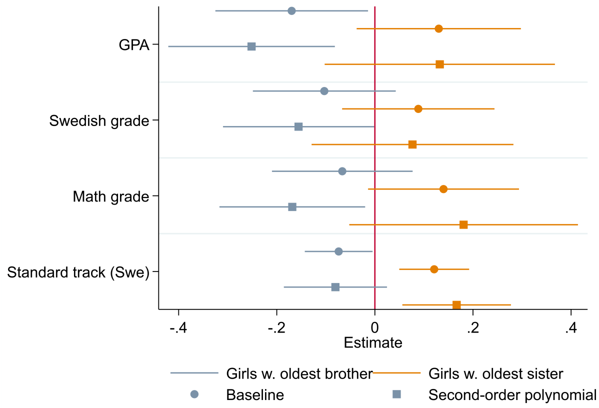


(b) Sibling spillovers younger sisters

Functional Form

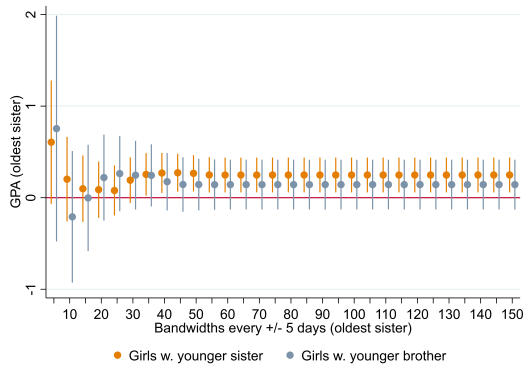


(c) Direct effects oldest sisters

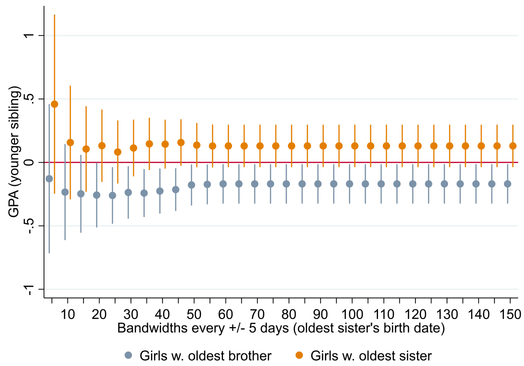


(d) Sibling spillovers younger sisters

Bandwidths



(e) Direct effects oldest sisters



(f) Sibling spillovers younger sisters

Notes: The figure presents robustness checks for the direct effects for first-born girls and sibling spillovers for younger sisters in migrant families. Panels (a)–(d) include a "baseline" estimate which corresponds to the main results. Panel (e)–(f) show the estimates at bandwidths every +/- 5 days around the cutoff, where 60 is the baseline.

Table A5: Robustness: Data Driven Bandwidth Selection

A. Direct Effects First-born Girls	With Younger Sister				With Younger Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	0.249**	0.256***	0.163	0.152***	0.139	0.119	-0.049	0.001
	(0.097)	(0.098)	(0.104)	(0.047)	(0.139)	(0.128)	(0.126)	(0.064)
Bandwidth (+/- days)	57	56	65	59	57	56	65	59
Observations	1,923	1,896	2,002	1,912	1,154	1,147	1,197	1,143
R-squared	0.045	0.052	0.031	0.042	0.039	0.040	0.030	0.054
Outcome mean	0.171	0.0872	-0.0474	0.708	-0.0346	-0.0662	-0.232	0.626

B. Sibling Spillovers Younger Sisters	With Oldest Sister				With Oldest Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	0.129	0.088	0.140*	0.121***	-0.172**	-0.107	-0.067	-0.074**
	(0.086)	(0.080)	(0.078)	(0.036)	(0.080)	(0.075)	(0.073)	(0.035)
Bandwidth (+/- days)	57	56	65	59	57	56	65	59
Observations	3,158	3,127	3,307	3,250	3,017	2,991	3,176	3,118
R-squared	0.042	0.041	0.038	0.080	0.045	0.047	0.034	0.075
Outcome mean	0.00237	-0.00365	-0.213	0.672	-0.0262	-0.0401	-0.223	0.681

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the direct effects and sibling spillovers of late school entry for first-born girls and younger sisters in migrant families when using a data-driven bandwidth selection. The method is developed by Calonico et al. (2017) which automatically chooses the marginal-squared-error optimal bandwidth for each outcome variable. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. Late entry indicates that the oldest sibling was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the younger sister was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

Table A6: Direct Effects by Family Traditional Background

A. First-born Girls with Younger Sister

	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	0.083	0.321***	0.255	0.241**	0.069	0.193	0.046	0.195***
	(0.162)	(0.123)	(0.166)	(0.121)	(0.172)	(0.131)	(0.074)	(0.059)
Observations	667	1,312	671	1,318	671	1,318	645	1,267
R-squared	0.055	0.045	0.068	0.047	0.051	0.026	0.057	0.053
Outcome mean	0.260	0.126	0.190	0.0362	0.0207	-0.0802	0.772	0.675

B. First-born Girls with Younger Brother

	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	-0.006	0.204	-0.114	0.243	-0.142	-0.005	-0.072	0.017
	(0.227)	(0.179)	(0.217)	(0.160)	(0.208)	(0.160)	(0.100)	(0.082)
Observations	388	791	393	798	393	798	378	765
R-squared	0.077	0.032	0.095	0.032	0.056	0.027	0.138	0.043
Outcome mean	-0.0741	-0.0309	-0.0604	-0.0828	-0.250	-0.217	0.611	0.633

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the direct effect of entering school late on end-of-compulsory school outcomes for first-born girls in migrant families by gender-traditional background. A more gender traditional background is defined as having a mother who originates from a country with a Gender Inequality Index (GII) above the sample mean. GII is a composite metric developed by the United Nations Development Program (UNDP) based on nationwide measures of reproductive health, employment and the labor market. Index values were recorded during the same period as the birth cohorts, 1988–2003. Since only the country group of mother's origin is observed in the register data the GII value is the within-group average weighted by the total migrant population from each country in Sweden during the sample period. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of first-born girls born in Sweden to two non-Nordic migrant parents between 1988–2003. Late entry indicates that the first-born girl was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the girl was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

Table A7: Sibling Spillovers by Family Traditional Background

A. Younger Sisters with Oldest Sister	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	0.048	0.180*	0.059	0.083	-0.001	0.207**	0.052	0.160***
	(0.142)	(0.107)	(0.139)	(0.098)	(0.134)	(0.099)	(0.060)	(0.045)
Observations	1,105	2,149	1,117	2,170	1,117	2,170	1,106	2,144
R-squared	0.068	0.043	0.072	0.038	0.074	0.037	0.162	0.077
Outcome mean	0.0219	-0.0134	0.0247	-0.0269	-0.205	-0.219	0.712	0.652

B. Younger Sisters with Oldest Brother	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	-0.056	-0.203**	-0.110	-0.084	0.000	-0.092	-0.003	-0.113**
	(0.131)	(0.101)	(0.120)	(0.094)	(0.119)	(0.093)	(0.059)	(0.044)
Observations	1,079	2,043	1,087	2,060	1,087	2,060	1,084	2,035
R-squared	0.106	0.050	0.116	0.055	0.094	0.040	0.121	0.075
Outcome mean	0.0586	-0.0735	0.0337	-0.0767	-0.179	-0.248	0.707	0.667

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the spillover effect of having an oldest sibling who enters school late on younger sister's end-of-compulsory school outcomes in migrant families by gender-traditional background. A more gender traditional background is defined as having a mother who originates from a country with a Gender Inequality Index (GII) above the sample mean. GII is a composite metric developed by the United Nations Development Program (UNDP) based on nationwide measures of reproductive health, employment and the labor market. Index values were recorded during the same period as the birth cohorts, 1988-2003. Since only the country group of mother's origin is observed in the register data the GII value is the within-group average weighted by the total migrant population from each country in Sweden during the sample period. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. For estimation the sample of younger sisters is divided by the gender of the oldest sibling. Late entry indicates that the oldest sibling was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the younger sister was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

Table A8: Effects on Migrant Mothers' Labor Market Outcomes Child Age Seven

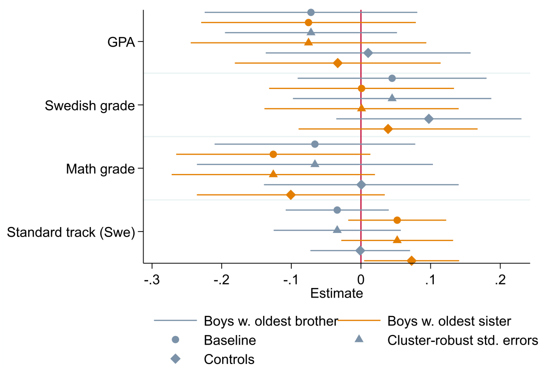
	Girls		Boys	
	(1) Mother earnings (percentiles)	(2) Mother employment	(3) Mother earnings (percentiles)	(4) Mother employment
Late entry	2.434***	0.058***	0.721	0.030**
	(0.819)	(0.015)	(0.795)	(0.014)
Observations	21,195	21,195	22,326	22,326
R-squared	0.006	0.027	0.004	0.023
Outcome mean	28.98	0.387	29.19	0.388

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the effects of having a girl or son who enters school late on migrant mothers' earnings and employment when the child is age 7. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of all children born in Sweden to two non-Nordic mi-grant parents between 1988-2003. For estimation this sample is divided by the gender of the child. Late entry indicates that the child was born in Jan–Feb as opposed to Nov–Dec. Mother's earnings are measured in percentiles which are ranked jointly for native and migrants. Mother employment is a dummy which takes on the value 1 if the mother earns at least half of the median income for 45-year-old women in the Swedish labor force. This value was calculated using the average annual incomes of women aged 44–46 between 1985–2018, deflated to 2018 values. The threshold amounts to 123,766 SEK (12,599 EUR).

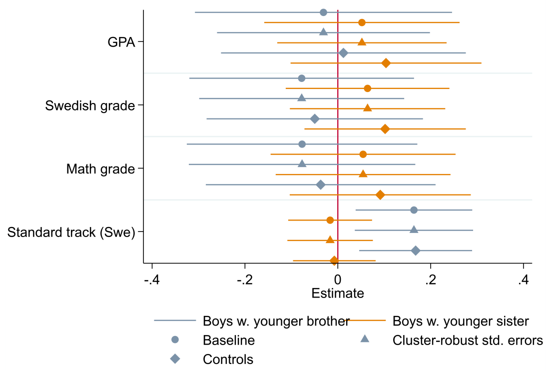
A.2 Second-Generation Migrant Boys

Figure A6: Second-Generation Migrant Boys: Robustness Checks

Clustering standard errors or adding controls

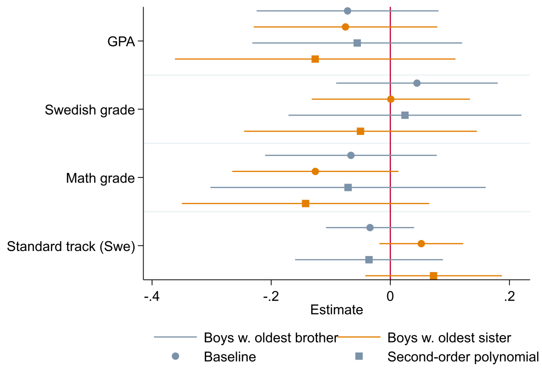


(a) Sibling spillovers younger brothers

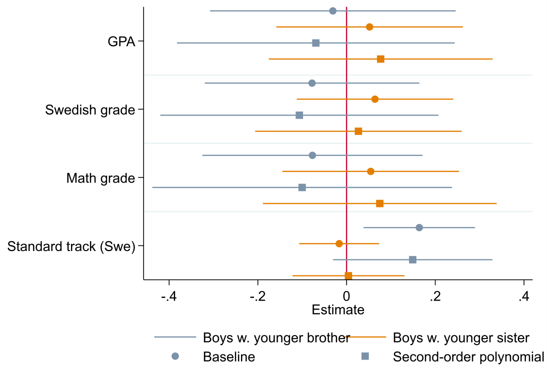


(b) Direct effects oldest brothers

Functional form

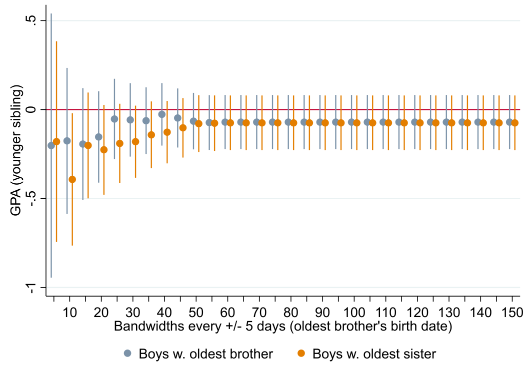


(c) Sibling spillovers younger brothers

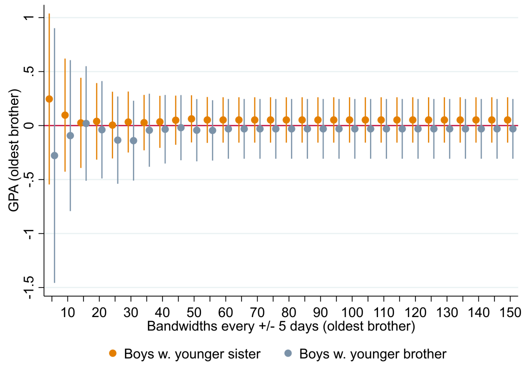


(d) Direct effects oldest brothers

Bandwidths



(e) Sibling spillovers younger brothers



(f) Direct effects oldest brothers

Notes: The figure presents robustness checks for the direct effects for first-born boys and sibling spillovers for younger brothers in migrant families. Panels (a)–(d) check includes a "baseline" estimate which corresponds to the main results. Panel (e)–(f) show the estimates at bandwidths every +/- 5 days around the cutoff, where 60 is the baseline.

Table A9: Second-Generation Migrant Boys: Data Driven Bandwidth Selection

A. Direct Effects First-born Boys	With Younger Sister				With Younger Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	0.052	0.061	0.055	-0.017	-0.036	-0.082	-0.076	0.164**
	(0.108)	(0.091)	(0.102)	(0.046)	(0.142)	(0.124)	(0.126)	(0.064)
Bandwidth (+/- days)	57	56	65	59	57	56	65	59
Observations	1,901	1,896	2,006	1,907	1,158	1,144	1,205	1,153
R-squared	0.047	0.032	0.029	0.051	0.024	0.042	0.022	0.053
Outcome mean	-0.212	-0.364	-0.105	0.673	-0.318	-0.443	-0.214	0.567

B. Sibling Spillovers Younger Brothers	With Oldest Sister				With Oldest Brother			
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) Standard track (Swedish)	(5) GPA	(6) Swedish grade	(7) Math grade	(8) Standard track (Swedish)
Late entry	-0.078	-0.002	-0.125*	0.052	-0.071	0.044	-0.065	-0.035
	(0.079)	(0.068)	(0.071)	(0.036)	(0.078)	(0.070)	(0.073)	(0.038)
Bandwidth (+/- days)	57	56	65	59	57	56	65	59
Observations	3,199	3,193	3,347	3,301	3,024	3,000	3,152	3,103
R-squared	0.056	0.059	0.046	0.083	0.037	0.040	0.032	0.048
Outcome mean	-0.345	-0.488	-0.241	0.632	-0.337	-0.480	-0.232	0.633

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the direct effects and sibling spillovers of late school entry for first-born boys and younger brothers in migrant families when using a data-driven bandwidth selection. The method is developed by which automatically chooses the marginal-squared-error optimal bandwidth for each outcome variable. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. Late entry indicates that the oldest sibling was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the younger sister was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

Table A10: Second-Generation Migrant Boys: Result by Family Traditional Background

A. Direct Effects: Boys with Younger Sister

	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	0.260	-0.036	0.192	0.021	0.232	-0.029	-0.017	-0.007
	(0.167)	(0.137)	(0.151)	(0.112)	(0.172)	(0.126)	(0.076)	(0.058)
Observations	682	1,280	695	1,293	695	1,293	673	1,234
R-squared	0.097	0.042	0.103	0.021	0.083	0.021	0.086	0.059
Outcome mean	-0.148	-0.251	-0.346	-0.378	-0.0757	-0.118	0.724	0.646

B. Direct Effects: Boys with Younger Brother

	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	0.040	-0.067	0.087	-0.134	-0.075	-0.090	0.130*	0.079
	(0.272)	(0.168)	(0.224)	(0.146)	(0.216)	(0.154)	(0.073)	(0.052)
Observations	352	834	356	838	356	838	777	1,673
R-squared	0.068	0.026	0.126	0.048	0.069	0.021	0.085	0.036
Outcome mean	-0.312	-0.330	-0.464	-0.438	-0.247	-0.199	0.615	0.611

C. Spillovers: Younger Brothers with Oldest Sister

	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	-0.283**	0.052	-0.100	0.040	-0.268**	-0.044	-0.084	0.124***
	(0.136)	(0.096)	(0.117)	(0.083)	(0.119)	(0.090)	(0.060)	(0.044)
Observations	1,107	2,179	1,126	2,205	1,126	2,205	1,113	2,188
R-squared	0.096	0.057	0.085	0.064	0.091	0.045	0.153	0.081
Outcome mean	-0.358	-0.346	-0.482	-0.492	-0.272	-0.223	0.641	0.627

D. Spillovers: Younger Brothers with Oldest Brother

	GPA		Swedish grade		Math grade		Standard track (Swe)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender traditional	Less	More	Less	More	Less	More	Less	More
Late entry	0.010	-0.126	0.129	0.010	0.073	-0.148*	-0.071	-0.032
	(0.140)	(0.095)	(0.125)	(0.083)	(0.134)	(0.089)	(0.068)	(0.046)
Observations	996	2,107	1,009	2,121	1,009	2,121	995	2,109
R-squared	0.060	0.052	0.083	0.048	0.067	0.041	0.095	0.058
Outcome mean	-0.353	-0.328	-0.496	-0.477	-0.253	-0.222	0.658	0.620

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses. This table presents the direct effects and sibling spillovers of late school entry for first-born boys and younger brothers in migrant families by gender-traditional back-ground. A more gender traditional background is defined as having a mother who originates from a country with a Gender Inequality Index (GII) above the sample mean. GII is a composite metric developed by the United Nations Development Program (UNDP) based on nationwide measures of reproductive health, employment and the labor market. Index values were recorded during the same period as the birth cohorts, 1988–2003. Since only the country group of mother's origin is observed in the register data the GII value is the within-group average weighted by the total migrant population from each country in Sweden during the sample period. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of siblings born in Sweden to two non-Nordic migrant parents between 1988–2003. For estimation the sample of younger brothers is divided by the gender of the oldest sibling. Late entry indicates that the oldest sibling was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one. The outcome "Standard track (Swedish)" is a dummy which takes on the value 1 if the younger sister was enrolled in the regular Swedish track in the final year of compulsory school, instead of the "Swedish as a second-language" track.

A.3 Natives

This appendix shows the main results replicated for native students. Before interpreting the results, it is important to note a major caveat: native parents seem to systematically the time of births after the January 1^{st} cutoff. Figure A7 shows the specification tests. Graph (a) shows bunching in the number of births of native children right after January 1^{st}, and the share of births remain relatively higher for the first 60 days of the year. The density-discontinuity test in Graph (c) also indicate manipulation. Furthermore, Table A11, columns (1)–(4), indicate that having high-earning or well educated parents predict being born after the cutoff.

Altogether it seems like native Swedish parents, especially those with high socioeconomic status, time the birth of their children to the beginning of the year. This creates a major issue for interpretation of the regression discontinuity design estimates for native students, since we cannot be sure that any positive effect on grades are due to the maturity advantage of late school entry or from systematically being more likely to come from a family with relatively high socioeconomic status.

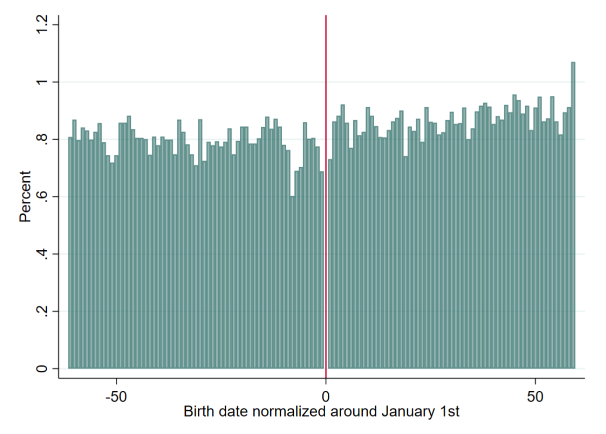
To handle this issue I select a sample of native students, for which the underlying assumption holds. This sample consists of children born in Sweden to two Swedish-born parents whose joint annual average earnings were at the 20th income percentile or below while the child was of day care age. Graphs (b) and (d) in Figure A7 indicate that there was no systematic manipulation of births after the cutoff for this sample. Columns (5)–(8) in Table A11 also show that parental background variables do not predict births after the cutoff.

In terms of socioeconomic status of the family, this new sample of native students are actually more comparable to second-generation migrant students. The annual average income for a family at the 20th percentile is around 123,766 SEK.[10] Comparing this to density distributions of native and migrant families across annual incomes in Figure 1.1, the median migrant family in the sample earns 171,057 SEK per year on average, while the median native family earns 435,321 SEK.

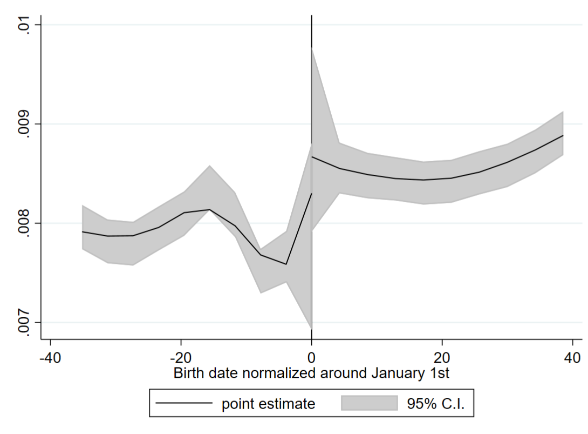
[10] The income percentiles are jointly calculated for the families in the native and second-generation migrant samples.

Figure A7: Natives: Specification Checks

Families with Low Socioeconomic Status

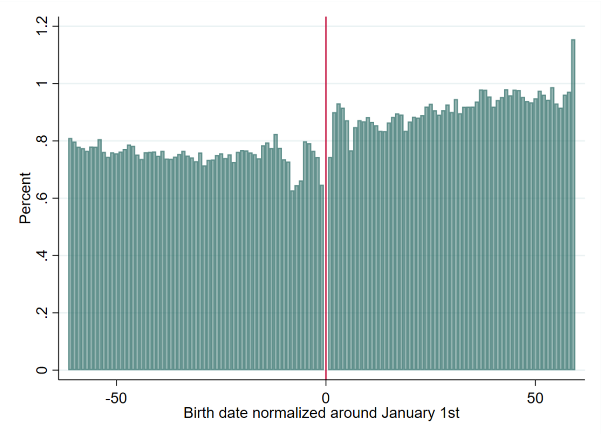


(a) Distribution of births

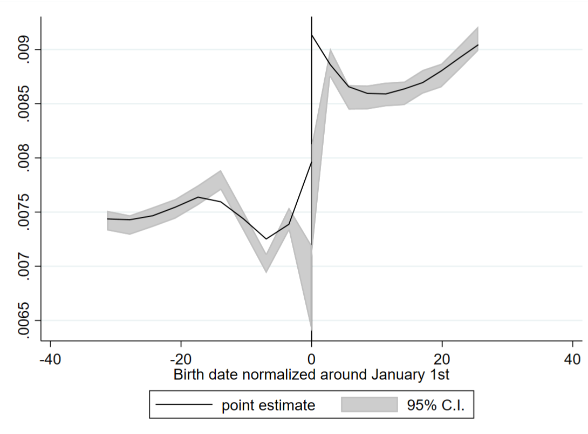


(b) Density test

Full Native Sample



(c) Distribution of births



(d) Density test

Notes: Panels (a)–(b) shows the percent of native children born each day between November 1^{st} and February 28^{th}. Leap-year births are recoded to February 28^{th}. Panels (c)–(d) shows the density discontinuity of the running variable (birthdate) at the January 1^{st} cutoff. This test uses local-polynomial density estimators as explained in Cattaneo et al. (2018).

Table A11: Placebo: Parent Background Characteristics

	Full Native Sample				Low Socioeconomic Status Sample			
	Fathers		Mothers		Fathers		Mothers	
	(1) Schooling (Years)	(2) Earnings (Percentiles)	(3) Schooling (Years)	(4) Earnings (Percentiles)	(5) Schooling (Years)	(6) Earnings (Percentiles)	(7) schooling (Years)	(8) Earnings (Percentiles)
Late entry	0.016	0.856***	0.063***	1.283***	-0.028	0.125	0.082	-0.059
	(0.023)	(0.251)	(0.020)	(0.243)	(0.064)	(0.243)	(0.057)	(0.475)
Observations	241,804	241,804	241,804	241,804	30,846	30,846	30,846	30,846
R-squared	0.017	0.024	0.042	0.047	0.017	0.007	0.019	0.025
Outcome mean	11.86	54.12	12.16	53.45	10.87	14.03	11.18	25.71

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents placebo regressions of first-born child being born after the January 1^{st} on parent's background characteristics. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Point estimates in columns (1)–(4) are estimated for all native children born in Sweden between 1988–2003, while those in (5)–(8) are estimated for native children in families with annual incomes at the 20th percentile or below when the child is of daycare age. Late entry indicates that the first-born child in the family was born in Jan–Feb as opposed to Nov–Dec. Parent's schooling and earnings are measured after the child is born, at ages 3–5, but before the child enters school.

Table A12: Native Families with Low Socioeconomic Status

A. Direct Effects: First-born Girls	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.176**	0.111	0.059	0.101	0.102	-0.052
	(0.079)	(0.085)	(0.074)	(0.112)	(0.123)	(0.105)
Observations	3,229	3,183	3,229	1,694	1,645	1,694
R-squared	0.036	0.041	0.028	0.040	0.049	0.035
Outcome mean	0.150	-0.0504	-0.141	-0.0411	-0.246	-0.307
B. Direct Effects: First-born Boys	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.167**	0.118	0.132*	0.135	0.074	-0.017
	(0.067)	(0.078)	(0.074)	(0.095)	(0.113)	(0.099)
Observations	3,466	3,402	3,466	1,956	1,906	1,956
R-squared	0.033	0.042	0.026	0.041	0.048	0.043
Outcome mean	-0.512	-0.482	-0.254	-0.569	-0.572	-0.334
C. Sibling Spillovers: Younger Sisters	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.082	0.042	0.022	0.062	0.064	0.074
	(0.074)	(0.068)	(0.061)	(0.077)	(0.068)	(0.063)
Observations	4,365	4,459	4,459	4,687	4,790	4,790
R-squared	0.015	0.020	0.013	0.013	0.016	0.012
Outcome mean	-0.231	-0.0143	-0.301	-0.295	-0.0914	-0.327
D. Sibling Spillovers: Younger Brothers	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	-0.024	0.015	-0.068	-0.028	-0.086	-0.059
	(0.074)	(0.061)	(0.062)	(0.070)	(0.058)	(0.059)
Observations	4,396	4,523	4,523	4,618	4,754	4,754
R-squared	0.013	0.011	0.010	0.014	0.015	0.017
Outcome mean	-0.605	-0.602	-0.368	-0.639	-0.625	-0.396

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the direct effect of entering school late on end-of-compulsory school outcomes for first-born children, as well as the spillover effect of having an oldest sibling who enters school late on younger sister's end-of-compulsory school outcomes in native families with low socioeconomic status. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of all native children born in Sweden between 1988–2003, in families with annual incomes at the 20th percentile or below when the child is of daycare age. For the estimations the sample of first-born children is divided by the gender of the second-born younger sibling, while the sample of younger siblings is divided by the gender of the oldest sibling. Late entry indicates that the first-born child was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean zero and a standard deviation of one.

Table A13: Full Native Sample

A. Direct Effects: First-born Girls	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.220***	0.160***	0.116***	0.163***	0.168***	0.130***
	(0.026)	(0.026)	(0.028)	(0.044)	(0.045)	(0.046)
Observations	25,413	25,311	25,413	10,003	9,917	10,003
R-squared	0.028	0.031	0.018	0.034	0.042	0.028
Outcome mean	0.485	0.390	0.257	0.360	0.227	0.104
B. Direct Effects: First-born Boys	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.181***	0.171***	0.148***	0.158***	0.119***	0.072*
	(0.025)	(0.025)	(0.027)	(0.038)	(0.039)	(0.041)
Observations	26,836	26,674	26,836	12,133	12,030	12,133
R-squared	0.024	0.038	0.021	0.029	0.043	0.027
Outcome mean	-0.166	-0.0391	0.104	-0.233	-0.129	0.0393
C. Sibling Spillovers: Younger Sisters	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.013	0.019	-0.000	-0.008	-0.027	-0.006
	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)	(0.024)
Observations	32,911	33,139	33,139	35,096	35,350	35,350
R-squared	0.006	0.006	0.006	0.007	0.008	0.005
Outcome mean	0.207	0.323	0.0652	0.164	0.281	0.0343
D. Sibling Spillovers: Younger Brothers	With Younger Sister			With Younger Brother		
	(1) GPA	(2) Swedish grade	(3) Math grade	(4) GPA	(5) Swedish grade	(6) Math grade
Late entry	0.006	0.018	0.012	0.006	-0.002	0.008
	(0.023)	(0.022)	(0.024)	(0.022)	(0.021)	(0.023)
Observations	34,542	34,851	34,851	36,607	36,991	36,991
R-squared	0.007	0.006	0.007	0.008	0.008	0.008
Outcome mean	-0.190	-0.305	-0.0458	-0.205	-0.323	-0.0521

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the direct effect of entering school late on end-of-compulsory school outcomes for first-born children, and the spillover effect of having an oldest sibling who enters school late on younger sister's end-of-compulsory school outcomes in native families. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Sample of all native children born in Sweden between 1988–2003. For the estimations the sample of first-born children is divided by the gender of the second-born younger sibling and the sample of younger siblings is divided by the gender of the oldest sibling. Late entry indicates that the first-born child was born in Jan–Feb as opposed to Nov–Dec. GPA, Swedish and math grades are standardized to have a mean of zero and a standard deviation of one.

Table A14: Effects on Native Mothers' Labor Market Outcomes Child Age Seven

A. Low Socioeconomic Status Sample	Girls		Boys	
	(1) Mother earnings (percentiles)	(2) Mother employment	(3) Mother earnings (percentiles)	(4) Mother employment
Late entry	0.325	0.009	0.429	0.021*
	(0.602)	(0.012)	(0.585)	(0.012)
Observations	30,904	30,904	32,566	32,566
R-squared	0.003	0.017	0.003	0.020
Outcome mean	30.22	0.408	30.23	0.412
B. Full Native Sample	Girls		Boys	
	(1) Mother earnings (percentiles)	(2) Mother employment	(3) Mother earnings (percentiles)	(4) Mother employment
Late entry	0.951***	0.024***	1.335***	0.028***
	(0.287)	(0.004)	(0.278)	(0.004)
Observations	197,980	197,980	210,500	210,500
R-squared	0.001	0.019	0.001	0.019
Outcome mean	51.75	0.729	51.79	0.729

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This table presents RD-regressions for the effects of having a girl or son who enters school late on native mothers' earnings and employment when the child is age 7. Reduced-form RD regression with a linear control function and a 60-day bandwidth around the cutoff. Panel A presents estimates for mothers in native families with annual incomes at the 20th percentile or below when the child is of daycare age, while panel B presents estimates for all native mothers. The sample consists of families where the siblings were born between 1988–2003. For estimation this sample is divided by the gender of the child. Late entry indicates that the child was born in Jan–Feb as opposed to Nov–Dec. Mother's earnings are measured in percentiles which are ranked jointly for native and migrants. Mother employment is a dummy which takes on the value 1 if the mother earns at least half of the median income for 45-year-old women in the Swedish labor force. This value was calculated using the average annual incomes of women aged 44–46 between 1985–2018, deflated to 2018 values. The threshold amounts to 123,766 SEK (12,599 EUR).

A.4 Conceptual Framework

This appendix provides details on the comparative statics discussed in Section 1.6.1. Specifically, I formally derive $\partial I_1^*/\partial\sigma_1$, capturing the parental investment response to an increase in child 1's school starting age. By the implicit function theorem, the sign of $\partial I_1^*/\partial\sigma_1$ is determined by the sign of $\partial G/\partial\sigma_1$, where G is defined in eq. 1.5. This derivative is given by:

$$\frac{\partial G}{\partial \sigma_1} = \gamma_1 \left[U_1 f_{12}^1 + f_1^1 \left(\gamma_1 U_{11} f_2^1 + \gamma_2 U_{12} f_2^2\right)\right] - \gamma_2 \left[U_2 f_{12}^2 + f_1^2 \left(\gamma_1 U_{12} f_2^1 + \gamma_2 U_{22} f_2^2\right)\right], \quad (1)$$

where the derivatives of $U(\cdot)$ and $f^i(\cdot)$ are defined analogous to those in the main text. Consider first the limiting case where $\gamma_2 \to 0$, i.e., the case of biased parental preferences where only the human capital of child 1 has weight in the parent's utility function. The derivative is then given by

$$\frac{\partial G}{\partial \sigma_1} = \gamma_1 U_1 f_{12}^2 + \gamma_1^2 f_1^1 f_2^1 U_{11}. \quad (2)$$

The sign of this derivative is primarily determined by the first term, as the second term is of second order and generally small. The first term is strictly positive, reflecting that when child 1 enters school later, the parent will increase investments in that child beforehand to maximize the benefits of late school entry. Since the parent faces a budget constraint, investments in child 2 will decrease as a result.

The same conclusion can be drawn by evaluating the derivative in eq. 1 at symmetry, i.e., where $f^i(\cdot) = f(\cdot)$. In this case,

$$\left.\frac{\partial G}{\partial \sigma_1}\right|_{sym} = sign(\gamma_1 - \gamma_2). \quad (3)$$

Thus, at symmetry, increasing child 1's school starting age will increase parental investment in them while reducing investment in child 2 if parental preferences are biased toward child 1.

More generally, however, the sign of $\partial G/\partial\sigma_1$ (and consequently $\partial I_1^*/\partial\sigma_1$) can also depend on technological differences in the production functions $f^1(\cdot)$ and $f^2(\cdot)$, even if parental preferences are neutral (i.e., $\gamma_1 = \gamma_2$). For example, even if $\gamma_1 = \gamma_2$, the derivative in eq. (D.1) can be positive if there are strong complementarities between child 1's school starting age and parental investments in them (f_{12}^1 significantly larger than f_{12}^2).

B Appendix to Chapter 2: Life After Divorce

Table B1: Sample Restrictions

Restriction	Cases	Observations
None	106,834	157,494
No legal guardian cases	91,585	142,224
No divorce & alimony-only cases	62,285	96,694
Case includes legal or physical custody ruling	56,195	87,082
Non-missing parent IDs	49,406	76,585
Mother age at first childbirth <45	49,300	76,422
Parent ID found in register	48,664	75,474
No unreachable defendant	44,825	69,195
No rubber stamp cases	39,508	61,145
No reopened cases	35,682	55,321
Judge handles at least 50 cases	11,641	17,981
At least 2 judges per court-year	11,608	17,948

Notes: This table describes how the sample of children observed in custody cases between 1992–2021 in the Swedish district courts changes as sample restrictions are applied . The initial sample consists of divorce and custody court cases provided by the 53 courts described in the Data section. The number of observations exceeds the number of cases since one observation is a child-case combination. Rubber stamp cases refers to cases that are two pages or less. After the step "No reopened cases", when the data is reshaped to be unique for each child, observations reflect children in the sample.

Table B2: Testing for Random Assignment of Custody Cases to Judges

	Joint Custody		Judge Propensity	
	(1) Coefficient Estimate	(2) Standard Error	(3) Coefficient Estimate	(4) Standard Error
Plaintiff is female	-.0927***	(.0106)	-.0024	(.0021)
Child female	.0033	(.0069)	.0009	(.0014)
Mother age	-.0018*	(.0010)	.0002	(.0002)
Father age	-.0001	(.0008)	-.0001	(.0002)
Child age	-.0063***	(.0012)	-.0001	(.0002)
Child foreign born	.0038	(.0177)	-.0034	(.0037)
Mother foreign born	.0036	(.0124)	.0011	(.0026)
Father foreign born	-.0676***	(.0123)	-.0002	(.0025)
Crimes committed 2 years before ruling, father	-.0506***	(.0104)	.0021	(.0022)
Crimes committed 2 years before ruling, mother	-.0373***	(.0125)	.0009	(.0026)
Parents cohabiting 2 years before ruling	-.0121	(.0110)	-.0018	(.0023)
Number of children in case	-.0085	(.0058)	.0022*	(.0013)
Positive earnings 2 years before ruling, mother	-.0914*	(.0483)	-.0107	(.0109)
Log(earnings) 2 years before ruling, mother	.0123**	(.0050)	.0008	(.0011)
Positive earnings 2 years before ruling, father	-.2829***	(.0505)	-.0017	(.0111)
Log(earnings) 2 years before ruling, father	.0369***	(.0050)	.0002	(.0011)
Father education level: Compulsory	-.0207	(.0292)	.0101	(.0071)
Father education level: High School	.0359	(.0289)	.0102	(.0070)
Father education level: College	.0574*	(.0298)	.0078	(.0071)
Mother education level: Compulsory	.0104	(.0306)	.0067	(.0063)
Mother education level: High School	.0088	(.0304)	.0070	(.0062)
Mother education level: College	.0426	(.0313)	.0070	(.0063)
Child age missing	-.0029	(.0472)	-.0052	(.0094)
Child foreign born missing	.0915	(.0751)	.0043	(.0145)
Mother foreign born missing	.0100	(.0448)	-.0004	(.0089)
Father foreign born missing	-.0048	(.0432)	-.0014	(.0086)
Crimes committed 2 years before ruling, father missing	.0174	(.0359)	-.0060	(.0056)
Crimes committed 2 years before ruling, mother missing	-.0293	(.0442)	.0080	(.0072)
Parents cohabiting 2 years before ruling missing	-.0247	(.0404)	.0163**	(.0082)
Log(earnings) 2 years before ruling, mother missing	.1043*	(.0631)	-.0105	(.0112)
Log(earnings) 2 years before ruling, father missing	-.0623	(.0473)	.0032	(.0110)
Constant	.4908***	(.0494)	.3635***	(.0110)
Observations	17948		17948	
F-statistic for joint test	18.1677		.7230	
p-value	.0000		.8694	

Notes: This table shows estimates from regressing children and parents characteristics on the case ruling and judge propensity. The estimation sample consists of children in the court data after applying relevant restrictions. The data come from multiple administrative records provided by Statistics Sweden. Columns (1) and (2) present the coefficient estimates and standard errors for regressing the presented variables on joint custody. Columns (3) and (4) present the coefficient estimates and standard errors for regressing the presented variables on the leave-out measure of judge propensity for joint custody. All estimates control for court-by-year fixed effects. The omitted category for education level for mothers and fathers is education level missing. Reported F-statistic refers to joint tests of the null hypothesis for all variables. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B3: First Stage

	Joint Custody	
	(1)	(2)
Judge Propensity for Joint Custody	.614***	.617***
	(.0385)	(.0376)
Observations	17948	17948
F-statistic (instrument)	254.6	37.80
Controlling for balance variables	No	Yes

Notes: This table depicts the first stage regression of the leave-out measure of judge propensity for joint custody on a dummy for joint custody being assigned in the case. The estimation sample consists of children in the court data after applying relevant restrictions. The regression controls for variables listed in table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B4: Heterogeneity Test Scores by Children's Age at Ruling

	(1) First Test After Ruling Grade 3	(2) First Test After Ruling Grade 6	(3) First Test After Ruling Grade 9
Joint Custody	.681*	.663	.156
	(.379)	(.621)	(.313)
Observations	5063	1917	1385
Mean of Dependent Variable	-.265	-.363	-.252

Notes: The table presents IV estimates of joint custody on the first national test taken after the custody ruling. Outcomes are average scores in Swedish and math. Mean of the Dependent Variable shows the sample mean of children included in the estimation sample, test scores were standardized to be mean zero on the full population of test takers. All estimates control for variables listed in table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B5: Effect of Joint Custody on Quality of Schools Attended

	(1) Residual School Quality Grade 3	(2) Residual School Quality Grade 6	(3) Residual School Quality Grade 9
Joint Custody	.368***	.0206	-.0327
	(.142)	(.101)	(.0692)
Observations	4239	3692	3032
Mean of Dependent Variable	-.0235	-.0237	.000667

Notes: The table shows IV estimates of joint custody on the quality of schools that children in the court data attend after ruling. The school quality measure is constructed by regressing each student's average math and Swedish score on the gender and age of the child, the migration status of the child and both parents, parents' highest completed education, and parents' earnings, separately by grade and year of the test. For this measure we include the full population of students in Sweden from whom we have math and Swedish scores. We take the residuals of this regression and calculate the leave-out residual. This measure of school quality gets us as close as possible to a school value-added measure with the data. All estimates control for variables listed in Appendix Table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B6: Effect of Joint Custody on Children's Mental Health Medications

	(1) 1 year after ruling	(2) 2 years after ruling	(3) 3 years after ruling	(4) 4 years after ruling	(5) 5 years after ruling
A. Prescriptions Year-By-Year					
Antidepressants					
Shared custody	-.0192	-.00195	-.00753	.0102	.0211
	(.0160)	(.0177)	(.0215)	(.0257)	(.0301)
Observations	14704	14480	13501	12428	11280
Mean of Dependent Variable	.00911	.0146	.0192	.0237	.0309
Anti-anxiety					
Shared custody	-.0208	.0134	-.0178	.0262	.0782
	(.0303)	(.0318)	(.0383)	(.0414)	(.0477)
Observations	14704	14480	13501	12428	11280
Mean of Dependent Variable	.0346	.0407	.0560	.0623	.0723
ADHD					
Shared custody	.0224	.0304	.0545	.0148	.0271
	(.0286)	(.0337)	(.0384)	(.0409)	(.0469)
Observations	14704	14480	13501	12428	11280
Mean of Dependent Variable	.0326	.0417	.0527	.0625	.0734
B. Continuous Prescriptions					
Antidepressants					
Shared custody		-.00750	-.00342	-.000764	-.00137
		(.0125)	(.0111)	(.00942)	(.00892)
Observations		14704	14704	14681	14704
Mean of Dependent Variable		.00673	.00449	.00313	.00218
Anti-anxiety					
Shared custody		.00688	.0111	.00502	.00780
		(.0246)	(.0206)	(.0181)	(.0166)
Observations		14704	14704	14616	14704
Mean of Dependent Variable		.0215	.0145	.00985	.00680
ADHD					
Shared custody		.0305	.0108	.00485	.000193
		(.0281)	(.0261)	(.0242)	(.0227)
Observations		14704	14704	14615	14704
Mean of Dependent Variable		.0282	.0224	.0171	.0135

Notes: The table shows IV estimates of joint custody on the probability of children having a prescription in year t after the custody ruling. Estimates for continuous prescriptions show the probability of having a prescription in all years 1-t after the ruling. All control for variables listed in Appendix Table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B7: Effect of Joint Custody on Fathers' Earnings

	(1) 1 year after ruling	(2) 2 years after ruling	(3) 3 years after ruling	(4) 4 years after ruling	(5) 5 years after ruling
A. Earning above age-adjusted earnings, 25th percentile					
Joint Custody	.113	.219***	.221***	.270***	.238***
	(.0753)	(.0766)	(.0774)	(.0793)	(.0827)
Observations	17630	17134	15932	14710	13486
Mean of Dependent Variable	.532	.533	.531	.544	.544
B. Earning above age-adjusted earnings, 50th percentile					
Joint Custody	.0667	.157**	.179**	.149**	.0885
	(.0701)	(.0712)	(.0731)	(.0727)	(.0754)
Observations	17630	17134	15932	14710	13486
Mean of Dependent Variable	.286	.289	.293	.298	.303
C. Earning above age-adjusted earnings, 75th percentile					
Joint Custody	.0506	.0600	.0377	.0188	-.00692
	(.0538)	(.0549)	(.0555)	(.0543)	(.0559)
Observations	17630	17134	15932	14710	13486
Mean of Dependent Variable	.128	.126	.127	.130	.131

Notes: This table show IV estimates of joint custody on the probability of the fathers' individual earnings being above the stated quartile of the same gender and age reference group of the full Swedish population in year t after the custody ruling. All estimates control for variables listed in Appendix Table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level.*** p<.01, ** p<.05, * p<.1.

Table B8: Effect of Joint Custody on Fathers' Mental Health Medications

	(1) 1 year after ruling	(2) 2 years after ruling	(3) 3 years after ruling	(4) 4 years after ruling	(5) 5 years after ruling
A. Prescriptions Year-By-Year					
Antidepressants					
Shared custody	-.199***	-.212***	-.258***	-.146**	-.0420
	(.0730)	(.0701)	(.0750)	(.0701)	(.0651)
Observations	14742	14541	13683	12551	11493
Mean of Dependent Variable	.123	.117	.119	.122	.118
Antianxiety					
Shared custody	-.0841	-.0927	-.0310	-.0564	-.0559
	(.0744)	(.0708)	(.0715)	(.0727)	(.0695)
Observations	14742	14541	13683	12551	11493
Mean of Dependent Variable	.145	.137	.137	.144	.135
B. Continuous Prescriptions					
Antidepressants					
Shared custody		-.216***	-.182***	-.136***	-.0910**
		(.0614)	(.0537)	(.0459)	(.0388)
Observations		14742	14742	14467	14742
Mean of Dependent Variable		.0750	.0536	.0401	.0292
Antianxiety					
Shared custody		-.120**	-.119**	-.109**	-.130***
		(.0587)	(.0501)	(.0458)	(.0415)
Observations		14742	14742	14410	14742
Mean of Dependent Variable		.0853	.0608	.0466	.0333

Notes: The Table shows IV estimates of joint custody on the probability of fathers having a prescription in year t after the custody ruling. Continuous estimates show the probability of having a prescription in all years 1-t after the ruling. All estimates control for variables listed in table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B9: Effect of Joint Custody on Mothers' Earnings

	(1) 1 year after ruling	(2) 2 years after ruling	(3) 3 years after ruling	(4) 4 years after ruling	(5) 5 years after ruling
A. Earning above age-adjusted earnings, 25th percentile					
Joint Custody	-.0469	-.0495	.0601	.0936	.0211
	(.0753)	(.0779)	(.0767)	(.0766)	(.0814)
Observations	17764	17354	16192	15024	13846
Mean of Dependent Variable	.599	.600	.610	.610	.618
B. Earning above age-adjusted earnings, 50th percentile					
Joint Custody	-.0256	-.0865	.0110	.0393	.00165
	(.0736)	(.0749)	(.0739)	(.0744)	(.0786)
Observations	17764	17354	16192	15024	13846
Mean of Dependent Variable	.321	.325	.334	.342	.353
C. Earning above age-adjusted earnings, 75th percentile					
Joint Custody	-.00508	-.0267	.0284	-.0125	-.0399
	(.0580)	(.0606)	(.0590)	(.0595)	(.0630)
Observations	17764	17354	16192	15024	13846
Mean of Dependent Variable	.134	.140	.140	.147	.152

Notes: The table shows IV estimates of joint custody on the probability of mothers' individual earnings being above the stated quartile of the same gender and age reference group of the full Swedish population in year t after the custody ruling. All estimates control for variables listed in table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table B10: Effect of Joint Custody on Mothers' Mental Health Medications

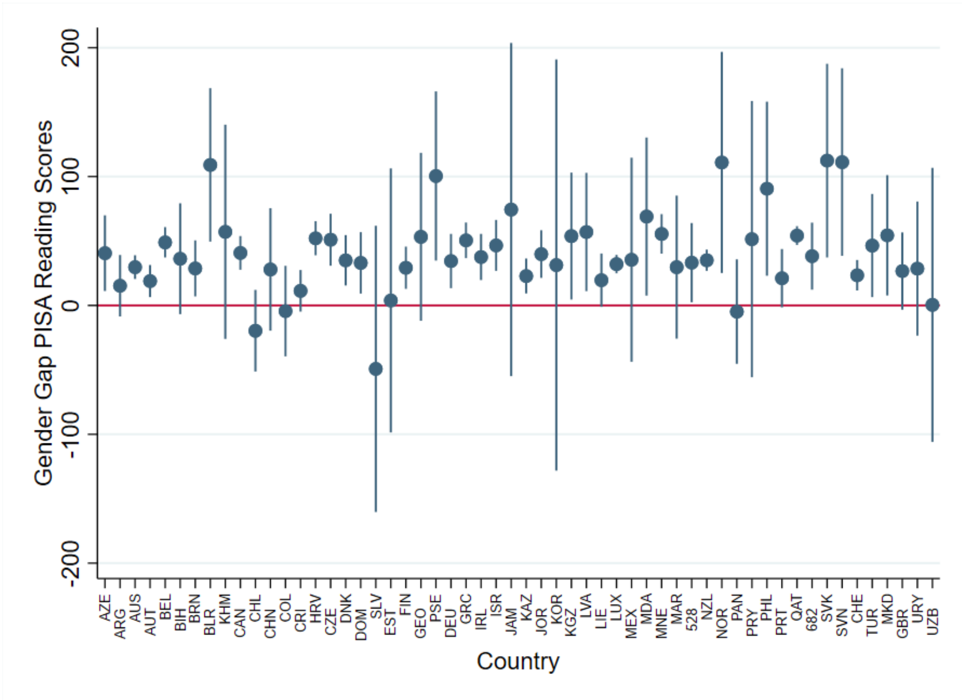
	(1) 1 year after ruling	(2) 2 years after ruling	(3) 3 years after ruling	(4) 4 years after ruling	(5) 5 years after ruling
A. Prescriptions Year-By-Year					
Antidepressants					
Shared custody	-.00983	-.0253	-.0544	-.0499	-.115
	(.0858)	(.0877)	(.0872)	(.0878)	(.0861)
Observations	14812	14688	13931	12818	11680
Mean of Dependent Variable	.198	.198	.197	.203	.198
Antianxiety					
Shared custody	-.0246	-.0426	-.0575	-.0495	-.0318
	(.0863)	(.0892)	(.0883)	(.0873)	(.0866)
Observations	14812	14688	13931	12818	11680
Mean of Dependent Variable	.202	.200	.197	.197	.203
B. Continuous Prescriptions					
Antidepressants					
Shared custody		.00638	.0126	-.0171	-.0396
		(.0751)	(.0677)	(.0631)	(.0548)
Observations		14812	14812	14326	14812
Mean of Dependent Variable		.128	.0926	.0727	.0531
Antianxiety					
Shared custody		.0101	.0608	.0417	.0259
		(.0721)	(.0625)	(.0576)	(.0513)
Observations		14812	14812	14365	14812
Mean of Dependent Variable		.121	.0868	.0684	.0533

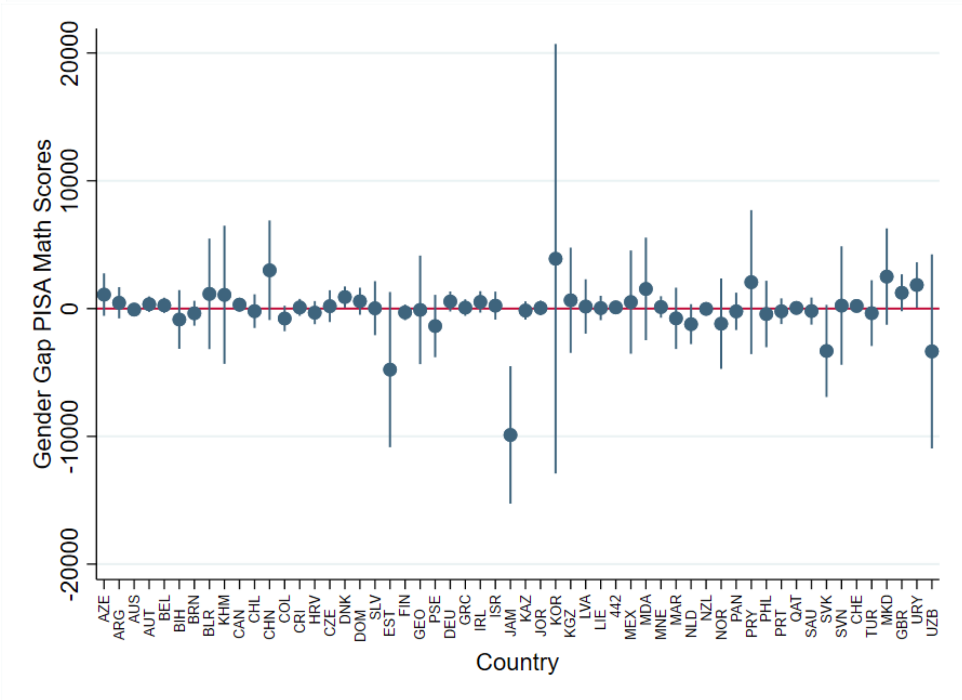
Notes: The table shows IV estimates of joint custody on the probability of mothers having a prescription in year t after the custody ruling. Continuous estimates show the probability of having a prescription in all years 1-t after the ruling. All estimates control for variables listed in table B2 and court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

C Appendix to Chapter 3: Linguistic Distance and the Gender Gap

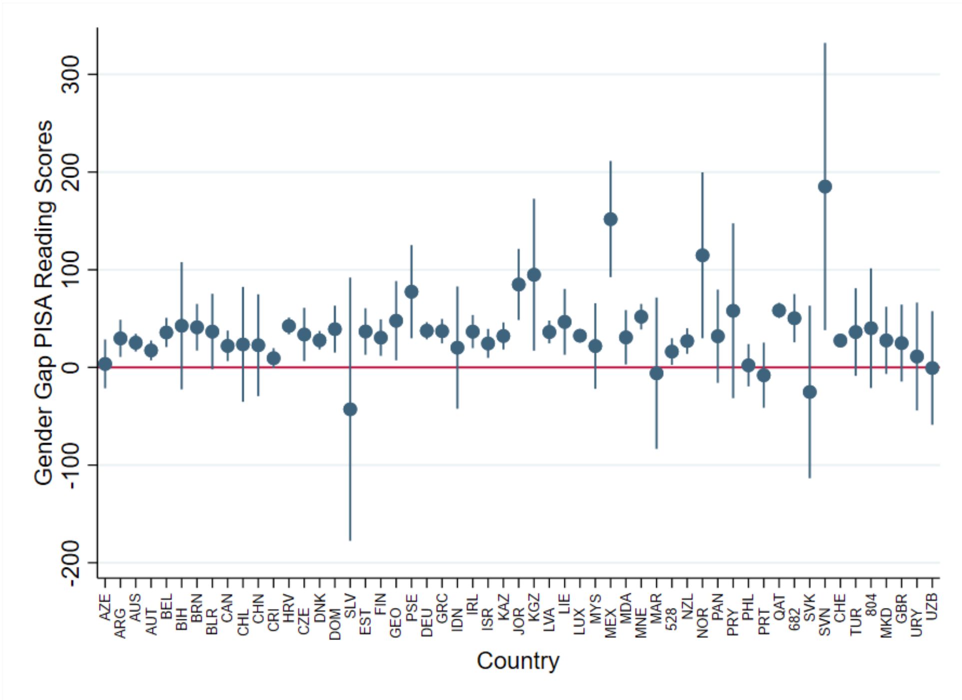
Figure C1: Average Migrant Gender Gap in PISA Test by Host Countries

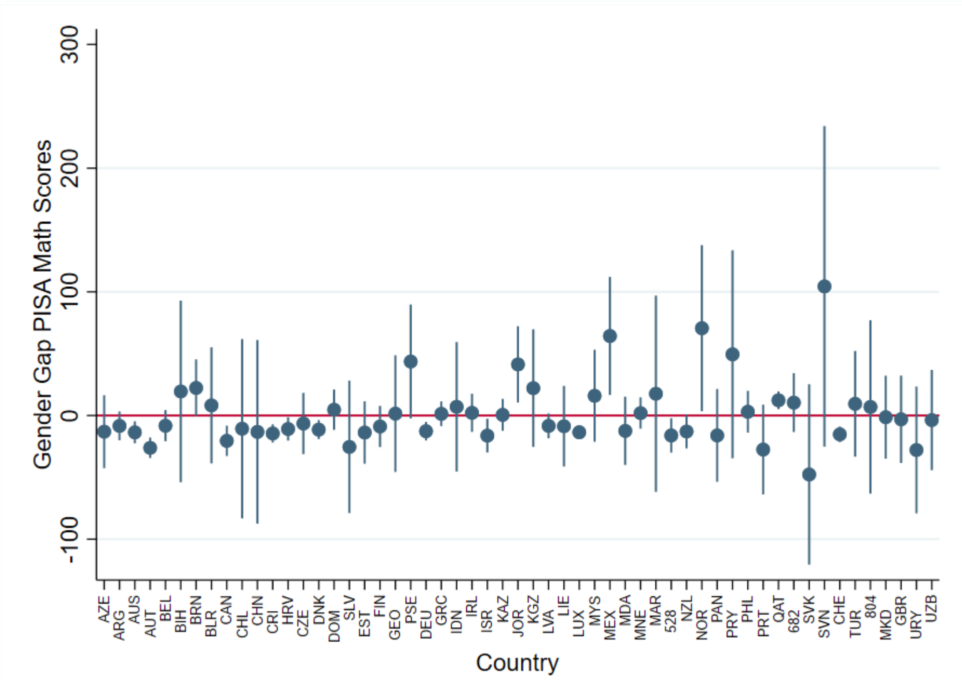
A. First-Generation Migrants





B. Second-Generation Migrants





Notes: The figures show the average migrant gender gap in PISA test scores for migrant students in each host country. Data from 2003, 2009, 2012, 2015, 2018 and 2022 PISA waves. Panel A. reports estimates for first-generation migrants, who migrated to the test-taking country. Panel B. reports estimates for second-generation migrants born in the test-taking country with foreign-born parents. Estimates above zero indicates a gender gap in favor of female students.

Table C1: Host Countries

	First Generation Migrant Students		Second Generation Migrant Students	
	(1) Obs.	(2) Origin Countries	(3) Obs.	(4) Origin Countries
Argentina (ARG)	649	Bolivia, Brazil, Paraguay, Uruguay	1,256	Bolivia, Brazil, Chile, Paraguay, Uruguay
Australia (AUS)	5,435	China, India, Italy, Korea, Malaysia, New Zealand, Philippines, South Africa, United Kingdom, USA, Vietnam	5,323	China, Germany, Greece, India, Italy, Korea, Lebanon, Malaysia, New Zealand, Philippines, South Africa, United Kingdom, USA, Vietnam
Austria (AUT)	1,826	Afghanistan, Bosnia-Herzegovina, Croatia, Germany, Hungary, North Macedonia, Poland, Romania, Russia, Serbia, Syria, Turkey	3,535	Afghanistan, Bosnia-Herzegovina, Croatia, Germany, Hungary, North Macedonia, Poland, Romania, Serbia, Turkey
Azerbaijan (AZE)	308	Georgia, Russia, Turkey	309	Georgia, Russia, Turkey
Belarus (BLR)	29	Russia	99	Russia, Ukraine
Belgium (BEL)	2,011	France, Germany, Netherlands, Turkey	1,398	France, Germany, Netherlands, Turkey
Bosnia-Herzegovina (BIH)	61	Croatia, Serbia	38	Croatia, Serbia
Brunei (BRN)	589	India, Indonesia, Malaysia, Philippines	473	India, Indonesia, Malaysia, Philippines
Cambodia (KHM)	16	Thailand	–	–
Canada (CAN)	4,138	Central African Rep., China, France, India, Iran, Korea, Pakistan, Philippines, Syria, United Arab Emirates, United Kingdom, USA	3,112	Central African Rep., China, France, India, Iran, Korea, Pakistan, Philippines, Syria, United Arab Emirates, United Kingdom, USA
Chile (CHL)	444	Colombia, Haiti, Peru, Venezuela	58	Peru
China (CHN)	199	Chile, North Macedonia, Philippines, Portugal	302	Chile, North Macedonia, Philippines, Portugal
Colombia (COL)	334	Venezuela	–	–
Costa Rica (CRI)	794	Colombia, Nicaragua	1,861	Colombia, Nicaragua, Panama
Croatia (HRV)	715	Bosnia-Herzegovina, Serbia	2,430	Bosnia-Herzegovina, Serbia
Czech Republic (CZE)	577	Russia, Slovakia, Ukraine, Vietnam	635	Russia, Slovakia, Ukraine, Vietnam
Denmark (DNK)	1,148	Afghanistan, Iceland, Iraq, Lebanon, Pakistan, Serbia, Somalia, Sweden, Syria, Turkey	4,171	Afghanistan, Iraq, Lebanon, Norway, Pakistan, Serbia, Somalia, Sweden, Syria, Turkey
Dominican Rep. (DOM)	332	Haiti, Spain, USA, Venezuela	333	Haiti, Spain, USA
El Salvador (SLV)	18	USA	16	Honduras
Estonia (EST)	16	Russia	268	Russia
Finland (FIN)	1,903	Afghanistan, China, Estonia, Iraq, Russia, Serbia, Somalia, Sweden, Turkey, Vietnam	1,551	Afghanistan, China, Estonia, Iraq, Russia, Serbia, Somalia, Sweden, Turkey, Vietnam
Georgia (GEO)	80	Armenia, Azerbaijan, Russia	86	Armenia, Azerbaijan, Russia
Germany (DEU)	715	Bosnia-Herzegovina, Croatia, Greece, Italy, North Macedonia, Poland, Russia, Serbia, Turkey	2,407	Bosnia-Herzegovina, Croatia, Greece, Italy, North Macedonia, Poland, Russia, Serbia, Turkey
Greece (GRC)	997	Albania, Bulgaria, Russia	1,781	Albania, Bulgaria, Romania, Russia
Indonesia (IDN)	–	–	47	Malaysia, Singapore
Ireland (IRL)	561	Lithuania, Poland, United Kingdom	588	Lithuania, Poland, United Kingdom
Israel (ISR)	1,097	Ethiopia, France, Russia, USA	2,008	Ethiopia, France, Russia, USA
Jamaica (JAM)	16	USA	–	–
Jordan (JOR)	1,711	Egypt, Iraq, Syria	250	Egypt, Iraq, Syria
Kazakhstan (KAZ)	1,017	China, Kyrgyzstan, Russia, Uzbekistan	1,386	China, Kyrgyzstan, Russia, Uzbekistan
Korea (KOR)	23	China	–	–
Kyrgyzstan (KGZ)	41	Tajikistan, Uzbekistan	77	Russia, Uzbekistan
Latvia (LVA)	177	Belarus, Russia, Ukraine	1,277	Belarus, Russia, Ukraine
Lichtenstein (LIE)	443	Austria, Switzerland	102	Austria, Germany, Switzerland, Turkey
Luxembourg (LUX)	3,412	Belgium, Cabo Verde, France, Germany, Italy, Portugal, Serbia, United Kingdom	5,260	Belgium, Cabo Verde, France, Germany, Italy, Portugal, Serbia
Malaysia (MYS)	–	–	126	Indonesia, Philippines
Mexico (MEX)	34	USA	38	USA
Moldova (MDA)	88	Romania, Russia, Ukraine	219	Romania, Russia, Ukraine
Montenegro (MNE)	860	Albania, Bosnia-Herzegovina, Croatia, Serbia	949	Albania, Bosnia-Herzegovina, Croatia, Serbia
Morocco (MAR)	67	France, Germany, Spain	27	France
Netherlands (NLD)	124	Germany, Iraq, Morocco, Suriname, Turkey	846	China, Iraq, Morocco, Serbia, Suriname, Turkey
New Zealand (NZL)	2,865	Australia, China, Fiji, India, Korea, Philippines, Samoa, South Africa, Tonga, United Kingdom	1,922	Australia, China, Fiji, Korea, Philippines, Samoa, South Africa, Tonga, United Kingdom
North Macedonia (MKD)	123	Albania, Bosnia-Herzegovina, Serbia	145	Albania, Bosnia-Herzegovina, Serbia
Norway (NOR)	66	Denmark, Sweden	78	Denmark, Sweden
Palestine (PSE)	76	Egypt, Iraq, Jordan, Syria	116	Egypt, Jordan, Syria
Panama (PAN)	348	China, Colombia, Dominican Republic, Nicaragua, Venezuela	135	China, Colombia
Paraguay (PRY)	56	Argentina, Brazil	68	Argentina, Brazil
Philippines (PHL)	91	China, Saudi Arabia, United Arab Emirates, USA	208	China, Saudi Arabia, United Arab Emirates, USA
Portugal (PRT)	756	Brazil, China	196	Brazil, China
Qatar (QAT)	5,646	Egypt, Jordan, Palestine, Yemen	3,920	Egypt, Jordan, Palestine, Yemen
Saudi Arabia (SAU)	455	Jordan, Kuwait, Qatar, Syria, USA, Yemen	61	Jordan, Kuwait, Syria, Yemen
Slovakia (SVK)	48	Czech rep., Hungary	61	Czech rep.
Slovenia (SVN)	36	Hungary, Italy	21	Italy
Turkey (TUR)	124	Syria	86	Bulgaria
Ukraine (UKR)	–	–	41	Russia
United Kingdom (GBR)	837	Germany, India, Ireland, Pakistan, Poland, Qatar	344	India, Ireland, Pakistan
Uruguay (URY)	107	Argentina, Brazil	82	Argentina, Brazil
Uzbekistan (UZB)	30	Kazakhstan	60	Kazakhstan, Tajikistan
Switzerland (CHE)	3,410	Albania, Austria, France, Germany, Italy, Portugal, Serbia,Spain, Turkey	7,155	Albania, Austria, France, Germany, Italy, Lichtenstein, Portugal, Serbia, Spain, Turkey

Notes: Number of observations in each host (PISA test-taking) country and related origin countries. Data from 2003, 2009, 2012, 2015, 2018 and 2022 PISA waves.

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