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Cristina Bratu

Immigration: Policies, Mobility, and Integration



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## Immigration: Policies, Mobility, and Integration



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### **Abstract**

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**Essay I:** Labor immigration is an important tool that countries can use to address labor shortages. The design of labor immigration policies may affect flows and the composition of immigrant workers, which can, in turn, have an effect on firms and workers in the host country. I quantify such effects by studying a major Swedish reform that made it significantly easier for firms to recruit non-Europeans. Using a difference-in-differences setup, I exploit variation in the strictness of immigration rules which affected industries differentially before and after the reform. Treated industries are predominantly lower-skilled, and concentrated in sectors like hotels and restaurants and retail trade sectors. Using linked employer-employee data, I study the effect of the reform on both firm-level and individual-level outcomes. I find that the mean earnings at firms in treated industries unambiguously increase. Firms also seem to take advantage of skill complementarities between natives and immigrants and intensify their overall hiring of high-skilled workers. Moreover, I follow native incumbents' employment and earnings over time and find heterogeneous effects along the skill and age dimensions.

**Essay II** (with Matz Dahlberg, Mattias Engdahl and Till Nikolka): We evaluate the importance of spillover effects of national migration policies by estimating the effect of stricter rules on family reunification in Denmark in 2002 on migration to neighboring countries. We reach two main conclusions. First, we show that stricter rules for reunification lead to a clear and significant increase in emigration of Danish citizens with immigrant background. Most of the emigrants left Denmark for Sweden, a neighboring country in which reunification was possible. Second, we demonstrate that a significant fraction of the individuals that came to Sweden to reunite with a partner left the country again; within two (eight) years around 20% (50%) had left, with the absolute majority leaving for Denmark. Our results indicate that potential spillover effects from national migration policies should be taken into account when forming migration policy.

**Essay III** (with Valentin Bolotnyy): We use administrative Swedish data to show that, conditional on parent income, immigrant children have similar incomes and higher educational attainment in adulthood than native-born Swedes. This result, however, masks the fact that immigrant children born into poor families are more likely than similar natives to both reach the top of the income distribution and to stay at the bottom. Immigrant children from high-income families are also more likely than natives to regress to the economic bottom. Notably, however, children from predominantly-refugee sending countries like Bosnia, Syria, and Iran have higher intergenerational mobility than the average immigrant child in Sweden.

**Essay IV** (with Valentin Bolotnyy): Home ownership is an important indicator of socio-economic status and a good proxy for wealth. We show that on average, children of immigrants are less likely to own their homes than children of natives at age thirty. The difference remains even after we control for socio-economic characteristics, parental background, and municipality of residence. We find that parental background - both in terms of parents' income and education, but also their own home ownership status - is the most important determinant of home ownership in adulthood. We additionally investigate the role of age at arrival on outcomes in adulthood and find a significant negative effect of age at arrival on income and education, which also translates into a lower probability of owning a home in adulthood. However, growing up in a highly-educated family may partly mitigate this negative effect.

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# Introduction

The number of immigrants across the world - defined as the number of people living outside their country of birth - has substantially increased over the last three decades: in 2017, there were around 258 million international migrants, compared to 153 million in 1990 (United Nations 2017). The scale of immigration has prompted heated debates about the consequences of immigration among policymakers and academics alike. From the host country perspective, two questions dominate the debate (see de la Rica et al. 2015): i) what are the labor market effects of immigration? and ii) how well do immigrants integrate in their host countries?<sup>1</sup> Essay I in this thesis aims to add to the evidence pool towards the first question by studying the effect of non-European labor immigration on firm- and individual-level outcomes in Sweden, while Essays III and IV investigate integration outcomes of children of immigrants. A natural precursor to both of these questions is the extent to which governments can influence the number and composition of immigrants through immigration policies. Although not its main focus, Essay I touches on this issue briefly, as it exploits variation in non-European labor immigration induced by a reform that removed admission restrictions, whereas Essay II addresses this question indirectly by studying how immigration policies in one country affect flows to a neighboring country.

All articles in this thesis take Sweden as a case study. I use Swedish register data from the GeoSweden database, which covers all individuals with a residence permit valid for at least one year for the 1990-2014 period.<sup>2</sup> There are a few aspects of this data that make it particularly suitable for studying the questions posed in this thesis: i) it contains information on residence permits, which is crucial for Essays I and II; ii) it allows me to link parents to children and thus study integration from an intergenerational perspective (Essays III and IV) and iii) it includes rich individual-level information on socio-demographic characteristics, educational attainment and labor market outcomes. In what follows, I first give a brief account of Sweden's immigration history. I next summarize each chapter and discuss their findings in relation to the literatures they contribute to.

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<sup>1</sup>There is a large literature that deals with the effects of emigration on the sending countries. The issues tackled by that literature revolve around the role of migration and remittances in poverty alleviation (e.g. Adams Jr and Page 2005); migration and human development (for example, McKenzie and Rapoport 2006; Hildebrandt and McKenzie 2005); the brain drain (see Docquier and Rapoport 2012 for a review). Summarizing the findings in that literature is beyond the scope of this introduction.

<sup>2</sup>GeoSweden is administered by the Institute for Housing and Urban Research at Uppsala University. The data is collected and anonymized by Statistics Sweden.

## Immigration to Sweden

Sweden is an interesting case study because the nature of immigration has changed dramatically over time. The post-World War II years saw an industrial boom and along with it a shortage of workers. The early immigration waves were hence predominantly workers, to a large extent from other Nordic countries, and especially Finland.<sup>3</sup> Non-Nordic workers were recruited from Yugoslavia, Greece, Turkey, Hungary, Austria and Italy (Skodo 2018). They entered either via bilateral agreements (1940s and 1950s) or as tourists (early 1960s). Tourists were allowed to search for jobs during the three months the visa lasted; if successful, they could switch to a work permit without leaving Sweden (Emilsson et al. 2014). Starting in 1966, however, the rules became more restrictive. Non-Nordic migrants were now required to obtain permits and housing before entering Sweden. Large flows continued to arrive despite the change until 1972, when the Swedish Trade Union Confederation (LO) sent out a circular to its unions asking them to reject applications for non-Nordic work permits, as fears that immigrants were displacing domestic workers mounted (OECD 2011). Essay I in this thesis studies a dramatic shift in this policy, which ended in 2008 with a profound liberalization of the system.

As occurred elsewhere in Europe, and somewhat unexpectedly from the point of view of policymakers, these workers turned their temporary status into a permanent one and also brought their relatives to Sweden through family reunification. Since 1980, family reunification has consistently been among the biggest admission category of immigrants in Sweden (Figure 1). Refugee immigration started in the 1970s, with refugees from Chile; from Iran, Iraq, and Lebanon in the 1980s; from Somalia, Eritrea, and Former Yugoslavia in the 1990s. The timing of refugee arrivals has mirrored the timing of conflicts around the world. The last two essays in this thesis study a group of immigrants that arrived between 1974 and 1999 as children from a large set of countries of origin.

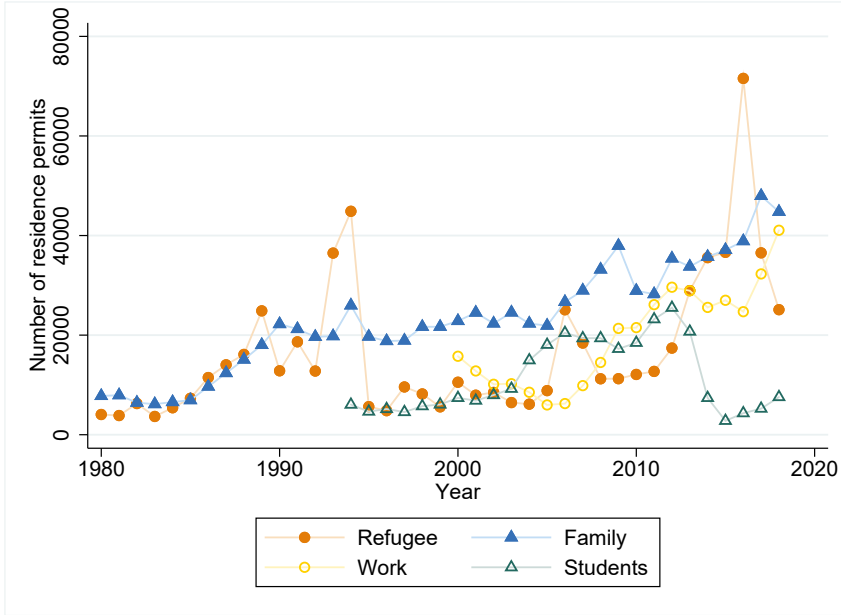
## Immigration policies, flows and composition

Economists model the decision to migrate as the result of a careful weighing of benefits (usually income gains) against costs (see Borjas 1987 and Sjaastad 1962). Within this framework, different immigration policies can either increase or decrease the cost of migration, depending on individual characteristics, and affect both the number and the composition of immigrants. An overall tough stance on immigration will reduce immigration flows across the board. At the same time, a policy that favors the high-skilled will reduce

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<sup>3</sup>Nordic citizens could take advantage of the 1954 Common Nordic Labor Market agreement that allowed them to work and reside in any of the Nordic countries without the need for a permit.

Figure 1. Number of residence permits granted 1980-2018



Notes: This figure plots the number of residence permits valid at least three months granted over the 1980-2018 period. It excludes adoptees and European Economic Area (EEA) migrants. Source: Migration Board.

the cost for these individuals, but increase it - potentially to infinity in the absence of alternative channels - for the lower-skilled.<sup>4</sup> Given persistent income differences across countries, there are large income gains to be made from migrating, but governments also have the power to select who benefits by adjusting their immigration policies. For example, Ortega and Peri (2013) use data on bilateral flows between many countries of origin and destination countries, along with data on immigration policies regulating entry requirements, and show that income per capita is a strong determinant of migration choices but that laws that tighten entry have strong negative effects on flows. Immigration policies can only go so far in controlling who moves, however, given the fact that many countries are signatories to international agreements such as the the 1951 Refugee Convention or part of communities such as the European Union where freedom of movement between member countries is a central tenet.

Against this backdrop, Essay I in this thesis shows that strong restrictions on firms' ability to recruit workers from non-European countries indeed kept labor immigration flows low. As soon as the policy changed in December 2008, the number of non-European workers increased. Even if the purpose of the policy was to allow firms to recruit non-European immigrants in order to

<sup>4</sup>See Clark et al. (2007) for a model that explicitly accounts for costs imposed by immigration policies.

address shortages in the labor market, the highest increase took place among lower-skilled workers in occupations that were not necessarily considered to be in shortage. This development goes to show that there are limits to how much the government can regulate the composition of immigrants.<sup>5</sup>

Countries may change their own immigration policies but they do not have control over what other countries do. Evidence from cross-country studies supports the hypothesis that strict immigration policies in one country can divert flows to other destinations (e.g. Bertoli and Fernández-Huertas Moraga 2013, Brekke et al. 2016). Essay II (co-authored with Matz Dahlberg, Mattias Engdahl and Till Nikolka) studies this question from the point of view of a country neighboring the country imposing a stricter immigration policy. In 2002, Denmark passed a series of strict laws regulating family reunification between Danish residents and partners with a non-European citizenship. Among the most important changes were the so-called “24-year rule” that stipulated that reunification on marriage grounds was impossible unless both parties are 24 years old or older; the “attachment requirement”, whereby reunification would be granted only provided that the partners could show proof of a stronger affiliation to Denmark than to any other country, measured as their combined number of years of residence in different countries; and strong financial requirements on the part of the sponsor (that is, the person living in Denmark). The number of permits granted on family grounds reduced drastically in the years following the reform. In Essay II we show evidence that while the reform may have led to fewer family migrants in Denmark, it also led to affected individuals reuniting in Sweden with their non-European partners instead. Sweden’s geographical closeness and accommodating family reunification regulatory framework facilitated this response to the policy.

Methodologically, Essay II proceeds in two parts. In the first part, we analyze individual emigration behavior from Denmark, comparing affected versus not affected individuals, in the years before and after the reform, by estimating a difference-in-differences model based on repeated cross-sections. We find that the probability to emigrate to Sweden increases substantially after the reform, relative to the pre-reform migration rates in the analyzed population. In the second part, we corroborate our findings by studying the immigration behavior of affected and not affected couples that reunite in Sweden before and after the reform. Our results are highly robust to a number of alternative hypotheses. Furthermore, we analyze return behavior of the reunited individuals and find that up to a half leave within eight years, with the majority going back to Denmark. The results in this essay suggest that spillover effects of national migration policies can be substantial and should be considered when shaping new, country-specific, immigration policies.

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<sup>5</sup>The reform was adjusted in 2012 such that firms in certain (predominantly lower-skilled) industries were subject to additional hurdles before being able to sponsor a permit.

## Labor market effects of immigration

Economists have typically assumed that the labor market adjusts to immigration through wages.<sup>6</sup> The assumption is that immigrants and natives are perfect substitutes within the same skill cell.<sup>7</sup> The extent to which wages are affected therefore depends on the skill composition of immigrants relative to domestic workers. Depending on the elasticity of labor supply, relative wage changes may lead to adjustments to employment levels as well.

The empirical evidence is mixed and points to either at worst minor negative effects on native wages (e.g. Card 2009) or strong negative effects (e.g. Borjas 2003). In light of these contradictory findings, research in this area has moved towards considering different adjustment mechanisms.<sup>8</sup> On the worker side, adjustment can take place via educational attainment (Hunt 2012) or task specialization (see Foged and Peri 2016 for evidence on low-skilled natives moving away from manual tasks to more communication-intensive ones in response to a large inflow of low-skilled immigrants; and Peri and Sparber 2009 that show that high-skilled natives switch to managerial positions when faced with immigrants specializing in occupations requiring math-analytical skills). Firms, on the other hand, can respond by adjusting their production technologies so as to make use of the relatively more abundant skill group or by changing the output mix (see Dustmann et al. 2008 for a discussion). Finally, recent evidence shows that differences in labor market institutions may have a mediating role in the competition between native and immigrant workers (Foged et al. 2019).

In terms of methodology, most empirical studies use one of the following three strategies (Dustmann et al. 2016): i) the national skill-cell approach that uses variation in the share of immigrants across skill cells (e.g. Borjas 2003), ii) the spatial correlation approach, that uses variation across regions (e.g. Altonji and Card 1991) and iii) the mixture approach, which uses variation across both skill cells and regions (e.g. Card 2001). Each come with their own challenges in terms of identification, primarily stemming from the fact that immigration inflows are not exogenous and that immigrants do not randomly sort across regions (see de la Rica et al. 2015 for an in-depth discussion).

The first essay in this thesis also takes up the question of how immigration affects the labor market in Sweden. Compared to previous studies, it looks at a particular group of immigrants, namely non-European immigrants whose work permits are sponsored by local firms. I analyze the effect of non-European labor migration on firm performance and individual domestic workers.

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<sup>6</sup>The account that follows is largely based on Dustmann et al. (2008), Peri (2016) and Dustmann et al. (2016).

<sup>7</sup>Precisely how to define skill cells has been subject to some debate, but a binary division in terms of college- versus non-college-educated is the most widely agreed upon (Peri 2016).

<sup>8</sup>Dustmann et al. (2016) welcome this development, but argue that the main reason why studies reach such different results is because they identify fundamentally different parameters.

To put the methodology in perspective, I use a strategy that partitions the labor market in industries within the same sector. In that sense, it is close in spirit to the so-called mixture approach studies described above.<sup>9</sup> I do this as it allows me to take advantage of an immigration policy that generated higher inflows in some industries but not in others. As mentioned earlier, non-European labor immigration flows were very small from the 1970s until the end of 2008, when the reform studied in this paper was passed. Before the reform, firms that recruited in certain kinds of industries were not restricted. The reform virtually removed restrictions for all industries. Therefore, the change was felt more strongly in industries that were previously more restricted. These are predominantly lower-skilled, in sectors such as hotels and restaurants, retail and construction. My results thus touch on the effects of low-skilled immigration.

I use a difference-in-differences strategy where I compare firms in two groups of industries, before and after the reform: those for whom the restrictions were loosened the most (treated) versus those for whom there were no restrictions before (control). I find that employees in firms in treated industries earn more on average. Treated firms hire more at both ends of the skill distribution, but hire more intensely at the upper end relative to the pre-reform period. Thus, firms seem to take advantage of skill complementarities between immigrants and natives. I further follow native incumbents' employment and earnings over time, allowing them to move across firms. I find heterogeneous effects by age: the group of young (below 40) low-skilled workers in treated industries face worse employment prospects after the reform, whereas the group of older low-skilled workers have both better employment and earnings outcomes. Partitioning the data by pre-reform earnings quartiles instead, I find positive employment and earnings effects at the bottom of the income distribution, concentrated especially among non-European immigrants that had already been in Sweden.

## Children of immigrants

Immigrants are often driven to leave their countries of origin by a desire to ensure a better life for their children. Once in the host country, a number of factors may come together to either facilitate or prevent this desire from being fulfilled. Parents may not be able to integrate in the labor market immediately, depending on the extent to which they can transfer their skills.<sup>10</sup> They may face obstacles getting recognition for their foreign credentials or they may face discrimination by employers. Friedberg (2000) finds evidence that the educa-

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<sup>9</sup>However, I only estimate a reduced-form parameter.

<sup>10</sup>Countries may affect the skill transferability of immigrants by altering their admission criteria. However, there is little evidence on how intergenerational outcomes differ across admission classes (Sweetman and van Ours 2015).



tion and labor market experience obtained abroad are significantly less valued than human capital obtained in the host country. Using a correspondence testing design in Sweden, Carlsson and Rooth (2007) find that job applicants with Middle Eastern names are significantly less likely to receive callbacks than identically skilled applicants with Swedish names. To the extent that there exists intergenerational transmission in economic status, the children's future outcomes will be more or less affected by their parents'. Parents may also be highly positively selected on unobservable characteristics, regardless of the realized success on the labor market.

In Essay III, co-authored with Valentin Bolotnyy, we study the net effect of these forces by looking at the outcomes in adulthood (at age thirty) for a group of immigrants who arrive in Sweden before the age of fifteen. We show that conditional on family income, immigrants and natives earn similar levels of income and are slightly more likely to obtain a university degree. We further zoom in on the immigrant group and find that refugee children from countries like Bosnia, Syria, and Iran have higher intergenerational mobility than the average child immigrant. While immigrant parents from these countries on average find themselves with lower incomes than those from other countries, their children show some of the highest levels of income in adulthood among all immigrant children.

Given the findings in Essay III, the last essay - also co-authored with Valentin Bolotnyy - asks whether success in the labor market translates into success on the housing market, by studying patterns of home ownership for the same sample of immigrants who arrive as children. Home ownership is an important indicator of socio-economic status and a good proxy for wealth (e.g. Enström Öst 2012). Home ownership has been found to help individuals smooth consumption in the face of negative income shocks (Sodini et al. 2016). Housing wealth also allows parents to make investments in their children's education (Lovenheim 2011) and housing careers (Enström Öst 2012). We show that on average, immigrants are less likely to own their homes than natives at age thirty. The difference remains even after we control for socio-economic characteristics, parental background, and municipality of residence. We find that parental background - both in terms of parents' income and education, but also their own home ownership status - is the most important determinant of home ownership in adulthood. We argue that these findings provide support for the hypothesis that parents influence children's tenure choices via both direct financial transfers and socialization, the latter by shaping children's expectations for what constitutes a desirable standard of living (Henretta 1984). We also find suggestive evidence that preferences for owning and renting may be formed in childhood and depend on the composition of renters versus owners among co-nationals in the neighborhood one grows up. Finally, we investigate the role of age at arrival on outcomes in adulthood and find a significant negative effect of age at arrival on income and education, which also translates into

a lower probability of owning a home in adulthood. However, growing up in a highly-educated family may partly mitigate this negative effect.

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# 1. Firm- and Individual-Level Responses to Labor Immigration

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## 1.1 Introduction

How should countries regulate labor immigration? Labor immigration policies around the world lie on a spectrum between being entirely immigrant-driven and entirely employer-driven.<sup>1</sup> In immigrant-driven systems, governments select individuals based on certain characteristics, such as education, age and experience, without imposing the condition for the applicant to already have a job. An example would be a points-based system, such as the one practiced in Canada. Employer-driven systems, whereby it is the firm that initiates recruitment, require individuals to have a job offer in hand before they are able to apply for a permit. Most countries impose a so-called labor market test that specifies the kind of occupations that are available for foreign recruitment, the necessary qualifications an individual needs to have before being eligible, or the salary threshold that needs to be reached in order to be granted a permit.<sup>2</sup> The design of such policies influences the flows and composition of workers that are recruited.<sup>3</sup> In turn, these are likely to have effects on the host economy. This paper aims to quantify the effects on domestic firms and individuals of a change in the Swedish system that removed all previous restrictions and made it entirely employer-driven.<sup>4</sup>

I study the effect of non-European labor immigration on firm- and individual-level outcomes in Sweden. I exploit variation in the strictness of the immigration system induced by a reform that differentially lifted restrictions across industries.<sup>5</sup> Immigration policy tends to move slowly, changes are often small and take the form of tweaks to an existing system. Instead, the reform studied in this paper meant that Sweden went from having one of the most restrictive labor immigration system to one of the most liberal in the OECD (OECD 2011). The group of treated industries is the group for whom restrictions were loosened the most. They are predominantly lower-skilled, in sectors such as hotels and restaurants, retail and construction. Therefore my results touch on the effects of low-skilled immigration. I compare the two groups using a difference-in-differences (DiD) design. I find that employees in firms in treated industries earn more on average. Treated firms hire more at both ends of the skill distribution, but hire more intensely at the upper end relative to the pre-reform period. Thus, firms seem to take advantage of skill complementarities between immigrants and natives. I further follow native incumbents' employment and earnings over time, allowing them to move across firms. This

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<sup>1</sup>In the European Union, these refer to policies regulating immigration of third country nationals, referred to from now on as non-Europeans (non-EU).

<sup>2</sup>For example, the H1-B visa system in the US requires applicants to have at least a bachelor's degree in order to be eligible.

<sup>3</sup>See Czaika and Parsons (2017) for evidence on how different immigration systems affect the skill composition of workers.

<sup>4</sup>Before the change, the system was still employer-driven but many government actors were involved in the permit approval decision process.

<sup>5</sup>Industries are understood here as economic activities.



analysis yields estimates that are small in magnitude and weaker in terms of precision.

The shift from a highly restrictive system to a highly liberal one makes Sweden an ideal laboratory to study both the effect of immigration policies on the behavior of the main actors (here, the firms) and the subsequent effect of newcomers on firm and individual outcomes. The policy change took place in December 2008. As a result of the reform, firms were free to decide how many and what kind of non-European workers they needed to recruit from abroad.<sup>6</sup> Before the reform, firms that wanted to recruit in occupations that were not considered to be in shortage by the Public Employment Service had low chances of getting a permit approved. The reform eliminated the shortage criterion from the decision-making process. This gives rise to a situation where certain industries experience a more significant change to the policy environment, whereas for others the change is marginal. I use this variation to evaluate the effect of immigration on firm- and individual-level outcomes in a DiD setting.

In the first part of the analysis, I take the perspective of the firm. Firms became major players after the 2008 reform so it is natural to ask how they changed their hiring behavior in its aftermath. I use a matched employer-employee dataset that allows me to follow firms over time and study their hiring and separation patterns, size and average earnings. I find strong evidence that treated firms offer higher average earnings and weaker evidence that they grow. The fact that I can link employees to firms allows me to look at skill-specific hiring and separation and to study skill composition at the firm over time. I find that the firm hires more high-skilled workers relative to the pre-reform period, which results in a higher share of high-skilled at the firm. Since the newcomers go to predominantly low-skilled industries, these results are consistent with a skill complementarity framework.

I next follow the native individuals employed at these firms and look at their outcomes over time. Firms and individuals solve different optimization problems, so it may well be that what is beneficial to the firm is not necessarily beneficial to the individual. Using a similar design as for the firm-level analysis, I find that, on average, low-skilled individuals in treated industries are more likely to stay at the firms they are matched to in 2008 and also earn more (although results are imprecisely estimated). However, the average result masks heterogeneous effects along the age dimension. I find that young (below 40) low-skilled natives are less likely to be employed and may also earn less. I show the opposite holds for the group of older low-skilled, which suggests a higher degree of complementarity between older low-skilled natives and younger low-skilled immigrants. Surprisingly, non-European immigrants that had already been in Sweden at the time of the reform seem to benefit the

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<sup>6</sup>The only two requirements placed on them was to advertise the vacancy for at least ten days at the Public Employment Service and to pay prevailing wages.

most.<sup>7</sup> Previous literature tends to find the opposite (Card 2001, Ottaviano and Peri 2012).

The use of matched employer-employee datasets in the immigration literature is still relatively rare. Furthermore, unlike this paper, most studies that take the perspective of the firm focus on the effect of high-skilled immigration. There are two reasons for that: i) most studies use US data and examine workers on H1-B visas who are by definition high-skilled (Doran et al. 2014, Kerr et al. 2015)<sup>8</sup> and ii) the immigration inflows during the study period happen to be predominantly high-skilled (Mitaritonna et al. 2017).<sup>9</sup> One exception is Malchow-Møller et al. (2012) who look at the effect of low-skilled immigration on native wages at the firm in the Danish context. They find negative effects on native wages at the firm, particularly the low-skilled. They argue that the most likely mechanism is through a loss in bargaining power: if the newly-hired immigrants have worse outside options, the firm signals that it has an alternative pool of potentially cheaper workers to hire from, thereby reducing the bargaining power of incumbents. The effect is likely to be accentuated when the degree of substitutability between immigrants and natives is high. My work is complementary to this study since I also look at employment outcomes, hiring and separation rates, which paints a more complete picture of the different ways in which firms adjust to an increase in the potential pool of labor supply.

The variation I exploit allows me to study firms across different sectors. Previous studies often restrict their analysis to one sector (e.g. Bratsberg and Raaum 2012 look at construction workers, Mitaritonna et al. 2017 study manufacturing firms). I also make no significant firm size restrictions, whereas previous studies focus exclusively on large firms (e.g. Kerr et al. 2015).<sup>10</sup> My

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<sup>7</sup>A large share of this group is composed of older immigrants arriving from the Former Yugoslavia, Bosnia, Iraq, Turkey and Iran. Immigrants from these countries of origin have to a large extent been in Sweden for a long time.

<sup>8</sup>This literature finds mixed results. Doran et al. (2014) find that new H1-Bs crowd out natives with similar observable levels of innovation and that employment at the firm at most modestly increases, with some evidence for at least partial crowding-out of other foreigners. However, they conclude that their results are not incompatible with the possibility that in the aggregate, H1-Bs might still lead to greater innovation and employment, since the crowded-out workers can move to firms where they can increase innovation. Kerr et al. (2015) study the effect of young skilled immigration on the hiring and separation patterns of firms. They find that the share of skilled workers at the firm increases with the share of young skilled immigrants at the firm, with the result being driven by an increase in the share of young skilled natives at the firm to the detriment of the share of older skilled natives. They interpret their results as evidence for a higher degree of complementarity between young skilled natives and young skilled immigrants.

<sup>9</sup>Mitaritonna et al. (2017) study French manufacturing firms that increase their hiring of foreigners following an increase in the (mostly high-skilled) labor supply of foreigners in the local labor market. They find that average wages of natives increase at the firm. They also show evidence for increased mobility of high-skilled natives, who move to firms that hire fewer immigrants.

<sup>10</sup>The only restriction I impose is for firms to have at least 2 employees in 2008.

sample is primarily made up of small firms, with at most 50 employees, which is a more realistic reflection of the size distribution of firms in the economy. Small firms hire fewer immigrants per firm, but collectively they hire around the same amount as bigger firms. In Sweden, small firms also have the largest number of vacancies so they are potentially the most likely to benefit from a liberalized immigration system (IOM 2012).

The individual-level findings resonate with previous findings from the few existing studies that use longitudinal data. In the Danish context, Foged and Peri (2016) exploit a refugee dispersal policy to show that an increase in the number of predominantly low-skilled refugee-country immigrants pushed less educated native workers to change occupations towards occupations with less manual content. This result is especially salient when allowing individuals to move across establishments and municipalities. This implies that occupational adjustments are accompanied by movements across firms. They further find similar wage effects regardless of whether they allow individuals to move across firms. They thus conclude that the native unskilled that stay at the firm are those that are the most complementary to the newcomers in terms of task specialization. Although I cannot test the hypothesis that those that stay at the firm start performing different tasks than the newcomers, I do find that the low-skilled are more likely to stay at the firm and also earn more, which would be consistent with the same mechanism operating in this case. Bratsberg and Raaum (2012) follow Norwegian individuals in the construction sector over time. They exploit the fact that certain industries within the construction sector have licensing requirements and others do not, thereby generating exogenous variation in the inflow of immigrants across industries in the sector. They find that the wages of the highly-exposed natives are reduced and conclude that at least in the construction sector, immigrants and natives are close to perfect substitutes. In the sectors I study - predominantly services - I find (weak) evidence for a decrease in earnings only for the group of young low-skilled.

This is also the first paper that examines the effect of non-European labor immigration in Sweden.<sup>11</sup> With respect to the policy itself, I show that even in the presence of loose labor immigration restrictions, in absolute numbers, there are few firms in the private sector that take the opportunity. The main reason is that many occupations that are currently classified as being in shortage by the Public Employment Service are concentrated in the public sector, which I exclude from my analysis. However, those also tend to be occupations that require Swedish licenses (doctors, nurses, teachers).

Labor immigration at both ends of the skill distribution is an important tool that countries can use to address labor shortages due to increased population aging and age dependency ratios. Moreover, Sweden in particular has stopped giving permanent permits to refugees since 2015, and since family reunifica-

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<sup>11</sup> Several reports (OECD 2011, Emilsson et al. 2014) provide descriptive-level evidence of the impact of the reform.

tion is only granted to refugees with permanent permits, both of these channels are expected to contribute less to the total flows in the future. Hence, non-European labor immigration is likely to become an even more important channel in years to come. Understanding how firms and natives react is therefore crucial for the optimal design of labor immigration policy.

This paper is structured as follows: the next section gives the institutional background; section 1.3 lays down the empirical strategy; section 1.4 presents the firm-level results; section 1.5 describes the individual-level results. Finally, section 1.6 summarizes and concludes.

## 1.2 Institutional background

### 1.2.1 Pre-reform period

Non-Nordic labor immigration reached its peak in Sweden in the mid-1960s due to severe labor shortages that accompanied its post-war industrial boom.<sup>12</sup> Up until that point, workers entered either via bilateral agreements (1940s and 1950s) or as tourists (early 1960s). Tourists were allowed to search for jobs during the three months the visa lasted; if successful, they could switch to a work permit without leaving Sweden (Emilsson et al. 2014). Starting in 1966, however, the rules became more restrictive. Non-Nordic migrants were now required to obtain permits and housing before entering Sweden. Large flows continued to arrive despite the change until 1972, when the Swedish Trade Union Confederation (LO) sent out a circular to its unions asking them to reject applications for non-Nordic work permits (OECD 2011).

As a result, non-Nordic labor immigration continued to decrease until Sweden joined the EEA in 1994 and the EU in 1995. Afterwards, the highly restrictive rules applied only to non-European immigrants.

### 1.2.2 The December 2008 reform

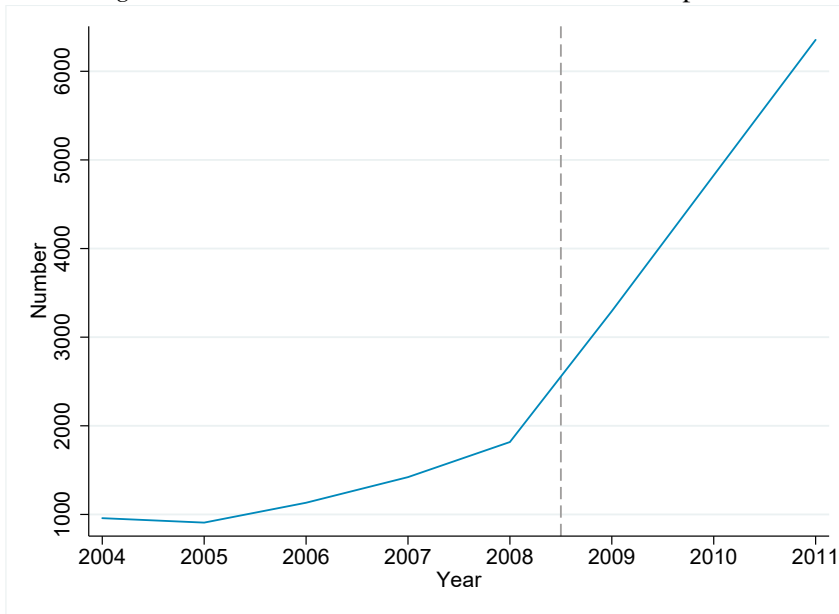
The system next went through a major change in December 2008, when the reform I study in this paper was introduced. Up until that point, employer organizations were demanding looser immigration restrictions because they could not find the right set of skills in Sweden. At the same time, unions were worried primarily about the effect of low-skilled immigration on domestic workers and they were resisting any change to the system. An investigation revealed severe shortages in certain sectors and regions and those that drafted the report advocated for a more liberal system (Ministry of Justice 2006). Two factors in particular helped pass the proposal: i) the fact that the 2004 and 2007 opening of the European borders did not result in a significant increase in flows

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<sup>12</sup>With the 1954 Nordic Agreement, Nordic citizens were free to move across the Nordic countries without the need for residence and/or work permits.

that might have helped bridge the gap in skills that by now was acknowledged to exist and ii) the 2006 election whereby a center-right government came to power. The change in government gave a stronger voice to employer organizations to the detriment of trade unions (Cerna 2009). The reform made the system purely employer-driven and reduced the role of unions, who, up until that point, had a *de facto* veto on which permits were approved and which were rejected (OECD 2011). I expand on this point later in this section. The main requirements for eligibility on the part of the employer are to show that they are able to pay prevailing wages and that they advertised the vacancy for at least ten days at the Public Employment Service (PES).<sup>13</sup> The firm does not have to show proof of interviewing other candidates who respond to the job ad prior to recruiting a non-EU worker. There are no skill requirements and no quotas. Figure 1 shows a significant increase in the number of work permits valid for at least a year granted before and after the reform.

Figure 1. Number of non-EU individuals on work permits



Notes: The figure plots the number of non-EU individuals on work permits that are valid for at least 12 months. Source: Statistics Sweden

In order to obtain a work permit, a non-European citizen needs to hold a job offer from a firm in Sweden.<sup>14</sup> Before the new rules came into place, in order for a work permit to be approved, the Swedish Migration Board was

<sup>13</sup>Prevailing wages are either in accordance with collective agreements or prevailing practice in the industry.

<sup>14</sup>If granted, a work permit is given for an initial period of maximum two years. During this time, the employee is tied to both the employer and the occupation for which the permit is given. If

consulting a shortage list, drawn by the Public Employment Service twice a year and that identifies occupations for which there is a shortage of suitable job-seekers. The higher the score, the higher the shortage, and therefore the higher the probability of getting a permit approved. Each occupation gets a score from 1 to 5, where 5 means there is very low competition for jobs within that occupation.<sup>15</sup> The PES assesses the situation in the labor market by running an employer survey, where they ask around 12000 private firms about their recruitment needs, and by discussing with municipal authorities and county councils. The final score is a qualitative assessment of the information coming from these various sources.<sup>16</sup>

The shortage list stopped being used as a criterion in judging work permit applications after December 2008 (IOM 2012). For firms that wanted to recruit in the pre-reform period in occupations high on the shortage list, the odds would have been high already in the pre-reform period. For firms that instead wanted to recruit in occupations lower on the shortage list, the chances would have been rather small. The December 2008 reform therefore removed institutional restrictions to a larger extent for firms that employ workers in occupations that the PES considers low in shortage. Regardless of the self-assessed need of the firm, a firm wanting to employ, for example, a cleaner, would have had a hard time doing so before the reform. That changed after the reform.

Figure 2 shows the number of permits valid for at least three months by occupation group.<sup>17</sup> There are two important things to note here: one, the number of permits granted in 2005 is much smaller than in 2010 and 2011 (even when we account for the fact that the data for 2005 cover only half a year), and two, the occupational distribution is skewed towards professionals in the pre-reform period. Whereas this group makes up a significant portion of workers in the post-period, the number of workers in elementary occupations is significantly larger.

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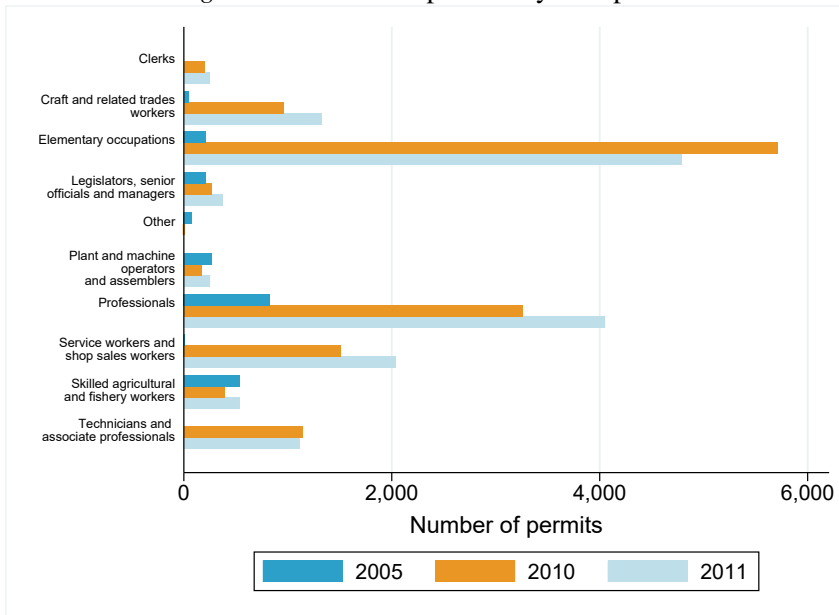
the employee wants to change employers regardless, they have to apply for a new permit, which implies that they need to have a new job at the time of application. If a worker loses their job, they have three months to find a new one. If by the end of this period they haven't found a new job, they have to leave Sweden. When the first permit expires, it is possible to apply for an extension of the permit, for an additional two years maximum. During this time, the worker is free to switch employers but not occupations. After four years, the worker can apply for permanent residence (OECD 2011).

<sup>15</sup>Each regional PES office gives a score for each occupation that is relevant in their local labor market. The final score is a weighted average of all the regional scores.

<sup>16</sup>Author's communication with a PES employee.

<sup>17</sup>The information for the post-reform period is of higher quality; for the pre-reform period, the only year for which this information is available is 2005; the data cover the months from January to July.

Figure 2. Number of permits by occupation



Notes: The 2005 data only covers the months from January to July. Sources: Migration Board and Public Employment Service

In the aftermath of the reform, the fact that indeed workers started being recruited in occupations that the PES considers in low shortage, started to make headlines. Two main complaints were put forward soon after the reform was put in place. One, that the lack of oversight left room for abuses of the system, and two, that there were too many workers in low shortage occupations and that unemployed domestic workers were cut out from access to these jobs as a result.<sup>18</sup> This debate led to an adjustment to the reform in 2012. Starting in January 2012, firms in certain industries have to go through additional hurdles in the application process.<sup>19</sup> For this reason my sample period ends in 2011.

Relative to all immigrants that come to Sweden for work purposes, the non-EU immigration channel has become more important in recent years. In 2008, non-EU workers represented 30% of all workers who are registered in Sweden

<sup>18</sup>However, recent evidence shows that reducing low-skilled immigration does not necessarily result in an increase in native employment (Clemens et al. 2018, Lee et al. 2017).

<sup>19</sup>The 5-digit-level industries concerned are primarily in the hotels and restaurants sector, construction, and retail trade. They now have to show evidence that they can provide a salary to the person they are planning on bringing for at least three months. Firms that have previously employed non-EU workers have to additionally provide the last three monthly tax account statements. Firms operating for less than a year are subject to the rules regardless of the industry they were operating in. Firms with more than 50 employees, however, are exempt from the rules (Migrationsverket 2018).

for at least a year.<sup>20</sup> By 2011, that number almost doubled, to 57%.<sup>21</sup> These numbers represent a lower bound, since they only concern individuals who register in Sweden for at least one year.

## 1.3 Empirical strategy

### 1.3.1 Definition of treatment

As highlighted in the previous section, the reform I study meant a removal of all restrictions in place before with respect to recruiting foreign workers. For identification of the reform effect, I use the fact that the pre-reform restrictions were not affecting firms equally. Hence their removal gave rise to variation in the possibility to hire from abroad.

I use the 2009 report “Where are the jobs” (Public Employment Service 2009) to obtain the list of occupations and their respective shortage scores for 2008. Given that the reform took place in December 2008, 2008 is the last pre-reform year and 2009 the first post-reform year. I match the scores by occupation name to occupational codes (SSYK 1996).<sup>22</sup>

I first create a mapping between occupations and industries, which in turn allows me to create the index at the industry level.<sup>23</sup> I use information from the 2008 wage register (*Lönestrukturstatistik*) which covers a sample of private firms and the individuals matched to them. For each individual, I know their occupational and industry (SNI) codes. I calculate the index for each industry  $i$  using the following formula:

$$index_i = \sum_o \alpha_{oi} \times score_o \quad (1.1)$$

where  $\alpha_{oi}$  is the share of people working in occupation  $o$  in industry  $i$ ;  $score_o$  is the shortage score for occupation  $o$ . For those occupations that are not on the list, I impute the average score in the industry.<sup>24</sup> This formula has the advantage of capturing i) the fact that some occupations are more likely to occur in certain industries than in others and ii) the fact that certain occupations are given a higher score than others.

<sup>20</sup> Author’s calculations based on data from Statistics Sweden. All workers include EU workers, including the self-employed, and non-EU workers.

<sup>21</sup> Relative to all flows, labor immigration represents a minor immigration channel. In 2008, it represented around 2.5% of all flows. In 2011, however, it made up around 9.4% of all flows. See also A.2.

<sup>22</sup> Most scores are given for 4-digit occupational codes, but a minority are given for 3-digit occupational codes. There are certain occupations that have the same SSYK code but in the list they are given different scores (e.g. bartenders/baristas). In that case I take an average of the scores corresponding to that code.

<sup>23</sup> I do this because my main dataset does not contain information on occupations.

<sup>24</sup> Very few industries have no occupations on the list. I drop these.



I calculate the index at the 5-digit industry level. I keep only those industries that employ at least 30 individuals. The sectoral distribution of the industries for which I am able to calculate the index versus the population is shown in Figure A.1 in the Appendix. The sectoral distribution is largely maintained.

I take the negative of the index obtained with the formula above for ease of interpretation: *the higher the index, the lower the shortage, the more affected the industry*. I standardize the index to have mean zero and standard deviation one.<sup>25</sup>

### 1.3.2 How well does the exposure measure predict non-EU hiring?

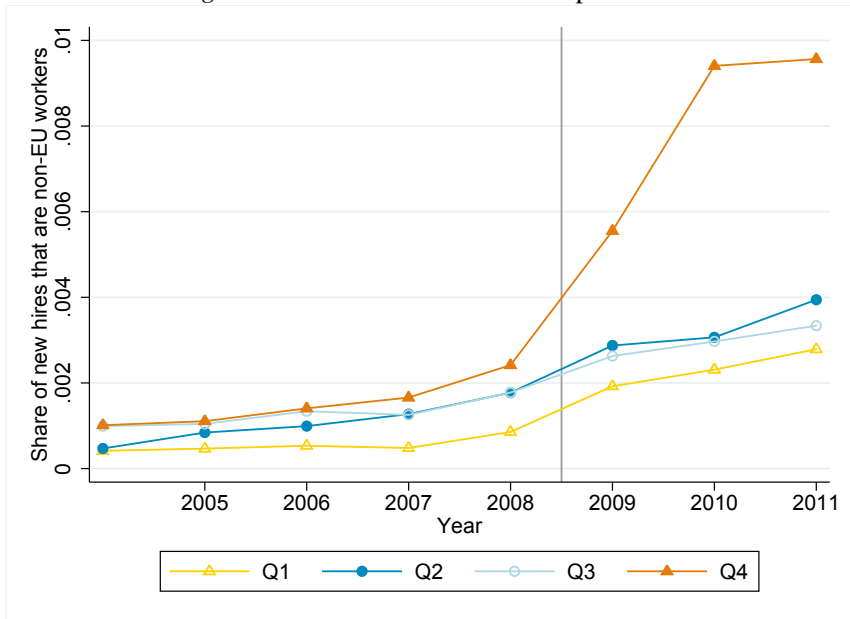
Firms in industries that were classified as being in lower shortage before the reform were more affected by the December 2008 reform than firms in industries classified as being in higher shortage. Given that fewer restrictions were put in place for the higher-shortage firms, these firms would have had the opportunity to hire from abroad even before the reform. We therefore expect an increased response for firms in the upper part of the distribution of the index.

Figure 3a shows the share of newly-hired non-EU workers relative to all new hires, by year and quartile of the index. We see an increased response across all quartiles but a particularly high increase after the reform from firms in industries in quartile 4. Figure 3b shows that in the pre-reform period, firms in quartile 2 were doing the most hiring, with firms in quartile 3 and 4 doing roughly the same amount of hiring. Firms in quartile 1 hired the least, perhaps surprisingly. However, a lot of the high-skilled non-EU hires have short-term permits (valid for less than a year), in which case I wouldn't be capturing them. In the post-reform period, however, while firms in all quartiles hire more, firms in the fourth quartile hire significantly more compared to pre-reform, which is precisely what we would expect given the fact restrictions were lifted for industries in quartile 4 to a larger extent than for industries in the other quartiles. Since firms in quartile 4 react the most, I categorize these firms as treated and all the rest as control.

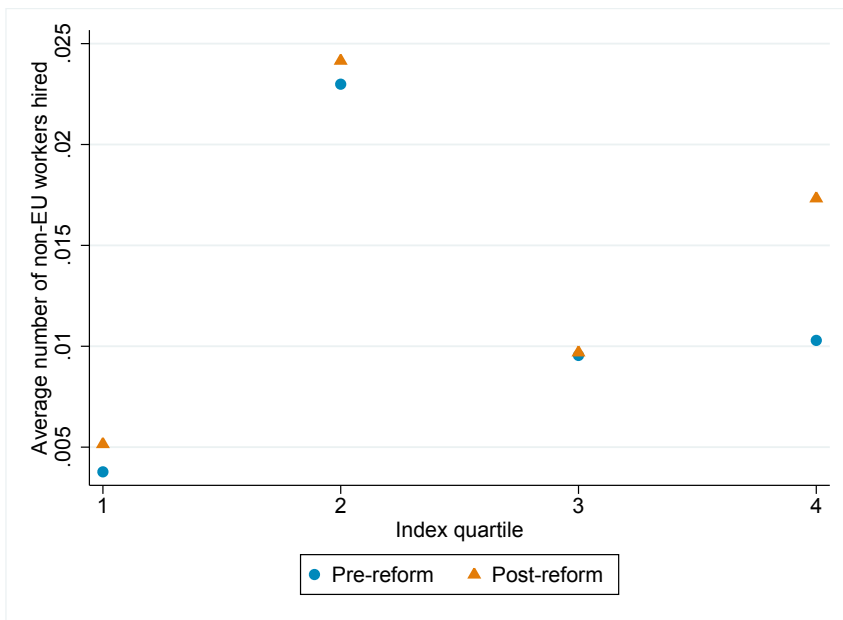
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<sup>25</sup>One worry could be that the 2008 score doesn't capture well the overall pre-reform shortages in the labor market. An alternative would have been to use the average score over the entire pre-reform period. I collect yearly scores from the entire pre-reform period and construct an average score as well. As we can see in Figure A.3, the 2008 score is highly correlated with the average score. The pairwise correlations among the pre-reform years are also high (see Table A.1).

Figure 3. Illustration of natural experiment



(a) Share of non-EU workers out of all new hires



(b) Number of non-EU hires, by quartile

Notes: Firms are grouped into quartiles depending on where the industry they belong to in 2008 lies in the index distribution.

### 1.3.3 Specification

#### Firm-level analysis

I use a difference-in-differences setup where the treatment group is made up of firms that are in the fourth quartile of the index distribution. As Figure 3 has shown, industries in the upper distribution of the index are those that respond the most to the reform. I estimate the following equation:

$$y_{ijt} = \alpha_i + \beta_t + \theta_{s,t} + \delta_j \times t + \gamma(I_{t \geq 2009} \cdot D_j) + \varepsilon_{ijt} \quad (1.2)$$

where  $y_{ijt}$  is the outcome in firm  $i$  in industry  $j$  and year  $t$ ,  $\alpha_i$  is a vector of firm fixed effects,  $\beta_t$  is a vector of year fixed effects,  $\theta_{s,t}$  is a vector of sector-by-year fixed effects, where sectors are at the 1-digit level, and  $\delta_j$  is a group-specific trend.<sup>26</sup>  $I_{t \geq 2009}$  is an indicator for an observation after the introduction of the December 2008 reform,  $D_j$  is an indicator for being in one of the treated industries.  $\varepsilon_{ijt}$  is the error term.  $\gamma$  is the coefficient of interest. Assuming that the trends in the outcome would have been similar in firms in industries for which the policy environment changed to a larger extent than for firms in sectors for which it changed to a lesser extent, the estimate  $\hat{\gamma}$  captures the causal effect of the reform.

#### Individual-level analysis

I follow largely the same strategy as in the firm-level analysis, with treated individuals defined as those in treated industries in 2008. I estimate the following equation:

$$y_{kjt} = \lambda_k + \tau_t + \eta_{s,t} + \mu_j \times t + \psi(I_{t \geq 2009} \cdot D_j) + \phi X_{kt} + \varepsilon_{ikt} \quad (1.3)$$

where the  $\lambda_k$ 's are individual fixed effects,  $\tau_t$  are year fixed effects,  $\eta_{s,t}$  are sector-by-year fixed effects and  $X_{kt}$  is a set of time-varying covariates: age, age squared, labor market experience, labor market experience squared, a dummy for being married and a dummy for having children.

## 1.4 Firm-level analysis

### 1.4.1 Data and descriptive statistics

#### Identifying non-EU workers in the data

The data I use for my main analysis contains a variable that designates the reason for settlement in Sweden. I am interested in individuals whose first permit in Sweden is a work permit, i.e. in workers who have been recruited directly from abroad. I therefore take the matched employer-employee sample and find the first firm the non-EU worker works at in Sweden. I cross check

<sup>26</sup>Including additional county-by-year fixed effects changes the results little. They are available upon request.

this information with information on year of arrival. For around 95% of individuals, the first time they are observed with a firm corresponds to their year of arrival in Sweden or one year later. The latter is largely due to the fact that the register gets updated in November so for those arriving later they would show up in the register one year later. The remaining 5% could be due to measurement error. I am therefore capturing a lower number of workers given that some people already in Sweden could switch to work permits.<sup>27</sup>

In Table A.2, I show descriptive statistics for non-EU workers for the year they are hired. I divide them in four groups, by treatment status and by period of arrival. We can see that individuals in treated industries earned less than those in control industries even before the reform, but earnings dropped in both groups after the reform. The share of low-skilled increased by more than half in the treatment group (but there is a significant proportion of newcomers with missing skill information). Non-EU workers arriving after the reform in treated industries are to a large extent drawn from the lower part of the skill and earnings distribution. This is important to keep in mind when interpreting the results.

### **Sample restrictions and definition of key variables**

I use a matched employer-employee panel for the period 2003-2011. In each year, I keep individuals aged 18-65 whose annual earnings are above the 10th percentile in the annual earnings distribution. This is so as to not consider individuals that are matched to a firm after only having worked a few hours at that respective firm. I impose two restrictions on the firm: 1) they have to exist in 2008 and 2009 and 2) they have to have at least two employees in 2008. The latter serves to remove the self-employed. However, I do include firms that are owned by self-employed who employ others. I restrict to private sector firms. I do the analysis at establishment level.<sup>28</sup>

For each firm, I measure *firm size* as the number of employees matched to the firm each year. I calculate *mean earnings* as the average annual earnings of the employees matched to a firm. I consider an individual as newly-hired if they are in the firm in year  $t$  but not in year  $t - 1$ . I define the *hiring rate* as the number of new hires as a share of total firm size in 2008. A new hire is a non-EU worker if I have previously identified those workers as non-EU work permit holders following the steps outlined above. I consider a person to separate from the firm if they were observed in  $t - 1$  but not in  $t$ . *Separation rates* are calculated analogously to hiring rates.

### **Descriptive patterns**

Table 1 shows descriptives for all firms, by treatment status. Regardless of treatment, the majority of firms have fewer than 50 employees. There are

<sup>27</sup>Students make up the group most likely to switch but I argue that they have a set of advantages with respect to their knowledge of the Swedish labor market that complete outsiders do not.

<sup>28</sup>I use establishment and firm interchangeably in the rest of the paper.

**Table 1.** *Firm-level characteristics (all)*

|  | Treated |         | Control |         |
|--|---------|---------|---------|---------|
|  | Mean    | SD      | Mean    | SD      |
| <i>Firm size</i>   | 11.799  | 34.319  | 16.005  | 86.840  |
| <i>Mean earnings</i>                                     | 241.374 | 103.309 | 291.990 | 147.037 |
| <i>Non-EU</i>  | 0.120   | 0.251   | 0.051   | 0.151   |
| <i>Women</i>   | 0.560   | 0.330   | 0.216   | 0.261   |
| <i>Age</i>   | 39.994  | 8.793   | 41.909  | 7.688   |
| <i>Low-skilled</i>                                       | 0.738   | 0.274   | 0.811   | 0.264   |
| <i>Medium-skilled</i>                                    | 0.128   | 0.175   | 0.096   | 0.160   |
| <i>High-skilled</i>                                      | 0.127   | 0.204   | 0.086   | 0.190   |
| <i>Missing skill</i>                                     | 0.008   | 0.051   | 0.007   | 0.048   |
| <i>Average skill content</i>                             | 1.708   | 0.358   | 1.917   | 0.170   |
| <i>Firm age</i>  | 4.239   | 1.446   | 4.364   | 1.344   |
| <i>Primary sector</i>                                    | 0.001   |         | 0.069   |         |
| <i>Manufacturing</i>                                     | 0.005   |         | 0.213   |         |
| <i>Electricity, gas and water supply</i>                 | 0.000   |         | 0.016   |         |
| <i>Construction</i>                                      | 0.000   |         | 0.239   |         |
| <i>Wholesale and retail trade</i>                        | 0.407   |         | 0.138   |         |
| <i>Transportation and storage</i>                        | 0.006   |         | 0.118   |         |
| <i>Hotels and restaurants</i>                            | 0.160   |         | 0.005   |         |
| <i>Information and communication</i>                     | 0.005   |         | 0.004   |         |
| <i>Financial, insurance and real estate activities</i>   | 0.062   |         | 0.014   |         |
| <i>Professional, scientific and technical activities</i> | 0.077   |         | 0.063   |         |
| <i>Personal services</i>                                 | 0.277   |         | 0.123   |         |
| <i>Firm size <math>\leq 50</math></i>                    | 0.966   |         | 0.950   |         |
| <i>Firm size 51-100</i>                                  | 0.023   |         | 0.029   |         |
| <i>Firm size 101-200</i>                                 | 0.008   |         | 0.013   |         |
| <i>Firm size <math>&gt;200</math></i>                    | 0.003   |         | 0.008   |         |
| <i>Observations</i>                                      | 59,868  |         | 86,071  |         |

*Notes:* Firm-level characteristics refer to 2008. Treated firms are firms in industries that are in the fourth quartile of the index distribution. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each quartile. Firm age data starts in 2003, so the oldest firm can be at most 5.

slightly bigger firms in the control group. The treated firms have employees that on average earn less than the employees in the control group. They are also slightly younger and predominantly female. Treated firms are found largely in the wholesale and retail trade, services and hotels and restaurants sectors.<sup>29</sup> More than twice as many employees in treated firms are born in a non-European country.<sup>30</sup> A higher share of employees in the treated firms are high-skilled.<sup>31</sup>

Table A.4 shows the same type of descriptives but only for firms that actually hire non-EU workers at least once during 2009-2011. The earnings, gender and age patterns remain for treated firms. Employees in treated firms earn almost twice as much on average than those in control firms. More treated firms that hire during 2009-2011 are small, relative to firms in the control group. Almost 60% of treated firms that hire non-EU workers are in the hotels and restaurants sector. More than half of the employees in treated firms are born in a non-European country. Note that firms in the control group also have a significant share of non-Europeans, which is in line with survey evidence on the likelihood of hiring from abroad that shows that firms with a larger pool of foreigners are more likely to hire foreigners (Winkelmann 2001).<sup>32</sup> Note that overall, firms in treated industries are more likely to have high-skilled employees (see Table 1), but the firms that actually take up the opportunity to hire non-EU workers are less likely to have high-skilled workers as compared to control firms that take it up (11% compared to 19%). This suggests that firms that take up the program are drawn from the lower part of the skill distribution among those eligible.

## 1.4.2 Results

Figure 3 showed descriptively that firms in industries in the highest quartile of the shortage index distribution hire the most non-EU workers after the reform. To probe that finding more formally, I estimate equation (1.2) for two outcomes: the number of newly-hired non-EU workers and the share of newly-hired non-EU workers relative to all the new hires. Table 2 shows the estimated

<sup>29</sup>For the purposes of this table, I put the following sectors together: agriculture, forestry and fishing and mining and quarrying (primary); electricity, gas, steam and air conditioning supply and water supply, sewerage, waste management and remediation activities (electricity, gas and water supply); financial and insurance activities and real estate activities (financial, insurance and real estate activities); administrative and support service activities, public administration and defence, compulsory social security, education, human health and social work activities, arts, entertainment and recreation and other service activities (personal services).

<sup>30</sup>Note that the non-EU definition here only looks at country of birth and not citizenship.

<sup>31</sup>In Table A.3, I show what predicts being in the treatment group, accounting for sector-by-year fixed effects.

<sup>32</sup>Another mechanism at work here could be that immigrant managers hire immigrant workers, as found in Åslund et al. (2014).

coefficient on the interaction,  $\gamma$ , in equation (1.2) for these two outcomes. Column 1 says that firms in treated industries hire 0.009 non-EU workers more, which translates roughly into nine workers for each 1000 firms. The pre-reform average in the control group is 0.008 workers. Hence, the reform had the effect of doubling the number of workers in treated firms relative to the control firms. The share of non-EU workers relative to new hires increases by 0.2 percentage points. Given that the pre-reform average in the control group is 0.002, the share of non-EU workers relative to new hires also doubles. Below I test whether this effect is driven by a shift in the composition of hires or if the overall hiring rate also goes up.

In Figure A.4a I show the coefficients from running the same regression on number of non-EU hires separately for each firm size category. Firm size categories are calculated in 2008. We see that the bigger firms are affected the most in terms of number of non-EU hires. However, when looking at relative shares, we see that small and big firms do roughly the same amount of non-EU hiring (Figure A.4b). This result corroborates descriptive findings in OECD (2011) and Emilsson et al. (2014) that the reform provided the opportunity for smaller firms to recruit from abroad.

**Table 2.** *Effects of December 2008 reform on non-EU hiring outcomes: Difference-in-differences with binary treatment*

|                       | Number of<br>non-EU workers<br>(1) | Share of non-EU<br>relative to new hires<br>(2) |
|-----------------------|------------------------------------|---|
| Coefficient           | 0.009<br>(0.003)                   | 0.002<br>(0.001)                                |
| Observations          | 1,074,568                          | 630,042   |
| Clusters              | 449                                | 449   |
| Pre-reform<br>average | 0.008                              | 0.002   |

*Notes:* Regressions include year, firm fixed effects, sector-year fixed effects and a group-specific trend. Treatment is defined as being in the fourth quartile of the index. Pre-reform averages refer to the control group. Standard errors in parantheses and clustered at the level.

The reform thus resulted in firms hiring significantly more non-EU workers. The question that follows is whether the inflow of immigrants had an effect on firm outcomes. Table 3 shows the estimates of the coefficient on the interaction in equation 1.2 from regressions on firm size (measured as the log of the number of employees), the overall hiring rate, the overall separation rate and mean earnings (in logs). Firms in treated industries grow by around 2%, but this result is not significant. The reform has a positive effect on hiring rates

and a negative effect on separation rates, but the coefficients are never significant. The average result on hiring is driven by an initial positive effect and a subsequent negative effect (Figure 4b), whereas the yearly coefficients on separation rates are rather stable over time (Figure 4c). In sum, these results suggest that treated firms not only hire more non-EU immigrants, but they hire more overall, and keep their workforce to a larger extent. This results in an increase in firm size.

The most striking results in Table 3 are on mean earnings: they go up by around 2.5% on average, jumping in the first year after the reform then roughly stabilizing at the higher level (Figure 4d). Figure 5 shows that the average result is driven by the smallest firms.

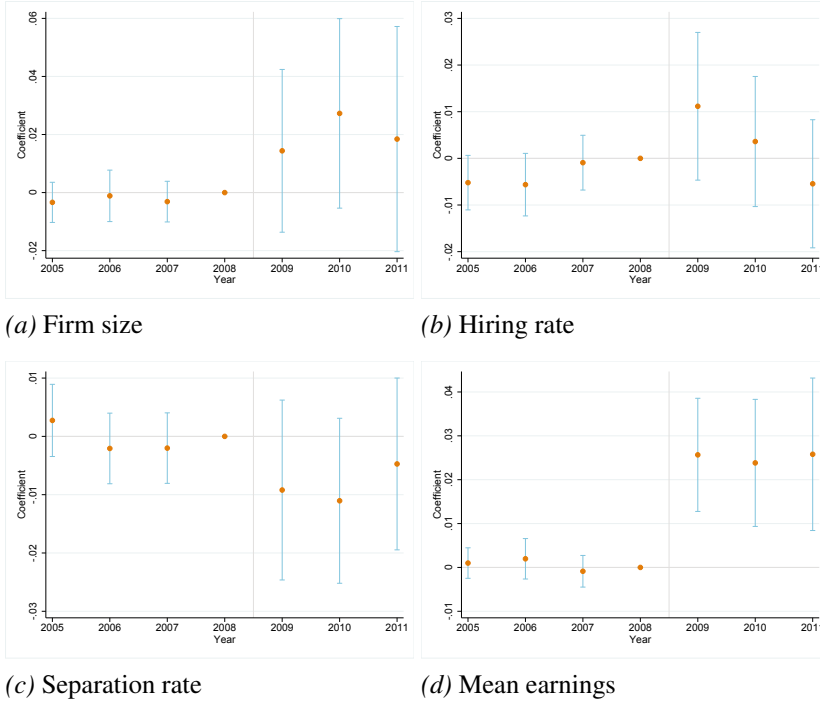
**Table 3.** *Effects of December 2008 reform on firm-level outcomes: Difference-in-differences with binary treatment*

|                       | Firm size<br>(1) | Hiring rate<br>(2) | Separation rate<br>(3) | Mean earnings<br>(4) |
|-----------------------|------------------|--------------------|------------------------|----------------------|
| Coefficient           | 0.020<br>(0.015) | 0.010<br>(0.007)   | -0.008<br>(0.007)      | 0.025<br>(0.007)     |
| Observations          | 1,074,568        |                    |                        |                      |
| Clusters              | 449              |                    |                        |                      |
| Pre-reform<br>average | 1.785            | 0.169              | 0.161                  | 5.615                |

*Notes:* Regressions include year, firm fixed effects, sector-year fixed effects and a group-specific trend. Treatment is defined as being in the fourth quartile of the index. Pre-reform averages refer to the control group. Standard errors in parantheses and clustered at the level.

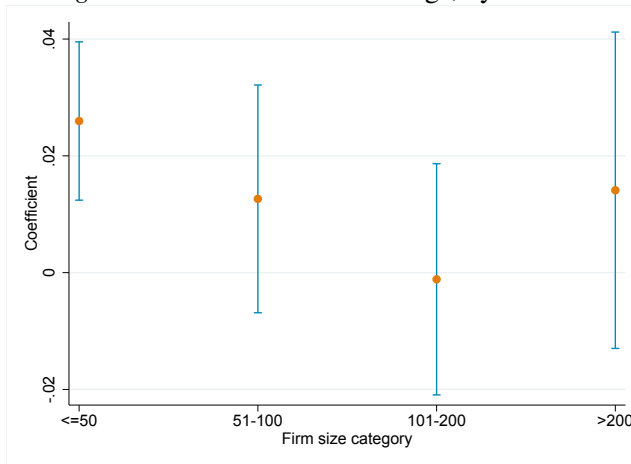


Figure 4. Firm-level outcomes: yearly coefficients



Notes: Firms are restricted to exist both in 2008 and 2009 and to have at least 2 employees in 2008. Firm size and mean earnings are measured in logs. Regressions include year, firm, sector-year fixed effects and a group-specific linear trend. Standard errors are clustered at the 5-digit industry level.

Figure 5. Effect on mean earnings, by firm size



Notes: Estimates from regressions with group-specific linear trend. Firm size category is measured in 2008.

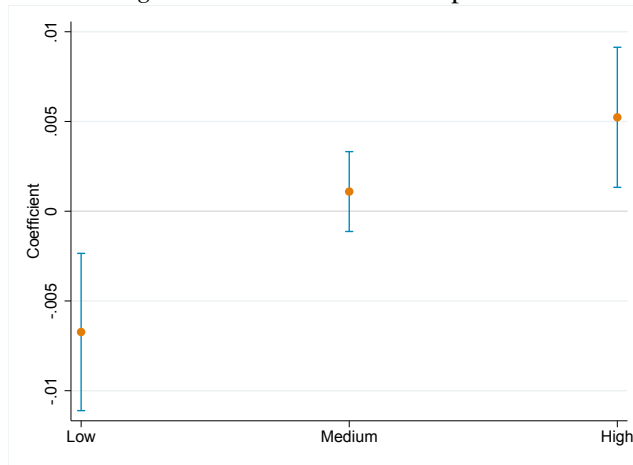
The average result on earnings could either be because incumbent workers are paid relatively more or because there is a change in the composition of workers at the firm. I therefore next look at how the skill composition changes at the firm. I run equation 1.2 on, respectively, the shares of low-skilled, medium-skilled and high-skilled. Figure 6 summarizes the results. We see the ratio of high-skilled to low-skilled going up (albeit insignificantly) with the share of low-skilled dropping by half a percentage point and the share of high-skilled increasing by around the same amount. This shift towards more high-skilled workers come from an increase in the intensity of hiring high-skilled workers, as Table 4 shows. Hiring rates go up for both types of workers, but firms are hiring slightly more high-skilled workers as a result of the reform. This is consistent with the share of high-skilled increasing at the firm. Separation rates also go down, more for the low-skilled, but results are insignificant for both groups.

Overall, even though not all coefficients are precisely estimated, these results all point in the same direction. Treated firms do better because of the reform: they hire more workers and fewer separate. Treated firms also take advantage of the skill complementarity between natives and immigrants and hire more highly-skilled as a result of facing fewer obstacles on hiring low-skilled from abroad. The group of small firms seems to benefit the most. These results need to be interpreted in light of the fact that I restrict firms to exist in 2008 and 2009. This means that I am not evaluating the effect of the policy on firm expansion.<sup>33</sup> Furthermore, these results are conditional on firm survival. However, they are not driven by firm survival. Estimating equation 1.2 on the probability of a firm to exist, the coefficient on the interaction term is small and insignificant (0.0004 with standard error 0.001).

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<sup>33</sup>Olney (2013) finds that low-skilled immigration significantly increases the number of establishments within US cities.

Figure 6. Effect on skill composition



Notes: Estimates from regressions with group-specific linear trend.

**Table 4.** Effects of December 2008 reform on skill-specific hiring and separation outcomes: Difference-in-differences with binary treatment

|                       | Hiring rate<br>low-skilled<br>(1) | Separation rate<br>low-skilled<br>(2) | Hiring rate<br>high-skilled<br>(3) | Separation rate<br>high-skilled<br>(4) |
|-----------------------|-----------------------------------|---------------------------------------|------------------------------------|--|
| Coefficient           | 0.003<br>(0.005)                  | -0.004<br>(0.005)                     | 0.004<br>(0.002)                   | -0.003<br>(0.002)                      |
| Observations          | 1,074,568                         |                                       |                                    |  |
| Clusters              | 449                               |                                       |                                    |  |
| Pre-reform<br>average | 0.126                             | 0.116                                 | 0.016                              | 0.013                                  |

Notes: Regressions include year, firm fixed effects, sector-year fixed effects and a group-specific trend. Pre-reform averages refer to the control group. Treatment is defined as being in the fourth quartile of the index. Standard errors in parantheses and clustered at the level.

## 1.5 Individual-level analysis

As the previous analysis shows, the composition of workers in firms changes, which is reflected in the effect of immigration on average earnings. Positive effects at the firm may mask potentially negative effects on individuals, if these are pushed into worse-paying firms or into unemployment. In this section I study earnings and employment dynamics at the individual level, following domestic individuals that were employed in 2008 over time.

### 1.5.1 Data and descriptive statistics

I start with the sample of *low-skilled* domestic employees assigned to a firm from the firm-level analysis in 2008. I create a panel for the years 2004-2011 and follow these individuals over time, regardless of whether they stay at their 2008 firms or whether they are employed at firms that satisfy the restrictions in the firm-level analysis section. I keep the part of the individual histories when they are of working age (18-65).

Table 5 shows individual characteristics for 2008, by treatment status. Individuals in treated industries earn less than those in control industries, are to a larger extent born in a non-European country, are younger and more than twice as likely to be female. They are more heavily concentrated in service sectors, relative to control individuals who work primarily in manufacturing.

**Table 5.** *Individual-level characteristics (low-skilled workers only)*

|  | Treated |         | Control   |         |
|--|---------|---------|-----------|---------|
|  | Mean    | SD      | Mean      | SD      |
| <i>Earnings</i>  | 241.822 | 151.041 | 301.081   | 135.553 |
| <i>Non-EU</i>  | 0.105   | 0.307   | 0.061     | 0.239   |
| <i>Women</i>   | 0.567   | 0.496   | 0.224     | 0.417   |
| <i>Age</i>   | 38.805  | 13.598  | 41.408    | 12.963  |
| <i>Average skill content</i>                             | 1.699   | 0.367   | 1.927     | 0.158   |
| <i>Primary sector</i>                                    | 0.001   |         | 0.027     |         |
| <i>Manufacturing</i>                                     | 0.011   |         | 0.436     |         |
| <i>Electricity, gas and water supply</i>                 | 0.000   |         | 0.022     |         |
| <i>Construction</i>                                      | 0.000   |         | 0.165     |         |
| <i>Wholesale and retail trade</i>                        | 0.431   |         | 0.098     |         |
| <i>Transportation and storage</i>                        | 0.027   |         | 0.102     |         |
| <i>Hotels and restaurants</i>                            | 0.149   |         | 0.005     |         |
| <i>Information and communication</i>                     | 0.004   |         | 0.005     |         |
| <i>Financial, insurance and real estate activities</i>   | 0.085   |         | 0.006     |         |
| <i>Professional, scientific and technical activities</i> | 0.032   |         | 0.021     |         |
| <i>Personal services</i>                                 | 0.260   |         | 0.113     |         |
| <i>Observations</i>                                      | 502,146 |         | 1,026,513 |         |

*Notes:* Individual-level characteristics refer to 2008. Treated individuals are those in industries that are in the fourth quartile of the index distribution. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each group.

## 1.5.2 Results

I look at four outcomes: annual earnings;<sup>34</sup> employment, defined as the probability of having non-zero income; firm stay, defined as the likelihood of being matched to the same firm as in 2008; and industry mobility, defined as the probability of being in an industry that is different from the industry in 2008.<sup>35</sup> The latter two serve to give an interpretation to the findings for earnings. If I find both earnings and firm stay to increase, that would suggest the adjustment happens at the firm. If instead earnings increase but firm stay goes down, then the adjustment happens by moving across firms.<sup>36</sup>

Given that the variation I exploit categorizes primarily low-skilled industries as treated, I focus on the sample of low-skilled natives. I also show results for different groups of low-skilled: young (aged below 40), older (aged above 40) and non-EU born who were already in Sweden. I show results for high-skilled for completeness.

Table 6 shows a summary of the results. In the first column, I show average results for the group of low-skilled. Overall, the results are small and insignificant, except for industry mobility, which goes down and is significant at the 10% level. Looking at columns (2), (3) and (4) reveals that these average results mask the fact that different groups are affected differently. The effects on the group of young low-skilled are consistently negative, regardless of the outcome. They earn less, are more likely to be unemployed, less likely to stay at their 2008 firms and less likely to switch industries. Only the result on employment is significant at the 5% level: young low-skilled domestic workers in treated industries are 0.8 percentage points less likely to be employed. Given that the group of newcomers largely falls in this same category of young and low-skilled, these results suggest a high degree of substitutability between the two. If this is the case, we would expect opposite effects for the group of older low-skilled. Column (3) shows that indeed, the earnings of the older group significantly increase by 2.7%. Older individuals are also more likely to be employed and to stay with their 2008 firms (these effects are insignificant, though).

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<sup>34</sup>Note that most previous studies of immigration on individual outcomes look at hourly wages and not annual earnings (Bratsberg and Raaum (2012), Foged and Peri (2016)). One exception is Cattaneo et al. (2015).

<sup>35</sup>The industry codes change in 2007 such that for most codes there is no one-to-one mapping. In order to have the full industry history, I create a mapping between the previous version of the codes (SNI 2002) and the new version (SNI 2007). I pool all workers over the 2007-2009 period, as during these years I have both codes. This allows me to calculate relative frequencies, that is, to see the transitions between SNI 2002 and SNI 2007 that are most frequently encountered. In case I am left with industries without a mapping, I take the first transition that shows up in the correspondence tables provided by Statistics Sweden.

<sup>36</sup>The underlying mechanism could be the same in both cases. Foged and Peri (2016) find that low-skilled natives in areas with a higher share of immigrants transition to more complex jobs but only do so by moving across firms; they don't find this sort of task adjustment within firms.

The most surprising result is for the group of non-EU-born that were already in Sweden. Their earnings see an increase of 13% (significant at the 10% level). Those born in non-European countries are the least likely to switch industries and the most likely to stay with their 2008 firms (the latter effect is insignificant, though). Previous literature generally finds that older immigrants tend to be hurt the most by new immigration (Card 2001, Ottaviano and Peri 2012). However, the older immigrants I look at here are potentially more successful than on average, given that they are already employed in 2008. They are also predominantly from countries of origin that have a longer history in Sweden (Former Yugoslavia, Bosnia, Iran). Moreover, whether these workers should be categorized as low-skilled is also not straightforward: immigrants often work in jobs for which they are overqualified if the education and experience they obtained abroad is not valued in the host country (Friedberg 2000, Joona et al. 2014). The current analysis also does not allow me to conclude anything about those that were unemployed in 2008 that would have worked in treated industries. These results are not inconsistent with that group being potentially more negatively affected. (as found, for example, in Dustmann et al. 2017).

If the mechanism underlying these results is skill complementarity, whereby the workers that are most complementary to the newcomers benefit the most, we should find larger effects on earnings for the high-skilled. The first row of column (5) in Table 6 shows that the average effect on the high-skilled is almost double the one on the low-skilled in column (1) but it is imprecisely estimated.

The individual-level analysis provides at least suggestive evidence for complementarities both within skill group (between low-skilled natives and low-skilled immigrants) and across groups (between low-skilled immigrants and high-skilled natives). However, not all low-skilled are equal. There is a smaller degree of substitutability between older low-skilled natives and immigrants than there is between younger low-skilled natives and immigrants, largely due to the age composition of the newcomers.

**Table 6.** *Effects of December 2008 reform on individual-level outcomes*

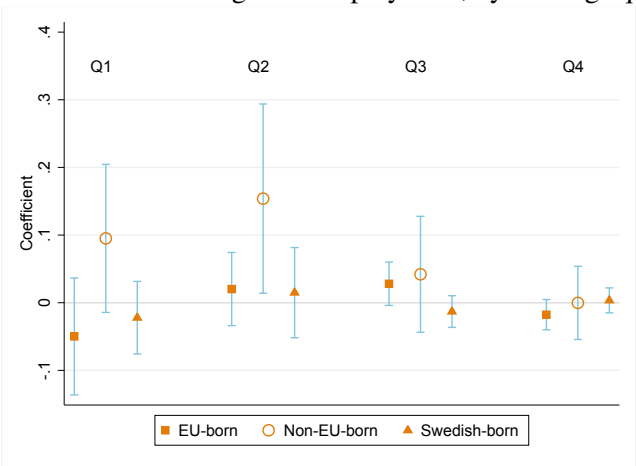
|                       | Low-skilled<br>(1) | Young<br>low-skilled<br>(2) | Older<br>low-skilled<br>(3) | Non-EU<br>low-skilled<br>(4) | High-skilled<br>(5) |
|-----------------------|--------------------|-----------------------------|-----------------------------|------------------------------|---------------------|
| Earnings              | 0.009<br>(0.024)   | -0.018<br>(0.023)           | 0.027<br>(0.011)            | 0.134<br>(0.069)             | 0.016<br>(0.013)    |
| Observations          | 11,382,943         | 5,420,355                   | 5,962,588                   | 817,813                      | 2,222,149           |
| Clusters              | 449                | 449                         | 349                         | 448                          | 449                 |
| Pre-reform<br>average | 5.474              | 5.247                       | 5.653                       | 5.237                        | 5.697               |
| Employment            | -0.002<br>(0.004)  | -0.008<br>(0.004)           | 0.002<br>(0.003)            | 0.019<br>(0.015)             | -0.003<br>(0.003)   |
| Observations          | 11,717,797         | 5,626,295                   | 6,091,502                   | 879,908                      | 2,277,510           |
| Clusters              | 449                | 449                         | 449                         | 448                          | 449                 |
| Pre-reform<br>average | 0.976              | 0.962                       | 0.988                       | 0.925                        | 0.973               |
| Firm stay             | 0.003<br>(0.021)   | -0.013<br>(0.021)           | 0.019<br>(0.014)            | 0.081<br>(0.056)             | -0.014<br>(0.019)   |
| Observations          | 11,382,943         | 5,420,355                   | 5,962,588                   | 817,813                      | 2,222,149           |
| Clusters              | 449                | 449                         | 449                         | 448                          | 449                 |
| Pre-reform<br>average | 0.773              | 0.688                       | 0.840                       | 0.719                        | 0.673               |
| Industry<br>mobility  | -0.054<br>(0.032)  | -0.035<br>(0.024)           | -0.068<br>(0.028)           | -0.154<br>(0.052)            | -0.028<br>(0.028)   |
| Observations          | 11,363,406         | 5,404,527                   | 5,958,879                   | 815,959                      | 2,219,237           |
| Clusters              | 449                | 449                         | 449                         | 448                          | 449                 |
| Pre-reform<br>average | 0.228              | 0.291                       | 0.179                       | 0.268                        | 0.321               |

*Notes:* Regressions include year, sector-year, individual-level fixed effects, time-varying co-variates and a group-specific linear trend. Pre-reform averages refer to the control group. Treated individuals are those in industries that are in the fourth quartile of the shortage index distribution. Standard errors in parantheses and clustered at the 5-digit industry level. \*, \*\*, \*\*\* indicate significance at the 10, 5, and 1 percent level, respectively.

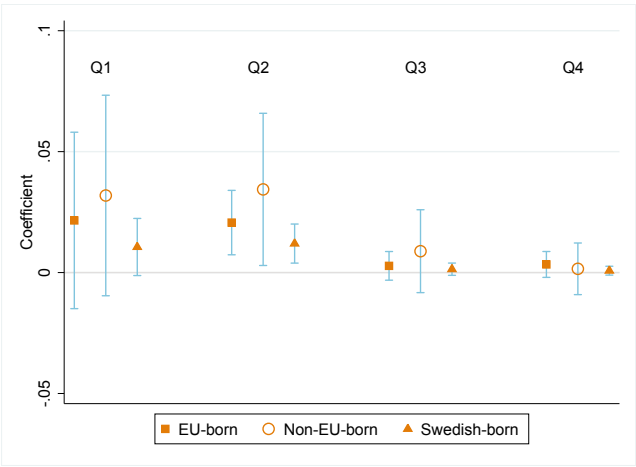
Given the fact that education levels are often poorly recorded for immigrants, and the fact that previous work has found effects of immigration along the wage distribution (e.g. Dustmann et al. 2012), I next do the following exercise. I break down the sample of individuals employed in the pre-reform period into earnings quartiles, depending on their position in the national earn-

ings distribution. I then estimate equation 1.3 on three groups of workers, for each earnings quartile: Swedish-born, EU-born and non-EU born. Figure 7a plots the results for earnings and and Figure 7b for employment. We see that the previous result that previous (non-EU) immigrants are the most positively affected remains (for both earnings and employment) and is concentrated at the bottom of the distribution. Those born in European countries and who were in the bottom quartile before the reform are the most hurt in terms of earnings but not in terms of employment.

Figure 7. Effect on earnings and employment, by earnings quartile



(a) Earnings



(b) Employment

Notes: Earnings are measured in logs. Regressions include year, firm, sector-year, individual-level fixed effects, time-varying covariates and a group-specific linear trend. Standard errors are clustered at the 5-digit industry level.



## 1.6 Conclusion

Many European countries are facing severe labor shortages across the skill distribution. Free mobility within the European Union has so far not been sufficient to eliminate these issues. Most states are in favor of opening up the borders to workers from non-European countries, and there is high competition for talent from abroad. In this paper, I show that low-skilled labor immigration also has mostly positive effects. I do so by exploiting variation induced by a major Swedish reform that lifted restrictions on hiring from abroad differentially across industries.

I first study firms and find that employees in firms in treated industries - those industries for which restrictions were lifted to a higher extent - have higher average earnings after the reform. This result seems to be driven by a change in composition at the firm, towards a more high-skilled workforce. Firms appear to take advantage of complementarities between natives and immigrants.

I also look at individuals and show some evidence that on average they earn more. Not all low-skilled workers are affected in the same way, however. The young and low-skilled, who are arguably competing the most with the newcomers, are more likely to become unemployed and may also earn less. An unexpected result of the individual-level analysis is that it is non-European immigrants that had already been in Sweden that benefit the most in terms of earnings, contrary to what previous literature has found. Further research is needed to probe that result and understand the reasons behind it.

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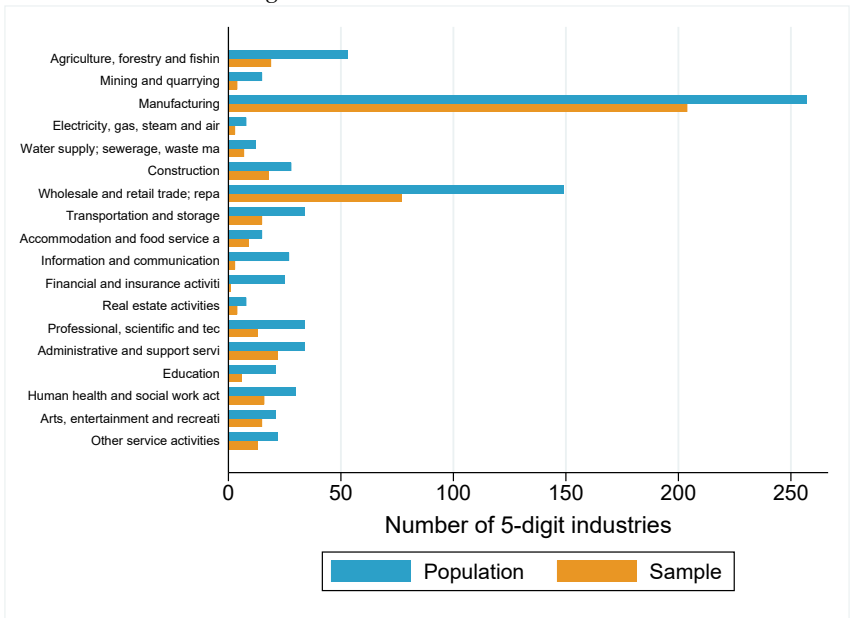
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Appendix

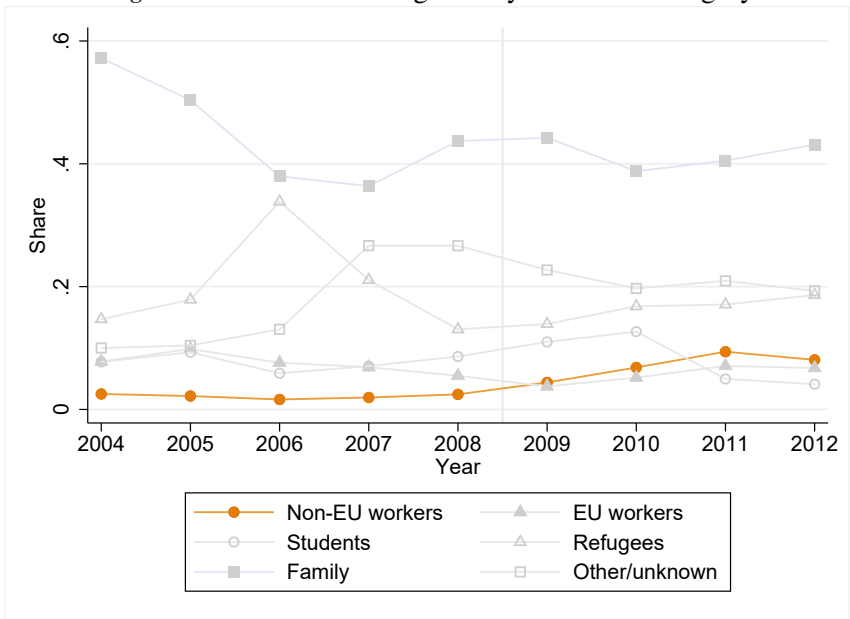
Figures

Figure A.1. Sectoral distribution



Notes: This figure shows the number of 5-digit industries in each sector, overall (population) and considering only the 5-digit industries I can calculate scores for (sample).

Figure A.2. Share of immigrants by admission category



*Figure A.3. 2008 shortage score against pre-reform average score*

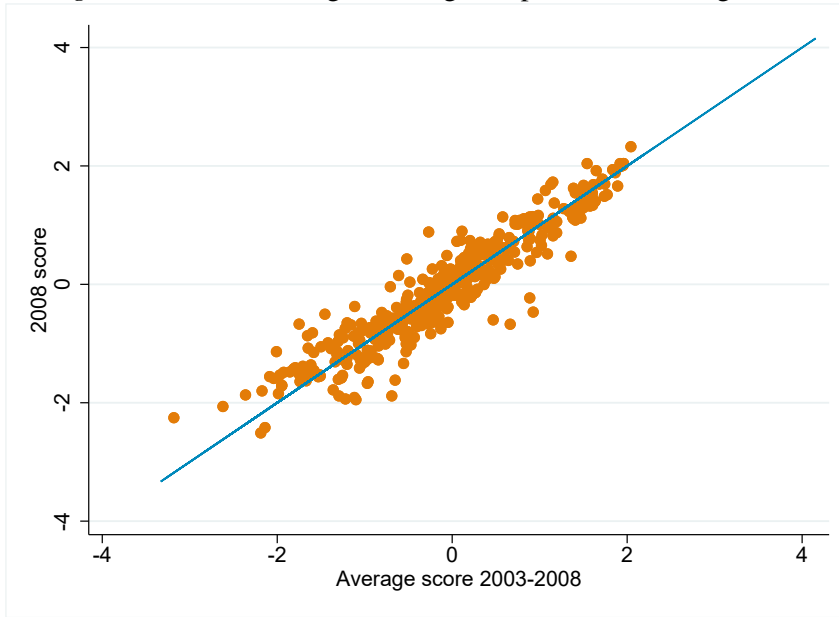
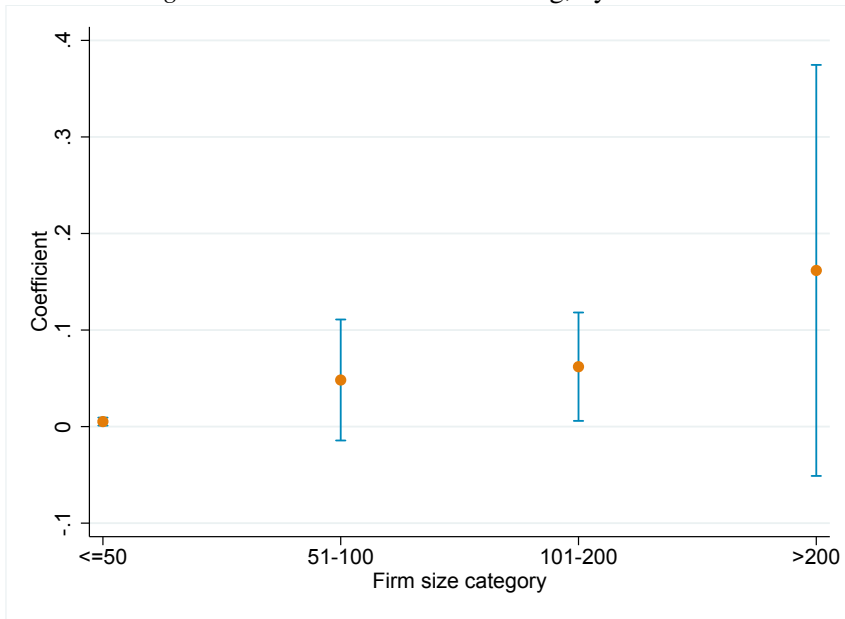
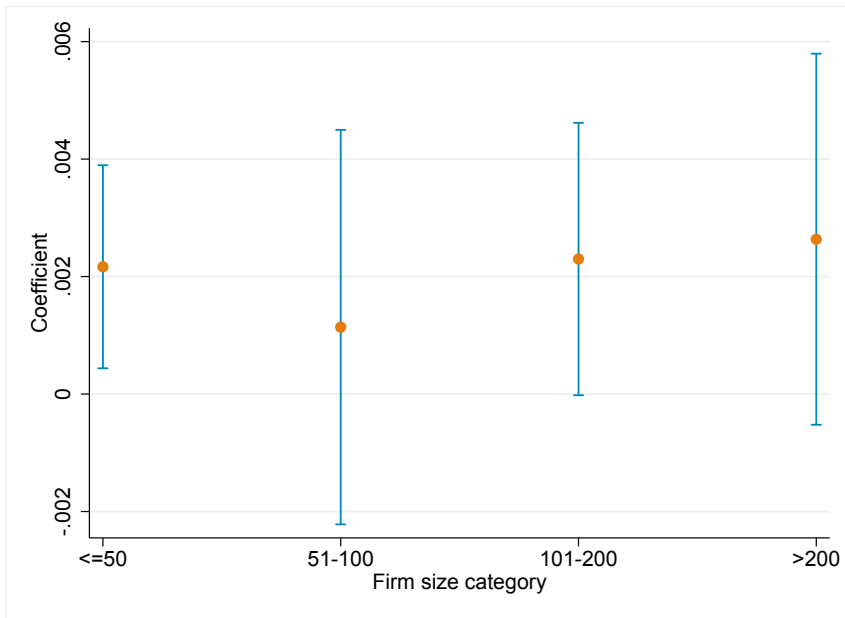


Figure A.4. Effect on non-EU hiring, by firm size



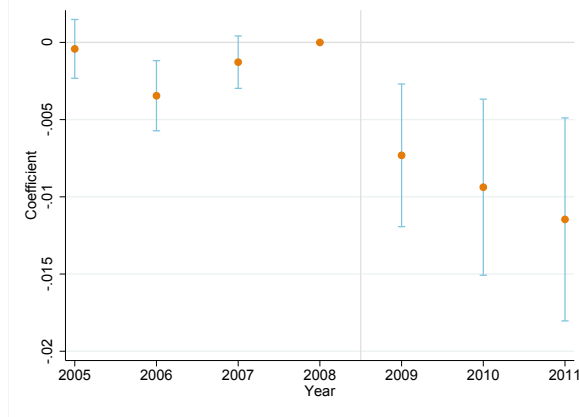
(a) Number of non-EU



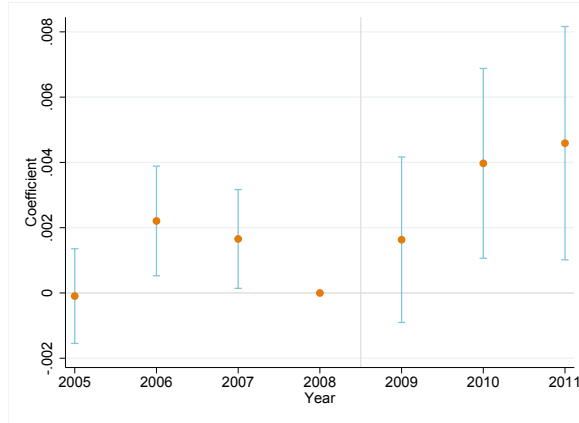
(b) Share of non-EU relative to all hires

Notes: Estimates from regressions with group-specific linear trend. Firm size category is measured in 2008.

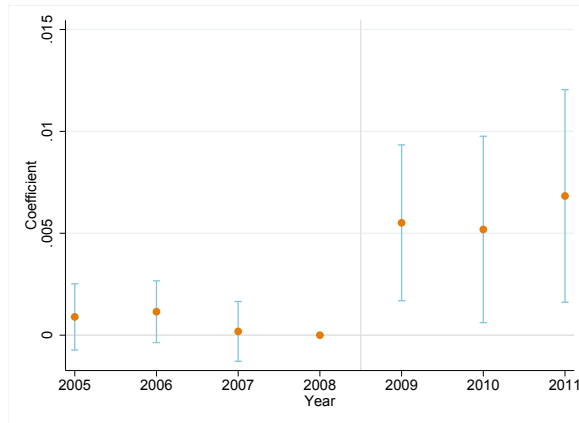
Figure A.5. Skill composition: yearly coefficients



(a) Low-skilled



(b) Medium-skilled



(c) High-skilled

Notes: Firms are restricted to exist both in 2008 and 2009 and to have at least 2 employees in 2008. Regressions include year, firm, sector-year fixed effects and a group-specific linear trend. Standard errors are clustered at the firm level.

Tables

**Table A.1.** *Correlation matrix between shortage scores*

|      | 2003  | 2004  | Yearly score |       | 2007  | 2008 |
|------|-------|-------|--------------|-------|-------|------|
|      |       |       | 2005         | 2006  |       |      |
| 2003 | 1     |       |              |       |       |      |
| 2004 | 0.909 | 1     |              |       |       |      |
| 2005 | 0.804 | 0.901 | 1            |       |       |      |
| 2006 | 0.754 | 0.839 | 0.920        | 1     |       |      |
| 2007 | 0.675 | 0.766 | 0.811        | 0.933 | 1     |      |
| 2008 | 0.706 | 0.808 | 0.842        | 0.896 | 0.917 | 1    |



**Table A.2.** *Individual-level characteristics (non-EU workers only)*

|  | Treated            |                     | Control            |                     |
|--|--------------------|---------------------|--------------------|---------------------|
|  | Pre-reform<br>Mean | Post-reform<br>Mean | Pre-reform<br>Mean | Post-reform<br>Mean |
| <i>Earnings</i>  | 200.136            | 181.134             | 428.245            | 354.910             |
| <i>Women</i>   | 0.262              | 0.218               | 0.210              | 0.165               |
| <i>Age</i>   | 34.536             | 32.534              | 33.289             | 33.566              |
| <i>Low-skilled</i>                                       | 0.168              | 0.255               | 0.072              | 0.129               |
| <i>Medium-skilled</i>                                    | 0.103              | 0.092               | 0.047              | 0.053               |
| <i>High-skilled</i>                                      | 0.200              | 0.153               | 0.388              | 0.364               |
| <i>Missing skill</i>                                     | 0.529              | 0.499               | 0.493              | 0.455               |
| <i>Average skill content</i>                             | 1.413              | 1.343               | 1.926              | 1.922               |
| <i>Primary sector</i>                                    | 0.000              | 0.003               | 0.017              | 0.030               |
| <i>Manufacturing</i>                                     | 0.029              | 0.008               | 0.401              | 0.392               |
| <i>Electricity, gas and water supply</i>                 | 0.000              | 0.000               | 0.003              | 0.005               |
| <i>Construction</i>                                      | 0.000              | 0.000               | 0.041              | 0.098               |
| <i>Wholesale and retail trade</i>                        | 0.107              | 0.101               | 0.071              | 0.083               |
| <i>Transportation and storage</i>                        | 0.001              | 0.001               | 0.027              | 0.038               |
| <i>Hotels and restaurants</i>                            | 0.502              | 0.530               | 0.001              | 0.005               |
| <i>Information and communication</i>                     | 0.003              | 0.002               | 0.001              | 0.000               |
| <i>Financial, insurance and real estate activities</i>   | 0.027              | 0.014               | 0.002              | 0.005               |
| <i>Professional, scientific and technical activities</i> | 0.034              | 0.023               | 0.267              | 0.193               |
| <i>Personal services</i>                                 | 0.297              | 0.319               | 0.170              | 0.151               |
| <i>Observations</i>                                      | 964                | 2,919               | 2,043              | 2,412               |

*Notes:* Individual-level characteristics refer to the first year when the non-EU worker is hired. Treated firms are firms in industries that are in the fourth quartile of the index distribution. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each group.

**Table A.3.** *Selection into treatment*

|                                       | Coefficient | SE    |
|---------------------------------------|-------------|-------|
| <i>Mean earnings (logs)</i>           | -0.091      | 0.026 |
| <i>Non-EU</i>                         | 0.115       | 0.036 |
| <i>Women</i>                          | 0.272       | 0.067 |
| <i>Age</i>                            | -0.001      | 0.001 |
| <i>Low-skilled</i>                    | 0.024       | 0.041 |
| <i>Medium-skilled</i>                 | 0.015       | 0.063 |
| <i>High-skilled</i>                   | -0.080      | 0.078 |
| <i>Number of establishments</i>       | 0.000       | 0.000 |
| <i>Average skill content</i>          | -0.349      | 0.124 |
| <i>Firm size <math>\leq 50</math></i> | -0.003      | 0.022 |
| <i>Firm size 51-100</i>               | 0.004       | 0.020 |
| <i>Firm size 101-200</i>              | -0.010      | 0.017 |
| <i>Observations</i>                   | 771,902     |       |
| <i>R-squared</i>                      | 0.561       |       |

*Notes:* This table shows coefficients from a regression of treatment status on firm-level characteristics, for the 2003-2008 pre-reform period. The regression includes sector-by-year fixed effects. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Average skill content refers to the skill content of the occupations assigned to the industries in each quartile. Earnings are measured in logs. The omitted categories are missing skill and firm size greater than 200 for the skill and firm size categories, respectively. Standard errors in parantheses and clustered at the 5-digit industry level.

**Table A.4.** *Firm-level characteristics (firms that hire at least one non-EU worker 2009-2011)*

|  | Treated |         | Control |         |
|--|---------|---------|---------|---------|
|  | Mean    | SD      | Mean    | SD      |
| <i>Firm size</i>   | 31.511  | 115.076 | 157.234 | 617.656 |
| <i>Mean earnings</i>                                     | 200.298 | 99.431  | 322.849 | 175.343 |
| <i>Non-EU</i>  | 0.555   | 0.362   | 0.218   | 0.287   |
| <i>Women</i>   | 0.395   | 0.274   | 0.253   | 0.236   |
| <i>Age</i>   | 35.897  | 6.562   | 39.234  | 6.297   |
| <i>Low-skilled</i>                                       | 0.722   | 0.246   | 0.652   | 0.289   |
| <i>Medium-skilled</i>                                    | 0.110   | 0.138   | 0.120   | 0.126   |
| <i>High-skilled</i>                                      | 0.110   | 0.182   | 0.194   | 0.240   |
| <i>Missing skill</i>                                     | 0.058   | 0.118   | 0.034   | 0.095   |
| <i>Average skill content</i>                             | 1.377   | 0.330   | 1.910   | 0.185   |
| <i>Firm age</i>  | 4.086   | 1.405   | 4.252   | 1.456   |
| <i>Primary sector</i>                                    | 0.001   |         | 0.046   |         |
| <i>Manufacturing</i>                                     | 0.005   |         | 0.334   |         |
| <i>Electricity, gas and water supply</i>                 | 0.000   |         | 0.006   |         |
| <i>Construction</i>                                      | 0.000   |         | 0.134   |         |
| <i>Wholesale and retail trade</i>                        | 0.121   |         | 0.128   |         |
| <i>Transportation and storage</i>                        | 0.002   |         | 0.067   |         |
| <i>Hotels and restaurants</i>                            | 0.595   |         | 0.011   |         |
| <i>Information and communication</i>                     | 0.001   |         | 0.000   |         |
| <i>Financial, insurance and real estate activities</i>   | 0.017   |         | 0.006   |         |
| <i>Professional, scientific and technical activities</i> | 0.025   |         | 0.090   |         |
| <i>Personal services</i>                                 | 0.233   |         | 0.178   |         |
| <i>Firm size <math>\leq 50</math></i>                    | 0.886   |         | 0.689   |         |
| <i>Firm size 51-100</i>                                  | 0.057   |         | 0.095   |         |
| <i>Firm size 101-200</i>                                 | 0.027   |         | 0.078   |         |
| <i>Firm size <math>&gt;200</math></i>                    | 0.030   |         | 0.138   |         |
| <i>Observations</i>                                      | 1,618   |         | 1,092   |         |

*Notes:* Firm-level characteristics refer to 2008. Treated firms are firms in industries that are in the fourth quartile of the index distribution. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each quartile. Firm age data starts in 2003, so the oldest firm can be at most 5.



## 2. Spillover Effects of Stricter Immigration Policies

with Matz Dahlberg, Mattias Engdahl and Till Nikolka

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## 2.1 Introduction

The extent to which public policies affect migration behavior and location choices of households has been an important research question in public economics for long. While the literature on welfare-related migration examines to what extent heterogeneous welfare policies across different jurisdictions affect welfare-prone individuals' migration behavior and location choices over these jurisdictions (see, e.g., Borjas 1999, Brueckner 2000, McKinnish 2007, and Edmark 2009), the literature on tax-related migration similarly studies how jurisdictions' tax-setting behavior affects the location choices of resource-strong individuals (see, e.g., Kirchgassner and Pommerehne 1996, Liebig et al. 2007, Kleven et al. 2013a, and Kleven et al. 2013b). Whether other public policies, such as immigration policies, also affect households' location choices, has been much less studied. With this paper, we start filling this lacuna by examining if stricter immigration policies in one country affect the migration behavior of affected individuals and lead to spillover effects to neighboring countries.

This is an important question, and, just as fiscally-induced migration behavior might lead to strategic interactions between local jurisdictions and a "race-to-the-bottom" in the setting of tax and welfare policies (see, e.g., Oates 1999, Brueckner 2000, Feld 2000, and Dahlberg and Edmark 2008), spillover effects of migration policy can lead to a "race-to-the-bottom" in the setting of migration policy, with sub-optimal levels in the generosity of policies, across countries.

The ongoing discussion on immigration policies within the European Union (EU) highlights the importance of this question from a policy-setting perspective. The large variation in migration policies across the member states of the EU has spurred an active debate on whether immigration policies should be set at the European or at the national level, a discussion that was propelled to the top of the European political agenda following the "refugee crisis" in 2015.<sup>1</sup> At the heart of the discussion lies the role of national migration policies as drivers of refugees' and other immigrants' location choices. Migration flows to different EU countries can be seen as communicating vessels, where changes in migration policies in one country affect migration flows to both that country and to other EU states. National migration policies may act as pull factors, affecting the initial choice of country for immigrants arriving in Europe. However, less generous migration policies in one EU country may act as push factors, creating spillover effects to neighboring countries.

In this paper we focus on push factors. We exploit a change in Denmark's family reunification policy to empirically test for spillover effects of migration policy. The reform made it much harder for Danish residents to reunite (or form couples) with partners from outside the EU. We examine if and to

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<sup>1</sup>Even though there are centrally set guidelines and minimum requirements for immigration policies within the EU, variation across countries remains.

what extent affected individuals moved abroad to reunite with their partners as a consequence of the tougher immigration policies in Denmark. Applying a difference-in-differences design on detailed Danish register data, we find that the emigration rate of Danish citizens with immigrant background increased strongly. We further find that the most popular destination was Sweden, a neighboring country with, at the time, more generous rules for family reunification.<sup>2</sup> We also examine whether those that left did so indefinitely or returned at a later stage. To accomplish that, we use Swedish register data and identify all couples that reunited in Sweden as a consequence of the reform in Denmark. We demonstrate that a significant fraction of the individuals that came to Sweden to reunite with a family member left the country again; within two (eight) years around 20% (50%) had emigrated, with the absolute majority moving to Denmark. Thus, the reform caused both emigration from Denmark and later return migration.

Applying an interrupted time-series (ITS) design on Swedish data also confirms the robustness of our results based on Danish data, i.e. that the stricter rules for reunification led to a clear and significant increase in family-related migration to Sweden. Using an ITS design with a control group approach, we are also able to rule out alternative explanations to the strong inflow of migrants following the reform. In particular, we show that the year 2000 opening of the Öresund bridge that connects Copenhagen (the capital of Denmark) with Malmö (the third largest city of Sweden) cannot explain the inflow to Sweden that we observe around the time of the reform. We also exclude the possibility that Sweden became a particularly attractive destination for migrant couples from neighboring countries for reasons unrelated to the Danish reform by examining inflows from Norway and Finland.

To the best of our knowledge, we are the first to study the research question posed in this paper causally. Most of the existing studies use cross-country data and examine how differences in migration-related policies and institutions in host as well as destination countries are related to asylum seekers' choice of country (see, e.g., Hatton 2009, 2016, Brekke et al. 2017, Ortega and Peri 2009, Neumayer 2004 and Böcker and Havinga 1998). These cross-country studies do however suffer from unsolved endogeneity problems (stemming from the endogenous location choice of refugees); see the discussion in, e.g., Brekke et al. (2017).<sup>3</sup>

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<sup>2</sup>These findings are in line with the descriptive evidence presented in a Danish report by Schmidt et al. (2009), who, among other things, study out-migration propensities of ethnic minorities in Denmark following the reform. One important difference between our study and Schmidt et al. (2009) is that the latter is purely descriptive while we use a control group approach. Also, while we use the full set of affected individuals, they limit their analysis to a couple of age cohorts.

<sup>3</sup>We are aware of only one study that causally examines the role of a country's refugee policies on the number of asylum seekers (that is, an examination of the pull effect); Andersson and Jutvik (2018).

The family reunification policy that we analyze in this paper is also interesting *per se*. Family reunification has been one of the most important channels of migration to the EU during the last decades, making it an important policy area that has been studied to a much lesser extent than the labor immigration channel (Hatton 2014). Despite the directive on the right to family reunification that establishes common rules for family reunification in all member states with the exception of the United Kingdom, Ireland and Denmark, there is considerable room for member states to impose stricter conditions and still comply with the directive.<sup>4</sup> The types of conditions imposed at the national level, e.g. requirements on adequate housing and sufficient resources, vary across member states, which results in some countries being more generous than others in terms of the possibility of reunification.

Apart from being related to the literature examining the effects of tax and welfare policies on households' migration behavior, as discussed above,<sup>5</sup> our results can also be interpreted through the lens of the determinants of international migration. In the neoclassical model of individual choice, potential migrants weigh the costs and benefits of different location alternatives and choose the location (country) that maximizes their utility (see, e.g., Borjas 1987). Residents of a country wanting to reunite with a spouse are thus likely to weigh in the regulations on family reunification when deciding on whether to stay on in a country or move elsewhere. Our results give strong support for this notion.<sup>6</sup> Conceptually, the two strands of the literature mentioned here are very similar, but they seldom speak to each other, and our results are of relevance for both strands.

The rest of the paper is organized as follows. In Section 2.2 we discuss the 2002 Danish reform that we exploit, as well as the rules regulating family reunification in Denmark and Sweden, and we explain why Sweden is an attractive alternative residence country for couples reuniting after the reform. In Section 2.3 we describe the Danish register data that we use, our methodological approach, and the estimated effects on emigration from Denmark. In Section 2.4 we turn to the analysis based on Swedish data. We first describe our data and empirical approach, and then present our findings. Finally, in Section 2.5, we conclude.

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<sup>4</sup>The directive determines the conditions for non-EU residents in a member state to be joined by their family members. For family members of EU citizens other rules apply.

<sup>5</sup>The same underlying reasoning as in that literature can of course be applied to location choices of international migrants, if, as in our study, family reunification is important for the utility of migrants; some individuals will move to countries where it is easy to reunite with a spouse.

<sup>6</sup>Similarly, the migration flow to a country that introduces stricter rules for family reunification is expected to diminish if the possibility to reunite is valued highly among (potential) immigrants.



## 2.2 Stricter rules for family reunification and their effects on affected individuals' migration behavior

In this section we describe the legal framework that regulates family reunification in Denmark, including the reform in 2002, explain how it affects family formation, and discuss why affected individuals might find the alternative of moving to Sweden attractive, as well as the legal framework that allows them to do so.

### 2.2.1 Family reunification policies in Denmark

Family reunification between Danish residents (including both citizens and non-citizens) and third-country nationals (non-EU citizens) is regulated at the national level, unless the Danish resident is a citizen who has exercised their freedom of movement right within the EU, in which case family reunification is regulated at the European level, under the Free Movement Directive (Van den Broucke et al. 2016).

During the decades before the policy that we study, two main changes took place in the rules guiding family reunification. The 1983 Danish Aliens Act introduced the automatic right to family reunification for close relatives of Danish citizens and residents. However, in 1992, the requirements were tightened such that reunification was generally not granted if the sponsor had not been a Danish resident for at least five years. Furthermore, the sponsor had to submit evidence that they could financially support the family member they wanted to reunify with (Hedetoft 2006). These changes effectively brought an end to the automatic right to family reunification.

The next major change in rules came in 2002, and this is the reform that we exploit in this paper. The reform was announced in January 2002 and passed in June the same year (Skyt Nielsen et al. 2009). The changes include the introduction of: (i) the “24-year rule”, according to which reunification on marriage grounds is impossible unless both parties are 24 years old or older, (ii) the attachment requirement, whereby the partners must show proof of a stronger affiliation to Denmark than to any other country, measured as their combined number of years of residence in different countries, (iii) adequate housing requirement, (iv) ability of the sponsor (i.e., the spouse living in Denmark) to provide financially for the family and evidence of the sponsor not having received social assistance in the year prior to the application, and (v) a bank collateral in case the family member benefits from social assistance after arrival (Rytter 2013).<sup>7</sup>

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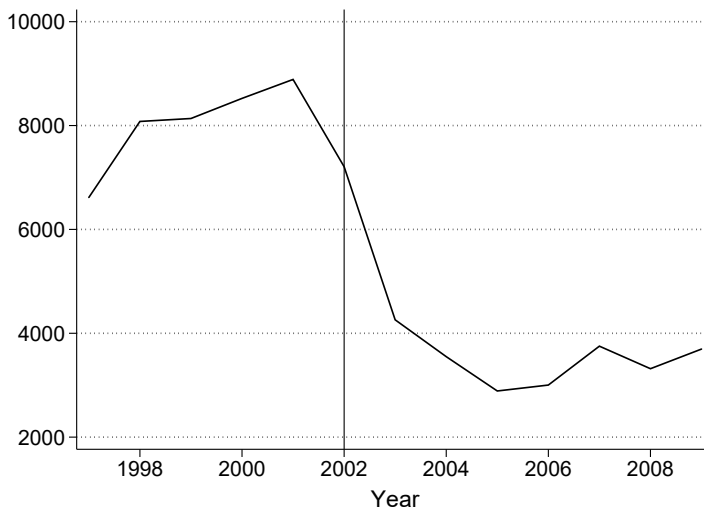
<sup>7</sup>In 2003, the attachment requirement was somewhat loosened. Thereafter, the requirement could be waived if the sponsor had been a Danish citizen for at least 28 years, or was born in Denmark and had resided in the country for at least 28 years. The required number of years of residence was reduced to 26 years in 2006. The 2002 attachment requirement replaced a similar

The same year, the public income transfers to immigrants were cut through the introduction of the so-called “Start Help” program. The program targeted all individuals (immigrants and Danish citizens returning from abroad) who had not been residents in Denmark for at least seven out of the most recent eight years. Income transfers were cut by around 35%. Hence, on top of the stricter requirements for reunification that were introduced in 2002, potential cash transfers to partners successfully fulfilling the new requirements were lower after the reform, which would make them more vulnerable economically.

Further changes with regards to family reunification have occurred in subsequent years. During our study period, the “biggest” change came in July 2005. From then on, applicants for family reunification have had to sign a “declaration of integration”, whereby they commit to making an effort to integrate. This reform should however be considered small in comparison with the changes in 2002.

Figure 1 shows the number of family ties permits granted in Denmark between 1997 and 2009 and is suggestive of the reforms in 2002 having reduced the flow of tied family members to Denmark, both immediately and in the long-run.<sup>8</sup>

*Figure 1. Number of permits granted on family grounds in Denmark*



*Source:* Statistics Denmark.

requirement introduced in 2000 but that only applied to foreign citizens; the 2002 requirement applies to Danish citizens as well.

<sup>8</sup>The figure captures the number of permits granted to both spouses and other family members; it should however be noted that our focus is on permits granted to spouses only.

### 2.2.2 Spillover effects: why would families choose to reunite abroad and what makes Sweden an attractive alternative?

The fact that family reunification in Denmark became more difficult in 2002 suggests that Danes and foreign residents residing in Denmark wanting to (re)unite with a partner had to move abroad or abstain from getting married if they were not fulfilling the criteria for reunification. In this section we argue that Sweden was (and still is) an attractive destination for those wanting to reunite, both in terms of the rules governing family reunification and geographic closeness.

First, Sweden is one of two countries that has a close border connection to Denmark (the other one being Germany); see the left-hand side map in Figure 2. Second, the most southern region of Sweden (Skåne) is easily accessible from the part of Denmark in which a large part of individuals affected by stricter reunification rules reside (c.f. the right-hand side map in Figure 2), which zooms in on the area in the left-hand side map in Figure 2 covering the most eastern island in Denmark, Zealand, and the most southern region in Sweden, Skåne). As can be seen from the map, the Copenhagen region in the southern part of Zealand is geographically very close to the Malmö region in the south of Sweden. Traveling across the Öresund bridge that connects Copenhagen with Malmö (the third largest city of Sweden) takes around 30 minutes. Also, North Zealand is closely connected to the Helsingborg region in Skåne: the ferry between Helsingör (on the Danish side of the border) and Helsingborg takes around 20 minutes.

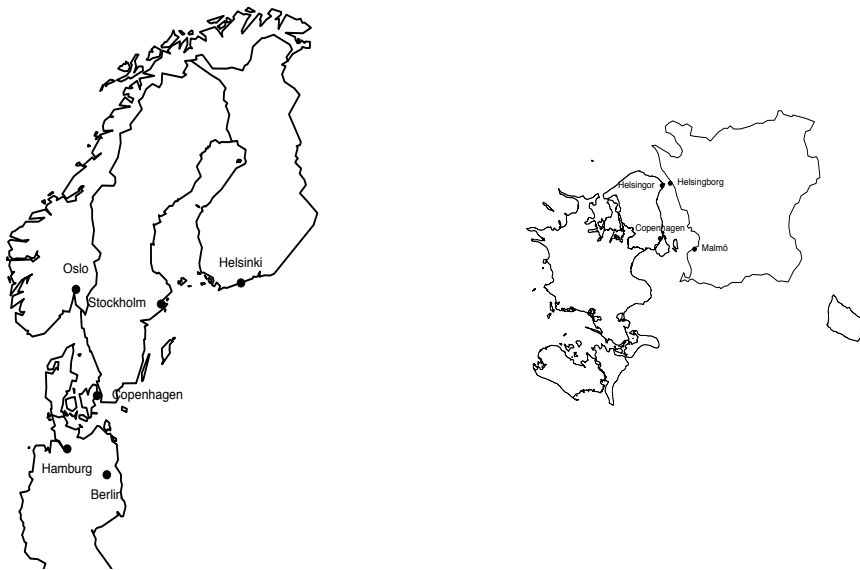
Third, the short distances within the Öresund region, and the cultural and linguistic similarities between Denmark and Sweden imply that the actual moving costs should be relatively low in comparison with a move to other countries.<sup>9</sup>

Fourth, at the time the rules under which affected individuals could apply for reunification in Sweden were more generous than the Danish rules. Two legal frameworks regulate the possibility of family reunification in Sweden. Since 1954, Nordic citizens (including Danish citizens) are allowed to reside and work in any Nordic country without a residence or work permit. By virtue of this agreement, a Danish citizen who moves to Sweden and wishes to bring his/her spouse to Sweden via family reunification can do so in accordance with Swedish rules. Non-EU spouses are generally allowed to apply for a residence permit from Sweden provided that the couple has already lived together out-

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<sup>9</sup>For example, the short distance suggests that the travel costs and the costs of transportation of goods (furniture etc.) would be limited. Another advantage is that migrants moving from the Copenhagen area can keep their jobs in Denmark (as long as they settle within a reasonable commuting distance in Sweden). The short distance also means that it is easy to stay in touch with (or meet) friends and relatives on the other side of the border. In addition, the extensive linguistic and cultural overlap between the two countries means that Danish can usually be understood in Southern Sweden and that integration into Swedish society would be relatively unproblematic.

Figure 2. Denmark and neighboring countries (left); Zealand, Denmark and Skåne, Sweden (right)



side of Sweden as a married couple or in a registered partnership. If that is not the case, the non-EU partner planning to marry or cohabit with the Danish citizen has to apply from their country of origin.<sup>10</sup> If the application is successful, the foreign spouse obtains a residence permit that is valid for two years, after which it is possible to apply for a permanent residence permit. At the time, there were no income and accommodation requirements and there was no minimum period of legal residence required in order to qualify for family reunification; however, both partners had to be at least 18 years old (Pascouau et al. 2011).

The other legal framework follows the Free Movement Directive at the European level discussed above. EU citizens who exercise their freedom of movement right within the EU may apply for family reunification under EU law, regardless of the nationality of their partners. Danish citizens moving to Sweden fall under this category. Family reunification is possible as long as the sponsor can provide proof of legal residence in Sweden (i.e. document their status as a worker, a self-employed person, a student, a pensioner or a person

<sup>10</sup>The application processing time may vary. There is no yearly data on average decision times but as of July 2016, it could take even up to 18 months for applicants from certain countries of origin. However, in the early 2000s, the processing times were probably much lower.

with sufficient resources).<sup>11</sup> If a residence card is granted following EU law, it is valid for five years, after which the non-EU partner can apply for a permanent resident permit.<sup>12</sup> For people who have been granted a residence permit in an EU country, family reunification is possible in accordance with the EU Family Reunification Directive (Council Directive 2003/86/EC on family reunification). The possibility for non-EU citizens in Denmark to move to other EU countries is however limited as Denmark does not grant long-term residence status to third country nationals due to Denmark's special arrangements for immigration and asylum policy. Long-term residence status is a requirement for free mobility within the EU, thus the possibility for this group to move to Sweden to reunite with a partner is limited.

Regardless of the set of rules under which the couple reunifies, the couple can go back to Denmark (the home country of the sponsor) and obtain family reunification rights under the Free Movement Directive. Alternatively, if the couple spends enough time in Sweden, the non-EU partner can obtain Swedish citizenship and therefore move to Denmark under the Nordic agreement.

Finally, it can also be noted that there is a Danish organization, "Aegteskab Uden Graenser" ("Marriage Without Borders") that provides legal counseling to people affected by the reform. They give information on both the Swedish and the EU rules (which can of course be applied for moves to Germany, for example), but stress that the Swedish rules may be more favorable because it takes less time until the partner is able to obtain a permanent residence permit (two vs. five years under EU rules).<sup>13</sup> Furthermore, the Swedish rules at the time imposed no maintenance requirements, whereas under EU rules the sponsor has to prove they are undergoing some kind of economic activity, as explained above.

Taken together, these reasons make Sweden an attractive and very plausible alternative for reunification purposes for those affected by the reform. The arguments for going to Germany for reunification purposes are weaker, and as we will show later, very few, if any, of the affected individuals seem to react to the reform by going to Germany.

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<sup>11</sup>In case the couple is not married or not in a registered partnership, family reunification *cannot be granted* under EU rules. Note the contrast with Swedish rules, where the intention to marry or cohabit with someone in Sweden is sufficient as long as one can prove the relationship is genuine.

<sup>12</sup>Note that referring to the residence permit as such under Swedish rules and as residence card under EU rules is not by chance; this is the distinction that the Swedish authorities make.

<sup>13</sup>See <http://aegteskabudengraenser.dk/raadgivning/sverigeu> for the information the organization promotes (in Danish). (The page was last visited on May 28, 2018.)

## 2.3 How did the Danish reform affect emigration from Denmark?

The aim of this section is to analyze whether the 2002 reform increased emigration rates of Danish residents who were affected by stricter rules for family reunification. We start off by describing the Danish register data that we use and how we define the group affected by the reform. Next, we proceed by providing some descriptives that are indicative of the reform causing an outflow individuals in the treatment group. We then present a formal statistical analysis for the effect of the reform on emigration to Sweden. We provide statistical inference on the change in emigration rates of affected compared to unaffected residents in a repeated cross section analysis. Additionally, we show how Kaplan-Meier survival estimates differ between treated and untreated individuals when following a fixed cohort over time after the reform.

### 2.3.1 Danish register data

To analyze how the tightening of family reunification rules has affected out-migration from Denmark we use Danish full population register data for the years 1995 to 2009. The data combines administrative information on socio-economic characteristics of individuals residing in Denmark, such as their age, municipality of residence and family status. For each resident we can link these characteristics with migration data including the date of emigration and the destination country. In Denmark, it is compulsory to report out-migration if someone leaves the country for more than six months. Furthermore, the full population data also allows us to add information on married or cohabiting individuals' partners.<sup>14</sup>

### 2.3.2 Which Danish residents were affected by the 2002 policy reform?

Family reunification in Denmark is regulated by the national policy if a Danish resident is either a Danish or non-EU/EEA citizen and the partner is non-EU/EEA citizen. In this case residents who want to reunite with their spouse from a non EU/EEA country in Denmark have to fulfill the stricter requirements imposed by the 2002 reform or move to another country where they face fewer or no restrictions. We restrict attention to individuals who were 18 years or older in a given year. In our subsequent analysis we focus on Danish citizens as they can easily move to a neighboring country and apply for family reunification under the EU rules.<sup>15</sup> We define a treatment group affected by

<sup>14</sup>See the Appendix for more details on the Danish data and sample selection.

<sup>15</sup>We also analyzed emigration behavior of non-EU/EEA citizens who were affected by the stricter migration policy but could not easily move to a neighboring country in order to reunify

the 2002 change in the Danish family reunification policy and a control group that was not directly affected by the policy change. We consider single Danish citizens not cohabiting with a partner in Denmark as our treatment group that faces stricter rules for potential family reunification after the reform. In our control group we consider Danish citizens who cohabit with a non-EU/EEA partner. This implies that the partner must already hold a residence permit in a given year and those in the control group should not be affected by the family reunification reform.

### 2.3.3 Empirical results on emigration from Denmark

We start by analyzing whether emigration rates in the treatment group affected by the reform (as defined in the former section) increase after stricter reunification requirements were implemented. We analyze repeated cross section data which are summarized in Table 1. The table presents average characteristics of individuals who are in the treatment and the control group in a given year, pooled over the sample period. Individuals in the treatment group are singles, while individuals in the control group are cohabiting or married with a non-EU-citizen. We present summary statistics for all individuals in the treatment group in Column 1 and all individuals in the control group in Column 3. Columns 2 and 4 restrict both groups to individuals with immigrant background.<sup>16</sup> Given our definition of treatment and control group demographic characteristics differ between the groups. The treated individuals are considerably younger and fewer have children. Table 1 shows that a large fraction of individuals live in Zealand, the most densely populated region in Denmark and home to the capital city, Copenhagen. There are no big differences between treatment and control group regarding the share of Zealand residents, only the share of Zealand residents without immigrant background in the treatment group seems to be slightly lower. The share of individuals born in Denmark and the share of females is higher in the treatment group. Among those with immigrant background Turkey and Pakistan are the two most important countries of origin in both treatment and control group.

Figure 3 plots the yearly emigration rates to all foreign destinations for the treatment group as well as for the control group from 1995 to 2009 (with the vertical line indicating the reform year). Panel A in Figure 3 includes *all* individuals in the treatment and control group in a given year. Even though all

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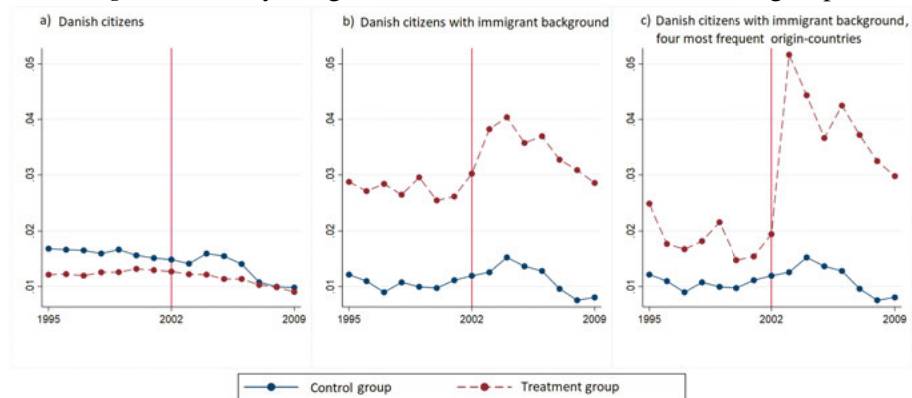
with their spouse under EU law (see the discussion in Section 2). Figure A.8 in the Appendix shows no migration response of non-EU/EEA citizens to the reform and confirms the restriction to the population of interest.

<sup>16</sup>Danish citizens with an immigrant background were either born abroad or born in Denmark to parents that were both born abroad and were not citizens.

**Table 1.** *Descriptive statistics*

|                   | Treatment group |                           | Control group |                           |
|-------------------|-----------------|---------------------------|---------------|---------------------------|
|                   | All             | With immigrant background | All           | With immigrant background |
| Age               | 32.89           | 28.78                     | 39.42         | 37.47                     |
| Female            | 41.46%          | 41.54%                    | 33.00%        | 32.67%                    |
| Any children      | 7.03%           | 7.32%                     | 59.84%        | 74.80%                    |
| Zealand residents | 47.25%          | 65.85%                    | 62.60%        | 68.71%                    |
| Born in Denmark   | 98.42%          | 37.81%                    | 63.98%        | 12.60%                    |
| Country of origin |                 |                           |               |                           |
| Turkey            | 0.30%           | 11.79%                    | 11.63%        | 28.22%                    |
| Pakistan          | 0.20%           | 7.85%                     | 5.12%         | 12.42%                    |
| Former Yugoslavia | 0.12%           | 4.63%                     | 2.25%         | 5.46%                     |
| Bosnia            | 0.09%           | 3.33%                     | 1.03%         | 2.50%                     |
| Observations      | 12,278,256      | 317,534                   | 464,310       | 191,377                   |

*Notes:* The treatment group are Danish citizens, older than 18, not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens, older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Citizens with immigrant background are born abroad or born in Denmark and neither parent holds Danish citizenship. The reported countries include Denmark and the four most important countries of origin of the population with non-EU/EEA background in Denmark during the studied time period. Reported numbers refer to averages over the sample period 1995-2009.

**Figure 3.** Yearly emigration rates of treatment and control group

*Notes:* The treatment group are Danish citizens older than 18 years, not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18 years, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Four most frequent origin-countries are Turkey, Pakistan, Former Yugoslavia and Bosnia.



of the individuals in the treatment group are theoretically affected by the reform, the majority of them are probably not *de facto* affected (namely those that would not have brought a partner from a non-EU country even in the absence of the reform). Assuming that those with an immigrant background on the parental side were more affected than those with Danish-born parents, we restrict the sample to Danish citizens with an immigrant background in Panel B. Narrowing the sample down even further, in Panel C we look at Danish citizens with immigrant background from one of the four most important countries of origin in Denmark: Turkey, Pakistan, Bosnia, and Former Yugoslavia (these countries account for more than 50% of the population with non-EU/EEA immigrant background in Denmark during the considered time period).

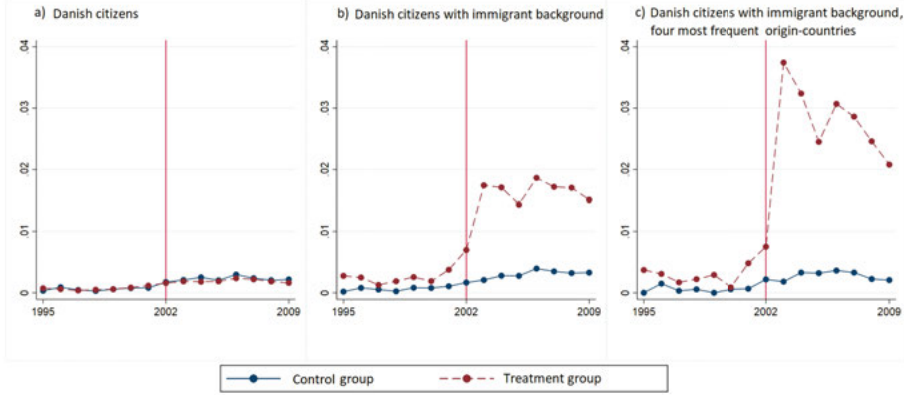
From Figure 3, Panels B and C strongly suggest that the reform had an effect on single Danish citizens with an immigrant background. For example, among the top four countries of origin, 1.6 percent of the affected group emigrated from Denmark the year before the reform (2001), while the corresponding figure was 5.4 percent the year after the reform (2003) (see Panel C). For the control group, there is far less movement in emigration rates around the reform year. While we see a similar pattern in Panel B, there is no visible movement in Panel A for the full (theoretical) treatment group, indicating that the reform had no large impact on emigration rates among single Danish citizens without an immigrant background.

Figure 3 showed emigration to all destinations. However, as argued earlier, Sweden is an especially attractive country to emigrate to for family reunification. We therefore next examine what the emigration rates to Sweden look like. Figure 4 shows corresponding panels as in Figure 3, but for emigration rates to Sweden only. Again, panels B and C strongly suggest that the reform led to an outflow of Danish citizens with immigrant background to Sweden. In addition, comparing the emigration rates in Figures 3 and 4 it is clear that a very large fraction of the emigrants moved to Sweden after the reform (over 72% when looking at the emigration rate in 2003 for the treated group in Panel C in the two figures; 3.9/5.4).<sup>17</sup>

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<sup>17</sup>The pattern is consistent with the descriptions found in Schmidt et al. (2009). We also provide emigration rates to Sweden on a half-year basis in Figure A.1, showing that the increase in emigration rates to Sweden in 2002 can be attributed to the second half of the year in which the reform was implemented. In Figure A.2 we show that emigration rates among individuals in the treatment group also increased for out-migration to Germany; compared to migration to Sweden, the observed patterns are much weaker and the scale much smaller (Figure A.3).

Figure 4. Yearly emigration rates of treatment and control group to Sweden



Notes: The treatment group are Danish citizens older than 18, not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. The top four countries of origin are Turkey, Pakistan, Former Yugoslavia and Bosnia.

The geographical distribution of the increase in emigration rates on the municipality level depicted in Figure 5 indicates that those migrants moving to Sweden originate mostly from the neighboring municipalities in Zealand.

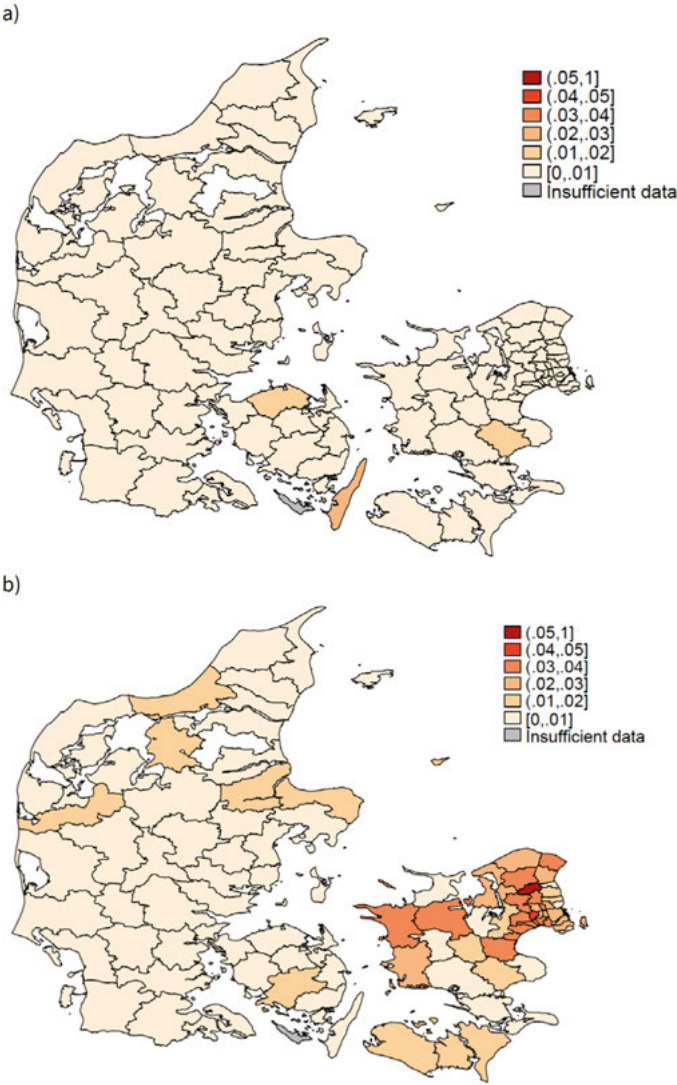
The dynamics in emigration rates for treatment and control group around the date of the Danish family reunification policy reform show that individuals with immigrant background moved out of Denmark after the reform. The presented descriptive evidence indicates that Sweden is *de facto* an attractive destination country for those residents affected by stricter rules for a potential family reunification.

To assess the statistical significance of stricter family reunification rules in Denmark on migration to Sweden we estimate a difference-in-differences type of model. We focus on Danish citizens with immigrant background based on our insights from Figure 4. We estimate the following model:

$$EM_{it} = \beta_0 + \beta_1 D_{it} + \beta_2 T_t + \gamma(D_{it} * T_t) + u_{it} \quad (2.1)$$

The dependent variable  $EM_{it}$  in equation 2.1 is set to one if an individual  $i$  emigrates to Sweden in a given year  $t$  and set to zero if a person stays in Denmark.  $D_{it}$  is an indicator set to one if individual  $i$  belongs to the treatment group in year  $t$  and equal to zero for individuals in the control group in  $t$ .  $T_t$  is a vector of time fixed effects. We are interested in the set of coefficients in the vector  $\gamma$ , i.e. the coefficients on the interaction terms between treatment status and the period dummies. The model is estimated for Danish citizens with immigrant background in the treatment and control group. We estimate

Figure 5. Average migration rates to Sweden, Danish citizens with immigrant background in the treatment group by municipality



Notes: Panel a) refers to 1995-2001 and panel b) to 2003-2009.

the model on repeated cross-section data with OLS and cluster standard errors at the individual level to account for serial autocorrelation in the error term.<sup>18</sup>

Figure 6 presents coefficient plots for  $\gamma$  (reference category:  $t = 2001$ ,  $D = 0$ ) in equation 2.1. The estimates in Figure 6 confirm our findings from Figure 4. We plot the coefficients of the interaction term for all citizens with immigrant background in the treatment and control group (Panel A), for Zealand residents only (Panel B) and for residents from the remaining Danish regions (Panel C). In the years before 2002 the coefficient estimates do not provide any evidence for a statistically significant difference in the likelihood to migrate from Denmark to Sweden between individuals in the treatment and control group. After 2002 the estimation shows a statistically significant increase of the likelihood to emigrate for treated individuals. According to the estimates the probability to emigrate to Sweden increases by up to 1.3 percentage points after the reform. This increase is large compared to average pre-reform migration rates to Sweden in the analyzed population: the average emigration rate between 1995 and 2001 to Sweden is 0.2%.<sup>19</sup>

Figure 6 also plots coefficient estimates separately for the sample of residents living in Zealand, the most densely populated island in Denmark and located closest to the Skåne region in Sweden (Panel B and C). Estimates confirm the patterns depicted in Figure 5. The increase in the likelihood to emigrate to Sweden is stronger for the sample of Zealand residents. For treated individuals in the remaining regions estimated post-reform coefficients for the interaction term are much smaller.

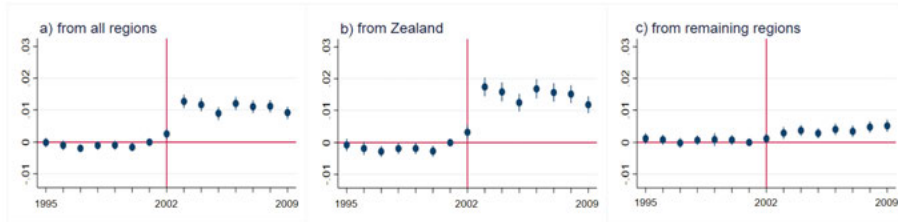
Furthermore, Figures A.6 and A.7 in the Appendix show that the reform response is almost entirely driven by those individuals who are below the age of 29.

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<sup>18</sup>In a robustness analysis, we re-estimate the model including dummies for age, gender and children in the household as additional control variables. The results are presented in Table A.2 and Figure A.5 and are similar to those without additional control variables.

<sup>19</sup>In addition to the estimation results for emigration to Sweden, we also show results for emigration to all destination countries in the Appendix. In Figure A.4 we present coefficient plots for out-migration rates of treatment and control group to all countries (corresponding to our descriptive analysis in Figure 3. We also estimated the regressions corresponding to panels A), B) and C) in Figure 6 for emigration from Denmark to Germany (Figure A.9); the effect of the reform on emigration to Germany seems much weaker than for emigration to Sweden; when running the regressions for emigration of those with immigrant background to Germany separately for Zealand and the remaining regions, there is no statistical significant difference between treatment and control group in none of the two specifications. This supports our arguments that Sweden is a more attractive as residence country than Germany for Danes affected by the reform.

*Figure 6.* Coefficient plots for interaction effect on migration of Danish citizens with immigrant background to Sweden



*Notes:* The sample includes Danish citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year* and *treatment*. The figure shows coefficient estimates for the interaction effect *treatment* x *year*. Confidence intervals show statistical significance at the 5% level, with standard errors clustered at the individual level.

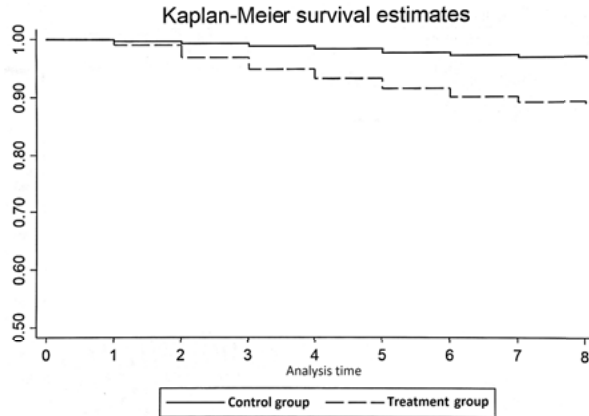
A potential concern when interpreting our estimates from Figure 6 as a causal effect of the policy change on treated individuals might be that individuals self-select into and out of the treatment and control groups based on conditions on the marriage market. For example, those individuals affected by stricter reunification rules might be more likely to marry a Danish partner instead of a foreign citizen after 2002. To address this potential endogeneity concern, we perform a duration analysis for which we present results in Figure 7. Here we consider individuals in the treatment or the control group who have immigrant background and are between 18 and 29 years old in 2001.<sup>20</sup> We assign these individuals to treatment and control in 2001 and follow them over the subsequent years and until 2009 (without any change in treatment status). A description and average characteristics of the data used for this analysis can be found in Appendix Table A.3. Figure 7 plots Kaplan-Meier survival estimates for out-migration of treated and untreated individuals to Sweden. We observe that individuals in the treatment group have a much higher likelihood to leave Denmark in the following years than those assigned to the control group.<sup>21</sup> Among those with immigrant background, only 3.6% of the untreated cohort but 12.1% of the treated cohort had left Denmark for Sweden in 2009. This result confirms our findings from Figure 6 and alleviates potential endogeneity concerns. Results from both analyses provide evidence for a causal effect of the Danish policy reform: individuals with immigrant background in the treatment group respond to stricter family reunification rules by

<sup>20</sup>We observed the strongest change in emigration rates after the reform for young individuals with immigrant background according to Appendix Tables A.6 and A.7. When we conduct the duration analysis without the upper age restriction the results are qualitatively similar.

<sup>21</sup>We also perform this analysis for emigration from Denmark to all destination countries in Appendix Figure A.10. These results also show a clear difference between survival function estimates of treated and untreated individuals.

emigrating, in particular to Sweden as an attractive alternative country of residence. Estimates show that this response is both statistically and economically significant.

*Figure 7.* Kaplan-Meier estimates for emigration from Denmark to Sweden



*Notes:* The treatment group are Danish citizens between 18 and 29 years old in 2001, not cohabiting with a partner in that year. The control group are Danish citizens between 18 and 29 years old in 2001, cohabiting with a non-EU/EEA citizen in 2001 in Denmark. Only individuals with immigrant background included. Individuals are excluded from the analysis if they drop out of the sample during the analysis period until 2009 and are not registered as emigrants.

## 2.4 How did the Danish reform affect immigration to Sweden?

After having shown that the Danish reform led to an increase in emigration, and that most of the emigrants moved to Sweden, we turn to Sweden. First, we describe the data used in the analysis and how we identify the couples that came to reunite in Sweden as a consequence of the reform. Second, we confirm the findings based on Danish data, i.e. that the reform caused an inflow of migrants to Sweden. We also check the robustness of our results. Third, we demonstrate that many of the affected individuals left Sweden after a relatively short period of stay, with the majority leaving for Denmark.

### 2.4.1 Swedish register data

To analyze the effect of the Danish reform on reunification-related migration to Sweden we use the database GeoSweden, which contains register data from Statistics Sweden covering the full population between 1990-2014. The data combine information from several different administrative registers and include information on country of birth, date of immigration/emigration, from

(to) which country the individual immigrated (emigrated), reason for residence (including family ties permits), municipality of residence in Sweden, labor income from Sweden (and Denmark), and a number of individual characteristics, such as age, gender, marital status, children and education, among other things.<sup>22</sup>

Our population of interest includes all individuals that immigrated between 1995 and 2009 (that are either Danish-born, have moved to Sweden from Denmark, or have registered as living together with a former Danish resident in Sweden), which means that we can follow all individuals for at least five years after immigration as long as they do not out-migrate or die. We restrict the analysis to individuals who are 18 years or older at the time of immigration.

#### 2.4.2 How do we identify couples reuniting in Sweden?

In Section 2.2 we discussed the fact that there are two legal frameworks that regulate the possibility of family reunification in Sweden: Danish citizens can reunite with a partner in Sweden following Swedish rules or rules regulated by the EU Free Movement Directive. Both of these routes to reunification presuppose Danish citizenship. In our data, however, we only have information on country of birth, thus we cannot confirm the citizenship status of those that reunite in Sweden. This is likely to be a small concern, since we saw in Section 2.3 that it was Danish citizens that responded to the reform by emigrating. For foreign citizens in Denmark we found no effects.<sup>23</sup>

Since we lack information on actual citizenship, we rely on information on country of birth to identify couples that reunite in Sweden, and we focus on family reunification between couples of two types: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark or elsewhere) and the other is born in a non-EU country and immigrated from there, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark.<sup>24</sup> Thus, we combine information on current household status (i.e. whether the migrant is married and lives together with the spouse or cohabits with a partner with common children), country of arrival, and the country of birth to identify the affected couples.

By contrast, we consider couples where *both* partners immigrate from Denmark to Sweden as couples who migrate to Sweden for reasons *unrelated* to the possibility of family reunification. That is, since both partners moved to

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<sup>22</sup>GeoSweden is compiled at Statistics Sweden and administered by the Institute for Housing and Urban Research at Uppsala University.

<sup>23</sup>However, if some individuals belonging to the latter group are able to move to Sweden they can reunite with a partner following the EU directive on family reunification. For more details see Section 2.2.

<sup>24</sup>Since EU citizens residing in Denmark are not affected by the reform (see Section 2.2) we assign each individual EU/non-EU status based on their country of birth, combined with information on year of accession to the EU, when applicable.

Sweden from Denmark it is unlikely that they encountered problems with respect to permission to stay on marriage grounds in Denmark.<sup>25</sup> These couples are used as one of the control groups in the analysis that follows below. Table 2 visually summarizes our definition. The Appendix provides more details on how we identify the affected couples in our data.

**Table 2.** *Definition of affected couples*

| <i>Restrictions</i>             | Affected       |                   | Unaffected     |                   |
|---------------------------------|----------------|-------------------|----------------|-------------------|
|                                 | Non-EU couples | DK-non-EU couples | Non-EU couples | DK-non-EU couples |
| <i>Both arrive from DK</i>      |                |                   | ✓              | ✓                 |
| <i>Only one arrives from DK</i> | ✓              | ✓                 |                |                   |
| <i>Neither arrives from DK</i>  |                | ✓                 |                |                   |

### 2.4.3 Description of the couples reuniting in Sweden

Table 3 shows the characteristics of the affected and unaffected groups, respectively. Several things can be noted with regards to the affected group. First, the number of individuals reuniting in Sweden in the pre-reform period was very small. After the reform the number rises sharply for the affected group. We also observe some increase in the number of arrivals of the unaffected group, which we are going to discuss in more detail in the following subsection. Second, the mean age of the partners reuniting in Sweden after the reform is lower than before the reform. This is in line with the new age requirement that was part of the reform, according to which both partners must be at least 24 years old to be able to reunite in Denmark (see Section 2.2). Third, the partners arriving after the reform are less likely to have children. Fourth, we observe that a higher share of the partners arriving from Denmark were born there while very few of the partners arriving from elsewhere were born in Denmark after the reform. Fifth, after 2002, a significantly higher proportion of the partners arriving from Denmark after the reform have labor income from Denmark after they move to Sweden (cross-border commuting in the border region is possible).

In Section 2.2 we discussed the hypothesis that it is likely that geographic closeness matters for migration decisions, a hypothesis that is supported by the fact that migrants from Zealand are overrepresented among those that left Denmark due to the reform. Similarly, if distance matters we would expect to see a higher share of the couples that reunited in Sweden as a consequence

<sup>25</sup>We consider non-EU couples where neither partner arrives from Denmark as neither affected, nor unaffected, as we cannot distinguish whether they actively choose Sweden as their destination country or whether they are pulled to Sweden because Denmark is not an option anymore.

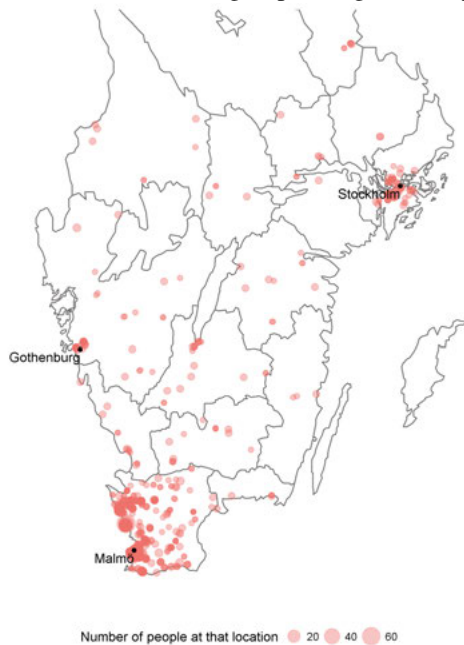


**Table 3.** *Descriptive statistics*

|                                     | Arrival<br>1995-2001 |                             |                                 | Arrival<br>2002-2009 |                             |                                 |
|-------------------------------------|----------------------|-----------------------------|---------------------------------|----------------------|-----------------------------|---------------------------------|
|                                     | All                  | Affected<br>From<br>Denmark | Unaffected<br>From<br>elsewhere | All                  | Affected<br>From<br>Denmark | Unaffected<br>From<br>elsewhere |
| Age                                 | 36.68                | 35.60                       | 37.09                           | 27.76                | 28.36                       | 27.20                           |
| Female                              | 48.78                | 26.67                       | 57.14                           | 50.04                | 45.79                       | 53.97                           |
| Any children                        | 52.44                | 40.00                       | 57.14                           | 28.45                | 27.29                       | 29.53                           |
| <i>Education</i>                    |                      |                             |                                 |                      |                             |                                 |
| Compulsory education or less        | 6.10                 | 11.11                       | 4.20                            | 7.75                 | 7.27                        | 8.19                            |
| Secondary education 3 years or less | 11.59                | 11.11                       | 11.76                           | 11.32                | 12.21                       | 10.50                           |
| Post-secondary education            | 18.90                | 20.00                       | 18.49                           | 20.80                | 21.77                       | 19.90                           |
| Post-graduate education             | 4.88                 | 4.44                        | 5.04                            | 0.53                 | 0.45                        | 0.60                            |
| Missing education                   | 58.54                | 53.33                       | 60.50                           | 59.60                | 58.29                       | 60.80                           |
| Income from Sweden                  | 31.71                | 28.89                       | 32.77                           | 8.63                 | 9.31                        | 8.01                            |
| Income from Denmark                 | 7.32                 | 17.78                       | 3.36                            | 37.77                | 75.74                       | 2.68                            |
| <i>Country of birth</i>             |                      |                             |                                 |                      |                             |                                 |
| Denmark                             | 37.80                | 46.70                       | 34.50                           | 27.90                | 53.90                       | 3.90                            |
| Turkey                              | 3.70                 | 6.70                        | 2.50                            | 19.60                | 12.90                       | 25.80                           |
| Pakistan                            | 1.80                 | 2.20                        | 1.70                            | 9.10                 | 4.10                        | 13.80                           |
| Former Yugoslavia                   | 4.90                 | 8.90                        | 3.40                            | 0.90                 | 1.50                        | 0.40                            |
| Bosnia                              | 2.40                 | 8.90                        | 3.40                            | 1.90                 | 3.90                        | 3.60                            |
| Observations                        | 164                  | 45                          | 119                             | 5096                 | 2448                        | 2648                            |

*Notes:* Statistics refer to the year of reunification. Income from Sweden is defined as having positive income in Sweden. Information on income from Denmark is only available from 2001 and onward. The countries of origin (other than Denmark) that we show statistics for are the top four countries of origin for immigrants in Denmark.

Figure 8. Geographic location of the affected group during the first year in Sweden



(a) Location across the southern half of Sweden



(b) Location across the Skåne county

Notes: The size of each dot represents the number of individuals settling in a 100x100 area.

of the reform to settle in Skåne (the most southern region in Sweden) in comparison with more remote regions. The upper panel in Figure 8 represents the southern half of Sweden and it shows the geographic locations of reunified partners. The majority of partners in reunified couples settle in Skåne after the reform (over 90% of the partners). The map in the lower-panel of Figure 8, which zooms in on the county of Skåne, shows that within Skåne, the most popular destinations are Malmö, Landskrona and Helsingborg on the western coast of Skåne. The pattern observed in Figure 8 hence indicates that Sweden is a viable alternative for those that want to relocate following the 2002 reform in Denmark and that the affected individuals actually reacted on the stricter rules imposed in 2002.

#### 2.4.4 Effects of stricter reunification policy in Denmark on reunification-related migration to Sweden

To investigate the effects of the tougher Danish reunification rules on reunification-related migration to Sweden, we conduct an Interrupted Time Series Analysis (ITSA).<sup>26</sup> To that end, we estimate the following model:

$$IM_t = \beta_0 + \beta_1 T_t + \beta_2 D_t + \beta_3 D_t \times T_t + \varepsilon_t \quad (2.2)$$

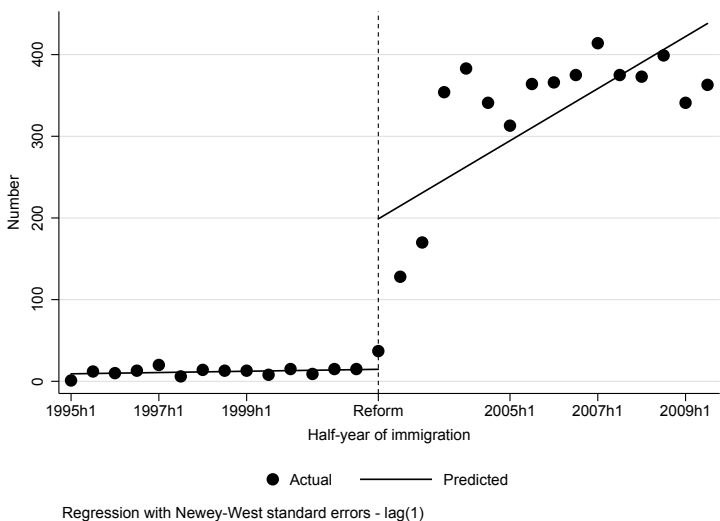
where  $IM_t$  is the number of immigrants (defined as the group of individuals affected by the 2002 reform in Denmark) to Sweden in time period  $t$ ,  $T_t$  is the time in period  $t$  since the first time point in the data,  $D_t$  is a dummy taking the value 1 for post-reform years and the value 0 for pre-reform years, and  $D_t \times T_t$  is the interaction term. To account for autocorrelation and heteroscedasticity, we estimate Newey-West standard errors with one lag. We run the analysis at a half-year frequency, with the reform taking place in the first half of 2002 (since the policy was announced in January of 2002).<sup>27</sup>

The coefficients obtained when estimating equation (2.2) are plotted in Figure 9a;  $\beta_0$  gives the initial immigration level,  $\beta_1$  the slope of the immigration variable in the pre-reform period,  $\beta_2$  the change in level when the reform was implemented in 2002 (implying that  $\beta_2$  can be interpreted as the immediate treatment effect), and  $\beta_3$  the difference between pre- and post-reform trends (implying that  $\beta_3$  can be interpreted as the treatment effect over time).

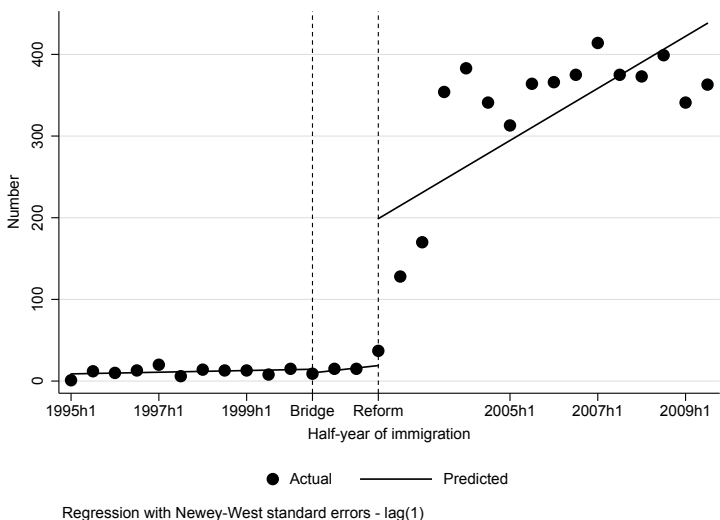
<sup>26</sup>Interrupted time series analysis is a quasi-experimental research design that has the potential to provide good internal validity, especially when applied to both a treatment and a control group (see, e.g., Campbell and Stanley 1966 and Shadish et al. 2002 for a discussion on this). According to Shadish et al. (2002), “[b]oth interrupted time series and regression discontinuity often yield excellent effect estimates”. See also Linden et al. (2015) for an implementation of ITSA in Stata and for further discussion and references.

<sup>27</sup>We have also done the analysis with the reform taking place in the second half of 2002, when the policy was officially implemented; this does not alter our conclusions. The results are available on request.

Figure 9. Stricter reunification rules in Denmark and immigration to Sweden



(a) 2002 intervention



(b) 2000 and 2002 interventions

Notes: The figure displays the regression results based on equation 2.2. The model is estimated using “Interrupted time series analysis” and Newey-West standard errors with one lag are used. Regression estimates are found in Table A.3 in the Appendix. Individuals belonging to the following type of couples are considered treated and included in the estimation: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark) and the other in a non-EU country, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark.

There are three things that can be noted from the figure. First, as was apparent from the summary statistics in Table 3, very few affected individuals migrated to Sweden to form a couple before the reform was instigated in 2002. Second, there is a sharp increase in the number of affected in-migrants after 2002; between 2003 and 2009 there are approximately 300 to 350 individuals that migrated to Sweden to form a couple every half-year. Since the reform was decided on in the beginning of 2002 and instigated on the first of July the same year, we can notice a jump already in 2002. Third,  $\beta_2$  turns out highly significant, while  $\beta_3$  is significant at the 10% level (see left column in Table A.5 in the appendix).<sup>28</sup>

A concern one might have with the ITSA specification in equation (2.2) is the potential interfering effects from the opening of the Öresund Bridge in July 2000.<sup>29</sup> Since the Öresund Bridge offers an easy and fast connection between Copenhagen in Denmark and Malmö in Sweden, and since it is cheaper to live on the Swedish side than on the Danish side, an increase in immigration from Denmark to Sweden as an effect of the bridge could be expected. However, Figure 9a indicates no increase in immigration of the group of individuals affected by the 2002 reform before 2002. When conducting the ITSA analysis with two interventions, one in 2000 and one in 2002, it is also clear that there is no change in the in-migration rate in 2000 (see Figure 9b)

Another concern one might have is the possibility that something else happens in Denmark in 2002 (another reform or change) that explains the increased immigration to Sweden in that year. To examine this, we combine the ITSA analysis in equation (2.2) with a control group approach:

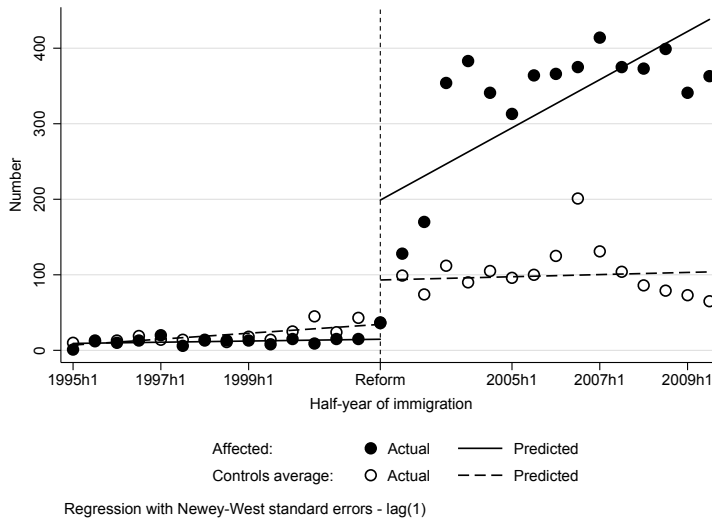
$$IM_t = \theta_0 + \theta_1 T_t + \theta_2 D_t + \theta_3 D_t \times T_t + \theta_4 TREAT_t + \theta_5 TREAT_t \times T_t + \theta_6 TREAT_t \times D_t + \theta_7 TREAT_t \times T_t \times D_t + \eta_t \quad (2.3)$$

where  $TREAT_t$  is a dummy-variable assigning individuals into treatment and control groups (taking the value 1 for those affected by the 2002 reform in Denmark and 0 for those unaffected; c.f. Table 2 for definitions of affected and unaffected). Coefficients  $\theta_4 - \theta_7$  hence refer to the treatment group and coefficients  $\theta_0 - \theta_3$  to the control group. Estimating equation (2.3) gives the results presented in Figure 10a (the dotted lines are for the control group and the solid lines for the treatment group). As is clear from the figure, the unaffected group follows a very different time pattern, with a gradual increase in the number of immigrants of this type from year 2000 and onward, indicating that the individuals unaffected by the 2002 family reunification reform rather started to react on the opening of the Öresund Bridge. This is made clear from

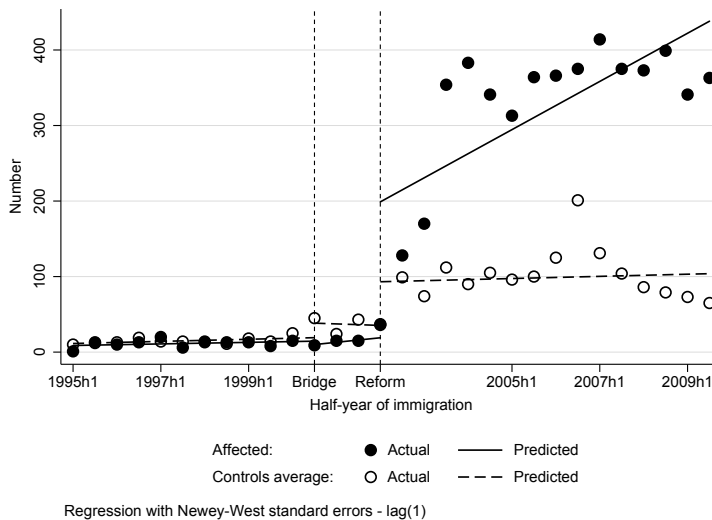
<sup>28</sup> All the results from the ITSA-estimations can be found in the Appendix.

<sup>29</sup> More generally, the concern is that there might be some interfering effects from some other events happening close in time to the year 2002 that might affect immigration to Sweden. For our specific case, we know of no other such threat than the new bridge.

Figure 10. Stricter reunification rules in Denmark and immigration to Sweden: Adding a Danish control group



(a) 2002 intervention



(b) 2000 and 2002 interventions

Notes: The figure graphically displays the regression results based on equation 2.3. The model is estimated using “Interrupted time series analysis” and Newey-West standard errors with one lag are used. Regression estimates are found in Table A.6 in the Appendix. Individuals belonging to the following type of couples are considered treated: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark) and the other in a non-EU country, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark. The control group consists of couples where *both* partners immigrate from Denmark to Sweden as couples who migrate to Sweden for reasons *unrelated* to the possibility of family reunification.

an estimation of the ITSA specification in equation 2.3 augmented with an intervention in 2000 as well (c.f. Figure 10b); while the treatment group does not react on the opening of the bridge in 2000, there is a small, discrete jump for the unaffected group in that year.<sup>30</sup> From the analysis on the Danish data when using half-year frequencies, it is clear that the big effect on out-migration to Sweden takes place in the second half of 2003/first half of 2004. This is likely the explanation for the “additional” discrete jumps observed in these time periods in Figures 9 and 10. When adding an additional “intervention” in the second half of 2003, the yearly effect of the reform in Denmark on out-migration to Sweden in the longer run is clearly visible (c.f. Figure A.11 in the Appendix).

A final concern with the ITSA specification in equation (2.2) is that there might be something else happening in 2002 in Sweden, e.g. a reform making Sweden a particularly attractive country for migrant couples, that could explain the increased immigration to Sweden from 2002 and onward. To examine the relevance of this worry, we once again adopt a control group approach and compare the migration pattern of individuals affected by the immigration reform in Denmark with the migration pattern of the same type of individuals from Sweden’s other two neighboring countries (Norway and Finland). That is, we rerun equation (2.3), but let immigrants from Finland and Norway constitute the control group instead of the unaffected individuals from Denmark.<sup>31</sup>

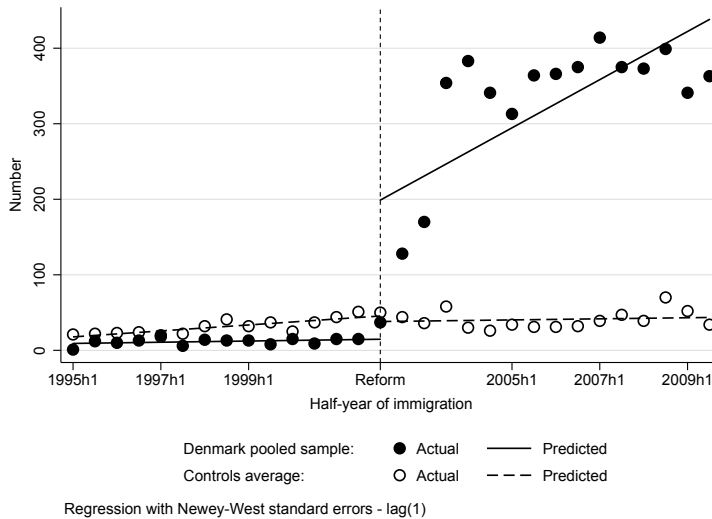
The results, presented in Figure 11 strongly suggest that there are no other things happening around 2002 that can explain the results; while immigration from Denmark sharply increases after 2002, immigration from Finland and Norway remains constant at very low levels.

The patterns observed in Figures 9–11 indicate that the increased reunification-related migration to Sweden would not have happened in the absence of the stricter reunification rules instigated in Denmark in 2002. To put the figure of approximately 350 individuals immigrating to Sweden as an effect of the Danish reform in perspective, it can be worth noting that all the increase in emigration from Denmark after 2001 seems to be related to emigration to Sweden (as

<sup>30</sup>We consider the time pattern after year 2000 for the unaffected group to be mainly a function of the opening of the bridge in that year. However, since the group of unaffected couples consists of at least one foreign-born individual, we cannot rule out that part of the discrete increase in 2002 for the group unaffected by the stricter reunification rules to be a result of increased general discontent with the tougher immigration policies instigated in Denmark in that year (compare also with the results in Table A.6 in the Appendix).

<sup>31</sup>An important assumption here is that there were no major changes in the family reunification policies in Norway and Finland, which is indeed the case. The same goes for Sweden’s reunification policies.

Figure 11. Stricter reunification rules in Denmark and immigration to Sweden: Comparing with immigration from Finland and Norway



Notes: The figure graphically displays the regression results based on equation 2.3. The model is estimated using “Interrupted time series analysis” and Newey-West standard errors with one lag are used. Regression estimates are found in Table A.6 in the Appendix. Individuals belonging to the following type of couples are considered treated: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark) and the other in a non-EU country, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark. The control group consists of the same type of individuals belonging to couples but with a connection to Norway or Finland.

revealed by our earlier analysis on Danish data).<sup>32</sup> The next obvious question is: did the reunited couples stay in Sweden or did they return to Denmark?

#### 2.4.5 After reunification: is there any return migration to Denmark?

Our results strongly indicate that the tougher immigration policies implemented in Denmark in 2002 caused part of the group affected by the reform to move to Sweden. The move to Sweden might have been either a temporary one –

<sup>32</sup>These results are also in line with the two tables on pages 94-95 in Schmidt et al. (2009). Even though the analysis in Schmidt et al. (2009) does not account for the potential effects of the Öresund bridge, the potential effects of reforms taking place in Sweden at the same time, or affected individuals not residing in Denmark, and only considers a limited sample of all individuals in Denmark that could have been affected (they look at 20-year-olds and 25-year-olds with an ethnic minority background), their result also indicates that the figure we find for Sweden could not have been much larger (since there was no increase in emigration to the country of origin or to some other country).



used to be able to reunite with a partner and then return to Denmark – or a more permanent one.

To examine what the pattern looks like we follow partners in all couples that formed in Sweden between 2002 and 2009, and explore their migration behavior from the time the couple is formed until the last time we observe each *partner* in the registers.<sup>33</sup> In order to put the outcomes of interest in perspective, we will compare the migration behavior of the group affected by the reform with those of the group of couples where both partners arrive from Denmark (which served as one of our control groups in the analysis in section 2.4).<sup>34</sup>

From Figure 12, which plots Kaplan-Meier survival estimates by years since immigration for the affected and the unaffected group arriving after 2002, we can draw three main conclusions. First, as Figure 12a shows, a non-significant share of the affected group arriving in 2002 or later leaves Sweden within a few years since arrival; approximately 20 (50) percent leave within two (eight) years (c.f. the dotted line).<sup>35</sup> Second, when comparing with the unaffected group (dotted vs. solid line), the affected group emigrates to a larger extent in every year since arrival (with a widening gap between the two groups over time). The propensity to leave after 10 years in Sweden is around 10 percentage points higher for the affected group than for the unaffected group. Third, when focusing on the affected group only (see Figure 12b) and comparing those that have arrived from Denmark (dotted line) with

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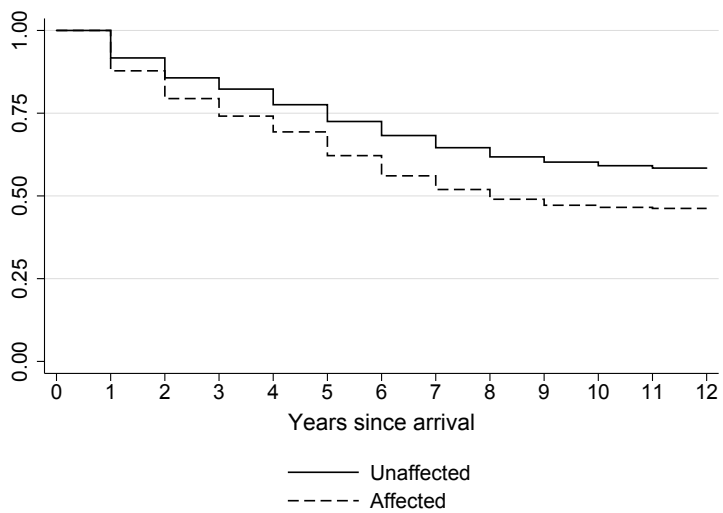
<sup>33</sup>That is, we follow individuals even if the couple breaks up at some point after arrival. Furthermore, we focus on how the migration spell corresponding to reunification ended for each partner. Some partners leave permanently, others temporarily and others don't leave at all. By looking only at how the reunification migration spell ends, we cannot say whether the exit is permanent or temporary.

<sup>34</sup>Since return or temporary migration is a widespread phenomenon, regardless of the original reason for migration (for an overview see Dustmann and Görlach 2015), we expect mobility to be high. Earlier studies have also shown that cross-border mobility within the Nordic countries is particularly high (Edin et al. 2000, Jensen and Pedersen 2007). Edin et al. (2000), for example, show that about 45% of the Nordic immigrants to Sweden leave the country within five years after arrival. It can also be noted that the motives behind the move to Sweden are likely to vary between these two groups. While it is true that the move across Öresund is a voluntary choice for both groups, the Danish reforms left little room for the affected group to reunite with a partner in Denmark. Thus, the two groups are likely to maximize different objectives when deciding on whether they should move to Sweden or not. Whether we should expect the affected couples to leave Sweden to a larger extent than the unaffected group is however difficult to say. The 2002 reforms made Denmark less welcoming to migrants in general, which might have lowered the groups' overall propensity to return to Denmark. Furthermore, the connection to Denmark is arguably weaker for the affected group than for the unaffected group (where both partners arrive from Denmark) since one of the partners in the couples that reunite in Sweden arrives from a country outside the EU.

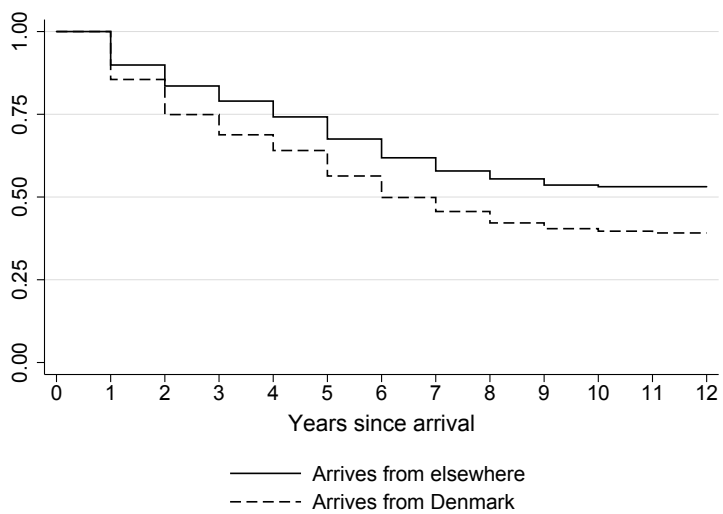
<sup>35</sup>Our onward migration figures are larger than those found in Schmidt et al. (2009). This can be due to the fact that we have a control group approach and/or that we consider a longer post-reform time period.

those that have arrived from elsewhere (solid line), it is also clear that the former group emigrates from Sweden to a larger extent than the latter group.

Figure 12. Onward migration



(a) Affected vs. unaffected



(b) Only affected, by country of arrival

Notes: The sample includes all individuals belonging to couples that were formed in Sweden between 2002 and 2009. Panel (a) plots Kaplan-Meier survival estimates - where survival is defined as being in Sweden in 2014 or the year before death, whichever comes first - by years since immigration, for the affected and the unaffected group arriving after 2002. Panel (b) does the same but only for the affected group, by whether they arrive from Denmark or elsewhere.

But to which countries do those that leave Sweden emigrate to? Do they move to Denmark or choose a different destination? From Table 4 it is clear that the absolute majority (around 87 percent) of the individuals in the affected group that leave Sweden go to Denmark. This figure is also larger than the corresponding figure for the unaffected group (around 80 percent). When looking at the affected group, we find that as many as 95 percent of those that arrived from Denmark to Sweden go back to Denmark (conditional on emigration). The corresponding figure is lower (77.7 percent) for those that arrived to Sweden from elsewhere.

**Table 4.** *Onward migration statistics*

|                               | Affected |                      |                        | Unaffected |
|-------------------------------|----------|----------------------|------------------------|------------|
|                               | All      | Arrives from Denmark | Arrives from elsewhere |            |
| % leaving Sweden before 2014  | 53.10    | 59.70                | 46.90                  | 42.40      |
| <i>Conditional on leaving</i> |          |                      |                        |            |
| % going to Denmark            | 87.10    | 95.00                | 77.70                  | 80.10      |
| % going to their home country | 4.30     | 0.80                 | 8.40                   | 5.50       |
| % going elsewhere             | 8.60     | 4.20                 | 13.80                  | 14.30      |
| Mean no. of years in SE       | 2.95     | 2.94                 | 2.97                   | 3.12       |
| (st. dev. in parentheses)     | (2.45)   | (2.44)               | (2.46)                 | (2.64)     |
| Observations                  | 5095     | 2447                 | 2648                   | 1576       |

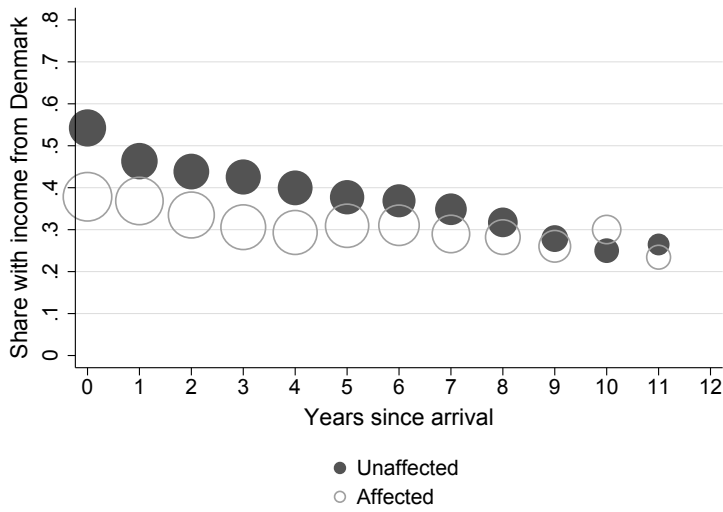
*Notes:* The sample includes all individuals belonging to couples that were formed in Sweden between 2002 and 2009.

The results in Table 4 indicate that the move to Sweden may have been from the beginning seen as a temporary one, motivated by the inability to reunite with a partner in Denmark but still having Denmark as the preferred country to live in. This interpretation is also backed up by the geographical location in Sweden of the affected individuals that we presented earlier (with the majority settling in areas very close to Denmark).

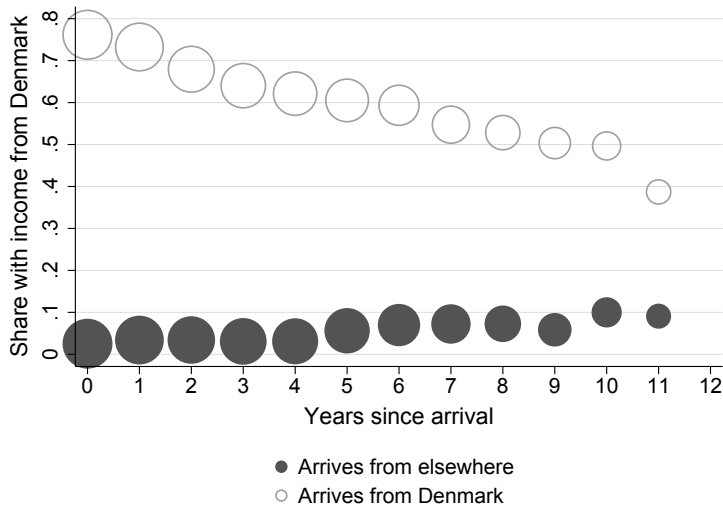
That the migration decision for many individuals was a temporary one is an interpretation that is also in line with the fact that a clear majority (around 80 percent) of those in the affected group that migrated to Sweden in the wake of the Danish reform have labor earnings from Denmark during the year of arrival (see Figure 13b); over time the share falls but remains above 40 percent throughout the period of study.<sup>36</sup> Thus, it is evident that many of the migrants coming to Sweden as a consequence of the reform remain employed in Denmark.

<sup>36</sup>In the Swedish register data we can observe whether an individual has work-related earnings from Denmark.

Figure 13. Earnings from Denmark



(a) Affected vs. unaffected



(b) Only affected, by country of arrival

Notes: The sample includes all individuals belonging to couples that were formed in Sweden between 2002 and 2009. The information for income from Denmark is missing for 2014. In both figures, each circle radius is equal to the square root of the number of observations in each group, for each year since arrival.

## 2.5 Conclusions

In this paper we address two questions. First, we examine if and to what extent tougher immigration policies in one EU country cause individuals to move to a neighboring country (i.e., if there are spillover effects of a country's immigration policies). Second, we explore whether those that leave because of tougher policies do so indefinitely or return at a later stage. To answer these questions, we estimate the effect of a 2002 immigration policy reform in Denmark that made it much harder for families to reunite in Denmark. The new rules made it impossible for Danish residents under the age of 24, and very hard for those above 24, to reunite with partners from outside the EU.

Starting with the first question, using Danish register data, we find strong evidence in support of the reform causing an increase in emigration from Denmark. After 2002 our results show a statistically significant increase of the likelihood to emigrate for those affected by the reform. Our analysis shows that Sweden absorbed the absolute majority of those potentially affected by the Danish reform.<sup>37</sup> We also show that the increase in the likelihood to emigrate (to Sweden) is stronger for the Danish residents residing on Zealand, the Danish region located most closely to Sweden.

Using interrupted time series analysis on Swedish register data, we confirm the findings based on Danish data. We find a clear and significant effect of the reform in Denmark on reunification-related migration to Sweden. While that type of migration was almost non-existent before the reform, approximately 350 individuals migrated to Sweden every six months after the reform to form a partnership with a non-EU partner.

One explanation of the magnitude of the effect, that we have already touched upon, might be the geographic closeness and easy access to the most southern part of Sweden from Zealand, where most individuals affected by the reform live.<sup>38</sup> Examining the location pattern of the affected immigrants in Sweden, we find that, to a very large extent, they locate in the southern-most region in Sweden (and then mainly in the cities with very good ferry- and bridge-connection with Zealand, Malmö and Helsingborg).

With the use of Swedish data we can also rule out alternative explanations to the strong immigration flow to Sweden following the reform. We can rule out that the opening of the Öresund bridge, that connects Copenhagen (the capital of Denmark) with Malmö (the third largest city of Sweden), just two years before the reform can explain the inflow, or that there is something else happening in Sweden, such as a reform that made Sweden particularly attractive for migrant couples, around the years of the reform.

Regarding the second question, whether those that migrated to Sweden as an effect of the reform in Denmark stayed on in Sweden or not, our results

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<sup>37</sup>This is also indicated by the figures presented in Schmidt et al. (2009).

<sup>38</sup>Schmidt et al. (2009) also find that most of the emigration from Denmark to Sweden in the group they study takes place from the Copenhagen area.

show that a non-trivial fraction of those affected by the Danish reform seem to have considered the move to Sweden as a temporary one; already after two years approximately 20% out-migrate from Sweden and after eight years the corresponding figure is approximately 50%. The out-migration rate is significantly higher for the affected group than for a similar but unaffected group. In addition, the absolute majority of those that out-migrate after forming a couple in Sweden go back to Denmark, indicating that Denmark was their preferred choice of location (also after the move).

The results in this paper suggest that spillover effects of national migration policies can be substantial and should be considered when shaping new, country-specific, immigration policies. An important task for future research is to examine whether this type of spillover effects affect the policy-setting behavior of neighboring countries. In this respect, the paper is related to the literature on strategic interactions among different regions (countries, states, etc.) in the determination of fiscal policies (see e.g. Brueckner 2000 and Dahlberg and Edmark 2008). If political decision-makers believe that generous rules for family reunification attract immigrants, and if it is assumed that nobody wants to be the most generous jurisdiction in the region, a “race-to-the-bottom” in the setting of migration policies levels is likely to materialize. Evidence on this, in combination with the evidence found in this paper, constitute important input to the active debate within the EU on whether immigration policies should be set at the European or at the national level.

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Reunification of Third Country Nationals in EU Member States. *Policy Research  
Centre Integration; Antwerp, Belgium.*

## Appendix

### Danish register data and sample selection

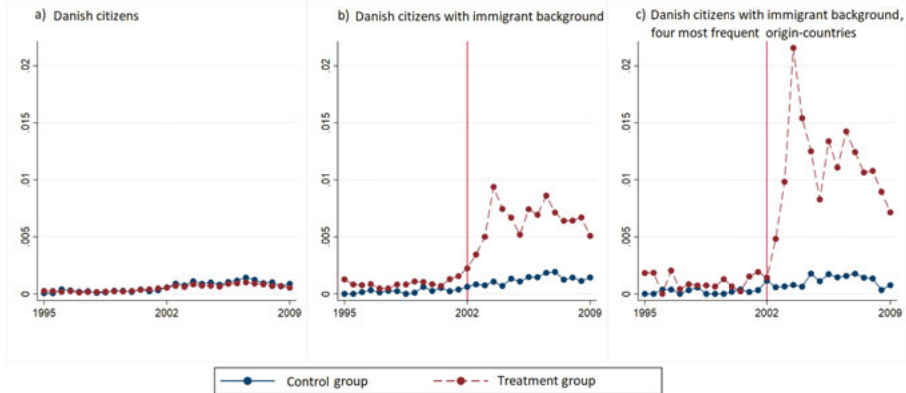
For our analysis regarding emigration of Danish residents we use Danish administrative data for the years 1995 to 2009 from the population and migration registers held by Statistics Denmark. The population register contains information on gender, age, municipality of residence, migration background, citizenship and country of origin for each resident registered in Denmark in a given year. The data also provides information on family status of an individual such as whether the person has a married/registered/cohabiting partner as well as the number and age of children living in the household. The migration register contains all registered in- and out-migration events including date and country of origin/destination. Registering emigration is compulsory for Danish residents if leaving the country for more than six months.

Concerning the migration data we consider in our analysis, all emigration events to any country - except the Faroe Islands and Greenland as these are autonomous Danish overseas territories - are recorded. We do not impose any restriction on the length of the migration spell; if an individual emigrates more than once in a given year we only consider the latest emigration event in that year. We merge the migration and the population data for each of the considered cross-section years using an anonymized identifier based on each resident's social security number. For residents living with or married to a partner in Denmark we also merge information on the partner's age, country of origin and citizenship. In our final data set we keep only individuals that are registered as residents in Denmark in a given year and that either emigrate to a foreign country (except for Faroe Islands and Greenland) or do not emigrate in a given year and show up in the register data in the subsequent year.

## Additional empirical results on emigration from Denmark

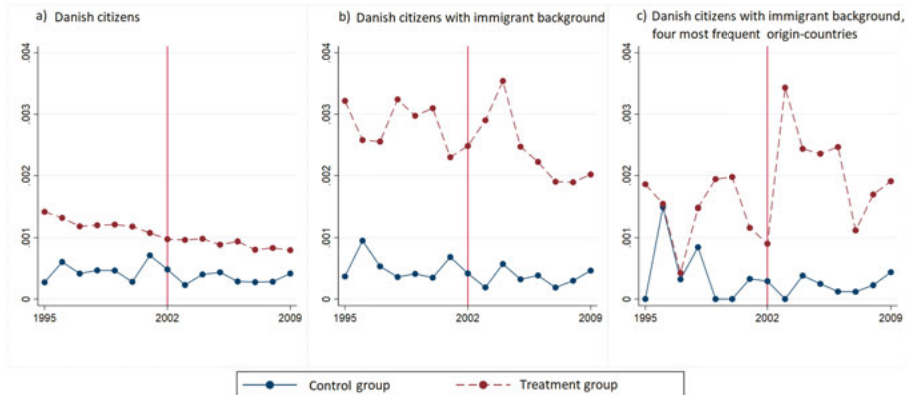
### Figures

*Figure A.1. Half-yearly emigration rates of treatment and control group to Sweden*



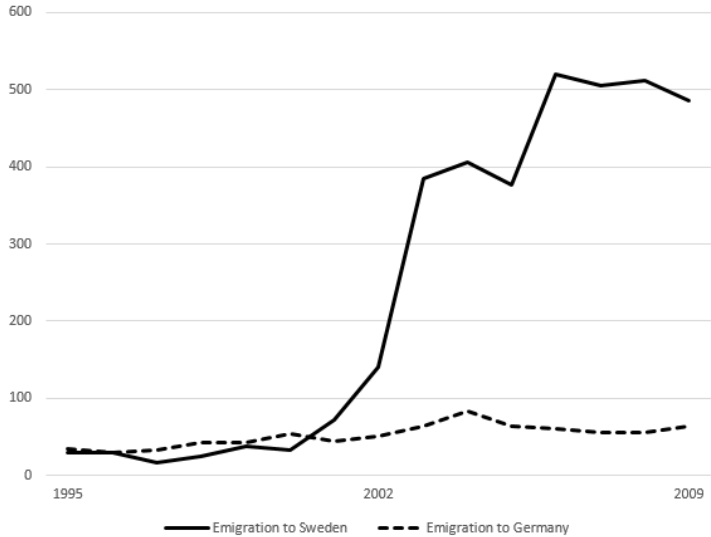
*Notes:* The treatment group are Danish citizens older than 18, not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark.

*Figure A.2. Yearly emigration rates of treatment and control group to Germany*



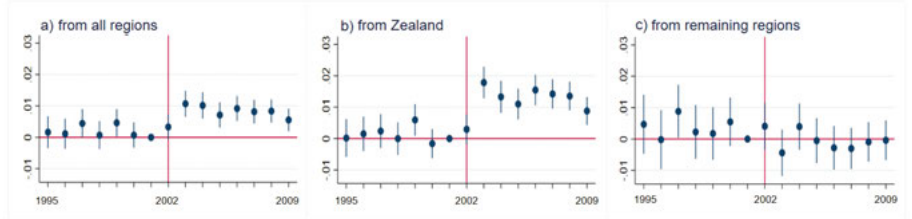
*Notes:* The treatment group are Danish citizens older than 18, not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark.

Figure A.3. Number of emigrants with immigrant background in treatment group to Germany and Sweden



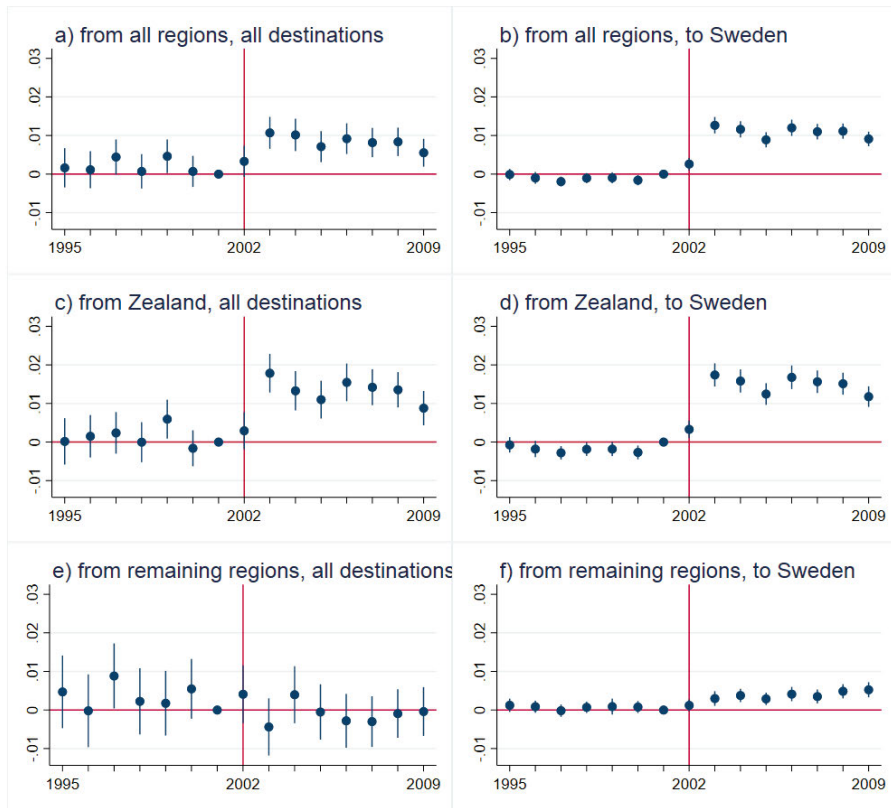
Notes: The treatment group are Danish citizens older than 18, not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark.

Figure A.4. Coefficient plots for interaction effect on emigration of Danish citizens with immigrant background to all destinations



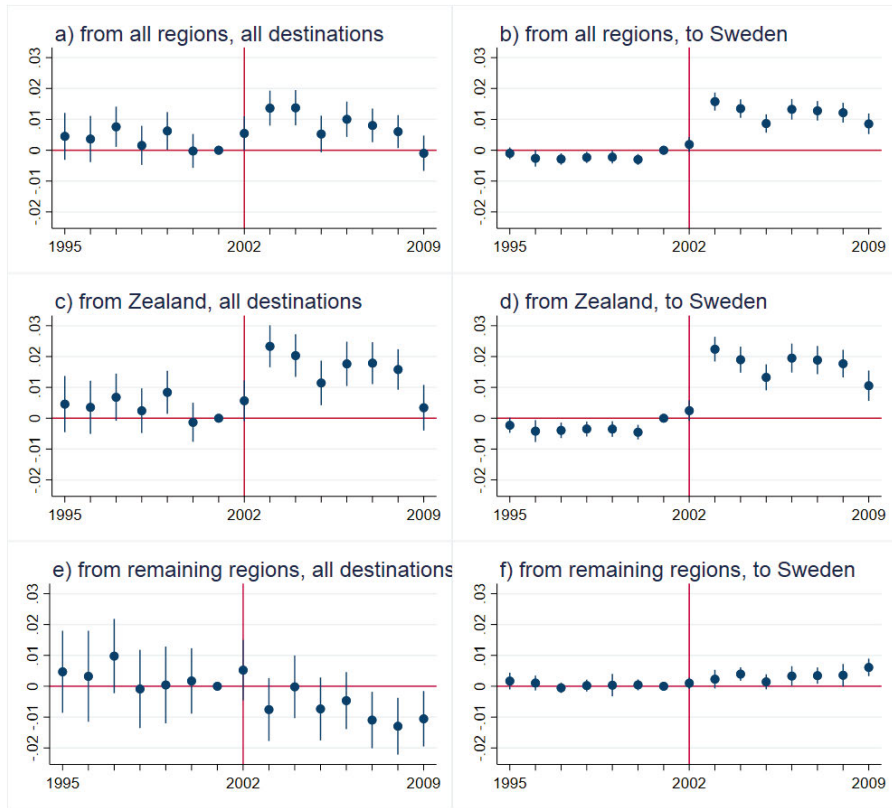
Notes: The sample includes Danish citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for year and treatment. The figure shows coefficient estimates for the interaction effect  $treatment \times year$ . Confidence intervals indicate statistical significance at the 5% level, with standard errors clustered at the individual level.

Figure A.5. Coefficient plots for interaction effect on migration for Danish citizens with immigrant background. Regressions with additional control variables



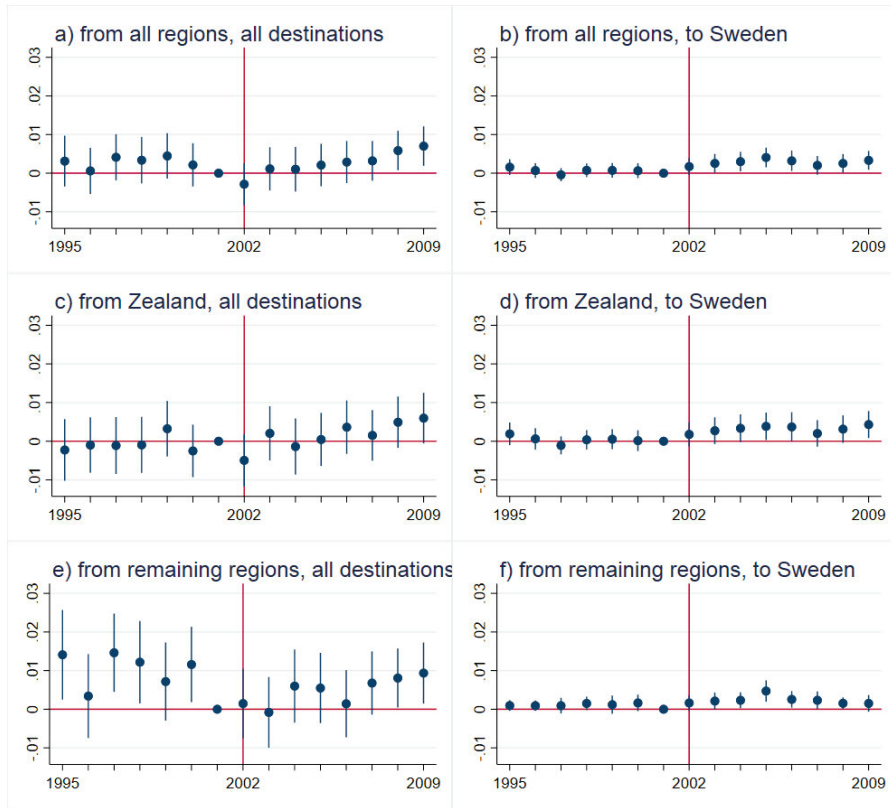
Notes: The sample includes Danish citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year*, *treatment*, *year x treatment*, *age*, *female*, *children*. The figure shows coefficient estimates for the interaction effect *treatment x year*. Confidence intervals indicate statistical significance at the 5% level, with standard errors clustered at the individual level.

Figure A.6. Coefficient plots for interaction effect on migration for Danish citizens younger than 29 with immigrant background



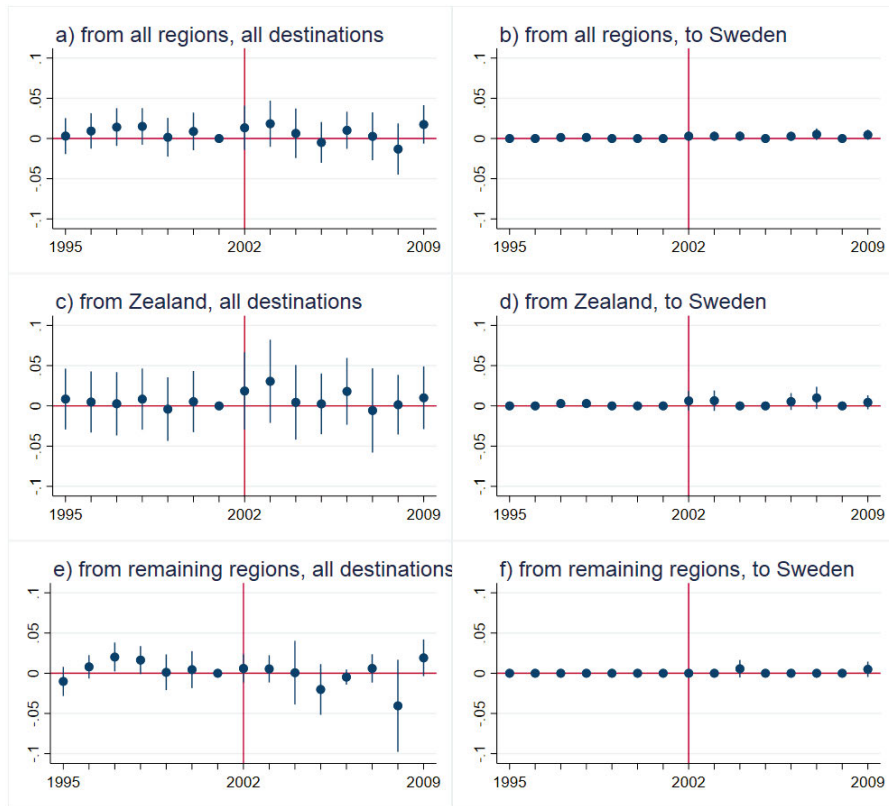
Notes: The sample includes Danish citizens with immigrant background, older than 18 and younger than 29 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year* and *treatment*. The figure shows coefficient estimates for the interaction effect  $treatment \times year$ . Confidence intervals indicate statistical significance at the 5% level, with standard errors clustered at the individual level.

Figure A.7. Coefficient plots for interaction effect on migration for Danish citizens aged 29 or older with immigrant background



Notes: The sample includes Danish citizens with immigrant background, aged 29 or older in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year* and *treatment*. The figure shows coefficient estimates for the interaction effect *treatment* x *year*. Confidence intervals indicate statistical significance at the 5% level, with standard errors clustered at the individual level.

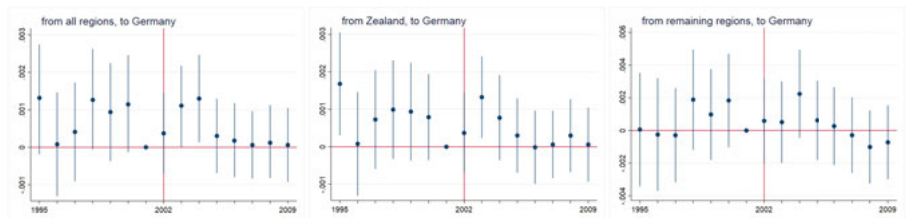
Figure A.8. Coefficient plots for interaction effect on migration for Danish citizens with immigrant background and non-EU/EEA citizenship



Notes: The sample includes non-EU/EEA citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year* and *treatment*. The figure shows coefficient estimates for the interaction effect  $treatment \times year$ . Confidence intervals indicate statistical significance at the 5% level, with standard errors clustered at the individual level.

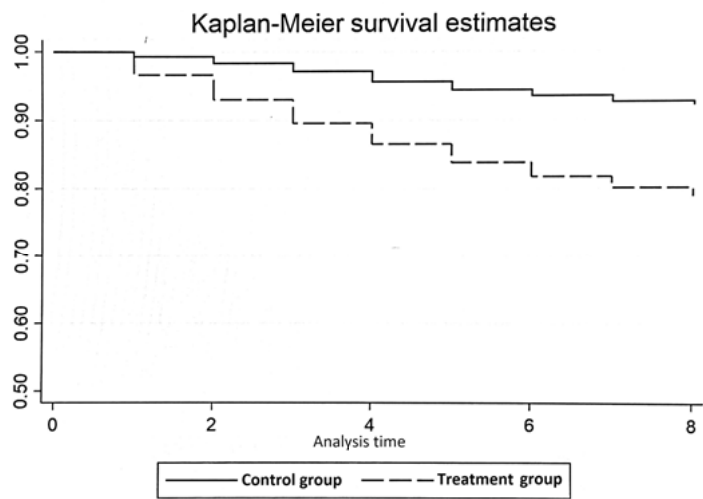


Figure A.9. Coefficient plots for interaction effect on migration to Germany for Danish citizens with immigrant background



Notes: The sample includes Danish citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year* and *treatment*. The figure shows coefficient estimates for the interaction effect *treatment* x *year*. Confidence intervals show indicate statistical significance at the 5% level, with standard errors clustered at the individual level.

Figure A.10. Kaplan-Meier estimates for emigration from Denmark to all destinations



Notes: The treatment group are Danish citizens between 18 and 29 years old in 2001, not cohabiting with a partner in that year. The control group are Danish citizens between 18 and 29 years old in 2001, cohabiting with a non-EU/EEA citizen in 2001 in Denmark. Only individuals with immigrant background included. Individuals are excluded from the analysis if they drop out of the sample during the analysis period until 2009 and are not registered as emigrants.

## Tables

**Table A.1.** *Difference in difference regression*

|                 | Out-migration to<br>all countries |                       |                              | Out-migration to<br>Sweden |                        |                              |
|-----------------|-----------------------------------|-----------------------|------------------------------|----------------------------|------------------------|------------------------------|
|                 | All                               | From<br>Zealand       | From<br>remaining<br>regions | All                        | From<br>Zealand        | From<br>remaining<br>regions |
| $\gamma_{1995}$ | 0.0016<br>(0.0026)                | 0.00016<br>(0.0031)   | 0.0047<br>(0.0048)           | -0.0001<br>(0.00075)       | -0.0007<br>(0.0010)    | 0.0012<br>(0.0009)           |
| $\gamma_{1996}$ | 0.0011<br>(0.0025)                | 0.0015<br>(0.0028)    | -0.0020<br>(0.0048)          | -0.0010<br>(0.0008)        | -0.0018*<br>(0.0011)   | 0.0009<br>(0.0008)           |
| $\gamma_{1997}$ | 0.0044*<br>(0.0023)               | 0.0024<br>(0.0028)    | 0.0088**<br>(0.0043)         | -0.0020***<br>(0.0007)     | -0.0028***<br>(0.0009) | -0.0002<br>(0.0008)          |
| $\gamma_{1998}$ | 0.0007<br>(0.0023)                | 0.0000<br>(0.0027)    | 0.0023<br>(0.0044)           | -0.0010<br>(0.0007)        | -0.0019**<br>(0.0009)  | 0.0007<br>(0.0008)           |
| $\gamma_{1999}$ | 0.0046**<br>(0.0022)              | 0.0059**<br>(0.0026)  | 0.0018<br>(0.0043)           | -0.0010<br>(0.0007)        | -0.0018*<br>(0.0009)   | 0.0009<br>(0.0011)           |
| $\gamma_{2000}$ | 0.0007<br>(0.0021)                | -0.0016<br>(0.0024)   | 0.0055<br>(0.0040)           | -0.0016**<br>(0.0008)      | -0.0027***<br>(0.0009) | 0.0008<br>(0.0008)           |
| $\gamma_{2001}$ | -<br>-                            | -<br>-                | -<br>-                       | -<br>-                     | -<br>-                 | -<br>-                       |
| $\gamma_{2002}$ | 0.0033<br>(0.0021)                | 0.0029<br>(0.0025)    | 0.0041<br>(0.0038)           | 0.0026***<br>(0.0008)      | 0.0033***<br>(0.0012)  | 0.0012<br>(0.0008)           |
| $\gamma_{2003}$ | 0.0107***<br>(0.0021)             | 0.0178***<br>(0.0026) | -0.0044<br>(0.0038)          | 0.0127***<br>(0.0011)      | 0.0174***<br>(0.0015)  | 0.0030***<br>(0.0010)        |
| $\gamma_{2004}$ | 0.0102***<br>(0.0021)             | 0.0133***<br>(0.0026) | 0.0040<br>(0.0038)           | 0.0116***<br>(0.0011)      | 0.0158***<br>(0.0015)  | 0.0038***<br>(0.0009)        |
| $\gamma_{2005}$ | 0.0071***<br>(0.0021)             | 0.0110***<br>(0.0025) | -0.0005<br>(0.0037)          | 0.0089***<br>(0.0010)      | 0.0124***<br>(0.0014)  | 0.0029***<br>(0.0008)        |
| $\gamma_{2006}$ | 0.0092***<br>(0.0020)             | 0.0155***<br>(0.0025) | -0.0028<br>(0.0036)          | 0.0120***<br>(0.0011)      | 0.0168***<br>(0.0016)  | 0.0041***<br>(0.0009)        |
| $\gamma_{2007}$ | 0.0082***<br>(0.0019)             | 0.0142***<br>(0.0024) | -0.0030<br>(0.0034)          | 0.0110***<br>(0.0010)      | 0.0156***<br>(0.0015)  | 0.0035***<br>(0.0009)        |
| $\gamma_{2008}$ | 0.0084***<br>(0.0019)             | 0.0135***<br>(0.0023) | -0.0009<br>(0.0032)          | 0.0111***<br>(0.0010)      | 0.0151***<br>(0.0015)  | 0.0049***<br>(0.0009)        |
| $\gamma_{2009}$ | 0.0055***<br>(0.0019)             | 0.0088***<br>(0.0023) | -0.0004<br>(0.0032)          | 0.0091***<br>(0.0010)      | 0.0118***<br>(0.0014)  | 0.0053***<br>(0.0010)        |
| $\beta_1$       | 0.0150***<br>(0.0015)             | 0.0134***<br>(0.0017) | 0.0184***<br>(0.0027)        | 0.0027***<br>(0.0005)      | 0.0036***<br>(0.0007)  | 0.0007<br>(0.0005)           |
| $\beta_0$       | 0.0111***<br>(0.0009)             | 0.0110***<br>(0.0011) | 0.0115***<br>(0.0017)        | 0.0011***<br>(0.0003)      | 0.0014***<br>(0.0004)  | 0.0002<br>(0.0002)           |
| R-squared       | 0.005                             | 0.006                 | 0.004                        | 0.006                      | 0.009                  | 0.002                        |
| Mean            | 0.0208                            | 0.0189                | 0.0251                       | 0.0018                     | 0.0021                 | 0.0011                       |
| Observations    | 508,911                           | 340,605               | 168,306                      | 500,683                    | 335,820                | 164,863                      |

*Notes:* The sample includes Danish citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes dummy variables for all years. The mean given in the table refers to the average out-migration rate over the 1995-2001 period. Standard errors are clustered at the individual level. Stars denote levels at which coefficients are statistically significantly different from zero: \*\*\* 1% level, \*\* 5% level, \* 10% level.

**Table A.2.** *Difference in difference regression with control variables*

|                 | Out-migration to<br>all countries |                       |                              | Out-migration to<br>Sweden |                        |                              |
|-----------------|-----------------------------------|-----------------------|------------------------------|----------------------------|------------------------|------------------------------|
|                 | All                               | From<br>Zealand       | From<br>remaining<br>regions | All                        | From<br>Zealand        | From<br>remaining<br>regions |
| $\gamma_{1995}$ | 0.0032<br>(0.0027)                | 0.0020<br>(0.0031)    | 0.0061<br>(0.0050)           | 0.0009<br>(0.0008)         | 0.0007<br>(0.0011)     | 0.0017*<br>(0.0009)          |
| $\gamma_{1996}$ | 0.0022<br>(0.0025)                | 0.0027<br>(0.0029)    | 0.0012<br>(0.0050)           | -0.0003<br>(0.0008)        | -0.0010<br>(0.0011)    | 0.0014<br>(0.0009)           |
| $\gamma_{1997}$ | 0.0054**<br>(0.0024)              | 0.0034<br>(0.0029)    | 0.0099**<br>(0.0045)         | -0.0014*<br>(0.0007)       | -0.0021**<br>(0.0009)  | 0.0002<br>(0.0008)           |
| $\gamma_{1998}$ | 0.0009<br>(0.0024)                | 0.0002<br>(0.0027)    | 0.0024<br>(0.0045)           | -0.0007<br>(0.0007)        | -0.0014<br>(0.0010)    | 0.0010<br>(0.0008)           |
| $\gamma_{1999}$ | 0.0046**<br>(0.0023)              | 0.0062**<br>(0.0027)  | 0.0011<br>(0.0044)           | -0.0007<br>(0.0008)        | -0.0015<br>(0.0010)    | 0.0011<br>(0.0011)           |
| $\gamma_{2000}$ | 0.0004<br>(0.0021)                | -0.0014<br>(0.0025)   | 0.0042<br>(0.0041)           | -0.0016**<br>(0.0007)      | -0.0028***<br>(0.0010) | 0.0011<br>(0.0008)           |
| $\gamma_{2001}$ | -<br>-                            | -<br>-                | -<br>-                       | -<br>-                     | -<br>-                 | -<br>-                       |
| $\gamma_{2002}$ | 0.0020<br>(0.0021)                | 0.0021<br>(0.0025)    | 0.0018<br>(0.0040)           | 0.0025***<br>(0.0009)      | 0.0031**<br>(0.0012)   | 0.0012<br>(0.0008)           |
| $\gamma_{2003}$ | 0.0096***<br>(0.0022)             | 0.0171***<br>(0.0027) | -0.0063<br>(0.0039)          | 0.0126***<br>(0.0012)      | 0.0172***<br>(0.0016)  | 0.0029***<br>(0.0010)        |
| $\gamma_{2004}$ | 0.0083***<br>(0.0022)             | 0.0116***<br>(0.0027) | 0.0015<br>(0.0039)           | 0.0112***<br>(0.0011)      | 0.0153***<br>(0.0016)  | 0.0035***<br>(0.0009)        |
| $\gamma_{2005}$ | 0.0048**<br>(0.0021)              | 0.0085***<br>(0.0026) | -0.0025<br>(0.0038)          | 0.0080***<br>(0.0010)      | 0.0111***<br>(0.0015)  | 0.0030***<br>(0.0009)        |
| $\gamma_{2006}$ | 0.0070***<br>(0.0021)             | 0.0132***<br>(0.0026) | -0.0053<br>(0.0037)          | 0.0112***<br>(0.0011)      | 0.0155***<br>(0.0016)  | 0.0042***<br>(0.0010)        |
| $\gamma_{2007}$ | 0.0054***<br>(0.0020)             | 0.0116***<br>(0.0025) | -0.0062*<br>(0.0035)         | 0.0098***<br>(0.0011)      | 0.0141***<br>(0.0016)  | 0.0030***<br>(0.0009)        |
| $\gamma_{2008}$ | 0.0058***<br>(0.0020)             | 0.0108***<br>(0.0024) | -0.0031<br>(0.0034)          | 0.0096***<br>(0.0011)      | 0.0129***<br>(0.0015)  | 0.0045***<br>(0.0010)        |
| $\gamma_{2009}$ | 0.0027<br>(0.0019)                | 0.0058**<br>(0.0024)  | -0.0028<br>(0.0034)          | 0.0075***<br>(0.0010)      | 0.0095***<br>(0.0014)  | 0.0052***<br>(0.0011)        |
| $\beta_1$       | 0.0091***<br>(0.0015)             | 0.0078***<br>(0.0018) | 0.0111***<br>(0.0028)        | 0.0003<br>(0.0006)         | 0.0002<br>(0.0008)     | 0.0000<br>(0.0005)           |
| $\beta_0$       | 0.0145***<br>(0.0014)             | 0.0140***<br>(0.0018) | 0.0163***<br>(0.0025)        | 0.0027***<br>(0.0007)      | 0.0048***<br>(0.0010)  | 0.0002<br>(0.0007)           |
| R-squared       | 0.008                             | 0.009                 | 0.007                        | 0.008                      | 0.011                  | 0.003                        |
| Observations    | 508,911                           | 340,605               | 168,306                      | 500,683                    | 335,820                | 164,863                      |

*Notes:* The sample includes Danish citizens with immigrant background, older than 18 in a given year from 1995 to 2009. The treatment group are single individuals not cohabiting with a partner in a given year in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in a given year in Denmark. Estimation includes a constant and dummy variables for *year*, *age*, *female*, *children*. The mean given in the table refers to the average out-migration rate over the 1995-2001 period. Standard errors are clustered on the individual level. Stars denote levels at which coefficients are statistically significantly different from zero: \*\*\* 1% level, \*\* 5% level, \* 10% level.

**Table A.3.** *Descriptive statistics (duration analysis)*

|                      | Treatment<br>Group | Control<br>Group |
|----------------------|--------------------|------------------|
| Emigration           | 2,815              | 323              |
| Emigration to Sweden | 1,439              | 143              |
| Female               | 42%                | 56%              |
| Children             | 2%                 | 63%              |
| Zealand residents    | 69%                | 74%              |
| Born in Denmark      | 48%                | 40%              |
| Observations         | 13,292             | 4,130            |

*Notes:* The sample includes Danish citizens with immigrant background, older than 18 and younger than 29 in 2001 ( $t=0$ ). The treatment group are single individuals not cohabiting with a partner in year 2001 in Denmark. The control group are Danish citizens older than 18, cohabiting with a non-EU/EEA citizen in 2001. All individuals must be in the data for the full observation period if they do not emigrate, i.e. until 2009.

## Swedish register data and identifying the reunited couples

In this section we describe the way in which we identify the two types of affected couples in our data. There are (as we discussed in Section 2.4) two ways in which a Danish citizen can apply for family reunification in Sweden; either through the Swedish national rules or through the EU/EEA rules. These two different channels guide us in identifying the affected couples.

### **Couples where one partner is born in Denmark and the other one in a non-EU-country**

Turning first to couples consisting of one partner born in Denmark (that has moved to Sweden) and the other one in a third country (outside EU) we proceed in the following way to identify the affected couples: We allow for partners to move to Sweden from (i) different countries or (ii) the same country as long as this country is not Denmark.

(i) captures couples that are not married (or in a registered partnership), in which the third country national would need to apply for a residence permit in the country of origin before a move to Sweden is possible.

(ii) captures couples immigrating from the same country, provided that country is *not* Denmark. If that country is not a member of the EU, it would be the case that the Danish-born partner had traveled to that country, met their spouse and the couple decided together to move to Denmark (Sweden), with the non-EU spouse in need of a permit. If that country is a member of EU (with the exception of Denmark), the non-EU spouse would still need a per-

mit as the residence permit for one EU country is not valid for a different EU country if the stay is longer than three months.<sup>39</sup>

With respect to timing, we restrict our sample to couples who arrive within 6, 12 or 18 months of each other. This is to ensure that we capture both couples who apply under EU rules (with processing times up to 6 months) and couples who apply under Swedish rules (with longer processing times). We further restrict our sample to couples who are registered as belonging to the same household the year when the non-EU partner enters Sweden.<sup>40</sup> This is so as to not wrongly include couples who formed after their arrival in Sweden as single individuals.<sup>41</sup>

### **Couples where both partners are born in a non-EU country, with only one partner arriving from Denmark**

In this case, if being born in a non-EU country is a good proxy for citizenship, the application for family reunification can only be made under Swedish rules. However, there can of course be individuals who are born in a non-EU country but who have Danish citizenship, therefore they are eligible to apply under either type of rules. As we explained in section 2.4, we are confident that the latter case is the dominant one here.

We keep all couples where both partners are born in a non-EU country, with the restriction that only one of them migrates to Sweden from Denmark for reasons outlined in section 2.4. All other restrictions are as before.

### **Validation of our definition of treatment**

We check our definition of reunification against the data we have on “reason for migration”.<sup>42</sup> The information on reason for migration is only available for years 1997 and onward, regardless of whether the move was done before 1997. For example, if someone moved in 1995, they will show up with missing reason for migration in the 1996 register year, but non-missing in 1997. This allows us to extend the variable to 1995 and 1996 without worrying about measurement error. We ignore further changes in residence permit and assign individuals the permit they obtained upon arrival in Sweden.

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<sup>39</sup>There is an exception to that rule: if the non-EU spouse had previously obtained long-term resident status in an EU country other than Denmark, Ireland and the United Kingdom, they may be able to transfer that status to Sweden.

<sup>40</sup>In our data, married couples and cohabiting couples who have a child together share a common identifier.

<sup>41</sup>See Niedomysl et al. (2010) who use the same definition in their study on marriage migrants in Sweden.

<sup>42</sup>“Reason for migration” (“Grund för bosättning”) should be interpreted here as “type of residence permit”; a missing value for an EU individual simply indicates that that person does not need a residence permit. In our data we can identify four types of “reasons for migration”: i) work, ii) family, iii) refugee or protected status, and iv) other.

**Table A.4.** *Sample size and characteristics for reunified couples arriving within n months of each other*

|                                   | <i>n=6</i> | <i>n=12</i> | <i>n=18</i> |
|-----------------------------------|------------|-------------|-------------|
| <i>Number of couples</i>          | 1778       | 2630        | 2974        |
| <i>% with family ties permit</i>  | 94.66      | 95.48       | 95.26       |
| <i>% with missing information</i> | 4.39       | 3.33        | 3.21        |

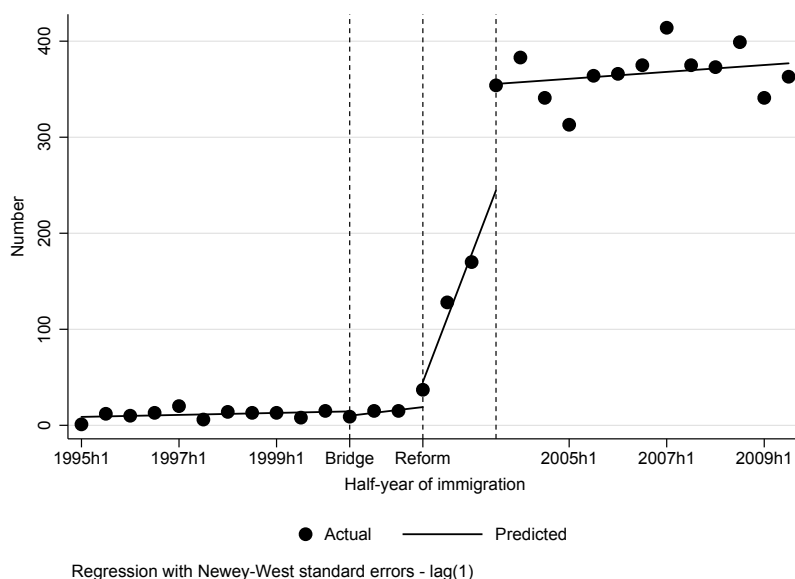
In Table A.4 we summarize the characteristics of the two types of couples affected by the reform (see Table 2). We see that regardless of the timing restriction (partners arriving within 6, 12, or 18 months from each other), the non-EU partner is in over 94% in Sweden on a family ties permit. This gives us confidence in our definition of reunification. Furthermore, for most of the remaining couples, the information on the reason for migration is missing so we can neither confirm nor deny the accuracy of our definition.<sup>43</sup>

<sup>43</sup>For the remaining couples, the reason for migration is different from family ties.

## Additional empirical results on immigration to Sweden

### Figures

Figure A.11. Stricter reunification rules in Denmark and immigration to Sweden: Adding an additional break in 2003



Notes: The figure displays the regression results based on equation (2). The model is estimated using “Interrupted time series analysis” and Newey-West standard errors with one lag are used. Individuals belonging to following type of couples are considered treated: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark) and the other in a non-EU country, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark.

## Tables

**Table A.5.** *Interrupted time series analysis results: no comparison group*

| Yearly number of partners            | 2002 treatment    | 2000 and 2002 treatments |
|--------------------------------------|-------------------|--------------------------|
| <i>2002 effect</i>                   | 184.19<br>(65.73) | 179.87<br>(67.90)        |
| <i>Change in trend post-2002</i>     | 15.55<br>(6.54)   | 12.95<br>(7.01)          |
| <i>2000 effect</i>                   |                   | -4.47<br>(2.53)          |
| <i>Change in trend<br/>2000-2002</i> |                   | 2.48<br>(0.88)           |
| Number of observations               | 30                | 30                       |

*Notes:* The figure displays the regression results based on equation (2). The model is estimated using "Interrupted time series analysis" and Newey-West standard errors with one lag are used. Individuals belonging to following type of couples, that we consider treated, are included: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark) and the other in a non-EU country, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark.



**Table A.6.** *Interrupted time series analysis results: with comparison group*

| Yearly number of partners             | Unaffected controls<br>2002 treatment | FI/NO controls<br>2002 treatment | Unaffected controls<br>2000 and 2002 treatments |
|---------------------------------------|---------------------------------------|----------------------------------|---|
| <i>Baseline mean level difference</i> | 3.70<br>(4.23)                        | -7.08<br>(3.96)                  | -2.45<br>(3.81)                                 |
| <i>Baseline mean slope difference</i> | -1.56<br>(0.53)                       | -1.58<br>(0.48)                  | -0.18<br>(0.54)                                 |
| <i>2002 effect</i>                    | 125.44<br>(68.11)                     | 191.54<br>(66.06)                | 122.07<br>(71.76)                               |
| <i>Change in trend post-2002</i>      | 16.80<br>(6.86)                       | 17.17<br>(6.59)                  | 11.24<br>(8.75)                                 |
| <i>2000 effect</i>                    |                                       |                                  | -23.70<br>(6.07)                                |
| <i>Change in trend<br/>2000-2002</i>  |                                       |                                  | 4.18<br>(5.47)                                  |
| Number of observations                | 60                                    | 60                               | 60  |

*Notes:* The figure graphically displays the regression results based on equation (3). The model is estimated using "Interrupted time series analysis" and Newey-West standard errors with one lag are used. Individuals belonging to following type of couples are considered treated: i) couples where one partner is born in Denmark (and has moved to Sweden from Denmark) and the other in a non-EU country, and ii) couples where both partners are born in a non-EU country with at most one of them migrating from Denmark. The control group consists of couples where *both* partners immigrate from Denmark to Sweden as couples who migrate to Sweden for reasons *unrelated* to the possibility of family reunification.



### 3. The Intergenerational Mobility of Immigrants and the Native-Born: Evidence from Sweden

with Valentin Bolotnyy

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### 3.1 Introduction

Immigration is an intergenerational process, often driven by parental desire to ensure a better life for subsequent generations and resulting in demographic changes in the host country that play out over numerous generations. In order to construct optimal immigration policy, it is thus important to consider the effects of multiple generations of immigrants on the host country and the effects of the host country on those multiple generations. We focus on the latter in this paper, documenting how immigrant children compare to native-born counterparts and demonstrating heterogeneities in the way immigrant children integrate into a new society. We primarily use income and educational attainment as measures of integration, but we also look at employment outcomes. Understanding these aspects of the immigration process is especially important today, with the world facing over 65 million displaced persons, the largest number on record (United Nations High Commissioner for Refugees 2016).

We might expect intergenerational mobility to be lower for immigrants than for natives if culture, language barriers, or traumatic origin-country experiences impede a child's ability to obtain a good-paying job or an education. It is also possible that state resources or other forms of social support crucial for intergenerational mobility are more easily accessible for natives than for immigrants. Alternatively, if familial characteristics or domestic investment in the child are especially important for intergenerational mobility, it might be that immigrants, many of whom are fleeing their home country in search of a better future for their children, are highly positively selected on exactly the characteristics that produce higher intergenerational mobility.

Our work investigates the net effect of these forces. We look at how the immigrant experience differs from that of natives using longitudinal data from Sweden. This data allows us to link parents to children over time and follow the children's income and education trajectories. We focus on immigrant children that are born abroad to foreign-born parents and arrive in Sweden before the age of 16.<sup>1</sup> By studying this group, we set ourselves apart from existing studies on the intergenerational mobility of immigrants, which look at children who are born in the host country to foreign-born parents (see Hammarstedt and Palme 2012, Niknami 2016, and Hermansen 2016, among others). Doing so allows us to work with a sample that more closely resembles the recent refugee waves. Moreover, we are able to see how children who do not spend a significant portion of childhood in Sweden fare compared to those who, along with their parents, are born there. As a country that has for decades been accepting large numbers of refugees, family migrants, and workers from all over the world, Sweden provides a useful setting for our analyses. Additionally, our work expands on the existing literature by administratively linking immigrant parents with children and separating out refugees from non-refugees.

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<sup>1</sup>These are often called the 1.5 generation in the immigration literature (Sweetman and van Ours 2015).

We start by documenting striking similarities in income and educational outcomes between immigrant and native children. We next zoom in on the immigrant group and find that refugee children from countries like Bosnia, Syria, and Iran have higher intergenerational mobility than the average child immigrant. While immigrant parents from these countries on average find themselves with lower incomes than those from other countries, their children show some of the highest levels of income in adulthood among all immigrant children. Still, we find substantial heterogeneity in intergenerational mobility across predominantly-refugee sending countries, revealing the importance of further research to try to understand the mechanisms behind these differences.

Our work stands on the shoulders of an active literature on the intergenerational mobility of immigrants. Focusing on male immigrants who arrive in Sweden before 1970 and their Swedish-born sons, Hammarstedt and Palme (2012) show that the absolute income of these children converges to that of the children of native Swedish fathers. In our sample of immigrants who arrive in Sweden between 1974 and 1999, 21% of children have information on only their mothers, suggesting that looking at parents and children of both genders is important to get the full intergenerational mobility picture. Furthermore, immigration to Sweden changed character quite dramatically in the early 1970s with waves consisting primarily of refugees and family migrants, as opposed to labor immigrants. We focus on immigrant children born outside of Sweden instead of the second generation, with the goal of seeing how those who spend only a portion of their childhood in Sweden do compared to native Swedish children. We measure a child's income when he or she is 30 years old, whereas Hammarstedt and Palme (2012) measure child income in 1997-1999 at ages that range from 20 to 64. Given how variable incomes are across those ages, we argue that our strategy provides a more stable measure of income in adulthood.<sup>2</sup> Similar to us, however, the authors find heterogeneities in income convergence, with children from Turkey, Greece, the Middle East, and Africa displaying the highest earnings gaps relative to natives.

Relatedly, Niknami (2016) looks at how the educational attainment of immigrant and native girls born in Sweden between 1960 and 1980 differs from the educational attainment of their mothers. She finds higher educational intergenerational mobility for girls born to immigrant mothers. The paper complements earlier work by Borjas (1992), Borjas (1993), Card et al. (2000), and Aydemir et al. (2009), who study the relationship between immigrant father earnings and child earnings. They conclude, among other things, that sons of immigrants have earnings in adulthood that closely resemble their father's earnings. In contrast to most of these studies, we do not restrict our focus to fathers and sons and we do not rely on a grouped data estimator since we can link children to their parents.

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<sup>2</sup>We also check the robustness of our results using later ages.

In the Norwegian context, Hermansen (2016) finds evidence of convergence of immigrant children to their native counterparts in terms of absolute income and education. Like us, he sees immigrant children of several non-European ethnic minorities achieve higher educational attainment and earnings than their native counterparts with similar parental socioeconomic backgrounds. Hermansen (2016)'s sample includes children born to foreign-born parents who were either born in Norway or who came to Norway before the school-starting age. Given prior work that shows children moving at earlier ages with higher incomes and education levels in adulthood (see Van den Berg et al. 2014 and Chetty et al. 2016), we also include children arriving in their teenage years in our sample to ensure a representative picture of immigrant intergenerational mobility.

In the next section we present Sweden's immigration history since World War II and describe how we selected the data and variables for our analyses. Section 3.3 dives into the main results, showing how immigrant intergenerational mobility compares to native intergenerational mobility and discussing potential sources of measurement error. Section 3.4 shows how immigrant intergenerational mobility differs across countries of origin. Section 3.5 discusses whether the patterns we observe in Section 3.3 can be explained by other family-level background characteristics. Finally, Section 3.6 concludes.

## 3.2 Background and data

### 3.2.1 Immigrants in Sweden

Sweden has for decades been a destination for large numbers of immigrants with widely different backgrounds. Since World War II, when Sweden became a net immigrant-receiving country, numerous immigration waves have occurred. The 1950s and 1960s were dominated by labor immigration, primarily from other Nordic countries like Finland, but also from Mediterranean countries like Greece, Italy, and Yugoslavia (Hammarstedt and Palme 2012).

Labor immigration from non-Nordic countries came to a halt in the early 1970s, but immigration continued in the form of family reunification and refugee immigration.<sup>3</sup> Refugees from Chile arrived predominantly in the 1970s; from Iran, Iraq, and Lebanon in the 1980s; from Somalia, Eritrea, and Former Yugoslavia in the 1990s. The timing of refugee arrivals has mirrored the timing of conflicts around the world. Given the volume of these refugee waves, 1970 marked a shift in Sweden towards mostly non-European immigration. Our sample, which consists of immigrants who arrived in Sweden between 1974 and 1999, shows 76% of foreign-born children with at least one refugee parent. As of 2016, about 17% of the Swedish population was foreign-born,

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<sup>3</sup>Nordic labor immigration continued, primarily from Finland, as the 1954 Nordic Agreement allowed free movement for citizens of the Nordic countries.

compared to less than 7% in 1970. By comparison, the share of foreign-born in the United States was at about 13% in 2013 (OECD 2017).

### 3.2.2 Sample Selection

We use Swedish register data from the GeoSweden database, which covers all individuals with a permanent residence permit valid for at least one year for the 1990-2014 period.<sup>4</sup> The data contains variables from several different registers, including the education, income, and employment registers. Parent identifiers for each individual are available, provided the parents have also registered in Sweden (either as a resident or as a citizen) at some point between 1990 and 2014.

In order to construct our sample, we first identify all parents of children born in the 1974-1984 cohorts for whom we have information in the population and employment registers. We then link them to their children, provided these can be found in the population and employment registers when they are 30 years old. For immigrant children, we follow Van den Berg et al. (2014) and impose the restriction that they arrive before the age of 16.

We focus on two groups: the native children in our analysis are children born in Sweden to Swedish-born parents. The immigrant children are born abroad to foreign-born parents. This implies that we exclude children born in Sweden to immigrant parents, children born abroad to Swedish parents, and children born to one Swedish parent and one foreign parent, regardless of the place of birth. Our sample restriction allows us to focus on those immigrant children for whom integration would likely be hardest. This, in turn, likely makes our results lower bounds for the entire population of immigrant children in Sweden.

### 3.2.3 Key Variables

We calculate *family income* as the average combined income of the parents in the register during the years when the child is 15 to 19 years old.<sup>5</sup> We include families with zero income. We follow Chetty et al. (2014) and define the family's *percentile rank* based on its position in the *national* distribution of incomes relative to all parents with children in the same birth cohort, regardless of immigrant status.

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<sup>4</sup>GeoSweden is administered by the Institute for Housing and Urban Research at Uppsala University. The data is collected and anonymized by Statistics Sweden.

<sup>5</sup>Our income variable includes income from employment and self-employment. Using instead only labor income gives similar results (not included). When the child has only one parent in the register, we measure family income as the average income of the existing parent during the years when the child is 15 to 19 years old. For the 1974 cohort, we measure family income when the child is between 16 and 20, because our income data start in 1990.

We measure *child income* as the individual income the child earns at age 30. Just as for the parents, we define the child's percentile rank based on his or her position in the national distribution of incomes relative to all children in the same birth cohort.

Both income variables are measured in 2014 SEK, adjusting for inflation using Statistics Sweden's Consumer Price Index.

We define *parental education* as the maximum level of education observed throughout the time the parent is in the register, so as to reduce the number of missing values for immigrant parents in their first years in Sweden.<sup>6</sup> We categorize families based on whether neither or at least one parent has a college degree or above.<sup>7</sup> In our data, this corresponds to having at least a post-secondary education that takes fewer than 3 years to complete.<sup>8</sup>

Similarly, we measure whether the child has a *college degree or above* at age 30.

A parent is a *refugee* if the *first* reason for settlement in Sweden is recorded as such. Sweden grants asylum to people classified as refugees in accordance with the Geneva Convention and also to those considered to be "in need of subsidiary protection" according to European Union regulations.<sup>9</sup>

We show summary statistics for native and immigrant children in Table 1. We have information on both parents for 97% of native children in our data.<sup>10</sup> Only 75% of the immigrant children in our sample have both parents in the register. The majority of those that have only one parent in the register are in Sweden with their mothers. The most likely reason a parent is missing from the register is that this parent lives abroad. Additionally, a parent could be missing in the register if he or she is deceased, has only a temporary residence permit - which allows for less than one year of residence in Sweden - or is somehow not registered at all.

On average, immigrants (Panel B) grow up in families that earn less than 35% of what native families earn. Yet, as adults, immigrant children earn

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<sup>6</sup>Immigrant parents might see their skills and degrees obtained abroad recognized some time after arrival.

<sup>7</sup>We do so only for families where both parents have non-missing education information when both parents are in the register (or where the one existing parent has non-missing information when only one parent is in the register). However, if we assign families the level of education from just one parent when only one parent has non-missing information, the average share of families with college or above changes only slightly, from 42.92% to 42.89% for natives and from 33.8% to 33.11% for immigrants.

<sup>8</sup>The equivalent in the United States would be an associate's degree.

<sup>9</sup>We have information on residence permits for only 83% of the immigrant sample (see Table 1).

<sup>10</sup>We restrict our attention to whether parents are present in the register during the period in which we are interested in measuring parental outcomes - when the child is between 15 and 19 years old. This means that we include children who either had only one parent or both parents in the register *throughout the entire 5-year period*. A further implication is that we are not capturing those children whose parents migrate in and out of Sweden during that time.



**Table 1.** *Summary statistics*

|   | Mean    | Std. dev. | No. of obs. |
|---|---------|-----------|-------------|
| <b>Panel A: Natives</b>                     |         |           |             |
| Parent family income                        | 455.43  | 243.31    | 819,422     |
| Parent percentile income rank               | 53.72   | 27.45     | 819,422     |
| Age mother when child 15-19                 | 44.57   | 4.88      | 814,610     |
| Age father when child 15-19                 | 47.17   | 5.35      | 800,860     |
| At least one parent with college or above   | 42.92   | n/a       | 818,014     |
| Both parents in the register                | 97.15   | n/a       | 819,422     |
| Only mother in the register                 | 2.27    | n/a       | 819,422     |
| Child individual income                     | 236.26  | 157.72    | 819,422     |
| Child percentile income rank                | 50.75   | 29.15     | 819,422     |
| Child has college or above                  | 48.20   | n/a       | 814,931     |
| Employed                                    | 92.26   | n/a       | 819,422     |
| At least one child                          | 43.48   | n/a       | 819,422     |
| Age at first child, men                     | 27.05   | 2.58      | 145,045     |
| Age at first child, women                   | 26.06   | 3.20      | 211,239     |
| Number of unique mothers                    | 543,430 |           |             |
| Number of unique fathers                    | 534,200 |           |             |
| <b>Panel B: Immigrants</b>                  |         |           |             |
| Parent family income                        | 153.38  | 177.00    | 52,772      |
| Parent percentile income rank               | 15.55   | 19.74     | 52,772      |
| Age mother when child 15-19                 | 42.11   | 5.34      | 50,943      |
| Age father when child 15-19                 | 46.29   | 6.26      | 41,552      |
| Mother years since arrival when child 15-19 | 8.35    | 4.08      | 50,027      |
| Father years since arrival when child 15-19 | 8.67    | 4.53      | 40,598      |
| At least one parent with college or above   | 33.80   | n/a       | 50,662      |
| Both parents in the register                | 75.27   | n/a       | 52,772      |
| Only mother in the register                 | 21.26   | n/a       | 52,772      |
| At least one parent refugee                 | 76.37   | n/a       | 43,983      |
| Child individual income                     | 191.07  | 162.09    | 52,772      |
| Child percentile income rank                | 40.82   | 30.77     | 52,772      |
| Child has college or above                  | 37.35   | n/a       | 52,336      |
| Employed                                    | 82.30   | n/a       | 52,772      |
| Average age at arrival                      | 8.99    | 4.04      | 52,772      |
| At least one child                          | 44.03   | n/a       | 52,772      |
| Age at first child, men                     | 26.37   | 2.91      | 9,660       |
| Age at first child, women                   | 24.67   | 3.50      | 13,574      |
| Number of unique mothers                    | 35,092  |           |             |
| Number of unique fathers                    | 27,515  |           |             |

*Notes:* This table reports summary statistics for natives and immigrants, respectively. Children are born between 1974 and 1984. Income is in thousands of 2014 SEK. Child income is individual income measured when the child is 30 years old. Parent family income is the combined income of the parents during the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. A college degree corresponds to having at least a post-secondary education that takes fewer than 3 years to complete. Employment includes self-employment. We classify a child as a refugee if at least one of his or her parents is classified as a refugee in our data. Where standard deviations are not reported, the Mean column shows shares.

about 80% of what native children earn. The average native parents and children are more likely to have college or above levels of education than the average immigrant parents and children, respectively.

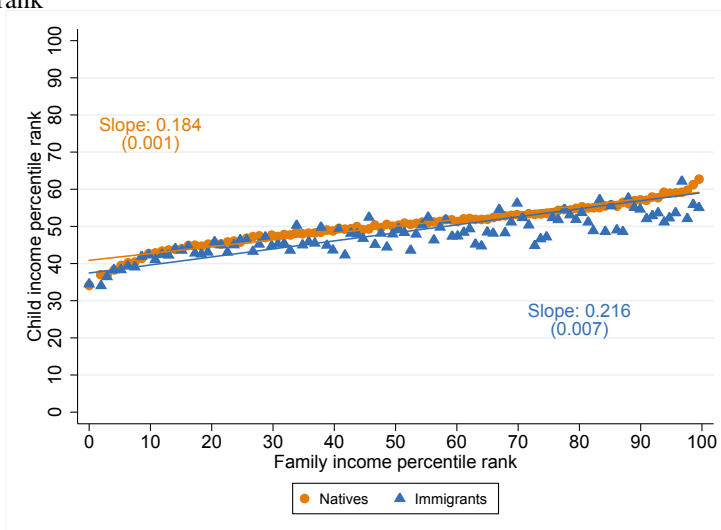
### 3.3 Results

#### 3.3.1 Immigrants vs. Native-born

To better understand how immigrants integrate into Swedish society, we turn our attention to intergenerational mobility. We measure the extent of integration by comparing the outcomes of immigrant children to the outcomes of native-born children from the same birth cohorts and the same family income.

Figure 1 plots child income ranks against parent income ranks, revealing a slightly higher intergenerational mobility for natives than for immigrants.

*Figure 1.* Average child income percentile rank, conditional on family income percentile rank



*Notes:* The figure plots the percentile income rank of children in the 1974-1984 birth cohorts at age 30 against the percentile rank of their parents for natives and immigrants, respectively. Child income is individual income at age 30. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

The rank-rank slope for immigrants is a little steeper, at about 0.22, than the 0.18 slope for natives. The ranks of the native and immigrant children born into the bottom of the income distribution are very similar, with differences

arising as we move up the parental income distribution in part because the number of immigrant parents decreases.<sup>11</sup>

These results, however, do not fully capture what is happening at the extremes. Looking at a child's probability of ending up in the top income quintile in Figure 2a, we can see that when the parents are in the first half of the income distribution, immigrant children have slightly higher probabilities than native children. At the same time, they are also more likely to end up in the bottom income quintile (Figure 2b), even if they start at high family income levels. This higher likelihood of regression to the bottom of the income distribution echoes findings in Chetty et al. (2018) where the authors look at the United States and find that black children born into high-income families are more likely to fall back into the bottom income quintile than white children.<sup>12</sup>

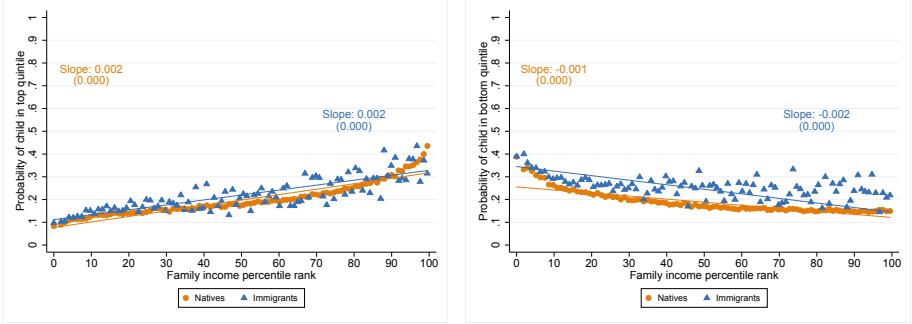
Turning to educational attainment, we can see in Figure 3 that immigrant children are considerably more likely than native children to complete college, especially at the lower parts of the parental income distribution. Our data also shows that the share of parents with college degrees at the bottom of the income distribution is higher for immigrant parents than it is for native parents, by somewhere between a few percentage points for the children born in the mid-1970s and as many as 15 percentage points for children born in the mid-1980s. Taken together, these findings suggest a strong familial transmission mechanism of the importance of education that is separate from family income. We see further evidence of this when we condition on parental educational characteristics instead of income characteristics in Figure A.2 and find that immigrant children and native children look similar. Children born into families where neither parent has a college education have about a 35% probability of obtaining a college education themselves, whether they are immigrants or natives. When only their mother has a college degree, that probability rises to about 60% for both groups. The largest gaps in college attainment between immigrants and natives occur when only the father has a college degree (50% for immigrants vs. 60% for natives) and when both parents are college-educated (70% for immigrants vs. 80% for natives).

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<sup>11</sup>Since we later discuss how the immigrant-native income gap varies by gender, it is worth pointing out here that both the slope and intercept we estimate for immigrant women are similar to those we estimate for native women. In contrast, immigrant men are both less mobile and do worse than native men on an absolute level. These results are available upon request.

<sup>12</sup>Importantly, as Figure A.1 shows, parental education levels cannot fully explain what is happening at the extremes. The gap between native and immigrant child income percentile ranks is virtually constant across parental education levels, suggesting that other factors are driving immigrant children to be concentrated at the extremes.

Figure 2. Average child outcomes, conditional on family income percentile rank

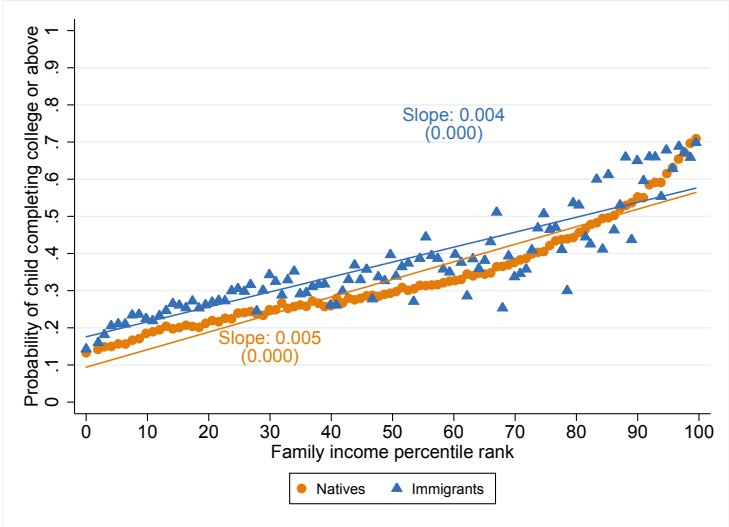


(a) Top income quintile

(b) Bottom income quintile

Notes: Figure 2a (2b) plots the probability of reaching the top (bottom) 20% in the income distribution for children in the same birth cohort, against the percentile income rank of their parents. Probabilities are shown for natives and immigrants. Children are born between 1974 and 1984. Child income is individual income at age 30. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

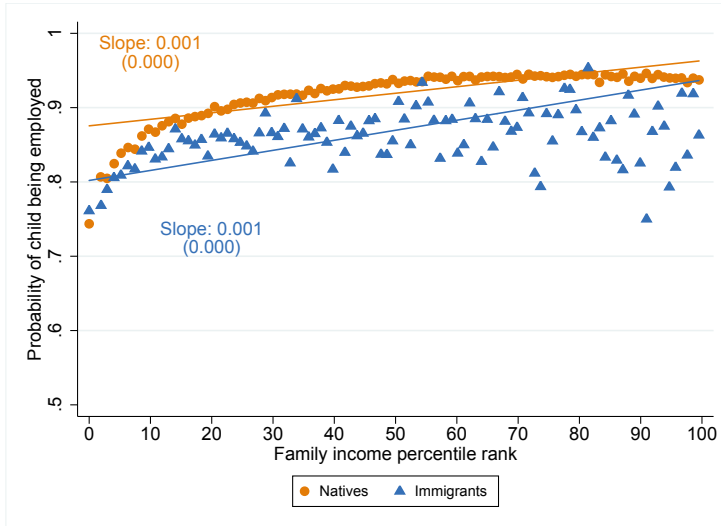
Figure 3. Average share of children obtaining college or above education conditional on family income percentile rank



Notes: The figure plots the probability of children having completed a college degree or above by age 30, against the percentile income rank of their parents. Probabilities are shown for natives and immigrants. Children are born between 1974 and 1984. A college degree corresponds to having at least a post-secondary education that takes fewer than 3 years to complete. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

Notwithstanding the fact that immigrant children, and especially those born to lower-income parents, are more likely to complete university education, they are slightly less likely to be employed than natives.<sup>13</sup> As Figure 4 shows, employment rates are high for both groups, but they are higher for natives across the parental income distribution. These patterns could be indicative of discrimination in the labor market.<sup>14</sup>

*Figure 4.* Average share of children who are employed conditional on family income percentile rank



*Notes:* The figure plots the probability of children being employed at age 30, against the percentile income rank of their parents. Probabilities are shown for natives and immigrants. Children are born between 1974 and 1984. Employment includes self-employment. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

Overall, however, whether the outcome of interest is income in adulthood or educational attainment, children of immigrants on average perform similarly or even better than children of natives when we condition on parental income (or, as we show in Figures A.1 and A.2, on parental education). On average, it seems that forces like cultural differences or language barriers or differential access to services, which might be hurting intergenerational mobility for immigrant children, do not outweigh the forces that immigrant parents bring with them to help propel their children upward.

<sup>13</sup>Our measure of employment includes self-employment.

<sup>14</sup>Using a correspondence testing design, Carlsson and Rooth (2007) find that job applicants with Middle Eastern names are significantly less likely to receive callbacks than identically skilled applicants with Swedish names. A significant fraction of the immigrants in our sample originate from Middle Eastern countries.

### 3.3.2 Robustness of baseline estimates

We now document whether our results are driven by measurement error. In what follows, we discuss sources of measurement error in both the dependent and independent variables and show that our results are robust to alternative specifications. We focus exclusively on the intergenerational income mobility estimates.

#### **Measurement error in the parental income measure**

*Life-cycle bias.* The intergenerational mobility coefficient we wish to estimate should reflect the correlation between the child's and parents' *lifetime* income. In order to do so, we need to make sure that parents are not very old or very young when we measure their income. As Table 1 shows, native (immigrant) mothers are about 44.6 (42.1) years old on average, whereas native (immigrant) fathers are slightly older, about 47.2 (46.3) years old. These averages are very similar to those of parents in the sample used in, e.g. Chetty et al. (2014).<sup>15</sup> However, given that among our sample of immigrants there are children who arrive as late as at age 15, starting to measure parental income when the child is 15 is the lowest age we *can* use in order to have a consistent measure between the two groups.

*Bias due to immigrant parents' low earnings upon arrival.* Given that immigrant children arrive before the age of 16 and we calculate parents' income when the child is between 15 and 19, there is significant variation in the amount of time immigrant parents have to enter the labor market before we measure their income. This likely contributes to the low incomes we observe for immigrant families. On average, however, we see in Table 1 that immigrant parents have been in Sweden for about eight years. Nevertheless, to understand if our estimates are sensitive to different measures of parental income, we do the following exercise. For immigrant parents, instead of measuring income when the child is between 15 and 19, we do so when the parents have been in Sweden for 10 to 14 years. We choose this time window as various reports have documented that a significant share of the immigrant population - and in particular refugees - are in employment ten years after arrival (see, e.g. Bevelander 2011 who shows that refugees, resettled refugees and family reunification immigrants who arrive after 1987 have employment rates between 60 and 70% 11-15 years after arrival). We have performed similar exercises looking also at the 15-19 and 20-24 years since arrival time windows, respectively. However, we run into the issue of large number of zero-income observations due to retirement. Therefore, by choosing the 10-14 time window, we give

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<sup>15</sup>Note that Chetty et al. (2014) report average parental ages during the first year in the period over which they calculate parental income. For us, the equivalent would be reporting average ages when the child is 15, in which case the average is 42.6 (40.1) for native (immigrant) mothers and 45.2 (44.3) for native (immigrant) fathers, respectively.

immigrant parents enough time to enter the labor market and we maintain an average parental age that is far enough from the retirement age to give us a reasonable estimate of lifetime income.<sup>16</sup> During this period, mothers are on average 45.9 years old and fathers are on average 49.8 years old. On average, the immigrant parents' income we observe during this period is indeed higher than in the baseline, at 201.13 (thousand) compared to 153.38. We show the new rank-rank plot in Figure A.3 in the Appendix. The rank-rank coefficient is lower than when using the baseline measure, but falls within the baseline estimate's confidence interval.<sup>17</sup> Hence, although we might be worried that using parents' income early with respect to their year of arrival might bias our results, addressing this issue by recalculating income after parents have spent a significant number of years in Sweden does not affect the results significantly.

*Bias due to parents being absent from the register.* We have seen in Table 1 that for 25% of immigrant children, we can only find one parent in the register. Since we calculate income at the family level, this means that immigrant parents will have a lower income rank by virtue of there not being two incomes that make up the family income. We test whether our results are driven by missing parents in the register by calculating average *parental* income when the child is between 15 and 19. With this method, we essentially get parental incomes for those with both parents in the register that are half as large as in the baseline. The parental income of those with only one parent in the register remains unchanged. The resulting rank-rank plot is shown in Figure A.4. For natives, the estimate barely changes, which isn't surprising given that few natives have only one parent in the register. For immigrants, the rank-rank slope goes down with respect to the baseline and it is very close to that of natives. We conclude that immigrants are at most slightly less mobile than natives but may even be at least as mobile.

### **Measurement error in the child income measure**

*Life-cycle bias.* As discussed earlier, results may be biased if incomes are measured too early or too late in life, as they will not accurately reflect lifetime incomes. Nybom and Stuhler (2016) have shown that for Swedish men

<sup>16</sup>We are able to calculate parental income during this time window for 97.7% of children in the immigrant sample. Note that ideally, we would observe income over the entire five-year period, for both parents (ten observations). In reality, we have on average 8.3 observations per family. The main reason is that our income data begins in 1990, hence the theoretically available number of observations starts declining for parents arriving in 1979. Furthermore, there may be return migration during this time window. It is often also the case that parents do not arrive during the same year, in which case the number of available observations might differ between parents. Therefore, in order to conduct this exercise we calculate average income over the period for each parent separately, so as to correctly account for the number of yearly income observations, and then we sum over the two averages.

<sup>17</sup>Adding dummies for the number of observations used to calculate income during the 10-14 time window does not alter the results. They are available upon request.

born between 1955 and 1957, a three-year average around the age of 33 gives an income measure that is highly correlated with the average of annual life-time income. Figure A.5 shows that calculating the income rank using the average annual income when children are between 32 and 34 does not significantly change our results: the rank-rank slope is now 0.19 for natives and 0.21 for immigrants.<sup>18</sup> Since our data ends in 2014, the exercise means that we are able to include only cohorts between 1974 and 1980. Nevertheless, these results show that prioritizing including more cohorts over observing their incomes at slightly later ages does not compromise the validity of our results.

*Bias due to parental leave.* We might be concerned that we are measuring income around an age when individuals have their first children. Sweden has a generous parental leave system that means that parents of small children, and in particular women, are on leave during the first one to two years of their child's life, which means that we would observe lower incomes for them than we would otherwise. As Table 1 shows, however, less than half of the sample - native or immigrant - have children by the time they are 30.<sup>19</sup> Furthermore, of those that have children, the average age at first child is around 25 (26) for immigrant women (men) and 26 (27) for native women (men). Hence, fertility patterns are very similar between groups. What may differ, however, is the propensity of taking parental leave in the first place, as well as the time spent on parental leave. Our results should be interpreted with this caveat in mind.

### 3.4 Country of origin differences

The similarities in intergenerational mobility that we uncover between native and immigrant children do, however, mask substantial heterogeneity in immigrants' later-life outcomes. Focusing on income, one such dimension along which we can see differences in later-life outcomes is country of origin.

Each circle in Figure 5 represents a different country of origin, with each circle radius equal to the square root of the number of children coming from each country. The y-axis captures the mean child income rank and the x-axis represents the mean parent income rank, both at the country level. The regression line and the estimated slope do not include native-born children,

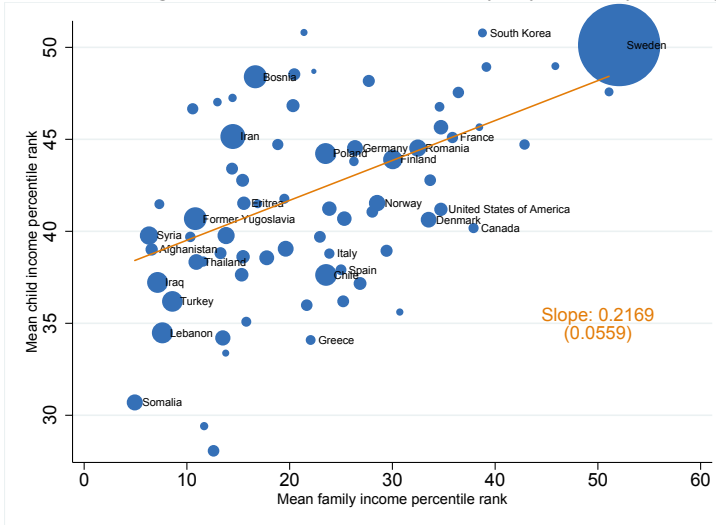
<sup>18</sup>This result is not surprising since the correlation between a child's income rank at age 30 and the average income rank at ages 32-34 is 0.7.

<sup>19</sup>Both native and immigrant women are more likely to have children by the time they are 30 than men, 53% and 55%, respectively, compared to a virtually identical share of 34% of men in both groups.



though we do include a circle for Sweden here for perspective. We label the countries representing our largest immigrant groups and some of the outliers.<sup>20</sup>

*Figure 5. Intergenerational income mobility, by country of origin*



*Notes:* The figure plots the mean child income percentile rank against the mean family income rank, for each country of origin. Child income is individual income at age 30. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. Each circle represents a different country of origin, with each circle radius equal to the square root of the number of children from each country. We include a circle for Swedish children as a point of reference, but the observation is not included in the regression. The slope is estimated using weighted OLS. Standard error in parentheses.

Most of the refugee-sending countries of origin are on the far-left of Figure 5, with parents on average starting off in the very bottom ranks of the income distribution. Though they start off at about the same point in the distribution, children from Somalia, Lebanon, Turkey, Iraq, Afghanistan, and Syria have mean income percentile ranks in adulthood that range from 30 to 40. Children from Iran, Bosnia, Former Yugoslavia, and Syria, countries whose vast majority of immigrant children are refugees (see Figure A.6), all have higher intergenerational mobility than the average intergenerational mobility across all immigrant groups.<sup>21</sup>

In contrast, though most of the children from Chile are refugees as well, their parents start off at about the same position in the income distribution as parents of Norwegian children and their intergenerational mobility is below

<sup>20</sup>Note that if we exclude the countries with fewer than 30 immigrant children in our sample, the estimated slope becomes 0.261 (standard deviation 0.075).

<sup>21</sup>Though Bosnia is also a former Yugoslavian country, it is labeled separately in our data. We maintain that separate labeling here.

the average across immigrant groups. Thus, not all refugees are the same, and some integrate into Swedish society better than others. We find similar heterogeneities by country of origin when we look at the probability of reaching the top quintile, the probability of ending up in the bottom quintile, and the probability of completing college or above (see Figures A.7 - A.9).

### 3.5 Immigrant-native intergenerational gaps

A different way to summarize our findings so far is to look at immigrant-native intergenerational gaps and understand what generates them. In particular, we focus on parental education, country of origin and parental wealth. We follow Chetty et al. (2018) who perform this exercise to study intergenerational gaps between black and white men in the United States and estimate regressions of the following type:

$$y_{ic} = \alpha + \beta_p y_{ip} + \beta_{im} \text{immigrant}_i + \beta_{imp} \text{immigrant}_i \times y_{ip} + \gamma X_i + \varepsilon_i \quad (3.1)$$

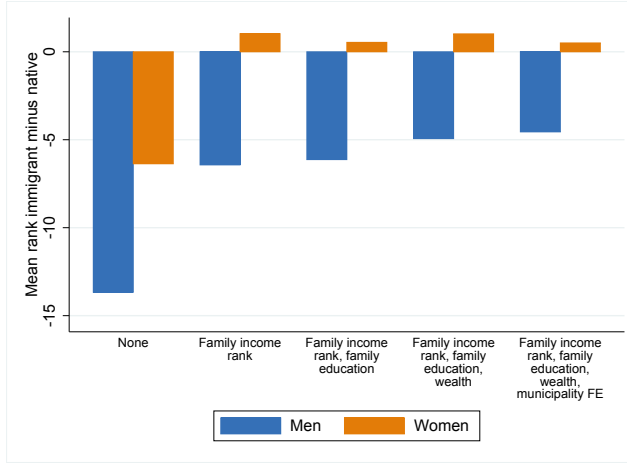
where  $y_{ic}$  is the child's income rank,  $y_{ip}$  is the family income rank,  $\text{immigrant}_i$  is a dummy that indicates immigrant status and  $X_i$  is a covariate. We are interested in the intergenerational gap in income at a given parental income rank  $\bar{p}$  and how it changes with  $X_i$ , given by  $\beta_{im} + \beta_{imp}\bar{p}$ .

Figure 6 shows our results. Panel (a) plots the estimates for  $\bar{p} = 25$  and Panel (b) for  $\bar{p} = 75$ . We run separate regressions by gender. The first group of bars in each panel shows the unconditional immigrant-native gap, which, for  $\bar{p} = 25$ , is -13.7 for men and -6.4 for women. Family income rank goes a long way in explaining this gap, as the second group of bars shows. The gap roughly halves for men and entirely disappears for immigrant women, who have a higher income rank than native women once family income rank is accounted for. Family education does not significantly alter the gap. We next control for parental wealth, as proxied by homeownership during the period when the child is between 15 and 19.<sup>22</sup> Again, the gaps do not change significantly. As a last explanation, we check how controlling for the municipality of residence affects the income gap. We define municipality of residence as the municipality where the child has spent the most time between the ages of 15 to 19.<sup>23</sup> Perhaps due to the fact that it is a broad measure of residential location, the results change little with the addition of this variable.

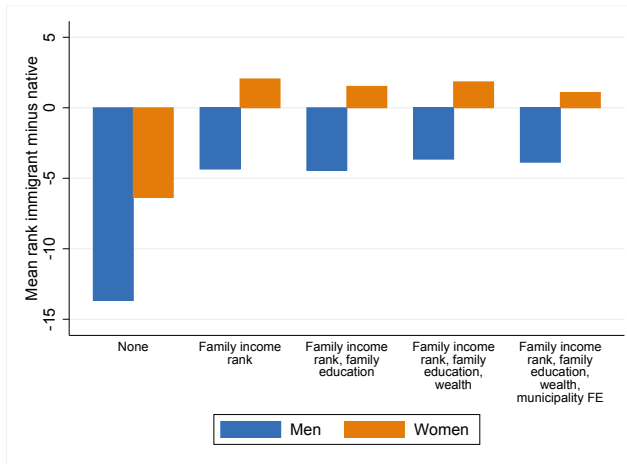
<sup>22</sup>We consider parents as homeowners if they lived in owned housing throughout most of the period when the child is between 15 and 19.

<sup>23</sup>To be more precise, we use the municipality where the mother resided during the relevant period and when the mother can be found in the register, and the father's municipality of residence otherwise.

Figure 6. Intergenerational income gaps



(a) Children with parents at the 25th percentile



(b) Children with parents at the 75th percentile

Notes: The figure shows how the immigrant-native income gap changes with family-level controls. Panel (a) shows estimates for  $\bar{p} = 25$  and Panel (b) for  $\bar{p} = 75$ . In each panel, the blue bars show estimates for men and the orange bars for women. The first group of bars shows the unconditional immigrant-native income gap. The next group shows the unconditional gap at  $\bar{p}$ . The third, fourth and fifth group show how the gap at  $\bar{p}$  changes as we add, respectively, family education, parental wealth as proxied by homeownership and municipality fixed effects.

To sum up, parental background explains more than half of the immigrant-native income gap for men born in families at the 25th and 75th percentile, respectively. For immigrant women, once we control for family characteristics, we find that they have higher income ranks than native women.

We further want to examine the immigrant-native income gap for children born in families at percentile rank  $\bar{p}$  and education level  $\bar{e}$ .<sup>24</sup> We therefore estimate the following equation:

$$y_{ic} = \sigma + \phi_p y_{ip} + \phi_{im} \text{immigrant}_i + \phi_{imp} \text{immigrant}_i \times y_{ip} + \phi_{ime} \text{immigrant}_i \times e_{ip} + \theta X_i + v_i \quad (3.2)$$

We summarize our results in Figures A.10 and A.11. In each panel of each of the figures, the first set of bars corresponds to the gap defined by  $\phi_{im} + \phi_{imp}\bar{p} + \phi_{ime}\bar{e}$ . The upper panel in each of the figures evaluates the gap at  $\bar{e} = 0$  and the lower panel at  $\bar{e} = 1$ . The subsequent sets of bars add our proxy for wealth and municipality of residence fixed effects, respectively. There are a few takeaways from these figures. First, if we focus on children born in families at the 25th percentile, we see that immigrant women do at least as well as native women regardless of their parents' education and they do especially well when their parents are highly-educated. Immigrant men from high-educated families do better than immigrant men from low-educated families but they do worse than natives in both cases. We observe similar patterns for children born in families at the 75th percentile. Together, these figures suggest that immigrants with college-educated parents, and in particular women, do better than immigrants whose parents do not have a high level of education, regardless of whether the parents are in the bottom or the top of the distribution. Given that immigrant parents at the 25th percentile are on average more likely to have a university degree than similar native parents, these patterns are to an extent the result of positively selected immigrant parents relative to native parents.

### 3.6 Conclusion

We use administrative Swedish data to document that, conditional on parent income, immigrant children have comparable incomes to their native-born counterparts. Digging deeper into the conditional expectation, we reveal that immigrant children born into poor families are slightly more likely than native children born into poor families to reach the very top of the income distribution. They are also considerably more likely to obtain a college degree. At the same time, immigrant children are also more likely than native children to stay at the very bottom of the income distribution or to regress from middle and high family incomes to the very bottom.

We additionally show that substantial heterogeneities in later-life child outcomes exist depending on the country of origin. Children from predominantly-refugee sending countries like Bosnia, Syria, and Iran have higher incomes and

<sup>24</sup>In our case, this means looking at children born in families at percentile rank  $\bar{p}$  with or without parents with a university degree.

higher intergenerational mobility than the average child immigrant to Sweden. Further research is needed to understand what helps the average immigrant child born in families at the bottom of the income distribution do as well as native children, why immigrant children who arrive at middle and high family incomes are more likely than native children to fall back to the economic bottom, and why some refugee children integrate better into Swedish society than other immigrant children.

Finally, we look at immigrant-native income gaps for children born in families at the bottom and top 25th percentile of the distribution, respectively and find that once family income rank is accounted for, the gap shrinks little as we add other family background controls. This exercise also reveals that immigrant women do better in terms of income relative to native women. The gender differences we uncover warrant further research into what generates them.

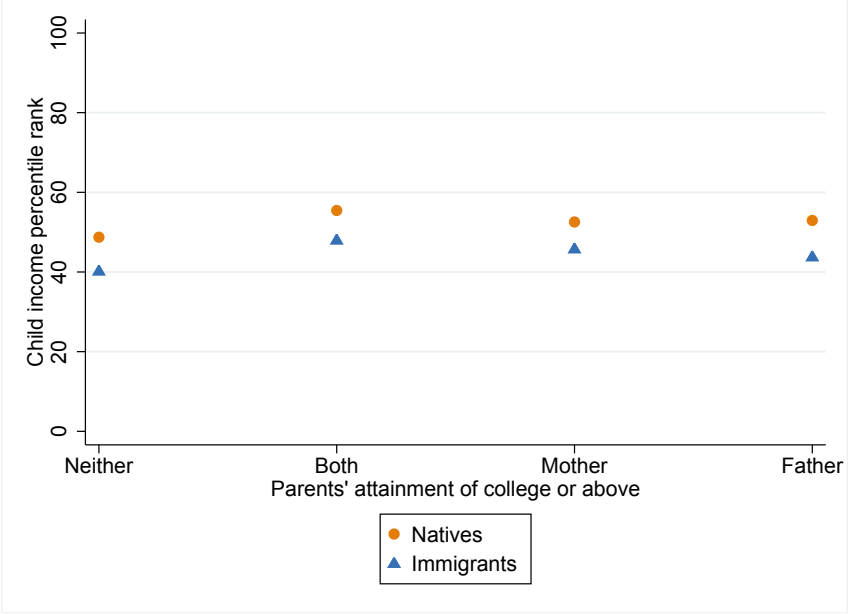
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# Appendix

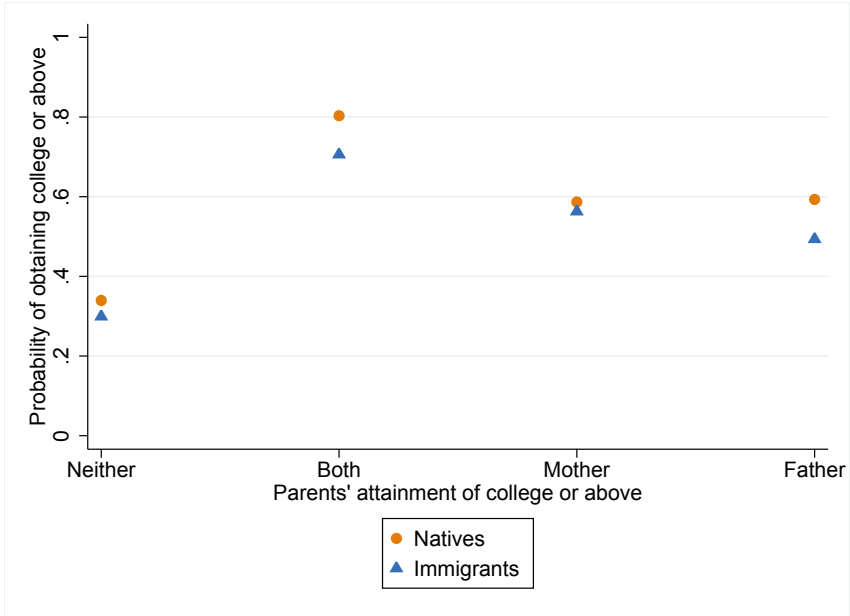
Figure A.1. Average child income percentile rank, by family education



Notes: The figure plots the average child income percentile rank by family education. Children are born between 1974 and 1984. Child income is individual income at age 30. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. A college degree corresponds to having at least a post-secondary education that takes fewer than 3 years to complete.

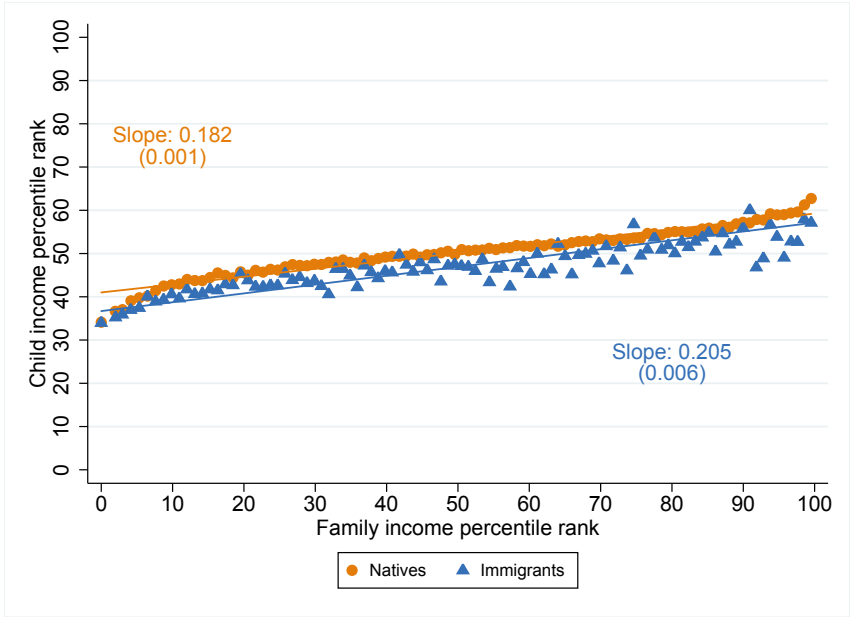


Figure A.2. Average share of children obtaining college or above education conditional on parents' education



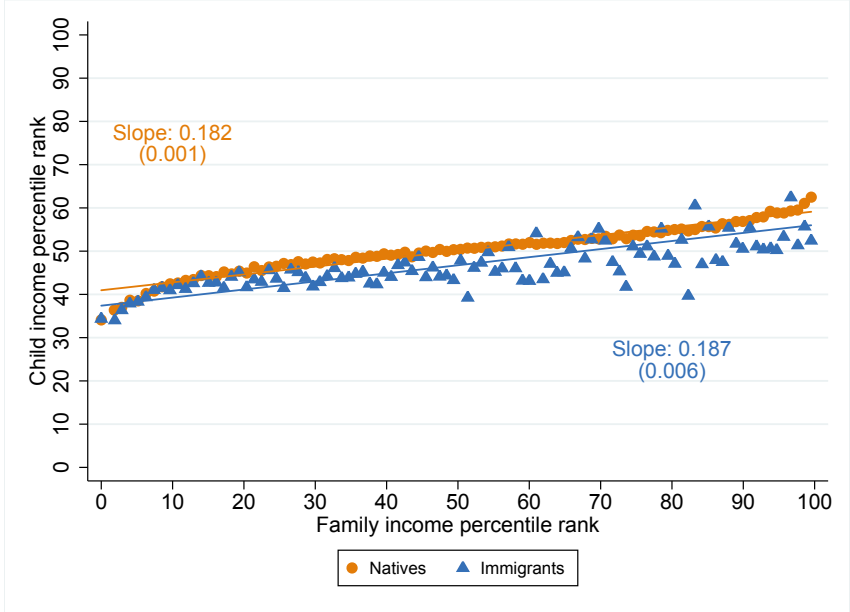
Notes: The figure plots the mean child probability of completing a college degree or above by family education. Children are born between 1974 and 1984. A college degree corresponds to having at least a post-secondary education that takes fewer than 3 years to complete.

Figure A.3. Average child income percentile rank, conditional on family income percentile rank (family income measured 10-14 years after arrival)



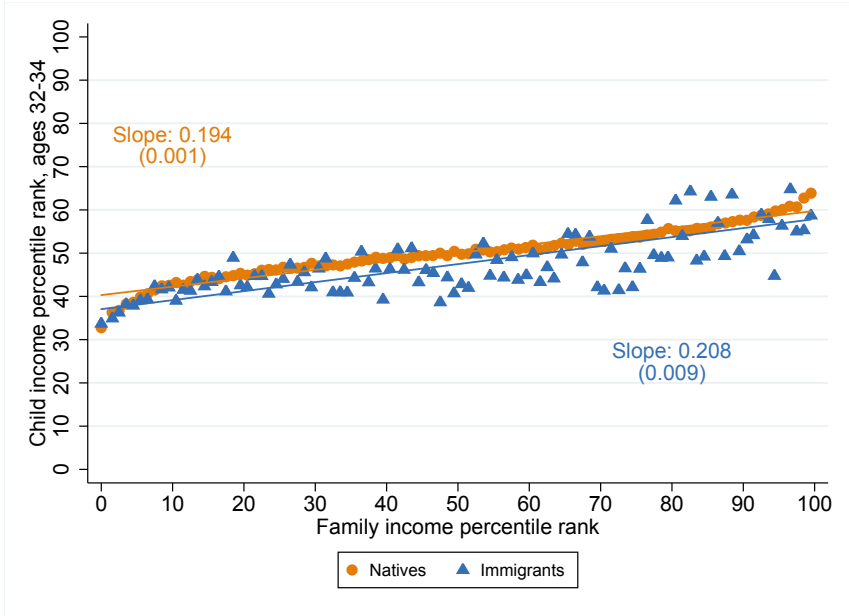
Notes: The figure plots the percentile income rank of children in the 1974-1984 birth cohorts at age 30 against the percentile rank of their parents for natives and immigrants, respectively. Child income is individual income at age 30. For natives, parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). For immigrants, parent family income is the average family income calculated 10-14 years after immigration. We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

Figure A.4. Average child income percentile rank, conditional on *parental* income percentile rank



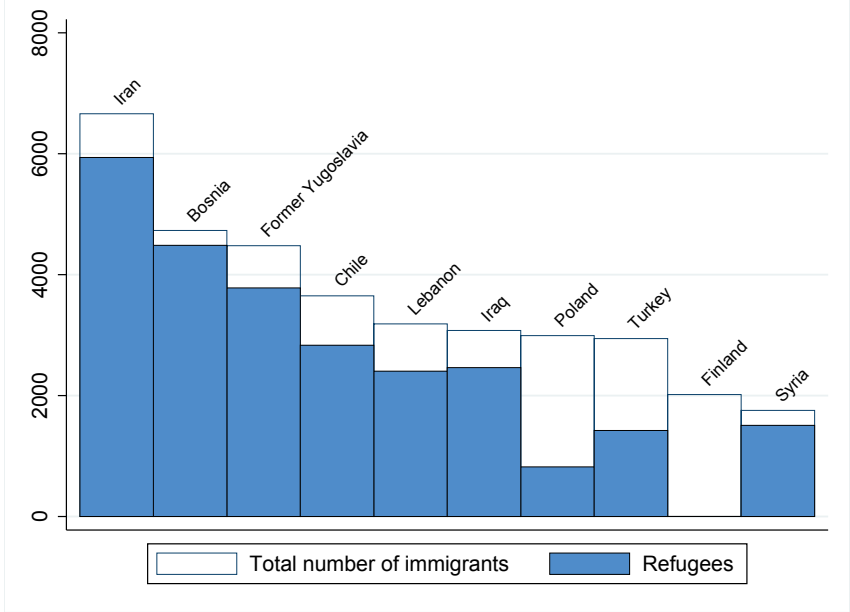
Notes: The figure plots the percentile income rank of children in the 1974-1984 birth cohorts at age 30 against the percentile rank of their parents for natives and immigrants, respectively. Child income is individual income at age 30. Parent income is the average parental income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

Figure A.5. Average child income percentile rank at ages 32-34, conditional on family income percentile rank



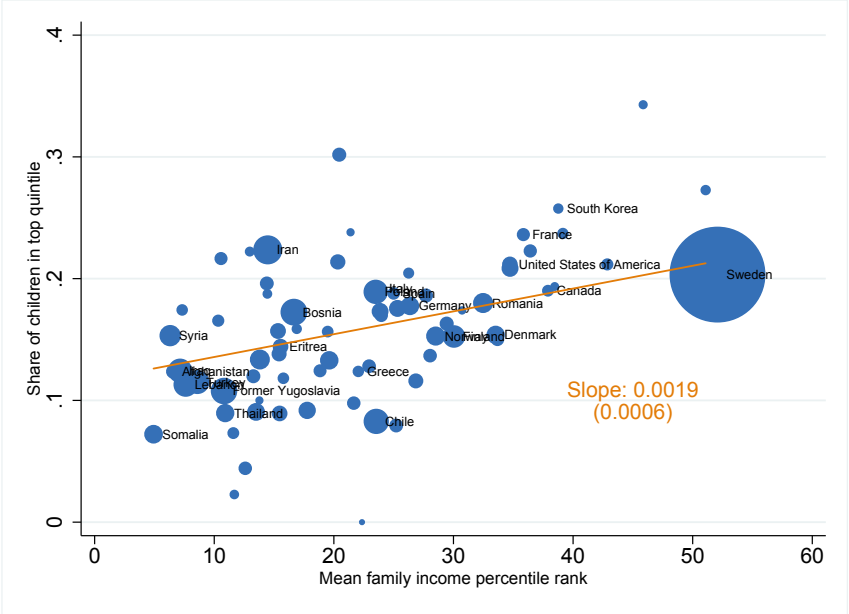
Notes: The figure plots the percentile income rank of children in the 1974-1980 birth cohorts at ages 32-34 against the percentile rank of their parents for natives and immigrants, respectively. Child income is average annual individual income when the child is between 32 and 34 . Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. The slopes are estimated using OLS. Standard errors are in parentheses.

Figure A.6. Top ten countries of origin for immigrant children, with refugee share



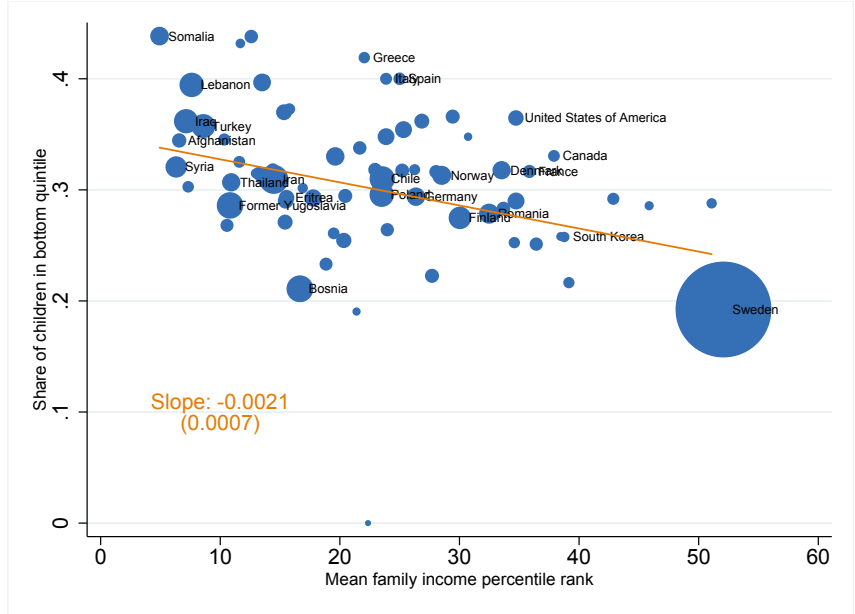
Notes: The figure plots the top ten countries of origin for immigrant children in Sweden and shows the share of refugees coming from each country. We classify a child as a refugee if at least one of his or her parents is classified as a refugee in our data. The information on residence permits is missing for some parents (see Table 1).

Figure A.7. Intergenerational mobility into top income quintile, by country of origin



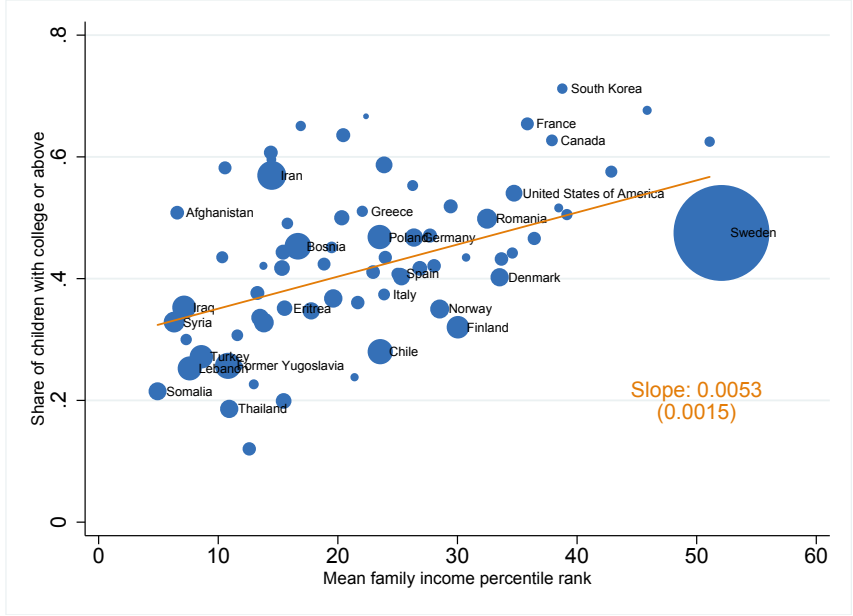
Notes: The figure plots the mean child probability of reaching the top 20% in the income distribution for children in the same birth cohort, against the mean family income rank, for each country of origin. Child income is individual income at age 30. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. Each circle represents a different country of origin, with each circle radius equal to the square root of the number of children from each country. We include a circle for Swedish children as a point of reference, but the observation is not included in the regression. The slope is estimated using weighted OLS. Standard error in parentheses.

Figure A.8. Intergenerational mobility into bottom income quintile, by country of origin



Notes: The figure plots the mean child probability of reaching the bottom 20% in the income distribution for children in the same birth cohort, against the mean family income rank, for each country of origin. Child income is individual income at age 30. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. Each circle represents a different country of origin, with each circle radius equal to the square root of the number of children from each country. We include a circle for Swedish children as a point of reference, but the observation is not included in the regression. The slope is estimated using weighted OLS. Standard error in parentheses.

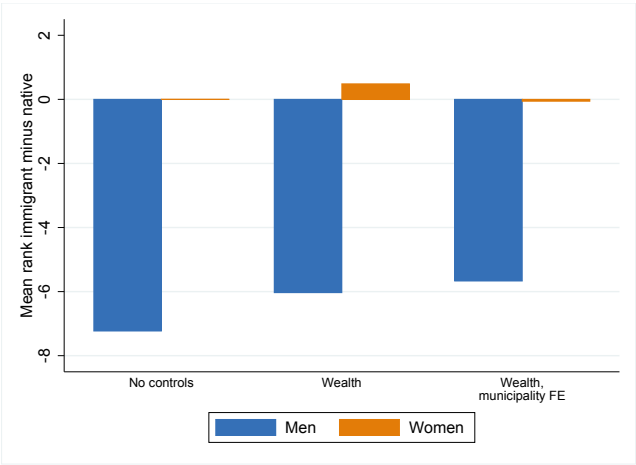
Figure A.9. Educational attainment conditional on family income, by country of origin



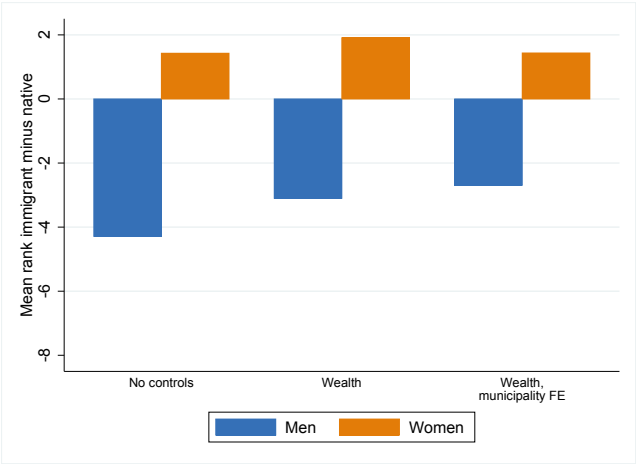
Notes: The figure plots the mean child probability of completing a college degree or above, against the mean family income rank, for each country of origin. Children are born between 1974 and 1984. A college degree corresponds to having at least a post-secondary education that takes fewer than 3 years to complete. Parent family income is the average family income over the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank parents relative to all other parents of children in the same birth cohort. Each circle represents a different country of origin, with each circle radius equal to the square root of the number of children from each country. We include a circle for Swedish children as a point of reference, but the observation is not included in the regression. The slope is estimated using weighted OLS. Standard error in parentheses.



Figure A.10. Intergenerational income gaps for children with parents at the 25th percentile



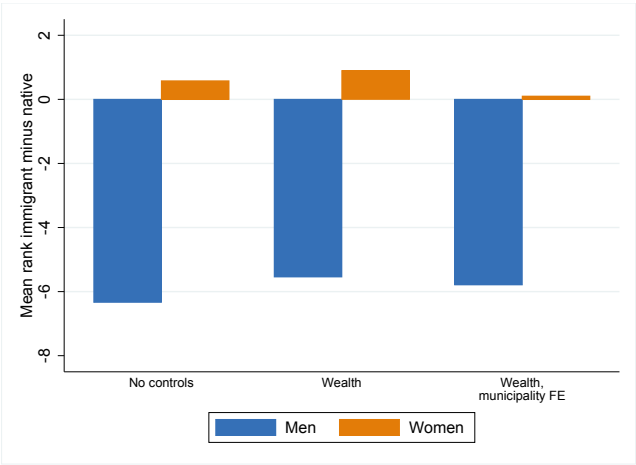
(a) Parents without college education



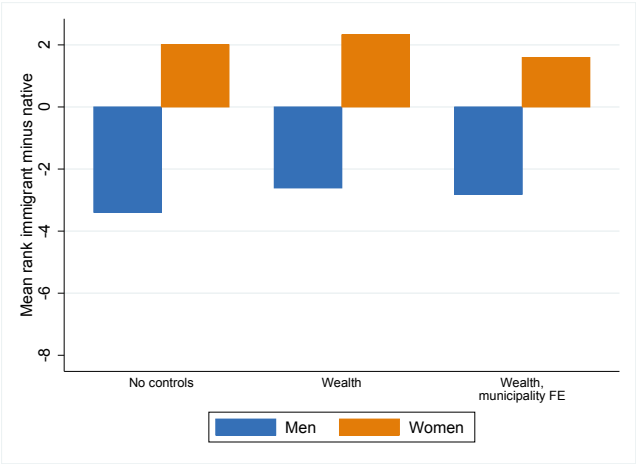
(b) Parents with college education

Notes: In each panel, the first set of bars corresponds to the gap defined by  $\phi_{im} + \phi_{imp}\bar{p} + \phi_{ime}\bar{e}$  for  $\bar{p} = 25$  in equation 3.5. The upper panel evaluates the gap at  $\bar{e} = 0$  and the lower panel at  $\bar{e} = 1$ . The subsequent sets of bars add our proxy for wealth and municipality of residence fixed effects, respectively.

Figure A.11. Intergenerational income gaps for children with parents at the 75th percentile



(a) Parents without college education



(b) Parents with college education

Notes: In each panel, the first set of bars corresponds to the gap defined by  $\phi_{im} + \phi_{imp}\bar{p} + \phi_{ime}\bar{e}$  for  $\bar{p} = 75$  in equation 3.5. The upper panel evaluates the gap at  $\bar{e} = 0$  and the lower panel at  $\bar{e} = 1$ . The subsequent sets of bars add our proxy for wealth and municipality of residence fixed effects, respectively.





## 4. Home Ownership among Children of Immigrants - the Role of Parental Background and Age at Immigration

with Valentin Bolotnyy

*Acknowledgments:* We would like to thank Ina Blind, Matz Dahlberg, Mette Foged, Peter Fredriksson and Hans Grönqvist for valuable feedback. We have also benefited from comments from participants in the EARN Integration Workshop.

## 4.1 Introduction

The immigration literature has long been concerned with documenting the outcomes of the children of immigrants and understanding the mechanisms behind the observed patterns. Many papers study education and earnings outcomes, though some are also concerned with fertility, marriage and health behavior (see Sweetman and van Ours 2015 for a review). A less studied aspect of the economic integration of descendants of immigrants is home ownership, which is the focus of the present study.

For most households, buying a home is one of the most important financial decisions they will ever make. Home ownership is thus an important indicator of socio-economic status and a good proxy for wealth (e.g. Enström Öst 2012). In Sweden, the country on which we focus in this paper, housing has been shown to make up a major part of non-financial wealth (Waldenström 2016). This holds for the United States as well, where for many households their home is their only asset (Lovenheim 2011). Home ownership has also been found to have a range of individual-level benefits. Sodini et al. (2016) exploit quasi-random variation in home ownership and find a strong housing collateral effect: home owners are able to borrow and smooth consumption when faced with a negative labor income shock, whereas renters reduce their consumption when faced with the same shock. Lovenheim (2011) shows that changes in housing wealth triggered by the 2000s housing boom in the United States led to increases in college attendance among individuals from low-income families. If immigrants are less likely to enter home ownership than natives, the already-existing disparities in labor market outcomes are bound to widen over time and perpetuate over generations.<sup>1</sup>

While there are a number of papers that examine home ownership patterns among immigrants, they do not specifically look at those who have spent some or most of their childhood in the host country (e.g. Borjas 2002, Constant et al. 2009). With this paper, we aim to fill this gap. We structure the paper in two parts. First, we document differences in home ownership rates between natives and immigrants in Sweden at age 30. We focus on immigrants who arrived in Sweden as children and therefore spent between 15 and 30 years in Sweden by the time we measure their outcomes. This choice is motivated by the fact that in the second part of the paper we examine the role of age at arrival, and therefore childhood exposure to the Swedish environment, on home ownership in adulthood.

Our first set of results show that on average, immigrants are less likely to own their homes than natives at age 30. The difference remains even after we control for socio-economic characteristics, parental background and municipality of residence. While own socio-economic characteristics explain a very small part of the immigrant-native home ownership gap (around 6%),

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<sup>1</sup>There is some evidence that children of home owners have better educational outcomes and fewer behavioral problems (Haurin et al. 2002).

differences in parental background play the most important role in explaining it. Family income and education explain about a third of the gap, whereas parental home ownership during childhood explains a further third of the remaining gap. These findings provide support for the hypothesis that parents influence children's tenure choices via both direct financial transfers and socialization, the latter by shaping children's expectations for what constitutes a desirable standard of living (Henretta 1984). Borjas (2002) identifies differential location choices between natives and immigrants in the US as one of the main determinants of the home ownership gap. While we do find that location, as measured by municipality of residence explains a non-trivial part of the gap, our results suggest that once family background is taken into account, geographical sorting matters less. We further test the socialization hypothesis by studying how the composition of renters versus owners among co-nationals in the neighborhood one grows up in affects tenure choice later in life. We exploit the 1990s Swedish refugee dispersal policy and use the fact that initial neighborhood allocation is orthogonal to individual characteristics. Even though the sample on which we perform this exercise is quite small, our results point to the existence of important peer effects in tenure choice.

In the second part of the paper, we instead focus on the immigrant group and ask what role age at arrival plays in generating the home ownership patterns we uncover in the first part. Age at arrival might matter for home ownership to the extent that arriving earlier affects income, and therefore one's capacity to own. As we show later, arriving at age 15 translates into placing 15 percentile ranks lower in the income distribution, relative to arriving at ages 0-1. Hence, we expect the probability to own to decrease with age at arrival. Individuals might also differ in their preferences for owning versus renting. Arriving at a younger age might affect the desire to own and therefore to act on the ability to do so.<sup>2</sup> Using variation in age at arrival between siblings, we find a strong negative relationship between age at immigration and home ownership.<sup>3</sup> Children who arrive at age 15 have a 20 percentage-point lower probability of owning their home at age 30. Furthermore, we find that family education is an important mitigator of the negative effect of age at arrival.

The negative effect of age at arrival on a range of outcomes, from education (Böhlmark 2008, Hermansen 2017, Ansala et al. 2019), to health (Van den Berg et al. 2014) and social integration (Åslund et al. 2015) is well-documented, including for immigrants to Sweden. However, no previous study has examined home ownership. Moreover, most of these studies focus on shorter-term outcomes, whereas we are interested in the outcomes of immigrants when they reach adulthood and therefore after having spent a significant

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<sup>2</sup>This would happen if the majority in Sweden aspire to own a home in adulthood, and spending longer time in Sweden means that immigrants' aspirations become similar to natives.

<sup>3</sup>When we disaggregate between the different types of ownership, we see that this effect is driven by the probability of owning an apartment in a multi-dwelling building, as opposed to a single-family detached home.

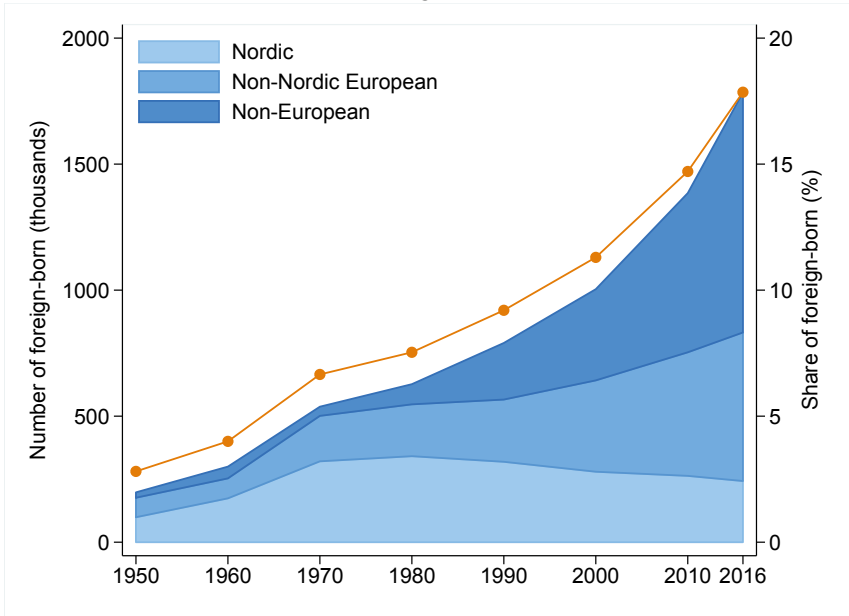
portion of their lives in the host country. Our exclusive focus on immigrants born abroad to both parents born abroad also sets us apart. Most studies include in their analysis those born in the host-country to foreign parents, and some also allow children to be born in native-immigrant families. Compared to the previous studies using Swedish data, we look at more recent immigrant cohorts who are children of refugees to a larger extent and who come primarily from non-European countries.

This paper is structured as follows: the next section provides a short overview of the institutional background and the data used, section 4.3 documents a series of descriptive trends in family background and outcomes in adulthood for natives and immigrants, section 4.4 introduces the empirical strategy we employ for getting at the effects of age at arrival, as well as shows and discusses these results. Section 4.5 concludes.

## 4.2 Institutional background

### 4.2.1 Immigrants in Sweden

Figure 1. Immigrants in Sweden



Notes: The solid areas show the stock of foreign-born from each sending region over the 1950-2016 period (left-hand axis). The orange dots give the share of foreign-born in the population over the same period of time (right-hand axis). Source: Statistics Sweden.

Sweden has for decades been a destination for large numbers of immigrants with widely different backgrounds. Our sample period covers the gradual shift



in country of origin composition from primarily Nordic immigration (mostly from Finland) to non-European immigration (Figure 1, in blue). This shift also meant a move away from labor immigration to family-related and refugee immigration. As of 2016, about 17% of the Swedish population was foreign-born, compared to less than 7% in 1970 (Figure 1, in orange). By comparison, the share of foreign-born in the United States was at about 13% in 2013 (OECD 2017).

#### 4.2.2 The housing market in Sweden

In Sweden, a person can rent, be a tenant-owner, or an owner-occupier.<sup>4</sup> In this section, we explain what each means in the Swedish context.

The Swedish rental market is characterized by rent setting, whereby rents are negotiated between landlord and tenant associations (Sodini et al., 2016). In order to have access to a first-hand contract, one generally has to join a housing queue. The longer one spends in the queue, the higher the probability of finding a flat to rent. Housing companies can be public - owned by municipalities - or private. Public housing in Sweden does not mean social housing, as it does in countries like the U.S., and there are no income thresholds above which one stops being eligible.<sup>5</sup> However, there is evidence that shows that vulnerable families (such as low-earners, single-parent families, immigrants) are in fact overrepresented in this tenure type, especially in the largest cities (see, e.g. Magnusson and Turner 2008).

If a person is a tenant-owner, he or she lives in a flat that he or she has bought the right to use. In practical terms, that means that the owner bought a share in the association of tenant-owners who own the building together (also called a cooperative). A tenant-owner needs the approval of the cooperative before renting out the apartment. Tenant-owners are allowed to sell their dwelling on the market. Tenant-ownership tends to be concentrated among apartments in multi-dwelling buildings.

An owner-occupier owns the house and has the right to use it. Owner-occupied dwellings are usually restricted to detached houses and very rarely to apartments in multi-dwelling buildings.

There is a so-called second-hand rental market, whereby owners sublet their dwellings for a limited amount of time. In the case of tenant-owners, the

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<sup>4</sup>In our data, we can infer the tenure type using information on the type of housing (e.g. detached house, multi-dwelling building etc.) and the legal form of ownership. See Blind (2015, p. 138) for details on how this is achieved. We can additionally identify a residual category “Other” that consists of “farms regardless of legal form of ownership, real estates owned by the state, the church, directly by the municipality or by the County Council, estates of dead persons and those real estate (*sic*) for which data on legal form of ownership and house type is missing” (Blind 2015, p.139).

<sup>5</sup>A more accurate term for public housing would be municipal housing. We use both terms interchangeably throughout the paper.

amount of time allowed to rent out the apartment depends on the tenant-owner association.<sup>6</sup>

Since the goal of this paper is to study home ownership, we do not distinguish between the two categories of owners in the main analysis. When we do show disaggregated results, we refer to tenant-owners as apartment owners and to owner-occupies as house owners.

Figure A.1 shows that overall home ownership rates in the population are quite high, reaching almost 70% in 2014. There has been an upward trend in the share of tenant-owners over the 1990-2014 period, as well as a slight decrease in the share of people who rent from municipal rental housing companies.<sup>7</sup>

## 4.3 Data and descriptive statistics

### 4.3.1 Sample selection and key variables

We use data from the GeoSweden database, which covers all individuals with a permanent residence permit valid for at least one year for the 1990-2014 period.<sup>8</sup> We have information from several different registers, including the education, income, and employment registers. Importantly, we can identify the parents of each individual, provided they have also registered in Sweden (either as a resident or as a citizen) at some point between 1990 and 2014. As in our earlier work, Bolotnyy and Bratu (2019), we look at cohorts born between 1974 and 1984 whose outcomes we observe at age 30. We focus on children born in Sweden to Swedish parents - from now on, natives - and children born abroad to foreign-born parents who arrive in Sweden before the age of 16 - from now on, immigrants.<sup>9</sup>

We measure *family income* as the average combined income<sup>10</sup> of the parents in the register during the years when the child is 15 to 19 years old.<sup>11</sup> We include families with zero income. We follow Chetty et al. (2014) and

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<sup>6</sup>Note that in our data, the tenure variable characterizes the dwelling where the individual currently lives and not the individual's status. In the case of second-hand rental contracts, an individual that is in fact a renter will show up as an owner in our data. To the extent that immigrants are more likely to rent on the second-hand market, we may overestimate the share of owners among immigrants.

<sup>7</sup>This pattern is likely due to the fact that at various points during this time period, municipal rental housing companies have offered their tenants the possibility to buy the apartments they were renting.

<sup>8</sup>GeoSweden is administered by the Institute for Housing and Urban Research at Uppsala University. The data is collected and anonymized by Statistics Sweden.

<sup>9</sup>The description of key variables in this section closely follows Bolotnyy and Bratu (2019).

<sup>10</sup>Our income variable includes income from employment and self-employment. Using alternative variables gives us similar results. These are available upon request.

<sup>11</sup>When the child has only one parent in the register, we measure family income as the average income of the existing parent during the years when the child is 15 to 19 years old. For the 1974

define the family's *percentile rank* based on its position in the *national* distribution of incomes relative to all parents with children in the same birth cohort, regardless of immigrant status.

*Child income* is defined as the individual income the child earns when he or she is 30 years old. Just as for the parents, we define the child's percentile rank based on his or her position in the national distribution of incomes relative to all children in the same birth cohort.

*Parental education* is the maximum level of education observed throughout the time the parent is in the register, so as to reduce the number of missing values for immigrant parents in their first years in Sweden.<sup>12</sup> We categorize families based on whether neither or at least one parent has a college degree or above.<sup>13</sup> In our data, this corresponds to having at least a post-secondary education that takes at least two but fewer than three years to complete.<sup>14</sup>

Similarly, we measure whether the child has a *college degree or above* when the child is 30 years old.

A parent is a *refugee* if the *first* reason for settlement in Sweden is recorded as such. Sweden grants asylum to people classified as refugees in accordance with the Geneva convention and also to those considered to be "in need of subsidiary protection" according to EU regulations. Children are considered refugees if at least one of their parents is classified as one.

We look at the parents' *housing tenure* when the child was between 15 and 19. Our data for the earlier years allows us to observe the tenure type for the full 5-year period only for the 1980-1984 cohorts.<sup>15</sup> We define *parental housing tenure* as the tenure type parents lived in for the largest amount of time during the 5-year period. Despite incomplete information on housing tenure for the 1974-1979 cohorts, results when using only the 1980-1984 cohorts are similar to those when using the full sample. For this reason, we show results from the full sample throughout this paper.<sup>16</sup>

We focus on *child housing tenure* when the child is 30 years old.

We show summary statistics for native and immigrant children in Table 1. For immigrants, we also show statistics for the siblings sample, as we exploit siblings variation in age at arrival in a later section. We see that on average

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cohort, we measure family income when the child is between 16 and 20, because our income data starts in 1990.

<sup>12</sup>Immigrant parents might see their skills and degrees obtained abroad recognized some time after arrival.

<sup>13</sup>We do so only for families where both parents have non-missing education information when both parents are in the register (or the one existing parent has non-missing information when only one parent is in the register). However, if we assign families the level of education from just one parent when only one parent has non-missing information, the average share of families with college or above changes only slightly, from 42.92% to 42.89% for natives and from 33.8% to 33.11% for immigrants.

<sup>14</sup>The equivalent in the United States would be an associate's degree.

<sup>15</sup>We do not have housing tenure information for the years 1991-1994.

<sup>16</sup>Results using the restricted sample are available upon request.

**Table 1. Summary statistics**

|   | Mean  | Std. dev. | No. of obs. |
|---|-------|-----------|-------------|
| <b>Natives</b>                            |       |           |             |
| Child percentile income rank              | 50.75 | 29.15     | 819,422     |
| Child has college or above                | 48.20 | n/a       | 814,931     |
| Parent percentile income rank             | 53.72 | 27.45     | 819,422     |
| At least one parent with college or above | 42.92 | n/a       | 818,014     |
| Owner-occupier                            | 36.02 | n/a       | 811,703     |
| Tenant-owner                              | 23.87 | n/a       | 811,703     |
| In public rental                          | 12.59 | n/a       | 811,703     |
| In private rental                         | 15.13 | n/a       | 811,703     |
| Lives with parents                        | 6.08  | n/a       | 819,422     |
| <b>Immigrants: full sample</b>            |       |           |             |
| Child percentile income rank              | 40.82 | 30.77     | 52,772      |
| Child has college or above                | 37.35 | n/a       | 52,336      |
| Parent percentile income rank             | 15.55 | 19.74     | 52,772      |
| At least one parent with college or above | 33.80 | n/a       | 50,662      |
| Average age at arrival                    | 8.99  | 4.04      | 52,772      |
| At least one parent refugee               | 76.37 | n/a       | 43,983      |
| Owner-occupier                            | 16.96 | n/a       | 52,065      |
| Tenant-owner                              | 26.19 | n/a       | 52,065      |
| In public rental                          | 29.36 | n/a       | 52,065      |
| In private rental                         | 16.57 | n/a       | 52,065      |
| Lives with parents                        | 13.35 | n/a       | 52,772      |
| <b>Immigrants: siblings sample</b>        |       |           |             |
| Child percentile income rank              | 40.29 | 30.48     | 25,738      |
| Child has college or above                | 35.50 | n/a       | 25,540      |
| Parent percentile income rank             | 13.87 | 18.46     | 25,738      |
| At least one parent with college or above | 31.43 | n/a       | 24,587      |
| Average age at arrival                    | 9.49  | 3.60      | 25,738      |
| At least one parent refugee               | 85.54 | n/a       | 22,716      |
| Owner-occupier                            | 15.95 | n/a       | 25,436      |
| Tenant-owner                              | 25.68 | n/a       | 25,436      |
| In public rental                          | 30.89 | n/a       | 25,436      |
| In private rental                         | 16.46 | n/a       | 25,436      |
| Lives with parents                        | 12.96 | n/a       | 25,738      |

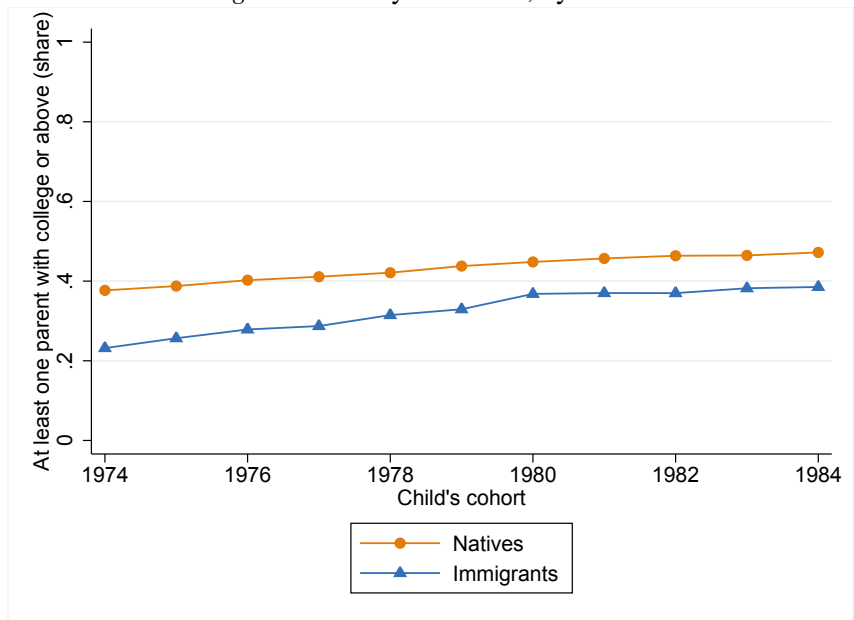
*Notes:* Children are born between 1974 and 1984. Income is in thousands of 2014 SEK. Child income is individual income measured when the child is 30 years old. Parent family income is the combined income of the parents during the period when the child is between 15 and 19 (between 16 and 20 for the 1974 cohort). We rank children relative to all other children in their birth cohort. We rank parents relative to all other parents of children in the same birth cohort. A college degree corresponds to having at least a post-secondary education that takes fewer than 3 years to complete. We classify a child as a refugee if at least one of his or her parents is classified as a refugee in our data. Where standard deviations are not reported, the Mean column shows shares.

immigrants grow up in families that earn less than 40% of what native families earn. However, as adults, immigrant children earn about 80% of what average native children earn. Both native families and children are more likely to have college or above than immigrant families and children, respectively. The average age at arrival is around 9 years old. A higher share of immigrant children live with their parents. We can see already from this table that immigrants are less likely to own and more likely to rent. In the rest of this section, we give a comprehensive view of the childhood environments immigrants and natives grow up in and how these relate to their housing outcomes in adulthood.

### 4.3.2 Family background

Looking across birth cohorts, we observe that immigrant parents have been consistently less likely than native parents to have completed college or above (Figure 2). Figure 2 also shows that there has been a general increase in the share of families where at least one parent has a college degree among both natives and immigrants, although the rate of increase has been higher for immigrant families.<sup>17</sup>

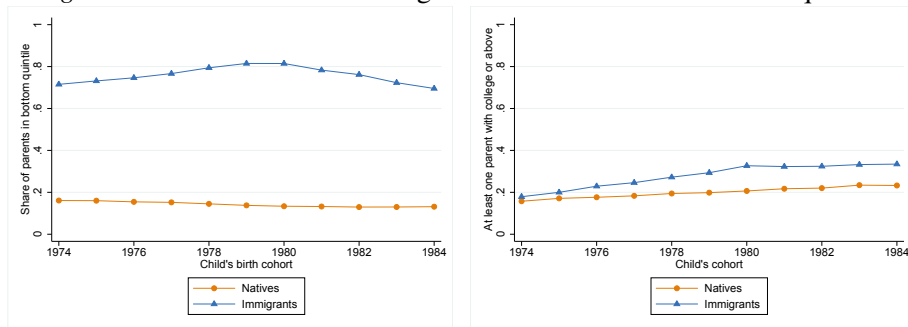
Figure 2. Family education, by cohort



<sup>17</sup>One possible explanation for this is that, starting with the 1979 birth cohort, the share of immigrant children who come to Sweden with both parents rises from about 70% to almost 80% (see Figure A.2).

The story, however, is very different when we compare the earnings of immigrant parents with the earnings of native parents.<sup>18</sup> As we can see from Figure 3a, the share of immigrant parents in the bottom income quintile has consistently exceeded the share of native parents in the bottom income quintile by a substantial amount. The share of immigrant families in the bottom quintile has fluctuated between 70% and 80% and, unlike with education, has in no way narrowed the gap with the share of native children who are born into the bottom quintile. The result, as Figure 3b makes clear, is that the share of immigrant parents in the bottom income quintile who have a college or above education is higher than the share of native parents in the bottom income quintile with a college or above education. Having a college education takes native Swedish parents much farther than it takes immigrant parents. Figure A.4 illustrates this dramatically: native families where both parents are college educated have an average income percentile rank of about 80, while immigrant families where both parents are college educated have an average income percentile rank of about 35.

*Figure 3. Education levels among families in the bottom income quintile*



(a) Share of families in bottom income quintile

(b) Share of families in bottom income quintile with college or above education

When it comes to housing, native children are much more likely to grow up in housing that is owned, whereas immigrant children are more likely to live in rental housing (Figure A.5).<sup>19</sup> These patterns have been quite stable across birth cohorts.<sup>20</sup>

Family income seems to explain a significant share of the trends we observe. In Figure 4, we see that the home ownership gap between immigrants

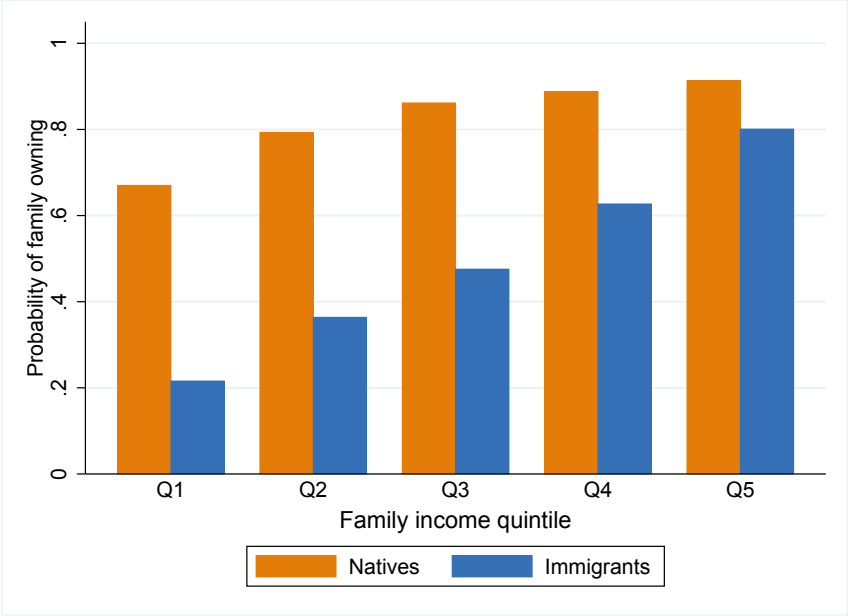
<sup>18</sup>These large differences are partly driven by the fact that by the time we measure their incomes, some parents have been in Sweden for longer than others, hence they had longer time to integrate in the labor market. See our previous work, Bolotnyy and Bratu (2019) for an in-depth discussion.

<sup>19</sup>A likely reason behind that is that many of the families in our sample arrived as refugees who were provided with rental housing by municipalities.

<sup>20</sup>Since relatively few families are in "other" types of housing, we focus on families who either own or rent for the rest of this section.

and natives gets smaller and smaller, the higher we go in the family income distribution. There's a difference of more than 40% in the probability of owning for those in the bottom quintile, which gets reduced to about 10% for those in the top quintile. Hence, the native poor and the immigrant poor in Sweden are generally living in different environments.

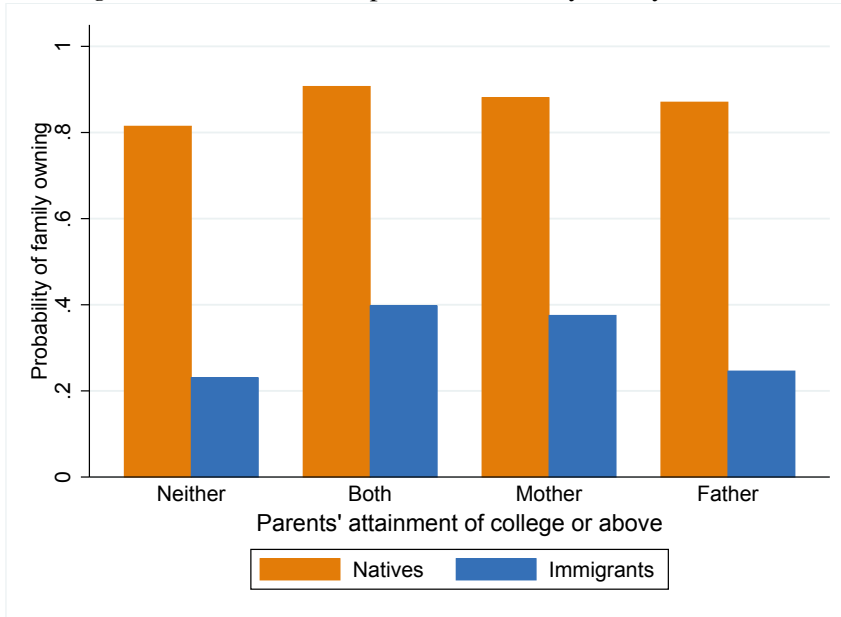
Figure 4. Home ownership in childhood, by family income



By contrast, these differences do not disappear when we condition instead on parental education. In Figure 5 we see that for immigrants, the probability of owning is almost twice as high in families where both parents have a college education, relative to families where neither parents do. Though this probability increases for native parents as well, the percentage increase is smaller. For natives, parent education does not seem to matter a lot for housing tenure, but it matters substantially for immigrants.

To summarize, immigrant children grow up in families that are on average less educated - to the extent that the education variable captures parents' education accurately. Their parents also have fewer financial resources at their disposal: they are ranked lower in the income distribution and they are less likely to own their homes. Some of these differences get smaller at the top of the income distribution, but are not eliminated entirely.

Figure 5. Home ownership in childhood, by family education



#### 4.3.3 Outcomes in adulthood

We now ask how these family background characteristics relate to children's housing outcomes at age 30.

We first note that despite the fact that they grow up in very different childhood environments, by the time they are 30 immigrants place on average only about 10 percentile ranks lower than natives in the national income distribution (Table 1). About 37% have a university degree or above, compared to 48% of natives.

In terms of housing, native and immigrant children look more similar than during childhood, as we see in Figure 6: immigrants own their homes to a larger extent than their parents did, though not to the same extent as natives at age 30. Immigrants still rent to a larger extent than natives. These patterns are similar across cohorts. Children of immigrants therefore converge to some extent to the children of natives in terms of home ownership, but not fully.<sup>21</sup>

Figure 7 looks at how these trends correlate with child income.<sup>22</sup> We find a similar pattern as when we compared childhood backgrounds: natives are more likely to own their homes than immigrants, but the difference gets smaller at the top of the income distribution. However, we still see that the highest-earning immigrants are only slightly more likely to own their homes than the *lowest-earning* natives.

<sup>21</sup>When we disaggregate by type of ownership, we see that most of that convergence can be explained by immigrant and native children moving into owning apartments.

<sup>22</sup>As for families, we focus on children who either own or rent for the rest of this section.



Figure 6. Home ownership in adulthood, by cohort

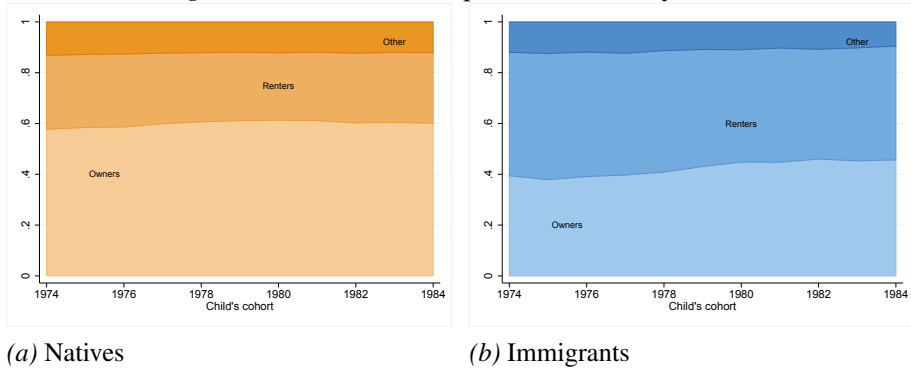
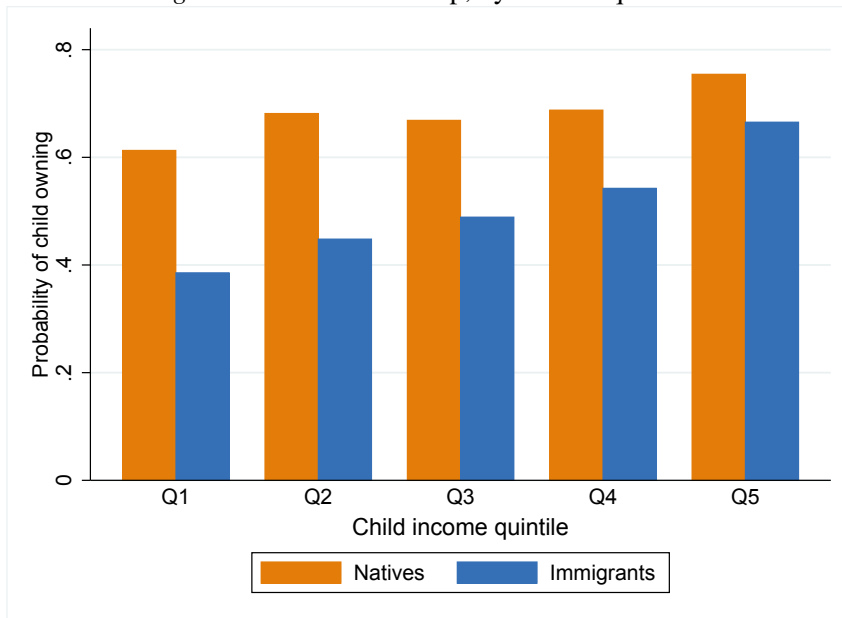


Figure 7. Home ownership, by income quintiles



So far we have seen that natives and immigrants with similar incomes have diverging home ownership patterns. We now examine the immigrant-native home ownership gap in a more systematic way. In the first column of Table 2, we run a regression of a home ownership indicator on the immigrant status dummy. This gives us the unconditional difference in outcomes between immigrants and natives. We see that immigrants are almost 20% less likely to own their homes than natives.

In column 2, we control for a set of child characteristics that have been shown to be associated with home ownership: child income as measured by the percentile rank, education level, marital or cohabiting status and number

of children.<sup>23</sup> The gap gets smaller, but only slightly. Borjas (2002) also finds that differences in socioeconomic characteristics between immigrants and natives do not play a significant role in explaining the home ownership gap. The next column adds parental background characteristics (family income rank and education). The gap gets reduced by a third. Controlling for whether one grew up in owned housing reduces the immigrant-native gap further (Column 4).<sup>24</sup> Together, these results suggest there is strong intergenerational transmission of home ownership, and that the channels could be either direct via financial help from parents, and indirect via shaping children's preferences for owning. In Section 4.5 of the Appendix we show further suggestive evidence that preferences for renting and owning might be shaped by the environment children grow up in.

**Table 2.** *Home ownership in adulthood*

|                            | (1)               | (2)               | (3)               | (4)               | (5)               |
|----------------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
| Immigrant                  | -0.199<br>(0.002) | -0.187<br>(0.002) | -0.130<br>(0.002) | -0.093<br>(0.002) | -0.063<br>(0.002) |
| Child income rank          |                   | ✓                 | ✓                 | ✓                 | ✓                 |
| Child college or above     |                   | ✓                 | ✓                 | ✓                 | ✓                 |
| Married or cohabiting      |                   | ✓                 | ✓                 | ✓                 | ✓                 |
| Number of kids             |                   | ✓                 | ✓                 | ✓                 | ✓                 |
| Family income rank         |                   |                   | ✓                 | ✓                 | ✓                 |
| Family college or above    |                   |                   | ✓                 | ✓                 | ✓                 |
| Family owned in childhood  |                   |                   |                   | ✓                 | ✓                 |
| Municipality fixed effects |                   |                   |                   |                   | ✓                 |
| Observations               | 757,480           | 754,008           | 751,020           | 731,606           | 731,602           |
| Share natives who own      | 0.684             |                   |                   |                   |                   |
| Share immigrants who own   | 0.484             |                   |                   |                   |                   |

*Notes:* Each panel shows the coefficient on a dummy that indicates immigrant status. Each coefficient is from a separate regression. All outcomes are measured at age 30. Standard errors are shown in parentheses.

<sup>23</sup>For example, Deurloo et al. (1994) find that the birth of a couple's first child, as well as positive income shocks trigger transitions to home ownership in the US. Henretta (1984) also finds marital status and own income to be strong determinants of home ownership.

<sup>24</sup>Enström Öst (2012) also finds parental home ownership to be an important predictor for the child entering home ownership.

Immigrants are more likely than natives to live in municipalities with higher population density, regardless of income (Figure A.3).<sup>25</sup> High-density municipalities are likely to be characterized by different local housing market structures. We control for these differences by adding municipality fixed effects in column 5. The immigrant-native gap is further reduced by 3 percentage points. Nonetheless, after controlling for a significant number of variables that have been found to be determinants of home ownership, there remains a gap of around 6%, which corresponds to slightly more than 30% of the initial gap.

## 4.4 Effects of age at arrival

In this section, we investigate the extent to which age at arrival plays a role in generating the home ownership patterns among immigrants that we uncovered in the previous section. We begin by describing the empirical setup. Before we get to the home ownership results, we first look at how age at arrival affects labor market outcomes. This is motivated by the fact that success on the housing market could be related to success on the labor market.

### 4.4.1 Empirical setup

We estimate the following equation:

$$y_i = \alpha + \sum_{a=2}^{15} \beta_a I(a_i = a) + \lambda \text{first-born}_i + \gamma \mathbf{X}_i + \eta_i \quad (4.1)$$

where  $y_i$  is the outcome of child  $i$ ,  $a_i$  is child age at arrival in Sweden<sup>26</sup>, and  $\mathbf{X}_i$  is a vector that includes controls for country of origin, year of arrival, parent education, family income rank, a dummy for whether there is only one parent in the register, number of siblings, and gender. Following previous literature (Böhlmark, 2008), we also include a dummy variable denoting whether the child is first-born. Hence, our sample will consist only of children who have at least one sibling. Note that we restrict to families where both siblings are born abroad.

Assuming that the controls capture all child and parent characteristics that might be driving earlier arrival in Sweden and thus later-life outcomes, the

<sup>25</sup>They are more likely to live in one of the top three largest municipalities in Sweden as well (see Figure A.6).

<sup>26</sup>Since there are few children that arrive during the same year they are born - that is, at age zero, we pool those arriving at age zero and at age one and we use this group as the reference category, so as to improve precision. Results are qualitatively and quantitatively very similar, but standard errors are lower using the pooled reference category. Results using age zero as the reference category are available upon request.

effect of spending every additional year of childhood in Sweden as measured here can be considered causal.

However, it may well be that parents with better unobservables migrate when their children are young, which would threaten our identification strategy. We therefore also estimate the following family fixed effects specification, which allows us to identify the effect of every additional year of childhood spent in Sweden on later-life outcomes using only within-family differences in age at arrival. Controlling additionally for differences between siblings along birth-order and gender lines, we are able to isolate a plausibly-causal effect of spending a particular year of childhood in Sweden on a child's later-life outcomes.<sup>27</sup>

$$y_{ij} = \delta + \sum_{a=2}^{15} \theta_a I(a_{ij} = a) + \rho \text{first-born}_{ij} + \sigma \text{female}_{ij} + \phi_j + \varepsilon_{ij} \quad (4.2)$$

where  $y_{ij}$  is the outcome of child  $i$  in family  $j$ ,  $a_{ij}$  is the child's age at arrival in Sweden, and  $\phi_j$  is the family fixed effect that captures unobserved family characteristics that are common to all siblings in the same family and constant over time.<sup>28</sup>

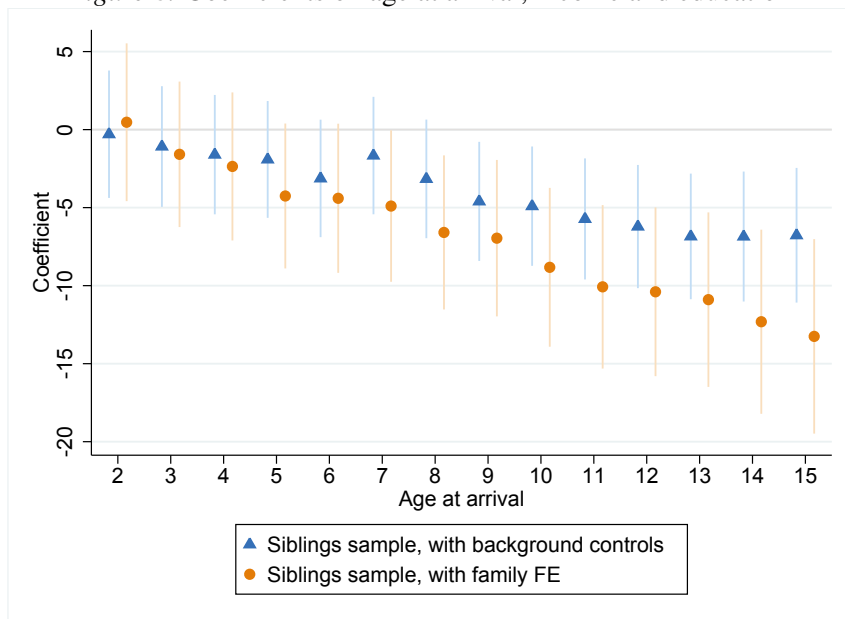
#### 4.4.2 Results

Figure 8 plots the coefficients on the age at arrival dummies from estimating equation 4.2 for percentile income rank (8a) and educational attainment (8b). We first note that for education, results from the specification with background controls are very similar to those from the family fixed effects specification. Though we use different immigrants cohorts than previous studies, we find a similar pattern to what has been observed before in the age at immigration literature: later arrivals have worse income and education outcomes. Arriving at ages 0-1 instead of age 15, for example, results in about a 15 percentile rank higher expected income and a 20 percentage point higher probability of achieving a college education.

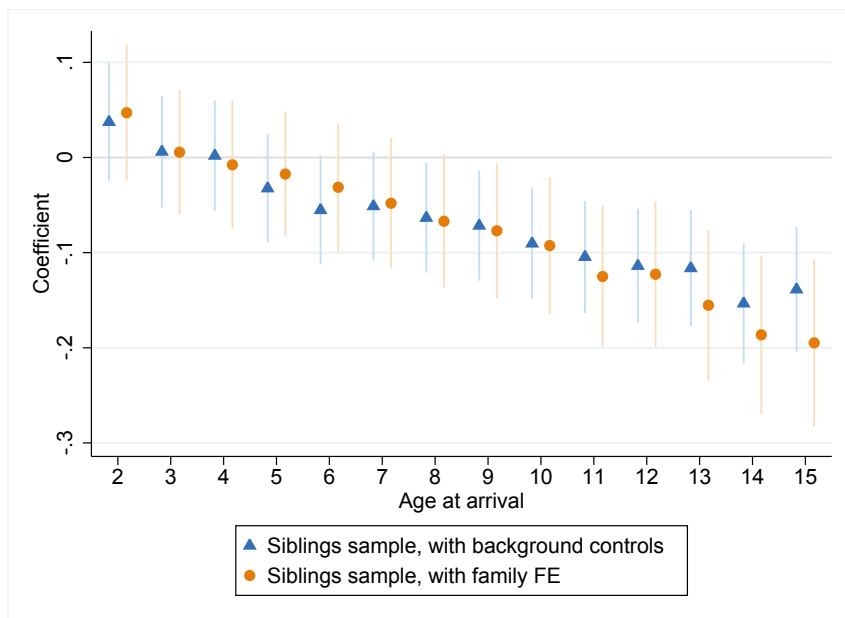
<sup>27</sup>Using this approach, we cannot, however, separate out the effect of age at exposure from length of exposure.

<sup>28</sup>There is very little variation in year of arrival between siblings, hence results are very similar when using a specification with year of arrival dummies.

Figure 8. Coefficients on age at arrival, income and education



(a) Child income percentile rank

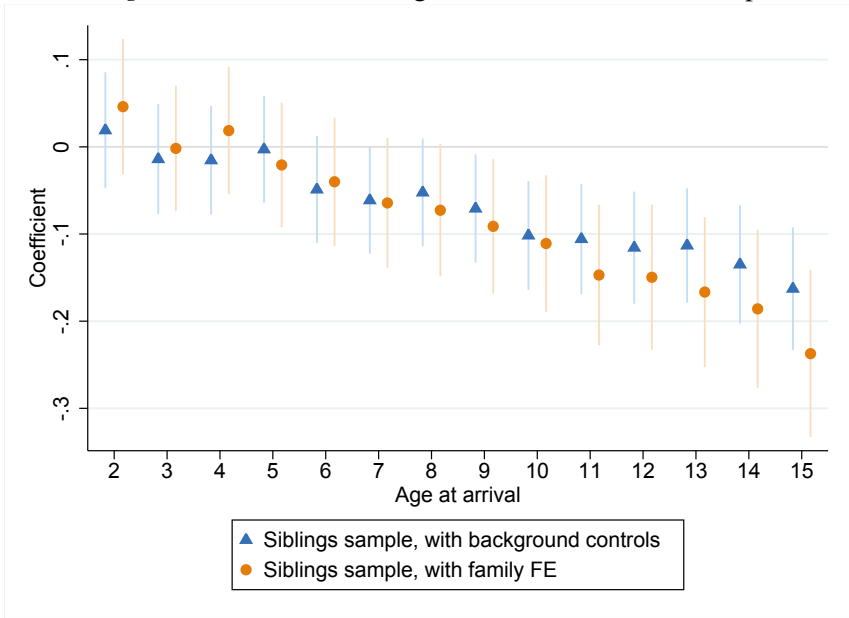


(b) College or above attainment

Notes: The figures plot the coefficients on the age at arrival dummies obtained from estimating equation 4.1 (in blue) and equation 4.2 (in orange), on child income percentile rank and college or above, respectively. Both outcomes are measured at age 30. All estimations are done on the siblings sample.

When it comes to home ownership, we see in Figure 9 that arriving at ages 0-1 instead of age 15 results in about a 20 percentage point higher probability of owning a home. This result, coupled with the finding that immigrants that arrive earlier place higher in the income distribution, indicates that labor market integration does translate into housing market integration. However, when we disaggregate the outcome into different types of owning, we see that the negative effects are primarily driven by a lower probability of owning an apartment the later one arrives. Age at arrival has no effect on the probability of owning a single-family detached home (Figure A.7). This non-effect might be driven by a combination of factors, such as preferences, information barriers or discrimination. Single-family detached homes may be located in different neighborhoods than the ones where immigrant children grew up. To the extent that adult children have preferences for living close to their parents, their tenure choice is dictated by the tenure mix in the neighborhood.<sup>29</sup>

Figure 9. Coefficients on age at arrival, home ownership



Notes: The figure plots the coefficients on the age at arrival dummies obtained from estimating equation 4.1 (in blue) and equation 4.2 (in orange), on the probability of being a home owner at age 30. Both estimations are done on the siblings sample.

<sup>29</sup>On average, around 56% of immigrants in the siblings sample live in the same municipality as the one they spent most time in while they were 15 to 19 years old.

Co-residence might explain some of these results. Given that in the parent generation immigrants are more likely to rent, the low levels of home ownership we observe in the child generation might simply be due to a mechanical effect. Table 1 showed that around 13% immigrant children live with their parents at age 30. However, age at arrival does not seem to have an effect on the probability of co-residence. Figure A.8 shows that while the coefficients on age at arrival are always positive (though imprecisely estimated), children arriving earlier are as likely as children arriving later to live with their parents at the age of 30.

### **Heterogeneous effects**

We have shown that the amount of exposure a child has to Sweden during childhood is important for that child's later-life outcomes. We might additionally want to know whether time spent in Sweden is similarly important for all children or if it varies in importance based on certain characteristics.<sup>30</sup>

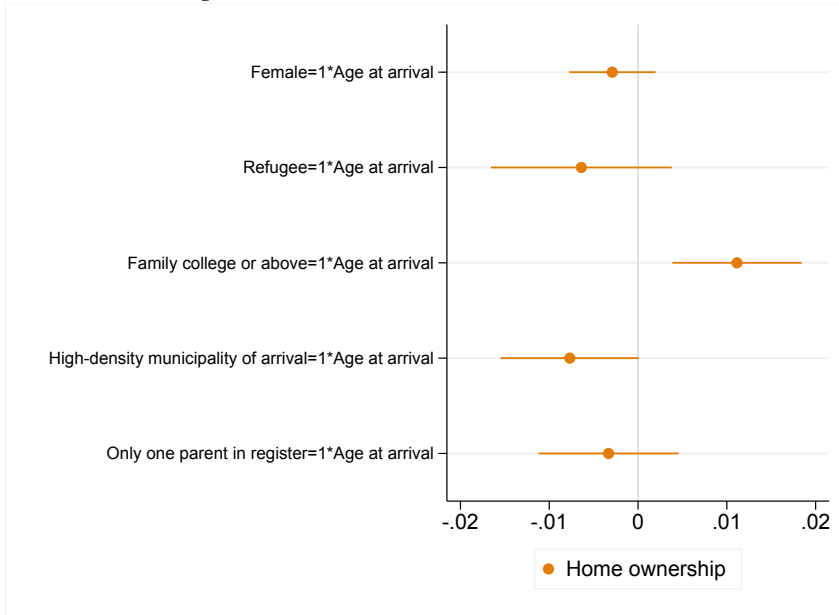
We proceed by re-estimating a version of equation 4.2 where we add interaction terms between the characteristics of interest and the age at arrival variable. Since we saw that the relationship between age at arrival and our outcomes of interest is fairly linear and because a linear term allows us more statistical power, we also replace dummy variables for age at arrival with a linear term in our specifications.

Figure 10 summarizes our results. The main take-away is that having parents with a college education mitigates the negative effect of arriving later on the probability of home ownership in adulthood. This finding reinforces the descriptive patterns in Figure 5, where we saw that families where both parents have a university education are almost twice as likely to own when the child is between 15 and 19, coupled with the results in Table 2 showing a strong inter-generational relationship in home ownership. Women and refugees who arrive later, as well as children who arrive later with only one parent may be facing a double disadvantage: the negative effects of age at arrival on all outcomes are intensified for these individuals (the coefficients are not precisely estimated, however).

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<sup>30</sup>The characteristics we are interested in are: parental education; whether the municipality of arrival was high-density; whether the child only has one parent in the register; being a woman; being a refugee. For the municipality of arrival, we consider the municipality where the mother lived during the year of arrival for those arriving in 1990 or later, and the municipality in 1990 for those arriving before 1990. We consider the father's municipality when the mother is not in the register. Population density data starts in 1991, so we use 1991 information for both 1990 and 1991. A child is defined a refugee if for at least one of their parents, the first residence permit in Sweden is a refugee permit.

Figure 10. Coefficients on interaction terms



Notes: The figure plots the coefficients on the interaction terms between the age at arrival dummies and the characteristics of interest. They are obtained from estimating a version of equation 4.2 where we add these additional interaction terms. The estimation is done on the siblings sample.

## 4.5 Conclusion

In this paper, we document large differences in childhood environments between natives and immigrants, as captured by family income and education. Even conditional on these parental characteristics, immigrant parents are less likely than natives to own their homes. We show that by the time they are 30, immigrants close some of these earlier gaps, but not entirely. Additionally, we show that the number of childhood years spent in Sweden matters for the probability of home ownership in adulthood. We find some evidence that arriving later is especially harmful to women, refugees and those arriving with only one parent. These results contribute to our understanding of immigrant children's integration patterns and they highlight the importance of intergenerational correlations in home ownership. Previous research has shown that home owners are better able to smooth consumption when faced with negative income shocks than renters and they are also better positioned to make investments in their children's education. If immigrants are less likely than natives to become home owners even conditional on socio-economic characteristics, immigrant-native gaps in education and income are also likely to widen over generations.



In future work, we plan to take advantage of the longitudinal aspect of our data and follow immigrants' and natives' housing trajectories during adulthood to examine whether life course events such as family formation, the birth of a child and family dissolution affect natives and immigrants differently. Our previous work (Bolotnyy and Bratu, 2019) has uncovered substantial country of origin differences in terms of income and education, which is another aspect that we aim to look further into.

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Appendix

Figures

Figure A.1. Share individuals in each tenure type

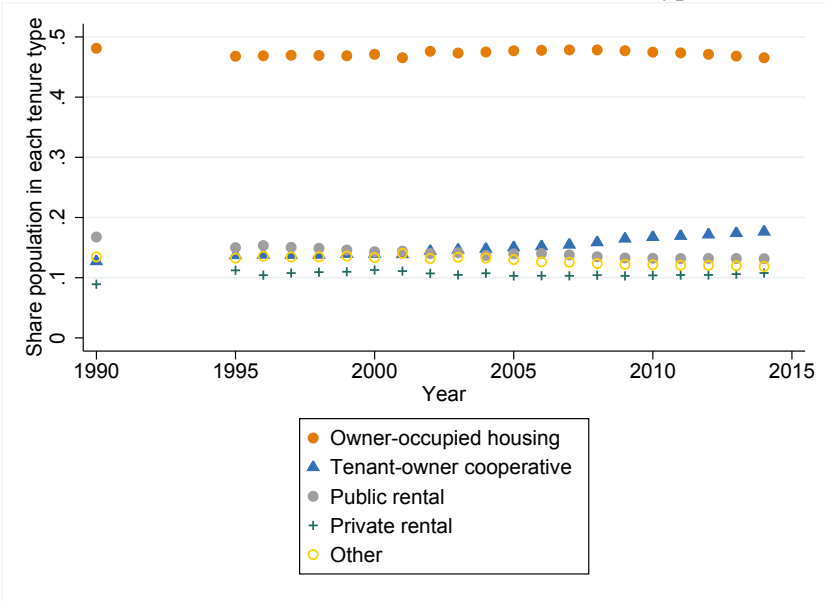


Figure A.2. Share of children with both parents in the register

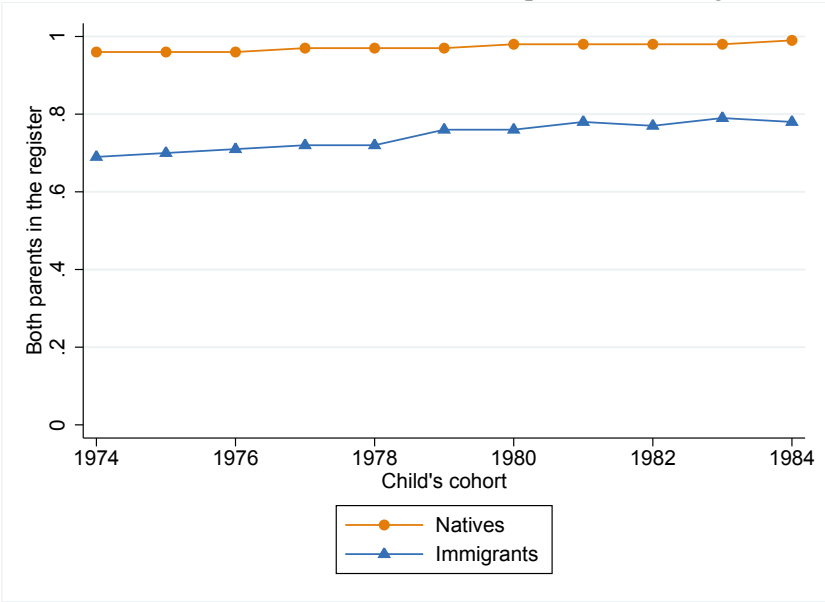


Figure A.4. Average family income percentile rank, by family education

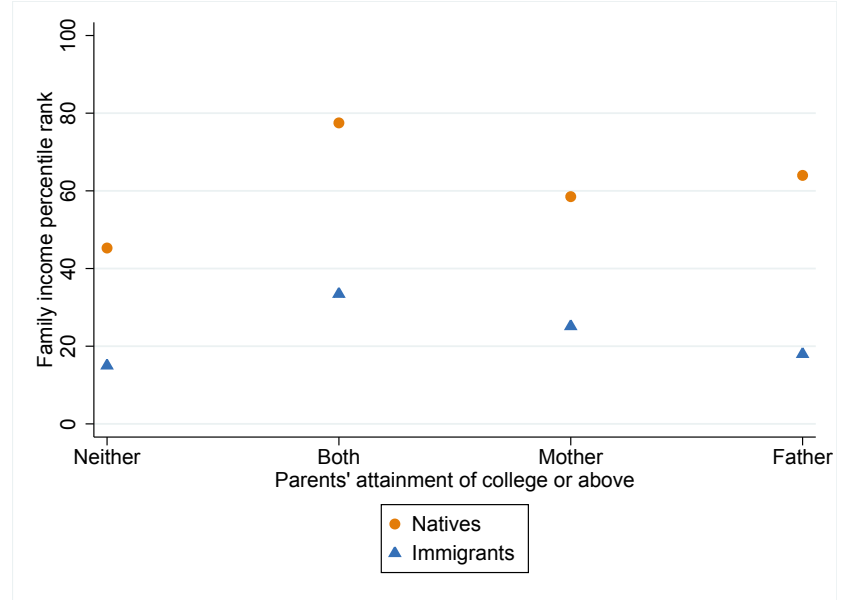
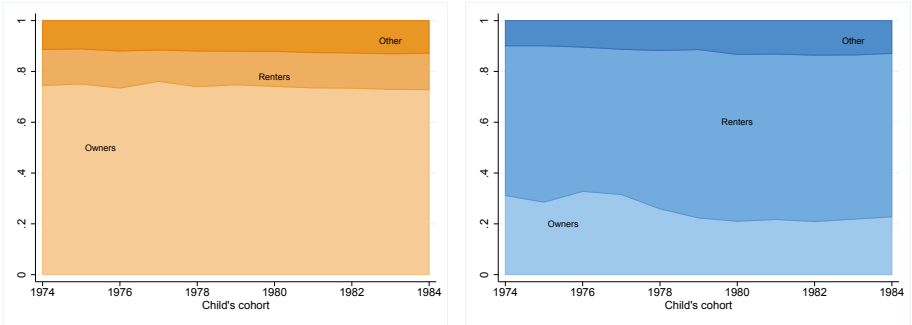


Figure A.5. Home ownership in childhood, by cohort



(a) Natives

(b) Immigrants

Figure A.3. Municipality density, by income quintiles

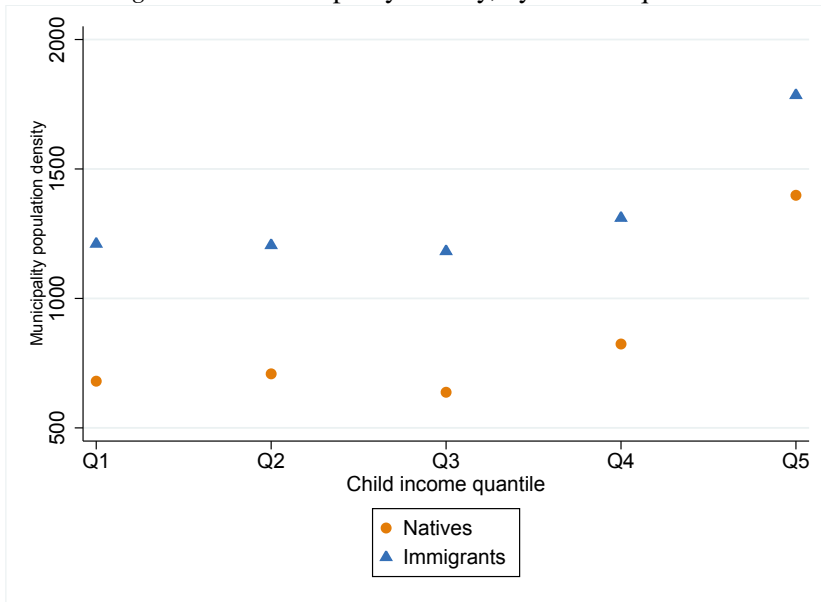


Figure A.6. Share of children in the top 3 municipalities

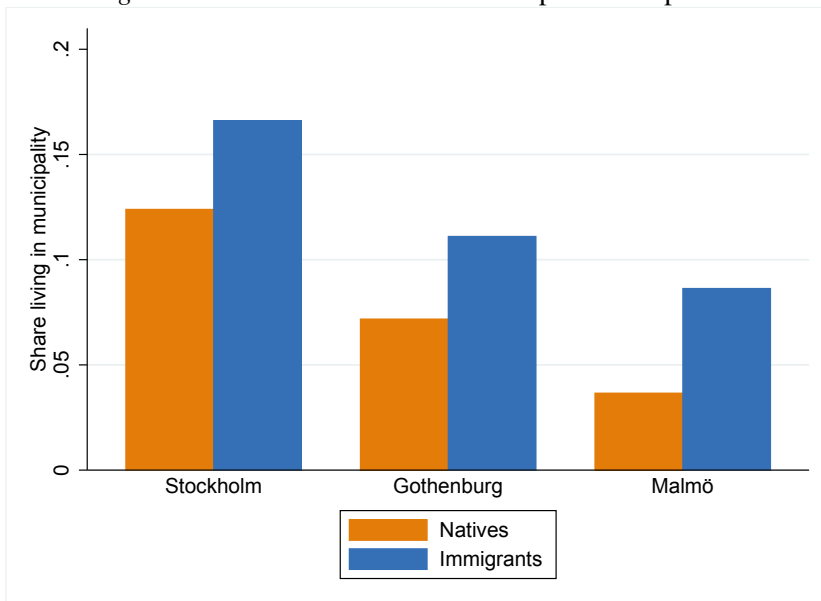
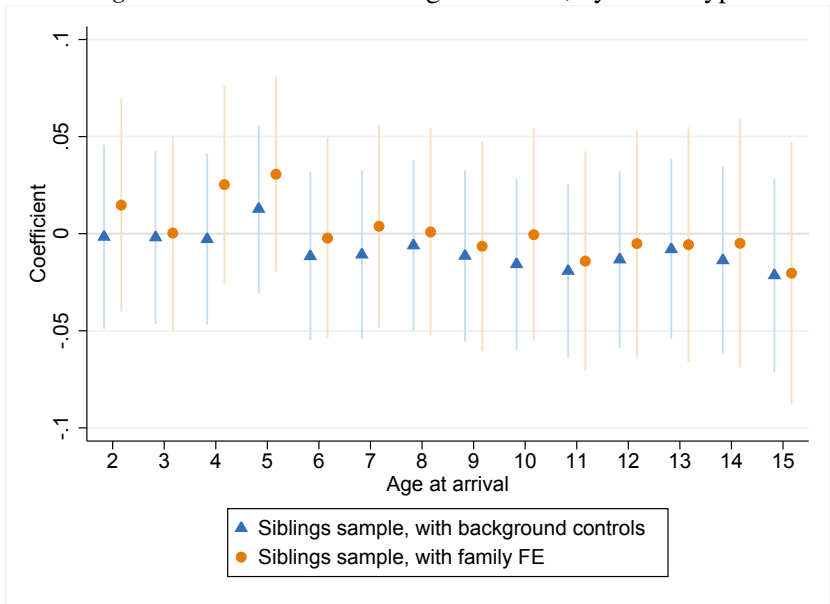
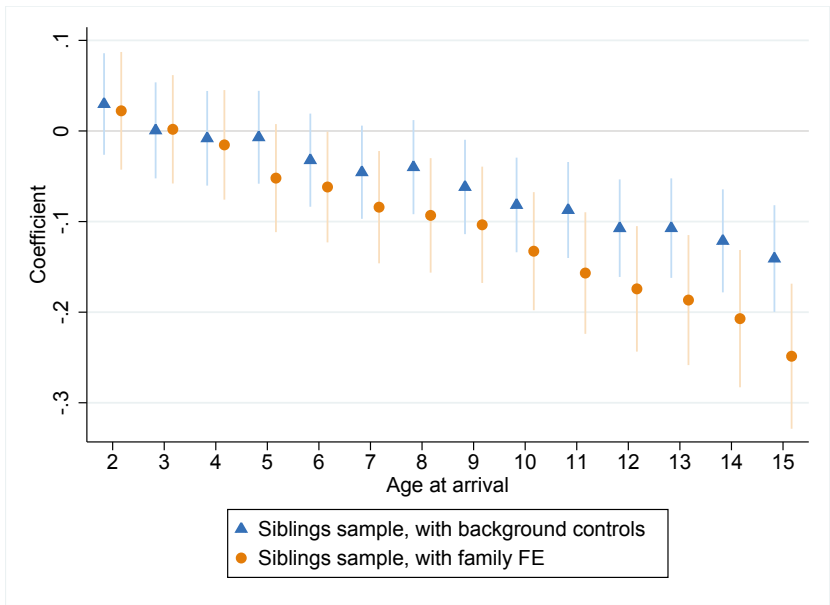


Figure A.7. Coefficients on age at arrival, by tenure type



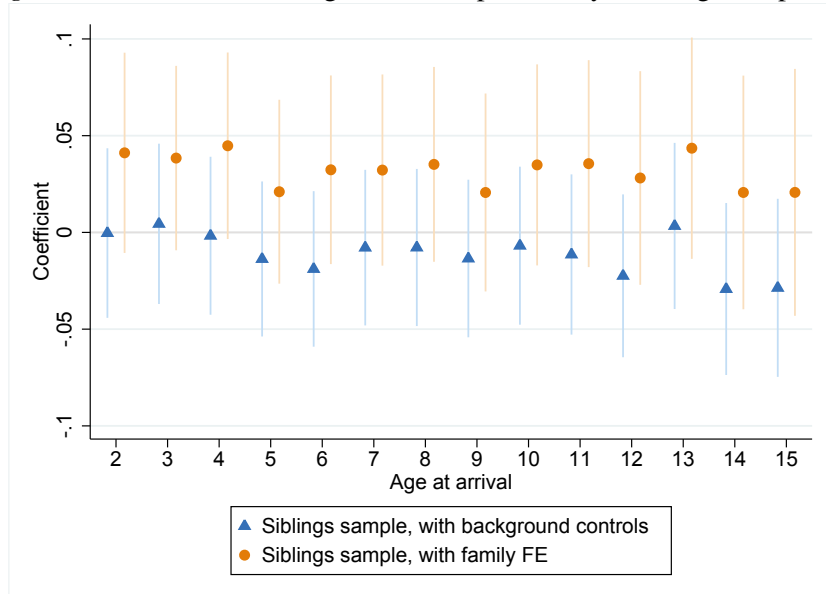
(a) Probability of living in owner-occupied housing



(b) Probability of living in tenant-owner cooperative housing

Notes: The figures plot the coefficients on the age at arrival dummies obtained from estimating equation 4.1 (in blue) and equation 4.2 (in orange), on probability of living in owner-occupied housing and tenant-owner cooperative housing, respectively. Both outcomes are measured at age 30. All estimations are done on the siblings sample.

Figure A.8. Coefficients on age at arrival, probability of living with parents



Notes: The figure plots the coefficients on the age at arrival dummies obtained from estimating equation 4.1 (in blue) and equation 4.2 (in orange), on the probability of living with parents at age 30. Both estimations are done on the siblings sample.

## Exposure to renters during childhood

The results in Section 4.4.2 suggest that income does not fully explain the home ownership patterns of immigrants who spend their childhood in Sweden. In this section, we test the hypothesis that the composition of renters and owners among co-nationals in the neighborhood one grows up in affects the propensity to rent or own in adulthood. Bertrand et al. (2000) argue that social networks affect behavior through norms and information. In our context, we might observe peer effects either because an individual's own preferences for renting or owning are shaped by their peers' behavior; or because the individual is surrounded by people that are more or less knowledgeable about how to go about buying an apartment, for example.

Residential location is, however, not randomly allocated. Parents who locate in areas with more home owners might have other characteristics that also affect their children's ability to own later in life. In order to circumvent this issue, we take advantage of the refugee dispersal policy that was implemented in Sweden starting with the late 1980s and through the mid-1990s.<sup>31</sup> We argue that this policy experiment provides us with exogenous variation in residential location. This allows us to further exploit variation in the share of

<sup>31</sup>For a comprehensive description of the policy and its implementation, see Edin et al. (2003).



co-nationals who rent or own in the neighborhood to which refugees were - from the refugees' perspective - exogenously allocated.<sup>32</sup>

### Sample restrictions and descriptive statistics

To get at the effect of neighborhood composition on later-life outcomes, we do the following exercise. We select all immigrants whom we can classify as refugees and who further arrived in 1990.<sup>33</sup> For each neighborhood, we calculate exposure measures, where exposure is defined as the share of co-nationals who rent among all the co-nationals in the neighborhood. To understand if the sheer size of the group matters, we include a measure of the size of the ethnic group in our regressions as well.

**Table A.1.** *Summary statistics on refugees who arrive in 1990*

|  | Mean   | Std. dev. | No. of obs. |
|--|--------|-----------|-------------|
| <b>Panel A: Family background</b>            |        |           |             |
| Parent percentile income rank                | 12.59  | 17.34     | 3,513       |
| At least one parent with college or above    | 29.87  | n/a       | 3,411       |
| Number of residents in building              | 171.05 | 196.40    | 3,285       |
| <b>Panel B: Neighborhood characteristics</b> |        |           |             |
| Share co-nationals who rent                  | 67.49  | n/a       | 3,281       |
| Share who rent in neighborhood               | 43.47  | n/a       | 3,285       |
| Share co-nationals who own                   | 15.73  | n/a       | 3,281       |
| Share who own in neighborhood                | 43.90  | n/a       | 3,285       |
| Share co-nationals who are low-earners       | 79.22  | n/a       | 3,278       |
| <b>Panel C: Individual characteristics</b>   |        |           |             |
| Percentile income rank                       | 38.82  | 31.09     | 3,513       |
| College or above                             | 36.57  | n/a       | 3,489       |
| Married or cohabiting                        | 42.58  | n/a       | 3,513       |
| Any children                                 | 57.22  | n/a       | 3,513       |
| Number of children                           | 1.06   | 1.14      | 3,513       |
| Share who live with parents                  | 15.06  | n/a       | 3,513       |
| Share who rent                               | 33.87  | n/a       | 3,513       |
| Share who own                                | 40.22  | n/a       | 3,513       |

In Table A.1 we show summary statistics for the refugees in our sample. From Panel A, we highlight the fact that refugees live in multi-dwelling buildings with quite a large number of residents. On average, the share of co-nationals who rent is high, and much higher than the share who rent in the neighborhood as a whole. Figure A.9 shows the variation in exposure to renters by SAMS. As the average statistics suggested, a significant share of

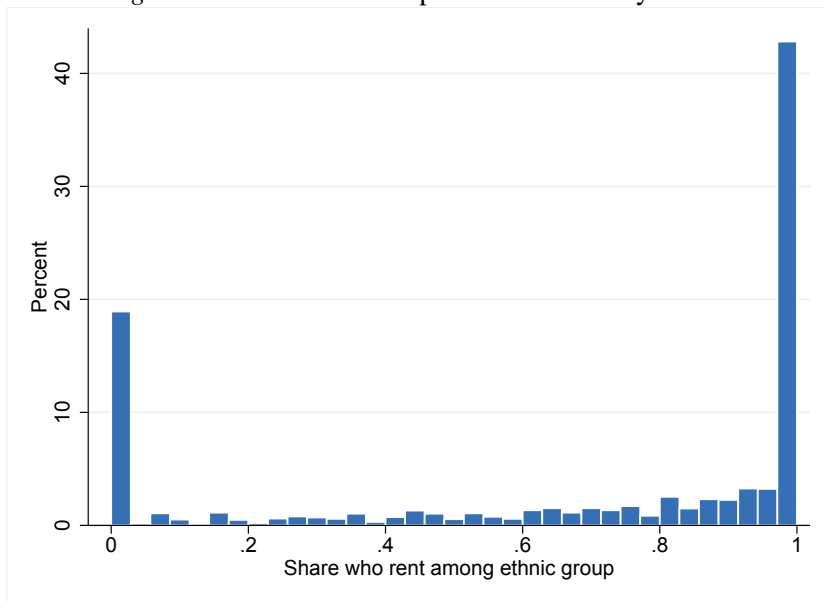
<sup>32</sup>We consider the SAMS (Small Area Market Statistics) as our neighborhood measure. There are around 9000 SAMS in total in Sweden, each with around 1000 inhabitants.

<sup>33</sup>As documented in Edin et al. (2003), the policy was implemented in its strictest form up to 1991; however, our data do not contain housing information for 1991.

individuals grew up in neighborhoods where all co-nationals lived in public housing. Nonetheless, there is some variation in the share who rent, although it is concentrated on the right-hand side of the distribution.

Table A.1 also shows that when it comes to housing outcomes in adulthood, around 34% are renters and 40% are owners.

Figure A.9. Variation in exposure to renters by SAMS



### Empirical strategy

We estimate the following reduced-form equation on the sample of refugees who at 30 years old do not live with their parents or in “other” types of housing:<sup>34</sup>

$$rent_{ics} = \alpha + \beta size_{cs} + \gamma exp_{cs} + \theta \mathbf{X}_i + \phi_c + \sigma_s + \varepsilon_{ics} \quad (4.3)$$

where  $size_{cs}$  is the size of the ethnic group and  $exp_{cs}$  is the exposure measure, calculated as the share who rent among individuals from country of origin  $c$ , out of all individuals from country of origin  $c$  who live in SAMS  $s$ .<sup>35</sup> We include a full set of country of origin dummies,  $\phi_c$  and SAMS of arrival fixed effects ( $\sigma_s$ ). We control for the following covariates in  $\mathbf{X}_i$ : gender, mar-

<sup>34</sup>We make this restriction so as to not capture a mechanical effect of renting by virtue of living with parents who rent.

<sup>35</sup>Community size is calculated with the formula  $size_{cs} = \frac{n_{cs}/n_s}{n_c/n}$ , where  $n_{cs}$  is the number of individuals from country of origin  $c$  in SAMS  $s$ ;  $n_s$  is number of individuals in SAMS  $s$ ;  $n_c$  is number of individuals from country of origin  $c$  in the population and  $n$  is the size of the population. To facilitate interpretation, we take logs of both the size and the exposure measures.

ital/cohabiting status, number of children, college or above attainment, percentile income rank, family background (family income rank, family college or above).<sup>36</sup>

## Results

**Table A.2.** *Probability of renting at age 30*

|  | (1)               | (2)               |
|--|-------------------|-------------------|
| Size ethnic group  | -0.009<br>(0.033) | -0.016<br>(0.034) |
| Share who rent among ethnic group                        | 0.157<br>(0.054)  | 0.247<br>(0.123)  |
| Size ethnic group ×<br>share who rent among ethnic group |                   | -0.036<br>(0.034) |
| Observations   | 1,349             | 1,349             |

*Notes:* Regressions estimated on the sample of children who do not live with their parents or in other types of housing at age 30. All regressions include controls for gender, marital/cohabiting status, dummies for number of children, college or above attainment, income percentile rank, family background (family income percentile rank, family college or above), SAMS fixed effects, country of birth fixed effects. Standard errors in parentheses and clustered at the SAMS level.

We show the results from equation 4.3 in Table A.2. The coefficient in column (1) implies that a 10% increase in the share of renters among ethnic group increases probability to rent in adulthood by 1.6 percentage points. The size of the community does not seem to matter. However, we might think that the exposure effect varies with the size of the community. In column 2, we add an interaction term between the size of the ethnic group and the exposure measure. The coefficient on the exposure measure increases substantially while the coefficient on the interaction is negative and insignificant.

We interpret these findings as evidence that there are peer effects in the decision to rent. It could be argued that the effect we observe is not due to exposure to renters per se, but due to exposure to low-earners. If we estimate equation 4.3 using instead exposure to low-earner co-nationals as our explanatory variable, we get a coefficient that implies that a 10% increase in the share of low-earner co-nationals results in a 0.4 percentage point increase in the probability to rent in adulthood (insignificant).<sup>37</sup> This is a smaller effect than what we observed in Column 1 of Table A.2, which suggests that the effect is not only explained by economic factors.

<sup>36</sup>This specification is similar to the one estimated in Bertrand et al. (2000) and Åslund and Fredriksson (2009) to get at peer effects in welfare dependence.

<sup>37</sup>Low-earners are defined as earning below the median in the SAMS. Results available upon request.



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