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Essays in Labor Economics, Parenthood, Immigration, and Education

Anton Sundberg

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Essays in Labor Economics

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The Department of Economics at Uppsala University has a long history. The first chair in Economics in the Nordic countries was instituted at Uppsala University in 1741.

The main focus of research at the department has varied over the years but has typically been oriented towards policy-relevant applied economics, including both theoretical and empirical studies. The currently most active areas of research can be grouped into six categories:

* Labour economics
* Public economics
* Macroeconomics
* Microeconometrics
* Environmental economics
* Housing and urban economics

Anton Sundberg

Essays in Labor Economics

Parenthood, Immigration, and Education

UPPSALA
UNIVERSITET

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Abstract
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Essay I: This paper examines the impact of parenthood on labor market outcomes for both men and women using population-wide annual income data from 1960 to 2021 in Sweden. First, I document the contemporary child penalties across several labor market outcomes. Second, I show that while the motherhood penalty in earnings declined significantly during the 1960s, 1970s, and early 1980s, the rate of decline slowed from the late 1980s onwards. Third, I identify a fatherhood penalty emerging since the 1980s, particularly pronounced among men in more gender-egalitarian households (proxied by the father's share of parental leave) and among fathers who have sons relative to daughters.

Essay II (with Olof Åslund and Arizo Karimi): We explore the effect of gender equality norms and shared institutional and economic contexts on the size of the motherhood penalty, studying child migrants and children of immigrants in Sweden. While there are results pointing to a moderate but statistically robust negative association between source country gender equality and the labor market impact of motherhood, the overall picture is more one of similarity across highly diverse groups. All groups of mothers exhibit qualitatively comparable labor market trajectories following first childbirth, but penalties are somewhat greater among those descending from the most gender-unequal societies.

Essay III (with Demid Getik and Anna Sjögren): We examine how exposure to recent migrants and asylum seekers affects the academic performance of incumbent students in Sweden between 2008 and 2022, a period characterized by large migration inflows. To identify the effect, we exploit variation in contemporaneous and cumulative exposure to recent migrants between siblings and across cohorts within schools. We find a small but statistically significant positive impact on native students' test scores from cumulative exposure to recent migrants. However, students with immigrant backgrounds do not experience similar benefits. A closer look at the more acute 2015 refugee crisis corroborates our main findings.

Essay IV (with Mounir Karadja): We study the economic effects of gaining access to the taxi labor market. Comparing individuals who pass the required written exams for a taxi license with those who have not yet done so, we find that immigrants increase their monthly earnings by nearly 50 percent between 1 and 3 years later and reduce their reliance on social insurance programs. Natives experience smaller gains of about 10 percent. Recently arrived immigrants reap the largest gains, suggesting that their outside options are limited, leading to a larger impact of taxi driving on their earnings.

Keywords: Parenthood, child penalties, gender earnings gap, immigrant integration, native-immigrant earnings gap, refugees, education, peers

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Till Hugo

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Stockholm, February 2024

Contents

Introduction

Over the past century, the labor markets of high-income countries have undergone significant transformations, notably marked by the increased female labor force participation and the heightened levels of immigration.[1] Along with the increased presence of women and immigrants in the labor market, there is also a persistent segmentation of economic activity based on gender and immigrant status. In fact, inequality by gender and immigrant status is evident in most facets of economic success.

Immigrants often occupy lower-paying jobs, are more likely to be overqualified for their jobs, and face substantial barriers in entering the labor market relative to natives (Carlsson and Rooth, 2007; OECD/EU, 2015; Baert, 2018). Women, traditionally perceived as primary caregivers, confront significant obstacles reconciling market work and family responsibilities (Goldin, 2014; Bertrand, 2020). Moreover, although women as a group are more educated than men across most high-income countries today, they are still more likely to make educational and occupational choices that consistently result in lower earnings (Kahn and Ginther, 2018; Bertrand, 2020).

Understanding the root causes of these inequalities in outcomes has been a fundamental part of research in economics for a long time. Early research focused on discrimination (Becker, 1957) and human capital accumulation (Mincer and Polachek, 1974). More recent additions to the potential mechanisms are identity (Akerlof and Kranton, 2000, 2010) and culture and preferences (Bisin and Verdier, 2001, 2011). All essays in this thesis build on these seminal contributions to the research on economic inequalities.

Why is it important to consider inequalities in economic outcomes? As argued by Bertrand (2018, 2020), there is one argument about fairness and another about efficiency. The argument that individuals should have the same opportunities regardless of gender and immigrant status is likely an argument that is appealing to many. Perhaps this argument may suffice to justify why this is a relevant and important topic to study. But, within a stricter economic framework, one can also argue for efficiency. Women account for roughly half of the population, and many countries today have large and increasing immigrant populations. If there are labor market barriers that are based solely on gender and immigrant identity—and not on relevant abilities or aptitude—a

[1] These trends have been consistent across most OECD countries, although magnitudes differ. For female labor force participation across countries, see Olivetti (2014) and Olivetti and Petrongolo (2016), and for the share of immigrants in the population, see UN (2020).

large number of individuals will not be efficiently allocated in the labor market. This potential inefficiency will, in turn, lower the overall economic well-being of a society.[2] Hence, both in terms of a general notion of fairness and in terms of what is beneficial for society, uncovering inequalities and their underlying mechanisms is of first-order relevance in economic research.

This thesis consists of four self-contained empirical essays that encompass various questions within labor economics but have a common denominator: they are all related to inequalities in opportunities or outcomes. Each essay addresses this issue from a different perspective, and the thesis aims to improve our understanding of the mechanisms or the importance of the context for each of them. The first two essays focus on the gender gap in labor market outcomes from parenthood. The latter two focus on immigrant peers in schools and immigrant labor market integration, respectively. A short introduction and summary of each essay follow.

Essays I and II

A few months ago, Claudia Goldin received the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel for "having advanced our understanding of women's labor market outcomes" (The Royal Swedish Academy of Sciences, 2023). The press release stated:

> Historically, much of the gender gap in earnings could be explained by differences in education and occupational choices. However, Goldin has shown that the bulk of this earnings difference is now between men and women in the same occupation, and that it largely arises with the birth of the first child.
>
> *—The Royal Swedish Academy of Sciences, 9 October, 2023.*

The first two essays in my thesis build on Goldin's work and focus on the differential impact of parenthood on the labor market outcomes for women relative to men in Sweden.

Essay I: The Child Penalty in Sweden: Evidence, Trends, and Child Gender

Women have now entered the labor market at almost the same rate as men and have even surpassed men in educational attainment in many high-income countries. Moreover, the gender gaps in earnings, hours of work, wages, and occupational rank have all narrowed considerably (Goldin, 2006). Goldin (2014, p. 1) refers to this development as "among the grandest advances in society and the economy in the last century."

[2] See Hsieh et al. (2019) for theoretical and empirical evidence on the economic importance of the misallocation of talent based on gender and race.

This development notwithstanding, it is well documented that gender gaps in labor market outcomes are still present and appear to be persistent (Olivetti and Petrongolo, 2016; World Economic Forum, 2023). The main driver of this persistence is the female-male differentials in the impact of parenthood (Bertrand, Goldin, and Katz, 2010; Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019; Bertrand, 2020; Cortés and Pan, 2023; Kleven, 2023). Parenthood accounted for about 80 percent of the earnings gap in Denmark in 2013 (Kleven, Landais, and Søgaard, 2019) and nearly 70 percent of the pay gap in the US in the 2010s (Cortés and Pan, 2023). The substantial negative impact of children on women's earnings is often referred to as the "motherhood penalty." It contrasts with the non-existent or even positive impact of children on men's earnings (the "fatherhood premium").[3]

In my first essay, I focus on the gender inequality that stems from family formation using a panel of 62 years of annual income data in Sweden. First, I document contemporary child penalties for men and women across several labor market outcomes. Second, I show that the motherhood penalty decreased significantly during the 1960s, 1970s, and early 1980s but has been only modestly reduced since the early 1980s. Third, I show that since the late 1980s, there has been a fatherhood penalty, and this penalty is higher in more gender-egalitarian households (proxied by the father's use of paternity leave) and among men having sons relative to daughters.

The substantial reduction in the motherhood penalty coincides with a significant entry of women into the labor market (Olivetti, 2014; Molinder, 2022) and major family policy reforms in Sweden in the early 1970s. These reforms include individual income taxation, publicly funded universal child care, and the introduction of job-protected and gender-neutral parental leave. The reduction in the motherhood penalties shown in this essay, therefore, adds to the overall picture of a greater transformation in terms of women's labor market outcomes in Sweden in the 1960s to 1980s. It also corroborates previous research showing that the impact of children is crucial in understanding gender gaps in labor market outcomes and that a lot of the progress in closing the gender gaps in labor market outcomes has slowed in the recent decades (Blau and Kahn, 2006; Goldin, 2006; Blau and Kahn, 2017; Bertrand, 2020; Kleven, 2023).

[3]Although earlier papers explicitly referred to a "motherhood *wage* penalty," e.g., Budig and England (2001) and Anderson, Binder, and Krause (2002), the terminology is now so well established that the labor market outcome (earnings, wages, or, employment) is implicitly assumed and therefore omitted. Nevertheless, the term "motherhood penalty" should be viewed as very specific and implicitly refers to the negative impact of parenthood on a labor market outcome. Although the terms "penalty" and "premium" have normative connotations, the terms encompass all underlying causes, including, for example, discrimination, hours worked, and choice of workplace. It has also become more common to use the term "child penalty," even though the term also includes the impact of additional children.

Although the impact of parenthood on the labor market outcomes of men is small compared to women, I show a fatherhood penalty emerging in the 1980s. This penalty in earnings is primarily driven by reduced labor supply in the first years after the child is born (due to the use of paternity leave) but also persists in the longer horizon. The penalty in the long run is driven by reduced hours worked and lower wages but is relatively small in magnitude. While the long-run motherhood penalty is largely unaffected by the length of parental leave and the distribution of parental leave within the household, there is a linear increase in the fatherhood penalty for men using more paternity leave. These results indicate that differential gender norms across households matter for variations in the size of the fatherhood penalty.

The fatherhood penalty is also higher among men having sons relative to daughters. This finding contrasts studies from the US and Germany (Lundberg and Rose, 2002; Choi, Joesch, and Lundberg, 2008; Dahl and Moretti, 2008) where fathers of sons have higher earnings relative to fathers of daughters. One potential explanation for this discrepancy is that gender norms in Sweden are different from those in the US and Germany. While the higher earnings of men with sons are often discussed in the literature in terms of a role model effect (Raley and Bianchi, 2006), this effect might only be present in societies with a more pronounced breadwinner norm rather than in environments characterized by more gender-egalitarian norms, such as Sweden.

Essay II: Origin, Norms, and the Motherhood Penalty (with Olof Åslund and Arizo Karimi)

Although the existence of motherhood penalties is well established, the underlying causes still need to be understood. The long-run child penalty is nearly identical for women birthing and adopting children, challenging the notion that biological factors (e.g., pregnancy, delivery, breastfeeding) explain the gender earnings gap post-parenthood (Kleven, Landais, and Søgaard, 2021; Rosenbaum, 2021; Andresen and Nix, 2022).

Family policies have also shown limited importance in explaining the size of long-run child penalties. Expansions of parental leave in Germany (Schönberg and Ludsteck, 2014) and Austria (Lalive and Zweimüller, 2009; Lalive et al., 2014; Kleven et al., 2022) had very modest impacts on long-run earnings for women. Earlier research from Sweden, the US, and the UK, also showed that a higher uptake of parental leave did not lead to higher long-run motherhood penalties (Waldfogel, 1998; Albrecht et al., 1999). The same results hold for childcare subsidies in Austria (Kleven et al., 2022).

Recent studies also question the importance of specialization within the household (Becker, 1981) and male-female differentials in human capital accumulation (Mincer and Polachek, 1974) in contributing to child penalties. First, in the Nordic countries, the differential impact of children on men and women is much larger in opposite-gender couples compared to same-gender couples even when couple characteristics such as education and earnings are

held constant (Andresen and Nix, 2022; Vleuten, Evertsson, and Moberg, 2023). Second, in the US, the motherhood penalty is even higher in couples where women outearn their husbands (Almond, Cheng, and Machado, 2023).

The leading candidate for understanding the impact of parenthood on earnings is the male breadwinner norm; women are expected to take the primary responsibility for child rearing (Boelmann, Raute, and Schönberg, 2021; Kleven, Landais, and Søgaard, 2021; Andresen and Nix, 2022; Kleven, 2023; Vleuten, Evertsson, and Moberg, 2023). Cross-country comparisons also suggest a positive relationship between child penalties and elicited traditional gender norms (Kleven et al., 2019), offering additional support for this explanatory factor.

In my second essay, we add to the literature on child penalties by focusing on the role of culture and gender norms in determining the size of the motherhood penalty in earnings. We study whether people living in the same economic and institutional context (Sweden) but with different cultural backgrounds are affected differently by entering parenthood. We focus on child migrants and children of immigrants to hold institutional and overall societal exposure during adolescence and early adulthood constant across groups. Gender norms across groups are proxied by a parental country of origin ranking according to the World Economic Forum's Global Gender Gap Index (GGI). This study, therefore, uses the "epidemiological approach" to study the importance of culture for economic outcomes (see Fernández, 2011, for a review of the method and its applications). Hence, we also add to an expanding literature that explores the importance that cultural variables have in determining economic choices and outcomes (Guiso, Sapienza, and Zingales, 2006; Alesina and Giuliano, 2015; Giuliano, 2021).

We find that gender norms among child migrants and children of immigrants show pre-parental similarities not only with first-generation adult migrants sharing their geographic origin but also with the gender equality indicators seen among the populations of these countries. We also find that gender norms influence the size of the motherhood penalty, but mainly that mothers from different backgrounds show strong similarities in their earnings trajectories after entering parenthood. The findings highlight that while parental gender norms matter to some extent, motherhood penalties are strikingly similar across groups with very different cultural backgrounds.

Essays III and IV

My third and fourth essays focus on different dimensions of immigration and integration. Globally, around 300 million people (3.5 percent of the global population) live in a country other than their country of birth. Migrants tend to move to relatively richer countries, and more than 1 in 10 residents in the EU and the member countries of the OECD are foreign-born. The number for

Sweden is more than 1 in 5, which is the fifth highest in the EU (Frattini and Bertino, 2023).

Moreover, as of July 2023, more than 6.2 million refugees had fled from Ukraine after Russia's full-scale invasion in February 2022 (UNHCR, 2023a), and the UNHCR estimates that there were around 36 million refugees globally at the end of 2022 (UNHCR, 2023b). Recent developments in terms of climate change, population growth, and military conflicts are indicating that the number of migrants and refugees will be high also in the future. In a globally representative survey in 2021, 16 percent of adults answered that they would like to permanently migrate to another country if they could (Gallup, 2023).

In tandem with the increasing number of migrants and asylum seekers, the political question of immigration and integration has risen to prominence in most high-income countries in the last decades. Figure 1 shows how the question has gained increasing significance following surges in asylum seekers in Sweden. During the 2015 European refugee crisis, immigration was declared the main challenge facing the EU in all member countries except Portugal (European Commission, 2015).

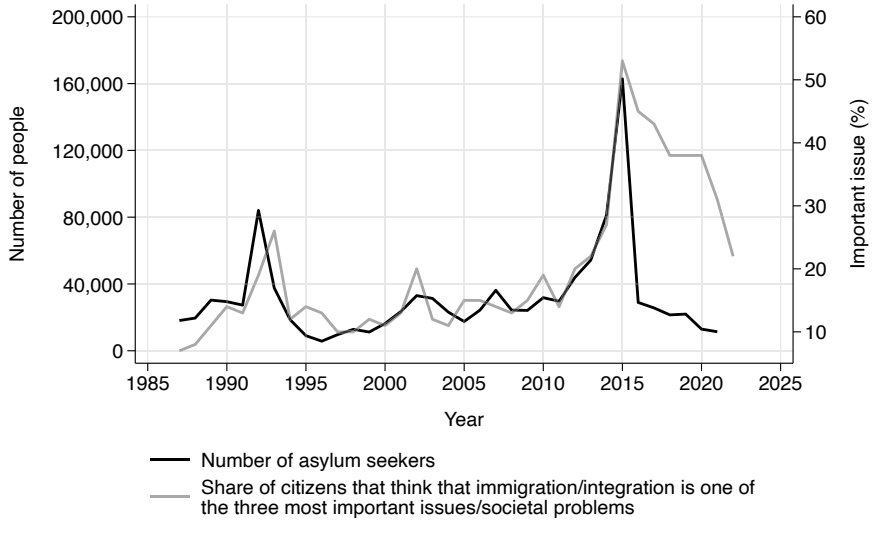

Figure 1. Changes in the number of asylum seekers and the perceived importance of the political question of immigration/integration over time in Sweden. The right axis shows the percentage of people who included immigration/integration as one of the three most important issues/societal problems. *Sources*: Data from the SOM Institute (2023) and the Swedish Migration Agency (2024).

Essay III: Recent Migrant Peers and the School Performance of Incumbent Students (with Demid Getik and Anna Sjögren)

From a host country perspective, immigration raises concerns regarding its potential to adversely affect the labor market opportunities for natives and its potential to burden public institutions and the functioning of the welfare state (Rica, Glitz, and Ortega, 2015). Although these questions are empirically hard to study, the interest for them in economics research has risen with the increasing number of migrants in the last decades. Pioneering work by Chiswick (1978) and Borjas (1985) focused on the labor market outcomes of immigrants in the host country (the US), and their work has been followed by extensive work on the fiscal impact of immigration and the impact of immigration on the labor market.[4]

Compared to the extensive literature on the impact of immigration on the labor market outcomes of natives, less research has been devoted to the effects of immigration on the educational outcomes (Figlio et al., 2023). This notwithstanding, children are largely over-represented among refugees and correspond to more than 41 percent of the refugees in the world (UNHCR, 2023b). The influx of the 2015 refugee crisis in Europe led to political debates in many European countries about the strain migration put on school systems. This debate spurred again following Russia's full-scale invasion of Ukraine in 2022.

The importance of school peers on educational performance is a well-known fact, going back to the Equality of Educational Opportunity Study (known as the "Coleman Report") studying school segregation in the 1960s in the US (Coleman, 1966). More recent studies have corroborated these early findings on the importance of peers (Hoxby, 2000; Sacerdote, 2011) as well as the importance of students' relative performance in the classroom (see Delaney and Devereux, 2022, for a review). A sudden surge in migrants (as in 2015 in Europe) might also lead to a sudden reorientation of teaching activity, cause classroom disruptions, and increase competition for resources in the short run (Fix and Zimmermann, 1993; Lazear, 2001; Card, 2009). Changes to the student composition due to migration flows may additionally affect school choices and cause families to change neighborhoods or schools (often referred to as "native flight"), which further alters the student composition of receiving schools (Grodzins, 1957; Clotfelter, 1976, 2001; Betts and Fairlie, 2003).

In my third essay, we ask: How are native students affected by exposure to newly arrived immigrants in terms of their educational outcomes? The setting is the influx of migrant children into Swedish schools over the last 15 years. Sweden had the highest per capita refugee admission in the OECD during the peak of the 2015 refugee crisis, amounting to 163,000 asylum seekers, corresponding to 1.6 percent of the population (OECD, 2017). In the

[4]See Chiswick and Miller (2015) for a more recent review on the overall economic consequences of immigration.

academic year 2016/17, 12 percent of the students in Swedish compulsory schools were foreign-born, and two-thirds of the foreign-born students were recent migrants.[5]

We use administrative student registers with data on school assignments and test scores for all Swedish compulsory school students (including asylum seekers) to study the impact of exposure to recent migrants on native students' academic performance. The fact that we have information on the student composition at both school and classroom levels for all grades in compulsory school means that we can follow individuals over time and study exposure throughout the compulsory school. Moreover, this data is merged with registers on families, allowing for sibling comparisons in exposure and outcomes. Thus, the data allows us to include changes in student composition due to potential native flight.

Our results suggest that the negative association between migration and the school performance of native students stems from a significant negative sorting of migrants and native students to schools that have a high inflow of recent migrants. Once we account for this sorting, we find that both contemporaneous and cumulative exposure have small positive effects on native students' performance. The overall conclusion is, therefore, that exposure to recent migrants had a modest, albeit significantly positive, impact on the educational performance of native students. There are, however, signs of a small negative effect on students with an immigrant background. An event study analysis of the more acute exposure during the 2015 refugee crisis corroborates our main findings. It shows that while classrooms in exposed schools initially became more crowded, schools reacted to the migrant influx by reducing class sizes. This finding points to an important role for resources.

Essay IV: The Labor Market Impact of a Taxi Driver's License (with Mounir Karadja)

In most countries in the EU, foreign-born are less likely to be employed compared to natives (Frattini and Bertino, 2023). Moreover, immigrants with a non-Western immigrant background have worse labor market outcomes than natives and their Western immigrant counterparts (Eriksson, 2010; Aldén and Hammarstedt, 2014; Brell, Dustmann, and Preston, 2020). My fourth essay focuses on a labor market with a large over-representation of non-Western immigrants (roughly half of all taxi drivers in Sweden are foreign-born). This over-representation raises the question of what role this and other occupations with similar characteristics—service sector with low formal qualification requirements—play in the native-immigrant employment and earnings gaps.

To the best of our knowledge, this project is the first to estimate the effects of a taxi driver's license on labor market outcomes. Examining the impact

[5]Recent migrant is defined as a foreign-born student being granted a residence permit within the last four years or asylum-seeking students in the asylum process who have not yet received a residence permit.

of access to certain occupations with low formal requirements and barriers to entry is complicated because access to these occupations is rarely formally restricted. However, being a taxi driver requires a specific driver's license, meaning the individual must pass several exams. We use these previously unused data on all written exam results for the taxi driver's license in Sweden between 2004 and 2017, matched with administrative data on individual labor market outcomes.

We find that gaining access to the taxi labor market positively affects both natives and immigrants. However, the taxi labor market has a more significant positive impact on immigrants relative to natives, which is evident both in levels and relative terms. Immigrants are also more likely to enter the taxi labor market from unemployment, while natives are more likely to have been employed. Moreover, more highly educated individuals take up taxi driving among immigrants than natives. Highly educated immigrants have higher post-taxi earnings than lower-educated immigrants, while the opposite is true for natives. We interpret this as an indication that outside options are generally lower for highly educated immigrants, compared to highly educated natives, in line with studies finding foreign-acquired human capital having lower economic returns (Friedberg, 2000).

We also find that immigrants who pass the taxi exams show no decline in taxi driving as their main occupation throughout our sample period. Thus, we find no indication that immigrants use taxi driving as a stepping stone to other occupations within our time frame. Natives, by contrast, display a peak in taxi driving as their main occupation in the first 12 months after passing exams, after which it decreases gradually. Taxi driving for immigrants appears to represent a more stable and long-standing shift in labor market status, while it is a more temporary occupation for natives.

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Essay I. The Child Penalty in Sweden: Evidence, Trends, and Child Gender

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1 Introduction

In most industrialized countries, women have entered the labor market at almost the same rate as men and have even surpassed men in educational attainment, often referred to as a gender revolution (Goldin, 2006; England, 2010). However, there are still persistent gender gaps in wages, hours worked, and the representation of women in manager positions (Olivetti and Petrongolo, 2016). Moreover, women's earnings drop sharply relative to men's after entering parenthood (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019).[1] The differential impact of parenthood on women relative to men is the main driver of the persistent gender gap in labor market outcomes across industrialized countries (Kleven, Landais, and Søgaard, 2019; Bertrand, 2020; Cortés and Pan, 2023; Kleven, 2023).

This study focuses on the impact of parenthood on labor market outcomes in Sweden. Sweden provides an interesting setting for a study on child penalties since it is often seen as a forerunner in gender-egalitarian norms and the main proponent of the dual-earner/dual-carer model (Ferrarini and Duvander, 2010). Many European countries have followed Sweden's lead, with the EU increasingly emphasizing the role of fathers as family carers.[2] Sweden's historical and contemporary setting therefore provides an important context to study, particularly for countries seeking to implement comparable policies aimed at challenging the male breadwinner norm and promoting gender equality in the labor market.

In this essay, I first document the contemporary child penalties for both men and women across a number of labor market outcomes in Sweden. Second, I show that while the motherhood penalty in earnings declined significantly during the 1960s, 1970s, and early 1980s, the rate of decline slowed substantially from the late 1980s onwards. Third, I identify a fatherhood penalty emerging since the 1980s, particularly pronounced among men in more gender-egalitarian households (proxied by the father's use of paternity leave) and among fathers of sons compared to daughters.

Over the 10 years following first childbirth, women in Sweden experience a child penalty in earnings of 37 percent. The penalty 10 years after the first childbirth is 25 percent. Compared to other high-income countries, this is a relatively low long-run motherhood penalty (Kleven et al., 2019). The 10-year motherhood penalty in employment is 6 percent, which is substantially lower than most other OECD countries (Kleven, Landais, and Leite-Mariante, 2023) and is reflecting the fact that few women leave the labor market altogether after family formation in Sweden. The 10-year penalty in hours worked and wages (both conditional on employment) are 14 percent, respectively. Therefore, the

[1] See also Korenman and Neumark (1992), Waldfogel (1997), Budig and England (2001), and Anderson, Binder, and Krause (2003) for early evidence on motherhood penalties in wages.

[2] See e.g., EU Directive 2019/1158 on work-life balance for parents and carers, which also mandates that member states must offer a minimum of 10 days of paternity leave.

long-run penalty in earnings primarily stems from lower wages and an increase in part-time employment.

The motherhood penalty in earnings over the 10 years following childbirth decreased from 63 percent for first childbirths in the early 1960s to 43 percent for childbirths in the early 1980s and to 34 percent for childbirths in the early 2010s. The significant reduction in child penalties between the 1960s and 1980s coincides with a substantial entry of women into the labor force and Sweden's implementation of several major family policy reforms to enable and incentivize women with children to work. The slowdown in the reduction in the size of the child penalty is aligned with the broader narrative that gender equality in economic outcomes made significant progress in the 20th century but has slowed in recent decades (Blau and Kahn, 2006; England, 2010; Blau and Kahn, 2017; Kleven, 2023). It also corresponds with the trend observed in the few other countries that have studied the evolution of motherhood penalties. Generally, these countries experienced significant reductions in the motherhood penalties up to the 1990s, but progress slowed after that.[3]

Although often overlooked in the literature, this essay also focuses on heterogeneity in the labor market outcomes of men following parenthood. Despite substantial changes for women in the labor market in the 20th century (Goldin, 2006), the impact of children on men's labor market outcomes has been non-existent (or even positive) across countries (Kleven, Landais, and Leite-Mariante, 2023). However, I show that within the comparatively gender-egalitarian Swedish context, there is a fatherhood penalty in earnings for men having children. The fatherhood penalty has increased from non-existent to a child penalty in earnings of 7 percent following the 10 years after first childbirth and a 10-year penalty of 4 percent. The short-run penalty is primarily driven by reduced labor supply in the first years after the child is born due to paternity leave. The long-run penalty is driven by lower wages and, to some extent, fewer hours worked.

Recent literature suggests that gender norms are important for the size of the motherhood penalty (Boelmann, Raute, and Schönberg, 2021; Kleven, Landais, and Søgaard, 2021; Andresen and Nix, 2022b; Kleven, 2023). In this essay, I also show the importance of gender norms for the size of the fatherhood penalty. I proxy gender norms by the father's share of parental leave. Previous research has shown a positive relationship between the use of paternity leave and gender-egalitarian norms within the household (Duvander, 2014; Aldén, Boschini, and Tallås Ahlzen, 2023).[4] As the number of days of paternity leave has increased substantially during the studied period, I use an individual's placement in the distribution of father's share of parental leave in

[3]For the evolution of motherhood penalties from the 1970s, see Kleven (2023) for the US, Andresen and Nix (2022a) for Norway, and Huttunen and Troccoli (2023) for Finland.

[4]Father's share of parental leave is also an indicator of gender equality that is frequently used by government authorities in Sweden (Swedish Social Insurance Agency, 2018; Haandrikman, Webster, and Duvander, 2021).

a given year of childbirth as a proxy for gender norms. This implies that the institutional framework remains constant—focusing on Sweden during a specific year of childbirth—while varying gender norms across individual households.

Men taking a higher share of the parental leave within the household also have a higher long-run fatherhood penalty. The comparison of men depending on their use of paternity leave highlights a large variation in the observed long-run fatherhood penalties, ranging from non-existent for men taking no leave to a penalty of 10 percent for men in the highest decile. In contrast, the long-run motherhood penalty is essentially unaffected by the use of parental leave. This result suggests that gender norms within the household play a significant role in shaping the fatherhood penalty while having less influence on the motherhood penalty.

The differential impact of parental leave on earnings for men and women indicates that leave-taking does not solely go through the loss of experience and human capital (Mincer and Polachek, 1974; Albrecht et al., 1999). The suggested explanation is instead that the differential impact of leave-taking goes through a more gender-egalitarian division of household chores. In other words, men in more gender-egalitarian households make career decisions that translate better to family care relative to market work, much in the same way that women do (Bertrand, Goldin, and Katz, 2010; Bertrand, 2020). This explanation is supported by previous research showing that men using more paternity leave also tend to have a higher engagement in child rearing as the child gets older (Haas and Hwang, 2008; Almqvist and Duvander, 2014). An alternative explanation is that higher use of paternity leave signals less career ambition to the employers for men but not for women and that men using more paternity leave are "punished" for doing so (Albrecht et al., 1999).

Research in economics has also identified variations in labor market outcomes based on the gender of children. In both the US and Germany, studies indicate that men tend to experience an increase in their hourly wage rates and annual hours worked when they have a son compared to having a daughter (Lundberg and Rose, 2002; Choi, Joesch, and Lundberg, 2008; Pollmann-Schult, 2017). Additionally, studies have revealed differences in parental behavior influenced by the child's gender, including at what age they engage their children in activities such as reading, singing songs, and teaching letters and words (Lundberg, McLanahan, and Rose, 2007; Bertrand and Pan, 2013; Baker and Milligan, 2016). Studies have also shown that fathers spend more time interacting with their children when they have sons. In contrast, mothers' earnings and time allocation remain relatively unaffected by the child's gender (Lundberg, 2005; Raley and Bianchi, 2006; Mammen, 2011).

In this paper, I show that the motherhood penalty is unaffected by the child's gender, but the fatherhood penalty is relatively larger for men with a first-born son than a first-born daughter, and the gender of the first child accounts for 7 percent of the long-run fatherhood penalty. This result, therefore, contrasts

previous findings from the US and Germany (Lundberg and Rose, 2002; Choi, Joesch, and Lundberg, 2008; Dahl and Moretti, 2008). While the higher earnings of men with sons are often discussed in the literature in terms of a role model effect (Raley and Bianchi, 2006), this effect may only be present in the context of stronger breadwinner norms, but not in a more gender-egalitarian environment such as Sweden.

The rest of the paper is organized as follows. In Section 2, I present the institutional setting for the study and discuss the relevant policies introduced in Sweden over the last decades. Section 3 presents the empirical strategy using three empirical specifications and the necessary identifying assumptions. In Section 4, I present the data and the sample restrictions. In Section 5, I show the child penalties across different labor market outcomes and regions in Sweden, the development of child penalties in earnings over time, and variation in child penalties across households depending on gender norms and child gender. The paper ends with concluding remarks in Section 6.

2 Background

Sweden consistently ranks high in gender equality indices with gender egalitarian views on women in the labor force and the highest maternal employment rate in the OECD (OECD, 2016).[5] Individual income taxation, expansion of publicly subsidized childcare, and extensive parental leave are all policies implemented in Sweden to increase the incentives for women with children to work.

In 1971, Sweden abandoned joint taxation for households to increase the labor market participation rate of married women. The impact was the strongest for women with children married to high-income earners, where the marginal gain of female labor force participation increased the most (Selin, 2014). Three years later, Sweden was the first country in the world to introduce an earnings-based, job-protected, and gender-neutral parental leave scheme. This reform meant that men and women had the right to economic compensation for being at home with their children. Men and women were allocated half of government-paid days of parental leave each but could transfer these days without any restrictions within the household. In 1974, the paid leave was six

[5]Examples of gender equality rankings are the World Economic Forum's Global Gender Gap Index (GGI) and the Gender Inequality Index (GII) in the Human Development Report by the United Nations Development Program (UNDP). Sweden ranks 4th behind Iceland, Finland, and Norway (GGI, 2020) and 6th with Belgium behind Switzerland, Norway, Finland, Netherlands, and Denmark (GII, 2019). During the period studied in the main analysis of this paper (1990–2021), Sweden's female labor force participation rate has ranged between 86 percent and 90 percent of the male labor force participation rate. Swedes were the most likely to disagree with the statement that when jobs are scarce, men should have more right to a job than women (Inglehart et al., 2014). Sweden also has the third lowest gender gap in time spent on unpaid/care work relative to paid work in the OECD (OECD, 2016).

months, but it incrementally increased up to 15 months in 1989. The parental leave system was also reformed with "earmarked" parental leave for each parent. In 1995, one month of the total 15 months of paid leave could not be transferred to the other parent. In 2002, it was extended to two months (with an increase in total paid leave from 15 to 16 months), and in 2016 to three months. In addition to the 480 days of parental leave following a child's birth, parents are entitled to government-paid temporary parental leave to care for sick children. There are no restrictions on the division of temporary parental leave between the parents.

Together with short or non-existent parental leave, expensive childcare is often discussed as one of the main obstacles to women's participation in the labor market (Olivetti and Petrongolo, 2017). However, Sweden was also the first country to introduce public and heavily subsidized universal childcare at a very low cost to families (Lundin, Mörk, and Öckert, 2008). In 2019, 89 percent of two-year-olds and 94 percent of children aged three to five attended preschool (Statistics Sweden, 2019).

3 Empirical framework

I use two empirical specifications to estimate child penalties. In my first specification, I follow Kleven, Landais, and Søgaard (2019) and run the following regression:

$$Y_{it} = \boldsymbol{\beta}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it}, \tag{1a}$$

where Y_{it} is the labor market outcome of interest for individual i in event time t. In all empirical specifications, boldface is used to denote vectors. $\boldsymbol{D}$ refers to vectors of a full set of dummies for event time, age, and calendar year. Individuals are included from 5 years before first childbirth to 10 years after, meaning that the event time dummies are indexed from -5 to 10, where $t = 0$ is the year of first childbirth. Event time $t = -1$ is omitted to provide the baseline. Therefore, the event time coefficient $\beta_t \in \boldsymbol{\beta}$ is the impact of children relative to one year before the first childbirth. I do not restrict the number of children, so this estimation should be viewed as the impact of family formation rather than the impact of a child. I also follow Kleven, Landais, and Søgaard (2019) and convert the coefficients to percentage effects using the following equation:

$$P_{it} \equiv \frac{\tilde{\beta}_t}{\boldsymbol{E}[\tilde{Y}_{it} \mid t]}, \tag{1b}$$

where $\tilde{Y}_{it}$ is the predicted counterfactual outcome of having children.

All individuals in the regressions have a child at some point in time. As follows, identification comes from comparing individuals born in the same

calendar year, who have their first child at different ages. I am interested in the heterogeneity in the impact of children across childbirth cohorts, the number of children, location choices and characteristics such as parental leave take-up. Therefore, I run Equation 1a separately for each group, which allows for group-specific age and calendar year effects. In the main analysis, I exclude observations where the individuals are students, so that the estimated changes in labor market outcomes are not an artefact of labor market entry.[6]

The key assumption in the event study method is that the decision to have children is not determined by the labor market outcome studied. This assumption is strong for the long-run outcomes since one cannot rely on the smoothness assumption when extrapolating earnings profiles. If unobserved earnings potential is correlated with the timing of the first childbirth, the estimated long-run child penalties will be biased. In Norway, Bensnes, Huitfeldt, and Leuven (2023) use IVF treatments to show that women tend to time their fertility as their earnings profile flattens, which leads to an overestimation of the motherhood penalty using the standard framework. This bias is due to an overestimation of the counterfactual earnings for the women who had children at an early age. However, Melentyeva and Riedel (2023) shows that bias can also arise if there is heterogeneity in the effects of motherhood on earnings by maternal age at first childbirth. In Germany, this heterogeneity leads to an underestimation of the long-run child penalty in the standard framework. Given the potential challenges associated with the conventional event study framework and its applicability to long-run outcomes, I mainly focus on the average outcome in the 10 years following the first childbirth.

The event study method handles decisions taken close to entering parenthood well. For example, if individuals change to a lower-paying job with more work flexibility in a period close to having their first child, this would be detectable as a pre-trend and violate the identifying assumption. Thus, the method is well suited to estimate child penalties related to decisions or outcomes that occur close to entry into parenthood. However, the method cannot incorporate the effect of anticipating becoming a parent on early career investments. For example, individuals could self-select into specific occupations that enable a job with better family-oriented work flexibility but lower earnings potential in anticipation of parenthood (Kahn and Ginther, 2018; Bertrand, 2020). This self-selection is also a potential channel for child penalties that are not accounted for in the event study method, given that it only includes individuals who have children at some point and normalizes outcomes to a pre-child level. From this perspective, the estimated child penalties should be seen as a lower bound on the actual child penalties (Kleven, Landais, and Søgaard, 2019).

[6] See Figure C2 for a comparison when including and excluding students from the population.

In the second empirical specification, I modify Equation 1a to estimate the effect of having sons relative to daughters on labor market outcomes to study the impact of child gender:

$$\begin{aligned} Y_{it} = \boldsymbol{\phi}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\beta}' \left(\boldsymbol{D}_{it}^{\text{Event}} \times D_i^{\text{Son}} \right) \\ + \boldsymbol{\delta}' \boldsymbol{D}_{it}^{\text{Children}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it} \end{aligned} \tag{2}$$

where I include an interaction term with the event time dummies and a dummy for having sons along with a full set of dummies for the number of children. Conditioning on the number of children means that the impact of sons is distinguished from the effect of additional children. The coefficient of interest $\beta_t \in \boldsymbol{\beta}$ is the effect of having sons relative to having daughters. In this model, I use the randomness of the gender of the first child to estimate the impact of the child's gender on earnings. Therefore, this model relies on a weaker assumption than Equation 1a.[7]

I run Equation 2 in two versions. First, I only focus on the gender of the first-born child. This specification has the benefit of relying on the arguably weak assumption that the gender of the first child is random. The downside is that instead of estimating the effect of having sons relative to daughters, it estimates the impact of having a first-born son relative to a first-born daughter. It should, therefore, be seen as a lower bound as individuals in the control group (individuals with a first-born daughter) are potentially treated later in the post-period (higher order child being a son).

Second, to estimate the impact of having sons relative to daughters, I run the model only for time periods in which the individuals have no children (pre-period) or one or more children of the same gender (post-period). This restriction means that individuals are censored if they have children of opposite genders. The benefit of this restriction is that it estimates the impact of sons relative to daughters, assuming the identifying assumption holds. The downside is that the model relies on a stronger assumption: individuals cannot be following a fertility-stopping rule based on the gender of the children.[8]

In addition, I run Equation 2, including indicator variables for whether the individual is living (i) with their partner, (ii) in a single household with children, or (iii) in a single household without children to control for relationship

[7] Table A4 shows the statistics for the main sample of analysis, one year before the birth of the first child, divided by the gender of the parent and the gender of the first child. All the descriptive statistics are very similar with respect to the gender of the child, which supports the identifying assumption that the gender of the first-born child is random.

[8] As seen in Table B3, if a man has a first-born son, the likelihood of having at least two children increases by 0.29% (0.57% for women). This result is aligned with previous research in the Swedish context (Andersson et al., 2006) and more recent data on the US (Blau et al., 2020) but contrasts earlier research from the US (Dahl and Moretti, 2008). Given that there is a tendency for both men and women to have more children when their first child is a son, a fertility-stopping rule might be in place. Although the effect sizes are small, interpretations of the estimates should be made with this in mind.

status. The reason for these control variables is to rule out that the impact of child gender on earnings goes through the relationship status of the parents.

4 Data

I use longitudinal population-wide administrative data on individuals in Sweden. The data links multiple registers through unique identifiers and covers all individuals residing in Sweden between the ages of 16 and 74. It includes annual information on earnings, social benefits, education, and place of living, combined with multigenerational data on parent-child relationships. Relationship status between individuals is identified through marriage or having a joint child.

I restrict the main analysis to a fully balanced panel of individuals, from five years before to ten years after the first child's birth. I have population-wide data on all variables needed for the main analysis from 1990 to 2021. Consequently, I focus on individuals with their first child born between 1995 and 2011. This sample restriction means that only individuals known, alive, and residing in Sweden for the full sequence of years are included. I do not impose any restrictions on the marital or cohabitation status of the parents, nor that it must be the first child for both parents (only that it must be the first child for the individual). I do not impose any restrictions on employment or positive earnings.

The main outcome is annual earnings from the Swedish Tax Agency, defined as labor income before taxes, excluding paid parental leave, tax deductions, and social benefits. Earnings are winsorized at the 99.5 percent level. Parental benefits are delivered by the Swedish Social Insurance Agency and include job-protected paid leave for parents to care for infants and paid temporary leave for parents to care for sick children. It is registered in spells and total amounts. Parental leave benefits are earnings-based and amount to around 80 percent of earnings up to a capped maximum amount for high earners.

For the analysis on parental leave, I use the number of net days of benefits paid out in the first two years from first childbirth.[9] The parental leave days taken by men have increased substantially during the main study period (Figure C1). The median share of men's relative leave-taking has increased from 5 percent in 1995 to 12 percent in 2011 for my main analysis sample.

In addition, the data are merged with matched employer-employee data. These data include a large and representative sample of individuals with information on monthly wages and contracted work hours (full-time equivalent).[10]

[9]Most leave is taken within two years, and leave-taking during the first two years is more important for the father's continued participation in childcare as the child gets older (Duvander and Johansson, 2019; Aldén, Boschini, and Tallås Ahlzen, 2023).

[10]The contracted work hours are stipulated in the work contract and state whether the individual is scheduled to work full-time (40 hours per week) or a percentage of full-time. If an individual's

The information is complete for individuals employed in the public sector. The data on workers in private firms include a representative sample with around 50 percent coverage. This data is used to study monthly wages, contracted work hours, and the probability of becoming a manager. The analysis of these outcomes is therefore conducted on a smaller sample of individuals than the other analyses. These outcomes are also conditional on having employment. Given that it is unusual for individuals to be included in this data set uninterrupted for all 16 periods around child birth, these outcomes are analyzed using an unbalanced panel. Managers are identified using the Swedish Standard for Classification of Occupations (SSYK), based on the International Standard for Classification of Occupations (ISCO). Table A1 shows the descriptive statistics for the main sample.

For the spatial analysis, I use data on the region in which someone lived one year prior to having their first child. The region is defined as a "local labor market," as defined by Statistics Sweden, with the aim to have regions that are "relatively independent from the outside world in terms of labor supply and demand" (Statistics Sweden, 2023). There are small variations over time in how these are defined, but they are relatively constant over time. I use the definition in the year 2018, which includes 69 regions in Sweden.

The multigenerational data goes back to individuals born from 1932 onwards who have been registered in Sweden at some point from 1961. The data from the Pension Authority on pensionable income goes back to 1960 for individuals born in 1938 onwards. In the historical analysis, I use an unbalanced panel in order to include as many years and observations as possible. Hence, I will look at men and women born from 1938 who had their firstborn child from 1961. See Tables A2 and A3 for descriptive statistics on income and age at first birth for cohorts using these data.

In 1974, work-related transfers (e.g., unemployment insurance and parental benefits) became taxable and hence part of pensionable income. I therefore use the Income and Taxation Register (IoT), which is available for years 1968 to 2021, to identify these work-related transfers and remove them from the pensionable income. Moreover, income from the Pension Authority is capped at both ends of the distribution. Income below one price base amount is counted as zero income, and income above 7.5 price base amounts are capped at that level. For child births from 1976 onwards, I therefore use the Income and Taxation Register (IoT). Income from the IoT is winsorized at the 99.5 percent level. All nominal variables are adjusted for inflation using the consumer price index for 2018.

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actual working hours exceed or fall short of the contracted work hours, this will not show in the data. Common examples of when this could happen are, for instance, that an individual's actual working hours exceed the limit of 40 hours per week (working overtime) or that an individual is on parental leave or leave for sickness. Monthly wages are the wages stipulated in the work contract and may also diverge from the actual earnings for the same reasons as contracted work hours.

5 Results

Figure 1 shows the contemporary child penalties along a number of labor market outcomes. Immediately after the birth of the first child, women and men diverge in terms of all labor market outcomes studied. The initial differential impact is starkest in earnings, where there is a 52 percentage point gap between men and women one year after the birth of the first child.[11] Some of this initial gender gap is compensated for with parental benefits, where the gender gap is 30 percentage points when adding parental benefits to earnings.[12]

10 years after family formation, the gender gap in child penalties is 21 percentage points in earnings and 18 percentage points when adding parental benefits to earnings. This result aligns well with the fact that parental leave is mainly taken in the first two years after a child is born. The slightly higher penalty in earnings without added parental benefits in the longer horizon is due to the fact that men and women might also be on parental leave for subsequent children (including temporary leave to care for sick children) and the fact that parental benefits do not account for 100 percent of the earnings.[13]

The fatherhood penalty one year after the birth of the first child is reduced by 9 percentage points when adding parental benefits, showing the importance of parental leave when estimating the impact of children also on male earnings. Consequently, in Sweden, also male labor market outcomes are negatively affected by parenthood, and the main driver of the fatherhood penalty is the use of parental leave. However, even when adding parental benefits, there is a fatherhood penalty of 4 percent over the 10 years following first childbirth.

The gender gap in child penalties in employment is 7 percentage points over the 10 years following first childbirth, which corresponds to less than a fourth of the gap in earnings. Contracted hours has a more pronounced initial dip for women and a gender gap at 15 percentage points over the 10 years following first childbirth. Hence, few men and women tend to leave the labor market completely due to children, and the impact along the intensive margin (part-time work) is relatively more important in understanding the gender gap in earnings. Both men and women have a worse wage trajectory after having children. The gender gap in wages is 5 percentage points over the 10 years following first childbirth and increases over time to 9 percentage points after 10 years. Consequently, both wage and contracted hours are important

[11] Note that since the outcomes are measured at the annual level, the impact in the year of childbirth is mitigated by the fact that earnings in the months before the birth of the child is included.

[12] Parental benefits include both job-protected parental leave to care for infants and temporary leave to care for sick children.

[13] Notably, the inclusion of parental benefits also makes the outcome for Swedish women very similar to the outcomes for Danish (Kleven et al., 2019) and Norwegian (Andresen and Nix, 2022b) women, both in the short and the long run. Given that parental benefits are included in the earnings measurement in both Danish and Norwegian register data, the pattern is, therefore, very similar for women across the Scandinavian countries.

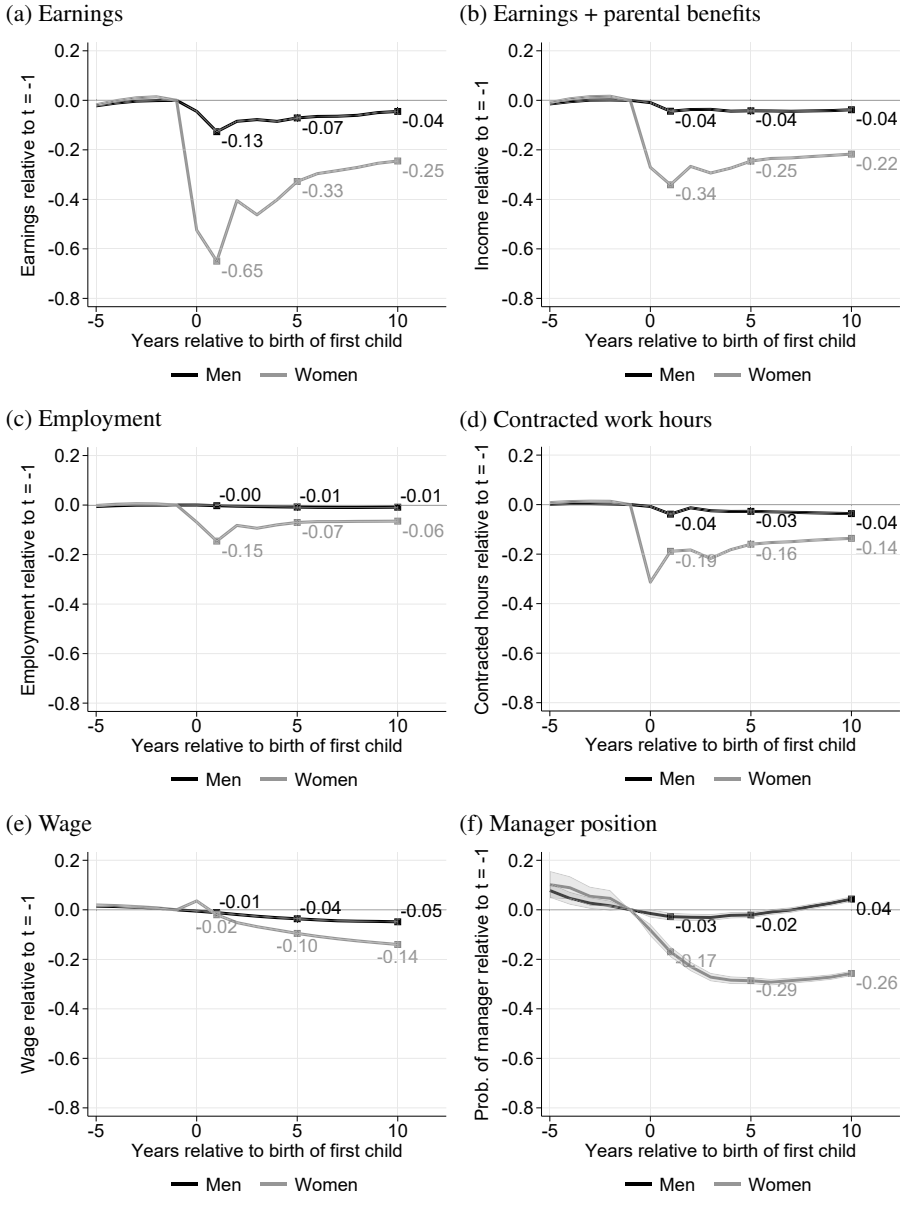

Figure 1. Impact of children on labor market outcomes. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. See Figure C3 for the raw earnings gap and Figure C4 for predicted counterfactual earnings and the impact of children on earnings in SEK. The empirical specifications are shown in Equations 1a and 1b in Section 3. Earnings, income, and employment are unconditional on labor market participation, while hours and wages are conditional on participation. Employment is an indicator variable for having earnings above the first quintile in the earnings distribution. 95 percent confidence intervals in shaded gray.

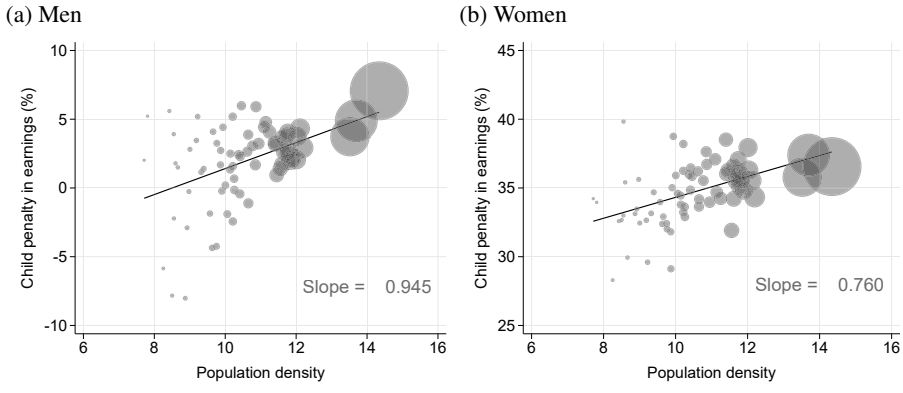

Figure 2. Child penalties across regions in Sweden. The penalty is the average penalty in earnings for the 10 years following first childbirth. Regions are defined as local labor markets according to Statistics Sweden. Population density is defined as the natural logarithm of the number of people living in a region. The size of the circles is the relative size of the population within a region. See Figure C5 for regions.

partial explanations for the earnings penalty.[14] Women are also less likely to become managers following parenthood. There is a substantial 24 percentage point gender gap in the likelihood of becoming a manager over the 10 years following first childbirth.[15]

The number of children is important for the size of the motherhood penalty (Figure C7).[16] The fatherhood penalty is essentially constant across the number of children. Therefore, the gender gap in child penalties over the 10 years following first childbirth increases from 18 percentage points for one child to 30 percentage points for two children, 37 percentage points for three children, and 47 percentage points for four children. It is also clear that the smaller second drop occurring for women three years after the first childbirth in terms of earnings, employment, and hours worked in Figure 1 is driven by the impact of a second child.

Figure 2 shows that there is a positive correlation between population density and the size of both the motherhood and fatherhood penalties, where the

[14] While earnings and income are measured on all men and women, the wage and hours are conditional on labor market participation. Moreover, contracted work hours and wage rates require a change in the employment contract; reduced work hours not included in the employment contract are not covered (e.g., working less overtime or being on leave). The drop in actual hours worked could be different than the drop in contracted hours.

[15] A bit of caution is warranted when interpreting the point estimates for men and women separately, given the pre-trends. The negative pre-trend is given by men and women having children earlier are less likely to be managers.

[16] An important note is that we cannot rule out endogeneity in the number of children. Men and women with one or four children have lower earnings and education levels than men and women with two or three children at the age of their first child (Table A5).

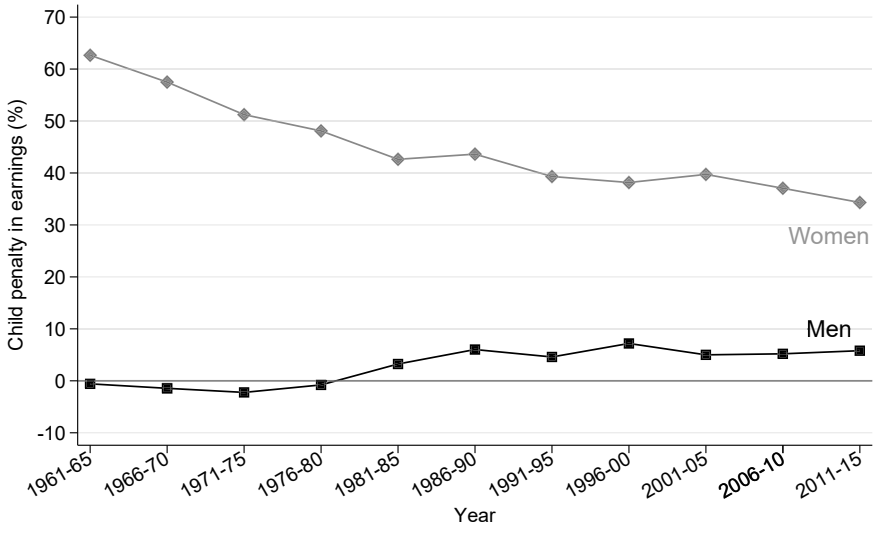

Figure 3. Child penalties for men and women having their first child between 1961 and 2015. Men and women are divided into childbirth cohorts of 5 years based on when their first child was born. The child penalties are defined as the average annual penalty in the 10 years following the first childbirth. See Figure C6 for underlying event study graphs.

penalty is higher in more urban regions. The correlation is stronger between population density and the size of the fatherhood relative to the motherhood penalty. For some rural areas, there is still a fatherhood premium, while the penalty is highest in the most densely populated regions. The range in the size of the fatherhood penalty is substantial, going from a fatherhood premium in some rural areas to a fatherhood penalty of 8 percent in the most urban areas.

5.1 Child penalties over time

Figure 3 shows the child penalties for men and women having their first child between 1961 and 2015. Most strikingly, the motherhood penalty in earnings has been significantly reduced during the 1960s, 1970s, and early 1980s, going from 63 percent for childbirth cohorts in the early 1960s to 43 percent for childbirth cohorts in the early 1980s. Hence, the child penalty in earnings was reduced by 20 percentage points over these two decades. From the late 1980s onwards, the motherhood penalty has been declining at a much slower rate, where the motherhood penalty in the early 2010s is at 34 percent, a reduction by 9 percentage points.

The magnitude of the fatherhood penalties is much smaller, with relatively little variation in the impact of children on men's earnings over the last six decades. Notable, however, is the fact that Sweden has a fatherhood penalty

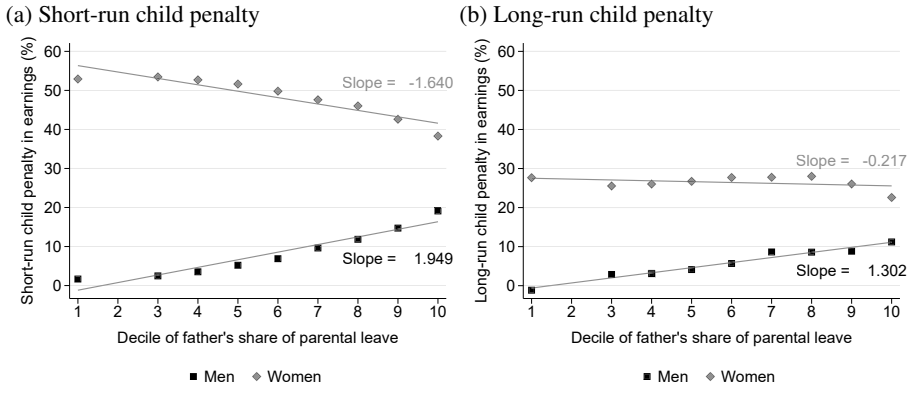

Figure 4. Short-run and long-run child penalties in earnings depending on the father's share of total parental leave within the household. The short-run penalty is the average annual penalty 0 to 4 years after first childbirth. The long-run penalty is the average annual penalty 5 to 10 years after first childbirth. Deciles are based on father's share of parental leave within the household within a given year of first childbirth. Deciles 1 and 2 are not separable as these are men using no leave for parts of the period. For underlying event study graphs, see Figure C11. For underlying distribution of father's share of parental leave, see Figure C1.

since the 1980s. The relatively small magnitude of the average fatherhood penalty means that the changes in the gender gap in earnings stemming from children is mainly driven by the reduction in the motherhood penalty during the early decades. The main reduction in motherhood penalties is driven by women born in the late 1930s to early 1950s, while the fatherhood penalty appears for men born in the late 1950s and is increasing for each generation of men having children (Figure C8).

5.2 Child penalties and gender norms

Figure 4 displays child penalties depending on how gender-egalitarian a household is. Gender norms are proxied by father's share of parental leave. The correlations in panel (a) are partly mechanical, as parental leave inevitably reduces earnings in the short run. In this case, both the motherhood penalty and the fatherhood penalty correlates with the relative use of paternity leave within the household. The penalty in the long run disappears for women but there is still a substantial gradient for men in panel (b). Hence, men in more gender-egalitarian households have a larger fatherhood penalty, while the motherhood penalty is essentially constant across households (except for the highest decile). The long-run fatherhood penalty ranges from non-existent for men using no paternity leave to above 10 percent for men in the highest decile. This finding is corroborated by dividing households by the distribution of days of maternity and paternity leave, respectively, instead of looking at

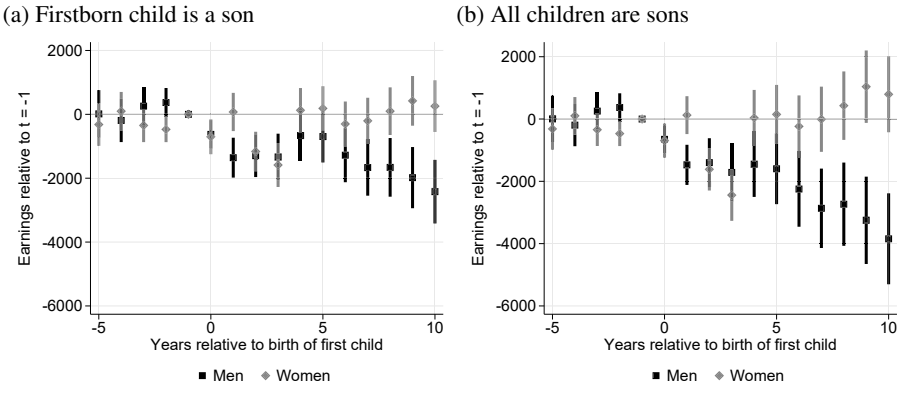

Figure 5. Child penalties from sons relative to daughters. The figures plot the estimates from additional child penalties in earnings from having sons. The left figure shows the impact of having a firstborn son relative to a firstborn daughter. The right figure shows the impact of having sons relative to the same number of daughters. The empirical specification follows Equation 2. The bars are 95% confidence intervals.

the relative use within the household (Figure C9). The result also holds when adding parental benefits to earnings, which means that the long-run impact is not driven by the impact of parental leave for subsequent children (Figure C10).

5.3 Child penalties and child gender

The fatherhood penalty is also higher for men with sons than for men with daughters, while the motherhood penalty is unaffected by the gender of the children. Using the randomness of the gender of the first child, panel (a) in Figure 5 shows the negative earnings effect for men having a firstborn son relative to a firstborn daughter. The figure shows an additional average annual negative impact of 1,688 SEK in the long run (5 to 10 years after first childbirth). Panel (b) shows the negative earnings effect for men in terms of the overall gender composition of the children. This figure only includes observations where the individual has same-gender children, establishing an average annual negative impact of 2,707 SEK from having sons relative to daughters in the long run.[17] Thus, the results are qualitatively similar, but the effect sizes are mitigated in panel (a) because some of the individuals in the control

[17] As shown in Tables B1 and B2, sons are 9.4 percent (6.7 percent focusing on the gender of the first child only) more likely to live with their fathers than daughters, conditional on being a single household. This result is also aligned with previous findings (Dahl and Moretti, 2008; Blau et al., 2020). Including controls for the relationship status when estimating child penalties does, however, not alter the conclusion that fathers to sons have higher child penalties than fathers of daughters (Figure C12). Therefore, the additional penalty having sons is not driven by single household fathers being more common with sons than daughters.

group (whose firstborn child is a daughter) are treated later in the post-period (higher-order child is a son). The long-run son penalty accounts for 11 percent (7 percent for the firstborn child) of the long-run fatherhood penalty. For women, the corresponding numbers are less than 0.3 percent. In conclusion, there is a notable difference in the size of the child penalties among fathers depending on the gender composition of the children but not among mothers.[18] The gradual increase in the "son penalty" indicates that the child's gender becomes more important as the child ages.[19] This finding highlights that it is when the child approaches school age and the age of leisure activities that the child's gender makes a difference in terms of earnings.

6 Conclusions

This paper contributes to the literature on gender inequality in the labor market by focusing on men's and women's labor market outcomes in an environment with less traditional gender norms. First, it shows that there is a considerable motherhood penalty and a small but existing fatherhood penalty in Sweden. Second, it shows that the motherhood penalty has been substantially reduced over time, from 63 percent in the early 1960s to a penalty of 34 percent in the early 2010s. This decline occurred primarily during the 1960s, 1970s, and early 1980s while being while being only modestly reduced from the 1980s up until today. The reduction in the motherhood penalty therefore coincides with a substantial entry of women into the labor market and major family policy reforms in the early 1970s. The observed trends thus contributes to the broader narrative of a transformative change in women's labor market outcomes in Sweden between the 1960s and 1980s.

For men having their firstborn child from the 1980s onwards, there is a fatherhood penalty in earnings. This penalty in earnings is primarily driven by reduced labor supply in the first years after the child is born (partly due to the use of paternity leave) but also persists in the longer horizon. The penalty in the long run is driven by reduced hours worked and lower wages but is small in magnitude. While the long-run motherhood penalty is relatively unaffected by the use of parental leave and the distribution of parental leave within the household, there is a linear increase in the long-run fatherhood penalty for men

[18] Notably, Tables B1 and B2 show that fathers of sons also take slightly more parental leave than fathers of daughters (1.2 percent for only sons relative to only daughters and 0.7 percent for a firstborn son relative to a firstborn daughter). The corresponding numbers for temporary leave to care for sick children are 5.8 percent and 3.3 percent, respectively. However, adding parental benefits (including also temporary leave to care for sick children) does not alter the result that there is an additional penalty from having sons relative to daughters (Figure C13).

[19] This is aligned with previous research on the importance of child gender for fathers' participation in activities with their child (Morgan, Lye, and Condran, 1988; Baker and Milligan, 2016).

using more paternity leave. This result indicates that differential gender norms across households matter for variations in the size of the fatherhood penalty.

Finally, the fatherhood penalty is higher for men who have sons compared to daughters. The gender composition of the children corresponds to 11 percent of the fatherhood penalty, and the gender of the firstborn child corresponds to 7 percent. The fact that fathers of sons have lower earnings than fathers of daughters contrasts with studies from the US and Germany. One potential explanation for this discrepancy is that gender norms in Sweden are different from those in the US and Germany. While the higher earnings of men with sons is often discussed in the literature in terms of a role model effect, this effect may only be present in a context with a stronger breadwinner norm, but not in a more gender-egalitarian environment such as Sweden.

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Appendix A: Descriptive statistics

Table A1. *Descriptive statistics—Main sample*

	Men	Women
Child birth year	2003.4	2003.4
	(4.881)	(4.885)
Age at first childbirth	31.00	29.11
	(4.734)	(4.501)
Years of education	12.43	12.93
	(2.098)	(2.070)
Annual earnings (1000 SEK)	282.1	223.7
	(167.1)	(133.9)
Employment	0.929	0.940
	(0.257)	(0.238)
Monthly wage (1000 SEK)*	27.90	23.86
	(9.451)	(6.934)
Hours worked in % of full-time*	0.756	0.796
	(0.416)	(0.348)
Manager position*	0.0606	0.0240
	(0.239)	(0.153)
Observations	561344	559547

Notes: The table shows descriptive statistics for the main sample. Standard deviation in parenthesis. All variables are measured one year prior to the birth of the first child, except age which is the age in the same year as first childbirth.
*These variables are based on a sample as described in Section 4.

Table A2. *Descriptive statistics for birth cohorts of children*

	Men										
	1961-65	1966-70	1971-75	1976-80	1981-85	1986-90	1991-95	1996-00	2001-05	2006-10	2011-15
Age	22.80	24.79	26.16	27.46	28.36	28.66	29.35	30.40	31.22	31.46	31.49
	(2.185)	(3.024)	(3.629)	(4.310)	(4.806)	(5.064)	(5.243)	(5.364)	(5.404)	(5.665)	(5.784)
Income	130468.5	166942.4	185748.8	181347.3	174975.5	175736.4	184832.4	202608.6	255880.5	267100.0	280920.9
	(50148.1)	(64900.2)	(68506.4)	(406775.1)	(741085.4)	(209591.0)	(148050.4)	(159984.0)	(205673.2)	(204760.6)	(207245.8)
Observations	81746	174767	230022	196545	187187	227055	223148	187257	214276	239264	238482

	Women										
	1961-65	1966-70	1971-75	1976-80	1981-85	1986-90	1991-95	1996-00	2001-05	2006-10	2011-15
Age	21.91	23.39	24.12	25.08	25.82	26.18	26.96	27.98	28.82	28.97	29.02
	(2.312)	(3.132)	(3.839)	(4.385)	(4.575)	(4.563)	(4.712)	(4.850)	(4.915)	(5.096)	(5.044)
Income	92630.3	121561.4	145712.5	140423.3	138322.3	129439.0	141546.3	149677.7	191973.6	201293.6	214545.0
	(31256.8)	(47294.5)	(55083.2)	(328227.4)	(662514.6)	(367572.1)	(89982.4)	(114309.7)	(141645.6)	(152192.2)	(157962.1)
Observations	98302	176554	232150	196720	187285	228428	222827	185471	213254	240158	238662

Notes: The table shows descriptive statistics in terms of age at first child birth and earnings (2018 SEK) one year prior to the year of first childbirth. Standard deviation in parenthesis.

Table A3. *Descriptive statistics for birth cohorts of parents*

	Men								
	1938-42	1943-47	1948-52	1953-57	1958-62	1963-67	1968-72	1973-77	1978-82
Age	27.80	27.26	27.68	28.82	29.59	29.84	30.75	31.28	30.84
	(4.931)	(5.305)	(5.659)	(5.904)	(5.851)	(6.014)	(5.947)	(5.333)	(4.727)
Income	181453.8	183485.6	169993.9	177178.5	189883.3	207948.7	239191.7	265976.4	277140.2
	(71239.3)	(73901.6)	(532779.3)	(243546.0)	(468079.5)	(177770.7)	(203132.5)	(206891.3)	(210622.4)
Observations	178169	261483	249863	231714	227582	263690	244017	234137	216367

	Women								
	1938-42	1943-47	1948-52	1953-57	1958-62	1963-67	1968-72	1973-77	1978-82
Age	26.10	25.09	25.08	25.54	26.53	27.07	27.95	28.94	28.99
	(4.149)	(4.499)	(4.713)	(5.229)	(5.181)	(5.191)	(5.547)	(5.254)	(4.879)
Income	127697.5	136487.9	131778.1	131806.8	138423.7	149847.9	171366.7	196069.8	211618.0
	(57113.8)	(60459.9)	(501833.5)	(455650.3)	(172141.1)	(325854.0)	(132556.1)	(152648.7)	(159742.5)
Observations	128878	223284	233258	232553	229396	262996	249309	238261	225273

Notes: The table shows descriptive statistics in terms of age at first child birth and pensionable income (2018 SEK) one year prior to the year of first childbirth. Standard deviation in parenthesis.

Table A4. *Descriptive statistics—Gender of the child*

	Men		Women	
	Son	Daughter	Son	Daughter
Child birth year	2003.4	2003.4	2003.4	2003.4
	(4.885)	(4.877)	(4.890)	(4.880)
Age	31.00	31.01	29.11	29.11
	(4.734)	(4.735)	(4.502)	(4.501)
Years of education	12.43	12.43	12.92	12.93
	(2.098)	(2.097)	(2.070)	(2.069)
Annual earnings (1000 SEK)	281.8	282.4	223.5	223.9
	(167.1)	(167.1)	(134.0)	(133.8)
Monthly wage (1000 SEK)*	27.88	27.92	23.86	23.86
	(9.428)	(9.473)	(6.926)	(6.942)
Contracted work hours*	0.756	0.757	0.797	0.795
	(0.417)	(0.416)	(0.347)	(0.348)
Observations	272,025	289,319	271,575	287,972

Notes: The table is separated by the gender of the individual and the gender of the individual's first-born child. All variables are one year before the birth of the first child (except age which is the age at the year of birth of the first child). Annual earnings (2018 SEK) are taken from tax registers and adjusted to the consumer price index in 2018. Annual earnings (percentile) are the placement in the income distribution of that given year. Employment is an indicator variable for earning more than the 1st quintile of the earnings distribution in a given year. Contracted work hours are the percentage of full-time work (40 hours per week). Observations are individuals.
*These variables are based on a sample as described in Section 4.

Table A5. *Descriptive statistics—Number of children*

	Men				Women			
	1	2	3	4	1	2	3	4
Child birth year	2003.1	2003.4	2003.4	2003.4	2003.4	2003.5	2003.4	2003.3
	(4.941)	(4.873)	(4.818)	(4.818)	(4.956)	(4.856)	(4.801)	(4.798)
Age	32.88	30.97	29.66	28.69	31.85	29.13	27.59	26.30
	(5.485)	(4.427)	(4.172)	(4.327)	(5.454)	(4.064)	(3.715)	(3.656)
Years of education	11.90	12.54	12.70	12.20	12.46	13.08	13.18	12.56
	(1.994)	(2.083)	(2.158)	(2.143)	(2.105)	(2.037)	(2.084)	(2.147)
Annual earnings (1000 SEK)	263.0	293.6	278.1	238.3	213.9	236.4	216.3	174.2
	(168.5)	(165.0)	(169.8)	(164.5)	(143.9)	(131.9)	(132.2)	(127.3)
Monthly wage (1000 SEK)*	27.48	28.21	27.77	26.21	24.25	24.09	23.65	22.46
	(9.331)	(9.497)	(9.566)	(8.773)	(7.460)	(7.024)	(6.617)	(6.141)
Hours in % full-time*	0.753	0.761	0.752	0.726	0.787	0.808	0.790	0.736
	(0.418)	(0.415)	(0.418)	(0.428)	(0.355)	(0.344)	(0.348)	(0.363)
Observations	108227	323378	107822	15269	92348	319245	103380	13990

Notes: The table is separated by the total number of children ten years after the individual's first child. All variables are one year before the birth of the first child (except age which is the age at the year of birth of the first child). Observations are individuals.
*These variables are based on a sample as described in Section 4.

Appendix B: Child gender and related outcomes

To estimate the importance of child gender on relationship status and parental leave take-up, I use the following specification and focus only on the post-child periods:

$$Y_{it} = \alpha + \beta D_i^{\text{Son}} + \boldsymbol{\phi}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\delta}' \boldsymbol{D}_{it}^{\text{Children}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it} \quad (3a)$$

where D^{Son} is a dummy variable equal to one if the first child is a son and zero if it is a daughter. The bold $\boldsymbol{D}$ refers to vectors of a full set of dummies for event time, number of children, age, and calendar year, respectively. Individuals are included from the year of the birth of their first child up to ten years later. The coefficient of interest is β, which is the effect of having either a first-born son relative to a first-born daughter or the impact of having only sons relative to only daughters. To estimate the percentage effects, I again convert the coefficient β using the following transformation:

$$P_i \equiv \frac{\tilde{\beta}}{\boldsymbol{E}[\tilde{Y}_i]}, \quad (3b)$$

where $\tilde{Y}_i$ is the predicted counterfactual outcome to having a son or only sons (i.e., having a daughter or only daughters). To estimate the effect of gender of the first child on fertility, I modify Specification 3a accordingly:

$$Y_i = \alpha + \beta D_i^{\text{Son}} + \boldsymbol{\gamma}' \boldsymbol{D}_i^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_i^{\text{Year}} + \varepsilon_i \quad (4)$$

where I look only at the number of children ten years after the birth of the first child. I follow Dahl and Moretti (2008) and estimate the impact of the gender of the first child on the number of children and the likelihood of having at least two, three, and four children, respectively. To estimate the percentage effects, I again convert the coefficient β using the following transformation:

$$P_i \equiv \frac{\tilde{\beta}}{\boldsymbol{E}[\tilde{Y}_i]}, \quad (4b)$$

where $\tilde{Y}_i$ is the predicted counterfactual outcome to having a first-born son.

Table B1. *Impact of sons relative to daughters—First-born child is a son*

	Men				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0008	0.0010	0.0142***	69.22***	126.60***
	(0.0011)	(0.0006)	(0.0020)	(19.94)	(8.91)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
No. children	Yes	Yes	Yes	Yes	Yes
Baseline	0.47	0.13	0.21	10197	3805
Percent effect	-0.2%	0.8%	6.7%	0.7%	3.3%
Observations	6019489	6019489	748942	6019489	6019489
	Women				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0005	0.0011*	-0.0079***	-26.79	73.43***
	(0.0011)	(0.0006)	(0.0018)	(27.86)	(9.83)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
No. children	Yes	Yes	Yes	Yes	Yes
Baseline	0.48	0.13	0.84	29728	3743
Percent effect	-0.1%	0.8%	-0.9%	-0.1%	2.0%
Observations	5699117	5699117	727557	5699117	5699117

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the impact of having a first-born son relative to a first-born daughter on a range of outcomes for men and women respectively. The baseline is the average predicted outcome when having a first-born daughter. The percent effect is the percentage increase in the relevant outcome when the first child is a son relative to a daughter. Model specifications are shown in Equations 3a and 3b.

Table B2. *Impact of sons relative to daughters—All children are sons*

	Men				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0020	0.0020**	0.0177***	109.46***	197.50***
	(0.0012)	(0.0008)	(0.0022)	(21.45)	(9.63)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
No. children	Yes	Yes	Yes	Yes	Yes
Baseline	0.43	0.16	0.19	9136	3433
Percent effect	-0.5%	1.3%	9.4%	1.2%	5.8%
Observations	4109206	4109206	591357	4109206	4109206
	Women				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0018	0.0022***	-0.0114***	29.44	122.93***
	(0.0013)	(0.0008)	(0.0020)	(29.94)	(10.98)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
No. children	Yes	Yes	Yes	Yes	Yes
Baseline	0.44	0.15	0.85	29426	3305
Percent effect	-0.4%	1.5%	-1.3%	0.1%	3.7%
Observations	3849434	3849434	570947	3849434	3849434

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the impact of having two first-born sons relative to two first-born daughters on a range of outcomes for men and women respectively. The baseline is the average predicted outcome when having two first-born daughters. The percent effect is the percentage increase in the relevant outcome when the first child is a son relative to a daughter. Model specifications are shown in Equations 3a and 3b.

Table B3. *First child's gender and fertility*

	Men			
		Breakdown by number of children		
	Total number of children	Two or more children	Three or more children	Four or more children
Son	0.0129***	0.0030***	0.0090***	0.0007
	(0.0019)	(0.0010)	(0.0011)	(0.0004)
Year	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Baseline	2.04	0.80	0.21	0.03
Percent effect	0.63%	0.38%	4.25%	2.43%
Observations	569117	569117	569117	569117
	Women			
		Breakdown by number of children		
	Total number of children	Two or more children	Three or more children	Four or more children
Son	0.0177***	0.0051***	0.0114***	0.0012***
	(0.0018)	(0.0009)	(0.0011)	(0.0004)
Year	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Baseline	2.07	0.82	0.21	0.03
Percent effect	0.86%	0.62%	5.31%	4.48%
Observations	567790	567790	567790	567790

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the impact of having a first-born son relative to a first-born daughter on fertility for men and women respectively. The baseline is the average predicted outcome when having a first-born daughter. The percent effect is the percentage increase in fertility when the first child is a son relative to a daughter. Model specifications are shown in Equations 4 and 4b.

Appendix C: Additional figures and tables

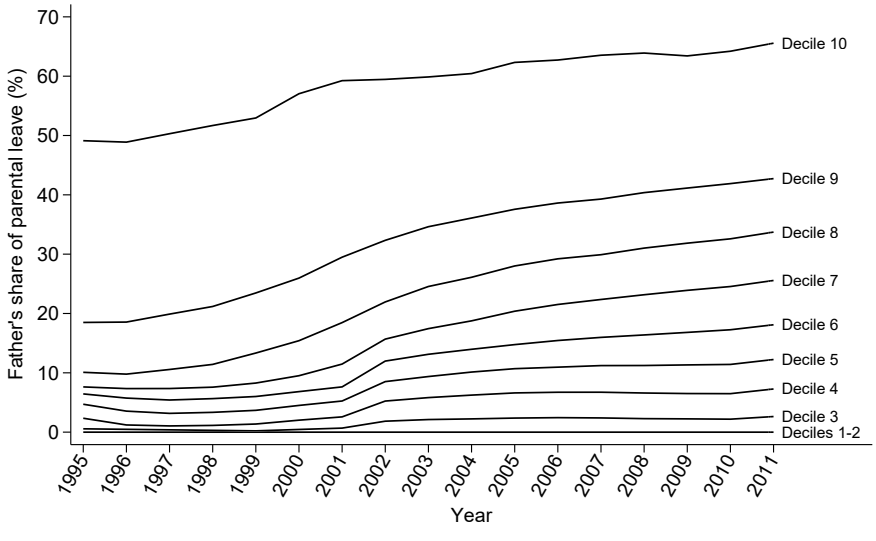
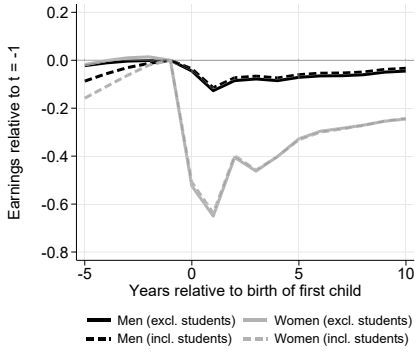


Figure C1. Distribution of father's share of parental leave over first childbirth years for the main analysis sample. Parental leave is the net days of paid parental leave. The first two deciles are combined as they are not separable for the early childbirth cohorts (men taking no leave).

(a) Child penalties in %



(b) Counterfactual earnings

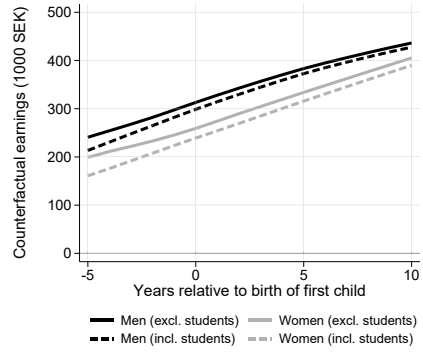


Figure C2. Student restriction. The figures plot the estimated child penalties and counterfactual earnings when including and excluding students. Earnings are adjusted to the consumer price index in 2018. The empirical specifications are shown in Equations 1a and 1b in Section 3. The shaded regions are 95% confidence intervals.

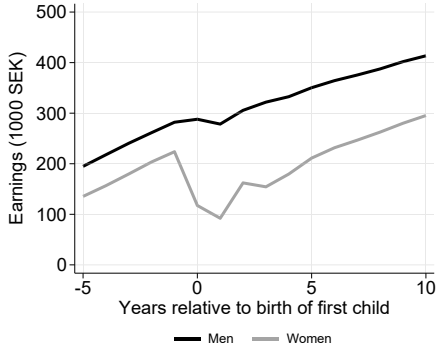


Figure C3. Raw earnings gap from parenthood for men and women having their first child between 1995 and 2011. Earnings are adjusted to the consumer price index in 2018.

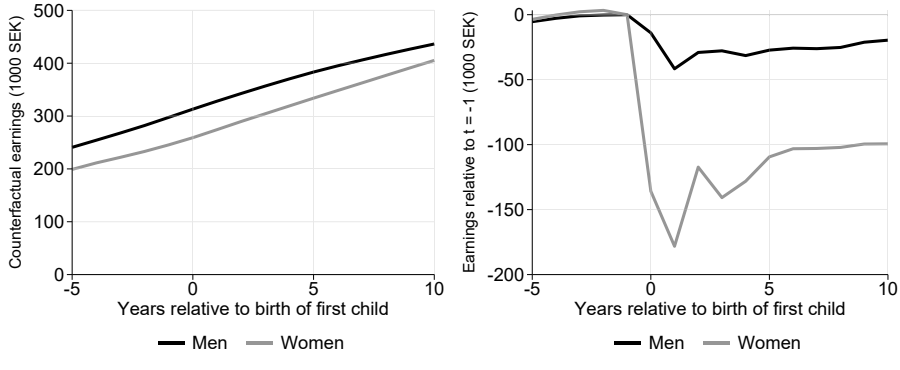


Figure C4. Counterfactual earnings and child penalties in SEK for men and women having their first child between 1995 and 2011. Earnings are adjusted to the consumer price index in 2018. The empirical specifications are shown in Equations 1a and 1b in Section 3.

(a) Men

(b) Women

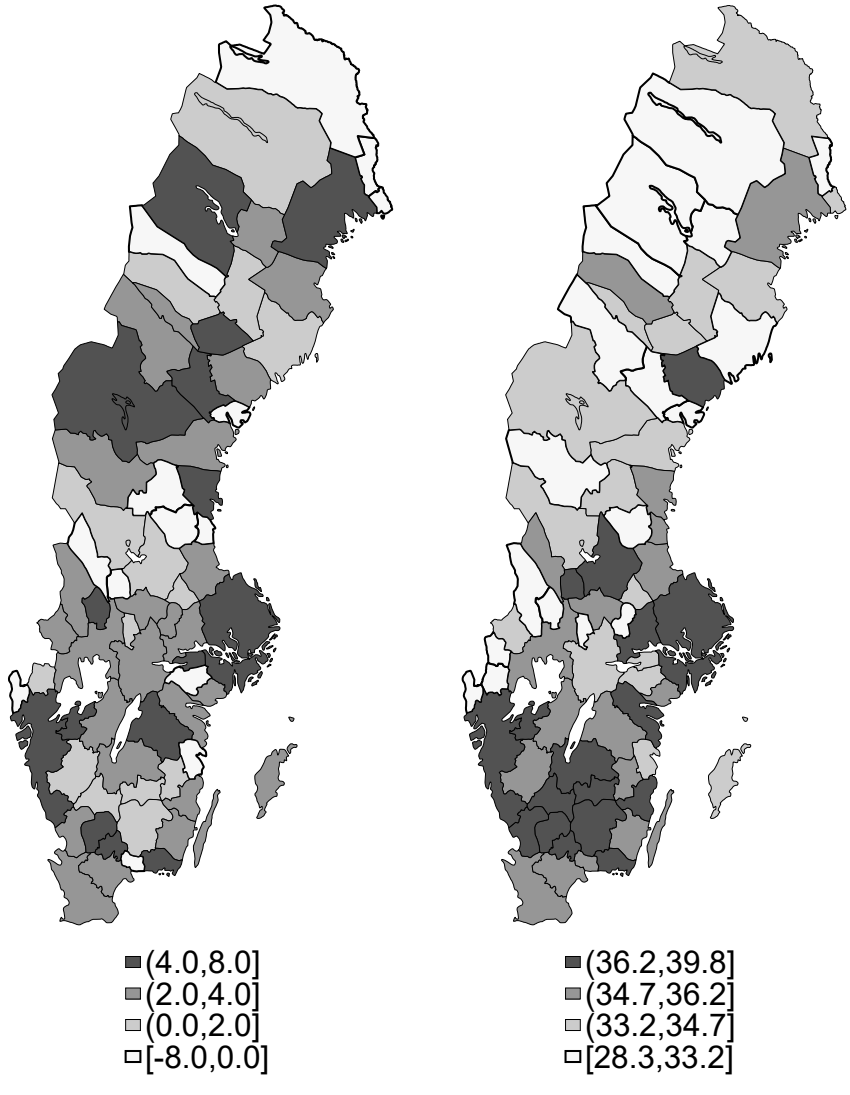


Figure C5. Child penalties for men and women across regions in Sweden. Darker regions have a higher penalty. The penalty is the child penalty in earnings (percent) for the 10 years following first childbirth. The empirical specifications are shown in Equations 1a and 1b in Section 3.

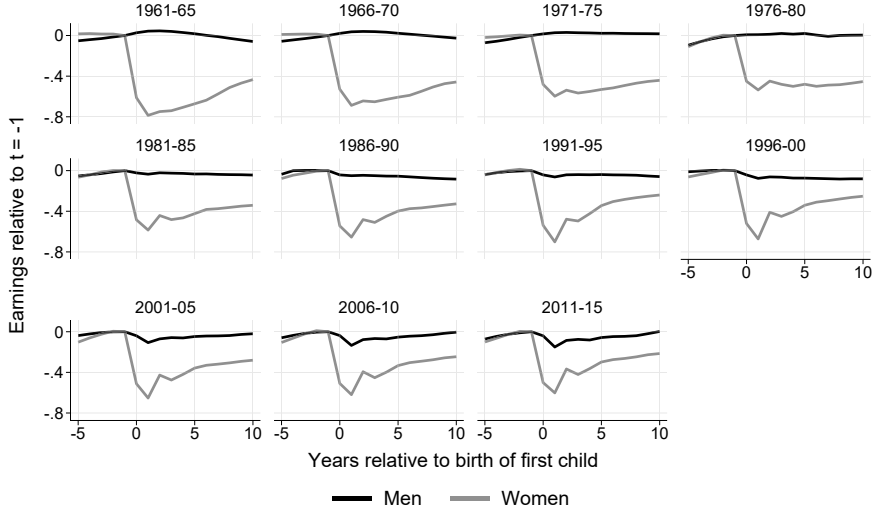


Figure C6. Child penalties for men and women having their first child between 1961 and 2015. Men and women are divided into childbirth cohorts of 5 years based on when their first child was born. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Equations 1a and 1b in Section 3. The shaded regions are 95 percent confidence intervals.

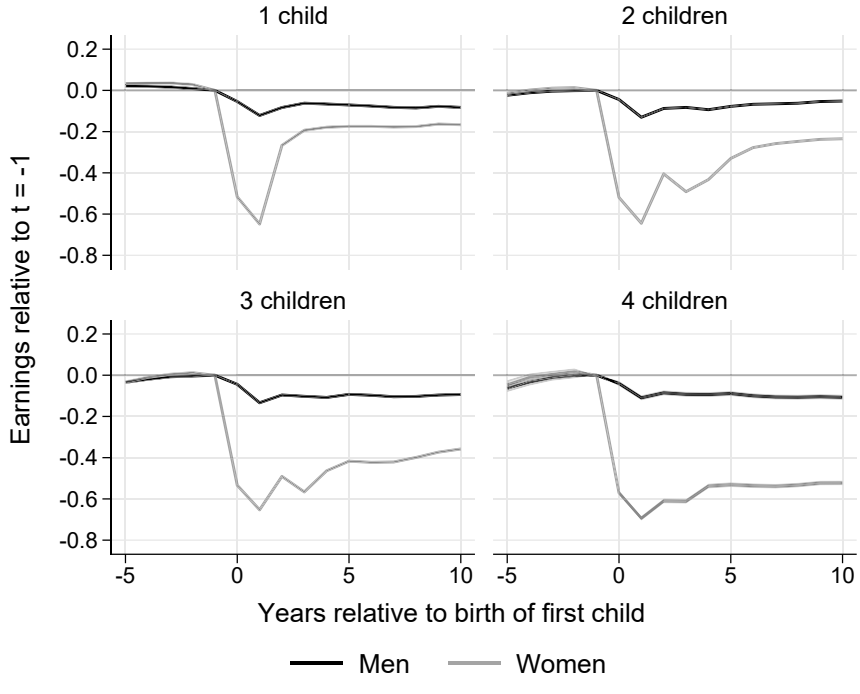


Figure C7. Child penalties in earnings by the number of children. The figure plots the estimates from child penalties in earnings. The legend shows the number of children ten years after the birth of the first child. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Equations 1a and 1b in Section 3. The shaded regions are 95 percent confidence intervals.

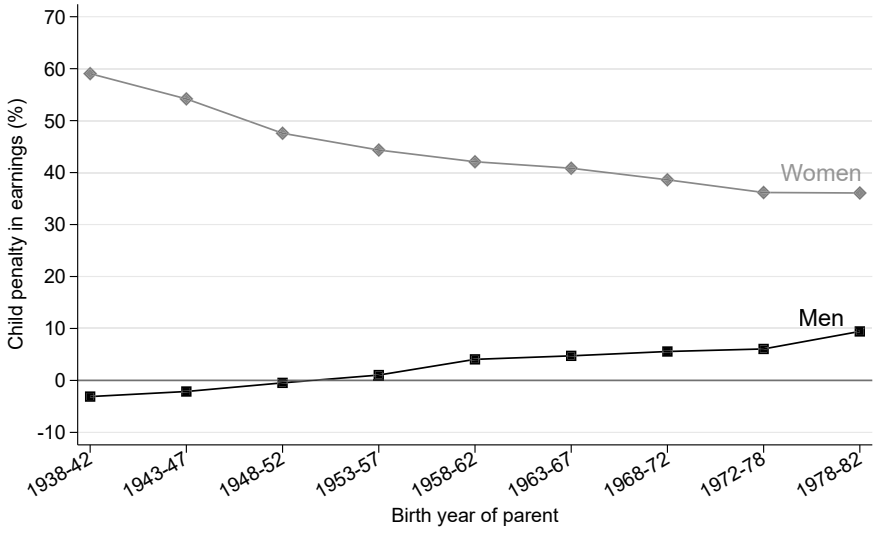


Figure C8. Child penalties for men and women born between 1938 and 1982. The child penalty is defined as the child penalty for the 10 years following the first childbirth. Men and women are divided into birth cohorts of 5 years based on when they were born. The empirical specifications are shown in Equations 1a and 1b in Section 3.

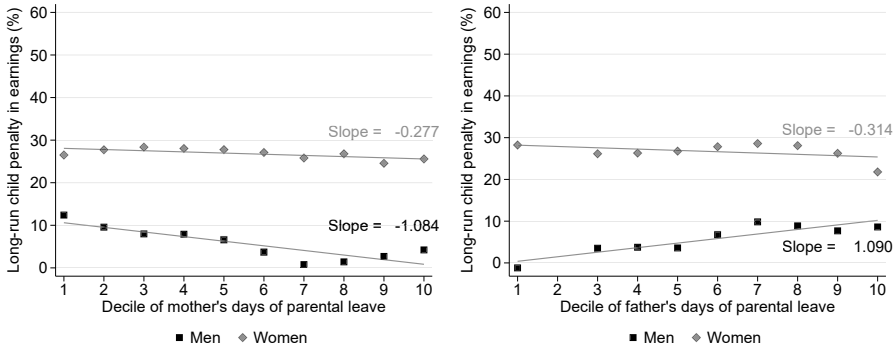


Figure C9. Long-run child penalties in earnings depending on the mother's and father's use of parental leave, respectively. Long-run penalty is defined as the child penalty 6 to 10 years after first childbirth. The empirical specifications are shown in Equations 1a and 1b in Section 3. Deciles are based on the net days of paid parental leave within a given year of first childbirth. Deciles 1 and 2 are not separable in the right panel as these are men using no leave for parts of the period.

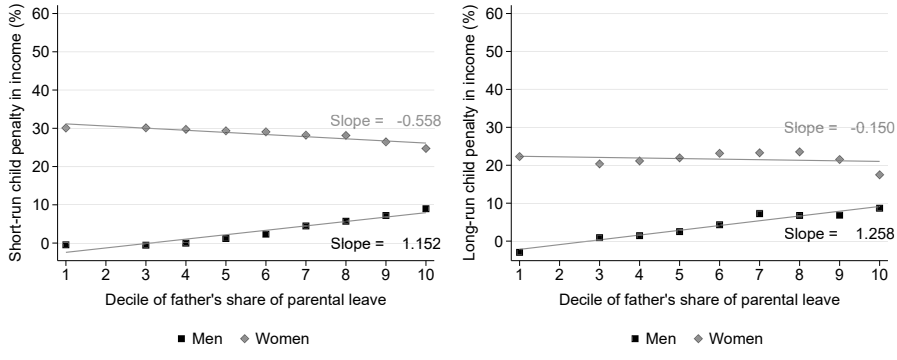


Figure C10. Short-run and long-run child penalties in income (earnings + parental benefits) depending on the father's share of total parental leave within the household. Short-run penalty is defined as the child penalty 0 to 5 years after first childbirth. Long-run penalty is defined as the child penalty 6 to 10 years after first childbirth. The empirical specifications are shown in Equations 1a and 1b in Section 3. Deciles are based on father's share of parental leave within the household within a given year of first childbirth. Deciles 1 and 2 are not separable as these are men using no leave for parts of the period.

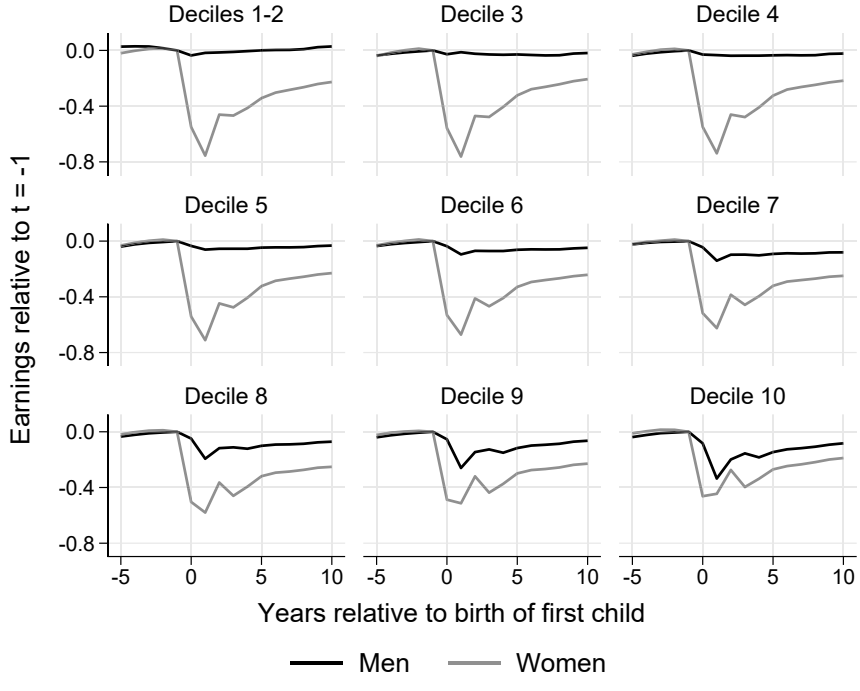
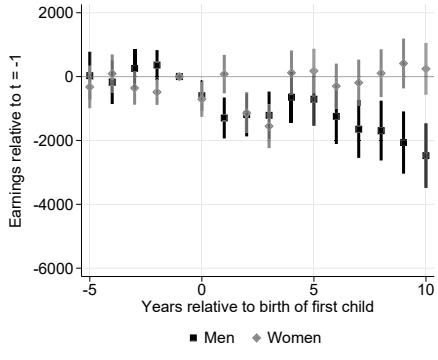


Figure C11. Child penalty in earnings depending on the father's share of parental leave within the household. Deciles are based on father's share of parental leave within the household within a given year of first childbirth. Deciles 1 and 2 are not separable as these are men using no leave for parts of the period. The empirical specifications are shown in Equations 1a and 1b in Section 3.

(a) First-born child is a son



(b) All children are sons

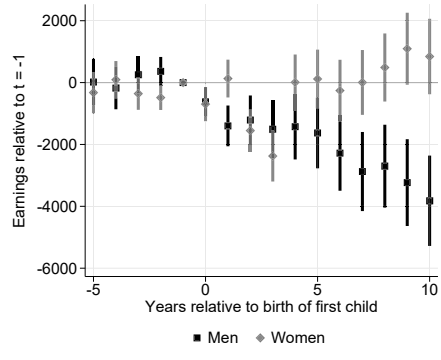
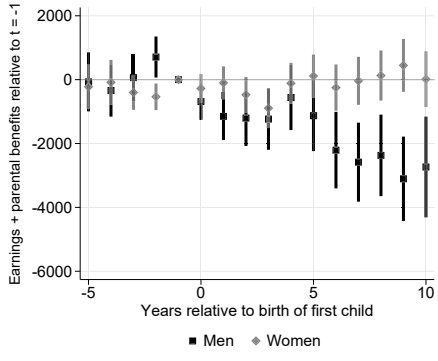


Figure C12. Child penalties from sons relative to daughters (with controls for relationship status). The figures plot the estimates from additional child penalties in earnings from having sons. The left figure shows the impact of having a first-born son relative to a first-born daughter. The right figure shows the impact of having sons relative to the same number of daughters. Event time is relative to the birth of the first child and the outcomes are relative to one year before the first childbirth. The empirical specifications are shown in Equation 2 in Section 3. The regressions include indicator variables for whether the individual is living (i) with their partner, (ii) in a single household with children, or (iii) in a single household without children. The bars are 95% confidence intervals.

(a) First-born child is a son



(b) All children are sons

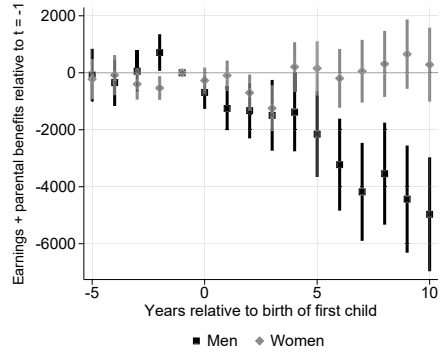


Figure C13. Child penalties from sons relative to daughters with parental benefits added to earnings. The figures plot the estimates from additional child penalties from having sons. The left figure shows the impact of having a first-born son relative to a first-born daughter. The right figure shows the impact of having sons relative to the same number of daughters. Event time is relative to the birth of the first child and the outcomes are relative to one year before the first childbirth. The empirical specifications are shown in Equation 2 in Section 3. The bars are 95% confidence intervals.

Essay II. Origin, Norms, and the Motherhood Penalty

Co-authored with Olof Åslund and Arizo Karimi

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1 Introduction

A rapidly growing literature highlights the role of parenthood in explaining gender gaps in the labor market. A significant body of evidence suggests that having children has substantial and long-lasting effects on the labor market outcomes of women, but typically not on those of men. This so called "motherhood penalty" has been documented in a large number of countries with varying institutional, social, and economic conditions (Dotti Sani, 2015; Kleven et al., 2019; Kleven, Landais, and Leite-Mariante, 2023).[1] Recent evidence from the US suggests that the penalty is substantial also where one could expect it to be less pervasive, e.g. due to strong (relative) female labor market positions (Almond, Cheng, and Machado, 2023).

But the underlying mechanisms are much less well understood. We explore the effect of gender equality norms on the size of the motherhood penalty, studying family formation among former child migrants and children of immigrants in Sweden 1990–2021. The analysis uses the fact that people of different backgrounds who reside in the same country face similar institutions and economic conditions, but are potentially exposed to different cultural factors depending on their ancestry. Thus, we combine the estimation of child penalties using an event study design with the epidemiological approach (Inglehart and Baker, 2000; Hofstede, 2001).

Our general approach in combination with rich population-wide administrative data from a country characterized by substantial and diverse immigration over several decades, provides the opportunity to investigate the role of norms at different levels. In addition to the cultural factors captured by the epidemiological approach, there may be context-specific norms affecting all families. In our setting, similarities in motherhood penalties across groups that differ in background characteristics can be seen as indications on such influences. Moreover, at the micro level, family-specific norms could operate across generations. By investigating the role of grandmother labor market outcomes, and their interaction with background-related gender norms, we uncover how norms at different levels influence individual behavior.

The event study approach to measure the labor market impact of parenthood outlined by Kleven, Landais, and Søgaard (2019) has been proven relevant in many settings and powerful in terms of its strikingly clear results.[2] The idea of comparing individual trajectories around the time of first childbirth to a counterfactual established by those who have not yet given birth, is in many ways appealing. Yet, some methodological concerns apply, not least when

[1] Examples of countries include Sweden (Angelov, Johansson, and Lindahl, 2016), Denmark (Lundborg, Plug, and Rasmussen, 2017; Kleven, Landais, and Søgaard, 2019), Norway (Bütikofer, Jensen, and Salvanes, 2018), Finland (Sieppi and Pehkonen, 2019), Spain (Quinto, Hospido, and Sanz, 2021), and the US (Chung et al., 2017). See also Kleven, Landais, and Leite-Mariante (2023) for a comparison of child penalties in employment across the world.

[2] There is an ongoing methodological discussion in the literature; see e.g. Bensnes, Huitfeldt, and Leuven (2023) and Melentyeva and Riedel (2023) for recent contributions.

using across-group variations to elicit the role of norms. The concept of a penalty is dependent on there being something to lose. If gender norms affect employment and earnings already before having children, a comparison of child penalties may underestimate their true influence. The epidemiological approach also entails a trade-off between on-the-one-hand studying people strongly affected by the origin culture, and on-the-other including individuals that are comparable in individual characteristics and exposure to host context factors.[3]

Focusing on child migrants and children of immigrants gives comparability in terms of institutional and overall societal exposure during adolescence and early adulthood. By documenting pre-child differences and supplementing the baseline estimates by a coarsened exact matching approach to study child penalties among individuals with similar status and characteristics, we illuminate the potential influences of norms in a more complete way.

We estimate total child penalties over a ten-year period after first childbirth and relate this penalty to measures of gender inequality based on country of ancestry. Similar to e.g. Blau et al. (2020), we use the Global Gender Gap Index (GGI) from the World Economic Forum to measure culture and gender norms. The GGI takes into account social, political, and economic equality across the genders. We show that this measure is highly correlated with female relative labor force participation rates, which has been the main measure of gender inequality or norms used in the literature. We rank countries according to their GGI score and divide them into 12 groups.[4]

Our baseline findings point to origin related norms as determinants of the motherhood penalty (MP), but also show that mothers of diverse backgrounds exhibit striking similarities in a shared context. Further analyses reinforce that the story is more one of similarities than of differences, while not completely ruling out the influence of group-specific cultural norms.

We show that origin country GGI is related to pre-child female labor market outcomes also among the child migrants and children of immigrants constituting our main sample. The rank correlation between the gender equality index and labor income two years prior to first child birth is 0.35. Although substantial, the fact that this correlation is smaller than for the first generation (0.75) shows that integration across generations decreases the significance of source country factors.[5] Mothers originating in countries characterized by unequal

[3]For example, correlations between source country child penalties and those observed among people migrating as adults may reflect country/gender-specific determinants of pre-child investments and decisions, rather than norms influencing behavior in the host country. Furthermore, the often long process of labor market assimilation among immigrants (Duleep, 2015) also raises issues about comparable baseline trajectories of treated and controls.

[4]The division of groups is computer driven and based on having as similar-sized groups as possible conditional on the GGI ranking (see Table C7).

[5]The correlation across generations in country group earnings is 0.32 for years of education and 0.30 for earnings.

gender norms are slightly younger at first childbirth, have more children on average, and exhibit a greater age difference to their spouses. But there is no association between GGI and educational attainment in our main sample.

Event study estimates (following Kleven, Landais, and Søgaard, 2019) by ancestry/origin suggest a a non-linear association between the estimated earnings penalty and the GGI index. While the penalties in the three most unequal quantiles is about 45 percent, it is stable around 40 percent for higher quantiles. A negative association is also present in specifications controlling for other source region factors in terms of GDP and average fertility rates. Employment penalties over the 10-year period show an even stronger association with the gender equality measure of the region of origin. In general, the estimates confirm the presence of a substantial motherhood penalty in the Swedish context of comparatively strong family-friendly institutions and otherwise limited gender gaps. All origin groups exhibit a sharp income drop after child birth and incomplete long-term recovery.

As discussed above, the potential penalty from parenthood depends on the point of departure: If you earn very little, you don't have much to lose. Pre-child differences across groups can thus affect patterns of estimated penalties. Using coarsened exact matching we therefore compare deviations between the penalty of each GGI group and a sample of Sweden-origin mothers similar in terms of own and partner income and age. Results show that increasing comparability means an even stronger similarity in maternal earnings trajectories. However, a moderate but statistically significant association between the deviation from the matched comparison group and the quantile of gender equality remains, driven by the very most gender unequal source country groups.

Taken together, the results show that motherhood penalties are arguably more similar than different across groups characterized by highly diverse backgrounds. This suggests that reforms affecting common conditions in the host context are likely to have similar impacts in groups with varying background. The findings also suggest that differential responses to parenthood related to cultural background are not a main driver of gender earnings gaps being particularly large in some immigrant communities. Our results do not, however, rule out that gender norms are a key driver for the widely observed difference in the parenthood penalty across genders, but instead point to that these norms may be formed and operate largely within the shared context.

Our paper contributes to the literature on the mechanisms behind the child penalty. Previous work gives little support to biology in terms of pregnancy related factors when comparing penalties in families with adopted and biological children (Kleven, Landais, and Søgaard, 2021), or to gender-based comparative advantage when examining differences across heterosexual and same-sex couples (Moberg, 2016; Andresen and Nix, 2022). Variations in family policies have also been shown to have limited explanatory power for the long-run effects of children on women's earnings (see e.g. Lalive and

Zweimüller, 2009; Lalive et al., 2014; Schönberg and Ludsteck, 2014; Kleven et al., 2022).[6]

Norms and culture are factors receiving increasing attention in the general literature on gender labor market disparities.[7] The epidemiological approach has been used to study a variety of outcomes including female labor force participation and fertility (Guiso, Sapienza, and Zingales, 2006; Fernández, 2011; Giuliano, 2021). By culture, one typically refers to a collection of beliefs and preferences; in this context those specifically related to gender norms. Norms are usually proxied with past female labor force participation rates from individuals' country of ancestry (Fernandez and Fogli, 2009) or (as in our case) with summary measures of overall gender inequality.

In the context of motherhood penalties, Kleven et al. (2019) show a positive relationship between child penalties and elicited gender norms across countries, which is consistent with an important role for gender norms. Boelmann, Raute, and Schönberg (2021) find that East German mothers return to work sooner than West German mothers (living within the same commuting zone) even two decades after reunification, suggesting a strong persistence of the culture in which women were raised. Kleven (2023) shows that child penalties correlate with gender norms in the US. Building on the epidemiological approach, the study also finds strong associations between source region/country child penalties and the penalties among movers/migrants.

The literature on the role of culture in determining women's labor supply exploiting source country characteristics is by construction often related to the labor market integration of immigrant women (Antecol, 2000; Fortin, 2005; Blau, Kahn, and Papps, 2011; Blau, 2015; Blau and Kahn, 2015; Finseraas and Kotsadam, 2017; Neuman, 2018). A typical finding is that the labor market performance in the host country is positively associated with the female labor force participation in the source country. There are also investigations of other outcomes using epidemiological measures; e.g. do Blau et al., 2020 find that US immigrants allocate tasks within the household differently depending on the characteristics of their source countries. Other studies focus on intergenerational transmission of roles and attitudes (Fernández, Fogli, and Olivetti, 2004; Bütikofer, 2013; Farré and Vella, 2013; Bredtmann, Höckel, and Otten, 2020). These studies tend to find that immigrant source country gender roles influence immigrant and second generation behavior in the receiving country.

Previous work thus suggests that there exists a factor, i.e., culture or gender norms, that is distinguishable from human capital or social capital, which

[6]There is also evidence that women (in the US and UK) systematically underestimate the employment effects of motherhood, and that women and men tend to express more traditional values after becoming parents (Kuziemko et al., 2018).

[7]Despite significantly converging roles of men and women in the labor market and society, there are still sizable gender gaps in employment, wages, and representation in top jobs in virtually all countries (Olivetti and Petrongolo, 2016; Petrongolo, 2019; Bertrand, 2020; Cortés and Pan, 2023; Goldin, 2023)

affects economic behavior. At the same time, these studies also document that culture is malleable; there is substantial evidence of cultural assimilation among second generation populations. Similarly, studies on intergenerational transmissions of attitudes document significant effects of parents' attitudes and behaviors on those of their children. Overall, this literature establishes an important role of culture for economic outcomes, and of both vertical and horizontal transmission of norms and culture.

2 Data

We use administrative data on the Swedish population from several registers linked by unique identifiers. The data include annual information on all individuals aged 16–74 from 1990 to 2021 and have been compiled and pseudonymized by Statistics Sweden into collections held by the Institute for Evaluation of Labor Market and Education Policy (IFAU). There is detailed information on earnings, parental benefits, educational attainment, social benefits, and family relationships. All nominal variables are adjusted for inflation using the 2018 consumer price index. Earnings are taken from tax registers and are winsorized at the 99.5% level. The main outcome is annual earnings (income from employment). We also study employment, full-time equivalent monthly wages, and a labor income measure adding parental benefits (income from job-protected parental leave plus temporary leave to care for sick children) to earnings.

The annual data are merged with multigenerational information on child-parent relationships. Households are defined as a man and a woman with a joint child. Men and women are included from five years before the birth of their first child to up to ten years after. This means we include child births occurring over the time period 1990–2021. It does not have to be the first child for both the man and the woman, but only the first child for the focal person. The number of children is the number of own children, not the number of children in the household.

Individuals are excluded from the panel in years when they are studying (defined through the receipt of student benefits and loans). Otherwise, we place no restrictions on positive earnings or relationship status, which means that all individuals are included as long as they are in the population registers. If an individual dies or moves out of Sweden, they are included up to that point. Therefore, an individual does not have to be in the data for all 16 years (around childbirth) to be included in the analysis. Thus, the analysis is based on an unbalanced panel of individuals.

We have information on the place of birth of the individuals and the place of birth of their parents (if the individual is born in Sweden) for all individuals. To identify gender norms, we primarily use the Global Gender Gap Index (GGI) from the World Economic Forum (World Economic Forum, 2023).

Countries are ranked according to the level of gender inequality in that country according to the GGI (see Table C7).[8]

Our main analysis focuses on child migrants (at most ten years old at immigration) and children of immigrants (second-generation). A Sweden-born individual is defined as second-generation if both of the individual's parents were born outside of Sweden. To classify origin, we use the place of birth for child migrants and the place of birth of the individual's mother for second-generation individuals. We pool the samples of child migrants and second generation immigrants according to these definitions, and refer to the pooled sample as the group with immigrant background, and the sample of Sweden-born individuals with Sweden-born parents as natives.

3 Research design

3.1 Baseline analysis

We follow previous literature (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019) by estimating child penalties using an event study design including individuals that have children at some point. Identification comes from individuals of the same age in the same calendar year, but with a first child born at a different age since all individuals in the regressions have children at some point. Identification therefore comes from variation in the treatment timing, i.e. at which age they have their first child. Following Kleven et al. (2019), we add calendar year dummies and age dummies to control flexibly for business cycle trends and life cycle trends:

$$Y_{it} = \boldsymbol{\beta}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it}, \tag{1a}$$

where Y_{it} is the labor market outcome of interest (primarily labor income) for individual i in event time t. Boldface is used to denote vectors, where $\boldsymbol{D}$ refers to vectors of a full set of dummies for event time, age, and calendar year. Individuals are included from five years before first birth to ten years after. Event time $t = -1$ is omitted to provide the baseline. We also follow Kleven, Landais, and Søgaard (2019) and convert the coefficients to percentage effects using the following specification:

[8] If there are few people from a specific source country, the place of birth is grouped into a larger group of countries (a region). Since the gender norms are given at the country level, and in a few cases we only have the *region* of birth, we have weighted the GGI according to the number of immigrants from that country relative to the other countries in that region. Hence, the weights are proportional to the number of immigrants in Sweden during our period of analysis. Moreover, some countries are not included in the Global Gender Gap Index. In these cases have we imputed a GGI score based on the Gender Development Index (GDI), female labor force participation rate, fertility rate, and GDP for the country. For the countries that are grouped together and for the countries where GGI is imputed, see Table C7.

$$P_{it} \equiv \frac{\tilde{\beta}_t}{\boldsymbol{E}[\tilde{Y}_{it} \mid t]}, \tag{1b}$$

where $\tilde{Y}_{it}$ is the predicted counterfactual outcome of having children. Standard errors are clustered at the individual level and robust to heteroskedasticity.

Within this framework, we need to assume that the timing of births is random conditional on age and calendar year. Although it is impossible to test the validity of this assumption, we can look at pre-trends to rule out that the decision to have children is clearly correlated with unobservable characteristics that matter for labor market outcomes in the period prior to parenthood.[9]

For Sweden, the positive pre-trends for mothers are driven by the inclusion of students. From Figure C1, it is evident that the exclusion of students (identified by the reception of student benefits) also means that there are no longer any pre-trends present, although the size of the estimated child penalties are unaffected. Comparing the pre- and post-period, it seems that individuals in Sweden (both women and men) tend to wait with having children until they are done with their studies. Moreover, very few individuals become students after they have children. This pattern could potentially be driven by the fact that the relatively generous scheme for parental benefits is tied to earnings, generating strong economic incentives to enter the labor market before entering parenthood.

Conditional on not being a student, the parallel trends assumption holds. Earnings are stable until the birth of the first child and thereafter drop sharply. It is therefore unlikely that the short-run earnings drop after entering parenthood is due to something else than the event of having children. The flat pre-trends signify that the decision to enter parenthood is not driven by labor market outcomes, e.g., by waiting for promotion or having children as a response to becoming unemployed. In the long-run, we are not able to rely on the smoothness assumption to the same extent, and interpreting the long-run penalties requires stronger assumptions.

To compare across regional origins, we run Equation 1a for each regional group separately. That means that we allow non-parametric variation in terms of period (calendar year), cohort (year of birth for first child), and life-cycle (age of parenthood) across groups. Hence, we allow for the groups to differ in their counterfactual earnings trajectories. Given that we have essentially no pre-trends for any group, the common trend assumption within each group holds. To compare regional groups we again make the transformation in Equation 1b, which means that we compare the impact of children relative to the expected earnings for each GGI category. To relate child penalties to the level of gender equality in the source region we take the average of the estimated

[9]Positive pre-trends are common in the literature on child penalties (Kleven et al., 2019; Andresen and Nix, 2022).

child penalties over the 10-year horizon following the birth of the first child, and plot it against the GGI rank.

3.2 Other source country characteristics

Given that we use variation in regional origin as a measure of gender norms, we need to make sure that we are capturing differences in gender norms from the source country and not something else that correlates with these gender norms. Following Blau et al. (2020), we consider regional rankings in terms of GDP per capita and fertility. We run a series of regressions relating the outcome to GGI, GDP per capita, and the fertility rate, interacted with age and calendar year. The most extensive specification is:

$$\begin{aligned} Y_{it}^{j} = \alpha + \sum_{k=1}^{3} \beta_k D_i^{\text{Post}} \times X_k^{j} + \sum_{k=1}^{3} \gamma_k \boldsymbol{D}_i^{\text{Age}} \times X_k^{j} \\ + \sum_{k=1}^{3} \lambda_k \boldsymbol{D}_i^{\text{Year}} \times X_k^{j} + \tau \boldsymbol{D}_i^{\text{Event}} + \varepsilon_{it} \end{aligned} \tag{2}$$

for individual i, with parental region j, in event time t, where $X_1^j = \text{GGI}^j$, $X_2^j = \text{GDP}^j$, and $X_3^j = \text{Fertility}^j$.

3.3 Matched comparisons

To address the concern that the GGI groups may differ in characteristics (e.g. labor market attachment prior to parenthood) potentially related to the impact of family formation, we conduct an additional analysis in which we compare each GGI group with a matched group of native parents. We use a coarsened exact matching following the procedure described in Blackwell et al. (2009) and Iacus, King, and Porro (2012). We match on age, calendar year, educational attainment, and pre-parenthood earnings. As the characteristics of both the father and mother are potentially important, we match on the earnings and education level of both parents. We use one-to-one matching, i.e. only individuals with a perfect match are included, and the rest are excluded. To enable exact matching, earnings are binned into quintiles, while age and calendar year are binned into groups of five years. Given the large number of individuals in the native population, the number of individuals that needs to be excluded in the immigrant background group is relatively small (see Table C5).

4 Description

This section first presents statistics at the country group level underlying the ranking in terms of gender equality norms. Then we discuss individual and household characteristics of the sample used in the main analysis.

4.1 Gender equality ranking and other country characteristics

Table C7 presents the (parental) birth country groups used in creating the gender equality ranking. The World Economic Forum's gender gap index (GGI) discussed in Section 2 places Iraq at the bottom of gender equality and Iceland at the top. While there is some variation, countries in the Middle East are often found in the lower end of the ranking, whereas Northern European (in particular Nordic) countries are typically found in the upper part. The GGI is strongly correlated with other indicators of inequality, and also with economic development. High GGI values are associated with higher GDP per capita, and with lower fertility rates. In the analysis we will use both the ranking and the values for GGI (and other indicators).

4.2 Characteristics of the main sample

Table 1 displays characteristics for mothers in the main analysis sample. We divide the group of immigrant background (arriving before age 10 or born in Sweden) into high and low GGI countries, where the former constitute about two thirds of the sample, containing 52,883 mothers of immigrant background. The number of native mothers included amount to some 809,936.

Immigrant mothers are on average almost one year younger than natives at first childbirth, and those originating in countries with less equal gender norms are also substantially younger than those from more equal origins. They also exhibit a somewhat larger age difference to their partners, although the numbers are between 2.57 and 3.21 in all subgroups. The data show no strong signs of differences in completed fertility, although the figure is highest in the low GGI category (2.17 compared to 2.00 among the high GGI mothers).

Turning to earnings two years prior to first childbirth, we find that the region of origin differences typically seen among adult migrants are present also in our sample of child migrants and children of immigrants. Earnings are lower among individuals of immigrant background than among natives, and particularly among those originating in countries classified as less gender equal. Statistics based on the less crude grouping (Table C6), suggests a rank correlation between the GGI and individual earnings of 0.36. At first glance, this pattern is consistent with women originating in less equal countries prioritizing labor market outcomes to a lesser degree. However, further statistics in Table 1 signal that it may be premature to assign all of the differences to gender equality norms.

Table 1. *Descriptive characteristics—Women*

	Immigrants			Natives
	All	Low GGI	High GGI	
Age	27.23	26.68	27.39	28.06
	(4.89)	(4.56)	(4.97)	(4.75)
Age difference to partner	2.73	3.21	2.59	2.57
	(4.25)	(4.13)	(4.27)	(4.13)
Number of children	2.04	2.17	2.00	2.05
	(0.72)	(0.74)	(0.71)	(0.67)
Years of education	11.92	12.16	11.86	12.40
	(2.10)	(2.19)	(2.07)	(2.12)
Quantile of income	45.34	40.91	46.63	50.63
	(24.34)	(25.79)	(23.75)	(22.66)
Observations	52,883	11,867	41,016	809,936

Notes: The table shows descriptive statistics for our main analysis sample of women. Age is measured at the year of first childbirth. Number of children is the total number of children within eight years from first childbirth. Years of education are measured two years prior to first childbirth. Quantile of income is the income percentile two years prior to the first childbirth. Women with immigrant backgrounds are divided into two groups depending on being in the upper or lower part of the distribution in terms of source region GGI. See Table C7 for a ranking of source regions according to GGI. Low GGI are countries in the lower half of the ranking (1–46) and High GGI are countries in the upper half (47–92). See also Tables C1 and C2 for descriptive statistics for each GGI quantile in our main analysis samples and Tables C3 and C4 for descriptive statistics for the same GGI quantiles for 1st generation immigrants at age 45.

Most of the previous work using the epidemiological approach to study gender norms and the outcomes of immigrant women in the labor market have focused on first generation (adult) migrants. While we believe there are good reasons to focus an analysis of child penalties on the child migrants and the second generation (e.g. alleviating concerns about delayed fertility due to unobserved circumstances for adult migrants), comparing outcomes across generations within the region of origin groups is relevant. In Table C6, we document that the characteristics at the country of origin level among females of immigrant background are highly correlated across the first and second generations. Also, they are strongly associated with patterns in the region of origin. For example, the pre-child income of mothers in our main sample has a correlation of 0.30 with the income of women in the first generation, and a similar correlation with source country GDP and GGI. Years of education is related across generations (correlation coefficient 0.32), and negatively associated with fertility rates in the source region. The latter variable exhibits a positive association with the number of children born in the first and second generation of migrants to Sweden, however declining over generations.

5 Results

This section presents the results from the empirical analysis outlined in Section 3. First, we consider child penalties among mothers of different origins and their correlation with gender equality norms as reflected in the GGI. After a graphical representation of the estimates and the associations, we investigate whether the link between gender equality classifications and child penalties can be explained by other source region characteristics. Second, we perform a matched analysis comparing immigrant mothers to natives with similar individual and partner outcomes prior to first childbirth. This analysis addresses the concern that there may be adaptations due to gender norms already before family formation, and the possibility that child penalties are dependent on the point of departure (e.g. how much one stands to lose or that work-life adaptation opportunity varies with earnings levels). Third, we look at the importance of gender norms on child penalties by focusing on the relative income of grandparents.

5.1 Source region norms and the motherhood penalty

The upper graph of Figure 1 displays the event study graphs for motherhood penalties (MP) by GGI quantile group in the main sample. The first thing to notice is that all the categories exhibit the same characteristic pattern of a substantial drop in earnings, and an incomplete earnings recovery, over the first ten years following first childbirth. In other words, mothers of varying

background in Sweden share similarities not only with each other, but with mothers around the world

As seen in panel (b) of Figure 1, illustrating the association between the estimated penalties (over the 10-year period) and the GGI ranking, there appears to be some source region gender equality norms also among mothers fully or to a large degree grown up in the same broader Swedish context. The estimated earnings loss varies from 46 percent in the 1st and 3rd quantile, to 37 percent in the 7th and 9th quantile. The estimated slope for the 12 quantiles is -0.618 percentage points per step in the ranking. However, it seems like the association is nonlinear and driven entirely by the difference between quantile 1–3 and the higher GGI groups.

Corresponding analyses for other labor market outcomes reveal a substantial GGI gradient in the motherhood penalty for labor income including parental benefits (Figure A1) and for employment (Figure A2), where estimated impacts range between 33 and 24 percent (relative to pre-child levels) in income including parental benefits, and between 25 and 12 percent for employment. For full-time equivalent wages and contracted work hours (percent of full-time), the estimated impact of motherhood is smaller but also exhibits a negative association with GGI (Figure A3 and A4).

It is possible that the gender equality index is correlated with and captures other source country characteristics than gender norms. The first column of Table 2 displays estimates of Equation 2 described in Section 3, interacting the event (*Post*, which is first childbirth) with the linear GGI rank variable. Note that the estimations also allow both age and year effects to vary with GGI background. The point estimate suggests that moving up one step in the ranking means slightly below SEK 500 higher annual earnings post motherhood on average, i.e. a smaller child penalty. Columns 2 and 3 perform the same analysis, but replaces GGI with source country GDP and fertility rates, respectively. Estimates suggest that people with a family background in richer countries experience smaller child penalties, and those originating where fertility is higher have stronger penalties. Columns 4–6 combine the source country variables. The estimated impact of gender norms is not much affected by controlling for the other source region characteristics.[10]

[10]It could be noted that the sign of the fertility estimates changes when one accounts for GDP and the GGI ranking.

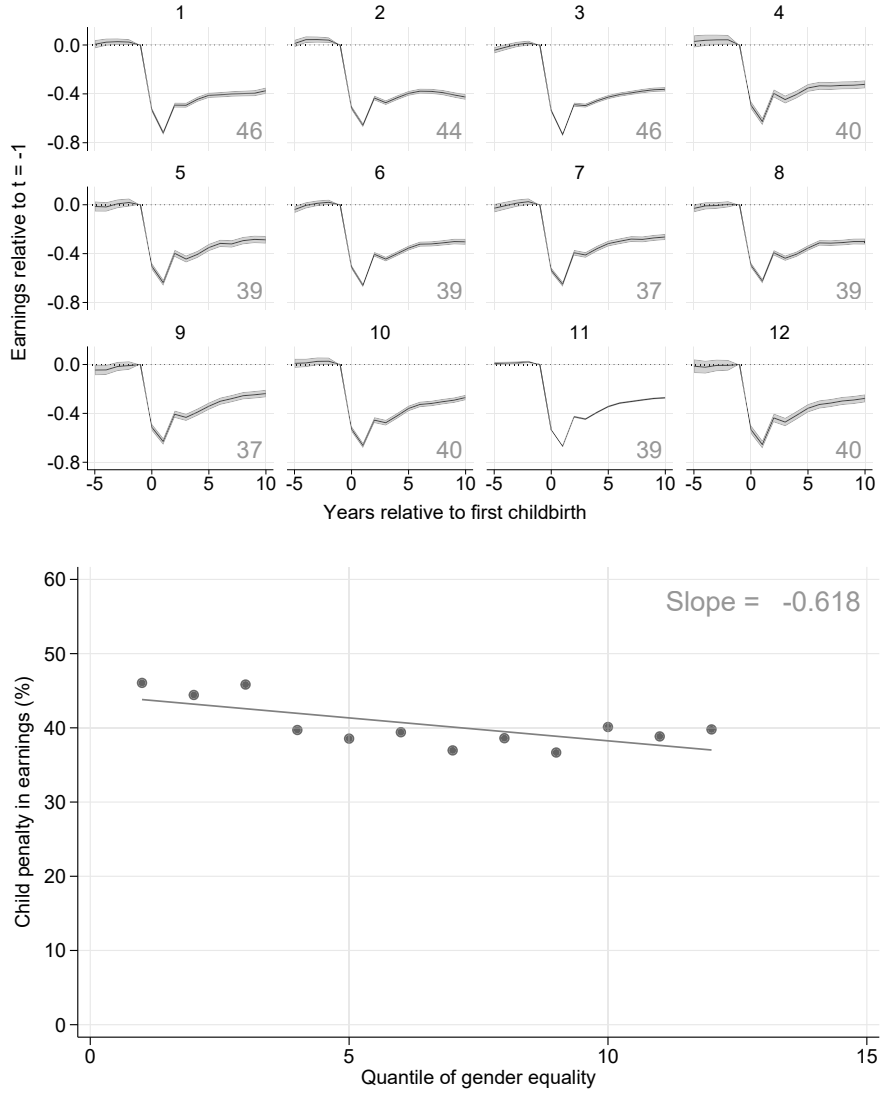

Figure 1. Motherhood earnings penalties. The upper graph shows the estimated child penalties in earnings for the main sample, by region of (parental) origin. See Section 3 for details. The lower graph displays the average penalty over the 10-year horizon following family formation by the GGI rank of the source country. The regression line represents a linear prediction.

Table 2. *Mothers' earnings: GGI and other source country characteristics*

	(1) Income	(2) Income	(3) Income	(4) Income	(5) Income	(6) Income
Event × Rank	490.3***			355.1***	474.6***	377.9***
	(10.95)			(21.89)	(19.01)	(22.97)
Event × GDP		746.0***		248.1***		331.2***
		(18.37)		(34.72)		(39.91)
Event × Fertility			-641.8***		-36.62	124.5***
			(17.08)		(29.65)	(34.90)
Post	✓	✓	✓	✓	✓	✓
GGI	✓			✓	✓	✓
GDP		✓		✓		✓
Fertility			✓		✓	✓
Year	✓	✓	✓	✓	✓	✓
Year × GGI	✓			✓	✓	✓
Year × GDP		✓		✓		✓
Year × Fertility			✓		✓	✓
Age	✓	✓	✓	✓	✓	✓
Age × GGI	✓			✓	✓	✓
Age × GDP		✓		✓		✓
Age × Fertility			✓		✓	✓
Event time dummies	✓	✓	✓	✓	✓	✓
Observations	835520	811001	834922	811001	834922	810403

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: This table shows the regressions following Equation 2. Post is a dummy variable for being in a time post childbirth. GGI, GDP, and Fertility are rank variables for source region characteristics. Event time dummies are dummy variables for event time, where event is birth of the first child. Age and Year are indicator variables.

5.2 The gradient in child penalty by gender inequality indices: norms or pre-existing differences?

The results presented so far are generally in line with the hypothesis that gender norms, captured by differences in source region characteristics, are related to the magnitude of the motherhood penalty. However, the observed gradient in the motherhood penalty by GGI category may also be driven by pre-existing differences, e.g. in the baseline level of earnings across individuals with varying backgrounds (Kleven, 2023). We saw in Table 1 that earnings two years before child birth differs substantially across low- and high-GGI mothers on the one hand, and between mothers of immigrant background to native mothers, on the other. To explore the role of baseline differences, we begin by performing an analysis comparing mothers of immigrant background to native mothers with similar own and partner economic status prior to having their first child, using Coarsened Exact Matching (CEM).

Matched comparisons

As discussed in the introduction, the event study approach to studying the labor market impact of parenthood builds on certain assumptions that could be questioned, in particular when making comparisons across groups and linking findings to cultural norms. It is possible that norms affect behavior already before family formation and that we therefore miss some of its impact. However, a basic idea in previous work emphasizing the role of parenthood for explaining gender gaps is that the event makes (traditional) norms salient. Systematic pre-child differences in socioeconomic status may also relate to expected effects in more mundane ways, e.g. by simply reflecting how much earnings one can lose, or affecting the bargaining position (or joint optimization) within households.

Figure 2 displays results from specifications using the CEM approach outlined in Section 3. For each GGI category, we compare the earnings profiles to a sample containing mothers of Swedish background matched on own and partner characteristics. A first impression is that when narrowing down the comparison to mothers with similar characteristics, most of the immigrant categories closely mirror their Sweden-origin counterparts. Close inspection, however, reveals that there is a tendency for low-GGI mothers to recover slightly less well compared to their observationally similar counterparts among natives, and for high-GGI mothers to outperform the comparison groups. This results in the gradient visible in the lower graph (panel B, Figure 2), where a DiD-type comparison going from the bottom to the top of the ranking implies a change of about -0.04 in the motherhood penalty. In other words, some of the association seen in the baseline results is no longer present in this comparison, although there is still a gradient. In particular, this gradient seems driven by a difference between the source-country groups with the lowest GGI values compared to the remaining groups; where the latter exhibit

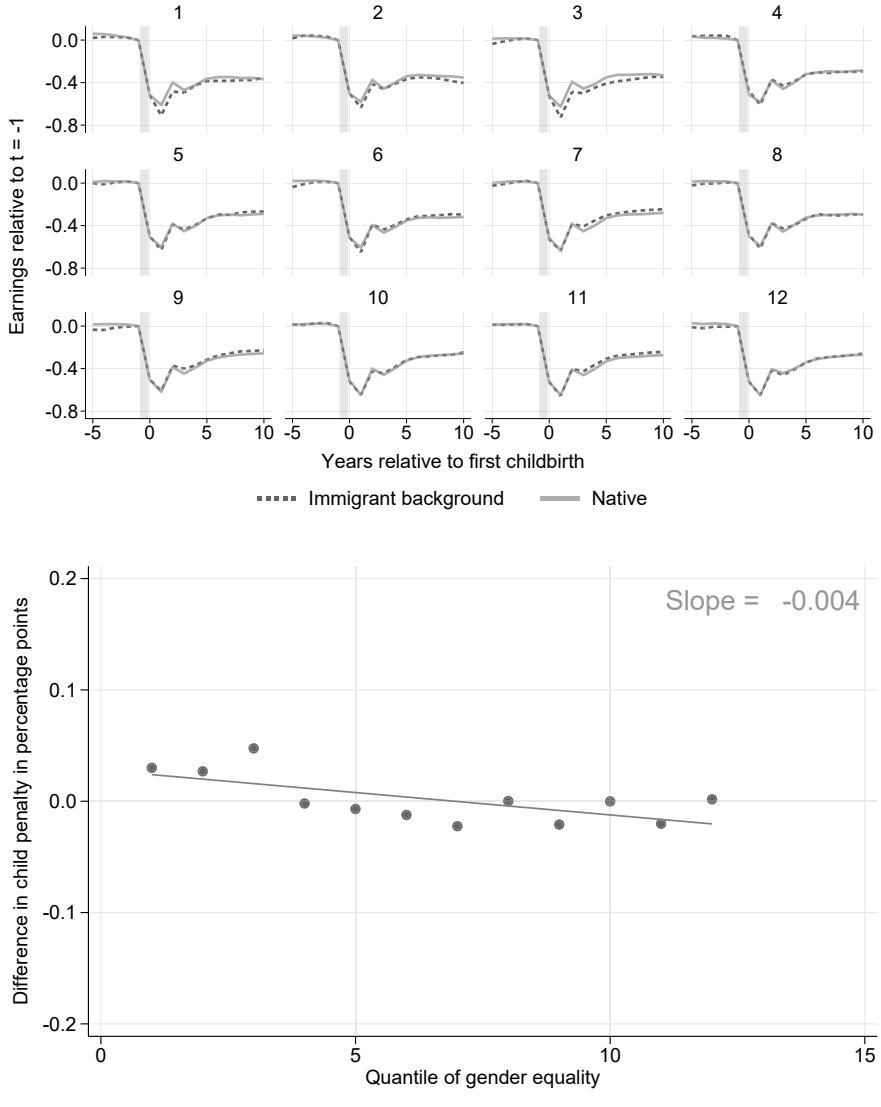

Figure 2. Comparison with natives with similar income level—Women. Outcome is earnings. The upper figure shows the results from out matched specification where we have matched each regional group to a sample of natives with similar characteristics. The lower figure shows the estimated difference in percentage points between the estimated child penalties in earnings for our main analysis sample (with an immigrant background) and the matched sample of natives. A positive difference means that the child penalty is higher for the group with an immigrant background, and a negative difference means the opposite.

no apparent gap in the motherhood penalty relative to their observationally similar native counterparts, while the former do.

Comparisons to fatherhood penalties

Sweden is a rare case of having a fatherhood penalty in earnings (Kleven et al., 2019; Sundberg, 2024), and it seems that this phenomenon is present also among fathers of immigrant origin (see panel (a) in Figure B1). Moreover, performing an analysis of fatherhood penalties using the same classifications methods as above reveals a GGI gradient in fatherhood penalties similar to what we see for mothers (see panel (b) in Figure B1). If the gradient in motherhood penalties documented above is indeed driven by gender norms, we would expect an opposite sign for the fatherhood penalty gradient with respect to GGI, i.e., more gender equal norms being associated with larger fatherhood penalties. We do, however, emphasize that there are cases where pre-trends for fathers may be a concern for the interpretation of the estimates. This could signal that the identifying assumption of exogenous timing of parenthood is not fulfilled in certain socioeconomic and demographic strata. When repeating the coarsened exact matching exercise for fatherhood, i.e., comparing with native fathers with similar income levels, the gradient of fatherhood penalties with respect to GGI is non-existing (Figure B2).

Taken together, these results suggest that the motherhood penalty across groups of varying backgrounds, but who have grown up in the same country and therefore faced similar institutions, is more a story of similarity than differences. However, some groups—those with origins in the least gender equal source countries—have somewhat larger penalties compared to natives and compared to women with origins in other parts of the world. While the gradient of fatherhood penalties with respect to source country GGI give rise to caution to applying a strictly norm-based explanation for the gradient in motherhood penalties, we note that the matched approach for fathers results in a flat gradient with respect to GGI, in contrast to the corresponding analysis for women.

5.3 Family and origin norms

Another dimension in which the impact of gender norms on child penalties can be examined is by focusing on the relative income of grandparents (Kleven, Landais, and Søgaard, 2019). Figure 3 shows the child penalties when our main analysis sample of women is disaggregated both by the region of origin (quintiles for GGI) and by their mothers' position in the earnings distribution in Sweden. The idea is to investigate whether women who grew up with less traditional (family-oriented) gender norms in their household, i.e. their mothers were more career-oriented, also have a lower child penalty. The consistent negative slope in the size of the child penalty relative to the grandmother's

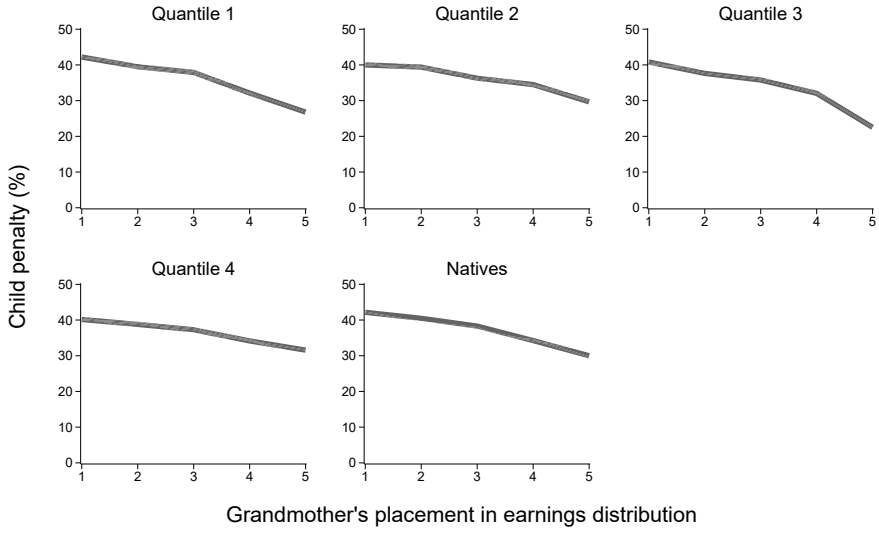

Figure 3. Grandmothers placement in earnings distribution (main sample by GGI). The figures show the total child penalty (average over the 10 years following first childbirth) for women relative to their mother's placement in the earnings distribution. The figures are separated based on the GGI in source region. The lower figure in the middle shows the same but for natives.

position in the earnings distribution suggests that this is indeed the case. This aligns with the idea that the size of child penalties for women are partly determined by within-family transmission of gender norms from parents to their children. The finding that regardless of GGI origin, the child penalties incurred by women are similarly related to how career-oriented their mothers were, adds to the broader picture of more similarity than difference in the responses to family formation.

6 Conclusions

The consequences of parenthood for women's labor market outcomes are in focus for a very active field of research. The so-called motherhood penalty has emerged as an empirical regularity across countries and socioeconomic groups. Our study contributes to this literature by investigating similarities and differences in the impact of parenthood among mothers and fathers sharing a common institutional and economic context in a comparatively gender equal society, while potentially entering adulthood with differing norms regarding gender roles through their background in different parts of the world.

We show that the main sample consisting of child migrants and children of immigrants in Sweden show pre-parental similarities not only with first gener-

ation adult migrants sharing their geographic origin, but also with the gender equality indicators seen among the populations of these countries. Thus, descriptions using our data appear to confirm previous research suggesting that there is a link between source country characteristics and migrant outcomes, potentially reflecting deeply rooted cultural norms and values. With this perspective, it is striking how similar the impact of motherhood is across groups of very different background in terms of gender equality. The earnings trajectories after first birth follow very similar profiles, and all country of origin groups experience long-term losses. One interpretation is that welfare state and labor market institutions shape behavior and limit the influence of inherited norms and values.

Nevertheless, our baseline findings suggest a negative association between the gender equality rank of the source region and the size of the child penalty in earnings. This association cannot fully be explained by other source region characteristics included in the analysis, and is seen also in employment and wages. However, matched comparisons between native and immigrant-background mothers similar in age and pre-child economic status indicates that the gradient is partly accounted for by differences in baseline earnings and characteristics across groups. But there is still a moderate gap in the motherhood penalty between women originating in the least gender equal countries relative to their native counterparts. A corresponding analysis of matched comparisons of fathers, on the other hand, reveal no gradient in the fatherhood penalty with respect to source country gender inequality.

Our interpretation is that motherhood penalties are arguably more similar than different across groups characterized by highly diverse backgrounds, captured by country of origin gender equality, and reflected in pre-child outcomes. This suggests that reforms affecting common conditions in the host context are likely to have similar impacts in groups with varying background. The findings also suggest that differential responses to parenthood related to cultural background are not a main driver of gender earnings gaps being particularly large in some immigrant communities.

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Appendix A: Variations on motherhood penalties

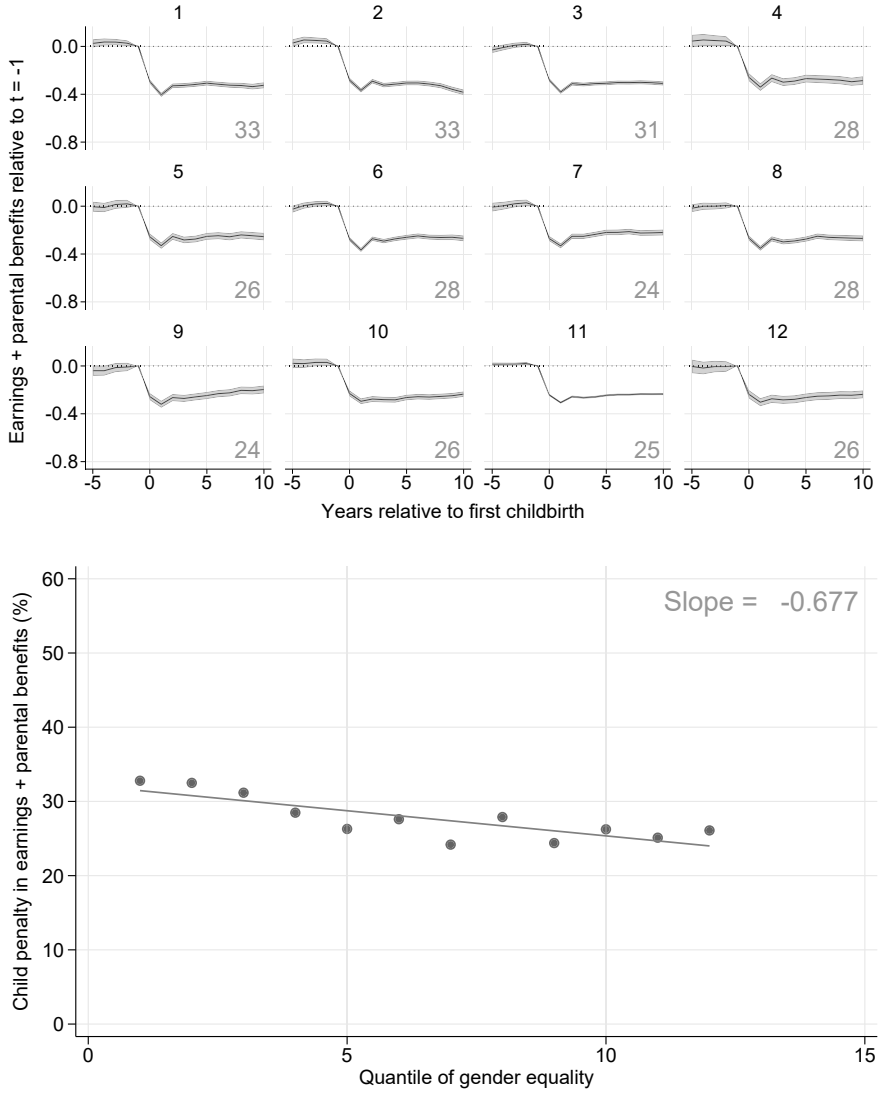


Figure A1. Motherhood penalty—Earnings + parental benefits. The upper graph shows the estimated child penalties in income for the main sample, by region of (parental) origin. See Section 3 for details. The lower graph displays the average penalty over the 10-year horizon following family formation by the GGI rank of the source country. The regression line represents a linear prediction.

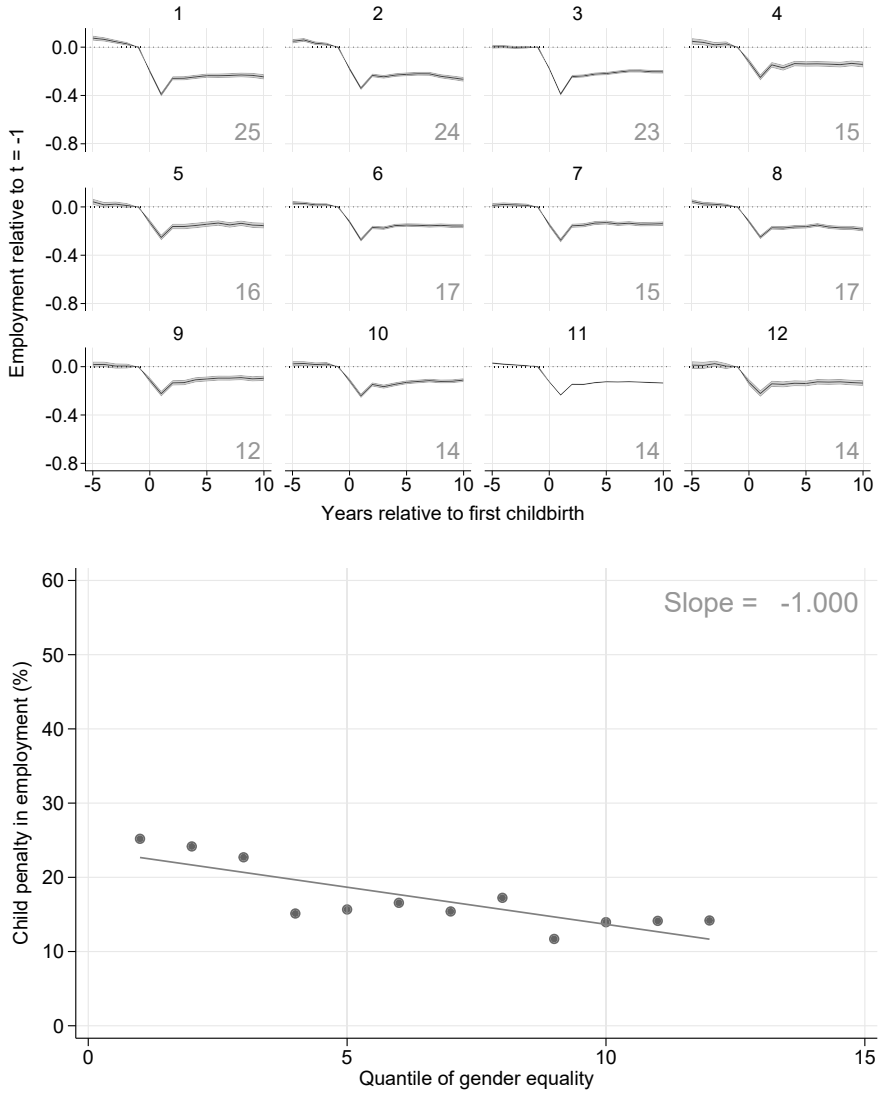


Figure A2. Motherhood penalty—Employment. The upper graph shows the estimated child penalties in employment for the main sample, by region of (parental) origin. Employment is defined as not being in the lowest two deciles in the income distribution in a given year. See Section 3 for details. The lower graph displays the average penalty over the 10-year horizon following family formation by the GGI rank of the source country. The regression line represents a linear prediction.

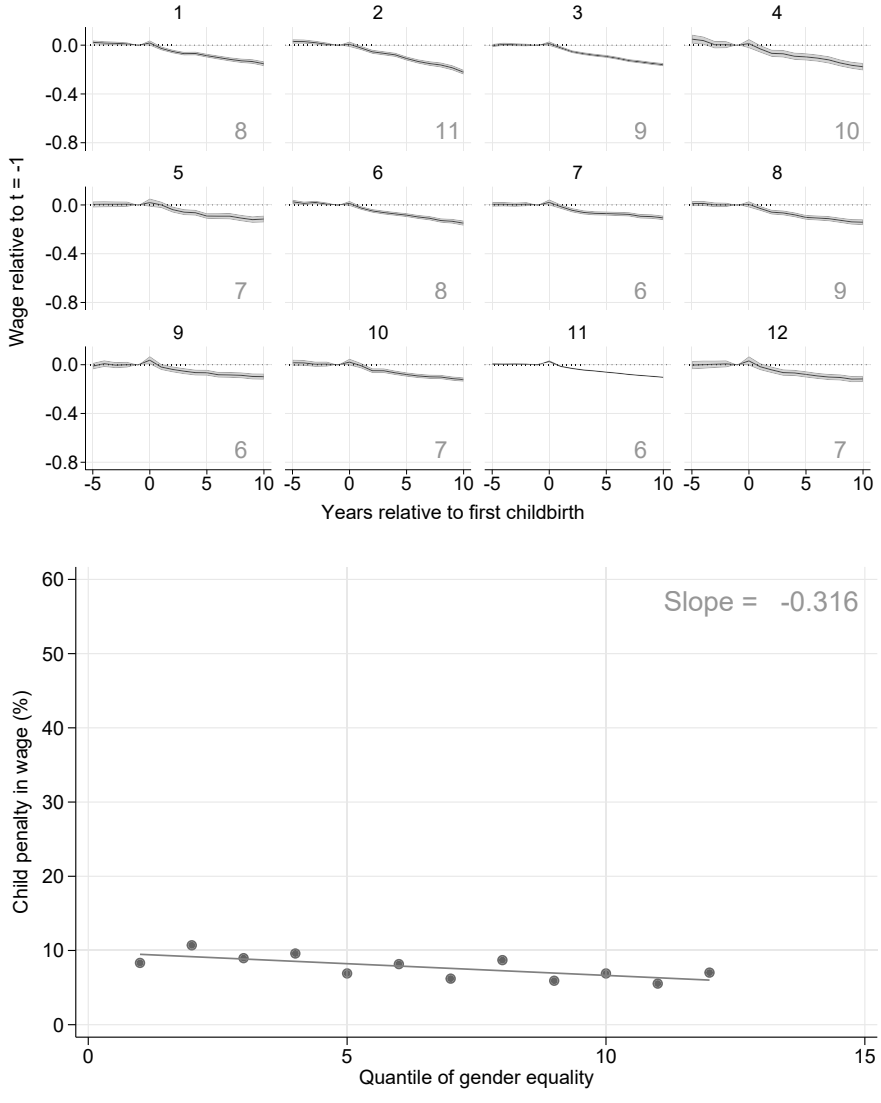


Figure A3. Motherhood penalty—Wages. The upper graph shows the estimated child penalties in contracted wages for the main sample, by region of (parental) origin. See Section 3 for details. The lower graph displays the average penalty over the 10-year horizon following family formation by the GGI rank of the source country. The regression line represents a linear prediction.

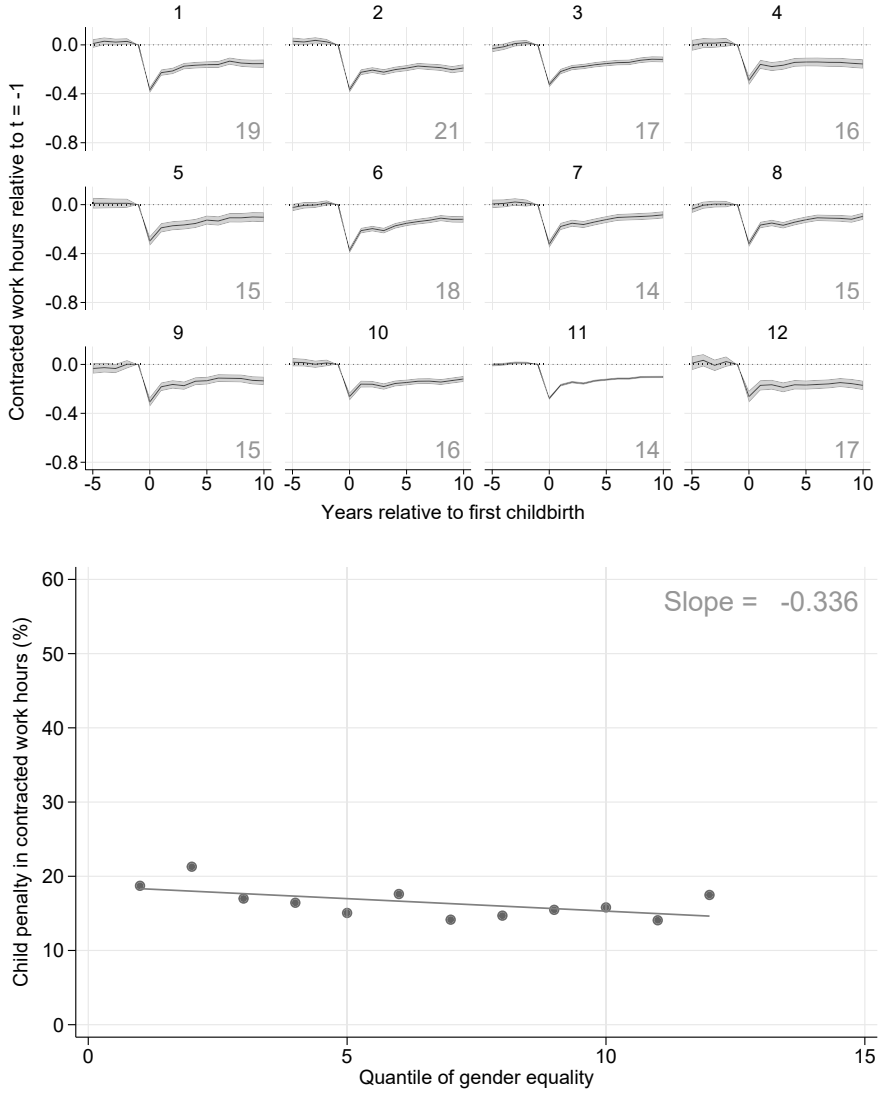


Figure A4. Motherhood penalty—Hours. The upper graph shows the estimated child penalties in contracted work hours for the main sample, by region of (parental) origin. See Section 3 for details. The lower graph displays the average penalty over the 10-year horizon following family formation by the GGI rank of the source country. The regression line represents a linear prediction.

Appendix B: Fatherhood penalties

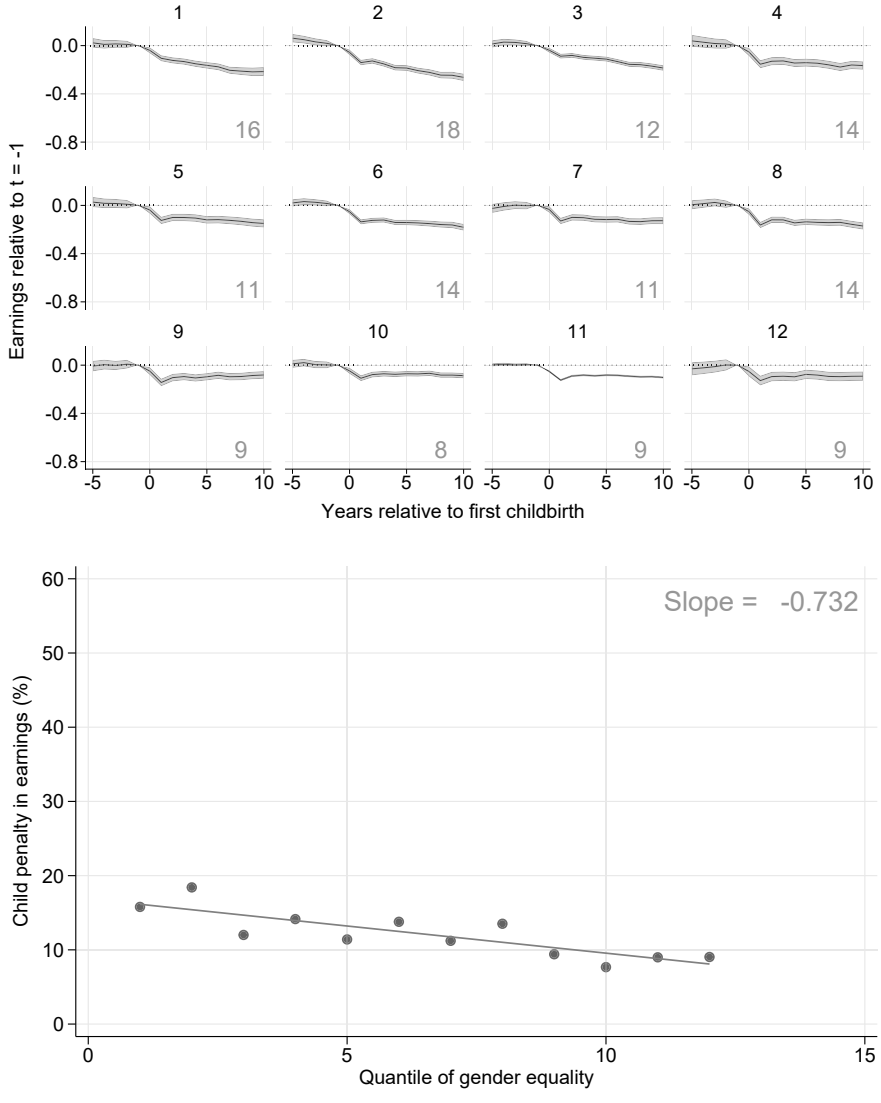


Figure B1. Fatherhood penalty—Earnings. The upper graph shows the estimated child penalties in earnings for men, by region of (parental) origin. See Section 3 for details. The lower graph displays the average penalty over the 10-year horizon following family formation by the GGI rank of the source country. The regression line represents a linear prediction.

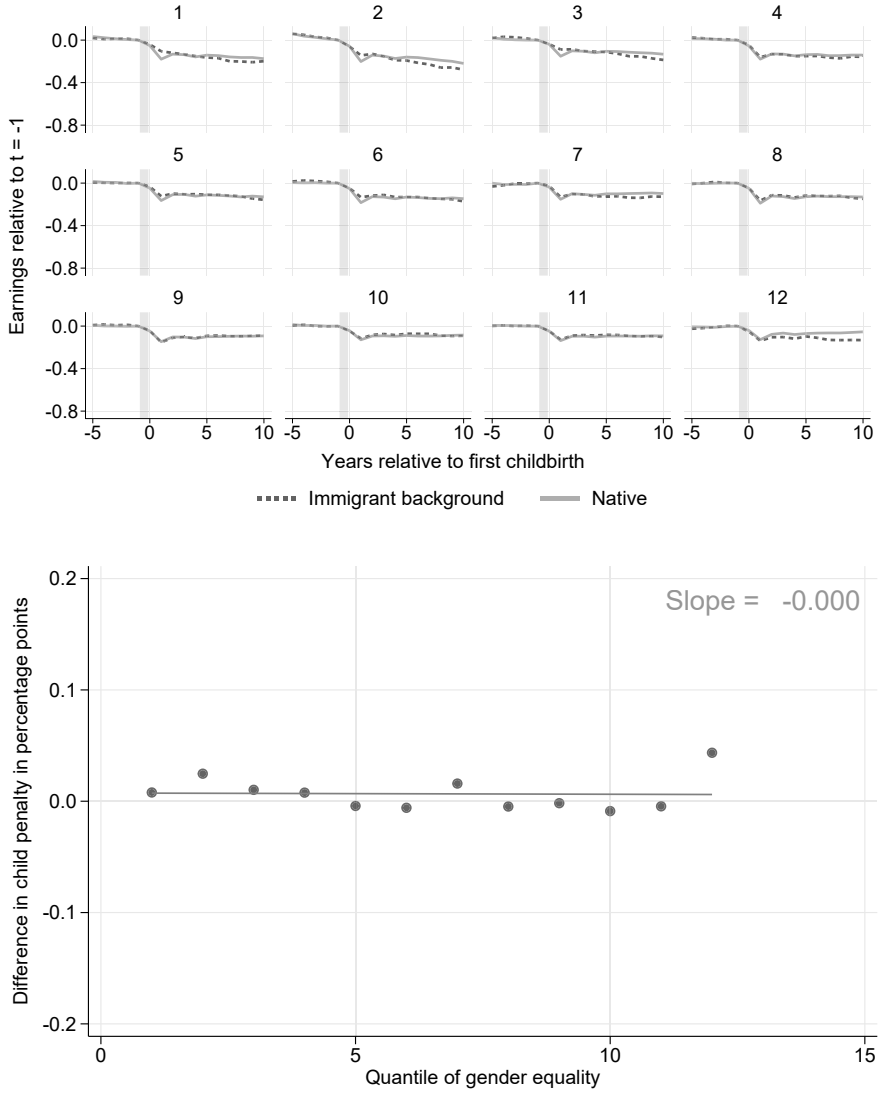
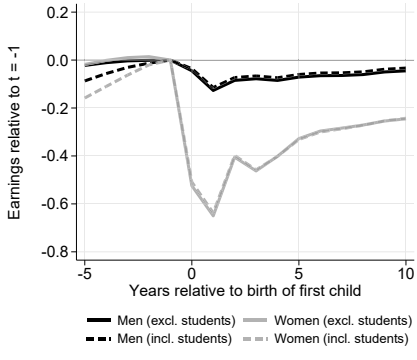


Figure B2. Comparison with natives with similar income level—Men. The upper figure shows the results from out matched specification where we have matched each regional group to a sample of natives with similar characteristics. The lower figure shows the estimated difference in percentage points between the estimated child penalties in earnings for our main analysis sample (with an immigrant background) and the matched sample of natives. A positive difference means that the child penalty is higher for the group with an immigrant background, and a negative difference means the opposite.

Appendix C: Additional figures and tables

(a) Child penalties



(b) Counterfactual earnings

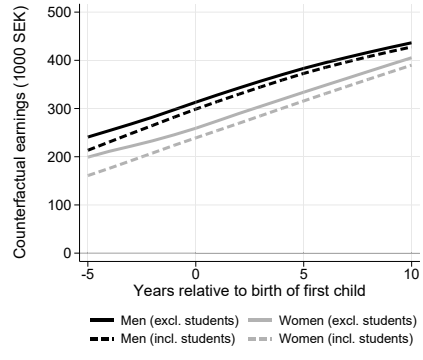


Figure C1. Student restriction. The figures shows a comparison when running the main Equations 1a and 1b when including our excluding students from the population. The left figure shows the estimated child penalties and the right figure shows the estimated counterfactual earnings.

Table C1. *Descriptive Statistics—Women (main sample)*

GGI Quantile	Years of Education	Quantile of Income	Number of Children	Single Household
1	11.97	33.91	2.288	0.223
2	12.31	38.20	2.219	0.238
3	11.82	37.95	2.277	0.201
4	12.59	50.13	1.981	0.238
5	12.43	45.52	2.020	0.195
6	12.42	45.50	2.024	0.229
7	11.83	40.81	2.009	0.316
8	12.82	47.57	1.992	0.253
9	12.57	51.13	1.949	0.230
10	11.86	49.26	2.016	0.222
11	11.67	47.16	1.999	0.246
12	11.76	47.44	2.017	0.269
Total	11.92	45.51	2.044	0.240

Notes: The table includes foreign born individuals with an age at immigration ≤ 10 and second generation immigrants (both parents foreign born, region of ancestry is source country of the mother). The country groups are listed in ascending order according to a weighted Global Gender Gap Index (2020). See Table C7 for a list of countries included in each quantile. Quantile of Income is the income percentile two years prior to first childbirth. Number of Children is the total number of children within 8 years from mother's first child. Single Household is equal to one when the parent is registered as a "single household with a child aged ≤ 18," 8 years from the first child's birth.

Table C2. *Descriptive Statistics—Men (main sample)*

GGI Quantile	Years of Education	Quantile of Income	Number of Children	Single Household
1	11.57	42.10	2.280	0.287
2	11.81	42.01	2.234	0.289
3	11.29	39.92	2.279	0.212
4	12.41	56.50	1.986	0.247
5	11.99	49.82	2.028	0.211
6	12.15	53.47	2.036	0.235
7	11.56	44.09	2.034	0.371
8	12.58	53.19	1.996	0.280
9	12.30	58.09	1.995	0.231
10	11.74	59.37	1.978	0.218
11	11.44	55.96	1.956	0.277
12	11.69	55.99	1.989	0.269
Total	11.65	53.20	2.019	0.264

Notes: See notes for Table C1.

Table C3. *Descriptive Statistics—Women (1st generation immigrants at age 45)*

GGI Quantile	Years of Education	Quantile of Income	Number of Children	Single Household
1	11.75	25.68	1.851	0.176
2	12.33	37.02	2.079	0.257
3	10.56	30.55	2.666	0.299
4	13.31	40.01	1.644	0.220
5	11.89	38.29	1.736	0.253
6	12.19	45.04	1.643	0.232
7	12.62	45.51	1.896	0.331
8	13.24	45.21	1.514	0.256
9	13.30	44.34	1.500	0.210
10	14.05	46.62	1.667	0.178
11	13.02	55.03	2.075	0.277
12	12.94	51.27	2.100	0.251
Total	12.44	40.10	1.852	0.239

Notes: The table includes foreign born individuals at age 45. The country groups are listed in ascending order according to a weighted Global Gender Gap Index (2020). See Table C7 for a list of countries included in each quantile. Quantile of Income is the income percentile two years prior to first childbirth. Number of Children is the total number of children within 8 years from mother's first child. Single Household is equal to one when the parent is registered as a "single household with a child aged $\leq$ 18," 8 years from the birth of mother's first child.

Table C4. *Descriptive Statistics—Men (1st generation immigrants at age 45)*

GGI Quantile	Years of Education	Quantile of Income	Number of Children	Single Household
1	11.79	29.33	2.126	0.147
2	12.15	39.28	2.220	0.227
3	11.18	36.33	2.833	0.270
4	12.97	46.48	1.943	0.218
5	11.52	44.93	1.916	0.242
6	12.19	52.47	1.858	0.202
7	12.12	51.95	2.213	0.367
8	12.88	50.01	1.590	0.231
9	13.17	54.32	1.689	0.225
10	13.82	53.90	1.658	0.181
11	11.94	57.55	2.004	0.297
12	12.74	56.94	1.940	0.239
Total	12.22	43.96	2.061	0.222

Notes: See notes for Table C3

Table C5. *Descriptive statistics on pre-parenthood characteristics for matched sample*

	Immigrant background								Native		
	Matched					Unmatched			Matched		
GGI Quantile	Number	Fraction	Income	Education	Age	Income	Education	Age	Income	Education	Age
1	11958	0.824	248670	12.80	25.62	248444	12.91	25.76	250053	12.86	25.80
2	14276	0.814	279939	13.26	26.80	275023	13.34	27.00	278020	13.30	26.88
3	19227	0.830	223447	12.37	25.63	223026	12.45	25.85	224300	12.45	25.70
4	5286	0.820	276788	13.21	27.59	270819	13.32	27.94	271572	13.21	27.56
5	7731	0.819	252896	12.96	27.13	249089	13.07	27.47	253580	13.01	27.09
6	11459	0.854	264306	13.16	26.11	262305	13.27	26.39	263592	13.16	26.22
7	9704	0.857	228216	12.41	26.18	227767	12.52	26.60	230091	12.52	26.11
8	10815	0.821	279895	13.38	27.28	274202	13.48	27.64	278519	13.39	27.26
9	10787	0.833	227783	12.45	27.09	224916	12.63	27.70	229235	12.55	27.03
10	3828	0.813	238003	12.52	28.17	235746	12.70	28.88	237582	12.53	28.01
11	61846	0.858	216058	12.08	26.50	215493	12.19	26.97	216809	12.18	26.44
12	4040	0.853	211392	12.08	26.53	211452	12.24	27.03	213017	12.20	26.47
Total	29739	0.840	237132	12.54	26.51	235690	12.66	26.89	237398	12.62	26.50

Notes: This table shows pre-parenthood characteristics for the matched sample of individuals. The first column shows the number of observations. Note that individuals are matched in the pre-parenthood period and are matched on all five years (if the individual is included in all years in the pre-period.) The fraction shows the percentage of individuals that are successfully matched to a native. Income is annual earnings given in SEK (in 2018 price levels). Education is given as years of education. Regional groups are ranked according to GGI.

Table C6. *Correlations: Mothers, 1gen and countries of origin*

	Educ. (2nd gen.)	Earnings. (2nd gen.)	No. Children (2nd gen.)	Educ. (1st gen.)	Earnings. (1st gen.)	No. Children (1st gen.)	GGI rank	GDP rank	Fertility rank
Educ. (2nd gen.)	1.00								
Earnings. (2nd gen.)	0.58***	1.00							
No. Children (2nd gen.)	-0.01	-0.30**	1.00						
Educ. (1st gen.)	0.32**	0.50***	-0.29**	1.00					
Earnings. (1st gen.)	-0.01	0.30**	-0.33**	0.58***	1.00				
No. Children (1st gen.)	-0.33**	-0.51***	0.37***	-0.66***	-0.43***	1.00			
GGI rank	-0.02	0.35**	-0.21	0.55***	0.75***	-0.42***	1.00		
GDP rank	0.08	0.38***	-0.17	0.61***	0.49***	-0.38***	0.61***	1.00	
Fertility rank	-0.07	-0.43***	0.29**	-0.61***	-0.55***	0.52***	-0.58***	-0.86***	1.00

Notes: This table shows the correlations between background and source country characteristics for 1st and 2nd generation immigrants.

Table C7. *Source country characteristics*

	GGI rank	GGI quantile	GGI value	GDP per capita	Relative FLFP	Fertility rate
Iraq	1	1	0.530	10565	0.104	5.882
Pakistan	2	1	0.564	4690	0.166	6.164
Syrian Arab Republic	3	1	0.567	.	0.284	5.309
Congo	4	2	0.578	1098	0.998	6.746
Iran	5	2	0.584	12389	0.121	4.691
Afghanistan*	6	2	0.587	2065	0.192	7.466
State of Palestine*	7	2	0.593	6245	0.167	6.718
Saudi Arabia	8	2	0.599	46962	0.184	5.911
Lebanon	9	2	0.599	14552	0.267	3.372
Somalia*	10	3	0.603	867	0.323	7.398
Morocco	11	3	0.605	7537	0.290	4.047
Eritrea*	12	3	0.609	.	0.811	6.496
Sudan*	13	3	0.617	4186	0.306	6.152
Jordan	14	3	0.623	10071	0.156	5.521
Gambia	15	3	0.628	2223	0.659	6.096
Egypt	16	3	0.629	11763	0.293	4.580
Rest of Northern Africa	17	3	0.634	11723	0.167	4.740
Nigeria	18	3	0.635	5135	0.824	6.490
Türkiye	19	3	0.635	28199	0.421	3.107
Tunisia	20	4	0.644	10756	0.296	3.476
Japan	21	4	0.652	41380	0.648	1.540
Rest of Western Asia 2	22	4	0.660	48667	0.488	3.880
Rest of Western Africa	23	4	0.666	3840	0.801	6.179
Uzbekistan*	24	4	0.666	7014	0.658	4.072
India	25	4	0.668	6714	0.357	4.045
Kenya	26	4	0.671	4330	0.908	6.066
Rest of Middle Africa	27	4	0.671	4852	0.905	6.384
Republic of Korea	28	4	0.672	42719	0.641	1.570
China	29	4	0.676	16092	0.864	2.309
Hungary	30	4	0.677	32554	0.747	1.870
Rest of Southern Asia	31	5	0.677	4693	0.869	5.208
Sri Lanka	32	5	0.680	13070	0.574	2.483
Rest of Western Asia 1	33	5	0.690	14248	0.727	2.556
Brazil	34	5	0.691	14764	0.502	2.902
Rest of Central Asia	35	5	0.695	17652	0.747	3.352
Rest of South-East Asia	36	5	0.695	22936	0.641	3.561
Viet Nam	37	5	0.700	8041	0.890	3.553
Greece	38	5	0.701	29723	0.537	1.390
Rest of Western Europe	39	6	0.701	47464	0.609	2.171
Ethiopia	40	6	0.705	2221	0.740	7.246
Russian Federation	42	6	0.706	27211	0.778	1.892

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	GGI rank	GGI quantile	GGI value	GDP per capita	Relative FLFP	Fertility rate
Czechia	43	6	0.706	40696	0.734	1.900
Italy	44	6	0.707	42675	0.528	1.330
Thailand	45	6	0.708	18453	0.788	2.113
North Macedonia	46	6	0.711	16600	0.637	2.206
Bosnia and Herzegovina	47	6	0.712	14897	0.592	1.772
Rest of Eastern Asia*	48	7	0.713	59586	0.601	1.292
Peru	49	7	0.714	12854	0.586	3.912
Uganda	50	7	0.717	2187	0.807	7.091
Slovakia	51	7	0.718	31871	0.817	2.090
Croatia	52	7	0.720	28754	0.682	1.630
Ukraine	53	7	0.721	12809	0.811	1.844
Chile	54	7	0.723	24968	0.426	2.579
Rest of South America 1	55	8	0.724	11637	0.568	3.615
Romania	56	8	0.724	29858	0.822	1.830
US	57	8	0.724	62631	0.746	2.081
Bangladesh	58	8	0.726	4754	0.280	4.495
Central America	59	8	0.727	12567	0.447	4.052
Bulgaria	60	8	0.727	23192	0.881	1.820
Rest of Eastern Africa	61	8	0.730	3178	0.917	6.411
Bolivia	62	8	0.734	8724	0.689	4.890
Poland	63	8	0.736	33121	0.766	2.060
Netherlands	64	9	0.736	56629	0.611	1.620
Serbia	65	9	0.736	18292	0.713	.
Caribbean and Bermuda	66	9	0.738	16919	0.583	2.352
Rest of South America 2	67	9	0.740	22219	0.596	2.828
Slovenia	68	9	0.743	38906	0.793	1.460
Portugal	69	9	0.744	34880	0.663	1.560
Austria	70	9	0.744	55833	0.621	1.460
Oceania	71	9	0.744	46710	0.694	2.039
Lithuania	72	9	0.745	37063	0.761	2.030
Rest of Eastern Europe	73	9	0.748	18009	0.808	2.015
Belgium	74	9	0.750	51743	0.602	1.620
Estonia	75	9	0.751	36830	0.747	2.050
Colombia	76	9	0.758	14585	0.605	3.082
UK	77	9	0.767	46406	0.702	1.830
Albania	78	9	0.769	13671	0.736	2.978
Canada	79	9	0.772	49007	0.762	1.830
Southern Africa	80	9	0.775	12350	0.588	4.061
Switzerland	81	9	0.779	70920	0.711	1.580
Philippines	82	9	0.781	8915	0.613	4.320
France	83	10	0.781	45834	0.707	1.770
Denmark	84	10	0.782	57678	0.827	1.670

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	GGI rank	GGI quantile	GGI value	GDP per capita	Relative FLFP	Fertility rate
Latvia	85	10	0.785	30859	0.750	2.020
Germany	86	10	0.787	53639	0.623	1.450
Spain	87	11	0.795	40806	0.501	1.360
Ireland	88	11	0.798	87786	0.513	2.110
Finland	90	11	0.832	48689	0.820	1.780
Norway	91	12	0.842	64453	0.784	1.930
Iceland	92	12	0.877	56914	0.838	2.300

Notes: The table shows the source region characteristics for the regions in our main analysis sample. * means that the value on GGI is imputed (see Section 2 for imputation details). Regions are ranked in ascending order according to their Gender Gap Index in 2020 (World Economic Form). Relative FLFP is the female to male labor force participation rate in 1990 (World Bank). GDP per capita in 1990 (World Bank), and fertility rate in 1990 (World Bank).

Essay III. Migration Inflow and School Performance of Incumbents

Co-authored with Demid Getik and Anna Sjögren

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1 Introduction

Growing migration and refugee flows have consequences for receiving host community labor markets and schools.[1] Immigration is often associated with poor school results and increasing school segregation as migrant students are disproportionately accommodated in disadvantaged schools and because the performance of migrant students generally lags that of their native peers (Card, 2009). Yet, causal evidence on the effects of exposure to migrants on incumbent students is inconclusive, with results ranging from negative (Gould, Lavy, and Daniele Paserman, 2009; Jensen and Rasmussen, 2011; Ballatore, Fort, and Ichino, 2018) to limited (Geay, McNally, and Telhaj, 2013; Ohinata and Van Ours, 2013; Brandén, Birkelund, and Szulkin, 2019; Figlio and Özek, 2019; Bossavie, 2020; Green and Iversen, 2022; Morales, 2022) to positive effects (Tumen, 2021; Figlio et al., 2023).

Exposure to migrants and newly arrived refugees potentially affect students because the student composition of schools and peer effects matter for student outcomes (Coleman, 1988; Hoxby, 2000), identity formation (Akerlof and Kranton, 2002) and teacher turnover (Karbownik, 2020). Competition for resources, classroom disruptions, and reorientation of teaching activity are possible reasons (Lazear, 2001; Card, 2009; Sacerdote, 2011), as are impacts on students' rank in the classroom (Delaney and Devereux, 2021; Dadgar, 2022; Delaney and Devereux, 2022) and effects of relative grading. Changes to the student composition due to migration flows may also affect school choices and cause families to change neighborhoods or schools, which further alters the student composition of receiving schools (Clotfelter, 1976, 2001; Aldén, Hammarstedt, and Neuman, 2015; Böhlmark, Holmlund, and Lindahl, 2016; Böhlmark and Willén, 2020). Depending on the initial student composition, school responses, and the composition of migrants, the net effect of these different channels may well be negative, neutral, or positive. Uncovering which mechanisms are present is thus important for the development of appropriate policy responses.

In this paper, we study the effect of exposure to recent migrants on incumbent students' school performance.[2] We focus on Swedish compulsory schools between academic years 2008/2009 and 2021/2022, a period characterized by an increasingly high migrant and refugee exposure. We follow the strategy of Brandén, Birkelund, and Szulkin (2019) and Figlio et al. (2023) and exploit i) within-school variation in migrant exposure across cohorts and over time and ii) within-sibling variation in exposure to migrants to account for non-random sorting of both migrant and native students to schools. We es-

[1] See Borjas (2014), Dustmann, Schönberg, and Stuhler (2016), and Brell, Dustmann, and Preston (2020) for reviews of the literature on the impact of immigrants and refugees on labor market outcomes.

[2] Incumbent students are students born in Sweden. Recent migrants are students born abroad and who were granted a residence permit within the last four years or asylum-seeking students in the asylum process who have not yet received a residence permit.

timate the effects of both contemporaneous migrant exposure and cumulative migrant exposure over students' school history on incumbents' national test scores.[3] To account for within-family selection, we study school changes directly and examine if families selectively change schools for their children or place younger siblings in a different school in response to the migrant influx. We then instrument younger sibling exposure with the exposure that it would have experienced had families put siblings in the same school. Moreover, as an alternative strategy to estimate the effects of refugee exposure on incumbent students, we use the substantial variation in how schools were affected by the 2015 refugee crisis in an event study approach. This strategy also captures broader school-level effects.

Our analysis is based on administrative student registers with information on school and class assignments throughout compulsory school grades (0 to 9) for academic years 2008/2009–2021/2022 for the universe of Swedish compulsory school students, including asylum seekers. School performance is measured by results on national tests in grades 3 (only Swedish and mathematics) and grades 6 and 9 (Swedish, mathematics and English). Test score outcomes are complemented with teacher assessments/grades in grades 6 and 9. We link student data to school-level data on teachers and to population and tax registers containing information on family links, birth records, migration background, and parental education and earnings.

The recent Swedish experience offers an excellent opportunity to study how exposure to recent immigrants affects incumbent students. The average share of foreign-born students in Swedish schools almost doubled from 7 to 13 percent from 2008 to 2019, which is high compared to the US and other European countries. The fraction of recent immigrant students rose rapidly during the Syrian conflict and reached a peak of 6.4 percent of all students in 2018, just after the 2015 European refugee crisis. This can be compared to the 2018 European average share of foreign-born students, which was 5 to 6 percent. In 2015 alone, Sweden received some 70,000 refugee minors, half of whom were unaccompanied, of mainly Afghan origin (Bunar, 2017). In addition, the distribution of migrants across schools is very uneven. While many schools were unaffected by the rapid influx of asylum seekers, other schools, typically in rural areas, saw their student body increase dramatically.

Our results suggest that the negative association between migration and school performance stems from the significant negative sorting of migrants and incumbent children to exposed schools. Once we account for this sorting, we find that both contemporaneous and cumulative exposure have small positive effects on native students' performance. At the same time, we find the opposite but insignificant effects for students from an immigrant background.

[3]Contemporaneous exposure refers to the fraction of recent migrant students in the student's grade and school in a given year, while cumulative exposure averages the student's recent migrant exposure in each school year up until the present.

These positive results are driven by the effect on male students' Swedish and English test scores. A closer examination of effects across the performance distribution shows that while boys in the middle and top of the distribution benefit, girls in the lower end of the distribution benefit.

We also explore the effects of exposure to different types of migrants. It seems that the positive effects of recent migrant exposure are driven by exposure to migrants from non-Western and low-income countries as well as asylum seekers. We further find that school performance of those with an immigrant background is suggestively negatively affected by cumulative exposure to non-Western migrants and migrants from low-income countries. In contrast, exposure to recent migrants from high-income countries has no impact on test scores.

In an attempt to explore mechanisms behind the overall positive effects and for the differences between native and immigrant background students, we find evidence of reduced classroom sizes in response to high recent migrant exposure. However, this does not fully account for the positive effects. We also find that the fraction of students who participate in home language classes increases among students of native background but decreases among students with an immigrant background. The analysis of the 2015 refugee crisis corroborates our finding of a modest positive effect on test scores of being exposed to recent migrants for native students. We also find evidence that while class sizes initially increased, schools responded to the migration inflow by reducing class sizes. Taken together, our results suggest that increased resources in response to migrant inflow have a role in explaining the positive results.

We contribute to two main strands of empirical literature. The first one studies the effect of different facets of peer composition in school on educational outcomes more broadly (e.g., Brenøe and Lundberg, 2018; Bietenbeck, 2020; Balestra, Eugster, and Liebert, 2022). The other strand focuses more specifically on the host country effect of migration, and in particular on the effects of exposure to migrant peers on the school performance of incumbent students (e.g., Card, 2009; Borjas, 2014; Figlio and Özek, 2019; Andersson, Berg, and Dahlberg, 2021; Green and Iversen, 2022). In what is perhaps the contextually closest study, Brandén, Birkelund, and Szulkin (2019) find limited effects of exposure to migrants in Sweden on the local students' compulsory school leaving grades for the period 1998–2012. We are able to extend their results by considering a later period that involves a more sudden and intense exposure spurred by the 2015 refugee crisis. In addition to that, we use standardized national test results for grades 3, 6, and 9, as opposed to more locally influenced grades given by the teacher in the final year of compulsory school. Access to student registers also allows us to measure both contemporaneous and cumulative exposure to recent migrants (including asylum seekers) over incumbent students' complete school history. Our findings broadly corroborate their results, showing limited effects on individual students' grades, while we also detect some positive effects on test scores. A recent study, very close to ours

methodologically, but in a US context, shows positive effects of exposure to foreign-born students (Figlio et al., 2023). Using detailed data on both the incumbents and recent migrants, we broadly corroborate their findings, but in the European context and a context of refugee migration. Our event study of the 2015 refugee crisis is a further contribution to the literature focusing on refugee migration, e.g., Figlio and Özek (2019) and Özek (2021). We are able to show that the positive effects on incumbent students also extend to an event that puts substantial pressure on receiving communities.

2 Evidence on the impact of immigration on school performance

There is a general concern in the debate that immigration is associated with poor school outcomes and school segregation (Card, 2009). Still, there is yet no consensus in the empirical literature on whether these adverse effects are causal or correlational. There is, however, a growing literature analyzing the causal effects of immigrant student exposure on the academic outcomes of native students. These studies use either quasi-random year-to-year variation across cohorts within schools or large sudden inflows, and sometimes in combination with sibling comparisons.

In the US context, Özek (2021) studies the influx of Puerto Rican migrants into public schools in Florida after Hurricane Maria in 2017. The study focuses on internal rather than cross-border migration and finds significant negative effects in the same year as the influx but no effects in the year after the influx. An important explanatory factor for their findings is the responsive compensatory allocation of resources within schools following the influx of migrants. Similarly focusing on the consequences of a natural disaster on host communities, Figlio and Özek (2019) examines the influx of low-income, non-English speaking Haitian migrants into Florida public schools following the 2010 earthquake. They find no or modest positive effects from the influx of migrants.

Morales (2022) examines the concentration of refugee children in schools in Georgia, USA, between 2008 and 2017 and finds no (English test scores) or modest positive (mathematics test scores) effects on the academic achievement of native students. Suggestive evidence supports the hypothesis that increased resources, such as higher teacher-per-student ratios and reduced class sizes, drive the positive effects on mathematics scores. Werf (2021) focuses on historical data using the influx of refugees from Southeast Asia to the US at the end of the Vietnam War and finds no impact on native students' test scores in mathematics and reading. Figlio et al. (2023) focus on the concentration of immigrant students in Florida schools rather than a sudden and unexpected influx of refugees. They study not only the contemporaneous but also the cumulative exposure to migrants over native students' school history, using the

longitudinal dimension of their data. Comparing the educational outcomes in mathematics and reading of siblings with different school- and cohort-specific exposures, they find a positive impact of immigrant exposure on native test scores.

There are a few of studies in a Scandinavian context. Brandén, Birkelund, and Szulkin (2019) examine the immigrant composition in schools in Sweden between 1998 and 2012 (before the large influx of refugees during the European migrant crisis) and find no effects on compulsory school grades but a small negative impact on the level of eligibility for upper secondary school using administrative data and a within school and sibling comparison. Using administrative Danish data and PISA test scores in the years 2000 and 2005, Jensen and Rasmussen (2011) find a negative impact of immigrant concentration in schools on native maths and reading scores, although the effect on reading is insignificant in the more demanding specification. Hassan et al. (2023) examines the period 2007–2015 in Denmark and finds no significant effects of refugee exposure on native students test scores. Green and Iversen (2022) use administrative school data in Norway for the 2007–2015 period and find zero effects on native 5^{th} graders' English and Norwegian test scores and negative impact on mathematics using within-sibling and within-school variation in exposure to refugees.

Outside the US and Scandinavia, Gould, Lavy, and Daniele Paserman (2009) focuses on an influx of refugees from the former Soviet Union to Israel in the early 1990s and finds adverse effects on the passing rates of the high school matriculation exam among incumbent students. Schneeweis (2015) focus on an earlier period (1980–2001) in Austrian primary schools and finds that immigrant concentration does not impact the likelihood of native students repeating a grade, nor their likelihood of continuing on an academic track. However, immigrant students (especially co-ethnic students) were negatively affected by immigrant concentration. Geay, McNally, and Telhaj (2013) rule out negative spillover effects from non-native English speakers in the UK. Ohinata and Van Ours (2013) and Bossavie (2020) study the Dutch context and find no or small negative effects of immigrant concentration on test scores. The small negative impact on verbal scores found in Bossavie (2020) diminishes with years since immigration. Ballatore, Fort, and Ichino (2018) find adverse effects of immigrant concentration on native test scores in Italy, and the impact is more negative when considering first-generation immigrants. Frattini and Meschi (2019) also find small negative effects of immigrant concentration in schools on natives' test scores in vocational schools in Italy, and the effects are particularly large for low-achieving native students. In a recent study of the 2015 refugee crisis, Tumen (2021) examines the influx of mainly Syrian refugees to Turkey and finds positive effects on PISA test scores in maths, science, and reading.

In sum, the impact of immigrant concentration and refugee influx in schools on the academic achievements of incumbent students varies from negative to

zero to positive, depending on methods, period, age group, outcome measures, and other contextual factors. There is tentative evidence of heterogeneous effects, and that some students may gain while others—in particular disadvantaged students—lose, and also that school responses and resources likely matter. Thus, it is not settled whether, when, and how exposure to immigrant or refugee students helps or hinders the performance of native students. These ambiguous results underscore the importance of carefully delineating both the institutional setting and uncovering potential mechanisms behind the effects, especially if we want to develop policies to improve the accommodation of migrant and refugee students in schools.

3 The Swedish context: Migration and institutional setting

We set the scene for the present study by first showing how the presence of migrant students has evolved in Swedish schools (and what this has implied for the exposure to migrants of different groups of students) and, second, by presenting the Swedish school system.

3.1 Refugee immigration in Sweden

Immigration, in a European and Swedish context, is signified by refugee migration, which contrasts the US setting, where refugee immigration is of less importance. The inflow of migrants during the refugee crisis in the mid-2010s was exceptionally high from a historical perspective, and about one million refugees (mainly from Syria) came to Europe within a couple of years. However, in the European context, only a few papers (e.g., Tumen, 2019; Green and Iversen, 2022) focus on this setting. Moreover, even within Europe, Sweden is somewhat of an outlier. Sweden had until 2016 (when migration policy changed radically) the highest per capita refugee inflow in Europe. Compared to other European countries, Sweden thus has a relatively high fraction of foreign-born (20 percent in 2020) and the highest number of refugees per capita (9 percent in 2020). In the past two decades, refugees from the Middle East and Northeast Africa have constituted the majority of the immigrant inflow.

In Figure 1, we show how the share of students with migrant backgrounds as a fraction of the total student population has evolved in Swedish compulsory schools since the academic year 2008/2009. Over that period, the fraction of native students with at least one parent born in Sweden has declined from approximately 82 percent to around 72 percent. There is a steady increase in the fraction of second-generation immigrant students from about 10 percent in 2008/2009 to some 13 percent in 2021/2022. At the same time, the total

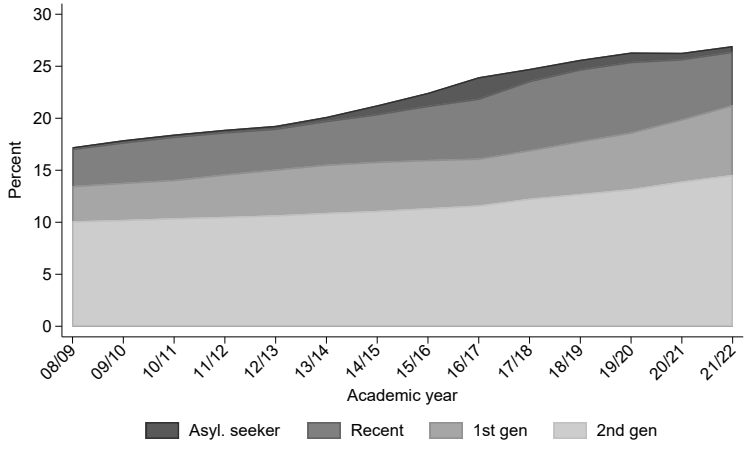
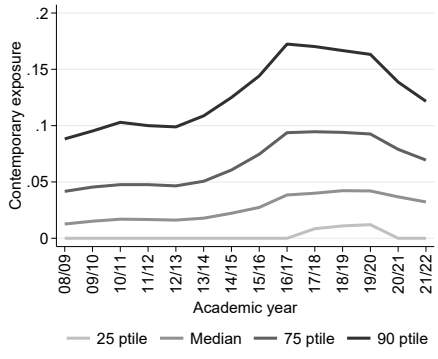


Figure 1. The Stock of compulsory school students by migration background. The figure shows the share of foreign-born and foreign background (two foreign born parents) students for the 2008–2022 period, by migrant status. "Asylum seekers" are non-resident students with asylum seeking status, "Recent" includes foreign-born students with at most four years of residency, "1st gen" are foreign-born students with more than four years of Swedish residency, and "2nd gen" are students born in Sweden to two foreign-born parents.

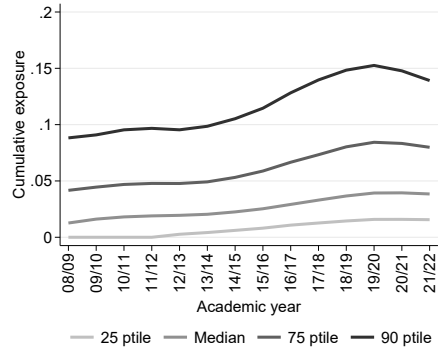
group of first-generation immigrant students (comprised of foreign-born students with more than four years of residency, recently arrived with at most four years of residency, and asylum seekers) doubled from about 6 percent to over 12 percent. Although asylum seekers make up a small share of the overall student population, there was a clear peak during the crisis years around 2015–2017. As these students become residents, the group of recent migrants grows. Over time, a growing fraction of first-generation immigrant students also accumulate more than four years of residency.

These average numbers hide significant heterogeneity in the share of migrant students across schools and also by incumbent students' migration background. The top panel of Figure 2 shows the evolution of the distribution of recent migrants by a) contemporaneous and b) cumulative exposure. While 25 percent of students remain largely unexposed to immigrant students throughout the study period, median exposure rises by a few percentage points from 1 to 4 percent during the refugee crisis. At the other end, the 75th and 90th percentiles of the distribution, migrant exposure rises from just below 5 and 10 percent, respectively to just below 10 percent and some 17 percent during the peak of the 2015 refugee crisis. The evolution of cumulative exposure is less dramatic and somewhat less unevenly distributed. At the 25th percentile, there is low but non-zero exposure from 2014/2015, and at the 90th percentile, cumulative exposure peaks at 15 percent in 2019/2020.

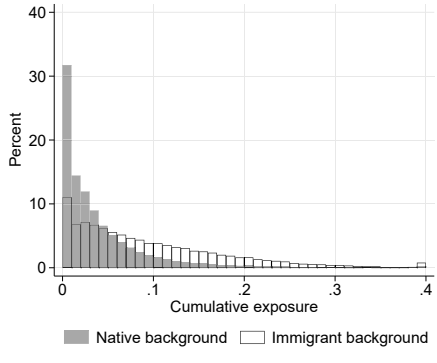
(a) Contemporaneous exposure at different percentiles



(b) Cumulative exposure at different percentiles



(c) Distribution of cumulative exposure by migration background



(d) Average cumulative exposure by migration background

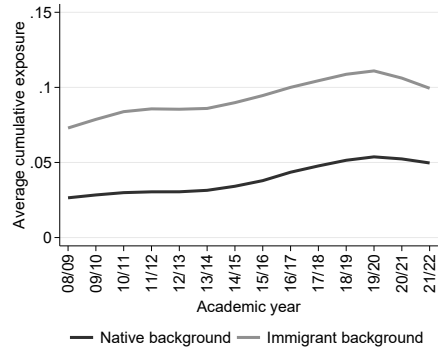


Figure 2. Exposure to recent immigrant and asylum seeking students. The panels (a) and (b) show trends in the distribution of school-by-grade level contemporaneous and cumulative exposure to asylum seekers and recent migrant students at different percentiles of the distribution in Sweden between 2008/2009 and 2021/2022. Panel (c) shows the distribution of cumulative exposure for natives and immigrant background students and panel (d) shows trends in cumulative exposure by migration background.

The bottom panel shows (c) the overall distribution of cumulative exposure for native and immigrant background students for the 2008–2022 period and (d) the time trend in average cumulative exposure for native and immigrant background students. The figures reveal a much higher spike at zero exposure for native students than for students of immigrant background and that native students have more mass at low levels of exposure. While average native student exposure to recent migrants rises from some 2.5 percent in 2008/2009 and peaks at around 6 percent during the refugee crisis years, immigrant background students have 5 percentage points (or approximately 100 percent) higher exposure to recent immigrants and asylum seekers, rising from a bit over 7 percent in 2008/2009 to 11 percent during the crisis.

These patterns are evidence of the clustering of immigrant background students in certain schools but also of the fact that the refugee crisis actually did not increase segregation: exposure increased similarly for native and immigrant background students. A reason for this is that during the crisis years, many refugees were received in small rural municipalities where accommodation was available but who had little previous experience of immigration.[4]

Our estimation strategies exploit 1) the year-to-year variation in exposure across grades within schools and 2) the variation in exposure to recent migrants in across schools and grades resulting from the refugee crisis. Arguably, variation in exposure across grades and years *within* schools should be as good as random because the age composition of recent immigrants in a particular municipality and school will vary in a plausibly random way, although there is non-random sorting of migrants to municipalities and schools. Moreover, the sudden nature of the refugee crisis and the need to rapidly accommodate new students also introduces an element of exogeneity in exactly which schools were more and less exposed depending on the availability of housing and refugee accommodation facilities.

We have argued that the Swedish context is one of refugee migration. Table 1 shows the composition of students in Swedish compulsory schools by country or region of origin in the years 2008–2022. In this table, the origin of students is defined by the county of birth of the student or the student's mother. The immigrant student population is very diverse, with no single group exceeding two percent of the student body. The largest groups are immigrants from former Yugoslavia and Bosnia and Herzegovina, Northeast Africa, Middle East and North Africa, and Iraq.[5]

[4]The ability of schools to accommodate the large influx of refugee children led to political debates in Sweden and many other European countries about the strain the crisis put on host communities. In Sweden, the government introduced both general support programs to improve refugee reception in schools and targeted support to heavily affected municipalities (see e.g., Bunar, 2017; Mörtlund, 2020).

[5]Table A1 presents the details of the country and region classification.

Table 1. *Country/region of origin of compulsory school students (grades 3, 6, and 9) in Sweden 2008–2022*

	Frequency	Percent	Cumulative
Sweden	2,435,790	88.67	88.67
Finland	6,788	0.25	88.92
Denmark	1,793	0.07	88.98
Norway and Iceland	1,647	0.06	89.04
Bosnia and Herzegovina	21,030	0.77	89.81
Former Yugoslavia	36,954	1.35	91.15
Poland	6,352	0.23	91.38
UK and Ireland	735	0.03	91.41
Germany	2,975	0.11	91.52
Mediterranean Europe	1,520	0.06	91.57
The Baltic states	1,576	0.06	91.63
E Europe, Caucasia, C Asia	9,897	0.36	91.99
Czechia, Slovakia, Hungary	1,632	0.06	92.05
Continental Europe	1,399	0.05	92.10
US and Canada	548	0.02	92.12
Mexico and Central America	1,828	0.07	92.19
Chile	6,704	0.24	92.43
South America	4,416	0.16	92.59
Northeast Africa	31,920	1.16	93.75
Middle East and N Africa	55,137	2.01	95.76
West, Central, South Africa	7,739	0.28	96.04
Iran	13,461	0.49	96.53
Iraq	44,972	1.64	98.17
Turkey	21,121	0.77	98.94
East Asia	3,794	0.14	99.08
Southeast Asia	11,288	0.41	99.49
South Asia and Mongolia	13,456	0.49	99.98
Oceania	108	0.00	99.98
Unknown	480	0.02	100.00
Total	2,747,060	100.00	

Notes: School composition by region of origin of students with siblings in 3rd, 6th and 9th grade. For a complete list of countries included in each source region, see Table A1.

3.2 The Swedish school system

Sweden requires resident children aged 6 to 16 to attend compulsory school and offers schooling to refugee children during the asylum process. Since the early 1990s, the Swedish school system is very decentralized. There is a national curriculum, but municipalities are responsible for financing schools, both municipal-run schools and independent schools. The latter are entitled to funding, provided they follow the national curriculum and do not charge fees to students (see e.g., Holmlund, Sjögren, and Öckert, 2019). Municipal schools are responsible for providing school placements for all students in the municipality, while independent schools can choose how many students to admit.

There is school choice in the sense that families can wish for a specific school, independent or municipal. Still, the school choice and placements are typically not coordinated between municipal and independent schools. Independent schools can choose to admit students based on residential proximity or queue time (in queues they administer themselves) while giving priority to siblings. Municipal schools are instead restricted to admitting students based on residential proximity and are required to provide slots within a reasonable distance from the home for all school-age children arriving in the municipality at any time during the year, including refugee children (Björklund et al., 2004). Hence, children moving to a new municipality during their school years (including recent migrants) are typically received in municipal schools due to these different rules regarding school assignments. According to Mörtlund (2020), municipalities actively try to counteract school segregation when assigning refugee children to schools.

Increased residential segregation and school choice have contributed to rising school segregation since the 1990s (Böhlmark, Holmlund, and Lindahl, 2016). Holmlund, Sjögren, and Öckert (2019) show that some 70 percent of the increase in the intra-school correlation in a composite measure of student background was due to rising residential segregation and the remainder due to school choice. However, the analysis in Holmlund, Sjögren, and Öckert (2019) also shows that school segregation in the immigrant/native dimension actually declined during the 2015 refugee crisis, the reason being that the fraction of all native schools, mostly in rural areas, declined. Grönqvist and Niknami (2017) document the school performance of refugee children in Swedish since the 1990s and find a substantial performance gap to native students. However, they also show that much of the gap is accounted for by socioeconomic background and neighborhood effects.

Compulsory school is organized into three school stages, comprising the lower stage from the pre-school year (grade 0) to 3rd grade, the middle stage from 4th to 6th grade, and the upper stage from 7th to 9th grade. The grade configuration of schools varies. At the beginning of our study period, about 60 percent of schools with 9th grade were 0th to 9th grade schools, 20 percent

were 7th to 9th grade schools, 18 percent were 6th to 9th grade schools, and the remainder were 4th to 9th grade schools. There are also feeder schools with grade configurations from 0th to 3rd or 0th to 6th grade.[6] This means that many students need to change schools in either 4th, 6th, or 7th grade. Because there are fewer 6th to 9th grade schools, it is less frequent to change schools in 6th grade. We will take this into account when creating measures for school changes.

At the end of each stage, students take mandatory national tests in the core subjects (mathematics and Swedish in grades 3, 6, and 9 and English in grades 6 and 9).[7] National tests are locally graded at the school using national guidelines. In 6th and 9th grade, they serve as guidance when teachers set the end-of-year grades.[8] In 9th grade, the national tests are crucial for students since they influence the final compulsory school grades, which determine high school eligibility. They also determine the student's ability to compete for admission to popular schools and high school programs. In this paper, we use the average of the student's grades on the national tests in mathematics, English, and Swedish for each grade (3, 6, and 9) as our main measure of student performance. All test scores are standardized within grade and cohort in the incumbent student population. We also use as outcomes the test scores in the individual subjects and the teacher set grades in 6th and 9th grade, also these standardized within grade and cohort in the incumbent student population.

4 Data and measurement

We aim to study how incumbent student school performance is affected by exposure to recent migrants in school. Hence, we need to measure exposure to migrants and student school performance, but we also need data that allow us to account for selection and capture how families and schools might adapt.

Our main body of data comes from the Student Register (*Elevregistret*), which includes the universe of primary and lower secondary school students (*grundskola*) in Sweden in each grade that they attended between 2008 and 2022, which defines our study period. Using this data, we can establish peer composition at the school-by-cohort level and, for a majority of the students, also at the classroom level. To this data, we match the national test scores in Swedish, English (only 6th and 9th grade), and mathematics at the end of each school stage, i.e., in grades 3, 6, and 9, which come from the National Exams Register (*Nationella provregistret*). These data are available from 2010 (grade

[6] See Holmlund, Sjögren, and Öckert (2019).

[7] In 9th grade, there are national tests also in one of the social science subjects (geography, history, religion, social science) and one of the natural science subjects (biology, chemistry, physics), which subject is randomized at the school level.

[8] Vlachos (2019) shows that although the test grades are subject to teacher subjectivity, they are more objective measures of student performance than the teacher set end-of-year grades.

3), 2012 (grade 6), and since 2003 (grade 9). We also add information on teacher set grades in grades 6 and 9 and school-level information on teachers. We link students to parents and siblings and match on background information using population registers (*Flergenerationsregistret* and *RTB*) containing information on family links, birth records, and country or region of origin and immigration year of parents and children. Socioeconomic information on parents, i.e., education and earnings data, come from the LISA register based on the Income and Tax Register (*Inkomst- och Taxeringsregistret*) and the Education Register (*Utbildningsregistret*).

In our sample of incumbent students, we include native students for whom we can observe the national test results in at least one of the three grades (3, 6, or 9) and who have a sibling for whom we can observe a test score outcome. This restriction allows us to include family-fixed effects. It leaves us with a panel of approximately 2.7 million student-by-year observations over fourteen years, during which we can measure exposure to recent migrants and outcomes in terms of national test scores. Table A2 shows how our sibling sample compares to the full sample.

As our main outcome variable measuring school performance, we use the students' average results on the national tests in mathematics, English, and Swedish. Test grades are first standardized at the cohort and grade level within the incumbent population. This standardization is done to avoid trending results in the native populations as the fraction of recent migrants increases over time. During the pandemic years (academic years 2019/20 and 2020/21), national tests were not mandatory, and results were not collected for students in compulsory school. To include these years in our study, we have imputed the test scores from teacher set grades in the corresponding subjects (mathematics, English, and Swedish). Furthermore, in 2018, the national test in mathematics leaked beforehand for grade 9, and the student's test scores were, to a large extent, dismissed. Again, we have imputed the test scores in mathematics from teacher set grades for that cohort. To ensure that systematic differences in our imputation do not drive our results, we also re-run our analysis on sub-samples without imputations.

We use two measures of exposure to recent migrants: contemporaneous and cumulative exposure. Contemporaneous exposure is the share of recent immigrants, i.e., immigrants who were granted resident status within the last four years and asylum seekers, in the same year when we measure the outcome in a given school, grade, and year. Because school performance in a given year is likely to depend not only on the current teaching environment and peers but also on previous experiences, we also follow Figlio et al. (2023) and compute a measure of the student's cumulative exposure. For each student i, in school s, in grade g, and in academic year t, we average exposure to recent immigrants

Table 2. *Summary statistics for students in grades 3, 6, and 9 with and without siblings 2008/09–2021/22*

	Native background		Immigrant background		Recent arrival	
	Mean	SD	Mean	SD	Mean	SD
Male	0.51	0.50	0.51	0.50	0.53	0.50
Birth order	1.89	0.94	2.28	1.34	1.97	1.21
Age in months	152.06	29.36	150.81	29.38	155.14	30.49
Mother income ptile	55.04	24.16	34.51	25.44	9.13	15.50
Father income ptile	72.22	24.01	47.81	30.30	18.53	23.90
Mother yrs education	13.20	2.21	11.52	2.63	11.04	2.99
Father yrs education	12.56	2.33	11.54	2.66	11.41	3.06
Predicted test score	0.04	0.37	-0.27	0.41	-0.87	0.46
Actual test score	0.05	0.97	-0.25	1.04	-0.93	1.19
Change school	0.06	0.23	0.09	0.28	0.10	0.31
Contempor. exposure	0.04	0.06	0.09	0.09	0.16	0.13
Cumulative exposure	0.04	0.05	0.09	0.08	0.17	0.13
Observations	2435790		311270		184751	

Notes: Summary statistics for the key background and outcome variables, and exposure measures for students in grades 3, 6, and 9, by student category. Parental income is percentile ranked within the childbirth cohort.

over the students' school history (grades 0 to 9) using the following equation:

$$\text{Cumulative Exposure}_{isgt} = \frac{1}{g} \sum_{g'<g} \text{Contemporaneous exposure}_{isg't}.$$

We also compute the corresponding measures at the classroom level. But, since schools might reorganize classrooms in response to migrant inflow, we use grade-level exposure as our main exposure measure. Also, families may respond to migrant exposure and change schools for their children. Sibling fixed effects partly account for this. Still, if parents respond by placing younger siblings in response to older sibling's exposure, there may be selection effects also within sibling pairs. Therefore, we also compute measures of expected exposure, which assumes that younger siblings attend the same school in a given grade as their older siblings.

Table 2 presents summary statistics for the population of students with Swedish residency, i.e., excluding asylum seekers for whom there is no information other than sex and age. The native population includes Swedish-born students with at least one Swedish-born parent. Students with immigrant backgrounds are Swedish-born students with two foreign-born parents. Recent immigrants are students who immigrated, i.e., received residency, within

the last four years. The incumbent population is comprised of the first two groups.[9]

Notably, there are some interesting differences between the groups of students. Higher birth orders for students of immigrant background suggest they have more siblings, on average. It is also clear that both mothers' and fathers' income percentiles and years of education are higher in the native population. Standardized test scores are higher among natives, and while children with immigrant backgrounds have test scores around 0.30 below the native population, recent immigrants do much worse. The measure of predicted test scores, which is a summary measure of the student's characteristics and family background, naturally reflects the differences in student performance.[10] The indicator for changing school from one grade to the next, excluding mechanic school changes due to the grade configuration of the school, shows that students of immigrant background and recent immigrants are more likely to change schools than native students. As we saw in Table 2, the exposure to recent immigrants also varies substantially across the groups.

5 Empirical strategy

There are a number of challenges that need to be overcome, given our aim to estimate the causal effect of being exposed to recent migrants on incumbent student school performance. First, exposure to migrants is unlikely to be random across schools since migrants and refugees are more likely to move to or be placed in some areas than others, even within municipalities. Newly arrived students are more likely to be assigned to schools where there are free slots or where the municipality can more readily arrange new places. Second, because of residential segregation and school choice, native and other incumbent students are not randomly distributed across schools. Better-informed and more resourceful families are more likely to have exercised school choice, and their children are thus more likely to go to oversubscribed schools, which are less likely to accommodate new students. Third, some families may react to the inflow of migrants and refugees and switch schools and/or seek out a different school for their younger child if an older child's school is exposed to the migrant influx. Fourth, schools might respond to migrant inflow by reorganizing classrooms, creating special migrant classes, or becoming more or less lenient in exempting students from national testing or in their grading policies.

In our main analysis, we follow a strategy proposed by Brandén, Birkelund, and Szulkin (2019) and Figlio et al. (2023) to overcome these identification

[9]Note that we do not include foreign-born students with more than 4 years of residency in our sample of incumbent students, as this group keeps changing as recent immigrants accumulate time in the country and as some of them go from being part of the exposure to being exposed.

[10]The measure is based on a prediction of test scores based on the student's sex, birth order, age, years since immigration, and parental background.

problems related to student and migrant sorting. First, we use the within-school cohort-to-cohort variation in migrant exposure to address the fact that exposure is not random at the school level. Second, we account for the non-random selection of native and other incumbent students to schools by controlling for family-fixed effects. We also account for possible selection, also within families, should families selectively choose schools for their depending on how they judge the child would be harmed by or benefit from exposure to migrant children. We do this by explicitly examining school changes and if siblings are placed in a different school in response to older sibling's exposure. We also instrument younger sibling exposure by the predicted exposure based on the school placement of the older sibling. Furthermore, we examine test-taking behavior, exploit classroom exposure, and the presence of immigrant classes.

In Section 7, we present an alternative estimation strategy where instead of using year-to-year variation in exposure within schools, we use an event study approach to compare outcomes of students in schools more or less impacted by the 2015 refugee crisis. This event-study strategy potentially captures also school-wide effects or spillover effects across grades and years of migration influx, which are captured by school-by-year fixed effects in our main identification strategy.

5.1 Main specification

We estimate the following main specification:

$$\begin{aligned} Y_{igst} = \beta_1 \times \text{Migrant exposure}_{igst} + \alpha_{\text{school} \times \text{year}} \\ + \delta_{\text{grade} \times \text{year}} + \sigma_{\text{fam}} + X_i\gamma' + e_{isgt} \end{aligned} \tag{1}$$

where Y_{igst} is the average test score for incumbent student i in grade g in school s in calendar year t. The explanatory variable is migrant exposure$_{igst}$, is either the contemporaneous exposure or the cumulative exposure to recent migrants and asylum seekers of a student i attending g in school s in calendar year t.

In our preferred specification, $\alpha_{\text{school} \times \text{year}}$ denotes school-by-year effects, $\delta_{\text{grade} \times \text{year}}$ grade-by-year fixed effects, and σ_{fam} family fixed effects. X_i is a vector of individual characteristics, sex, birth order, age in months, and parental characteristics reflecting the student's socio-economic background based on available parental data. For comparative purposes, we also estimate the simple OLS and specifications that only include school-by-year and grade-by-year fixed effects, as well as individual and family controls. We cluster standard errors on the school-by-cohort and family level, thus allowing students' outcomes to correlate within their respective school-cohort and within sibling pairs.[11] β_1 represents the coefficient of interest, measuring the effect of going from no exposure to an all-recent migrant class.

[11] Adding sibling clusters, however, makes little difference.

When analyzing peer effects at the school-by-cohort level, the primary threat to identification lies in the potential sorting of students. While the inclusion of school-by-year and grade-by-year fixed effects addresses the potential non-random placement of migrants to schools, it is still plausible that incumbent students sort into different schools based on a number of observable and unobservable characteristics. To address this, in our preferred specification, we include family fixed effects. This inclusion allows us to compare the outcomes of siblings who were exposed to different shares of recent migrants in their school. This strategy allows us to absorb selection into schools that occur based on familial socio-economic characteristics and unobserved family characteristics. See Figure B1 for the distribution of residuals when including also family fixed effects in our model.

Such a strategy, however, still leaves the possibility that families send siblings to different schools based on their scholastic performance and react differentially to inflows of migrants to their children's school cohorts. We address this in two ways. First, we examine school changes explicitly, both school changes in response to the student's own experiences of migrant influx and younger sibling school placements in response to an older sibling's exposure. Second, we estimate our family fixed effects specification using the student's expected, rather than actual, exposure to recent migrants, where the expected contemporaneous exposure is measured by assigning the contemporaneous exposure to the student it would have had the student been placed in the same school as the older sibling in the corresponding grade.

5.2 Threats to identification and balance tests

In order to examine if our identification strategy successfully accounts for the non-random sorting of incumbent students and recent migrant exposure, we estimate the model using predicted test scores as the outcome variable. The predicted test score of student i in grades $g = 3, 6, 9$ of compulsory school is based on the following model:

$$Y_{igt} = \beta \times X_{igt} + e_{igt} \tag{2}$$

where Y_{igt} is the test score of student i in grade g and X_{igt} is a vector of individual and family characteristics of the student.[12] Naturally, family fixed effects will account for any differences in predicted test scores common to the siblings. Still, if families choose schools for their children depending on scholastic aptitude and in a way that is correlated with characteristics that differ across siblings, we might see that exposure to recent migrants is correlated

[12] These characteristics include indicators for sex, birth order, being a first or second generation immigrant and country/region of origin dummies, age in months, indicators for maternal and paternal years of schooling, and measures of maternal and paternal position in the earnings distribution.

Table 3. *Correlation between exposure to recent migrants and natives' predicted test scores*

Exposure:	Predicted standardized scores			
Contemporaneous	-1.302***	-1.337***	-0.171***	0.016***
	(0.012)	(0.012)	(0.009)	(0.005)
Cumulative	-1.828***	-1.845***	-1.150***	0.008
	(0.016)	(0.016)	(0.016)	(0.007)
Grade x Year FE		X	X	X
School x Year FE			X	X
Family FE				X
Mean LHS	0.008	0.008	0.008	0.008
SD LHS	0.390	0.390	0.390	0.390
Observations	2,747,060	2,747,060	2,747,060	2,747,060
R-squared	0.063	0.065	0.230	0.891

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: Observations are the number of native students, including only students with at least one sibling in the sample. Native students are defined as students born in Sweden. Standard errors are clustered at the school-by-cohort and family level.

with predicted test scores, even when controlling for family fixed effects. Table 3 presents the results. The first three columns show that there is considerable negative sorting of students to schools and even to cohorts within schools that are exposed to recent immigrants. In the fourth column, which controls for family fixed effects, there is no longer any correlation between individual predicted test scores and cumulative exposure. However, it appears that within sibling pairs, high contemporaneous exposure is positively associated with higher predicted scores. This suggests that even within sibling pairs, the child with characteristics associated with better school performance, i.e., girls, firstborns, and children born early in the year, are more likely to experience high recent migrant exposure, which motivates including these individual characteristics as controls. Note, however, that the magnitude of the estimate is small: a 10 percentage point increase in contemporaneous exposure is associated with less than 0.002 of a standard deviation increase in predicted test scores.

As discussed, one further threat to identification could come from the students' selecting different schools as a response to exposure to recent migrants. In that case, our estimates could be reflecting compositional changes across schools as opposed to the true effect of the exposure. To address this concern, we can directly empirically test for evidence of such behavior by estimating our model using an indicator for whether a student changes schools between grade g, year t and grade $g+1$, year $t+1$. The results of this exercise are

Table 4. *Propensity to change school based on own exposure*

	Change school		
Exposure:	Native background	Immigrant background	Total
Contemporaneous	0.021***	0.030***	0.024***
	(0.004)	(0.008)	(0.004)
Grade x Year FE	X	X	X
School x Year FE	X	X	X
Individual Controls	X	X	X
Family FE	X	X	X
Mean LHS	0.047	0.079	0.051
SD LHS	0.211	0.269	0.220
Observations	8,488,970	1,242,856	9,738,417
R-squared	0.196	0.242	0.196

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is a binary variable equal to one if the students changed school in the following year, and zero otherwise. Immigrant background is defined as both parents being born outside Sweden. Observations are the number of native students, including only students with at least one sibling in the sample. Standard errors are clustered at the school-by-cohort and family level.

shown in Table 4. We do find such evidence in our sample, both for students with native and immigrant backgrounds. This suggests a flight behavior of students in response to exposure to recent immigrants. Again, it needs to be pointed out that the effects are rather small: a 10 percentage point increase in the exposure to migrants leads to an increase in the likelihood of moving schools by 0.21 percentage points, or 4.5 (0.21/0.047) percent among native students and by 0.3 percentage points, or 3.8 (0.3/0.079) percent among students with immigrant background. These modest responses may reflect that families do not worry about migrant inflow and/or that families may be reluctant to change schools for a child because of the costs in terms of disrupted social contacts and routines.

Such costs are lower when choosing a new school for a younger sibling. We thus examine if younger siblings are placed in a different school than the sibling in response to an older sibling's exposure to an immigrant influx. The results are presented in Table 5. The estimated effect sizes are much larger. A 10 percentage point increase in the older sibling's exposure to recent migrants causes a 0.024 percentage point or a 14 (0.024/0.17) percent increase in the likelihood of native families enrolling the younger sibling in a different school. The corresponding increase is 24 (0.058/0.245) percent for immigrant background families. These results suggest that families do react to migrant inflow

Table 5. *Propensity to change school based on older sibling's exposure*

	Change school		
Exposure:	Native background	Immigrant background	Total
Contemporaneous	0.235***	0.580***	0.311***
	(0.018)	(0.031)	(0.017)
Grade x Year FE	X	X	X
School x Year FE	X	X	X
Individual Controls	X	X	X
Family controls	X	X	X
Mean LHS	0.171	0.245	0.179
SD LHS	0.377	0.430	0.383
Observations	2,878,712	308,159	3,196,822
R-squared	0.201	0.225	0.192

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is a binary variable equal to one if the students changed school in the following year, and zero otherwise. Immigrant background is defined as both parents being born outside Sweden. Observations are the number of native students, including only students with at least one sibling in the sample. Standard errors are clustered at the school-by-cohort and family level.

but that they are reluctant to have their children change schools. This implies that within-family selection is an issue that we need to take into account. We do this by estimating our model while replacing the younger sibling's migrant exposure with a predicted exposure it would have experienced had it been enrolled in the same school as the sibling. The results are presented in Table A3. The results suggest that the positive main effects are stronger when using the predicted sibling exposure. IV estimates are much larger in magnitude, but it is clear that the exclusion restriction does not hold since older sibling exposure could very well affect younger sibling exposure in other ways than through the effects of migrant exposure.

If schools exempt students from national testing differently depending on how exposed the grade is to migrants, our sample of students would be selected. We rule out this threat to our identification by examining if migrant exposure predicts having a test score from the national tests. Results are presented in Table A4 and show that there is no effect of cumulative exposure on test taking.

We have established that our empirical strategy, including both school-by-year and family fixed effects, successfully eliminates the correlation between predetermined characteristics and exposure to migrants over a student's school history, but that some evidence of positive selection is present from contem-

poraneous exposure. Moreover, we have established that families do choose schools for their children in response to inflows of migrants, but that the effects are small when it comes to changing schools for a particular child and larger in choosing a new school for a sibling, something we will address in the results section. We have further ruled out that our outcome measure, test scores from national tests, is biased due to the effects of migrant exposure on test taking.

6 Results

We now turn to presenting the results of estimating the effects of exposure to recent migrants on school performance using the model shown in Equation 1. We first present effects on test scores for the whole incumbent student population consisting of students with native and immigrant backgrounds. We investigate heterogeneous effects for different groups of students, effects across the test score distribution, and whether the type of recent migrant exposure matters. We also explore potential mechanisms by investigating school resource responses.

Table 6 presents our main results and illustrates the importance of accounting for sorting of both migrant and incumbent students to schools and also that contemporaneous, short-run exposure does not necessarily have the same effects as long-run, cumulative exposure. The estimates in the first column show that the association between exposure and test scores within schools and across cohorts is negative. As controls for grade-by-year fixed effects and school-by-year fixed effects are introduced in the second column, the negative association between contemporaneous exposure and test scores is reduced markedly. Still, the association with cumulative exposure remains high. In the third column, we introduce controls for individual characteristics and family background. This introduction of controls reduces the negative estimates further, illustrating that there is a negative sorting of incumbent children to schools that have more migrant exposure. In the fourth column, we instead control for family fixed effects, accounting for unobservable characteristics shared by siblings. This introduction of fixed effects appears to address further the issue of negative selection, which we could see was eliminated in terms of predicting the test scores in Table 3.

Both the estimates of contemporaneous and cumulative exposure become positive, albeit relatively small in magnitude. Thus, in our preferred specification in column 4, a 10 percentage point increase in the cumulative exposure to recent immigrants increases the standardized test scores of incumbent students by approximately 0.009 of a standard deviation. The estimate is somewhat smaller for contemporaneous exposure. Both the pattern and the magnitude of the results are highly comparable to the findings of Figlio et al. (2023) in Florida schools, suggesting that the effect is similar in the Swedish context.

Table 6. *Effect of exposure to recent migrants on test scores*

Exposure:	Standardized scores			
Contemporaneous	-0.241***	-0.182***	-0.042	0.065**
	(0.033)	(0.033)	(0.032)	(0.030)
Cumulative	-1.482***	-1.100***	-0.338***	0.091***
	(0.035)	(0.034)	(0.030)	(0.035)
Grade x Year FE	X	X	X	X
School x Year FE	X	X	X	X
Individual controls		X	X	X
Family controls			X	
Family FE				X
Mean LHS	0.014	0.014	0.014	0.014
SD LHS	0.984	0.984	0.984	0.984
Observations	2,747,060	2,747,060	2,747,060	2,747,060
R-squared	0.142	0.165	0.257	0.624

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.

Notes: The dependent variable is our main measure of academic performance in school. When the student has taken the national tests in mathematics, Swedish, and English, the outcome is the average of the scores from these tests standardized within the cohort of incumbents. If the student has missed one of the tests, we instead use the teacher set grade in the corresponding subject (also standardized within the cohort of incumbents). For a comparison of results depending on the variable of academic performance, see Table A6. The regressions are run separately for contemporaneous and cumulative exposure. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Incumbent students are defined as students born in Sweden. Standard errors are clustered at the school-by-cohort and family level.

We might worry that grade level measurements of exposure do not reflect actual classroom exposure if schools with high migrant exposure are more likely to segregate students by forming recent migrant or reception classes. As is illustrated when comparing panel (a) to (b) in Figure B2, there are indeed more classrooms without recent migrants than there are school cohorts. There are also a number of classrooms that exclusively contain recent migrants. From panel (d), it is also clear that classrooms containing more than 90 percent of recent migrants tend to be very small. Hence, in Table 7, we present the results when the model is instead estimated to measure recent migrant exposure at the classroom level. Results are very similar to our main results, although somewhat larger in magnitude. In a further check, we examine to what extent the positive effects of migrant exposure are driven by schools and cohorts in which migrants are segregated into special classes. Again, focusing on column 4 of Table A5, the main results hold. Positive effects are slightly weaker but still present when the cohort has a special class and even stronger when there is a special class. How classrooms are organized within a school seems to matter. Still, there is no evidence that the positive effects of migrant exposure that we find are driven by schools that isolate native students from exposure to their migrant peers.

Having established that exposure to migrants actually matters positively for school performance as measured by average test scores, we examine different subjects separately as well as performance measured by teacher set grades. In Table A6, we re-estimate model 1 with our preferred specification from column 4 in Table 6, with standardized teacher set grades in 6^{th} and 9^{th} grade and test scores from each subject separately (without imputations for missing scores). As can be seen, the positive results in Table 6 are driven by positive effects for test scores in English and Swedish. There are no positive effects on mathematics test scores, and, with the exception of English grades, there are no effects on grades. These results are in line with Brandén, Birkelund, and Szulkin (2019) that find insignificant effects on 9^{th}-grade school grades and also suggest the same ordering of results as in Green and Iversen (2022) who find zero effects for Norwegian and English and negative effects in mathematics. Our results suggest that migration inflow affects the way teachers set grades in relation to test scores. We explore this specifically in Table A7 and find that grades in Swedish and English are set less generously relative to national test scores for students who have higher cumulative exposure to migrants.

Next, we examine if the effects of migrant exposure differ across school stages. Note that our estimation strategy is more restrictive in that it requires a sibling with a school result from the same grade. Hence, our sample becomes much smaller. In Table A8, we show the results for our preferred specification, estimated for each grade separately with standardized scores (including imputed scores when test scores are missing) and standardized grades as outcomes. The results show that there are no significant effects of migrant ex-

Table 7. *Effect of exposure to recent migrants on test scores—Exposure measured at the classroom level*

Exposure:	Standardized scores			
Contemporaneous	-0.200***	-0.138***	-0.008	0.080***
	(0.020)	(0.020)	(0.018)	(0.015)
Cumulative	-1.192***	-0.880***	-0.258***	0.131***
	(0.030)	(0.029)	(0.026)	(0.028)
Grade x Year FE	X	X	X	X
School x Year FE	X	X	X	X
Individual controls		X	X	X
Family controls			X	
Family FE				X
Mean LHS	0.014	0.014	0.014	0.014
SD LHS	0.984	0.984	0.984	0.984
Observations	2,747,060	2,747,060	2,747,060	2,747,060
R-squared	0.142	0.165	0.257	0.624

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.

Notes: The dependent variable is our main measure of academic performance in school. When the student has taken the national tests in mathematics, Swedish, and English, the outcome is the average of the scores from these tests standardized within the cohort of incumbents. If the student has missed one of the tests, we instead use the teacher set grade in the corresponding subject (also standardized within the cohort of incumbents). The regressions are run separately for contemporaneous and cumulative exposure. In the few cases that we do not have a classroom identifier, we measure exposure at the school-by-cohort level to keep the same sample of individuals as in Table 6. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Incumbent students are defined as students born in Sweden. Standard errors are clustered at the school-by-cohort and family level.

posure on test scores in 3rd grade when children are 10 years old. However, the magnitude of the estimated effect of cumulative exposure is similar to our main result. Instead, the positive effects on test scores of migrant exposure are present in 6th and 9th grades and are somewhat stronger. With this more restricted sample, we also find positive effects on school grades.

6.1 Effect of exposure to migrants for different groups of students

So far, we have seen that exposure to recent migrants has an overall positive but small effect on the school outcomes of incumbent students. In what follows, we investigate if the effects are similar for boys and girls, by socioeconomic status (as measured by high and low predicted test scores), and by student migration background. To examine gender effects, we introduce an interaction term between female students and exposure to avoid restricting the analysis to same-sex siblings. For the other background categories, we estimate split sample regressions. The results are presented in Table 8. The first column shows the effects for natives, the second column for students with immigrant backgrounds, the third and fourth columns for students with low and high predicted test scores, and the fifth and sixth columns for girls and boys, respectively.

First, it is clear from columns 1 and 2 that the overall positive effects of exposure to recent immigrants are only present for natives. There is even a negative but insignificant point estimate for cumulative exposure for immigrant background students. Columns 3 and 4 suggest that the effects of cumulative exposure are similar regardless of students' family background, while contemporary exposure only benefits high SES students. We can further note that for the incumbent population as a whole, columns 5 and 6 suggest that the effects of contemporaneous exposure are positive for boys and girls but that the positive impact of cumulative exposure is substantially larger for boys than for girls and only significantly positive for boys.

A possible reason for differential results between students with a native and immigrant background could be that they are, in fact, exposed to different types of migrants. In Figure B3, we investigate if there are significant differences in the type of recent migrants that natives and students with immigrant background are exposed to by plotting the distribution of predicted and actual test scores of the recent migrants that comprise the exposure of the respective groups. The evidence suggests that there are no systematic differences in background characteristics of the recent migrants that native and immigrant background students are exposed to. Instead, differential effects of exposure are likely to be the result of different responses to the exposure.

Table 8. *Effect of exposure to recent migrants on test scores for different groups of students*

	Standardized scores					
Exposure:	Native	Immigrant background	Low PS	High PS	Female	Male
Contemporaneous	0.081**	0.000	0.004	0.177***	0.058*	0.073**
	(0.032)	(0.067)	(0.032)	(0.032)	(0.032)	(0.032)
Cumulative	0.116***	-0.090	0.084**	0.102**	0.018	0.160***
	(0.039)	(0.073)	(0.037)	(0.040)	(0.038)	(0.038)
Grade x Year FE	X	X	X	X	X	X
School x Year FE	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X
Family FE	X	X	X	X	X	X
Mean LHS	0.049	-0.253	0.014	0.014	0.014	0.014
SD LHS	0.971	1.039	0.984	0.984	0.984	0.984
Observations	2,434,990	300,654	2,747,060	2,747,060	2,747,060	2,747,060
R-squared	0.627	0.643	0.625	0.625	0.624	0.624

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Notes: The regressions are run separately for contemporary and cumulative exposure. Observations are the number of incumbent students, including only students with at least one sibling in the sample. Standard errors are clustered at the school by cohort level.

We further explore heterogeneous effects by investigating if the effects of exposure to recent migrants are similar across the test score distribution, i.e., if high- and low-performing students are equally affected. Figure 3 shows the separate point estimates for the effects of cumulative exposure on the probability of obtaining test scores above a given percentile in the test score distribution. The top panel shows that for the overall incumbent population, there are significant positive effects of exposure in the middle (25^{th} percentile and 50^{th} percentile) and the top (90^{th} percentile) of the distribution. Hence, the weakest students do not seem to gain. This pattern is confirmed the middle panel for students with a native background. However, for students with an immigrant background, there are instead negative point estimates throughout the distribution, and it seems that the weakest immigrant background children may, in fact, be harmed by recent immigrant exposure. The bottom panel shows the results for girls and boys. The differences are stark, and boys, in general, gain from being exposed (with the exception of the weakest boys). For girls, the pattern is different, with scholastically weaker girls gaining while rather high-performing girls suffer. These patterns suggest that relative position in the classroom may matter for how students are affected, but also that the classroom composition may affect how the teachers adapt their instruction.

6.2 Exposure to different migrants

We next examine how exposure to different types of recent migrants affects incumbent student outcomes. Based on the country or region of birth of the migrant students and their parents, we compute separate exposure measures for exposure to recent migrants from non-Western countries, exposure to migrants from high-income and low-income countries, and one measure for exposure to asylum-seeking students. The classification of high-income and low-income countries is, naturally, somewhat arbitrary but designed with the idea of capturing differences in the migrant students' language skills and school preparedness.[13] We also classify countries/regions by the educational performance of students who come from the respective countries/regions. The results, presented in Table 9, show that native students benefit from exposure to non-Western recent migrants, and the positive effect of exposure to asylum seekers is substantial, as is the impact of exposure to migrants from low-income countries. As for immigrant background students, the sample size is much smaller, and standard errors are large. Yet, it is worth noting that there are insignificant negative effects of cumulative exposure to non-Western

[13] Europe, Northern America, Chile, East Asia, and Oceania are classified as high-income regions of origin. MENA countries, Africa, South and Southeast Asia, and Latin America (excluding Chile) are classified as low-income regions of origin. While being an asylum-seeking student, there is no information on the country of origin in the data. Still, during the period studied, many of these students come from Iraq, Iran, Afghanistan, Syria, and the Horn of Africa.

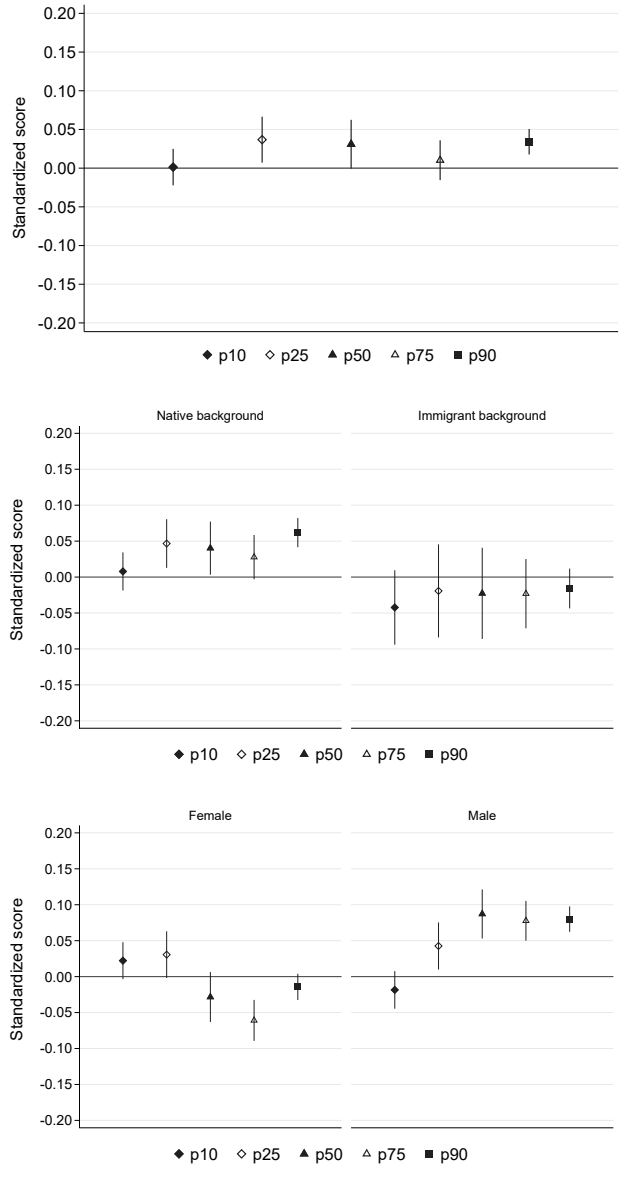


Figure 3. Heterogeneity of effects by position in the distribution of standardized scores. Each point in the graph represents the point estimate of effect of cumulative exposure to recent arrivals on the probability of having standardized scores above the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentile, respectively. The first graphs shows the results for the main analysis sample.

Table 9. *Exposure to migrants from different regions*

	Standardized scores	
Cumulative exposure:	Native background	Immigrant background
Non-Western immigrants	0.166***	-0.121
	(0.043)	(0.076)
Asylum seekers	0.514***	0.252
	(0.134)	(0.320)
Low-income countries	0.223***	-0.141
	(0.051)	(0.087)
High-income countries	-0.072	-0.015
	(0.068)	(0.145)
Grade x Year FE	X	X
School x Year FE	X	X
Individual controls	X	X
Family FE	X	X
Observations	2,434,990	300,654
R-squared	0.63	0.64

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: Europe, Northern America, Chile, East Asian and Oceania are classified as high-income regions of origin. MENA countries, Africa, South and Southeast Asia and Latin America (excluding Chile) are classified as low-income regions of origin. While being an asylum seeking student, there is no information of country of origin in the data, but during the time period studied, many of these students come Iraq, Iran, Afghanistan, Syria, the Horn of Africa.

recent immigrants and immigrants from low-income countries, but a positive point estimate for exposure to asylum seekers.

6.3 School responses

We have so far found that native students gain marginally from being exposed to recent migrants but that immigrant background students do not. What can explain these positive effects on native students? One mechanism suggested in the previous literature is that schools respond to the migrant influx by increasing resources. We investigate this possibility by estimating the effect on class size, which we can measure at the grade level, and also by investigating if students are more or less likely to participate in home language classes. Students who speak another language than Swedish at home are by law entitled to classes to learn and develop this language. Lack of teachers and too few students in the language in question are reasons why students are not provided home language classes. Participation is, however, voluntary, which also means that students' motivation and parents' demands matter for participation. Na-

tive students have a right to home language classes if at least one of the parents in the household speaks a language other than Swedish.

The effects of recent migrant exposure on class size are presented in Table A9. We can see that there is a clear negative association between class size and contemporaneous migrant exposure overall (column 3). This negative association also holds for students of immigrant background, but there is no significant effect for students with a native background. This result suggests that schools do respond to an influx of migrants by reducing class size, but it could also be that accommodation of migrants is more likely to take place in a small school cohort because there is room. Nevertheless, the magnitudes of the effects are very small relative to mean class sizes. In Table A10, we re-estimate our main results, including class size as a control, well aware that this is an endogenous control. Changes in class size cannot account for the improvement in test scores.

Table A11 displays the results for participation in home language classes. While exposure to recent migrants leads native students to increase their involvement in home language classes, the opposite is the case for immigrant background students. The initial level is, naturally, much higher for immigrant background students, where 42 percent take such classes, as compared to 5 percent among native students. These results suggest that native students actually gain access to teaching resources: there is a doubling of the fraction of native students taking these classes. For immigrant background students, there is instead a 25 percent reduction in the fraction taking home language classes. The reasons could be changing access, i.e., if the class is offered because of more students with the same language, or positive or negative changes in motivation or encouragement to take the class if more peers are speaking the language in the school.

7 European migration crisis

In this section, we focus more specifically on the effect of the 2015 migration shock that came in the form of a large immigration flow into Europe. During that year, Sweden admitted the largest number of migrants per capita: over 160 thousand relative to the then population of 10.5 million. This inflow was an acute and largely unexpected shock to the infrastructure of the country, including the schooling system.

Due to the acute nature of the shock, many refugees were placed in schools located in smaller and more rural municipalities where accommodation was more readily available (National Agency for Education, 2016). This led to higher exposure to asylum seekers in more rural schools that had previously seen relatively low shares of foreign students. We illustrate this in Figure 4. As can be seen in the figure, schools in smaller municipalities received a proportionally higher share of asylum-seeking students, and there was lit-

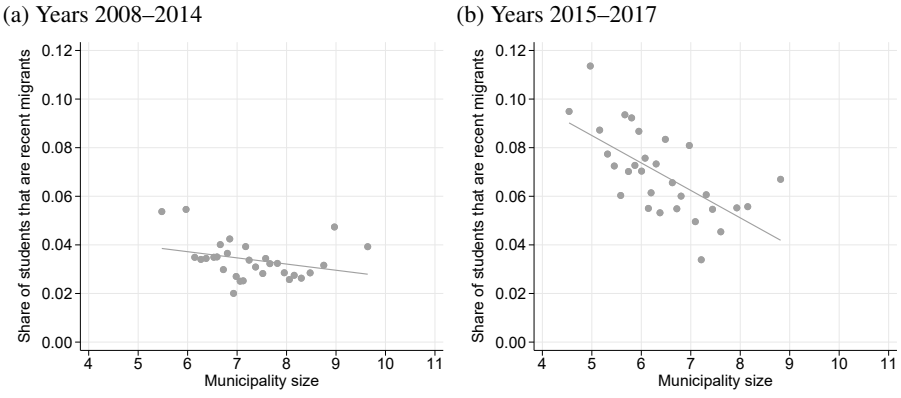


Figure 4. Share of recent migrants in schools by municipality size in grade 9 and years 2008–2014 and 2015–2017, respectively. Municipalities are binned into a group of 30 bins. For underlying distribution of exposure on the municipality level, see Figure B4.

tle correlation between previous refugee exposure and the exposure resulting from the crisis. These circumstances, therefore, create a suitable institutional framework for disentangling the effect of a sudden and significant inflow of refugee students.

Based on the pattern shown in the top panel of Figure 2, which clearly shows when the crisis was most acute, we create a measure of crisis exposure at the grade and school level as measured by the average share of recent migrants, i.e., asylum seekers and immigrants with less than four years of residency, in the school and grade during the school years 2015/2016 to 2017/2018. This exposure measure is used as a continuous treatment variable. We then estimate the following event study equation with the school year 2014/2015 as the reference year:

$$\begin{aligned} y_{igst} = \beta_t \times \sum_{k=2008}^{2021} 1_{t=k} \times \text{ Recent migrant share}_{gs2015-2017} \\ + \alpha_{\text{school s}} + \delta_{\text{year t}} + \sigma_{\text{fam}} + X_i\gamma' + e_{isgt} \end{aligned} \tag{3}$$

We also summarize the effect of crisis exposure by estimating the following continuous difference-in-differences specification:

$$\begin{aligned} Y_{igst} = \beta_1 \times \text{Recent migrant share }_{gs2015-2017} \times \ 1[\text{year} > 2014] \\ + \alpha_{\text{school}} + \delta_{\text{year}} + \sigma_{\text{fam}} + X_i\gamma' + e_{isgt} \end{aligned} \tag{4}$$

where Recent migrant share$_{gs2015-2017}$ is a continuous variable indicating the share of students who are recent migrants, i.e., either newly arrived (at most four years) or who are asylum seekers (with a pending asylum case) in school s and grade g during 2015–17 and $1[\text{year} > 2014]$ is an indicator variable for the period from 2015 onward. Similarly to Equation 1, the other terms

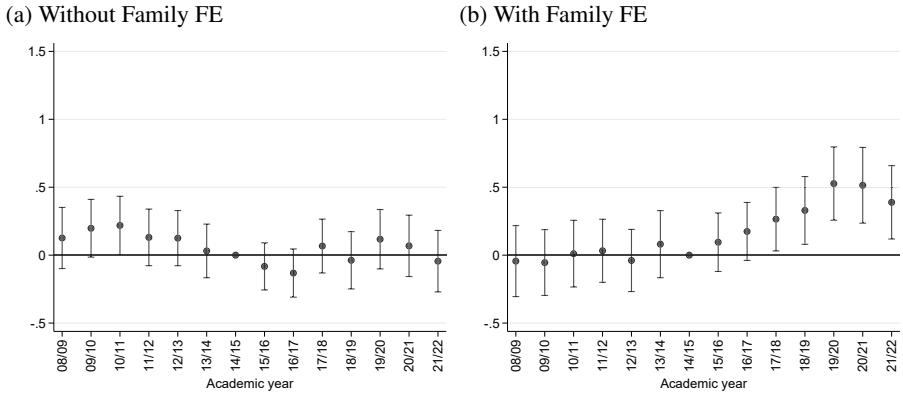


Figure 5. Crisis exposure for incumbent students. The figure shows the estimated coefficient of crisis exposure on student outcomes for incumbent students. Incumbent students are defined as students born in Sweden and immigrant background as native students with two foreign-born parents. The dependent variable is our main measure of academic performance in school. See Section 7 for a description of the identification. For a separation of native and immigrant background incumbent students, see Figure B5.

represent the respective fixed effects and the vector of individual controls. This identification strategy hinges on the recipient schools facing similar trends in school outcomes prior to the crisis. We show that to be the case in panel (b) of Figure 5, which displays the results of estimating Equation 3, while accounting for the selection of students to school using family fixed effects. We observe no relationship between test score development prior to the crisis and the exposure to recent migrants during the crisis, with none of the pre-crisis estimates being statistically significant at the 5 percent level.

Panel (a), which does not control for family-fixed effects, shows that test scores were on a decline in schools that later became exposed to the crisis. Since the negative trend disappears when family fixed effects are included in the model, it seems that this trend is related to deteriorating but unobserved family characteristics. Panel (b) shows evidence of improved test scores in schools that were more exposed to the crisis. As of the academic year 2017/2018, estimates are positive and significant. In Figure B5 we estimate the events study separately for students with native and immigrant backgrounds. While exposed natives' test scores improve, we see no such effect on students with an immigrant background.

We show the results of estimating the continuous difference-in-difference equation 4 in Table A12. In the first column, we restrict the follow-up treatment period to 2018, the year by which the migration crisis was no longer acute. In the second column, we use the entire post-crisis period when data is available to identify the effect. This adjustment does not appear to influence our results substantially. A 10 percentage point increase in the exposure

to recent migrant students increases the test scores of the incumbent students by approximately 0.018 of a standard deviation. The magnitude of the effect is somewhat larger, although comparable to our estimates from the previous analysis. Thus, we observe a positive and statistically significant, albeit mild, impact of the shock experienced by Swedish schools during the 2015 refugee crisis.

As in the previous section, we are interested in understanding how schools respond to migrant exposure. It is of particular interest to examine how schools responded to the refugee crisis, which was arguably more salient than year-to-year variation in exposure of different cohorts in a school. We do this by estimating how class and the fraction of students taking home language classes were affected. Results are presented in Table B6. Panel (a) shows that there was an initial increase in class size in exposed schools the first year of the crisis but that class sizes were significantly reduced a few years into the crisis. There is also evidence that the fraction of children taking home language classes gradually declined after the crisis in exposed schools. While the effects on class sizes seem to corroborate our previous findings, although the estimated effect sizes are larger, the results on home language classes diverge.

8 Conclusions

In this paper, we study the effect of recent migrant peers on incumbent students. We utilize data on the universe of compulsory school students in Sweden between 2008 and 2022, a period characterized by high levels of global and local immigration. To account for the non-random sorting of migrant and native students into schools, we combine school-fixed and family-fixed effects to account for non-observable characteristics on the family level.

Our findings suggest that the negative association between migration and school performance stems from the significant negative sorting of migrants and incumbent children to schools. Once we account for this sorting, we find that both contemporaneous and cumulative exposure to recent migrants have small positive effects on student outcomes. While native students are most likely to benefit, incumbent students with immigrant backgrounds display negative, albeit insignificant, effects. Our analysis also shows that the type of migrant exposure matters for incumbent students. The analysis of the 2015 refugee crisis supports our finding of an overall positive effect on test scores of being exposed to recent migrants.

In an attempt to explore mechanisms behind the overall positive effects and for the differences between students with native and immigrant backgrounds, we find evidence of reduced classroom sizes in response to high recent migrant exposure. However, this does not fully account for the positive effects. We also find that the fraction of students who participate in home language classes

increases among students with a native background but decreases among students with an immigrant background.

Our results add to the evidence of the positive effects of exposure to migrant students and also support that resource allocation matters. Of particular importance is that we find positive effects in a context with high refugee migration and also during a crisis that puts significant pressure on the receiving schools. The presence of negative effects of recent migrant exposure among students with immigrant backgrounds points to a risk that compensating resource allocation may not sufficiently reach this group of students.

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Appendix A: Additional tables

Table A1. *Countries and regions*

1.	Sweden	
2.	Finland	
3.	Denmark	
4.	Norway and Iceland	
5.	UK and Ireland	
6.	Germany	
7.	Mediterranean Europe	Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain
8.	Continental Europe	Andorra, Austria, Belgium, France, Liechtenstein, Luxembourg, The Netherlands, Switzerland
9.	US and Canada	
10.	Bosnia and Herzegovina	
11.	Former Yugoslavia	Croatia, Kosovo, Macedonia, Serbia, Montenegro, Slovenia, Yugoslavia
12.	Poland	
13.	The Baltic states	Estonia, Latvia, Lithuania
14.	E Europe, Caucasus and C Asia	Albania, Armenia, Azerbaijan, Belarus, Bulgaria, Georgia, Kazakhstan, Kyrgyzstan, Moldova, Romania, Russia, Tajikistan, Turkmenistan, Ukraine, Uzbekistan
15.	Czechia, Slovakia and Hungary	
16.	Mexico and Central America	
17.	Chile	
18.	South America	Argentina, Bolivia, Brazil, Colombia, Ecuador, Guyana, Paraguay, Peru, Suriname, Uruguay, Venezuela
19.	Northeast Africa	Djibouti, Eritrea, Ethiopia, Somalia, South Sudan, Sudan
20.	Middle East and North Africa	Algeria, Bahrain, Cyprus, Israel, Jordan, Kuwait, Lebanon, Libya, Morocco, Palestine, Qatar, Saudi Arabia, Syria, Tunisia, United Arab Emirates, Yemen
21.	West, Central, South Africa	Angola, Benin, Botswana, Burkina Faso, Burundi, Cabo Verde, Cameroon, Central African Republic, Chad, Comoros, Democratic Republic of the Congo, Ivory Coast, Egypt, Equatorial Guinea, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mauritius, Mozambique, Namibia, Niger, Nigeria, Rwanda, Sao Tome and Principe, Senegal, Seychelles, Sierra Leone, South Africa, Swaziland, Tanzania, United Republic of Togo, Uganda, Zambia, Zimbabwe
22.	Iran	
23.	Iraq	
24.	Turkey	
25.	East Asia	China, Hong Kong, Japan, Korea, Taiwan
26	Southeast Asia	Indonesia, Laos, Malaysia, Myanmar, Philippines, Singapore, Thailand, Vietnam
27.	South Asia and Mongolia	Afghanistan, Bangladesh, Bhutan, Brunei, Cambodia, India, Maldives, Mongolia, Nepal, Oman, Pakistan, Sri Lanka, Timor-Leste
28.	Oceania	Australia, New Zealand, Fiji, Kiribati, Micronesia, Nauru, Palau, Papua New Guinea, Samoa, Solomon Islands, Tonga, Vanuatu

Notes: This table shows the countries included for the regions used in the analysis. The categorization is done by the Institute for Evaluation of Labour Market and Education Policy (IFAU) and to a large extent based on the number of immigrants from each region.

Table A2. *Summary statistics when excluding students with no siblings*

	Population		Sample	
	Mean	SD	Mean	SD
Male	0.51	0.50	0.51	0.50
Birth order	1.85	1.00	1.93	1.00
Age in months	151.62	29.64	151.92	29.36
Mother income ptile	52.15	25.28	52.72	25.16
Father income ptile	68.34	26.48	69.51	25.94
Mother education	12.96	2.33	13.01	2.32
Father education	12.40	2.39	12.45	2.39
Predicted score	0.00	0.39	0.01	0.39
Actual score	0.00	0.99	0.01	0.98
Change school	0.06	0.24	0.06	0.24
Contemporary exposure	0.05	0.07	0.05	0.07
Cumulative exposure	0.04	0.05	0.04	0.05
Observations	3,570,199		2,747,060	

Notes: This table presents summary statistics for the key variables in the paper. Population includes all students in compulsory schools during our main study period. Sample includes incumbent students with at least one sibling that we can observe in the school registers. Observations are student-by-year.

Table A3. *Effect of exposure to recent migrants on test scores using an IV*

	Standardized scores		
Exposure:	IV	Red. form	OLS
Contemporaneous	0.236***		0.110***
	(0.057)		(0.035)
Contemporaneous predicted		0.081***	
		(0.020)	
Cumulative	0.415***		0.157***
	(0.064)		(0.046)
Cumulative predicted		0.203***	
		(0.035)	
Grade x Year FE	X	X	X
School x Year FE	X	X	X
Individual controls	X	X	X
Family FE	X	X	X
Mean LHS	0.091	0.091	0.091
SD LHS	0.952	0.952	0.952
Observations	1,702,186	1,702,186	1,702,186
R-squared	0.019	0.672	0.672

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The tables shows the results when the exposure to recent migrants that the student would have had going to the same school and grade as their older sibling is used as an instrument. First column shows the 2SLS coefficient, second column the reduced form coefficient, and third column the OLS coefficient. The dependent variable is our main measure of academic performance in school. When student has taken the (obligatory) national tests in mathematics, Swedish, and English, the outcome is the average of the scores from these tests standardized within grade and cohort in the incumbent student population. If student has missed one of the tests we instead use the course grade in the same subject standardized on the annual level. Exposure is measured at the school-by-grade level. Observations are student-by-year. Standard errors are clustered at the school-by-cohort level.

Table A4. *Taken national test*

	Taken national test			
Exposure:	Math	Swedish	English	All
Cumulative	0.007	0.004	0.007	0.010
	(0.008)	(0.006)	(0.010)	(0.009)
Grade x Year FE	X	X	X	X
School x Year FE	X	X	X	X
Individual controls	X	X	X	X
Family FE	X	X	X	X
Mean LHS	0.963	0.984	0.974	0.612
SD LHS	0.189	0.124	0.160	0.487
Observations	2,366,235	2,445,843	1,589,506	2,445,843
R-squared	0.399	0.452	0.530	0.893

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is a dummy variable for whether the student has taken the national test or not. The fourth column is a binary variable for whether the student has taken all tests. National tests in mathematics and Swedish are taken in grades 3, 6, and 9, while national tests in English is taken in grades 6 and 9. Exposure is measured at the school-by-grade level. Standard errors are clustered at the school-by-cohort and family level.

Table A5. *Effects of migrant exposure in schools with special recent migrant classes*

Exposure:	Standardized scores			
Contemporaneous	-0.256***	-0.193***	-0.047	0.064**
	(0.035)	(0.035)	(0.034)	(0.032)
Special class	-0.015	-0.015	-0.015	-0.016
	(0.011)	(0.011)	(0.011)	(0.010)
Contemporaneous × Special class	0.118	0.098	0.073	0.056
	(0.081)	(0.080)	(0.079)	(0.071)
Cumulative	-1.533***	-1.155***	-0.379***	0.066*
	(0.037)	(0.035)	(0.032)	(0.036)
Special class	-0.025***	-0.033***	-0.031***	-0.025***
	(0.010)	(0.010)	(0.009)	(0.008)
Cumulative × Special class	0.402***	0.440***	0.338***	0.220***
	(0.087)	(0.084)	(0.078)	(0.070)
Grade x Year FE	X	X	X	X
School x Year FE	X	X	X	X
Individual controls		X	X	X
Family controls			X	
Family FE				X
Mean LHS	0.014	0.014	0.014	0.014
SD LHS	0.984	0.984	0.984	0.984
Observations	2,747,060	2,747,060	2,747,060	2,747,060
R-squared	0.142	0.165	0.257	0.624

Notes: The dependent variable is our main measure of academic performance in school. A special class is defined as a classroom consisting of at least 90% recent migrants. See Figure B2 for the distribution of recent migrants across classrooms. The regressions are run separately for contemporaneous and cumulative exposure. Observations are student-by-year and includes incumbent students with at least one sibling that we can observe in the school registers. Incumbent students are defined as students born in Sweden. Standard errors are clustered at the school-by-cohort level and by family.

Table A6. *Effect of exposure to recent migrants on grades and test scores in different subjects*

	Standardized grades				Standardized test scores			
Exposure:	Math	Swedish	English	All	Math	Swedish	English	All
Contemporaneous	-0.007	0.066	0.021	0.034	0.020	0.149***	0.072	0.089***
	(0.046)	(0.048)	(0.047)	(0.045)	(0.033)	(0.033)	(0.046)	(0.031)
Cumulative	-0.014	0.033	0.107**	0.053	0.013	0.123***	0.137***	0.105***
	(0.044)	(0.045)	(0.047)	(0.044)	(0.038)	(0.038)	(0.049)	(0.037)
Grade x Year FE	X	X	X	X	X	X	X	X
School x Year FE	X	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X	X
Family FE	X	X	X	X	X	X	X	X
Mean LHS	0.026	0.015	0.001	0.016	0.024	0.011	-0.004	0.014
SD LHS	0.997	0.992	0.993	0.994	0.988	0.981	0.997	0.984
Observations	1,725,411	1,725,411	1,725,411	1,725,411	2,290,813	2,405,432	1,541,451	2,419,567
R-squared	0.695	0.694	0.676	0.723	0.603	0.562	0.683	0.628

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Notes: The regressions are run separately for contemporary and cumulative exposure. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Standard errors are clustered at the school-by-cohort and family level.

Table A7. *Grade generosity relative to test scores*

	Grade generosity relative to test scores			
Exposure:	Math	Swe	Eng	All
Contemporaneous	0.069*	-0.167***	-0.026	-0.062**
	(0.037)	(0.039)	(0.032)	(0.031)
Cumulative	0.032	-0.116***	-0.074**	-0.051*
	(0.032)	(0.037)	(0.029)	(0.027)
Grade x Year FE	X	X	X	X
School x Year FE	X	X	X	X
Individual controls	X	X	X	X
Family FE	X	X	X	X
Mean LHS	0.069	0.037	0.047	0.032
SD LHS	0.447	0.538	0.423	0.394
Observations	1,209,395	1,335,861	1,328,293	1,347,952
R-squared	0.503	0.476	0.488	0.516

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is the difference in the standardized grade and the standardized test score. A higher positive value means that the grade is relatively higher than the performance on the national test. Exposure is measured at the school-by-grade level. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Standard errors are clustered at the school-by-cohort and family level.

Table A8. *Effect of exposure to recent migrants on standardized scores and grades in grades 3, 6, and 9.*

	Standardized scores			Standardized grades	
Exposure:	Grade 3	Grade 6	Grade 9	Grade 6	Grade 9
Contemporaneous	0.017	0.064**	0.033	0.084**	0.017
	(0.042)	(0.032)	(0.031)	(0.035)	(0.031)
Cumulative	0.078	0.085*	0.167***	0.104**	0.105**
	(0.051)	(0.044)	(0.048)	(0.050)	(0.049)
Year FE	X	X	X	X	X
School FE	X	X	X	X	X
Individual controls	X	X	X	X	X
Family FE	X	X	X	X	X
Mean LHS	0.030	0.014	0.017	0.023	0.020
SD LHS	0.945	0.995	0.996	0.993	0.993
Observations	660,362	819,340	896,151	598,641	893,278
R-squared	0.644	0.708	0.736	0.741	0.736

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions are run separately for each grade. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Incumbent students are defined as students born in Sweden. Including family-fixed effects means that we only include observations where we have data on siblings in the same grade. Standard errors are clustered at the school-by-cohort and family level.

Table A9. *Classroom size as outcome*

	Classroom size		
Exposure:	Native background	Immigrant background	Total
Contemporaneous	-0.197	-0.612**	-0.378**
	(0.176)	(0.259)	(0.164)
Grade x Year FE	X	X	X
School x Year FE	X	X	X
Individual Controls	X	X	X
Family FE	X	X	X
Mean LHS	22.294	22.547	22.324
SD LHS	5.993	5.689	5.958
Observations	8,933,498	1,349,323	10,288,702
R-squared	0.707	0.648	0.697

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is the number of students in the classroom. See Figure B2 for the distribution of students across classrooms. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Incumbent students are defined as students born in Sweden. Standard errors are clustered at the school-by-cohort and family level.

Table A10. *Classroom size included in individual controls*

Exposure:	Standardized scores			
Cumulative	-1.464***	-1.077***	-0.323***	0.108***
	(0.036)	(0.035)	(0.031)	(0.036)
Grade x Year FE	X	X	X	X
School x Year FE	X	X	X	X
Individual controls		X	X	X
Family controls			X	
Family FE				X
Mean LHS	0.009	0.009	0.009	0.009
SD LHS	0.985	0.985	0.985	0.985
Observations	2,491,025	2,491,025	2,491,025	2,459,224
R-squared	0.148	0.171	0.263	0.636

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is our main measure of academic performance in school. When the student has taken the national tests in mathematics, Swedish, and English, the outcome is the average of the scores from these tests standardized within the cohort of incumbents. If the student has missed one of the tests, we instead use the teacher set grade in the corresponding subject (also standardized within the cohort of incumbents). The regressions are run separately for contemporaneous and cumulative exposure. We do not have complete coverage on classroom identifiers, which means that there are slightly fewer observations in this regression compared to our main analysis. Observations are student-by-year and include incumbent students with at least one sibling that we can observe in the school registers. Incumbent students are defined as students born in Sweden. Standard errors are clustered at the school-by-cohort and family level.

Table A11. *Effects of exposure to recent migrants on participation in home language classes*

	Participation in home language classes	
Exposure:	Native background	Immigrant background
Contemporaneous	0.082***	-0.033
	(0.016)	(0.027)
Cumulative	0.062***	-0.104***
	(0.019)	(0.037)
Year FE	X	X
School FE	X	X
Individual controls	X	X
Family FE	X	X
Mean LHS	0.050	0.418
SD LHS	0.219	0.493
Observations	1,590,661	198,559
R-squared	0.569	0.634

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The dependent variable is a binary variable for participating in home language classes, 1 if participating and zero otherwise. The number of observations are fewer than our main analysis sample due to home language classes only being available for grade 6 and 9 and not for all years in our data.

Table A12. *Differences-in-Differences*

	Standardized scores	
Exposure:	2008–2018	2008–2021
Contemporaneous × post	0.183**	0.178**
	(0.087)	(0.084)
Year FE	X	X
Grade FE	X	X
School FE	X	X
Individual controls	X	X
Family FE	X	X
Mean LHS	0.019	0.017
SD LHS	0.998	0.996
Observations	634,443	896,151
R-squared	0.744	0.736

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the effect of exposure to recent immigrants on test scores. Post is a dummy variable equal to one if year $\geq$ 2015. The first column has a post period up until 2018 and the second column up to 2021.

Appendix B: Additional figures

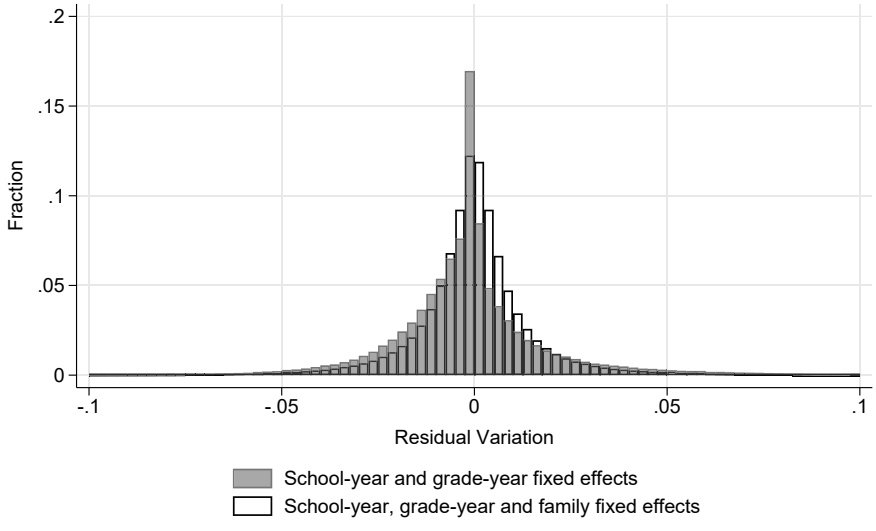
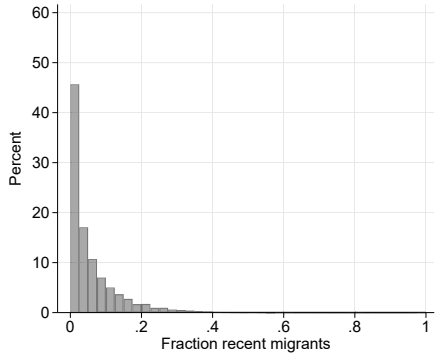
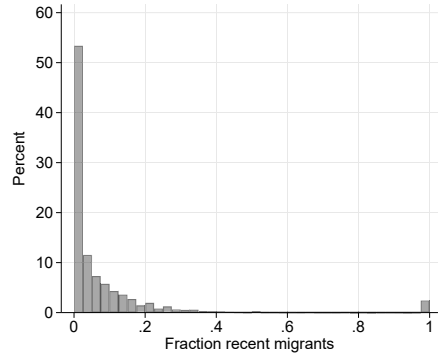


Figure B1. Distribution of residuals. The figures shows the distribution of residuals for our main model when excluding and including family fixed effects.

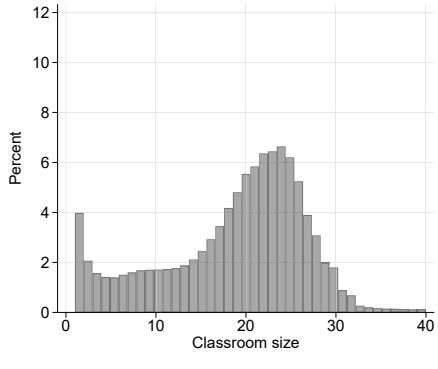
(a) Fraction recent migrants



(b) Fraction recent migrants per classroom



(c) Distribution of number of students per classroom



(d) Distribution of number of students per classroom if fraction of recent migrants > 0.9

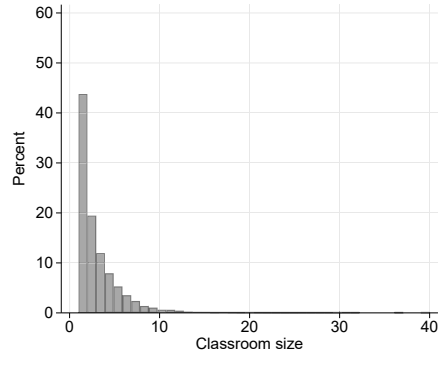
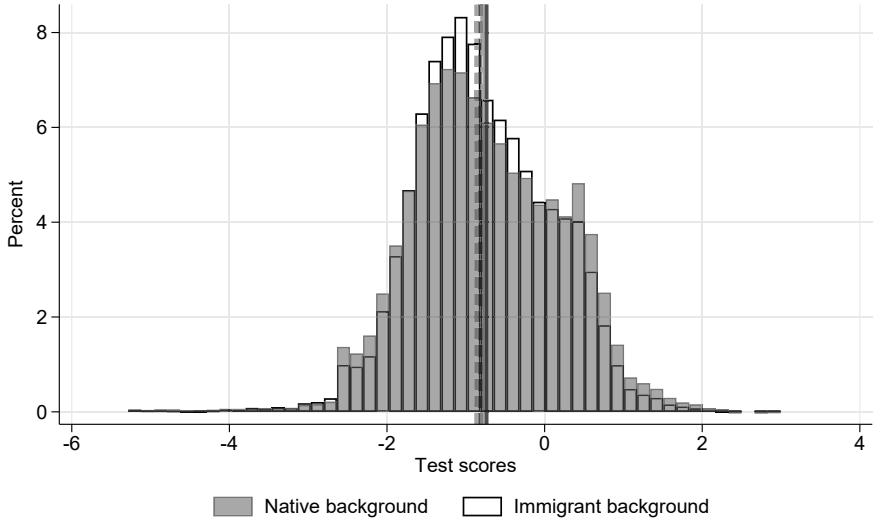


Figure B2. Distribution of recent migrants across classrooms. The upper panel shows the fraction of recent migrants per student (a) and per classroom (b) during our main study period (2008–2022). The lower panel shows the distribution of students per classroom (c) and the distribution of students per classroom in classes where at least 90 percent are recent migrants (d).

(a) Actual test scores



(b) Predicted test scores

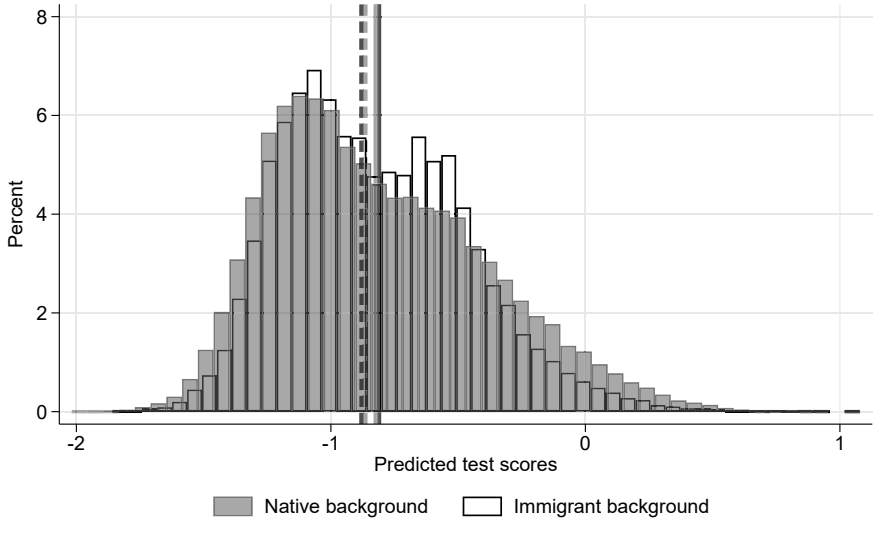


Figure B3. Distribution of test scores for recent arrivals. The figures shows the distribution of actual (upper) and predicted (lower) test scores among recent arrivals that students with native and immigrant background are exposed to, respectively. The solid dark (light) gray line shows the mean test score of newly arrived immigrants that native (immigrant background) students are exposed to. The dashed dark (light) gray line shows the median test score of newly arrived immigrants that native (immigrant background) students are exposed to.

(a) Years 2008–2014

(b) Years 2015–2017

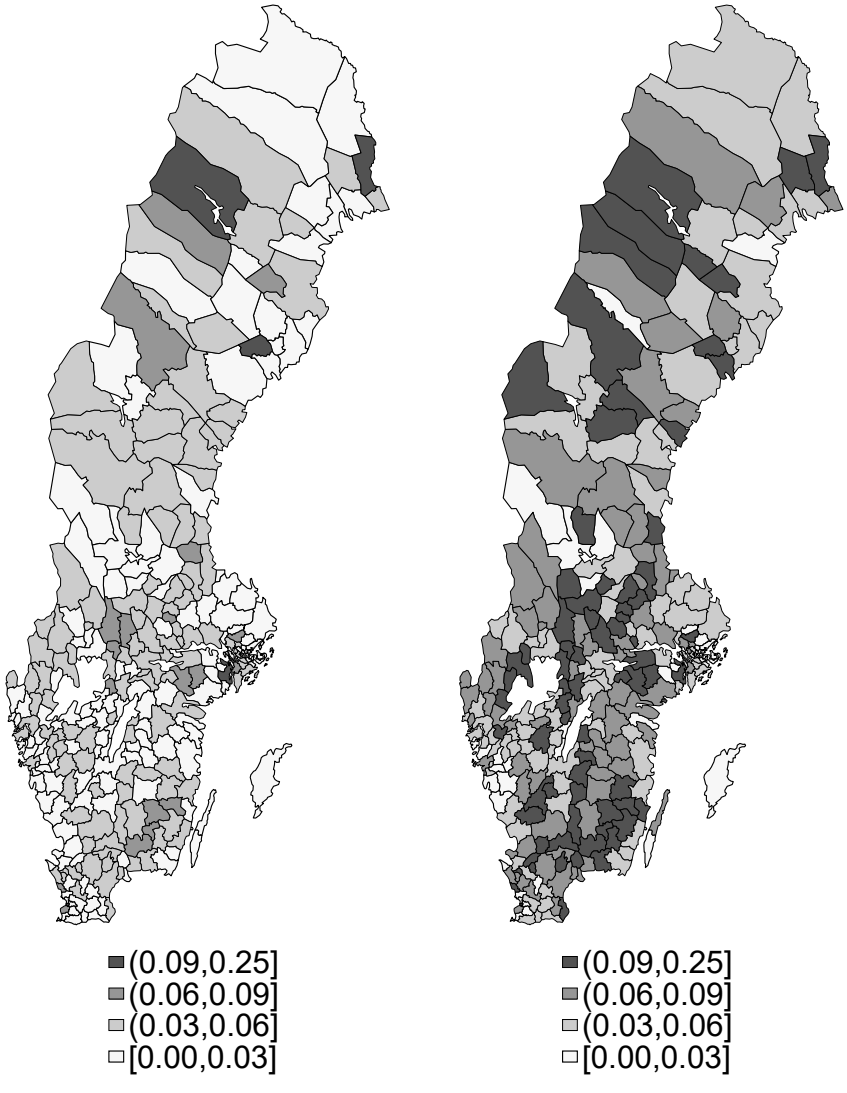


Figure B4. Share of recent migrants in schools in grade 9 by municipality. The figure shows spatial variation in the share of students that are recent migrants. Darker regions have a higher share of recent migrants.

(a) Standardized scores—Without Family FE (b) Standardized scores—With Family FE

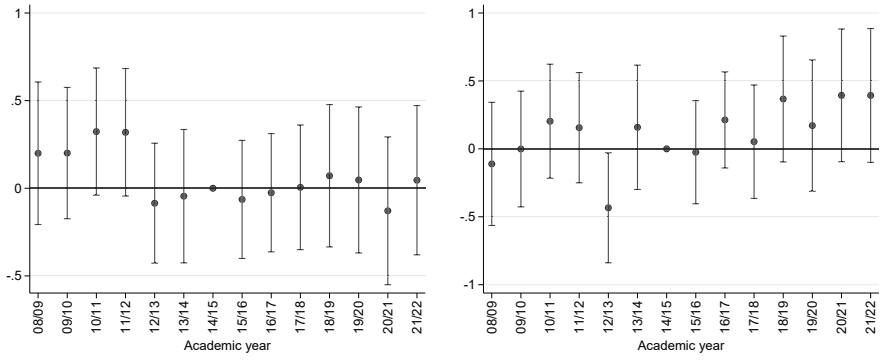


Figure B5. Crisis exposure for incumbent students with an immigrant background. The figure shows the estimated coefficient of crisis exposure on student outcomes for students with immigrant background. Incumbent students are defined as students born in Sweden and immigrant background as a native student with two foreign-born parents. The dependent variable is our main measure of academic performance. See Section 7 for a description of the identification.

(a) Classroom size—With Family FE (b) Home language classes—With Family FE

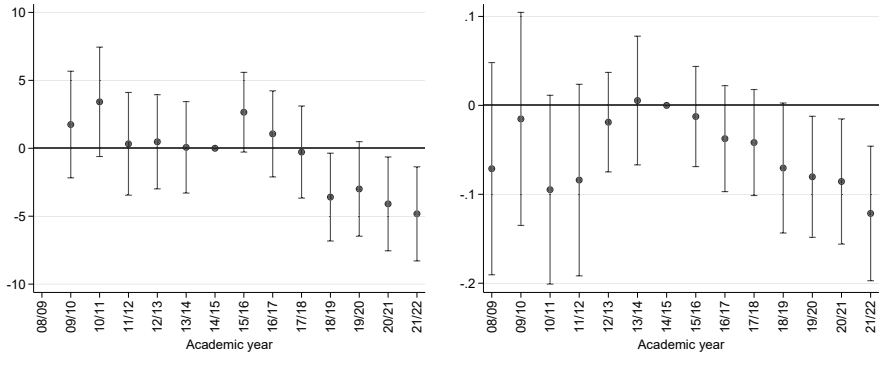


Figure B6. School responses to crisis exposure. The figure shows the estimated coefficient of crisis exposure on classroom size and probability for students to participate in home language classes. See Section 7 for a description of the identification.

Essay IV. The Labor Market Impact of a Taxi Driver's License

Co-authored with Mounir Karadja

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1 Introduction

Immigrants in developed countries generally have weaker labor market outcomes than their native counterparts (Fasani, Frattini, and Minale, 2022; Frattini and Bertino, 2023). Yet, despite facing overall challenges in the labor market, certain sectors and occupations tend to have very high shares of immigrants among employees. For example, in Sweden, immigrants make up 47 percent of taxi drivers and 48 percent of bus drivers (Statistics Sweden, 2017). This fact raises the question of what role these jobs—service sector occupations with low formal qualification requirements—play in the employment of immigrants. What would be the labor market effect of expanding these sectors or offering greater access to them?

This paper studies the importance of access to the taxi labor market for the economic outcomes of both immigrants and natives in Sweden. Examining the impact of access to particular occupations is made difficult by the fact that access is rarely determined for individual occupations. Moreover, it is seldom observed nor easy to define whether an individual has access to a particular labor market. To circumvent this issue, we use previously unexploited administrative data covering all written exam results for the taxi driver's license in Sweden between 2004 and 2017. These data allow us to clearly define access to the taxi-driving labor market on an individual level based on whether or not an individual has passed the requisite written exams. We combine data on the precise date of passing exams for over 25,000 test takers with high-frequency administrative data on individual labor market outcomes, as well as yearly data on usage of social insurance systems such as welfare benefits and unemployment insurance.

Our empirical strategy uses difference-in-differences and event-study designs. Our main specifications focus on individuals before and after they gain access to the taxi driver labor market and compare them to a sample of individuals who have not yet gained access.[1] We begin our analysis by using sector-specific employment spell data to estimate how passing the written taxi license exams affects employment and income derived from the taxi sector.[2] Our estimates indicate an immediate positive effect. Six months after passing written exams, immigrants and natives display large increases in the likelihood of employment in a taxi firm. One year later, approximately 60 percent of individuals in both groups had been employed by a taxi firm for at least one month, whereas pre-treatment averages were stable at zero. Over the same period, monthly taxi incomes increase by approximately 9,000 SEK for immi-

[1] We also replicate our results using the event-study estimator proposed by Dube et al. (2023), which takes into account recent discoveries about potential sources of bias in event-study estimation. In particular, it uses never-treated individuals in the control group.

[2] Obtaining a taxi driver's license requires passing three written exams as well as a driving test. We only observe written exam results, while data on driving test results or ultimate ownership of taxi drivers' licenses are restricted by law and not allowed for research. See Section 2.3 for additional details.

grants and 6,000 SEK for natives. Thus, passing the necessary written exams for the taxi driver's license impacts the likelihood of working as a taxi driver.

Selection into taxi driving is not random. Before passing written taxi exams, immigrants in treatment and control groups display parallel trends until 12 months before treatment, after which the treatment group's income declines significantly. This divergence may be due to several factors, such as separation from prior jobs, anticipation effects leading to lower labor supply, or investment costs related to preparing for exams and job search limiting labor supply. Pre-treatment patterns differ somewhat for natives, who experience more pronounced declines in income levels prior to passing written taxi exams. In percentage terms, however, pre-trends are similar for both groups. We discuss the possible causes of these diverging pre-trends and trace out their implications for interpreting our post-treatment estimates, arguing that, if anything, pre-trend patterns would bias our estimates towards zero. A separate test also indicates that our results are unlikely to be explained by an Ashenfelter's-dip type mechanism.

Our main results regard the impact of access to the taxi labor market on total monthly employment and income. While outcomes were relatively similar across origin groups when looking at taxi sector activity, differences in outside options in the labor market for natives and immigrants may lead to differences in the overall labor market effects. For example, descriptive statistics indicate that immigrants are more likely to enter the taxi labor market from unemployment, while natives are more likely to have been employed. Our results are consistent with this. 36 months after passing exams, immigrants' overall employment rate increases by 20 percentage points, while it increases by 11 percentage points for natives.

Interestingly, the effect on incomes across groups is larger. For immigrants, we observe a substantial and stable income increase after passing the written taxi exams. Between 12 and 36 months after gaining access to the taxi labor market, their monthly incomes are at least 5,000 SEK higher. When estimating the relative increase in incomes compared to pre-taxi averages, those with access to the taxi labor market have over 40 percent larger monthly incomes between 12 and 36 months after treatment. Natives also experience a rapid increase in monthly incomes after passing taxi exams. However, estimates of natives' incomes between 12 to 36 months after treatment indicate smaller increases of approximately 1,500 SEK per month compared to the control group. This effect represents an increase of roughly 10 percent. Thus, natives who enter the labor market can rapidly reverse their prior labor market decline, reaching higher incomes than the control group. Nevertheless, the overall effect is smaller than for immigrants.

Having observed positive income developments for individuals who pass the written taxi exams, a relevant question is whether there is an impact on the usage of social insurance programs. To measure this, we create a simple measure that takes value 1 if an individual does not receive any means-tested

welfare or housing transfers in a given year. We also study the usage of unemployment insurance (UI). For both natives and immigrants, the likelihood of receiving UI benefits increases in the year before passing taxi exams. This result is consistent with the declines in income and employment rates observed during this period. However, both groups quickly reversed this trend after treatment and became less likely to use UI after 3 years. We find similar results for means-tested transfers that are more pronounced for immigrants. Immigrants are 12.5 percentage points less likely to receive any transfers after 3 years, representing a 25 percent decrease over the mean two years pretreatment.

These results indicate that the taxi labor market has a more significant impact on immigrants than on natives, which is evident both in levels as well as in relative terms. We argue that our results are in line with immigrants having weaker outside options in the labor market, making access to taxi driving a more pivotal outcome than for natives. In line with this, we find that longevity in the occupation differs between groups. Using data on the largest source of income, we find that immigrants who pass the taxi exams show no decline in taxi driving as their main occupation throughout our sample period. Thus, we find no indication that immigrants use taxi driving as a stepping stone to other types of occupations within our time frame. Natives, by contrast, display a peak in taxi driving as their main occupation in the first 12 months after passing exams, after which it decreases gradually. Taxi driving for immigrants, therefore, appears to represent a more stable and long-standing shift in labor market status, while it is a more temporary occupation for natives.

We next investigate to what extent our results reflect differences in personal characteristics. Within the sample of immigrants, we find that the returns to taxi labor market access are substantially larger among those who have arrived relatively recently. For recent arrivals (between 3 and 5 years in Sweden), the effect on monthly income is twice as large as for immigrants who have been in Sweden for longer than five years. Next, investigating the heterogeneous effects of education, we report two findings. First, earnings effects are much larger for immigrants with a post-secondary degree than for similarly educated natives. Second, while highly educated immigrants earn more than low-educated immigrants, the pattern is reversed among natives. These results indicate that the taxi labor market has larger benefits for immigrants in part because it can absorb individuals with weak outside options, either due to having less experience in Sweden or individuals who find it challenging to find work with higher formal requirements.[3]

To our knowledge, this study is the first to estimate the effects of a taxi driver's license on labor market outcomes. The economics literature on taxi

[3]There are several reasons for which the taxi labor market may be particularly significant for individuals will weak labor market options, such as having very free wage setting and lower employment protection, likely contributing to low barriers to entry.

drivers has often tested theories of labor supply (Camerer et al., 1997; Farber, 2005). Häckner and Nyberg (1995) and Cairns and Liston-Heyes (1996) study the economics of taxi market regulation theoretically, while Ohlson (2008), SOU (2010), and Slavnic and Urban (2018) investigate the consequences of the Swedish taxi market deregulation of 1990. Several studies focus on the effects of car ownership on labor market outcomes (Ong, 2002; Raphael and Rice, 2002; Gurley and Bruce, 2005; Baum, 2009). Gautier and Zenou (2010) focus specifically on the impact for ethnic minorities. Moreover, recent research has studied the impact of allowing undocumented immigrants to obtain a regular driver's license (Amuedo-Dorantes, Arenas-Arroyo, and Sevilla, 2020; Cho, 2022). These studies indicate positive effects, partly related to the ability to commute longer distances. There is also a growing literature on the role of ride-hailing services on labor market outcomes (Berger, Chen, and Frey, 2018) and the role of work-hour flexibility (Angrist, Caldwell, and Hall, 2021). Adermon and Hensvik (2022) investigate the effects of gig-work experience on the job search, and Ek, Hammarstedt, and Skedinger (2020) studies the importance of low-skilled jobs for future earning trajectories for newly arrived refugees in Sweden.

Our study also relates to the literature on occupational licensing, as the taxi driver's license acts as an occupational regulation that raises barriers to entry and enforces minimum standards among workers. In the US and the EU, approximately one-quarter of workers hold an occupational license. Several studies investigate the role of occupational regulations, including their effects on immigrants (Chapman and Iredale, 1993; Kugler and Sauer, 2005; Gomez et al., 2015; Sweetman, McDonald, and Hawthorne, 2015; Tani, 2017). Brücker et al. (2021) find that immigrants who can validate their foreign qualifications and access regulated labor markets experience positive employment outcomes.

2 Background and institutional setting

2.1 Immigration to Sweden

Figure 1 shows two trends for the immigrant composition in Sweden between 1985 and 2018. First, Sweden has had a high inflow of immigrants in the last decades, and the stock of foreign-born people has steadily increased from around 8 percent in 1985 to 19 percent in 2018. Second, there has been a shift in the composition of immigrants over time, where non-Western immigrants made up 36 percent of the immigrant stock in 1985 and 78 percent in 2018.

Figure 2 shows that the number of immigrants from Africa and the Middle East has increased the most since 1985 and that it is the largest immigrant group in 2018 (constituting 38 percent of the foreign-born population in Sweden). Within this group, the main source countries for the immigrant inflows in the 1980s and 1990s were Iran, Iraq, Turkey, and Lebanon. During the 2000s

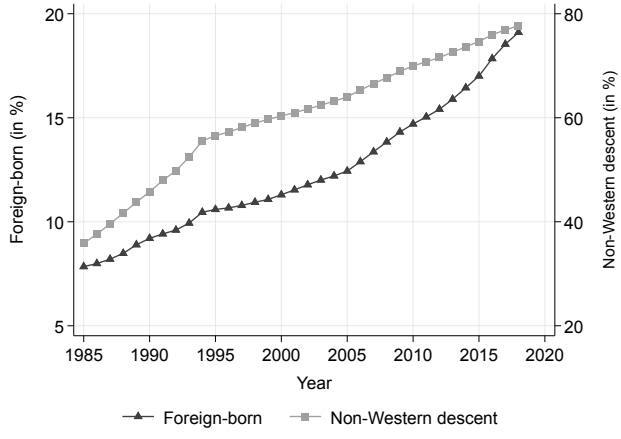


Figure 1. Foreign-born and the fraction of foreign-born with non-Western descent in Sweden between 1985 and 2018. *Source:* Statistics Sweden.

and 2010s, there was an increased inflow from primarily Iraq, Afghanistan, Somalia, Eritrea, and Syria. As is further discussed in Section 3, Africa and the Middle East are also the main source regions for immigrant taxi drivers in Sweden. Figure 2 furthermore shows that the group that has increased second most during the period 1985–2018 is East Europe, where there are two trend shifts. First in the mid-1990s, when there was a large spike in asylum seekers from former Yugoslavia, and second in the mid-2000s with the expansion of the EU (increased labor immigration from primarily Poland). Immigration from Asia and Oceania has mainly been coming from Thailand, China, and India, while Chile is the dominant source country for Latin America.

2.2 The taxi market

Roughly half of the taxi drivers in Sweden are foreign-born, and its workforce has the eighth-highest share of foreign-born individuals as of 2019. This statistic is in line with other high-income countries. In Canada, half of the taxi drivers were foreign-born in 2006 (Xu, 2012), while 1 in 7 taxi drivers in the UK is from Pakistan alone (Dawn, 2013). As shown in Table 1, being a taxi driver is the 20th most common profession for foreign-born men in Sweden in 2019.

Along with the development in many other OECD countries (Bekken, 2005), the labor market for taxi drivers was deregulated in Sweden in 1990 (Slavnic and Urban, 2018; Swedish Competition Authority, 2018). This deregulation meant that the limit on the total number of taxi cars was removed along with price regulations. The reform intended to increase competition in the market, but it also introduced stricter requirements for obtaining a taxi driver's license. Since the deregulation in 1990, both the number of taxi drivers and the fraction

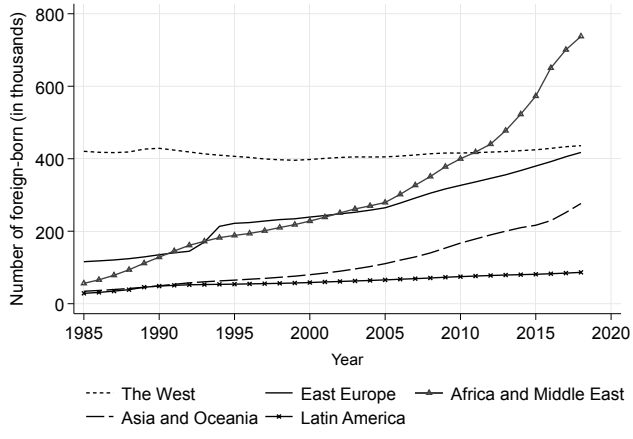


Figure 2. Regional origin of the stock of foreign-born in Sweden between 1985 and 2018. *Source:* Statistics Sweden.

of foreign-born taxi drivers have increased. There were over 17,000 registered taxi cabs in Sweden in 2016, up from 12,700 in 1990. The Stockholm region is the largest single market, with nearly 7,000 registered taxi cabs.

Demand for taxis comes from three types of clients: business, private, and publicly procured services such as medical and school transports. Publicly procured transports typically compose half of taxi firms' incomes (Svenska taxiförbundet, 2017). Even though the number of taxi cars and drivers has increased since 1990, there has been an increased consolidation in the market with fewer and relatively bigger taxi companies (Svenska taxiförbundet, 2017; Swedish Competition Authority, 2018). Many taxis are connected to "brand name" firms that act as franchises. Smaller firms, often owning only one or two taxis, can pay a fee to be connected to the brand name. By paying a fee and following the franchise firm's guidelines, they can connect to the franchise's centralized booking services in which clients reserve taxis via telephone or apps. In 2016, the most common type of taxi firm owned a single taxi cab (4,459 firms), while there were 2,315 active limited liability corporations in the sector (Svenska taxiförbundet, 2017).

A majority of taxi drivers are paid on commission. While there are collectively bargained agreements negotiated between unions and employers, these are voluntary for employers to adopt. Employers who do not adopt the collectively bargained agreement may set compensation freely, with no minimum wage requirement. A typical example is that a driver may keep 50 percent of their revenue, sometimes combined with an hourly wage.

Table 1. *Most common occupations for foreign-born men in Sweden in 2019*

Occupation	Foreign-born men	Total men	Foreign-born share
Warehouse and terminal staff	19241	68379	28%
Restaurant and kitchen helpers	16943	28190	60%
Cleaners and helpers in offices, hotels, etc.	14964	20726	72%
Home-based personal care and related workers	12404	20399	61%
Software- and system developers	12142	65773	18%
Bus and tram drivers	11912	20639	58%
Machine-tool operators	9869	43093	23%
Cooks and cold-buffet managers	9568	20616	46%
Heavy truck and lorry drivers	7966	53590	15%
Building caretakers	6711	34995	19%
Personal assistants	6550	18834	35%
Woodworkers, carpenters	6348	45886	14%
Assistant nurses, home care and elderly homes	6296	12401	51%
Primary school teachers	5807	27960	21%
Other service workers not elsewhere classified	5780	16428	35%
Shop sales, groceries	5729	30054	19%
Vehicle mechanics and repairers	5391	32240	17%
Shop sales, specialty stores	5268	43135	12%
Commercial sales representatives	5216	64397	8%
Taxi, car, and van drivers	5182	9588	54%

Notes: Occupations are based on 4-digit SSYK 2012 occupational codes, which are the occupational codes used by Statistics Sweden. SSYK 2012 is based on ISCO–08. Occupations are ranked in descending order according to the frequency of foreign-born men. *Source:* Statistics Sweden.

2.3 Becoming a taxi driver

A taxi driver's license is required to work as a taxi driver. Obtaining the taxi driver's license requires that the individual (i) is at least 21 years old, (ii) has held a standard driver's license[4] for at least two years without any revocation during that period, (iii) completes three written exams, and (iv) completes a road driving test. The individual must also pass the requirement on obedience to the law and medical requirements.

The written taxi exams in step (iii) must be completed within six months of each other but in no particular order. The exams cover three different subjects: (i) navigation and map reading, (ii) safety and good driving behavior, and (iii) laws and regulations (Swedish Transport Agency, 2021). There is a small fee of approximately 300 SEK per test, and exams are relatively easy and fast to schedule. In our data, conditional on passing all three tests, the average time from the first exam attempt to passing the last exam was 3 months. We note that this does not include the time it takes to study for the exams.

In addition to the written exams, a driving test must be passed to obtain a taxi driver's license. The driving test is largely similar to that for a regular driver's license, and it also includes tests of the ability to navigate using GPS and the ability to ensure passenger safety. As we do not observe information on driving tests nor on whether individuals obtain the final taxi driver's license, it is reassuring that the road test is very similar to a regular driver's license road test. Our prior is that this step is not a significant barrier to obtaining a taxi driver's license. Below, we will test the relationship between passing the written exams and indicators of actually working in the taxi sector, a measure of having passed all tests and obtained a taxi driver's license.

A foreign driver's license issued within the EEA fulfills requirement (ii) if the individual has sufficient documentation from the issuing agency and can show that it has not been revoked during the last two years. A driver's license issued in a non-EEA country is not valid as a driver's license when applying for a taxi driver's license. Hence, for a majority of immigrants that we define as non-Western, a Swedish standard driver's license will be required in order to drive a taxi.

3 Data and descriptive statistics

We use data on all scores for the written taxi driver's license exam in Sweden between 2005 and 2017 provided by the Swedish Transport Agency. For each exam taken, we observe a pseudonymized identifier for the individual taking it, a test center identifier, the type of exam taken, the score, and whether or not the individual passed the exam. The data on written exam scores are matched at the individual level to complete population register data on demographic

[4]This corresponds to types *B* or *D* in the Swedish driver's license nomenclature.

variables (LISA) and linked employer–employee data (RAMS) provided by Statistics Sweden. For each individual, we have information on the duration of an employment spell (start and end months), total income per spell, industry, sector, and employer. We observe the employment and income history of all individuals, both within the taxi sector and all other sectors. In all of our analyses, we include both incomes from employment and self-employment, and we thus include income derived from self-employed taxi drivers.

To create a measure of monthly income, we transform the spell-level data as follows. The first step is to divide the total income for a given spell by the spell's duration in months, such that we have the average monthly income during the spell. Note that spells are specified by year and can, therefore, be at most 12 months long, from January to December. As individuals may have overlapping spells from different employers, we then take the sum of monthly incomes across all active spells in a given month. As a second step, we refine this approach to avoid a misreporting error caused by the tendency of employers to report spells as having a duration of one year, January to December, even when spells may, in reality, be shorter. We apply a correction for such misreporting errors only for spells that occur during the year in which an individual passes the required written taxi exams. For such spells, we consider any income coming from taxi firms as misreported if it occurs in the months prior to passing the written tests. We thus shift any taxi firm income to the remaining months of the spell instead.

Note that we only perform this correction for *taxi* income occurring in the *same* calendar year as individuals pass the written taxi exams. For example, consider an individual who passes the exams in January and an employment spell at a taxi firm in the preceding months, i.e., in the preceding calendar year. In this case, we will not make any changes to the data and thus can detect potential pre-trends in taxi incomes. If the individual instead passes the exams in March, we shift any reported taxi income from January and February to the remaining months of the employment spell. Again, however, any taxi income occurring before January is left unchanged in the data. Thus, we only shift away taxi income arriving in the same year as passing written exams, but in the months before passing the exams. To check for the robustness of this correction, in Section 6, we also display results without the correction using the sub-sample of individuals who passed their tests from January to April in a given year, for whom the issue of misreported income will be the smallest.

In our main analysis, we divide the population into two groups: individuals of non-Western origin arriving in Sweden at an age older than 15 ("immigrants") and individuals born in Sweden ("natives"). This separation is motivated by previous research on labor market outcomes for individuals with an immigrant background. The main difference in outcomes between natives and immigrants is for immigrants originating in non-Western countries and arriving in Sweden at an older age (Eriksson, 2010; Åslund, Hensvik, and Skans, 2014). Place of birth is given by a variable that either identifies the specific

Table 2. *Regions of birth and characteristics for the main sample of taxi drivers*

Region	Count	Age	Years in Sweden
Afghanistan	544	33.6	5.9
Africa	241	38.9	8.9
Bosnia and Herzegovina	358	38.4	10.7
Central America	19	43.9	11.2
Central Asia	118	37.6	4.4
Chile	47	46.5	18.6
China	33	39.5	6.6
East Europe	299	36.1	7
Egypt	46	36.1	6.9
Eritrea	225	38.4	9.5
Ethiopia	264	39.3	12.5
India	56	36.7	10.3
Iran	954	41.2	12.2
Iraq	3895	35.2	6.4
Lebanon	315	37.9	12.1
North Africa and Middle East	542	36.4	8.2
Other	37	38	10.3
Poland	118	44.3	15.6
Romania	47	39.4	11.6
Somalia	933	32.2	5.8
South America	74	42.5	12.8
South Asia	250	37	9.2
South-East Asia	25	39.3	12.6
Sweden	16095	35	—
Syria	231	38.1	10.4
Turkey	526	36	10.9

Notes: The table shows the country or region of birth for the individuals included in our main analysis. For privacy reasons, data on the country of birth are grouped at the regional level for source countries with relatively few observations. "Other" includes the Baltic, East Asia, North America, Oceania, and Thailand. "Age" and "Years in Sweden" refer to the average of two years before passing the written exams.

country or a group of countries (in the case that the number of immigrants from that country is very few in numbers).

Table 2 displays the frequency distribution of countries of origin among our sample. Out of 26,292 individuals who passed the written taxi exams between 2004 and 2017, 16,095 (61 percent) were born in Sweden. Among immigrants, the most common country of origin is Iraq (3,895 individuals), followed by Iran (954) and Somalia (933). Further down are Afghanistan (544), North Africa and the Middle East excluding Iran and Iraq (542) and Turkey (526). Hence, all of the most common source countries for taxi drivers are located in Africa and the Middle East. We can also see some differences in the average age and years in Sweden before taking the tests to become a taxi driver.

Table 3 displays descriptive statistics for our sample and the total population, split between immigrants and natives. Taxi drivers are observed two years prior to passing exams, while the total population figures are averaged over the sample period. Taxi drivers are mostly male and younger than the population as a whole. Compared to non-taxi driving counterparts of the same origin, taxi drivers have lower earnings, are more likely to receive UI and welfare benefits, and have lower rates of higher education. However, the education gap is notably larger among natives, for which taxi drivers are 13 percentage points less likely to have higher education. In contrast, the difference among immigrants is less than a percentage point.

Immigrant taxi drivers also differ from their native counterparts in several regards. Firstly, immigrant drivers are almost exclusively male (97 percent versus 80 percent among natives), have 32 percent lower pre-taxi incomes, and are more likely to have been unemployed (though no more likely to receive UI benefits), and 37 percentage points more likely to receive welfare benefits. The average years of education are similar across the two groups. However, this masks substantial heterogeneity. There is a much larger fraction of higher-educated immigrants than natives, with 36.6 percent and 20.8 percent having more than 12 years of schooling, respectively. This is explained by studying the full distribution of schooling, as displayed in Figure 3. While distributions for both immigrants and natives are centered around high-school completion, immigrants have a more evenly dispersed distribution of education levels compared to natives, where the majority have either 11 or 12 years of education. Immigrants instead have higher shares of individuals with up to 10 years and at least 14 years of education.

Figure 4 displays distributions of years in Sweden and age at the time of passing exams. There is substantial variation in years since migration, which peaks at five years before slowly decreasing with time in Sweden. Age distributions are very different across natives and immigrants, with the most common test-taking ages among natives being just above the legal threshold of 21 years of age. Immigrants, by contrast, arrive at different ages, and their distribution is more bell-shaped.

Table 3. *Comparison of taxi drivers and non-taxi drivers*

	Taxi drivers		Others	
	Immigrants	Natives	Immigrants	Natives
Male	0.972	0.802	0.483	0.510
	(0.165)	(0.398)	(0.500)	(0.500)
Age	36.43	35.03	41.64	43.16
	(8.844)	(13.56)	(10.38)	(12.70)
Income	8807.0	12923.6	9057.9	16588.5
	(10072.7)	(12193.5)	(11934.7)	(15677.3)
No UI Benefits	0.812	0.811	0.864	0.899
	(0.390)	(0.392)	(0.343)	(0.301)
No Welfare Benefits	0.510	0.882	0.684	0.934
	(0.500)	(0.323)	(0.465)	(0.248)
Years of schooling	11.72	11.74	11.97	11.99
	(2.635)	(1.708)	(2.827)	(2.322)
Higher education	0.366	0.208	0.373	0.336
	(0.482)	(0.406)	(0.484)	(0.472)
Days in unemployment	74.42	35.00	27.82	12.01
	(108.6)	(73.79)	(70.49)	(46.32)
Observations	10,197	16,095	2,481,669	31,206,682

Notes: The table includes the individuals in our main analysis separated by immigrant status. "Income" is given in average monthly income in SEK. All variables for taxi drivers refer to two years before passing the written exams.

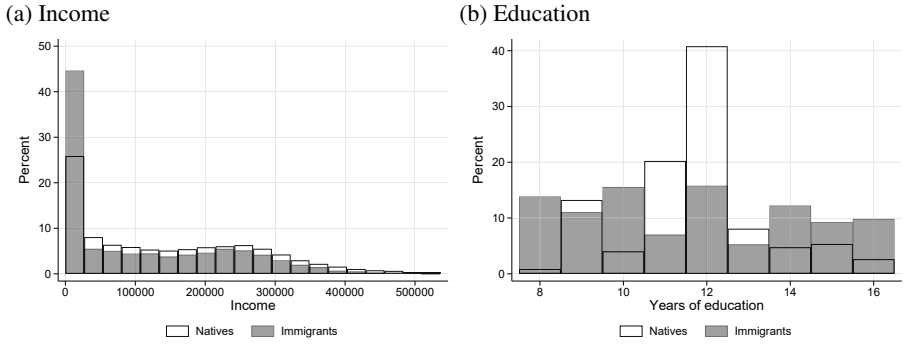


Figure 3. Income and education distribution before passing the written exams. Income and years of education are measured two years before passing the written exams. Income is given by annual income in SEK.

(a) Time of residency in Sweden (b) Age

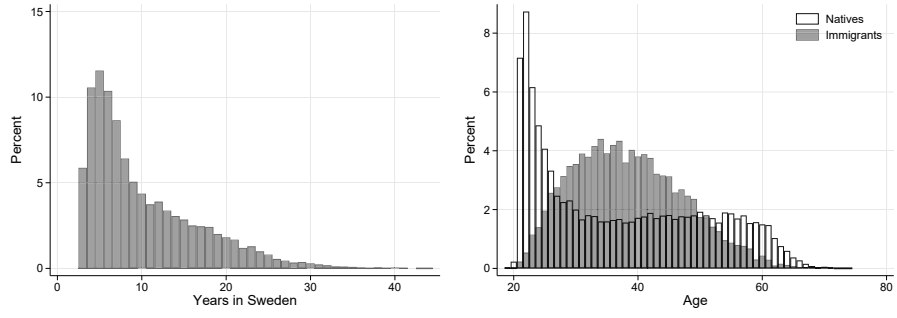


Figure 4. Statistics when taking the first test to become a taxi driver. Individuals with a time of residency in Sweden of three years or less are excluded due to the restriction of using a balanced panel in our analysis.

Lastly, Table 4 displays descriptive statistics for the immigrant sample separated by years since migration. More recent arrivals tend to be younger, have lower incomes, and are more likely to rely on welfare benefits but are, in contrast, more likely to have a post-secondary education.

4 Empirical framework

Our administrative data allow us to track individuals from age 16 or the time when they immigrated to Sweden. Moreover, we observe precise dates of written taxi driver's license exams, allowing us to observe individuals before and after they pass the required exams. In the following, we use "treatment" to refer to passing the three required written taxi exams and "treated individuals" to refer to individuals who have done so. In our main specifications, we use the panel dimension of our data to compare labor market outcomes between individuals who pass and those who have yet not passed but will do so later in the sample period. In Section 6, we also include individuals that never passed or took the test to become a taxi driver, applying the method of Dube et al. (2023) to take into account issues relating to heterogeneous treatment effects in staggered treatment designs.

We apply an event-study design, where the dynamic effect of treatment is obtained (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019). This method has two main benefits. First, it makes potential pretrends easier to detect. Second, the full trajectory of the labor market outcomes can be analyzed. The outcome variable Y_{ist} denotes labor market outcomes for individual i, in calendar year $\times$ month s and, in event time t. Even though our main outcome variable is income, we also focus on other relevant measures of labor market outcomes, such as employment, UI, and welfare benefits. Our baseline specification includes a balanced panel of individuals that we observe

Table 4. *Descriptive statistics for immigrants depending on years since migration*

	<5 years	5-10 years	>10 years	All
Male	0.987	0.969	0.955	0.972
	(0.111)	(0.172)	(0.207)	(0.165)
Age	32.40	34.96	43.27	36.43
	(7.630)	(7.651)	(7.445)	(8.844)
Income	6294.7	9845.7	10941.3	8807.0
	(8296.6)	(10081.7)	(11392.9)	(10072.7)
No UI Benefits	0.894	0.780	0.741	0.812
	(0.307)	(0.414)	(0.438)	(0.390)
No Welfare Benefits	0.329	0.545	0.707	0.510
	(0.470)	(0.498)	(0.455)	(0.500)
Years of schooling	11.86	11.50	11.76	11.72
	(2.830)	(2.683)	(2.281)	(2.635)
Higher education	0.433	0.343	0.305	0.366
	(0.496)	(0.475)	(0.461)	(0.482)
Observations	3,911	3,277	3,009	10,197

Mean coefficients; sd in parentheses

Notes: The table includes the individuals in our main analysis separated by years since immigration to Sweden. "Income" is given in average monthly income in SEK. All variables refer to two years before passing the written exams.

36 months before passing the written exams and 36 months after. Hence, the calendar year × month in which the individual passes the third and final required exam is time $t = 0$. We include event time dummies for all periods (t) ranging from 36 months prior to the event to 36 months after. The event time dummy for the period $t = -24$ is omitted and provides the baseline, i.e., all effects are relative to the outcome two years before the event. The coefficient of interest is β and shows the effect of receiving the license on labor market outcomes (Y_{ist}). We therefore run the following regression for individual i, observed t periods before or after treatment and in calendar year × month s, separately for natives and immigrants:

$$Y_{ist} = \sum_{j \neq -24} \beta_j \cdot \mathbf{I}[j = t] + \lambda_i + \lambda_s + \lambda_{is} + \varepsilon_{ist}. \tag{1}$$

We include a full set of indicator variables for gender (λ_i), calendar year × month (λ_s), and age and years since immigration (λ_{is}). We omit years since immigration for natives. Including a full set of indicator variables is important to control non-parametrically for life and business cycle trends. We are interested in identifying β_j, the effect of passing the written taxi exams on labor market outcomes. To do so, the identifying assumption is that the labor market outcomes of individuals who passed the exams would have evolved in parallel with those of individuals who have not yet passed the exams. The variation in age for when they pass the tests is used to identify the effects. This means that we, at any given calendar year × month, compare individuals with the same gender, age, and years since migration but at different periods relative to passing the test.

To keep individuals with zero income in the analysis, we use income in levels rather than logs as the outcome (Y_{ist}) in Equation 1. To study percentage change in income (P_t) we follow Kleven, Landais, and Søgaard (2019) and use the following transformation:

$$P_t \equiv \hat{\beta}_t \,/\, \mathbf{E}\left[\widetilde{Y}_{ist} \mid t\right], \quad \text{where} \quad \widetilde{Y}_{ist} \equiv \lambda_i + \lambda_s + \lambda_{is} \tag{2}$$

5 Results

5.1 Employment and income from taxi firms

We begin by estimating the impact of passing written taxi exams on the likelihood of ever being employed in a taxi firm. This estimation is a test of whether passing the written exams is predictive of later obtaining a taxi driver's license and beginning to work as a taxi driver. Figure 5 displays the effect using 36 months of pre- and post-treatment data. For each month, the estimate indicates the difference between treatment and control groups in their likelihood of ever

being employed in a taxi firm. Before passing exams, treatment and control groups have very similar outcomes, as is to be expected before either group passes the written taxi exams. Starting in the treatment month, however, panel (a) shows that immigrants' taxi firm employment rises sharply, by nearly 20 percentage points relative to the control group. This effect gradually increases to 60 percentage points after 15 months and ultimately reaches approximately 70 percentage points after 36 months. For natives, panel (a) also shows that effects are largely similar but with a faster development, where the indicator for ever having taxi firm employment reaches 60 percentage points after nine months. This may reflect differences in the ability to find a job at a taxi firm, even conditional on having a taxi driver's license, or differences in the ability to obtain the taxi driver's license after having passed the written tests.

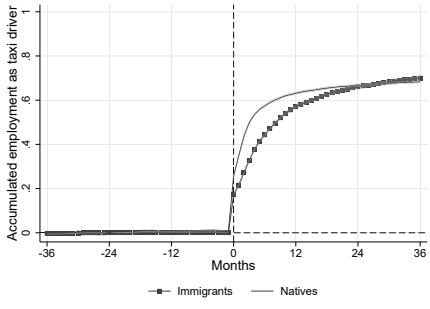
We next display the evolution of monthly incomes from taxi firms in panel (b) of Figure 5. For immigrants, taxi incomes rise immediately after passing written exams, increasing to 7,500 SEK after 12 months. Between 12 and 36 months post-treatment, taxi incomes rise somewhat and remain stable at above 7,500 SEK per month compared to the control group. For natives, the dynamic effects are somewhat different. Similar to the case with employment, taxi income rises faster than for immigrants. However, the effect of income reaches its peak at a lower level of roughly 6,000 SEK 12 months after treatment, after which it gradually declines to 5,000 SEK after 36 months.

Panels (c) and (d) display two related measures: the share of total monthly income from taxi firms and an indicator variable for having taxi income as the largest contributor to total income, respectively. Both panels show that immigrants experience an increase in these measures of at least 40 percentage points between 12 and 36 months after treatment. The increase remains at this level throughout the sample period. Natives, by contrast, experience a peak at approximately 40 percentage points within the first 12 months, after which there is a decline over time by ten percentage points. Note that these are reduced-form estimates that include all individuals who never became taxi drivers, as per panel (a), or who subsequently left the taxi sector.

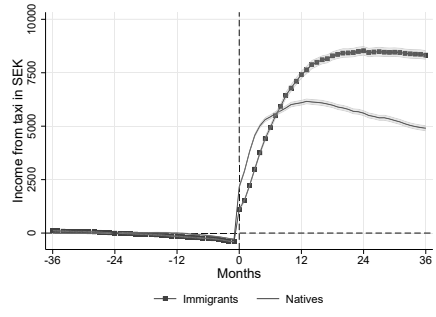
We conclude that passing the written taxi exams has a substantial and immediate impact on the likelihood of ever being employed in the taxi sector, on the level of income derived from taxi firms, as well as the propensity to have taxi driving as the main employment. Moreover, we note that taxi driving tends to be a longer-term occupation for immigrants, who show no signs of leaving the occupation or using it as a stepping-stone to other careers. By contrast, natives gradually decreased their employment in taxi driving after the first 12 months, indicating a greater use as a temporary solution.[5]

[5]It is possible that some individuals who are employed at taxi firms are not in fact taxi drivers. For example, individuals may do administrative or managerial work. However, it is unlikely that the timing of such employment would coincide with passing taxi exams, as such positions are not covered by occupational licensing.

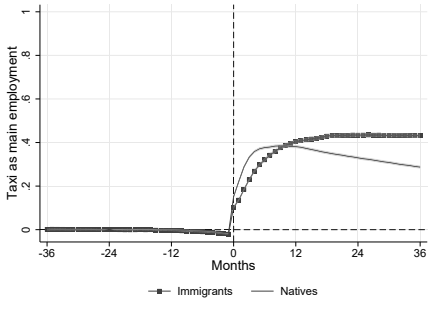
(a) Taxi employment



(b) Taxi income



(c) Taxi as main occupation



(d) Share of total income from taxi

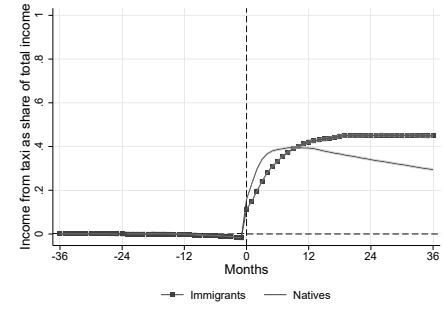


Figure 5. Impact of passing written exams on income and employment from taxi firms. "Accumulated employment as taxi driver" is equal to one if an individual has ever had income from a taxi firm up until the period in question and zero otherwise. "Taxi as main employment" is a dummy equal to one if the income from taxi firms is larger than income from non-taxi firms. In our data, we can see whether individuals were employed by firms in the taxi sector but not if they were employed as taxi drivers. However, as shown in the figure, income and employment within these firms increase substantially when passing the written tests, indicating that they are working as taxi drivers rather than with something else within the firm. This data limitation also explains why the treatment group can have a slight negative trend in "income from taxi" before passing the written exams.

Table 5. *Average impact of passing written exams on labor market outcomes*

	Immigrants		Natives	
	Income	Employment	Income	Employment
Passing	4522.2***	0.181***	2000.5***	0.135***
	(36.00)	(0.00129)	(32.48)	(0.000843)
Observations	744,381	744,381	1,174,935	1,174,935
Mean outcome	8956.6	0.575	12576.4	0.755

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the regression on labor market outcomes when including indicator variables for age and year on a balanced panel with individuals included 36 months before and after passing the written exams for the taxi driver license. "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-month. Mean outcome refers to two years before passing the written exams.

5.2 Employment and income from all sources

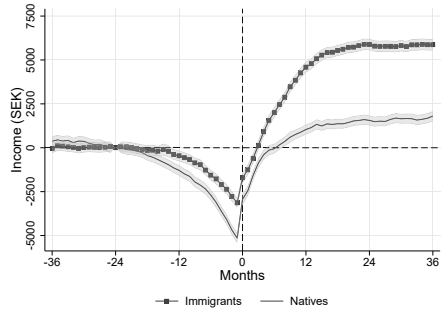
Having established a positive effect of passing the written taxi driver's license exams on engagement in the taxi labor market, we proceed to study its impact on overall employment and income. If individuals who access the taxi labor market would otherwise have found jobs of a similar type as before, we may find that the overall effect on labor market incomes is negligible despite having a large effect within the taxi sector.

Table 5 displays our results. Column 1 shows that total labor income increases by 4,522 SEK per month, which is statistically significant at the 1 percent level. This represents a large increase in incomes compared to pre-treatment levels. Relative to the pre-treatment average of 8,956 SEK, individuals who pass written taxi exams increase their monthly income by 50 percent. Column 2 shows that immigrants raise their overall employment rate by 18.1 percentage points, where employment is defined as having a positive income in a given month. Again, this is a large increase relative to the mean outcome of 57.5 percent two years before treatment.

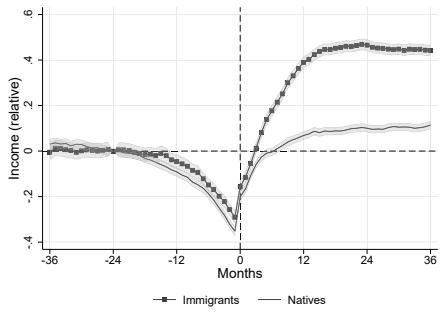
The next two columns of Table 5 display results for natives. Compared to immigrants, natives who pass taxi license exams experience a smaller but also positive effect on average monthly incomes, amounting to 2,000 SEK. Since natives have higher incomes before passing taxi exams, however, the relative effect is also smaller. Relative to the mean of 12,576 SEK, the incomes of natives increase by 15.9 percent. Employment rates increase by 13.5 percentage points on average, a relative increase of 17.8 percent.

Event-study estimates for the impact of access to the taxi labor market on income are presented graphically in Figure 6. For immigrants in panel (a),

(a) Income in levels



(b) Income relative to period -24



(c) Employment

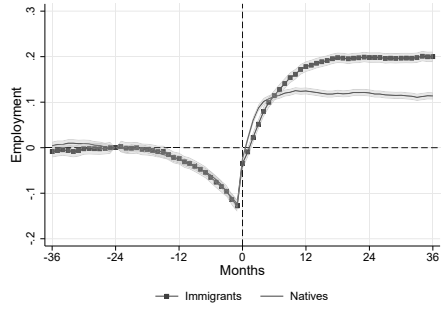


Figure 6. Impact of passing written exams on labor market outcomes. "Income in SEK" is income in Swedish Krona on a monthly level. "Income" is income in the given time period relative to the income two years before passing the test. "Employment" is equal to one if the individual has an income > 0 in the given time period and zero otherwise.

differences between treatment and control groups are close to zero and stable until approximately 12 months before passing the written taxi exams. Subsequently, incomes begin to decrease significantly for the treatment group. In the month before passing written tests, the treatment group has more than 2,500 SEK lower income than the control group, corresponding to a nearly 30 percent decline relative to $t = -24$ as shown in panel (b). Below, we discuss possible interpretations and reasons for this divergence from parallel trends.

Moving on to post-treatment outcomes, we observe a sharp increase in monthly income in the month of passing exams. Incomes continue to rise monthly until reaching a peak of nearly 5,000 SEK higher than the control group after 12 months. This effect increases somewhat over the remainder of the sample period. As there is a clear transition phase during the first post-treatment year, Table 6 displays regression results exactly 36 months after treatment to estimate the effects net of this transition. Column 1 indicates that overall income is 5,865 SEK higher for immigrants after 36 months, compared to the average effect of 4,522 SEK noted above. Panel (b) of Figure 6 displays the event-study estimates for incomes relative to 24 months before

Table 6. *Average impact of passing written exams on labor market outcomes at 36 months after passing the test*

	Immigrants		Natives	
	Income	Employment	Income	Employment
Passing	5865.5***	0.200***	1805.2***	0.114***
	(170.7)	(0.00595)	(152.4)	(0.00419)
Observations	744,381	744,381	1,174,935	1,174,935
Mean outcome	8956.6	0.575	12576.4	0.755

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions include indicator variables for age, year, month, gender, and years since migration (for immigrants). "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-month. Mean outcome refers to two years before passing the written exams.

treatment. The figure shows that between 12 and 36 months after passing exams, incomes increase by more than 40 percent in the treatment group relative to not-yet-treated controls. Column 2 of Table 6 indicates that the effects for employment are 20 percentage points after 36 months, also somewhat higher than the average effect.

Turning next to natives' dynamic effects, panel (a) of Figure 6 shows that natives experience an earlier and more pronounced decrease in income levels before passing the written taxi exams, visible around the start of the sample period. The decrease in natives' incomes reaches a low point of close to $-5,000$ SEK in the month prior to passing taxi exams, nearly twice what is observed for immigrants. However, in relative income in panel (b), natives and immigrants have a similar negative trend in the 12 months before passing the tests, indicating that the difference in levels is due to differences in pre-taxi incomes. Similar to the case for immigrants, incomes for natives increase rapidly post-treatment. After passing the written taxi exams, incomes rise sharply for several months. After 36 months, column 4 of Table 6 shows that natives' incomes are 1,805 SEK higher per month. As seen in Figure 6, natives display a pattern that may be consistent with an Ashenfelter's dip, raising the possibility that natives' incomes would have increased even in the absence of becoming taxi drivers. Section 5.2 investigates this possibility in further detail.

Lastly, panel (c) of Figure 6 displays dynamic effects on the probability of being employed. Interestingly, pre-treatment trends are highly similar in this outcome across immigrants and natives. However, a larger difference of approximately ten percentage points appears post-treatment, such that immi-

grants who pass taxi exams have employment rates that are higher by 20 percentage points after 36 months, while natives' employment rate is higher by 11 percentage points, as shown in Table 6.

Analysis of pre-treatment trends

Taken together, our results suggest that both immigrants and natives seek out the taxi driver labor market following an adverse shock in the labor market. After passing taxi exams, both groups see large income increases, with immigrants experiencing substantially larger effects. For our estimates to capture the causal treatment-on-the-treated effect of passing taxi exams, one must assume that trends in outcomes would have evolved in parallel between the treatment and control groups in the absence of treatment. While we cannot test this assumption, the observed divergence of pre-treatment trends between treatment and control groups indicates that the parallel trends assumption may be violated in our setting. In this section, we discuss possible causes for the observed pre-trends, which begin to diverge around 12 months prior to treatment and point to their implications for interpreting our estimates.

One possibility is that the observed negative pre-treatment trends indicate that the counterfactual outcomes for the treatment group would have been a continued deterioration relative to the control group, possibly bottoming out at permanently lower incomes and employment rates. Under this assumption, our estimates would be an *underestimate* of the causal effect of passing the written taxi exams on income since the control group's outcomes would be higher than the true counterfactual for the treatment group. Our estimates would thus provide a lower bound on the causal effect of passing taxi exams on income. However, it may be implausible to assume that the observed pre-trends would continue indefinitely in a negative direction, as the unemployed are likely to find other occupations in the absence of becoming taxi drivers. Nevertheless, as long as incomes are assumed to not fully recover to the pre-decline levels, our estimates would still represent underestimates of the treatment effect on the treated.[6]

We next distinguish between two additional mechanisms that may explain the negative pre-trends and discuss how they would affect the interpretation of our estimates. The first mechanism is that diverging pre-trends reflect anticipation effects. Individuals who anticipate switching occupations to taxi driving need to spend time studying and researching the labor market, leading to less time or effort spent in their current occupations. They also need to quit their current jobs, possibly leading to income caps for a period of time before beginning work as a taxi driver. This anticipation effect can thus be considered to reflect the investment cost of becoming a taxi driver and constitute part of the net return of becoming a taxi driver. The gross return, excluding these investment costs, can then be estimated by suitably excluding observations

[6]We provide some evidence for this claim later in this section.

Table 7. *Dropping observations 12 months pre-treatment to avoid anticipation effects*

	Immigrants		Natives	
	Income	Employment	Income	Employment
Passing	3607.9***	0.147***	680.3***	0.103***
	(42.63)	(0.00152)	(37.32)	(0.000971)
Observations	632,214	632,214	997,890	997,890
Mean outcome	9017.7	0.583	13086.0	0.773

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the regression on labor market outcomes when including indicator variables for age and year on a balanced panel with individuals included 36 months before and after passing the written exams for the taxi driver license. Observations 12 months before passing taxi exams are excluded. "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-month. Mean outcome refers to two years before passing the written exams.

before treatment. We propose to exclude 12 months, which is when we start to observe diverging pre-trends. Note that if diverging pre-trends are due to anticipation, the parallel trends assumption appears fulfilled as observed pre-trends are parallel approximately 12 months prior to treatment. Table 7 shows that doing so leads to an estimate of average post-treatment income of 3,607 SEK for immigrants, compared to 4,522 SEK in the standard specification. Thus, while considering anticipation effects decrease our estimated treatment effect, our estimates remain positive and both economically and statistically significant. The effect on employment decreases from 20 to 14.7 percentage points. Similarly, estimates for natives also decrease, with an estimated increase in a monthly income of 680 SEK (down from 1,805 SEK), but a largely similar effect on employment at 10 percentage points. Hence, estimates for immigrants are somewhat smaller but remain both economically and statistically significant when taking anticipation effects into account, while estimates for natives are more sensitive to this assumption.

The second potential mechanism for negative pre-treatment trends is that individuals tend to pass the taxi exams following a negative labor market shock, such as losing their employment. If so, it is plausible that incomes would recover to some extent over time as individuals find new jobs. Individuals who turn to the taxi labor market would, thus, have seen their labor market outcomes improve even in the absence of becoming taxi drivers, although it is a priori unclear to what extent they would be able to recover. This is a version of Ashenfelter's dip, a phenomenon in which individuals select into a labor market program in response to a negative shock, after which their incomes im-

prove due to mean reversion rather than a causal program effect (Ashenfelter, 1978). We note, however, that the standard Ashenfelter mechanism cannot explain the pattern of estimates that we observe for immigrants, as immigrants' incomes rise substantially over pre-treatment levels and thus appear unlikely to be explained by mean reversion.

Nevertheless, to quantitatively assess the extent to which mean reversion after job loss could explain the pattern of effects that we observe, we estimate a separate event-study design. More specifically, we use access to full population register data to follow individuals who experience sudden negative income shocks and study the effect on income dynamics in the subsequent years. The idea is to observe what occurs when individuals experience sudden labor market shocks outside of our sample of taxi drivers. We can then check whether labor market dynamics following income shocks match the pattern of results obtained after passing taxi exams. If post-shock incomes quickly recover and even surpass prior income levels, this would cast doubt on the claim that our estimates capture the causal effect of access to the taxi labor market. Instead, it would indicate that other labor market dynamics associated with job loss are driving our observed effects. To have similar individuals as the ones we have in our main analysis, we create a sample of non-taxi drivers that we match our taxi drivers using coarsened exact matching (Blackwell et al., 2009; Iacus, King, and Porro, 2012). The variables used to match are region of birth, age, years since migration, and education. This procedure means that we match taxi drivers in their pre-period (before passing the tests) with individuals who never took a test to become taxi drivers.

Using yearly data on income, we define an *income shock* as the year during which an individual has income below a threshold of approximately 100,000 SEK after having had an income above that level for at least five consecutive years.[7] We exclude women since taxi drivers are, in general, men (see Table 3), and we further restrict attention to individuals aged 20 to 60 years old to avoid identifying retirements. We estimate event-study models using the income shock as the event using the same methodology described in Section 4 but using yearly data.

Figure 7 displays our results with relative income as the outcome. We find that the income shock is associated with long-term adverse effects on income. One year after the shock, individuals have substantially lower incomes than the pre-shock level, approximately 85 percent lower. This effect persists for several years. Five years later, incomes remain more than 20 percent lower than in the control group. The impact of the income shock is similar for immigrants and natives in relative terms (panel a), but the short-run negative impact in SEK is slightly larger for natives (panel b). In the year of the shock, incomes

[7]This level corresponds to two *price base amounts*, a fixed figure determined annually by the Swedish government, used in calculations for various social benefits, taxes, and pensions. This can be compared to the median yearly income in 2017, which amounted to 360,000 SEK.

(a) Relative impact (b) Impact in SEK

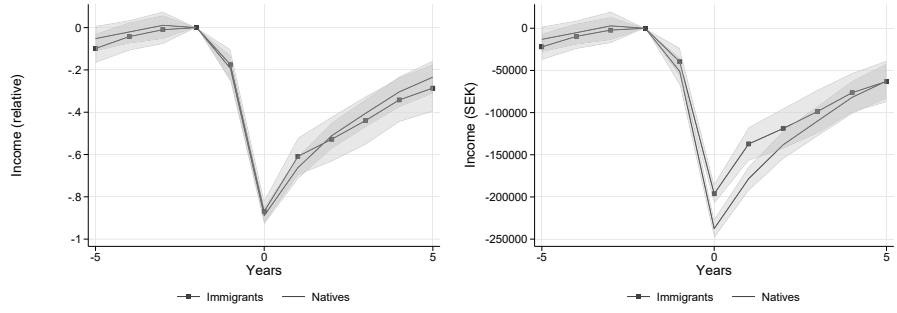


Figure 7. Effect of job loss on income. These graphs show the loss in income after a job loss (defined as going from an income above two price base amounts for at least 5 years to an income less than two price base amounts in a succeeding year). Individuals within the age range of 20 to 60 are included. The figures are not restricted to taxi drivers but rather a matched sample of individuals.

drop sharply and slowly recover over time. Yet, even five years later, incomes remain over 20 percent lower. These results thus stand in sharp contrast to what we observe for individuals who pass the written taxi exams. While our taxi estimates indicate that incomes quickly rise above the previous mean incomes, the effects of job loss are negative even 5 years later.

The results in Figure 7 indicate that individuals who are subject to an adverse labor market shock tend to experience long-term earnings losses and, on average, do not recover or come close to surpassing previous income levels in the way observed for taxi drivers. Therefore, it appears unlikely that individuals who pass the written taxi exams would have experienced the observed increases in income in the absence of treatment. Indeed, Figure 7 implies that our estimates are a lower bound for the causal effect of passing taxi exams, as the true counterfactual is to have an income that is lower than what we observe in the control group.

5.3 Usage of social insurance systems

In this section, we estimate how access to the taxi labor market affects the usage of the social insurance system using data at yearly intervals. We use two outcomes. The first is an indicator taking value one if an individual receives neither social welfare transfers nor housing subsidies, both of which are means-tested and zero otherwise. The second is an indicator of not receiving unemployment insurance (UI) benefits in a given month. Since unemployment insurance is partly driven by income and contributions to the UI system, we separately analyze such benefits as they have a more direct link to our outcome.

Table 8. *Average impact of passing written exams on welfare and UI benefits*

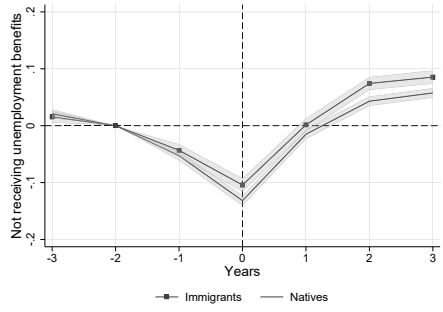
	Immigrants		Natives	
	No UI Benefits	No Welfare Benefits	No UI Benefits	No Welfare Benefits
Passing	0.00483	0.0377***	-0.0134***	0.0220***
	(0.00404)	(0.00448)	(0.00298)	(0.00219)
Observations	72,898	72,898	117,299	117,299
Mean outcome	0.804	0.508	0.801	0.883

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The table shows the regression on welfare and UI benefits when including indicator variables for age and year on a balanced panel with individuals included 3 years before and after passing the written exams for the taxi driver license. "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-month. Mean outcome refers to two years before passing the written exams.

Columns 1 and 3 of Table 8 show our results for UI benefits. For immigrants, we find that they are no less likely (in terms of statistical significance) to receive UI benefits on average in the three years after passing taxi exams, while natives increase their use of UI by 1.3 percentage points. The dynamic effects reported in panel (a) of Figure 8 show that before passing exams, UI usage increases for both groups in the year before treatment. In the treatment year, usage increases by at least 10 percentage points. This corresponds well with the observed pre-treatment trends in employment rates, which declined similarly for both groups, as seen in panel (c) of Figure 6. After passing taxi exams, however, both immigrants and natives rapidly reduced their usage of UI benefits. Focusing on the long-run effect three years after treatment, Columns 1 and 3 of Table 9 indicate that both immigrants and natives are significantly less likely to receive UI benefits, with a decrease of 8.5 and 5.8 percentage points, respectively.

Columns 2 and 4 of Table 8 show our results on receiving means-tested transfers. Two years before passing the written taxi exams, 50 percent of immigrants did not receive any means-tested welfare benefits. After treatment, this is increased by 3.8 percentage points, corresponding to an increase of 7.5 percent. Among natives, the pre-treatment usage of means-tested social welfare is considerably lower, as 88 percent receive no benefits. This is further increased by 2.2 percentage points after passing taxi exams. Panel (b) of Figure 8 shows corresponding event-study estimates. Interestingly, there are only minor trends in pre-treatment usage of welfare benefits among both immigrants and natives. After treatment, however, the share of individuals who receive no

(a) Not receiving UI benefits



(b) Not receiving welfare benefits

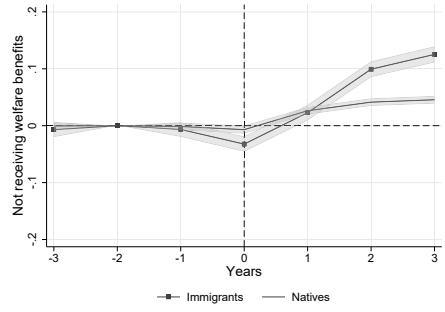


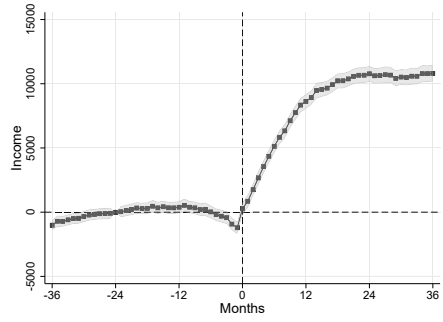
Figure 8. Impact of passing written exams on welfare benefits. "Not receiving welfare benefits" is equal to one if an individual does not receive means-tested welfare or housing subsidies in a given month and zero otherwise. "Not receiving unemployment benefits" is equal to one if an individual does not receive UI benefits in a given month and zero otherwise. The omitted category is two years before passing the written exams.

Table 9. *Average impact of passing written exams on welfare and UI benefits at 3 years after passing the test*

	Immigrants		Natives	
	No UI Benefits	No Welfare Benefits	No UI Benefits	No Welfare Benefits
Passing	0.0854***	0.125***	0.0576***	0.0456***
	(0.00626)	(0.00734)	(0.00457)	(0.00358)
Observations	72,898	72,898	117,299	117,299
Mean outcome	0.804	0.508	0.801	0.883

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions include indicator variables for age, year, gender, and years since migration (for immigrants). "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-year. Mean outcome refers to two years before passing the written exams.

(a) Between 3 and 5 years



(b) More than 5 years

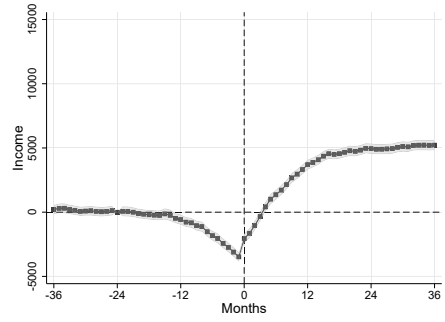


Figure 9. Effects of passing written exams on income by years since immigration. Figures display the income effect for immigrants with between 3 and 5 years in Sweden when passing the test (a) and more than 5 years in Sweden when passing the test (b).

welfare benefits rises rapidly, most notably among immigrants. Table 9 shows that immigrants are 12.5 percentage points more likely not to receive welfare benefits after three years, an increase of 25 percent over the pre-treatment average. Natives also see a larger effect at 4.6 percentage points, corresponding to an increase of 5 percent.

5.4 Heterogeneous effects

The results in Section 5.2 indicate that the immigrants who gain access to the taxi labor market see substantial gains in income, with a smaller effect visible for natives. In this section, we investigate whether there are other heterogeneous effects of treatment, specifically concerning education and length of residence in Sweden.

Figure 9 displays our results for immigrants split by those who are recent arrivals and have been in Sweden for 3 to 5 years in panel (a) and those who have been in the country for more than five years in panel (b). The panels display marked differences in outcomes. For recent arrivals, pre-treatment trends in income are small and display only a minor income decline in the months leading up to passing exams. By contrast, panel (b) shows that the pre-treatment decline in income that we observe in the full sample is driven by immigrants who have been in Sweden for at least five years. Interestingly, post-treatment incomes are substantially larger among recent arrivals, with estimates indicating that monthly incomes are twice as high in this group compared to those with more experience in Sweden.

Table 10, column 1 displays regression output for the heterogeneity, which indicates that average post-treatment incomes are 1,958 SEK smaller for individuals who have been in Sweden 10 years longer, relative to a baseline estimated income increase of 7,913 SEK. Column 2 shows that the same pat-

Table 10. *Average impact of passing written exams on labor market outcomes relative to years since immigration*

	Immigrants	
	Income	Employment
Passing	7913.2***	0.316***
	(56.04)	(0.00204)
Event × Years since migration	-195.8***	-0.00778***
	(4.926)	(0.000159)
Years since migration	334.0***	0.0127***
	(3.850)	(0.000137)
Observations	744,381	744,381
Mean outcome	8956.6	0.575

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions include indicator variables for age, year, gender, and years since migration (for immigrants). Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-month. Mean outcome refers to two years before passing the written exams.

tern holds for employment, as immigrants who have 10 years of experience in Sweden have a 7.8 percentage point lower effect on employment. Table 11 displays our results when looking at social insurance usage. Recent arrivals are less likely to be eligible for UI benefits, which likely explains that treatment decreases reliance on UI more for immigrants who have been in the country for a longer period. By contrast, the usage of welfare transfers follows the same pattern as labor market outcomes. The baseline effect on the likelihood of not receiving welfare transfers is 27.7 percentage points, which is reduced by 11 percentage points for individuals who arrive 10 years earlier. This result is consistent with returns to taxi driving being the largest for those with weaker outside options in the labor market, such as recent arrivals who tend to have weaker language skills, social networks, and formally recognized qualifications.

Next, we investigate the heterogeneous effects of education. To do so, we define a dummy, taking value one for individuals that have completed an education comparable to more than 12 years of school. Hence, this variable captures post-secondary education. Figure 10, panel (a) displays our results for immigrants. Both higher and lower-educated immigrants display similar dynamics up to one year after treatment, at which point the incomes of higher-educated immigrants increase relatively more. Table 12 shows that higher-educated immigrants earn a significantly higher income of 371 SEK per month, 8.5 percent higher than the low-education effect of 4,369 SEK. This

Table 11. *Average impact of passing written exams on welfare and UI benefits relative to years since immigration*

	Immigrants	
	No UI Benefits	No Welfare Benefits
Passing	-0.0827***	0.270***
	(0.00574)	(0.00703)
Event × Years since migration	0.00464***	-0.0111***
	(0.000468)	(0.000515)
Years since migration	-0.00790***	0.0273***
	(0.000383)	(0.000434)
Observations	72,898	72,898
Mean outcome	0.804	0.508

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions include indicator variables for age, year, gender, and years since migration (for immigrants). Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-year. Mean outcome refers to two years before passing the written exams.

result is interesting since it indicates that wages are highest among those immigrants who would presumably be able to earn higher incomes in other, more skilled professions, given their level of education. This stands in contrast with results from the same specification on the native sample. For natives, panel (b) of Figure 10, as well as column 3 of Table 12, show that higher-educated natives earn significantly less than their lower-educated counterparts. Table 13 repeats the analysis using welfare and UI benefits as outcomes, showing a statistically significant heterogeneous effect only for high-income natives' usage of welfare benefits.

The finding that more educated natives earn less than their lower-educated counterparts indicates that there is an earnings penalty for entering a lower-skilled occupation, such as taxi driving, instead of one that requires post-secondary schooling. The fact that highly educated immigrants do not experience this wage penalty indicates that higher-education degrees acquired abroad are difficult to use effectively in the Swedish labor market. The finding that higher-educated immigrants earn more than both higher-educated natives and lower-educated immigrants is in line with the phenomenon of "over-educated" immigrants who turn to low-skill occupations due to difficulties in transferring their foreign human capital to the host country.

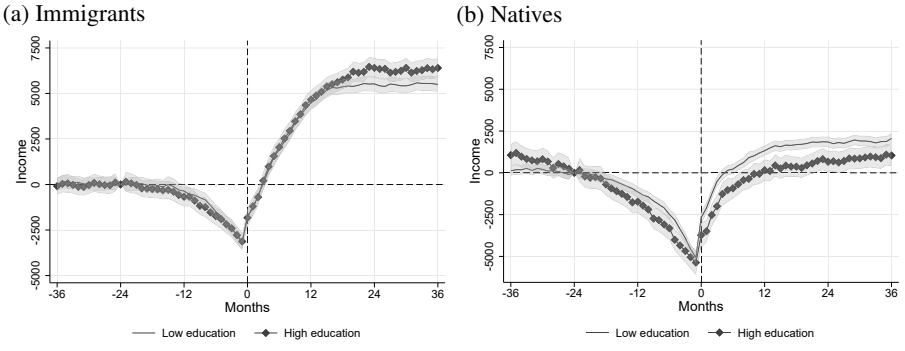


Figure 10. Effects passing written exams on income by type of education. "Low education" means 12 years of education or less, and "high education" means more than 12 years of education. 12 years corresponds to Swedish high school. The distribution of years of education among natives and immigrants is shown in figure 3.

Table 12. *Average impact of passing written exams on labor market outcomes relative to the level of education*

	Immigrants		Natives	
	Income	Employment	Income	Employment
Passing	4369.2***	0.180***	2107.8***	0.131***
	(41.47)	(0.00149)	(34.22)	(0.000936)
Event × Education	371.7***	0.00356*	-428.4***	0.0153***
	(59.57)	(0.00212)	(64.40)	(0.00163)
Education	300.8***	0.0114***	-308.7***	-0.0250***
	(41.44)	(0.00170)	(47.98)	(0.00132)
Observations	744,381	744,381	1,174,935	1,174,935
Mean outcome	8956.6	0.575	12576.4	0.755

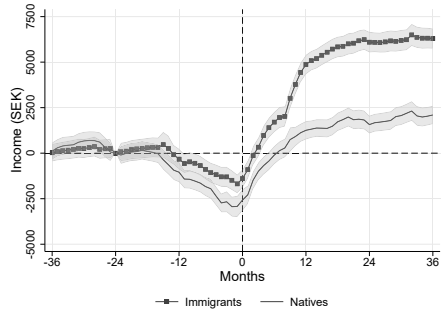
Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions include indicator variables for age, year, month, gender, and years since migration (for immigrants). "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-month. Mean outcome refers to two years before passing the written exams. Education is an indicator variable equal to one if having more than 12 years of education and zero otherwise.

Table 13. *Average impact of passing written exams on welfare and UI benefits relative to the level of education*

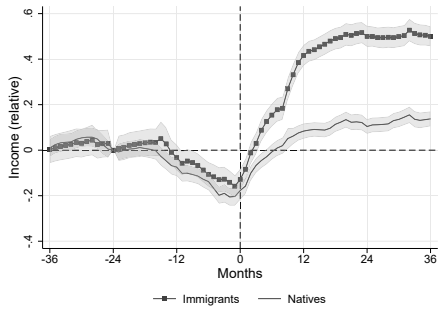
	Immigrants		Natives	
	No UI Benefits	No Welfare Benefits	No UI Benefits	No Welfare Benefits
Passing	0.00183	0.0379***	-0.0140***	0.0245***
	(0.00467)	(0.00523)	(0.00339)	(0.00253)
Event × Education	0.00663	-0.00476	0.00340	-0.00821**
	(0.00607)	(0.00712)	(0.00501)	(0.00377)
Education	0.00926**	0.0429***	0.0454***	0.0166***
	(0.00465)	(0.00558)	(0.00387)	(0.00303)
Observations	72,898	72,898	117,299	117,299
Mean outcome	0.804	0.508	0.801	0.883

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.
Notes: The regressions include indicator variables for age, year, month, gender, and years since migration (for immigrants). "Passing" is the event of passing the exams. Income is income in SEK. All specifications control for fixed effects for age, year, month, gender, and years since migration (for immigrants). Observations are individual-by-year. Mean outcome refers to two years before passing the written exams. Education is an indicator variable equal to one if having more than 12 years of education and zero otherwise.

(a) Income (SEK)



(b) Income (relative)



(c) Employment

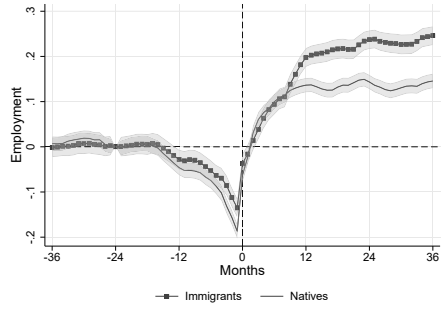


Figure 11. Impact of passing written exams (January–April). The figures show the same regressions as Figure 6 but only for individuals passing the tests between January and April.

6 Robustness checks

In Section 3, we describe how our spell-level data are adjusted to create a monthly income measure that avoids a common practice of employers reporting year-long employment spells rather than specifying exact dates. To test for the robustness of this adjustment, we next use the unadjusted data on the subset of individuals who passed their written taxi exams during the first four months of the year, January to April. For this group, misreporting of incomes will play a smaller role as there is less scope for employers to erroneously predate employment spells. Thus, by comparing results in this group with those of the full sample with adjusted income data we may ascertain if our adjustments have an impact on outcomes. Figure 11 displays our results. Comparing panels (a) through (c) shows that income and employment trends follow highly similar patterns in this subgroup compared to the full sample in which we adjust incomes. This is reassuring as it indicates that our results are not due to our adjustments of employment-spell data.

Recent advances in difference-in-differences and event-study methodology have highlighted certain specification issues inherent to these models. In particular, bias may arise with a staggered treatment design combined with

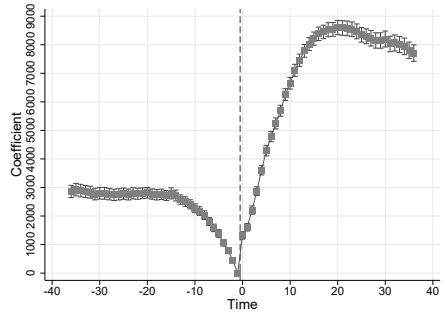
heterogeneous or dynamic treatment effects. The core of the problem with standard methods lies in the control group being incorrectly specified. For staggered treatments, Goodman-Bacon (2021) shows that the two-way fixed effects estimator is a weighted average of 4 different types of comparisons. One type of comparison uses already treated observations as a control group, even though their outcomes may still be dynamically affected by the treatment. Several estimators have been developed in recent years to address the potential biases of standard methods, including the local projections difference-in-differences (LPDID) estimator of Dube et al. (2023). The LPDID estimator is substantially more computationally efficient than most alternatives, making it especially suitable in our setting given the large number of observations. The estimator is also numerically equivalent to the one proposed by Callaway and Sant'Anna (2021) under a specific reweighing of the data, which we apply in our models (Dube et al., 2023).

Figure 6 displays our results for income and employment outcomes using the LPDID estimator in which controls include never-treated individuals. We first note that there is a positive level shift in all estimates relative to our baseline results. This is due to LPDID using the month prior to treatment as the omitted category, while our baseline models use $t - 24$ months. For immigrants in panel (a), pre-treatment pattern is very similar to our results in Figure 6. Post-treatment outcomes are also similar: incomes rise rapidly starting in the month of passing exams and reach a peak between 12 and 24 months later. Compared to our baseline model, however, incomes show a somewhat decreasing trend between 24 and 36 months after treatment. Nevertheless, incomes for those who passed the taxi exams remained substantially higher in levels throughout the sample period.

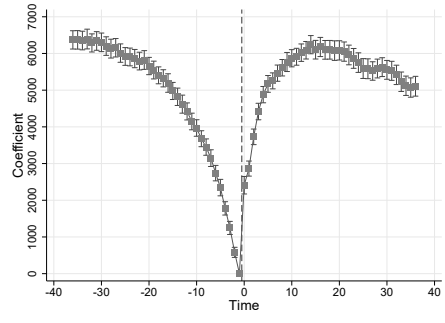
The pattern of results differs more for natives, however, as shown in panel (b). Here, estimates indicates that while natives' incomes do rise after passing the written taxi exams, they peak at a lower level and never exceed pre-exam earnings. Similar to the case with immigrants, estimates show a declining trend over time which is not observed in our baseline models. In fact, natives' income levels are somewhat lower after 36 months compared to the pre-period. This may be due to the use of never-treated individuals in the control group, which over time experience a stronger income trend and therefore catch up to the treated individuals.

Panels (c) and (d) display our results using employment as the outcome. Compared to our baseline model, immigrants display a small positive pre-taxi trend in employment, albeit a weak one compared to the substantial increase in employment post-treatment. Post-treatment results for both immigrants and natives are very similar to what our baseline results. Interestingly, therefore, the LPDID estimator indicates that natives increase their employment rates significantly over time compared to the control group, even though income levels are stagnant or even decrease. Overall, applying a robust event-study estimator, we find similarly large and positive effects for immigrants, albeit

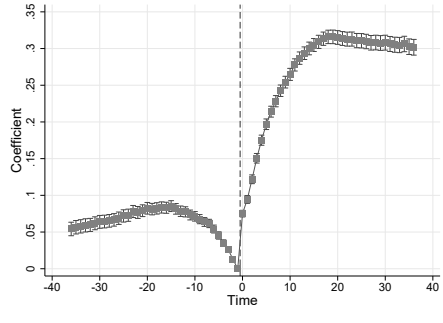
(a) Income—Immigrants



(b) Income—Natives



(c) Employment—Immigrants



(d) Employment—Natives

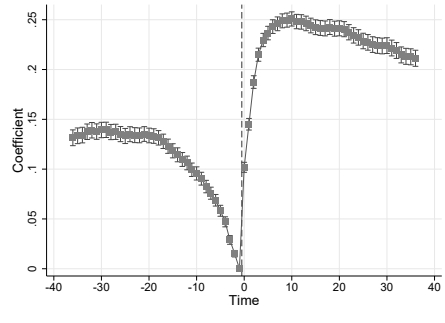


Figure 12. Impact of passing written exams on labor market outcomes including never treated. Event-study graphs based on the linear projection difference-in-differences estimator of Dube et al. (2023) using never-treated individuals as controls. The month prior to passing the written taxi exams is normalized to zero. "Income" is given in SEK. "Employment" is equal to one if the individual has an income > 0 in a given month, and zero otherwise.

with indications that the effect is attenuated somewhat over time. By contrast, the results for natives suggest that their returns to taxi driving are much smaller than for immigrants.

7 Conclusions

Despite difficulties faced by many immigrants in finding employment in high-income countries, certain sectors of the economy accommodate large shares of immigrants into their workforce. This paper focuses on one such sector—taxi driving—and estimates the impact of access to the taxi labor market separately for non-Western immigrants and native-born Swedes. To do so, we use individual-level data on written taxi exams as a determinant of who has access to the taxi labor market. We then apply an event-study design to study individuals who pass the necessary written exams required to obtain a taxi license.

Our results indicate that taxi driving plays a different role for immigrants and natives. Immigrants see large and persistently positive effects on their monthly incomes, with average relative increases of 50 percent over pre-taxi income levels. In addition, immigrants who pass the taxi driver exams become substantially less likely to rely on means-tested welfare benefits. Natives experience smaller effects on income relative to their pre-taxi averages, but still have higher rates of employment.

Moreover, our results indicate that selection into taxi driving coincides with declining labor market outcomes starting roughly 12 months prior to passing taxi exams. We discuss the implications for the parallel trends assumption as well as for interpretation of our estimates as causal, arguing that our pattern of results is unlikely to have occurred in the absence of individuals entering the taxi labor market. Estimates are not driven by the poor pre-treatment incomes artificially amplifying post-treatment effects as results are robust to dropping 12 months of observations before passing exams. Mean reversion is also unlikely to explain our results: a separate event study on a matched sample shows that individuals who experience a negative income shock have persistently lower incomes in the subsequent five years. By contrast, our results indicate that individuals catch up to and surpass their previous incomes. Indeed, both of these robustness tests indicate that our estimates represent lower bounds on the causal effect of passing taxi exams.

The fact that we observe substantially larger effects for immigrants, both in absolute and relative terms, is consistent with outside options in the labor market being very different across these groups. Whereas taxi driving constitutes one occupation among many for natives, the alternatives for immigrants appear to be fewer, thus yielding larger effects when compared to their relative control groups. Analysis of heterogeneous effects supports this hypothesis, as effects are largest for recently-arrived immigrants, who have less experience and location-specific skills. Moreover, many more highly educated individuals

take up taxi driving among immigrants than natives. Highly educated immigrants have higher post-taxi earnings compared to lower-educated immigrants, while the opposite is true for natives. We take this to indicate that outside options are generally lower for highly educated immigrants, compared to highly educated natives, in line with studies finding foreign-acquired human capital having lower economic returns (Friedberg, 2000).

Applying the recent event-study methodology of Dube et al. (2023) shows that our results for immigrants are robust to issues such as heterogeneity in treatment effects and using never-treated individuals as controls. However, these results indicate that the standard event-study estimates may be exaggerated with regard to natives' incomes. The fact that natives who enter the taxi sector regain or potentially surpass their pre-treatment income levels indicates that the taxi labor market represents an opportunity to cope with adverse labor market shocks for this group. In addition, this result further bolsters the divergence in treatment effects across immigrants and natives, highlighting the particular importance of this labor market for non-Western immigrants.

Taken together, our results indicate that the taxi labor market may represent a substantially positive earnings opportunity, especially for immigrants but also natives who experience declining labor outcomes. Moreover, the taxi sector also likely entails a positive fiscal effect, as higher incomes increase tax contributions and the lower use of social insurance systems decreases fiscal costs.

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