International mobility and the labor market

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Abstract
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This thesis consists of four self-contained essays.

Essay 1 (with Olof Åslund): We study the labor market impact of opening borders to low-wage countries. The analysis exploits time and regional variation provided by the 2004 EU enlargement in combination with transport links to Sweden from the new member states. The results suggest an adverse impact on earnings of present workers in the order of 1 percent in areas close to pre-existing ferry lines. The effects are present in most segments of the labor market but tend to be greater in groups with weaker positions. The impact is also clearer in industries which have received more workers from the new member states, and for which across-the-border work is likely to be more common. There is no robust evidence on an impact on employment or wages. At least part of the effects is likely due to channels other than the ones typically considered in the literature.

Essay 2: I study demand shifting effects of real exchange rate movements in border regions. Detailed geographic information on border crossings, the location of retail outlets, and where the population resides, allows me to explore the labor market effects of cross-border shopping. The impact is identified by comparing areas located close to the border with more remote areas. The relative effects are large; a ten percent decline in the value of the Swedish krona is followed by an increase in the number of employees in the retail industry by 3 percent. Similarly, the share of the population employed in retail increases by 0.3 percentage points and annual earnings by 2.7 percent.

Essay 3 (with Olof Åslund): We study the effects of performance bonuses in immigrant language training for adults. A Swedish policy pilot conducted in 2009–2010 gave a randomly assigned group of municipalities the right to grant substantial cash bonuses to recently arrived migrants. The results suggest substantial effects on average student achievement. But these were fully driven by metropolitan areas; in other parts of Sweden average performance was more or less unaffected. In line with theory, effects tend to be clearer where institutional features make the bonus more feasible, or where student characteristics suggest that the costs should be lower.

Essay 4: I study the association between naturalizations, labor market outcomes and family formation. The results show that the economic outcomes of immigrants from outside the OECD, on average, improve following naturalization. A strict causal interpretation of the results is not possible as the outcomes start to improve already before the acquisition of citizenship. The study also shows that for migrants from some country groups there is a positive correlation between naturalizations and the likelihood of getting married and having children. This is suggestive of immigrants naturalizing for family reasons. Further, my findings illustrate that modeling assumptions are of great importance. Models that are not flexible enough could lead to false claims regarding causality.

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Acknowledgements

The deadline for handing in this thesis is approaching fast. It has been quite a journey to get here. The last couple of years are full of happy memories but also periods of great uncertainty. At times I have felt like Sargent in James Joyce's Ulysses:

> "Sitting at his side Stephen solved out the problem. He proves by algebra that Shakespeare's ghost is Hamlet's grandfather. (…) In long shaky strokes Sargent copied the data. Waiting for a word of help his hand moved faithfully the unsteady symbols (…)".

Nevertheless, it has been a highly enlightening time. Partly so because that is what research is all about, but also because the Department of Economics and the Institute for Evaluation of Labour Market and Education Policy in Uppsala gives plenty of opportunities to interact with talented researchers.

First and foremost I would like to thank my supervisors Per-Anders Edin and Olof Åslund. Per-Anders, I first met you when I wrote my undergraduate thesis, and your inspiration was one of the reasons why I applied to the PhD program. Ever since your approach to economics has been a great guidance. Olof, at times we have worked closely. It has been very valuable to me, I have learned a lot. I am also thankful to you for always dedicating time to me when needed.

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Contents

Introduction

The individual decision to migrate has consequences. Not only for the migrant but also for the sending and the receiving country. For the migrant, moving abroad could be the most welfare enhancing decision during her lifetime. Whether the sending country gains or loses is an open question. The country might lose valuable competence but at the same time recoup this loss through e.g. remittances. Migration is also likely to affect the receiving country, including the labor market prospects of the already present population. No matter the consequences it is a fact that more people than ever live abroad (UN 2013). Immigration has also become one of the most contested topics in the public debate. Similarly, it is the core of a fast expanding academic literature and the uniting theme of this thesis.

The thesis consists of four self-contained essays. It contributes to two broad fields in migration research, but also to labor economics more generally. The first theme concerns labor market effects of international mobility and the second considers the integration of the foreign-born. I use the themes to divide the contents of the thesis into two parts.

The introduction continues as follows: In Section 1, the first part of the thesis is outlined. A simple framework to illustrate the potential impact of immigration on receiving countries is presented. In the section I also discuss the two first essays in the thesis. The first essay – *Open borders, transport links and local labor markets* (with Olof Åslund) – explores local labor market impacts of the EU enlargement in 2004. The second essay – *Foreign demand and border regions* – is a study of labor market effects of cross-border shopping in border regions.

In Section 2, the second part of the thesis is introduced. It starts with a short discussion about the recent Swedish migration experience and mechanisms that can explain the immigrant-native wage gap. The section also includes a presentation of the third and the fourth essay. The third essay – *The value of earning for learning: Performance bonuses in immigrant language training* (with Olof Åslund) – is an evaluation of a unique experiment conducted within the Swedish language tuition for immigrants. The purpose of the experiment was to test whether the introduction of financial incentives would improve student achievement. In the fourth and last essay – *Naturalizations and the economic and social integration of immigrants* – I study the labor market effects of acquiring Swedish citizenship.

1. Effects of International Mobility

From a simple economic framework it is straightforward to derive predictions of how immigration affects receiving labor markets. The starting point of the following discussion is the one-sector model.[1] This model builds on the assumption that the host country has one industry that produces a single output good. Both labor and capital is used in the production process. To simplify we also assume that the supply of labor is inelastic, i.e. that workers choose to work no matter the wage, and that capital is perfectly elastic.

The way immigration enters this type of model depends on the production technology. Given that two types of workers – skilled and unskilled – are required in the production process, what determines the labor market response of immigration is the relative supply of native and foreign workers within each skill group.[2] If the migrant inflow perfectly matches the composition of the domestic workers no labor market impact is predicted. The scale of the economy would simply increase. On the other hand, if the skillset differs from the native workforce the labor market will have to adjust before reaching a new equilibrium.

For simplicity, we turn to an example of a large inflow of unskilled workers. There are several mechanisms through which an economy can respond to this type of supply shock. In the framework just presented adjustment will occur through wages. The wages of the low skilled will fall, at least in the short run, due to the excess supply of this type of workers. In contrast, the wages of the high skilled will rise as they become relatively scarce in the economy. Thus, the impact of immigration is predicted to differ between groups. As there are winners and losers, the average wage effect is predicted to be small (Dustmann, Glitz and Frattini 2008).

What is critical for these predictions are the assumptions about capital mobility and elasticity of labor. If capital cannot adjust, immigration can have long-run effects redistributing incomes from low skilled workers to not only the highly skilled but also to capital (Dustmann, Glitz and Frattini 2008). In addition, if we relax the assumption that labor is perfectly inelastic, i.e. that workers work for any give wage rate, some workers might respond to falling wages by leaving the labor force altogether.

Further, the impact of immigration is not limited to wage or employment effects. Departing from the static one-sector model the economy can respond to immigration by adjustments of the industry structure and output mix (Dustmann, Glitz and Frattini 2008). Increased production of goods requiring low skilled workers could be one such response. That is, instead of

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[1] The discussion in this section builds on Dustmann, Glitz and Frattini (2008) and Dustmann et al. (2006).
[2] In the simplest model we assume that native and foreign workers within each skill group are perfect substitutes.

falling wages, an economy can absorb labor supply shocks by the growth of sectors that uses unskilled labor. The economy could also respond by technological change, i.e. industries can choose production technologies that are more intensive in the use low-skilled labor (Dustmann, Glitz and Frattini 2008).

From this discussion it should be clear that it is differences in the underlying assumptions of the theoretical models that determine what conclusions can be derived from theory. That the restoration of equilibrium involves at least short run changes in wages is straightforward. The long run effects are on the other hand more difficult to model, Dustmann et al. (2006) neatly summarizes the question at stake:

> "Models assuming limited flexibility of output mix or closedness to international trade tend to predict that immigration will have long run wage and employment effects. (…) On the other hand, models assuming sufficiently high degree of flexibility in output mix and openness to trade predict an absence of long run effects on labor market outcomes, at least to small scale migration" (p. 11).

Despite these theoretical predictions it has turned out to be very difficult to reach a consensus of how to best estimate the impact of immigration on the labor market. Those concluding that the impact is probably limited include e.g. Card (2005). Other researchers find that immigration significantly damages the labor market prospects of natives (see Borjas, Grogger and Hanson, 2008, 2010, and Borjas, 2003).

In a survey of the recent literature, Pekkala Kerr and Kerr (2011), conclude that most studies suggest that the impact of immigration on native labor market outcomes is likely to be limited. They do however point out that there is a growing body of evidence that shows that the effects vary across the wage distribution. E.g. Dustmann, Frattini and Preston (2013) find that immigration depresses wages at lower parts of the distribution, but slightly increases them in the upper part. Similarly, Bratsberg et al. (2010) find that the wage impact of immigration depends on the region of origin of the migrant inflow, with bigger influence for workers from neighboring countries who are likely to be closer substitutes to native workers.

To further complicate matters, immigration or more generally open borders, may affect the labor market of present workers through a number of channels, e.g.:

i. the number of migrant workers (permanent, temporary, posted, irregular), the channel just discussed;
ii. the overall economic activity (investments, firm location decisions, passenger and commercial transportation, tourism);
iii. the trade of goods and services;
iv. the bargaining power of employers and unions through potential competition.[3]

These mechanisms are of concern for the first essay in this thesis – *Open borders, transport links and local labor markets* – a study of the labor market impact of the enlargement of the European Union in 2004. Eight countries – Latvia, Lithuania, Estonia, Poland, the Czech Republic, the Slovak Republic, Hungary, Slovenia, Malta and Cyprus – had met the criteria for membership and joined the union that year. In the debate leading up to the enlargement widespread concerns were raised about the adverse labor market effects that the potential influx of immigrants could bring. The worries were based on the large income and employment discrepancy between the old and the new member states; furthermore, in contrast to earlier enlargements there was no history of free migration between Eastern and Western Europe.

As expected, the migration flow from the new member states increased after the enlargement. While Ireland and the UK received the largest share of migrants in relation to population size (Commission of the European Communities 2008) the inflow to Sweden was more modest, despite the open door policy (Gerdes and Wadensjö 2010).

The study exploits time variation provided by the 2004 EU enlargement in combination with transport links to Sweden from the new member states. Some regions had pre-existing ferry links to Poland or the Baltics, or both, and we compare outcomes in areas located close to the ferry ports with areas located somewhat further away. The results suggest an adverse impact on earnings of present workers in the order of 1 percent. Moreover, the effects are present in most segments of the labor market but tend to be greater in groups with weaker positions. The impact is also clearer in industries which have received more workers from the new member states, and for which across-the-border work is likely to be more common.

We find no strong evidence of migrants from the new member states clustering close to the ports, although we cannot observe e.g. posted workers, in our data. Thus, at least part of the effects is likely due to

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[3] The discussion in this section is limited to labor market effects. Migration is however likely to affect also other dimensions of society. For example, the house prices in cities attracting many migrants might be affected (see Saiz and Wachter 2011, Saiz 2007 and Ottaviano and Peri 2006). Immigration potentially also causes segregation (see Card, Mas and Rothstein 2008). In recent research the effects of immigration on crime has also received some attention (e.g. Bell, Fasani and Machin, 2012, and Butcher and Piehl, 1998).

channels other than the immigration channel typically considered in the literature.

Our findings are in line with the collected evidence of at most modest adverse effects of the enlargement. E.g. some UK studies point in the direction of small or insignificant effects on native labor market outcomes (see Reed and LaTorre, 2009, Lemos and Portes, 2008, Gilpin et al. 2006, and Portes and French, 2005).

The starting point of the second essay in the thesis – *Foreign demand and border regions* – is that not only do people cross borders to resettle permanently (or temporarily); people pass international borders for a number of reasons. Some on a daily basis. Examples include cross-border commuters, holiday makers, and those that pass a border to go shopping. In the study I explore the impact cross-border shopping on labor markets in border regions. Currency fluctuations changes relative prices between countries and there are plenty of studies showing that consumers respond to price differences by shifting part of their expenditures to where relative prices have fallen (e.g. Asplund, Friberg and Wilander, 2006, Tosun and Skidmore, 2007, and Manuszak and Moul, 2009). While the elasticity of demand has received a lot of attention in the literature, the effects of demand shifts on local labor markets is more unexplored. Consequently this is the focus of this study.

The economic activity in border regions is often oriented towards the neighboring economy. One example is the cluster of manufacturing firms on Mexican side of the US-Mexican border that has been the focus of a number of studies (e.g. Hanson, 1996, 1998, 2001), effects of the NAFTA agreement on retailers along the same border has also been studied by, e.g. Ford, Logan and Logan (2009) and Adkisson and Zimmerman (2004). Further, survey and anecdotal evidence suggests that retail firms and restaurants in border regions often rely on foreign customers (e.g. HUI 2011, Gerber and Patrick 2001). This makes border regions in particular vulnerable to exchange rate fluctuations. A better understanding of the forces that drive the economic development in border regions should, thus, be of relevance for policy.

The labor market effects of demand shifts in border regions relate to mechanism (ii) and (iii) above, i.e. the overall economic activity is likely to be affected as well as the trade of goods and services. Sweden shares its longest land border with Norway and cross-border shopping along this border has since long been a frequent phenomenon. Detailed geographic information on border crossings, where the population resides, and the location of establishments, allows me to explore effects of relative price changes on the retail industry. To identify the effects of annual exchange rate fluctuations, areas located close to the border are compared with areas located further away. The analysis demonstrates that the number of employees, annual earnings and the share of the population employed within the retail industry increase following currency depreciations.

The study relates closely to two U.S.-Canadian studies. Campbell and Lapham (2004) show that sales and average employment in the retail and restaurant industries in border regions are positively affected by relative price falls caused by real exchange rate depreciations. Further, Baggs et al. (2013), study firm level responses and find that firm sales, the average number of employees, profitability and survival in the retail industry, increases following a decline in the value of the domestic currency.[4]

In policy terms it can be worth reflecting on the fact that the value of the Swedish krona is unlikely to be affected by cross-border shopping as these flows are small in comparison with e.g. the overall trade between Sweden and Norway. Thus, from the perspective of the affected regions, changes in relative prices caused by real exchange rate fluctuations are exogenous. This illustrates the increasing vulnerability of the border area as it, over time, has become more dependent on cross-border shopping.

2. Integration of the Foreign-Born Population

The economic assimilation of immigrants is probably one of the most studied questions in migration research (e.g. Barth, Bratsberg and Raaum 2012, Lubtosky 2007 or LaLonde and Topel 1997 for an overview). Swedish and international evidence shows that migrants, on average, never fully catch up with the native population (e.g. Borjas 2013 and Eliasson 2013). There is also evidence of occupational downgrading, i.e. migrants taking jobs that they are overqualified for (Dustmann and Preston 2012 and Dustmann, Frattini and Preston 2013).

Table 1 shows that the unemployment rate among foreign-born women and men is around ten percentage points higher than for the Swedish-born population. It also shows that there are large differences in the employment rate between the native and foreign-born population with particularly low rates for foreign-born women.[5]

Over the last four decades, the foreign-born population in Sweden has grown steadily, from 6.7 percent of the total population in 1970 to 15.5 percent in 2013. The composition of the migrants changed during the same time period. Labor migration dominated the period following the Second World War until the late 1970s. This period was followed by a gradual shift toward immigration of humanitarian character. From the late 1980s refugee

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[4] The study is, of course, also related to the more general literature on demand shocks. For example, there are a large number of studies, theoretical and empirical, using exchange rate movements or trade liberalizations policies to study how sectors exposed to foreign competition are affected (e.g. Ekholm, Moxnes and Ulltveit-Moe, 2012, Gourinchas, 1999, and Melitz and Ottaviano, 2008).

[5] The variation in outcomes between (and within) migrant groups is of course also large, with migrants born in western countries generally performing better than migrants born elsewhere.

migration and immigration for family reunification have been the predominant forms of migration to Sweden.

Parallel to the compositional change of the migrants to Sweden the relative labor market performance of the foreign-born started to deteriorate. Sweden is now one of the OECD countries with the highest relative foreign-born to native-born unemployment rates.

Table 1. Labor market outcomes

	Employment	Unemployment rate
Native-born men	77.9	6.1
Foreign-born men	68.0	16.0
Native-born women	75.1	5.9
Foreign-born women	57.5	15.9

Notes: The data source is OECD (2013). The measures are from 2011. Employment is defined as persons in employment divided by the population. Unemployment refers to the number of persons that are unemployed divided by the labor force.

A common explanation of why migrants, on average, initially experience worse labor market outcomes than natives is that they lack country specific human capital. The acquisition of such skills is commonly also the predominant explanation of the relatively fast increases in income of migrants during the first years in receiving countries (e.g. Borjas 1999, LaLonde and Topel 1997 for overviews, or Friedberg 2000, Bratsberg and Ragan 2002, and Berman, Lang and Siniver 2003).

Apart from the lack of country-specific human capital, another problem that migrants face is the difficulty of correctly signaling skills. For employers it can be difficult to value foreign merits (degrees, labor market experience, etc.). This suggest that there might be sorting on the labor market, i.e. only certain types of employers are willing to employ newly arrived migrants. Recent evidence from Sweden e.g. shows that immigrant managers are much more likely to hire immigrants than native managers, (Åslund, Hensvik and Nordström Skans, forthcoming).

New migrants are also likely to lack networks that could provide information about vacancies. Further, networks could likewise provide information to employers about the quality of workers through recommendations and references. This could be of great importance in the hiring process (e.g. Kramarz and Nordström Skans (forthcoming), Dustmann, Glitz and Schönberg 2011, and Cingano and Rosalia 2008). Yet another explanation is that the preferences of some employers could disfavor the foreign-born. Some employers might be unwilling to hire migrants. Such preferences could be based on the perception that workers with a dissimilar background might lower productivity (e.g. Lazear 1999) or the result of discrimination (e.g. Oettinger 1996 and Altonji and Pierret 2001).

In the economic literature it is well-established that periods of unemployment, apart from the direct income loss, have long run negative effects on future labor market outcomes (e.g. Arulampalam, Gregg and Gregory 2001, Gregg 2001 and Jacobson, LaLonde and Sullivan 1993). Also other types of outcomes are likely to be affected, including the well-being and health status of the unemployed (see Machin and Maning, 1999, for an overview). Thus, unemployment comes with an individual price tag. It also induces costs for society as it put pressure on public finances.

Therefore (and probably due to other reasons as well), new immigrants to Sweden are offered a range of activities that aim to introduce them to society and to ease the transition to the labor market. These policies include introduction programs offered by the municipalities, education, and activities at the employment offices, among other things. A specific example is basic language training that is offered to migrants. These types of programs exist in many countries since language skills are often a requisite to find a job.[6] In general, the offered activities are often conditional on the type of residence permit that the migrants have. Thus, some of the programs are exclusive for refugees and subsequent family members.[7]

That said, and in the light of a widening foreign-native employment gap the last decades, policies to address these issues should be of high interest. In the third essay – *The value of earning for learning: Performance bonuses in immigrant language training* – one such policy is evaluated. A policy pilot conducted in 2009-2010 gave a randomly assigned group of municipalities the right to grant substantial cash bonuses to recently arrived migrants that participated in language training programs. The idea was to test whether the take-up of language skills was affected, as proficiency in the majority language is likely to ease the transition to the labor market. The previous literature suggests that economic incentives may be an effective tool but that it is far from certain that financial rewards has an effect in all settings.[8] In relation to the earlier literature the experiment analyzed in this essay is unique in the sense that it tests how financial rewards affect basic human capital accumulation among adults and because it is an incentive program targeted at immigrants.

The analysis shows that the introduction of performance bonuses had a considerable positive effect on student achievement. The effects, however, were concentrated to the metropolitan areas in the sample. Further, we find

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[6] The literature contains abundant evidence that migrants who master the dominant language of the destination country have higher employment rates and earnings than migrants who lack such skills. Some recent studies include Bleakley and Chin (2004), Dustmann and Fabbri (2003) and Dustmann and Van Soest (2002).

[7] See Erikson, Nordström Skans, Sjögren and Åslund (2007) for a discussion about some of the policies that are offered to Swedish migrants and e.g. Eriksson (2011) for an overview of studies on immigration and ethnicity in the Swedish labor market.

[8] See Rodríguez-Planas (2010) for an overview.

that the effects appear to be dependent on the perceived feasibility of reaching the bonus. The criteria for earning the bonus were strict relative to the average results within the tuition system and we find a greater impact on shorter courses. This is also where we find students with stronger educational backgrounds. Similarly, we see greater effects among the young, whose learning costs are likely to be lower. Moreover, this group arguably values the financial rewards higher. Finally, the analysis also shows that a relatively quick course start, to a large extent, was a requisite for receiving a bonus. Fast course starts were offered to a higher extent in the metropolitan areas (where we find effects). Thus, institutional differences can potentially partly explain why the impact was limited to larger cities.

The last essay in the thesis – *Naturalizations and the economic and social integration of immigrants* – studies the impact of naturalizations on labor market outcomes of migrants. Evidence from the US, Germany and France has shown that labor market outcomes of migrants improve following naturalizations (Bratsberg et al. 2002, Steinhardt 2012, Fougère and Safi 2009). One proposed explanation to why naturalized citizens experience these positive outcomes is that they move into better jobs (Bratsberg et al. 2002). For example, before naturalization, jobs that require citizenship are off limits. In addition, if employers perceive the choice to naturalize as a positive signal this could potentially also enhance labor market opportunities (e.g. OECD 2011).

More generally, citizenship is a legal status that formally regulates whom has the right to live in a country, enter the country freely and not to be deported. In some countries citizenship also determines the access to welfare, health and education services. Thus, a better understanding of the implications of acquiring citizenship should be of relevance for policy. This paper provides new insights into this stream of research. The main emphasis is on effects on labor market outcomes but it also contributes with unique evidence on the timing of the formation of families which cast new light on why immigrants decide to apply for citizenship.

The paper demonstrates that the economic outcomes of immigrants from outside the OECD, on average, improve following naturalizations. The results cannot be interpreted causally as the change partly precedes the acquisition of citizenship. Further, a positive correlation between naturalizations and the likelihood of becoming married and having children is found for some country groups. The latter results illustrate that naturalizations potentially are associated with a number of factors, not necessarily correlated with improved labor market outcomes. This is potentially important as other shocks (observed and unobserved) that coincide in time with naturalizations would bias estimates of the so called citizenship premium, if not properly accounted for.

In sum, the two studies in this part of the thesis provide new evidence that could be used as guidance for policy. The evaluation of the experiment

within the Swedish language tuition for immigrants gives some hints of when economic incentives work to improve student achievement. Further, the latter study illustrates that the citizenship legislation could be used as a policy tool as most migrants chose to naturalize. It is nevertheless difficult to predict the consequences of changing the requirements for naturalizations as it potentially would change both the pool of naturalized citizens as well as the related outcomes.

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Essay 1. Open Borders, Transport Links and Local Labor Markets*

Co-authored with Olof Åslund

1. Introduction

Immigration policy is a hotly debated and multifaceted issue. A central dimension of the discussion considers the consequences of opening borders to low-wage countries. This issue is at the heart of the debate on the EU enlargement, and also relates to the US and European strategies for handling immigration pressure on the south borders. This paper investigates the labor market impact of immigration reform leading to increased exposure to countries with relatively lower wages and levels of economic development. The empirical analysis exploits variation provided by the 2004 EU enlargement in combination with pre-existing ferry lines between Sweden and new member states. Since the regions served by the ferry lines in the new member states are highly populated, the market exposure and potential immigration flow to Sweden was large.[1]

Opening a border may affect the labor market of present workers through a number of channels, e.g.: (i) the number of migrant workers (permanent, temporary, posted, irregular); (ii) overall economic activity (investments, firm location decisions, passenger and commercial transportation, tourism); (iii) the trade of goods and services; (iv) the bargaining power of employers and unions through potential competition. Some of these are hard if not impossible to capture in data. In other words, the analysis is almost by definition at least partly of a reduced-form character.

Mechanism (i)—the labor market consequences of inflows of migrant workers with certain characteristics—has been the topic of a rapidly expanding literature during the last decade. There is no consensus on the presence or magnitude of detrimental effects on present workers and there is an ongoing debate on how to best measure these effects. Those concluding that the impact is probably limited include e.g. Card (2005). Even more positive views are expressed by e.g. Ottaviano and Peri (2012) who argue that the native population could actually gain from immigration in the long run. However, other relatively recent studies conclude that immigration imposes significant harm to the labor market prospects of natives (see Borjas, Grogger and Hanson, 2008, 2010; Borjas, 2003). There is also recent Scandinavian evidence pointing toward negative wage effects from immigration. Bratsberg and Raaum (2011) show slower wage growth for occupations more exposed to immigration than for other occupations within the construction industry.

Pekkala Kerr and Kerr (2011) survey the literature and conclude that most studies suggest that the adverse labor market impact for natives is likely to be limited, but more pronounced for low-educated workers and previous migrants (who, the authors argue, are close substitutes to the current

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[1] The period preceding the enlargement, the wage gap between the new and the old member states was as large as the gap between the US and Canada and Mexico when the NAFTA agreement was signed (Boeri and Brucker 2001).

immigration flows seen in Europe). Heterogeneous impacts is also the result in Dustmann, Frattini and Preston (2013) who find that immigration depresses wages at lower parts of the distribution, but slightly increases them in the upper part of the wage distribution. Furthermore, Bratsberg et al. (2010) find that the wage impact of immigration depends on the region of origin of the inflow, with bigger influence for workers from neighboring countries who are likely to be closer substitutes to native workers.[2]

As for mechanisms (ii) and (iii), regions closer to the new markets may benefit from increased economic activity resulting from the opened borders. Proximity to new markets could affect location decisions of firms (see Niebur and Stiller, 2002, for a survey). One example is the experience from the North American Free Trade Agreement, where Mexican manufacturing industry reallocated towards the US border, which in turn resulted in an increase in manufacturing employment in US border cities (see Hanson, 1996, 1998, 2001).

On the other hand, the positive impact of increased activity on individual labor market opportunities might be counteracted by increased competition in the products and services markets. Increasing competition by opening a border to a country with lower average wages may put pressure on native wages even though actual migration flows are not much affected, i.e. through mechanism (iv). A credible threat of finding services or labor abroad may be enough to influence the outcomes. Tentative evidence supporting this idea is found in e.g. Blanchflower and Shadforth (2009) who study the effects of the EU enlargement on the UK economy. Such effects are also likely to be larger in local labor markets closer to the new competition.

Free movement of production factors, goods and services is perhaps the most important cornerstone of the European Union. The expansion on May 1st, 2004, meant that ten new states joined the European Union. Eight were Central or Eastern European countries (the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, the Slovak Republic, and Slovenia (hereafter labeled EU8 countries)) and two Mediterranean countries (Cyprus and Malta). Migration from the new member states to the old increased after the enlargement, and there are indications that crowding out has occurred in certain sectors or within some occupations (Kahanec, Zaiceva and Zimmermann 2010).

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[2] Additional overviews include, e.g. Okkerse (2008) and Longhi et al. (2005). Dustmann, Glitz and Frattini (2008) discuss the mechanisms through which an economy can accommodate immigration. Recent contributions on the methodological problems are include Aydemir and Borjas (2011) and Dustmann and Preston (2012). Our quasi-experimental approach to some extent resembles those of e.g. Card (1990), Mansour (2010), Dustmann, Schoenberg and Stuhler (2012), and Glitz (2012).

The collected evidence of adverse effects is however limited and many studies are descriptive.[3] Some UK studies point in the direction of small or insignificant effects on native's labor market outcomes (see Reed and LaTorre 2009, Lemos and Portes 2008, Gilpin et al. 2006, and Portes and French 2005). More closely related to our study is Braakmann and Vogel (2010) who studied the effects of the EU enlargement in 2004 on German firms located close to the Polish border. They found a negative impact on the turnover and export intensity of large German firms and falling profits for smaller firms despite an increase in the turnover following the Enlargement.

Using longitudinal population-wide micro data, we find that the earnings of present workers decreased by about 1 percentage point in regions close to the transport links after the reform, compared to regions somewhat further from the ports. The result is robust to a number of specification tests and robustness checks. The negative effects tend to be greater among the young and the low-educated, the foreign-born, and in the lower part of the expected earnings distribution. We also find the clearest impact in industries where the rise in EU8 workers has been most pronounced, and where cross-border competition is likely to be particularly strong. We find no robust effects on employment or on full-time wages.

Furthermore, our data show that the 2004 EU expansion indeed led to a drastic increase in permanent as well as temporary migrants from the new member states. But while the fraction of EU8 migrants was higher in regions close to the transport opportunities already before the enlargement, there is only weak evidence that there was an increased clustering as a result of the reform. The fact that we do find robust evidence on an impact on labor market outcomes despite no evidence on increased registered immigration into these areas suggests that other mechanisms are at work. Higher competition from foreign firms and posted workers is one potential channel. Another is the decreased bargaining power of workers more exposed to low-wage competition from the new member states. In any case, our findings are an indication that the impact of the opening of a border does not solely arise through traditional labor migration.

The rest of the paper is organized as follows. Section 2 discusses the institutional background, the debate preceding the expansion, the recent history of immigration to Sweden and how the foreign-born fare on the Swedish labor market. Section 3 describes the empirical strategy and the data sources. Section 4 outlines the potential mechanisms at work and describes the characteristics and the development of permanent and

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[3] For studies with a descriptive approach see e.g. Constant (2011) for an overview of effects on the European Union, on the UK, see e.g. Pollard, LaTorre and Sriskandarajah (2008), Blanchflower and Lawton (2010), Drinkwater, Eade and Garapich (2006), Blanchflower, Saleheen and Shadforth (2007). For Ireland see Hughes (2007) and Barret (2010) and for Sweden see Doyle, Hughes and Wadensjö (2006) and Gerdes and Wadensjö (2010).

temporary migration and some other key variables. Section 5 presents our empirical results and section 6 concludes.

2. Background and Institutions

2.1. Transitional Arrangements, the Debate Preceding the Eastern Enlargement and Post-Accession Migration Flows

The free movement of workers between member states of the European Union is regulated by the EC Treaty, article 39. The Treaty guarantees freedom of movement of workers within the EU and promotes the removal of barriers to mobility. In the debate preceding the eastern enlargement in 2004, fears of social dumping and immigration of cheap labor from the new member states lead to the inclusion of a clause in the Accession Treaty in 2003 to limit this freedom.[4] The clause gave individual member states the right to restrict access to their labor markets for a maximum of seven years. During the first two years after accession access to national labor markets were fully regulated by national law and policies. Restrictions could then be prolonged for three plus two years if there were any serious disturbances (Kahanec, Zaiceva and Zimmermann 2010). Hence, the transitional period could not end later than April 30, 2011.

The Swedish debate contained the above-mentioned arguments and also emphasized the risk of attracting welfare-seekers (Doyle, Hughes and Wadensjö 2006). Against this stood e.g. the argument that since Sweden had actively promoted the enlargement it was not reasonable to implement restrictions. In the end, failing to reach an agreement on how to construct transitional arrangements, Sweden was one of only three countries who did not implement any (together with the UK and Ireland).[5]

As expected, the migration flow from the new member states increased after the enlargement (see also description below). The fears of mass immigration did not materialize and consequently most of the member states relaxed or abolished their transitional rules before the compulsory abolishment in 2011.[6] An early evaluation from the European Commission

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[4] Individuals from Cyprus and Malta were not covered by these restrictions.

[5] The UK and Ireland implemented some restrictions on access to social benefits. To qualify for welfare entitlements in the UK, workers from the new member states had to be in continuous employment for 12 months (Pollard, LaTorre and Sriskandarajah, 2008). In addition, migrants from the new member states in the UK were obliged to register on the Home Office administered Workers Registration Scheme (WRS). In Ireland, to gain employment or access state benefits, a personal identification number (Personal Public Ser-vice Number) was required.

[6] Greece, Spain, Portugal, Italy and Finland lifted their restrictions in 2006 followed by the Netherlands and Luxemburg in 2007. In 2008 France completely opened its labor market, Denmark and Belgium did so in 2009. Germany and Austria on the other hand did not lift their restrictions until the end of the transitional period in 2011.

pointed out that there was no apparent direct link between migration flows and the transitional arrangements put in place (Commission of the European Communities 2006). This view was confirmed in a later report (Commission of the European Communities 2008).

Nevertheless there is clear evidence that the distribution of immigrants from the new member states throughout the European Union is uneven; Ireland and the UK have received the largest share of migrants in relation to population size (Commission of the European Communities 2008). The two countries alone received almost seventy percent of the migrants to the old member states since 2003, indicating that there has been some migration diversion to the countries that did not restrict access to their labor markets (Kahanec, Zaiceva and Zimmermann, 2010; Boeri and Brucker, 2005). The inflow to Sweden was more modest despite the open door policy (Gerdes and Wadensjö 2010). We will return to this in section 4.

A feature of recent intra-EU migration is that temporary migration is becoming increasingly more common (see e.g. Blanchflower and Lawton 2010). Likewise is the posting of workers in other member states on the rise (Commission of the European Communities 2008; Dolvik and Eldring 2008), and possibly also the presence of foreign firms (this is further discussed in Section 4.4).

2.2. Immigration to Sweden and the Foreign-Born in the Swedish Labor Market

This section briefly sketches the recent immigration history to Sweden with a particular focus on migration from the EU8 countries. The period following the Second World War until the late 1970s was dominated by labor immigration from primarily Finland, Central and Southern Europe. Starting in the 1970s, there was a gradual shift toward immigration of humanitarian character. From the late 1980s, refugee migration and immigration for family reunification have been the predominant forms of migration to Sweden, although labor migration flows have remained non-trivial. Over the last four decades, the foreign-born population has been growing steadily, from 6.7 percent of the total population in 1970 to 14.3 percent in 2009. Parallel to the compositional change of the migrants to Sweden, the relative labor market performance of the foreign-born deteriorated. Sweden is now one of the OECD countries with the highest relative foreign-born to native unemployment rates.[7]

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[7] There are of course large discrepancies within the group of migrants, where those arriving for humanitarian reasons and succeeding family members perform substantially worse than labor migrants from EU and OECD countries. See e.g. Eriksson (2011) for an overview of studies on immigration and ethnicity in the Swedish labor market.

In the post WWII period, political turmoil in the EU8 countries caused some limited waves of refugees to Sweden, from Hungary (1956–1957), the former Czechoslovakia (1968–1969), and Poland (1982). Migration from the Baltic States was very limited until the collapse of the Soviet Union in the early 1990s. The inflow of migrants from the new member states increased somewhat due to the fall of the Soviet Union but remained on relatively low levels until the EU enlargement. Female marriage migration is and has for the last couple of decades been a relatively large part of the flows from Poland and the Baltic countries to Sweden. Until 2004, women outnumbered men by a factor of roughly 1.5–2.5. After the enlargement, the situation reversed due to increased flows of a majority of male labor migrants. This recent migration will be further described below.

3. Data Sources

3.1. Data on the Native Population and Permanent Migrants

Our main data come from administrative registers compiled into a database at the Institute for Evaluation of Labor Market and Education Policy (the IFAU database). The data we use cover the total population aged 16–64 years old for each year during 1994–2008 in the most southern part Sweden (counties of Skåne, Blekinge, Halland, Kronoberg and Kalmar). It was mainly collected by Statistics Sweden, with the registers including LOUISE, RAMS and RTB. LOUISE contains information on age, gender, marital status, children, education, country/region of birth, immigration year, employment status, earnings, and region of residence. RAMS is a linked employer-employee database that contains individual information on employment spells and earnings from different employers. Employer information includes industry and the geographical location of firms and their respective workplaces. RTB is a population register containing information on country of birth. See appendix A1 for variable definitions.

The population-wide information on labor market outcomes comes from tax registers. These do not include wage information. The database however also contains wages (corresponding to full-time monthly) for all public sector employees and for a sample covering about 50 percent of private sector employees. The sample is stratified by firm size, so that small firms are underrepresented. We will use the wage data in a supplementary analysis.

The data cover the entire population of people living in Sweden on a permanent basis. Some countries of birth are grouped in the data for confidentiality reasons, but we are able to separately identify individuals from all of the new member states except from Slovenia, Malta and Cyprus. This is likely to be a small concern as the post-accession migration inflow is

dominated by migrants from Poland and the Baltic States, i.e. our neighbours across the Baltic Sea (see Section 4). Migrants from Malta, Cyprus and Slovenia make up less than one percent of the change in the total stock of migrants from the new member states between 2004 and 2010. All registers are linked by an anonymized personal identification number.

3.2. Data on Temporary Immigration and International Mobility

For an immigrant to enter the "registered population" and be included in the data described above, the basic rule is that the expected duration of the stay (given work and residence permits) should be at least 12 months. The foreign-born meeting this criterion enter the population described above. Temporary migrant workers enter on shorter work permits and are typically not included in the registered population. To describe the increasing presence of temporary workers, we also use data on short-term migrant workers, taken from Statistics Sweden. This information has to our knowledge not been used in previous academic work.

The primary data source for information on temporary immigrants is a tax register that includes tax payments of persons that pay Special Income Tax for Non-Residents (Särskild inkomstskatt för utomlands bosatta). All persons who stay in Sweden less than six months are entitled to pay lower taxes than permanent residents. The administrative records include data on gender, age, nationality, income, and employers. Our data also include information on persons that do not apply for the special income tax but stay no longer than six months, and persons that stay in Sweden longer than six months but less than a year, all of which are excluded from the regular population registers. The dataset is combined with firm-level data from FRIDA, a firm database managed by Statistics Sweden, which contains the geographical location of the firm and workplace, industry, sector and number of employees of the respective firms. The quality of the dataset is in some dimensions poor, e.g. the coverage of the origin of the workers is low, but it is useful for a description of the change in migrant characteristics that followed the enlargement.

3.3. Transports

Finally, for descriptive purposes we also make use of travel statistics from the Swedish Institute for Transport and Communications Analysis (SIKA). The data contain information on passengers arriving to Sweden and is used to show how travel patterns between the new member states and Sweden have changed after the enlargement in 2004.

4. The Studied Regions and the Development since the Enlargement

This section first presents the regions included in the analysis and the way these are classified into treatment and control areas. Then we present descriptive characteristics on the estimation sample and the populations of the treatment and control areas prior to the reform. This is followed by a brief characterization of the development of travel patterns between EU8 countries and Sweden and a somewhat more detailed description on temporary migrants.

4.1. The Studied Regions

The broader question under study is whether the labor market outcomes of present workers are affected by changes in market conditions brought by migration policy reform. It is very difficult to derive plausible estimates of the impact on the national workforce following a change at the national level. Our strategy is instead to investigate whether those who are likely to be more exposed to increased competition fare differently than those who are less exposed to the direct results of the reform. We use proximity to transport opportunities as an indication on potential exposure.

There are of course many ways to travel from the EU8 countries to Sweden. To avoid endogeneity and selection problems, we restrict the analysis to pre-existing transportation links. Since airlines are arguably more mobile, we focus on ferry lines. Furthermore, we exclude ferry lines to the Stockholm region, for which it is very hard to find a suitable comparison region. These restrictions leave us with ferry lines to four municipalities in southern Sweden: Karlskrona, Karlshamn, Ystad and Trelleborg (see Figure 1). We also restrict the dataset to Södra Götaland (south of the red areas in the figure).

Our baseline analysis uses 50 km as the divider; municipalities whose center is within this distance from a ferry line harbor are treated, areas further away (but in the Södra Götaland region) are in the control group. We will also present results using a 25 km delineation, and also discuss several variations and robustness checks defining the control group in different ways. Overall, the results are robust to these variations. Furthermore, we present results from an analysis restricting the overall sample to the county of Blekinge (in the southeast, including Karlskrona and Karlshamn).

Figure 1. The studied regions

Götaland

Legend:
- Harbor in municipality
- Other treated municipality
- Control municipality
- Other municipality
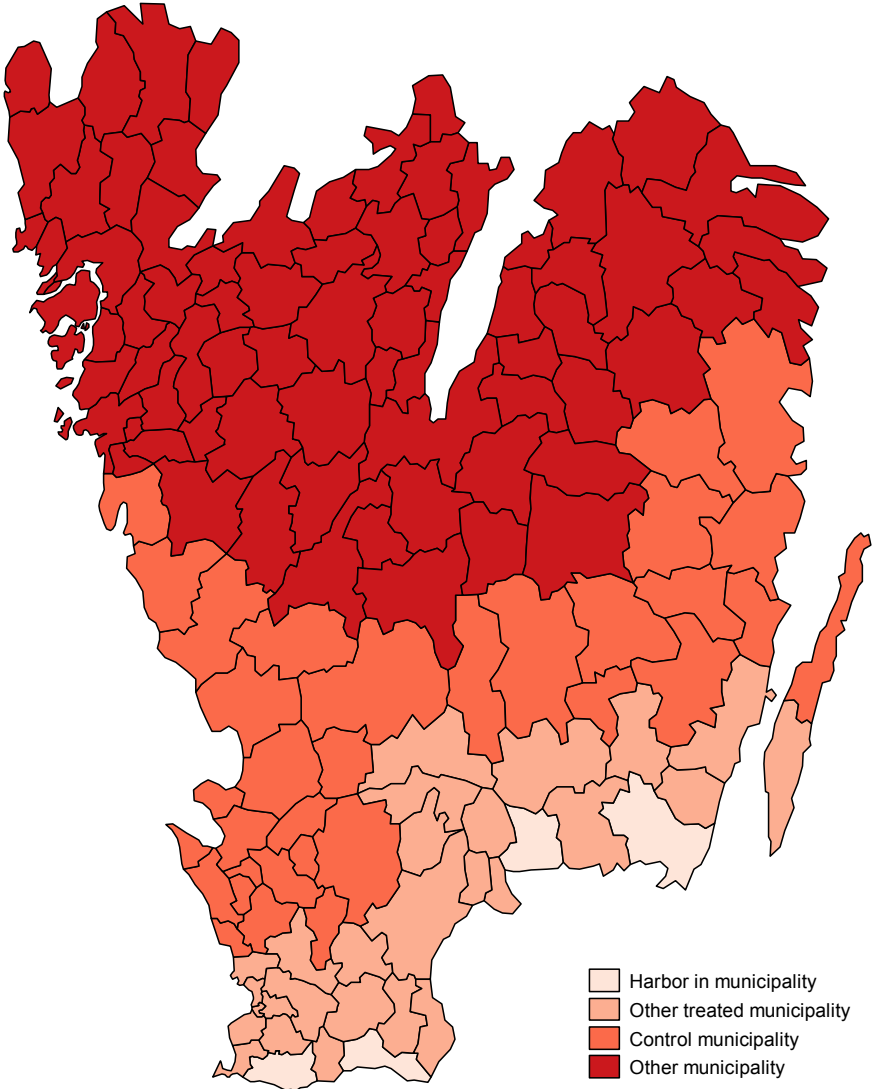
4.2. Estimation Sample and Pre-Treatment Characteristics

Our baseline estimation sample consists of individuals born outside the EU8 countries (i.e. native Swedes and other foreign-born), 16–64 years old, living in Södra Götaland (the counties of Skåne, Blekinge, Halland, Kalmar, and Kronoberg). We draw repeated cross sections for the years 2000–2008, imposing these restrictions annually.[8] The first column of Table 1 describes this sample. Average age is close to 40, about 40 percent have children living at home, and a slightly higher fraction is married. A quarter of the individuals have less than high school education, whereas approximately 30 percent have some tertiary education. The employment rate is 71 percent and annual earnings were on average 176,000 SEK during the period. The industry structure contains no big surprises; many people are employed in manufacturing, trade, health and education.

Our analysis hinges on the assumption that had the treatment and control regions been subject to the same shock, the development would have been the same. We will discuss and test the plausibility of this assumption below. But a starting point is to see whether the regions are similar in important dimensions. Columns two and three of Table 1 present characteristics for the baseline (50 km) treatment and control areas in the years prior to the EU enlargement. The demographic characteristics of the treatment and control areas are rather similar concerning age, gender, marital, and family characteristics. The level of education is slightly higher in the control group. The biggest difference is seen in the fraction foreign-born, which is higher in the treatment group as it includes Malmö, the largest and most immigrant dense city in the region. As for the individual economic outcomes, they are too quite similar across locations, although the employment rate is a bit higher in the control areas. Although there are some differences, the industry structure is also rather similar in the two groups. In other words, it does not seem like near-harbor areas are very different from the neighboring areas situated somewhat more distant from the transportation nodes.

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[8] Exploiting the panel data including individual fixed effects yields very similar baseline results, but becomes cumbersome considering the number of individuals included.

Table 1. Estimation sample statistics

	Estimation sample (2000–2008)	Pre-treatment (2000–2003)	
	All	Treatment	Control
Age	40.22	39.87	40.60
Sex (1=man)	0.51	0.51	0.51
Married	0.43	0.43	0.45
Children under 18 in household	0.41	0.39	0.42
Foreign born	0.14	0.14	0.11
No high school	0.25	0.25	0.28
High school	0.46	0.45	0.49
College	0.29	0.30	0.24
Employed in November	0.71	0.69	0.72
Annual earnings (SEK)	175,896	167,124	168,718
Monthly earnings conditional on emp. in Nov.	19,305	18,505	17,955
N	11,239,356	2,597,937	2,289,626
Industry (distribution cond. on empl. in November)			
Not classified	1.46	1.71	1.64
Agriculture, hunting, forestry, fishing	2.46	2.32	2.87
Mining and quarrying	0.10	0.09	0.10
Manufacturing	19.15	18.51	22.34
Electricity, gas and water supply	0.78	0.56	0.99
Construction	6.15	5.63	6.04
Wholesale and retail trade	12.96	12.26	13.13
Hotels and restaurants	2.35	2.23	2.17
Transport, storage and communication	6.10	6.32	6.17
Financial intermediation	1.45	1.58	1.38
Real estate, renting and business activities	10.52	11.12	8.46
Public administration	4.91	5.42	4.36
Education	10.94	11.77	10.46
Health and social work	16.53	16.32	16.12
Other community, social and personal service activities	4.15	4.16	3.75
Total	100	100	100

Notes: Population aged 16-64 years old, excluding migrants from the EU8, residing in Södra Götaland in 2000–2008.

4.3. Permanent and Temporary Migration from EU8 Countries

Following the 2004 enlargement, Södra Götaland experienced a rather rapid increase in the presence of permanent migrants from EU8. After increasing

only slightly since the year 2000, the number of EU8 migrants (age 16–64) living in this part of Sweden rose from 19,000 in 2003 to more than 25,000 in 2008.[9] Still, this group of migrants constitutes only a limited part of the population, with the fraction going from 1.5 to slightly more than 1.9 percent. In absolute terms, people of Polish origin constitute the majority of the previous and recent permanent migrants from EU8, but in relative terms there have also been substantial increases in the number of immigrants from the Baltic countries. Those that have arrived post-enlargement are younger, to a larger extent male and have less schooling compared to migrants still living Sweden that arrived prior to the enlargement (see Table A2 in the appendix). Furthermore, in comparison with the earlier cohorts, the post-enlargement migrants were much more frequently represented in the agricultural sector, the construction sector and within real estate, renting and business activities and underrepresented within the health and social work sector.

In terms of our identification strategy, how different demographic groups are located relative to the ferry lines are also relevant. It turns out that while the EU8 migrants in southern Sweden are relatively more concentrated within the 50 km limit (47 percent compared to 33 percent of the natives), they are not overrepresented in the harbor municipalities themselves. However, the recent permanent migrants tend to settle in these locations slightly more frequently compared to previous cohorts.

Passenger traffic statistics provide another indication on the increased exposure of the harbor regions. Figure 2 below shows the development of the number of passengers arriving by ferry from Poland, Estonia, Latvia and Lithuania to Swedish ports. Clearly, there has been an increase after the enlargement. In absolute numbers, the largest increase in the number of travelers was from Poland and Estonia. In relative terms the increase was larger for passengers from Latvia and Lithuania. By contrast, passenger ferry traffic from other countries (Denmark, Finland, Germany and the UK) did not change much during the period (not in figure). Worth noting is also that a recent survey indicates that the by far most common mode of transport to Sweden for Polish visitors is by ferry (IBIS 2011).

The four ports we study are those in Ystad and Trelleborg in the county of Skåne and Karlskrona and Karlshamn in the county of Blekinge (see Figure 1). The passenger lines include e.g. the line between Ystad and Świnoujście in Poland which has been in place since at least the 1960s. The traffic from Trelleborg, situated close to Ystad, is more orientated towards Germany with passenger lines to various destinations, including Sassnitz located close to Polish border. The traffic directly to Poland has been more periodic with direct lines serving Świnoujście. In Blekinge, the ferry line between

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[9] Own calculations using the IFAU data. Nationally, the number of permanent residents age 16–64 born in a EU8 country rose from 55,000 to more than 75,000 from 2003 to 2008.

Karlskrona and Gdynia, the port city close Gdansk in Poland, has carried passenger during the last decades while the ferry lines from Karlshamn primarily serves different destinations in the Baltic countries.

The ferry lines to/from Poland and the Baltics serve regions that have a relatively high population density in comparison to the destinations in Sweden. For example, the Pomeranian Voivodship, the region in Poland in which Gdansk is located, had around 2.2 million residents in 2013, and West Pomeranian Voivodeship that includes Świnoujście, had 1.7 million residents the same year. This could be compared to the populations of Skåne (1.2 million) and Blekinge (150,000). With regards to the destinations in the Baltics, e.g. Klaipeida and Liepaja, both destinations are located in regions with a population that is twice as large as the one in Blekinge. Thus, it should be clear that the EU8 catchment areas for the ferry lines are fairly large in terms of relative population.

Figure 2. Number of arriving passengers (1000s) by ferry from EU8
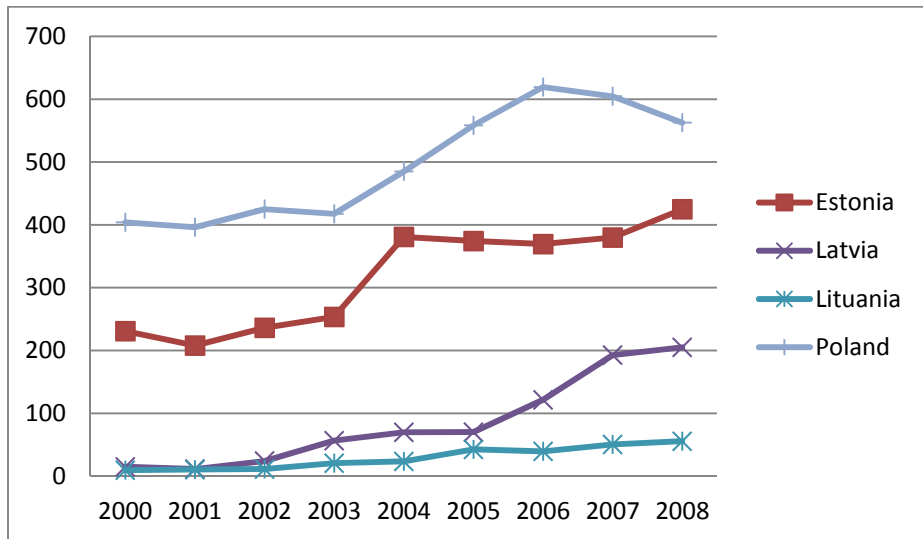
Notes: The graph shows the number of number of arrivals in thousands from the new member states on selected ferry lines 2000–2008 (no distinction between passengers from different countries is possible). Data source: SIKA, own tabulations.

4.3.1 Temporary Migrants

As an indication of increased labor market competition, the temporary labor migrants are of particular interest. Table 2 shows characteristics for EU8 nationals among the temporary workers during 2000–2008. From the upper panel of the table it is clear that the number of migrants has increased over time, starting before the EU expansion, but at an increased pace following

the accession.[10] An increasing majority of the workers are males, on average around 30 years of age. After 2004, the temporary workers on average stay longer in Sweden and have higher income both in total and from their main employer. Note that the average total earnings are only slightly higher than the average income from the main employer, suggesting that most workers have only one employer. In other words, there is an increased presence of labor from the new member states, both in terms of individuals and in terms of effective labor.

The lower panel of the table also suggests that the distribution across industries has changed rather dramatically. Before 2004 most temporary workers from the EU8 countries were found in the agricultural sector. Short-term contracts for these workers have for a long time been an established part of the production, and this has been particularly common in southern Sweden. As the Swedish labor market became generally available for the new member states, the share working in agriculture has decreased a lot, even though the absolute number has actually increased somewhat. The table reveals major increases in construction, business services and trade (including retail). The increased presence in these industries is a pattern found also for permanent migrants.

Table 3 shows the distribution of EU8 temporary migrants relative to the ferry ports. Compared to the permanent population, the temporary migrants tend to be somewhat more concentrated to the areas close to the ports, but not necessarily in the harbor municipalities. The development over time is somewhat hard to interpret. There is an increase in concentration starting already in 2002 and then a tendency to a decline in the later periods. The table thus gives no clear indications that the EU enlargement affected the location patterns of these migrants.

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[10] The nationality information in the data is incomplete. While the broader picture should be correct, one should be cautious regarding interpretations of the detailed information. See also the discussion in Section 3.2.

Table 2. Characteristics of the temporary immigrant work force

	2000	2001	2002	2003	2004	2005	2006	2007	2008
Age	32.3	32.7	31.1	30.3	29.9	29.7	29.5	29.5	29.5
Male	0.6	0.6	0.6	0.6	0.7	0.7	0.7	0.8	0.8
<Six months in Sweden	1.0	0.9	0.9	0.9	0.8	0.7	0.7	0.7	0.6
Earnings main employer (1000´s SEK)	26.8	34.7	34.2	36.4	36.5	49.8	56.7	63.8	72.8
Total earnings	27.5	35.6	35.0	37.2	37.2	50.2	57.3	64.4	73.2
N	3,366	3,594	4,306	4,876	6,211	6,560	7,312	8,917	10,756
Distribution across industries									
Not classified	4.8	5.0	5.2	8.0	11.6	12.9	9.1	8.1	11.0
Agriculture, hunting, forestry, fishing	78.8	78.4	76.5	76.1	66.9	57.3	52.7	50.3	48.6
Mining and quarrying	0.6	0.8	0.4	0.5	0.5	0.8	0.3	0.3	0.8
Manufacturing	2.5	2.1	3.9	2.9	4.7	5.4	6.8	7.6	8.6
Electricity, gas and water supply	0.0	0.0	0.1	0.0	0.0	0.0	0.0	0.0	0.1
Construction	0.8	1.1	1.7	1.2	1.9	5.2	9.0	10.5	9.1
Wholesale and retail trade	1.4	2.5	2.7	2.4	3.7	4.2	6.1	8.7	7.5
Hotels and restaurants	0.2	0.1	0.4	0.3	1.1	2.3	2.5	2.2	2.2
Transport, storage and communication	1.0	0.6	0.3	0.3	0.5	0.7	0.9	1.6	1.9
Financial intermediation	0.0	0.1	0.1	0.0	0.0	0.2	0.2	0.2	0.1
Real estate, renting and business activities	1.5	2.0	2.1	2.1	4.0	7.6	8.3	7.4	7.7
Public administration	0.5	0.5	0.5	0.9	0.2	0.1	0.3	0.2	0.0
Education	1.1	1.2	1.1	1.0	0.9	0.7	1.1	1.0	0.2
Health and social work	1.1	1.4	1.2	1.3	1.1	1.0	1.2	1.0	0.4
Other community, social and personal service activities	5.7	4.3	3.9	3.0	2.8	1.6	1.6	1.2	1.7
Total	100	100	100	100	100	100	100	100	100

Notes: Sample used is the total population of temporary workers from the new member states observed in Sweden 2000–2008. Observe that for this population we do not have information on country of birth. Instead we use nationality. Note that the data for this table are national (and thus not restricted to Södra Götaland).

Table 3. Where do the temporary workers settle?

	2000	2001	2002	2003	2004	2005	2006	2007	2008
Port in municipality	0.02	0.03	0.05	0.04	0.04	0.03	0.03	0.03	0.02
Port within 25 km	0.06	0.09	0.14	0.11	0.14	0.14	0.11	0.12	0.10
Port within 50 km	0.33	0.41	0.45	0.46	0.47	0.40	0.42	0.40	0.40
Port within 75 km	0.56	0.58	0.62	0.66	0.62	0.57	0.57	0.57	0.53
Port within 100 km	0.63	0.72	0.73	0.76	0.75	0.76	0.74	0.77	0.71

Notes: Share of the total number of EU8 workers that is working (or whose workplace is registered) in a municipality within a given range from a port with ferry lines to the new member states in Södra Götaland. Sample used is the total population of the temporary workers observed 2000–2008 in Södra Götaland. Observe that we do not have information on country of birth. Instead we use nationality.

4.4. Summary and Interpretation of Descriptive Patterns

The statistics presented above clearly show that the 2004 EU accession meant a greater overall inflow and presence of workers from the EU8 countries to Sweden. What is not so clear from the data, however, is whether the increased immigration affected the harbor regions to a greater extent. The location patterns of permanent migrants do not indicate that this would be the case, and the temporary migrants exhibit patterns that are hard to interpret.

On the other hand, travel statistics show a very marked increase in the numbers going to and from the new member states. It seems fair to argue that the economic impact of such changes should be larger in the areas where the transport links are located. It should also again be emphasized that we do not have information on the presence of posted workers or let alone irregular labor migration. Such workers are arguably more short-term by nature, and it is then reasonable to expect travel costs to play a bigger role. Of course, one could also hypothesize that the threat effect of cross-border competition is bigger the closer one gets to the competing firms and workers. The importance of such mechanisms is, however, extremely hard to quantify.

In sum, this means that whether the harbor regions were differentially affected by the EU expansion is an empirical question. Our description of the treated and non-treated areas above suggests that they fulfill reasonable requirements on similarity for the empirical analysis.

5. Empirical Analysis

This section presents the empirical analysis. We begin by describing the empirical specification and discussing ways of strengthening its credibility. Then we turn to the baseline results studying individual labor outcomes in treated and non-treated regions, pre- and post-enlargement. We then proceed by presenting robustness checks and variations, and also discuss

heterogeneous effects by background characteristics, across the earnings distribution, and across industries.

5.1. Empirical Specification

Our approach to investigating the impact of migration policy reform is to compare those who are likely more exposed to the effects of the enlargement opening up national borders, to those who are arguably less affected. Our empirical model is essentially a difference-in-differences specification, comparing the development of labor market outcomes in treated areas to the development in non-treated areas. Our baseline model has the following structure:

$$y_{ijt} = a + X_{it}\beta + \emptyset_t + \theta_j + D_{jt}\gamma + \varepsilon_{ijt} \qquad (1)$$

where y_{ijt} is the labor market outcome (employment, log annual/monthly earnings or log wage) of individual i in municipality j at time t. X_{ijt} is a vector of individual control variables (age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born), and industry (60 categories)), $\emptyset_i$ is a vector of time fixed effects, and θ_j is a set of municipality fixed effects. D_{jt} is an indicator variable taking the value one in the treated regions after the EU expansion (from 2004 and onwards), zero otherwise. γ is thus the parameter of primary interest capturing the average difference in pre-post development across treatment and control areas. We also try augmenting the specification above by linear/quadratic municipality specific time trends. Throughout we will cluster the standard errors by municipality, thus allowing for dependence across individuals living in the same location (also in different years).

For our analysis to capture a causal parameter, it must be the case that had the treatment and control areas been exposed in the same way, we would have expected to see the same development in the treatment as in the control group. This is by definition an identifying assumption that cannot be tested strictly. The above-described similarity in terms of individual demographic and economic characteristics, as well as in industrial structure could however be taken to indicate that this is a plausible assumption.

Yet another way to get some notion of the credibility of the specification is to perform a "placebo analysis". Table A3 shows results from an analysis using data for the years 1994–2002 (rather than 2000–2008) and "moving" the time for the reform to 1998. The idea is of course that a specification that handles the development over time well, and does not find "effects" where there should be none, has some credibility in working well also in the actual reform period. As can be seen in the table, the estimates are all small, and none of them are statistically significant at conventional levels.

5.2. Baseline Results

Table 4 below presents the baseline results on employment and earnings. Column (i) uses specification (1) above, columns (ii) and (iii) introduce also linear and quadratic trends respectively. In the interest of space, we display only the estimates of primary interest (full results are available upon request).

Starting with employment in the upper part of the table, column (i) suggests a small but statistically significant negative impact in the order of 0.6 percentage points on those living closer to the ferry ports in the years following the enlargement. But this result is sensitive towards the inclusion of municipality specific trends; in columns (ii) and (iii) the point estimates are close to zero. There is thus no strong evidence that employment was at all affected.[11]

The picture changes when we turn to annual earnings, where also the quite demanding specification (iii) suggests a significant negative impact in the order of 1 percent. The estimates from specifications (i) and (ii) are somewhat larger in absolute terms, but the overall impression is that the results are quite stable across specifications. The third outcome measure used in Table 4 is monthly earnings in the main employment spell covering the month of November. The logic for using this measure is that it reflects the individual's position and connection to one employer at a time of the year when seasonal work is less common.[12] The results for this outcome confirm the negative impact found for annual earnings.

In sum, the estimates suggest a modest but rather robust impact of being closer to the recently accessed countries. It should be stressed that this is not an estimate of the average impact on the Swedish labor market of the EU 2004 enlargement; it is the difference in the impact between locations close to and a little further away from the transport links. The overall impact may be positive or negative, or for that matter zero.

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[11] The employment measure used in the main analysis is constructed by Statistics Sweden. It follows the ILO definition and is defined as having worked at least one hour per week in November when the information is collected. Using other definitions does not alter the results. See column one and two in appendix A6 for additional results.
[12] November is usually the month for measurement of annual employment and wage statistics in Sweden.

Table 4. Baseline estimates – employment and earnings

	< 50 km from ferry * post-enlargement		
	(i)	(ii)	(iii)
Employment	-0.006*	0.001	0.002
	(0.002)	(0.001)	(0.001)
(log) Annual earnings (cond. on earnings>0)	-0.015**	-0.012***	-0.009**
	(0.004)	(0.003)	(0.003)
(log) Monthly earnings main employer (cond. on employed in Nov.)	-0.011*	-0.012*	-0.010*
	(0.005)	(0.005)	(0.004)
No trend	X		
Linear trends		X	
Quadratic trends			X
Covariates	X	X	X
Municipality FE	X	X	X
Year FE	X	X	X

Notes: OLS/LPM estimates, robust standard errors clustered on municipalities within parentheses. Treatment is defined as residing at most 50 km from a harbor measured by airplane interacted with time. Sample includes population aged 16–64 observed in 2000-2008 born in Sweden or elsewhere excluding individuals born in the new member states. Covariates include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born). For annual and monthly earnings, the specification also includes industry fixed effects. Sample size for employment (annual earnings) [monthly earnings] is 11,239,356 (8,919,620) [8,233,406].
* <0.05 ** <0.01 *** <0.001

5.2.1 Wages

Many of the mechanisms discussed in the introduction would predict an opening of the border to influence the price of labor, e.g. through an increased supply or through a change in the bargaining power of the agents. But one could also hypothesize, particularly in a labor market with high union coverage and collective bargaining, that earnings could also be influenced through a change in hours.

Table 5 presents estimates of wage impacts using the sample data described in Section 3. We choose to report the results by sector (and worker category where available), partly to get a more detailed picture, partly to reflect the differences in coverage (remember that the public sector is fully covered, but that the data for the private sector is a stratified sample where larger firms are overrepresented). As is clear, there is very limited evidence of any impact on (full-time equivalent) monthly wages. The only significant point estimate is found for workers in the municipal government, when using the model including quadratic municipality specific trends. One could argue that it is reasonable to find effects in this sector. The local governments are

responsible for day care, schools, elderly homes, streets and parks, and are major employers hiring a lot of low-wage manual labor. Despite this, and the fact that model (ii) is arguably the preferred specification, we would urge a great deal of caution in interpreting the evidence as saying that there is any impact on wages.

Table 5. Wage effects, by sector

	Blue-collar worker, private sector	White-collar worker, Private sector	Municipal government	County Council	Central government
(i) No trends	-0.001	-0.005	0.003	-0.000	0.008
	(0.003)	(0.003)	(0.002)	(0.003)	(0.005)
(ii) Quadratic trends	0.001	0.002	-0.008**	0.003	0.004
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
	Annual earnings, wage samples				
Quadratic trends	-0.012*	-0.012	-0.008	-0.002	-0.001
	(0.005)	(0.008)	(0.007)	(0.006)	(0.008)
Covariates Industry FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Year FE	X	X	X	X	X

Notes: Robust standard errors clustered on municipalities within parentheses. Treatment is defined as residing at most 50 km from a harbor measured by airplane interacted with time. Sample includes population aged 16–64 observed in 2000-2008 born in Sweden or elsewhere excluding individuals born in the new member states. Covariates include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born). Sample size in parentheses; blue collar worker (980,218), white-collar worker (1,025,062), municipal workers (1,474,064), county council workers (463,173) and the central government (341,274).
* <0.05 ** <0.01 *** <0.001

The difference between the estimates for wages and those for monthly earnings from the main employer is worth some attention. For a person working full-time the monthly wage and monthly earnings should be very similar. One interpretation is then that the negative influence on earnings is due to a reduction in hours. But there is at least one other possible source of the difference in the estimates: the coverage of the wage data. Table A4 and A5 in the appendix shows that among those for whom we observe monthly earnings, the fraction that is not found in the wage data correlates strongly and negatively with age and education and is also concentrated in the bottom of the earnings distribution. In other words, for some segments of the labor market, the wage data do not seem to tell the whole story. For this reason and due to the advantage of having population-wide data, we focus on earnings rather than wages in the remainder of the analysis. Note, though, that the last row of results shows that there are earnings effects also in the

wage samples; the baseline results are thus not purely driven by individuals for whom we do not observe wages. Precision is an issue, but finding a bigger impact in the private sector as compared to the public (in particular at the central and county levels) is expected.

5.3. Varying Distances and Regions

The 50 km line used to define treatment and control regions in the analysis above is of course somewhat arbitrary. This section presents variations on the distance criterion and also investigates whether the results are sensitive to the inclusion/exclusion of certain regions in the sample.

Table 6 displays estimates from a model where the treatment group has been separated into three mutually exclusive categories: harbor municipalities; 0–25 km (excluding harbor municipalities); 26–50 km. We then allow the treatment effect to vary across these categories, but within one common regression per outcome. The first set of estimates is from a model not including any trends, the second allows for the municipality specific quadratic trends.

Regardless of distance, there is little to suggest that employment probabilities were at all affected. For annual and monthly earnings on the other hand, the results tend to show that the effects are stronger for those closer to the ferries. This pattern is more pronounced when trends are not included, but (with the exception of annual earnings in harbor municipalities), it holds also in the models allowing for local trends. For annual earnings, there is also an impact in the 25–50 km category, but for monthly earnings the corresponding estimates are close to zero.

Another type of variation/robustness check is to investigate whether excluding certain cities and a county from the sample affects the results. Even though the placebo estimations discussed above lend support to our regions being suitable for the analysis, the choice of regions is of course a bit arbitrary. It is therefore reassuring to find that the overall pattern remains if we e.g. exclude Malmö (the largest city of the region and a major immigrant destination) or the counties of Skåne and Halland to create a geographically more coherent area that includes the ports situated in Blekinge only and bordering counties (see Figure 1).[13]

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[13] Results are available on request. In some instances precision is lower and all estimates are not statistically significant when some regions are excluded.

Table 6. Treatment effects by distance from harbor

Ferry line by distance*Period	Employment (i)	Annual earnings (ii)	Monthly earnings (iii)
		No trends	
Harbor municip.	-0.004	-0.018[*]	-0.018[**]
(reference > 50 km)	(0.003)	(0.007)	(0.005)
0-25 km, excl.	-0.010[***]	-0.020[***]	-0.020[***]
harbor municip.	(0.002)	(0.005)	(0.004)
25-50 km	-0.004	-0.009	-0.001
	(0.002)	(0.004)	(0.004)
		Quadratic trends	
Harbor municip.	0.003	-0.005	-0.018[**]
(reference > 50 km)	(0.002)	(0.004)	(0.006)
0-25 km, excl.	0.002	-0.008[*]	-0.013[**]
harbor municip.	(0.001)	(0.003)	(0.004)
25-50 km	0.002	-0.012[*]	-0.004
	(0.002)	(0.005)	(0.005)
N	11,239,356	8,919,620	8,233,406
Covariates	X	X	X
Industry FE		X	X
Municipality FE	X	X	X
Year FE	X	X	X

Notes: Robust standard errors clustered on municipalities within parentheses. Treatment is defined by distance from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 observed in 2000-2008 born in Sweden or elsewhere excluding individuals born in the new member states. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born), municipality of residence, year of observation and quadratic municipality specific trends.
* <0.05 ** <0.01 *** <0.001

A further step in this direction is to study only the country of Blekinge (in the southeast of Sweden), which consists of five municipalities, two of which have ferry lines to the new member states. In other words, this means going even more local. Table 7 presents the results, largely confirming the picture from the baseline analysis of a zero impact on employment but a significant negative impact on earnings[14]. The point estimate for monthly earnings is larger than in the overall sample, but some caution is warranted since it is also the case that the placebo analysis (second set of estimates in the table) indicates that something may have been going on in this dimension already before the enlargement. If one is willing to make a DDD-type of inference (deducting the placebo effect from the main estimate), one ends up with a point estimate of –0.016 (0.028–0.012) for monthly earnings, closer to the –0.010 found in the full sample.

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[14] Since the county of Blekinge consist of only five municipalities, we get low precision of the estimates as we cluster on municipalities.

Table 7. Blekinge county only

	Employment	Annual earnings	Monthly earnings
Ferry line in municipality*Period	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)
Effect on full population	0.001 (0.003)	-0.010 (0.013)	-0.028 (0.010)
N	834,552	673,065	619,056
Placebo	0.001 (0.006)	-0.001 (0.009)	-0.012 (0.009)
Quadratic trends	X	X	X
Industry FE		X	X
Covariates	X	X	X
Municipality FE	X	X	X
Year FE	X	X	X

Notes: Robust standard errors clustered on municipalities within parentheses. Treatment is defined as residing at most 50 km from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 years old born in Sweden or elsewhere excluding individuals born in the new member states observed 2000-2008. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign born), municipality of residence, year of observation and quadratic municipality specific trends. In the placebo regressions treatment is moved back to 1998, data from 1994-2002 used.
* <0.05 ** <0.01 *** <0.001

5.4. Heterogeneous Effects

The literature very briefly reviewed in the introduction suggests that while the labor market impact of immigration may be small for the total population, some workers may be more affected than others. In line with theoretical expectations, the empirical evidence tends to show that those who have a marginal position and/or are more likely to be closer substitutes to the recent migrants are also more affected by immigration. While realized permanent immigration is just one of the channels underlying our findings, the same type of argument is applicable here.

This section investigates whether the impact of the migration reform studied here varied across different parts of the population. First, we look at background characteristics of the incumbent population. Then we study whether the effects vary across the predicted earnings distribution. Finally, we perform separate analyses for different industries.

5.4.1. By Background Characteristics

Table 8 presents results from estimations on subsamples defined by basic individual characteristics: gender, age, level of education and region of birth. We focus here on annual and monthly earnings. For employment, the corresponding estimates are small and in almost every case statistically insignificant (results are available upon request). The table contains results using two definitions of treatment: the baseline 50 km limit, and the 25 km

limit (including harbor municipalities but excluding those in the 26–50 interval). Note that since each cell represents a separate regression, the estimates for the subgroups do not necessarily add up to the average effect in the overall population (shown in the first row of the table).

The baseline (50 km) specifications displayed in the left columns of the table, suggest that the effect is quite uniform across demographic and educational groups. At face value the negative estimates are greater among men than among women. Perhaps surprisingly, this set of results does not indicate bigger effects for the young or the low-educated than for older or more skilled workers. However, precision is a problem in these estimations. But it is noteworthy that there is an impact also for the highly educated. In the origin dimension, the pattern is more expected given the segregated Swedish labor market (Åslund and Nordström Skans 2010): the foreign-born are more affected than native workers.

If we instead look at the right part of Table 8 focusing on the areas where we found the most significant effects (cf. Table 6), the picture becomes more in line with expectations from theory and previous empirical work. In particular for monthly earnings, the point estimates are much larger in absolute terms for the youngest workers and for those with less than high school education. This is reasonable if one believes that part of the competition comes from an increase in short-term low-skill labor concentrated to the locations close to the ports. It is however also worth pointing out that when we move from the left to the right in the table, the gender pattern is blurred and for monthly earnings there is no longer any clear difference between natives and foreign-born.

Table 8. Heterogeneous effects – individual characteristics

Treated:	<50 km from ferry				<25 km (pop 25–50 excl.)			
	Annual earnings		Monthly earnings		Annual earnings		Monthly earnings	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Effect on full pop. (for reference)	-0.009*	(0.003)	-0.010*	(0.004)	-0.008	(0.004)	-0.016*	(0.005)
Women	-0.005	(0.004)	-0.007	(0.006)	-0.006	(0.005)	-0.019*	(0.007)
Men	-0.013**	(0.004)	-0.012**	(0.004)	-0.009	(0.006)	-0.013*	(0.006)
16-29 years old	-0.006	(0.010)	-0.011	(0.010)	-0.014	(0.011)	-0.038***	(0.011)
30-64 years old	-0.011***	(0.003)	-0.010**	(0.003)	-0.012*	(0.005)	-0.013**	(0.004)
25-55 years old	-0.015**	(0.005)	-0.010**	(0.004)	-0.010*	(0.004)	-0.011*	(0.005)
Less than high school	0.008	(0.015)	-0.010	(0.009)	-0.022	(0.010)	-0.030	(0.011)
High school	-0.016***	(0.004)	-0.007	(0.004)	-0.011	(0.006)	-0.011	(0.008)
College	-0.015*	(0.006)	-0.011*	(0.005)	0.004	(0.005)	-0.018*	(0.008)
Natives	-0.009*	(0.003)	-0.009*	(0.004)	-0.007	(0.005)	-0.016**	(0.006)
Foreign-born	-0.016	(0.013)	-0.025*	(0.010)	-0.025	(0.017)	-0.018	(0.028)
Covariates	X		X		X		X	
Industry FE	X		X		X		X	
Quadratic trends	X		X		X		X	
Municipality FE	X		X		X		X	
Year FE	X		X		X		X	

Notes: Robust standard errors clustered on municipalities within parentheses. A new row represents separate regressions. Treatment is defined as residing at most 50 (25) km from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 observed in 2000-2008 born in Sweden or elsewhere excluding individuals born in the new member states. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born), municipality of residence, year of observation and quadratic municipality specific trends. Sample size in parentheses for annual earnings, left panel; full sample (8,919,620), women (4,382,406), men (4,537,214), 16-29 years old (2,359,626), 30-64 years old (6,559,994), 25-55 years old (5,997,501), <High school (1,763,908), High school (4,317,093), College (2,838,619), Natives (8,052,998), Foreign born (866,622). Monthly earnings, left panel; full sample (8,233,406), women (4,044,695), men (4,188,711), 16-29 years old (1,945,763), 30-64 years old (6,287,643), 25-55 years old (5,735,172), <High school (1,507,879), High school (4,059,310), College (2,666,217), Natives (7,463,423), Foreign born (769,983). Sample sizes for the right column are available on request. * <0.05 ** <0.01 *** <0.001

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5.4.2 Impact across the Predicted Earnings Distribution

An alternative route to investigating heterogeneity in the impact of migration policy reform is to see if it varies across the predicted earnings distribution. In other words, do those whom we expect to have a strong labor market position fare differently than those with a weaker position? Table 9 shows results from regression where the sample has been divided into quartiles of predicted earnings (predicted by a Mincer-style regression, see table notes for details). The upper panel uses the 50 km threshold, the lower panel the 25 km limit (excluding individuals in the 25–50 km locations). Each panel contains results for the entire 16–64 age interval, as well as for a sample excluding the youngest workers (25–64 years of age).

The overall picture from the table is that the impact tends to be bigger in the lower part of the earnings distribution. In the 50 km specification using the entire 16–64 sample, the biggest effects are found for the second quartile, i.e. people with low but not the lowest expected earnings. When we exclude the youngest individuals, among which many are primarily in education and whose work consists of small part time jobs or employment during holidays, the impact is most significant in the bottom quartile of the earnings distribution[15]. We get similar (although somewhat clearer) results using the 25 km specification. Higher up in the predicted earnings distribution the estimates are typically smaller and only sometimes significant, although not miniscule even for the top quartile, which may seem a bit surprising.

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[15] This is in line with the fact that when we exclude individuals from the sample with "low" annual earnings the size of the estimate becomes smaller. See column 3 in appendix table A6.

Table 9. Heterogeneous effects – predicted earnings

	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)
	All	1st Q	2nd Q	3rd Q	4th Q
Ferry line within 50 km					
			Ages 16–64		
Annual earnings	-0.009**	-0.001	-0.024***	-0.003	-0.009*
	(0.003)	(0.012)	(0.007)	(0.005)	(0.004)
Monthly earnings	-0.010*	-0.015	-0.009	-0.009*	-0.008
	(0.004)	(0.009)	(0.007)	(0.004)	(0.004)
			Ages 25–64		
Annual earnings	-0.015***	-0.033***	-0.013*	-0.003	-0.012*
	(0.004)	(0.007)	(0.005)	(0.008)	(0.005)
Monthly earnings	-0.012***	-0.027***	-0.000	-0.010	-0.011*
	(0.003)	(0.008)	(0.007)	(0.006)	(0.005)
Ferry line within 25 km (pop 25–50 excl.)					
			Ages 16–64		
Annual earnings	-0.013**	-0.019*	-0.024**	-0.002	-0.007
	(0.004)	(0.008)	(0.008)	(0.006)	(0.005)
Monthly earnings	-0.009	-0.013	-0.010	-0.013*	-0.004
	(0.005)	(0.011)	(0.006)	(0.005)	(0.005)
			Ages 25–64		
Annual earnings	-0.013**	-0.030**	-0.012*	-0.004	-0.012*
	(0.004)	(0.010)	(0.006)	(0.009)	(0.005)
Monthly earnings	-0.012**	-0.023**	-0.006	-0.010	-0.007
	(0.004)	(0.009)	(0.007)	(0.007)	(0.005)
Covariates	X	X	X	X	X
Industry FE	X	X	X	X	X
Quadratic trends	X	X	X	X	X
Municipality FE	X	X	X	X	X
Year FE	X	X	X	X	X

Notes: Robust standard errors clustered on municipalities within parentheses. Treatment is defined as residing at most 50 km from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 observed in 2000-2008 born in Sweden or elsewhere excluding individuals born in the new member states. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born), industry, municipality of residence, and year of observation. The earnings measures used to divide the sample into quartiles are predicted by a "Mincer" regression including the above covariates excluding industry and quadratic trends.
* <0.05 ** <0.01 *** <0.001

5.4.3 By Industry

The increase in competition following the enlargement is unlikely to be uniform across industries. First, as shown by Table 2 the greater presence of migrant workers is clearly concentrated to certain industries. Second, the latent threat following an opened border is arguably stronger in some parts

of the labor market than in others. In this section we approach this issue by performing the analysis by industry.

Table 10 displays results for annual earnings. Looking at the left part of the table (the 50 km specification), a first observation is that most of the estimates are negative, and none are significant and positive. Quite strikingly, we find substantial negative effects in manufacturing and business services, two industries which have seen marked increases in EU8 labor in the years following the enlargement (cf. Table 2).

For construction, which also experienced a strong rise in the supply of labor from EU8, the point estimate is negative and relatively sizable, although not statistically significant.

Transport is the third industry where we find significant negative effects on earnings. Even though there appears to be no big inflow of people being hired in Swedish transport companies, the effects seems reasonable given the mobile nature of the services. Indeed, the Swedish Transport Workers Union and media have reported that the competition from foreign firms has increased following the enlargement (see e.g. Svenska Dagbladet 2011, or Sveriges Radio 2004, Sydsvenskan 2011). This type of mechanism is also likely to be a partial explanation to the impact on the business service industry, where foreign staffing companies is sometimes portrayed as important competitors (Petersson 2012).

Table 10. Effects by industry

	Annual earnings					
	Ferry within 50 km			Ferry within 25 km (25–50 excl.)		
	ATE	(S.E.)	Numb. of obs.	ATE	(S.E.)	Numb. of obs.
Effect on full pop. (for reference)	-0.009**	(0.003)	8919620	-0.008	(0.004)	7099869
Agriculture, hunting, fishing and forestry	0.024	(0.039)	163663	-0.018	(0.042)	142941
Mining and quarrying	-0.057	(0.059)	8496	-0.057	(0.078)	7805
Manufacturing	-0.018**	(0.006)	1678295	-0.007	(0.010)	1383989
Electricity, gas and water supply	-0.008	(0.019)	67449	-0.014	(0.029)	57107
Construction	-0.013	(0.011)	495262	0.008	(0.013)	407724
Wholesale and retail	-0.002	(0.008)	1143977	-0.003	(0.011)	910857
Hotels and restaurants	-0.015	(0.016)	273257	0.025	(0.026)	209114
Transport, storage and communication	-0.031**	(0.009)	534165	-0.049***	(0.013)	414508
Financial intermediation	-0.014	(0.026)	129719	-0.014	(0.045)	98807
Real Estate, renting and busn. services	-0.034*	(0.016)	985490	-0.019	(0.017)	729359
Public adm. and defense	-0.009	(0.017)	424032	0.011	(0.022)	335061
Education	0.008	(0.008)	983537	-0.004	(0.013)	787619
Health and Social work	0.024	(0.024)	1468833	0.006	(0.012)	1182418
Other service activities	0.003	(0.019)	392,003	-0.021	(0.033)	299654
Industry FE	X			X		
Quadratic trends	X			X		

Notes: Robust standard errors clustered on municipalities within parentheses. Each row represents two separate regressions. Treatment is defined as residing at most 50 (25) km from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 observed in 2000-2008 born in Sweden or elsewhere excluding individuals born in the new member states. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born), municipality of residence, year of observation and quadratic municipality specific trends. Some industries with too few observations are excluded from the analysis.
* <0.05 ** <0.01 *** <0.001

6. Conclusions

The individual labor market impact of immigration policies are likely to operate through several channels and vary depending on individual and regional characteristics. The 2004 EU enlargement meant that the Swedish labor market immediately became much more accessible for workers and firms from neighboring countries with substantially lower wage levels. We investigate whether workers living close to pre-existing ferry links to the new member states were differentially affected by this policy reform.

The data clearly show that the 2004 EU enlargement implied a greater presence of foreign workers, permanent and temporary, on the Swedish labor market. However, for permanent and registered temporary migrants, there are no strong signs that there was increased clustering in areas close to ferry lines. On the other hand, passenger traffic increased substantially and it is reasonable to think that the economic impact of such a development is to some extent regionally concentrated. Also, posted workers and firms operating on very short-term assignments are more likely to be sensitive to travel costs, and thus more likely to cluster close to the transport links. The same is of course true for irregular labor migrants. These groups are not easily observed in data, neither can we quantify the potentially differential threat effect of labor competition across a proximate border.

Our analysis of the impact on individual worker outcomes of being close to the transport links when borders were opened, suggests a small but robust adverse impact in the order of 1 percent on total annual earnings, as well as on monthly earnings from the main employer. In our treatment areas, the negative effects tend to be greater the closer to the ports one gets. We also present findings which by and large are consistent with previous studies concluding that workers who are closer substitutes to the new competition will also be more affected (see e.g. Bratsberg and Raaum 2011; Pekkala Kerr and Kerr 2011; Dustmann, Frattini and Preston 2012). The effects are to some extent greater among younger people, those with less education, the foreign-born, and in the lower tail of the predicted earnings distribution. Furthermore, we find the clearest negative impact in industries which have seen a greater increase in the presence of EU8 workers, or who are likely to be exposed to greater competition from the other side of the border.

It should be emphasized that we do not estimate the total impact of the EU 2004 enlargement on the Swedish labor market, but rather the difference in the impact between those closer to transport links and those somewhat further away. The total impact may be positive or more negative. We believe that the relatively modest size of the estimated impact appears plausible. The reform implied a major increase in the openness to neighboring countries with substantially lower GDP and wage levels, and it seems reasonable to see some impact of being more exposed to this competition. Yet, given the

previous literature, we would not expect to see huge effects on the labor market outcomes of present workers.

As we have stressed above, registered permanent and temporary migration is just one of several channels through which migration policy reform may affect the labor market. Indeed, our data only give limited support to the idea that the supply of this type of migrant labor increased more in the treatment as compared to the control locations in our study. The fact that we still see robust evidence of an impact on the labor market, suggests that other channels, e.g. posted/unregistered/irregular migrant workers, or competition from foreign firms operating from abroad, contribute to the impact. These are areas where data are scarce, but disentangling the mechanisms appears to be important for understanding the potential labor market effects of migration policy reform.

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Appendix

Table A1. Variable definitions

Variable	Definition
Ln(Yearly earnings)	= logarithm of annual earnings from labor.
Ln(Monthly earnings)	= logarithm of monthly income from labor. The measure is constructed by the use of an employer-employee register (RAMS). It is conditional on employment in November; an individual's primary employer is identified and the monthly income is given by dividing the total income from the main employer over the length of the employment spell.
Ln(Wage)	= Full-time monthly wages covering all public sector employees and around fifty percent of private sector employees. Sample is stratified by firm size so that small firms are underrepresented.
Employment	= 1 if an individual is employed in November.
Sex	= 1 if male, 0 if female.
Civil status	= 1 if married.
Children in household	= 1 if child under the age of 18 is present in the household.
Region of birth	= 1 if born outside Sweden.
Age	Years of age.
Industry	A vector of 60 industry dummies.
Municipality of residence	A vector of region dummies.
Educational attainment	A vector of educational dummies, 0<High school, 1=High school and 2=College.

Table A2. Characteristics of the permanent EU8 population in 2008

Characteristics and outcomes	Pre-Enlargement immigrants	Post-Enlargement immigrants
Age	45.52	33.99
Sex (1=man)	0.36	0.53
Married	0.45	0.48
Children under 18 in household	0.32	0.38
No high school	0.16	0.42
High school	0.49	0.28
College	0.35	0.30
Employed in Nov.	0.63	0.46
Annual earnings (SEK)	156,037	92,982
Monthly earnings conditional on emp. in Nov.	19,950	16,278
N	16,701	8,620
Industry (distribution conditional on empl. in November)		
Not classified	1.59	1.39
Agriculture, hunting, forestry, fishing	0.85	11.90
Mining and quarrying	0.03	0.05
Manufacturing	15.99	17.96
Electricity, gas and water supply	0.32	0.00
Construction	3.67	19.17
Wholesale and retail trade	11.51	10.51
Hotels and restaurants	3.35	4.36
Transport, storage and communication	6.02	4.05
Financial intermediation	0.71	0.10
Real estate, renting and business activities	12.91	17.02
Public administration	3.97	0.20
Education	9.89	2.08
Health and social work	25.25	8.89
Other community, social and personal service activities	3.92	2.30
Activities of households	0.00	0.00
Extra-territorial organizations and bodies	0.02	0.00
N	100	100

Notes: Population aged 16-64 years in Södra Götaland old born in the new member states observed in 2008.

Table A3. Placebo analysis – baseline specification

Ferry line within 50 km*Period	Employment		Annual earnings		Monthly earnings, emp. nov.	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Effect on full population	0.000	(0.002)	0.005	(0.008)	0.005	(0.006)
N	10,713,058		8,527,581		7,740,380	
No trend	X		X		X	
Industry FE			X		X	
Covariates	X		X		X	
Municipality FE	X		X		X	
Year FE	X		X		X	

Notes: Robust standard errors clustered on municipalities within parentheses. Treatment is defined as residing at most 50 km from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 year-olds born in Sweden or elsewhere excluding individuals born in the new member states. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth (native/foreign-born), municipality of residence, year of observation and quadratic municipality specific trends. Treatment moved back to 1998, data from 1994-2002 used.
* <0.05 ** <0.01 *** <0.001

Table A4. Correspondence of wage and earnings data – baseline specification

	Control municipalities			Treated municipalities		
	16-29 years old	30-64 years old	All	16-29 years old	30-64 years old	All
No high school	0.56	0.86	0.64	0.53	0.85	0.62
High school	0.48	0.63	0.51	0.46	0.63	0.50
Tertiary	0.34	0.54	0.37	0.33	0.58	0.39
Total	0.45	0.67	0.50	0.42	0.66	0.48

Notes: The entries show the fraction in each category for which there is an observation of monthly earnings conditional on employment in November, but no wage data in year 2000-2008. Since the wage data are sampled, the fraction should be smaller than one. For details about the sample see note Table 4.

Table A5. Correspondence of wage and monthly earnings data divided by income group

	Control municipalities	Treated municipalities	All
1st Quintile	0.81	0.82	0.82
2nd Quintile	0.46	0.46	0.46
3rd Quintile	0.40	0.36	0.38
4th Quintile	0.41	0.37	0.39
5th Quintile	0.41	0.38	0.40
All	0.50	0.48	0.49

Notes: See note Table A4.

Table A6. Variation of outcome measures

	Employment defined as annual earnings > 0 SEK	Employment defined as earnings>2 86,000 SEK	Log (Annual earnings) > 86,000 SEK
Ferry line in municipality *Period	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)
Effect on full population	0.002 (0.001)	0.000 (0.001)	-0.005*** (0.001)
N	11,239,356	11,239,356	6,916,652
Quadratic trends	X	X	X
Industry FE			X
Covariates	X	X	X
Municipality FE	X	X	X
Year FE	X	X	X

Notes: Robust standard errors clustered on municipalities within parentheses. Treatment is defined as residing at most 50 km from a harbor measured by airplane interacted with time. Sample includes population aged 16-64 years old born in Sweden or elsewhere excluding individuals born in the new member states observed 2000-2008. Controls include age, age squared, educational attainment, civil status, children in household, sex, region of birth.
* <0.05 ** <0.01 *** <0.001

Essay 2. Foreign Demand and Border Regions[♦]

1. Introduction

Real exchange rate movements changes relative prices between countries. Lower relative prices make it more attractive for foreign consumers to pass the border to go shopping. While the elasticity of foreign demand has received a lot of attention in the literature, the effects of demand shifts on local labor markets is less explored (for an overview of the literature, see Leal, López-Laborda and Rodrigo 2010). There are exceptions. Campbell and Lapham (2004) show that sales and average employment in the retail and restaurant industries in border regions are positively affected by falling relative prices. Further, Baggs et al. (2013), that study firm level responses, find that firm sales, the average number of employees, profitability and survival in the retail industry, increase after relative price falls. Both studies exploit shifts in demand caused by real exchange rate fluctuations.

This paper contributes to this literature. Sweden shares its longest land border with Norway and there is a long history of cross-border shopping along this border. Detailed geographic information on border crossings, where the population resides, and the location of establishments, allows me to explore effects of relative price changes on the retail industry as well as the full economy. In relation to the previous literature this is the first paper addressing this question in a European context. It is also the first paper that uses both population wide-data administrative registers and establishment level data to study the labor market response of real exchange rate movements.

The economic activity in border regions is often oriented towards the neighboring economy. One example is the cluster of manufacturing firms on the Mexican side of the U.S.-Mexican border that has been the focus of a number of studies (e.g. Hanson, 1996, 1998, 2001). Effects of the NAFTA agreement on retailers along the same border has also been studied by, e.g. Ford, Logan and Logan (2009) and Adkisson and Zimmerman (2004). Further, survey and anecdotal evidence suggests that retail firms and restaurants in border regions often rely on foreign customers (e.g. HUI 2011 and Gerber and Patrick 2001). This makes border regions in particular vulnerable to exchange rate fluctuations. Thus, strong depreciations (appreciations) of a currency can potentially have large effects on the local economy through rising (falling) demand of goods and services from foreign consumers.[1]

This study is, of course, also related to the more general literature on demand shocks. For example, there is a large a number of studies, theoretical and empirical, using exchange rate movements or trade liberalizations policies to study how sectors exposed to foreign competition are affected

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[1] That is, in addition to the overall effect of currency fluctuations on the whole economy (through trade), cross-border shopping increase the local demand for goods and services.

(e.g. Ekholm, Moxnes and Ulltveit-Moe 2011, Gourinchas 1999, and Melitz and Ottaviano 2008). For example, Ekholm, Moxnes and Ulltveit-Moe explore how firms react to large currency appreciations. They show that both net exporters and importers reacts by laying off employees but that only net-exporters experience increases in productivity.

Population-wide administrative registers, and registers containing all employment spells and establishments, are used in the analysis. The data covers a 25 year-period (1985–2009). I identify the effects of exchange rate movements on the retail industry by comparing areas located close to the border with areas located further away. The additional demand from foreign consumers is captured by an interaction between the real exchange rate and the areas located close to border. The more remote areas identify demand and cost shocks affecting the full economy. Two key assumptions, as noted by Campbell and Lapham (2004), must be fulfilled for the analysis to be valid. Firstly, retailers located close to the border cannot systematically differ from retailers located further away. And, secondly, exchange rate fluctuations must induce customers to cross the border for shopping.

That Norwegians exploit the relatively low price level in Sweden is a fact. Recent survey evidence indicates that in 2011, around 9.5 million trips to Sweden was conducted. Out of these two thirds were day trips with shopping as the main purpose (HUI 2011). To put this in perspective, on average, almost every Norwegian went to Sweden twice the year of the survey. The first assumption is harder to validate. I, however, provide some descriptive evidence that supports the underlying assumption that in the absence of cross-border shopping the growth rate of the retail sector in the border area would be similar to the one in more remote regions (more about these assumptions later).

Since the mid-90s the value of the Norwegian krone has steadily increased. Consequently it has become more beneficial for Norwegians to shop in Sweden. This paper demonstrates that, during this time period, the structure of the border economy tilted towards greater dependence on the retail industry. The econometric analysis supports this observation. It shows that following currency depreciations the number of employees and annual earnings within the retail industry increases. Likewise, the share of the total population employed within the retail industry grows.

The outline of the paper is as follows: In Section 2 the theoretical and empirical literature is summarized. Section 3 contains a description of the border region, cross-border shopping in Sweden, and the period of study. In Section 4 I describe the data sources used, the empirical strategy and the regression sample. Next, In Section 5 the empirical results are presented and, finally, I conclude by a short summary in Section 6.

2. Theoretical Motivation and the Empirical Literature

Price differences motivate cross-border shopping. Explanations of why prices differ between countries (or regions) include differences in production costs, taxes, regulations, the competitiveness of markets and exchange rates, among other factors (Christiansen 2003).[2] Kanbur and Keen (1993), the standard work in the literature on tax competition between countries, demonstrates how tax differentials can create incentives for individuals to shop in a neighboring country. Two conditions must be fulfilled; (i) the surplus of shopping abroad, including transportation costs, must exceed the surplus from buying the good locally and (ii) the surplus must be non-negative, otherwise the consumer will not shop at all.[3]

This line of reasoning is not limited to tax differentials. Foreign consumers living close to a border are likely to shift part of their expenditures to the neighboring country when the value of their currency increases. At the same time, the rationale for consumers on the other side of the border to shop in the neighboring country diminishes when relative prices increase. Thus, the demand from abroad as well as from domestic consumers is likely to rise when relative prices fall.

For producers, importing inputs from abroad becomes more expensive when the currency declines in value. This causes production costs to rise. While consumers in border areas can shift part of their expenditures across the border the direct effects on producers (through increased exports and more expensive inputs to production), are likely to be similar across the country. At least for firms in comparable industries. If these arguments hold, and nominal prices adjust slower than sales, the increase in demand for goods and services available along the border is likely to increase the demand for labor (e.g. through the expansion of existing retail outlets and/or the number of outlets).

The applied research in large confirms the theoretical results, i.e. when relative prices fall cross-border demand increases (for an overview, see Leal, López-Laborda and Rodrigo 2010). The literature focuses on retail sales (e.g. Campbell and Lapham 2004, and Baggs et al. 2013) or on particular goods or services. Some examples include groceries (e.g. Walsh and Jones 1988,

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[2] The fact that prices vary across countries, and that differences in relative prices of similar goods cannot be explained by transport costs, differences in taxation, and tariffs, has been the focus in a number of studies (e.g. Haskel and Wolf 2001, Engel and Rogers 1994, McCallum, 1995 and Gopinath et al. 2011). In an overview of the literature on exchange rates and goods prices Goldberg and Knetter (1997) conclude that the failure of the law of one price is likely to occur because of third-degree price discrimination, i.e. that prices vary by attributes such as location or customer segment and that the factors explaining this type segmentation are not fully understood.

[3] The primary focus of Kanbur and Keen (1993) is to explore how welfare can be increased by modifying taxes when there are incentives for cross-border shopping. Their research has been extended in a number of directions, for an overview see the summary by Leal, López-Laborda and Rodrigo (2010).

and, Tosun and Skidmore 2007), alcohol (e.g. Asplund, Friberg and Wilander 2006, and Beatty, Roed Larsen and Sommervoll 2009), tobacco (e.g. Chiou and Muehlegger 2008), gasoline (e.g. Manuszak and Moul 2009), lottery tickets (e.g. Knight and Schiff 2010), and restaurant visits (e.g. Campbell and Lapham 2004).[4]

Of particular relevance for this study are the studies by Asplund, Friberg and Wilander (2006), and, Beatty, Roed Larsen and Sommervoll (2009), as they focus on Scandinavia. The first study shows that a tax cut on alcohol in Denmark caused the relative demand for alcohol to fall in areas in located close to the border in Sweden. The authors attribute the effect to an increase in purchases of alcohol in Denmark. The second study is on Norway and it shows that alcohol and tobacco sales are substantially lower close to the Swedish border than further inland. They explain this pattern by the lower tax on these goods in Sweden.[5]

While most of the above studies are interested in the price elasticity of demand, there are two exceptions. Closely related to my study are Campbell and Lapham (2004) and Baggs et al. (2013). Campbell and Lapham (2004) study the effect of demand shocks caused by real exchange rate movements on establishments located close to the U.S.-Canadian border. Exchange rate movements are shown to have a significant effect on both the number of establishments and average employment in the retail and restaurant industry. In the study by Baggs et al. (2013) firm level data is used to explore the effect of exchange rate movements on retail firms. Their results demonstrate that currency depreciations have a positive effect on firm size, profitability and the survival of firms.

3. Background and Studied Region

3.1. The Period of Study

The period of study includes the years 1985-2009. During the late 1980s the Swedish economy experienced a macroeconomic boom. When the boom eventually broke in the early 1990s it caused the largest economic crisis in Sweden since the great depression.[6]

A contributing factor to the crisis was the deregulation of the credit markets in the mid-1980s. Excessive lending lead to a rapid increase in asset

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[4] For a structural model on the decision to travel across an international border to shop, see Chandra, Head and Tappata (2012).

[5] Recent Swedish evidence also suggests that alcohol taxes matters for where purchases are made (Johansson, Pekkarinen and Jouko Verho 2012). The focus of the study is on health outcomes.

[6] The period of study, the late 1980s and early 1990s, Norway (and Finland) similarly experienced a major banking crisis. The presentation in this section focuses on Sweden. For Norway, see Moe, Solheim and Vale (2004).

prices. This in turn, induced over-building the years preceding the crisis (Englund 1999). Other contributing factors were the government's expansionary fiscal policy, the constraints on the monetary policy caused by the fixed exchange rate regime, and features of the tax system (Englund 1999). Together, these factors created a high inflation environment. By the bust of the boom Sweden had devalued its currency six times since 1973.[7]

The first signs of the boom breaking were seen in 1989 when reports from the property market showed that it was increasingly difficult to find tenants at current rent levels. This caused the real estate and construction stock price index to fall. During the same period (1989-1991) interest rates increased rapidly because of the turmoil caused by the German reunification. In addition, a change in the domestic policy towards a low inflation regime further pushed the interest rates upwards (Englund 1999). A tax reform that reduced tax deductions for interest rates and capital income was also implemented. This lead to a fall in real estate prices which caused solvency problems for banks. The crisis soon spread to the real economy.

During the crisis, Sweden was also affected by the crisis of the European exchange-rate mechanism in 1992. Given the recent history of high inflation and recurring devaluations the international funding of the bank system became increasingly more difficult (Englund 1999). As a consequence the Riksbank further increased the interest rate. The pressure on the krona eventually became so high that the Riksbank had to raise the overnight interest rate to five hundred percent in September 1992. The situation stabilized somewhat, but two months later speculation against the krona continued and the government decided to the let the currency float. By the end of the year the value of the Swedish krona had fallen substantially (more about this in Section 3.3).

In terms of labor market outcomes the economy hit the bottom in 1993/1994 before it started to recover. Both earnings and employment had by then fallen substantially. While earnings recovered in the aftermath of the crisis employment did not return to its pre-crisis level.

The post-crisis period is a period of growth, interrupted only by the bust of the IT-bubble in 2001 and the financial crisis starting with the collapse of Lehman Brothers in 2008.

3.2. Geography of the Border

Sweden shares its longest land border with Norway. It stretches over 1,600 km from north to south. In Figure 1 border crossings to Norway by highway or other major roads are marked by pins. In northern Sweden and Norway the population density is low. Therefore, there are relatively few border crossings. From Trondheim (marked on the map) and to the south the

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[7] See Englund (1999) for a more details about the crisis.

population density, on both sides of the border, is a little bit higher. The area is however predominantly rural.

Only the South-West side of the border is highly populated (that is, in a Scandinavian context). This is where, Oslo, the capital of Norway, is located. In total, the three south eastern counties in Norway (Oslo, Østfold and Akershus) had around 1.4 million habitants in 2013.

Figure 1. Border crossings to Norway
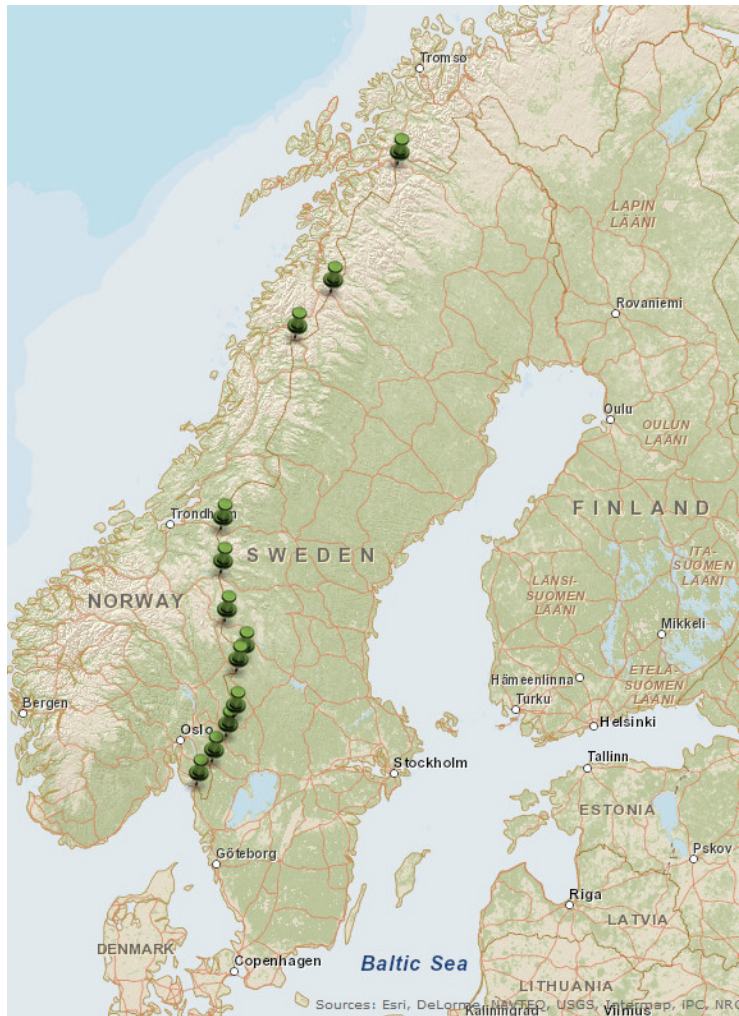
Notes: The pins on the map include all border crossings by highway (Europaväg) or roads considered to be important for the national infrastructure (Riksvägar).

3.3. Cross-Border Shopping

Table 1 illustrates the striking price differences on certain food items, alcoholic beverages and tobacco between Sweden and Norway. Norway is not a member of the European Union. Nevertheless, the country is subject to many EU laws and regulations through its membership in the European Economic Area (EEA), the internal market of the European Union. Taxation and excise duties are the responsibility of the individual member states. This partly explains the large price wedge between Norwegian and Swedish/EU27 goods and services. Another explanation is that Norway exercises its right to set up customs barriers to protect its internal market.

Table 1. Price levels of food items, alcohol and tobacco

	Food	Bread and cereals	Meat	Fish	Milk, Cheese and eggs	Alcohol	Tobacco
EU27	100	100	100	100	100	100	100
Sweden	104	114	107	99	90	138	130
Norway	153	145	162	122	169	234	219

Notes: Price level indices on selected items in 2009. European Union, 27 member states, is the reference category. Source: Eurostat (2013).

A survey of cross-border shopping in Sweden by HUI (2011) estimates the number of trips of Norwegians to Sweden to 9,5 million in 2011. This correspond to, on average, two trips per year for every Norwegian resident. Two thirds of the visitors stated that the main purpose of the trip was shopping. Around half of the trips were 100 km or shorter one-way. Most visitors traveled to Sweden by car (HUI 2011). Norwegian residents residing close to the border visit Sweden more frequently. A study from 2002 show that when the distance to shopping opportunities in Sweden was 30 km or less the average Norwegian shopper crossed the border 29 times annually. Visitors that had to drive 100 km or more passed the border seven times (Lavik and Nordlund 2009).

There is evidence suggesting that the amount spent on each trip increases by distance (Lavik and Nordlund 2009). This is natural as transportation costs must be compensated for a trip to be economically worthwhile. The most common non-durable retail goods that were purchased include groceries, sweets, non-alcoholic beverages, alcohol and tobacco. Among durable goods, clothes, shoes and interior decoration was stated to be the most common goods (HUI 2011). Permanent and mobile check points exist along the border. Quotas apply for e.g. meat, cheese, tobacco and alcohol but in 1996, only around 0.17 percent of vehicles were checked (Lund, Trolldal and Ugland 2000).

In 2011, most visitors from Norway went to the county of Västra Götaland (with 4,7 million visits) and Värmland (2,4 million visits), the two counties bordering the south east of Norway. The second most popular destination was Jämtland (1,3 million visits), a county located to the east of Trondheim (see Figure 1). The share of total turnover due to cross-border shopping in these counties is estimated to between 12 % (Västra Götaland) and 40 % (Jämtland) of total retail sales. (HUI, 2011)

The intensity of cross-border shopping depends on the relative price level between countries and exchange rate movements are a good approximation of fluctuations in relative prices (Section 2). As discussed in Section 3.1, Sweden abandoned its fixed currency regime in November 1992. This led to a strong depreciation of the effective exchange rate. The bilateral real exchange rate between the Swedish krona and Norwegian correspondence was similarly affected (see Figure 2 below). After a strong increase, the years prior to the crisis the value of the krona fell fast and started to trend downwards.

Unfortunately, there are no estimates of the value of cross-border shopping or the number trips from Norway to Sweden covering the full time period. Statistics Norway however provides estimates from 2004 and onwards. In Figure 3 the relationship between the value of the Swedish krona and Norwegians expenditures in Sweden is illustrated. It shows that when the krona declines in value the intensity of cross-border shopping appears to increase, although the relationship is far from perfect.

Figure 2. Real exchange rate (NOK/SEK)
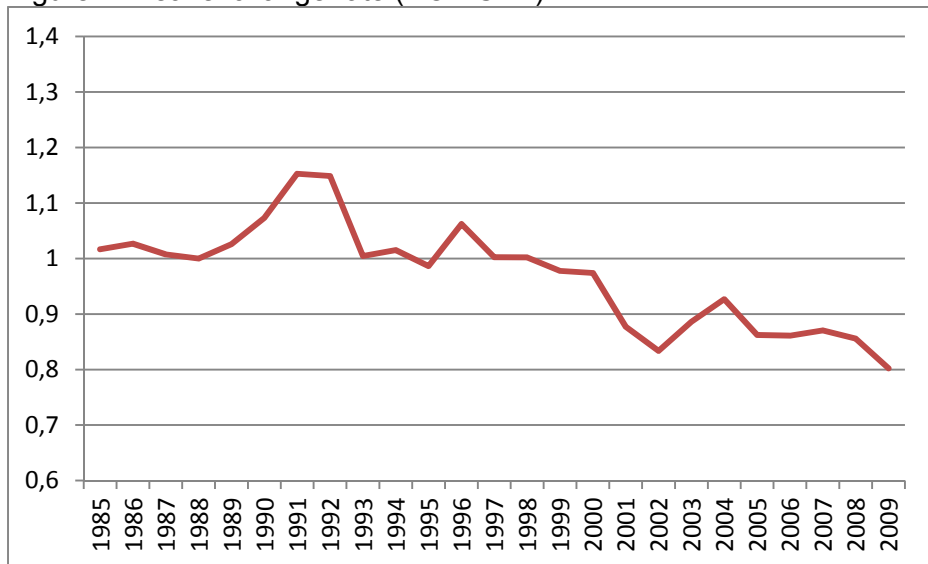
Notes: Real bilateral exchange rate NOK/SEK adjusted for inflation by the CPI for all items. The figure is based on data from Riksbank (2013) and OECD (2013).

Figure 3. The real exchange rate and cross-border shopping 2004-2011
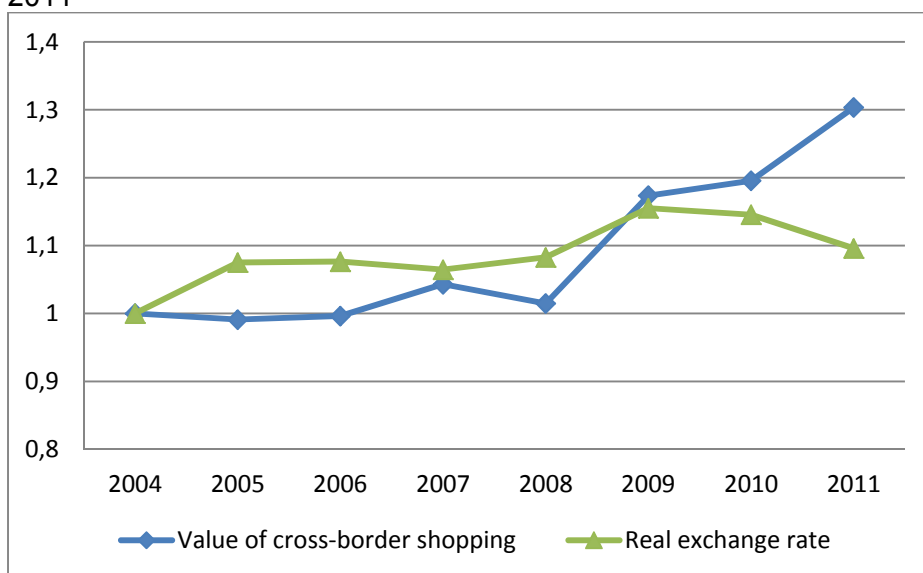
Notes: Estimated value Norwegian consumption in Sweden (2004=1). Real exchange rate based on CPI for all items used (SEK/NOK). The figure is based on data from the Swedish Riksbank (2013), OECD (2013) and Statistics Norway (2013).

4. Data Sources and Empirical Specification

4.1. Data

The main data source for this project is the longitudinal database LOUISE. It is a database compiled by Statistics Sweden. It contains information on individual characteristics, e.g. gender, country of birth, age, education, and labor market related outcomes including the employment status, annual earnings, and industry classification, among other things for the full working-age population. The data covers the years 1985-2009.

The dataset includes fine-level information on where the population resides following the division of the population into "neighborhoods". The classification system, known as Small Areas for Market Statistics (SAMS), was created by Statistics Sweden. It is a subdivision of municipalities into smaller areas, based on information on the location of the real estate in the municipality or in smaller municipalities, electoral districts. Out of the 290 municipalities in Sweden around 9,200 areas are created. These sub-areas are in turn linked to geographical coordinates that are used to calculate the distance to border crossings to Norway (more about this in Section 4.3). The coordinates are measured in 2005 and defined as the geographical population weighted center of each area. The quality of the data is good, less than 0.5

percent of the population lack geo-information. Individuals without this information are dropped from the analysis. From the dataset the main outcome variables and controls are constructed (see Table A1 in the appendix for variable definitions).

In the baseline analysis the outcomes of interest includes the size of the retail industry (i.e. number of employees), the share of the population employed in retail, and the annual earnings in the industry. These outcomes are defined based on where the population resides.

An alternative is to use the location of retail outlets. A measure based on the location of retail outlets potentially better capture the true treatment area since foreign consumers are likely to shift part of their expenditures to specific retail outlets. Further, the use of the location of retail outlets imply e.g. that we do not have to concerned about the commuting patterns of the population residing close to the border. As discussed in Section 4.3 part of the population residing along the border is likely to be employed in Norway. To this end, a matched employer-employee dataset is constructed. I link the employment register from Statistics Sweden to a firm register.

The employment register contains all employment spells and information on all wage payments. The firm register includes a limited set of information on all establishments. This includes the number of employees and the industry classification. It also contains information on the location of the establishments following the SAMS-classification. The quality of the geo-information for this dataset is poorer in comparison with dataset discussed above. Around 18 percent of the workplaces cannot be connected to geographical coordinates and are dropped from the analysis. Over time the quality of the data becomes better due to improvements in the data collection procedure. It should however be noted that the estimates stemming from this part of the analysis contains more uncertainty than the baseline analysis.[8]

From this dataset the sum of all wages from the retail industry is calculated. I also construct alternative measures of the size of the industry (number of employees measured at the establishment level and the number of employment spells).

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[8] As long as the there are no systematic differences across regions and time this is not likely to be a problem. To my knowledge there are no indications of this being the case.

4.2. Model

To model the effects of real exchange rate movements on the retail industry I follow the approach by Campbell and Lapham (2004).[9] The regression equation below illustrates the basic model.

$$y_{it} = a_i + \mu_t + \beta'(s_i * e_t) + \varepsilon_{it} \tag{1}$$

y_{it} is the outcome of interest measured at the SAMS-level i at time t. a_i is the SAMS-specific intercept and μ_t the time-specific intercept common to all SAMS-areas. μ_t captures demand and cost shocks affecting the full economy. e_t is a vector containing the current and lagged real exchange rate.

The real exchange rate is defined as the average yearly bilateral exchange rate between the Swedish krona and Norwegian krone. It is based on the monthly average nominal exchange rate, and is adjusted for inflation based on the CPI for all food items. The exchange rate, is defined as the number of Swedish krona required to buy a Norwegian krone (SEK/NOK), thus a higher value of e_t correspond to an appreciation (depreciation) of the Norwegian krone (Swedish krona). s_i is a dummy set to 1 if the SAMS-area is located within 50 km from the nearest border crossing, 0 for other SAMS-areas. The cutoff is somewhat arbitrary and I will return to this in the next section. The parameter of interest, β', is assumed to capture the additional demand from foreign customers induced by real exchange rate movements. Throughout the presentation an augmented model that includes a linear distance specific trend is also presented. This takes into account that areas located close to the border could follow a different long run trend than more remote areas. All models are estimated by OLS.

4.3. Regression Sample

The baseline sample includes all observations (individuals between 18-64 years old or establishments) within 150 km from the nearest border crossing to Norway. Distance is defined by the airplane distance from the center of a SAMS-area to the nearest border crossing. Border crossing by highway (Europaväg) and roads considered to be important for the national infrastructure (Riksväg) are included. The crossings are marked by pins in Figure 1. The use of the airplane distance instead of the actual driving distance to the border implies that the measurement error could be substantial. On the other hand most population centers are located close to the major roads.

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[9] There is one difference in the approaches. Campbell and Lapham model the relationship as a dynamic equation while I use a static model as the interpretation of the regression estimates are more straightforward. In Section 5.3 I however demonstrate that the results stand also in a dynamic framework.

In the baseline sample, treatment is defined by distance from the border. All SAMS-areas within 50 km from the nearest border crossings are assumed to be affected by real exchange rate movements. A map is provided in the appendix that illustrates the treated areas and the control areas (see Figure C1). As an extension I have elaborated with other cutoffs but the estimates shows that the effect of cross-border shopping appears to be geographically limited to the immediate border area (this is discussed in Section 5.3 below). This makes sense; the rationale for driving longer than needed after having passed the border, given a similar supply of goods and services, should be limited. Moreover, this observation is also consistent with the fairly long driving distances to reach the border for a large share of the border-shoppers (Section 3.3). Similarly, I have checked whether the definition of the control area matters by increasing and decreasing the geographical cutoff and by excluding regions. These variations only have minor effects on the results.

In Figure 4 the population density and the location of retail outlets is illustrated. From the figure it is clear that the immediate border region is sparsely populated. It also shows that there are a large number of retail outlets close to the border. This could be interpreted as a first indication of the importance of cross-border shopping for the local economy in the border region.

Figure 4. Distribution of population and establishments
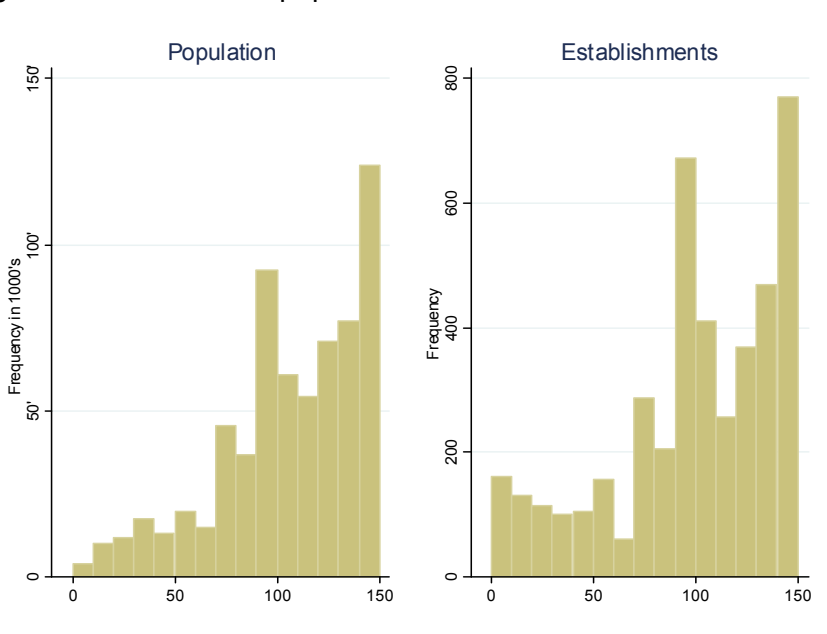
Notes: Distance in km to the closest border crossing on the x-axis. The left panel includes the full population between 18 and 64 years old and the right all establishments in the retail industry in 2009.

For the empirical approach to be valid it is crucial that the areas affected by cross-border shopping are comparable to the more remote areas that are used to capture shocks affecting the full economy. This assumption cannot be tested, but one way of exploring the validity of this assumption is to check whether other observable characteristics differ between the areas during the period of study. This is of course far from a perfect procedure but gives a hint of the comparison being reasonable.

In Table 2 the regression sample is presented. It shows that in terms of age structure, gender and family structure (the share that is married and has children living at home) there are no large differences. Close to the border, however, the share of Norwegians is substantially higher. This is likely to, at least partly, explain the relatively low level employment and annual earnings of the population living within 50 km from the border (Table 2). That is, it is reasonable to assume that a fraction of the population is employed in Norway. Data from 2004 and onwards support this notion (StatNord 2013). In terms of the industry structure it can be noted that in both regions the manufacturing industry is the main employer. The table also shows that the retail industry is only somewhat overrepresented close to the border. Overall, my reading of the table is that the empirical strategy is likely to be valid.

Table 2. Regression sample

	Treatment group	Control group
	Panel A: Characteristics and outcomes	
Age	42.26	41.60
Sex (1=man)	0.53	0.52
Born abroad	0.11	0.07
Born in Norway	0.07	0.01
Employment (annual earnings>0)	0.78	0.82
Annual earnings (100's SEK)	1223.01	1464.73
Social assistance receipts	0.05	0.05
No high school diploma	0.31	0.26
High school	0.52	0.52
College	0.17	0.22
Married	0.43	0.46
Children in household	0.38	0.40
	3055	25495
	Panel B: Industry structure	
Not specified	1%	1%
Agriculture, forestry and fishing	7%	4%
Mining and quarrying	0%	1%
Manufacturing	21%	23%
Electricity, gas and water supply	1%	1%
Construction	7%	7%
Sale, maintenance and repair of motor vehicles and motorcycles; retail sale of automotive fuel	2%	2%
Wholesale trade	2%	3%
Retail trade	7%	6%
Transport, storage and communication	5%	4%
Post and telecommunications	2%	2%
Financial intermediation, except insurance and pension funding	1%	1%
Insurance and pension funding	0%	0%
Real estate activities	1%	2%
Renting of machinery and equipment	0%	0%
Computer and related activities	0%	1%
Other business activities	3%	4%
Education	7%	7%
Research and development	0%	0%
Health care	7%	8%
Child care	0%	1%
Health and social work activities	11%	9%
Other health and social activities	1%	2%
Hotels and restaurants	4%	2%
Activities of membership organizations	2%	2%
Recreational, cultural and sporting activities	3%	2%
Other service activities	0%	1%
Public administration etc.	4%	5%
Total	100.00	100.00

Notes: Sample consists of individual aged 18-64 years old residing within 150 km from the nearest border crossing in 1985-2009. In Panel A, the observations below the line are restricted to the years 1990-2009 (information prior to this period is not availabile in the data set).

5. Empirical Results

5.1. Graphical Evidence

Before proceeding to the regression results I want to turn the attention to the long run development of the main outcomes of interest. In Figure 5 the development since 1985 and onwards is illustrated. What should be noted from these figures is the relatively strong increase in the number of employees, the share of the population working in the retail industry, and their earnings since the second half of the 1990s. This development correlates with the decline in value of the Swedish krona during this period (see Figure 2). Particularly striking is the strong increase in the share population employed in retail. This illustrates the over time growing importance of this industry for the border region.

Figure 5. Retail industry 1985-2009
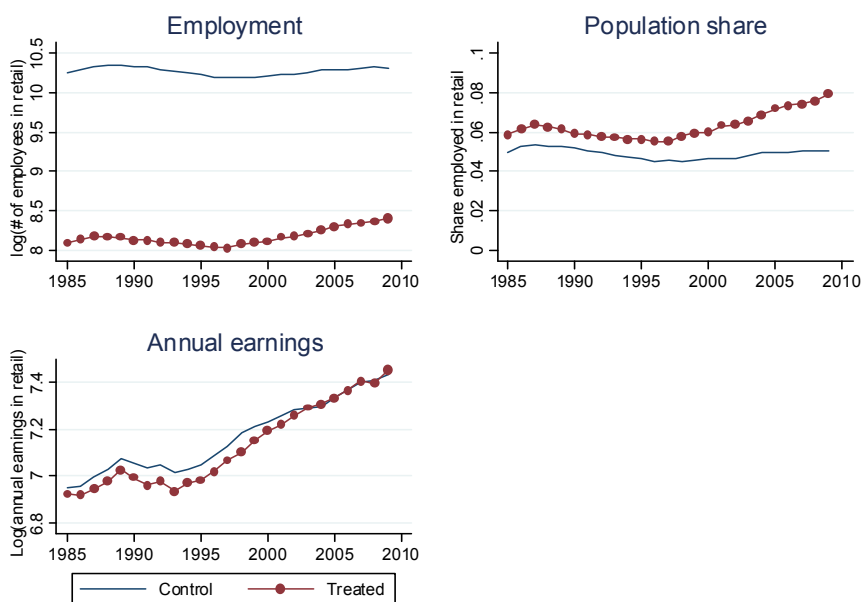
Notes: All individuals residing within 50 km from the nearest border crossing are included in the treated group. The figures are based on the full population aged 18-64 years old residing within 150 km from the nearest border crossing and that are obseved at least once between 1985-2009.

5.2. Baseline Results

In Table 3 I present the baseline results. The real exchange rate is defined in logs and the point estimates should be interpreted as follows: A ten percent

increase (decrease) in the value of the Norwegian krone (Swedish krona) is associated with an increase in the number of employees in the retail industry with 7 percent (model i). Model (ii) shows that when taking linear trends into account the size of the estimate is halved.

The model with trends captures e.g. improvements of the road network, the strong relative growth of Norwegian GDP, and other "linear" processes.[10] A specification with trends is more conservative, as it is less likely that the true effect of real exchange rate movements is overestimated. Therefore, the presentation hereafter will focus on the models that include trends.

Also the share of the population employed in the retail industry is affected by the value of Swedish krona. A ten percent decline in the value is associated with an increase in the population working in the industry with around 0.3 percentage points (model iv). Similarly, the estimates show that the annual earnings of the population employed within the retail industry increases with around 2.7 percent.

Table 3. Effects on the retail industry

	Log(Number of employees in retail)		Share employed in retail		Log(Annual earnings in retail)	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Real exchange rate	0.705***	0.343*	0.056***	0.031**	0.435***	0.271**
	(0.172)	(0.128)	(0.012)	(0.010)	(0.093)	(0.087)
N	27398	27398	28550	28550	27304	27304
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific, i.e. the area within 50 km is interacted with a linear trend.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

An extension of the basic model is to include the lag of the real exchange rate as it is not obvious that the impact of real exchange rate movements is immediate. In Table 4, the introduction of a lag demonstrates that this appears to be a valid observation.[11] It shows that there is a direct effect of exchange rate movements on the outcomes of interest as well as a lagged effect. Further, the combined effects sum to the direct effect captured in Table 3. An interpretation of these results is that it takes time for establishments to adjust following a demand shift.

In addition, the average yearly exchange rate is used as the main explanatory variable. This means that large swings in the exchange rate that

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[10] As an extension, in Section 5.4 below, I explore the effect of the strong growth in GDP in Norway during the period of study.
[11] As a specification check I have included up to four lags but the overall message from Table 4 remains the same.

occurs late during the year affects the average exchange rate, while the actual impact of the shock is concentrated to the later part of the year. It is therefore reasonable to expect part of the effect to be lagged.

Table 4. Effects on the retail industry – introducing lags

	Log(Number of employees in retail)		Share employed in retail		Log(Annual earnings in retail)	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Real exchange rate	0.510***	0.245**	0.039***	0.021***	0.223**	0.123
	(0.135)	(0.083)	(0.008)	(0.006)	(0.084)	(0.078)
Real exchange rate t-1	0.251*	0.153	0.022**	0.016*	0.272**	0.233*
	(0.107)	(0.115)	(0.007)	(0.008)	(0.104)	(0.108)
N	27398	27398	28550	28550	27304	27304
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

In Section 3.2 I discussed the fact that, in general, the northern part of the border area is sparsely populated. I also showed that cross-border shopping is a more widespread phenomenon the further south you get. To this end, in Table 5, I have split the sample into different regions. For reference the results from the use of the baseline sample is included on the first line in the table. All lines represent separate regressions. The second line excludes the two most northern regions (Norrbotten and Västerbotten). This does not affect the estimates which is natural given that the numbers of excluded observations are very few. The two following lines include estimates for the two most southern regions along the border (Värmland and Västra Götaland). These are the regions located fairly close to a large population center, including the capital of Norway. From the survey evidence discussed in Section 3.2 we know that this is also where the frequency of cross-border shopping is the highest. The point estimates support this notion; or rather they indicate that these are the regions where real exchange rate movements in general have the largest impact on the retail industry. Panel A shows that this is the case for the size of the industry, and Panel B, that the same holds for the share of the population employed in the retail industry. Lastly, Panel C, presents the results for annual earnings where a similar pattern is found although the statistical uncertainty is larger.

Table 5. Effects by region

| | Panel A. Log(Number of employees) | | | |
| | Log(Real exchange rate) | | Log(Real exchange rate t-1) | |
	ATE	(S.E.)	ATE	(S.E.)
Full sample (for reference)	0.245**	(0.083)	0.153	(0.115)
Full sample excluding Norrbotten and Västerbotten län only	0.244**	(0.084)	0.168	(0.116)
Västra Götaland	0.411*	(0.161)	0.448*	(0.175)
Värmland	0.413***	(0.106)	0.089	(0.160)
Dalarna	-0.231	(0.274)	0.133	(0.350)
Jämtland	-0.065	(0.447)	0.223	(0.564)
	Panel B. Share of population employed in retail			
Full sample (for reference)	0.021***	(0.006)	0.016*	(0.008)
Full sample excluding Norrbotten and Västerbotten län only	0.021***	(0.006)	0.016*	(0.008)
Västra Götaland	0.054***	(0.015)	0.059**	(0.021)
Värmland	0.018**	(0.006)	0.001	(0.007)
Dalarna	-0.012	(0.013)	-0.004	(0.016)
Jämtland	-0.002	(0.017)	0.002	(0.031)
	Panel C. Log (Annual earnings)			
Full sample (for reference)	0.123	(0.078)	0.233*	(0.108)
Full sample excluding Norrbotten and Västerbotten län only	0.121	(0.078)	0.241*	(0.108)
Västra Götaland	0.168	(0.135)	0.565***	(0.151)
Värmland	-0.054	(0.117)	0.084	(0.148)
Dalarna	0.733***	(0.205)	-0.416	(0.382)
Jämtland	-0.034	(0.253)	-0.612	(0.389)
Linear trend	X	X	X	X

Notes: Robust standard errors clustered on SAMS within parentheses. All lines represent separate regressions that are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Another variation is to split the sample by different time periods. In Table 6 the results of doing so are presented. The table shows that it is the later period that drives the results. An interpretation of the relatively weak effect of exchange rate movements during the first time period is not straightforward. One factor that is hard to ignore is however the fact that Sweden went through its largest economic crisis since the great depression during the early 90s. This potentially, put constraints on the development of the retail sector the immediate years following the crisis. Thus, despite the

strong fall in value of the krona, it is possible that an expansion was not possible due to e.g. the lack of financing. The crisis in Norway could potentially play a role (Section 3.1). Yet another interpretation is that the variation was not large enough to identify the effects during the earlier time period.

Table 6. Labor market outcomes by period

	Log (Number of employees)		Share employed in retail		Log (Annual earnings retail)	
	1985-1997	1998-2009	1985-1997	1998-2009	1985-1997	1998-2009
Real exchange rate	0.117	0.537***	0.010*	0.042***	0.092	0.215
	(0.097)	(0.127)	(0.005)	(0.009)	(0.119)	(0.123)
Real exchange rate t-1	0.030	0.240	0.002	0.025**	0.168	0.231
	(0.136)	(0.158)	(0.007)	(0.009)	(0.135)	(0.136)
N	14217	13181	14803	13747	14151	13153

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* p<0.05, ** p<0.01, *** p<0.001

5.3. Additional Sensitivity Checks

In this section I present a number of variations to test the stability of the baseline results.

A natural variation is to vary the treatment area as the 50 km cut-off was chosen somewhat arbitrarily. It was based on the assumption that after having passed the border into Sweden there are few reasons to continue further than needed to go shopping. This builds on the assumption that the supply of goods and services do not change substantially the further you get from the border. One way to test this assumption is to expand the treatment area. Table B1 in the appendix displays the effects of expanding the treated area to include all SAMS-areas within 75 km. It shows a similar pattern as the baseline results but the estimated effects are much smaller. Hence, the results are suggestive of the treated area being relatively small.

Another variation is to alter the control group. In Table B2 three different variations are presented. In models (i) and (ii) the control group includes all observations between 50 and 100 km from the border (in the baseline 50-150 km). Further, in models (iii) and (iv) the control is expanded to include all observations up to 200 km from the border. This cutoff includes the metropolitan area of Göteborg which dramatically increases the number of

observations. Finally, in model (v) and (vi) the 200 km cutoff is used but Göteborg metropolitan area is excluded.

The table shows that the results are in line with the baseline estimates presented in Table 3. Nevertheless, the inclusion of Göteborg has an effect in some instances. Given that the border area is mainly rural this is not surprising. My reading of the results is however that this doesn't alter the conclusions from the main analysis.

Finally, in Table B3 the demand shifting effects of real exchange rate movements is modeled as a dynamic equation. Thus, it includes the first lag of the dependent variable. This is in line with the modeling approach by Campbell and Lapham (2004). The table shows that the results stand using this type of specification as well.

5.4. Extensions of the Basic Model

A natural extension of the basic model is to include a measure of the divergence in growth rates between Norway and Sweden. Over the last decades the Norwegian economy has grown relatively fast in comparison with the Swedish economy. While the long run real exchange rate is likely to reflect this discrepancy it is not certain that this is true in the short run.

Table 7 presents the results of interacting the log of the GDP ratio in real terms between Norway and Sweden with all observations within 50 km from the closest border crossing. The table shows that a ten percent increase in the ratio is associated with a 3.8 percent increase in the number of employees in the retail industry (model i). When trends are taken into account the effect falls to 2.6 percent (model ii). The GDP ratio similarly has an effect on the share of the population employed in retail and their earnings. The inclusion of this variable causes the estimated effects of real exchange rate movements to drop somewhat in comparison to the baseline results; the qualitative conclusions are nevertheless not altered. What should also be noted is that the introduction of a trend in this context causes a much smaller fall in the size of the point estimates in comparison with the baseline results. This indicates that the trend captures part of the increase in the GDP ratio observed in the data.

An alternative to the ratio is to include the interaction between Norwegian real GDP and the treated area. The use of this measure causes the precision and the size of the point estimates of the effects of real exchange rate movements to fall (see Table B4 in the appendix). The estimates however point in the same direction as when controlling for the ratio. Altogether, the results in this section are suggestive of the real exchange rate not being the sole explanation for the relatively strong growth rate in the retail industry located close to the border since the mid-1990s. That is, the growing difference in GDP (income) between Norway and Sweden also appears to matter.

Table 7. Labor market outcomes – controlling for the GDP ratio

	Log (Number of employees in retail)		Share employed in retail		Log (Annual earnings in retail)	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Real exchange rate	0.361**	0.276*	0.034***	0.027**	0.305**	0.253**
	(0.128)	(0.122)	(0.010)	(0.010)	(0.094)	(0.091)
GDP Ratio	0.387***	0.259**	0.025***	0.015**	0.143*	0.067
	(0.094)	(0.087)	(0.005)	(0.004)	(0.061)	(0.076)
N	27398	27398	28550	28550	27304	27304
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. The ratio is defined in logs. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

5.5. Effects on Establishments

The analysis so far has been based on where the population resides. In this section I instead make use of the fact that I have access to data on where establishments are located. The same cutoff is used as earlier; establishments within 50 km from the border are defined as treated. A benefit of using establishment level data is that we know exactly where they are located (cf. the commuting patterns of the population that is an unknown). Three outcomes, that are comparable to the baseline outcomes above, are defined. Two of them are measures of the size of the industry and one, a measure of all wages paid out by retail outlets. The first employment measure includes the sum of all employment spells at establishments in a given area and the second is based on the number of employees that the retail outlets reports to the tax authorities in January each year. The latter measure does not include information on shorter employment spells. The difference is potentially important as the retail industry is an industry where the number of employees can vary substantially across the seasons. Further, the wage sum-measure includes all wages paid out during a given year to employees in retail. Thus, wages of the self-employed are not included.

In Table 8 I present the results. The positive effects on the number of employment spells and employees are quantitatively larger than the effects in the baseline analysis presented in Section 5.2. This could be because the distance measure based on establishments better reflects the true treatment area. On the other hand, these estimates are more sensitive to the inclusion of a trend – a trend that potentially captures the relative strong growth in GDP in Norway (Section 5.4). Moreover, the two employment measures yield similar results which indicate the choice of measure is not of large

importance for the conclusions that can be drawn from this part of the analysis. There is also an effect on the total wage sum in the industry.

Further, something that can be noted is that the numbers of observations are substantially lower than in the baseline analysis. This depends on the fact that retail outlets do not exist in all SAMS-areas and possibly also because of the relatively large share of establishments dropped from the analysis due to missing geographic information (see Section 4.1). Nevertheless, the results based on both datasets points in the same direction, i.e. currency depreciations positively affect the retail industry.

Table 8. Industry employment and wage sum

	Log (Sum of all employment spells in retail)		Log (Sum of all employees at establishments in retail)		Log (Wage sum in retail)	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Real exchange rate	1.623**	0.692*	1.584**	0.454	1.754**	0.618
	(0.563)	(0.317)	(0.605)	(0.324)	(0.600)	(0.341)
N	18090	18090	17364	17364	18090	18090
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS in parentheses. Sample includes all establishments with at least one employment spell within 150 km from the nearest border crossing. Controls include SAMS-fixed effects, and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

5.6. Effects on the Full Economy

While cross-border shopping directly affects the retail industry, and possibly also restaurants and other service places, these industries only makes up a small part of the total economy (Table 2). To this end I provide some tentative evidence on the correlation between exchange rate movements and effects on the full economy. This is of course a much harder exercise since it is not obvious what drives these results. Spillover effects from the retail industry, the existence of clusters of firms whose primary export market is Norway and changes in cross-border commuting patterns, among other factors could affect them.

Anyhow, before turning to the regression results a few graphs can be useful to illustrate the long run development of the border region. Figure C2, in the appendix, shows that the employment rate is falling in the border region from the second half of the 1990s and onwards. It also shows that there is no corresponding drop in annual earnings. Moreover, the lower left panel illustrates the strong influx of Norwegians residing close to the border

during this time period. Further, the lower right panel displays that a relatively large fraction of the Norwegian born population appears not to be working in Sweden.

The results in Table 9 can to some extent help us in understanding these observations. The table shows that a decline in the value of the Swedish krona is negatively correlated with the overall employment rate observed in Swedish data. That is, while the retail industry is positively affected by exchange rate depreciations the overall employment effect is negative. Why is hard to tell and by nature the following discussion becomes speculative. One interpretation is that a larger share of the population is employed in Norway as the relative value of a Norwegian salary increases following currency depreciations. Another explanation is that there other forces that drive the negative effect such as e.g. the increasing costs of imports from Norway.

Annual earnings, however, grow conditional on employment, and the Norwegian population share increases (Table 9). As the effect on earnings is conditional on having positive earnings from labor in Sweden this does not contradict the negative employment effect. The size of the point estimates indicate that a ten percent decline of the value of the Swedish krona is associated with a lagged increase of annual earnings with around 1.5 percent. An effect that is smaller than the earnings effect on the retail industry. Several explanations can explain this pattern. For example, they could potentially be driven by spillover effects from the retail industry, or other sectors in the economy that are positively affected by real exchange rate movements.

Moreover, Figure C2 illustrates that by the end of 2009 a large fraction of the Norwegian born population appear not to be working in Sweden. This is suggestive of Norwegians moving to Sweden while staying employed in Norway following currency depreciations. This is plausible as a move to Sweden implies both a higher salary in real terms and relatively cheap housing.

Table 9. Effects on the full economy

	Employment		Log (Annual earnings)		Share of the population born in Norway	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Real exchange rate	-0.003	-0.028*	0.072*	-0.012	0.025***	0.006
	(0.013)	(0.011)	(0.035)	(0.035)	(0.006)	(0.004)
Real exchange rate t-1	-0.014	-0.022*	0.183***	0.154***	0.037***	0.030***
	(0.010)	(0.011)	(0.034)	(0.035)	(0.007)	(0.006)
N	28550	28550	28492	28492	28550	28550
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

6. Conclusions

Lower relative prices make it more attractive for foreign consumers to pass the border to go shopping. In this paper I explore demand shifting effects of real exchange rate movements in border regions. The analysis shows that the number of employees in retail, the share of the population working within the industry and annual earnings increase following a decline in the value of the Swedish krona. Since the late 1990s the krona gradually declined in value and the retail industry grew larger. By the end of the first decade of the new millennium the whole economy in the border region had tilted towards greater dependence on retail trade.

The evidence presented in the paper relates to the papers by Campbell and Lapham (2004) and Baggs et al. (2013) that studied demand shifting effects on service industries in the US and Canada respectively. The Swedish-Norwegian setting is however somewhat different. Two features stand out. Firstly, the relative price differences between Norway and Sweden are very large. This is not only the consequence of the relative decline in the value of the Swedish krona. It also reflects Norway's protectionist trade policy as well as the strong economic growth in Norway the last decades. Secondly, the Swedish side of the border is predominantly rural with a low population density. Thus, in the absence of the large relative price differences it is likely that the retail industry in this part of Sweden would be relatively small.

This highlights the potential vulnerability of the region. Strong currency depreciations decrease the incentives for Norwegians to shop in Sweden. This in turn is likely to cause an economic downturn in the border area. In the light of the large price differences between Norway and Sweden the

value of the Swedish krona however would have to increase substantially before all price differences would be erased. Nevertheless, given the relative fall in overall employment in the border area the last decades it is hard to abstract from the growing importance of foreign demand for this region.

From the point of view of the border regions, changes in the real exchange rate are exogenous. That is, other forces than cross-border shopping are likely to be more important determinants of the value of the bilateral exchange rate. The bilateral trade flow and the discrepancy in growth rate between Norway and Sweden, for example, arguably play a much larger role. Thus, in policy terms there will always be a great amount of uncertainty regarding the growth potential of industries that rely on foreign customers. This factor should potentially be taken into account in discussions regarding e.g. investments in infrastructure and the construction of new shopping malls, among other things, along the border.

Left for future research, or a natural extension of this paper, is to explore the mirror image: the effects off cross-border shopping on the Norwegian side of the border. It is likely that these effects are substantial due to the high volumes of shoppers passing the border every day. It is also likely that the effects are geographically less concentrated than in Sweden. One indication of this is the survey evidence discussed that shows that a large share of the foreign consumers drives great lengths to reach the border. In contrast the results from this study show that the impact of cross-border shopping is limited to the immediate border area. This indicates that after having passed the border the incentives to drive further than needed are limited. Thus, the catch-up area of potential consumers within Norway appears to be larger than the affected area in Sweden.

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Appendix A. Variable Definitions

Table A1. Variable definitions

Variable	Definition
Number of employees in retail industry	= Number of individuals earning their primary income from the retail industry in a SAMS-area. Swedish Standard Industrial Classification 1992 used.
Share employed in retail industry	= Number of employees in retail industry dived by the population in a SAMS-area.
Annual earnings from retail industry	= Mean annual earnings if primary employer belongs to the retail industry in a SAMS-area.
Wage sum	= Sum of all wages paid out by establishments in retail in a SAMS-area.
Number of employment spells	= Sum of employment spells in retail outlets in a SAMS-area.
Number of employees in retail industry in November	= Sum of employees reported by establishments to Statistics Sweden in a SAMS-area.
Employment	= Mean employment rate in a SAMS-area. Employment defined as having positive earnings from labor.
Annual earnings	= Mean earnings from labor unconditional on industry in a SAMS-area.

Appendix B. Additional Results

Table B1. Labor market outcomes – Treatment within 75 km

	Log (Number of employees in retail)		Share employed in retail		Log (Annual earnings in retail)	
Real exchange rate	0.091	0.068	0.018***	0.007	0.105	0.057
	(0.105)	(0.064)	(0.005)	(0.004)	(0.071)	(0.065)
Real exchange rate t-1	0.067	0.059	0.010*	0.006	0.202*	0.184*
	(0.077)	(0.080)	(0.004)	(0.004)	(0.079)	(0.083)
N	27398	27398	28550	28550	27304	27304
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table B2. Labor market outcomes – varying the control group

	Control 50-100 km		Control 50-200 km		Control 50-200 km excl. Göteborg	
Employees in retail	0.658***	0.384**	0.532**	0.220	0.811***	0.351**
	(0.187)	(0.137)	(0.170)	(0.128)	(0.170)	(0.127)
N	11111	11111	70978	70978	43791	43791
Share employed in retail	0.051***	0.032**	0.056***	0.029**	0.062***	0.032**
	(0.012)	(0.010)	(0.012)	(0.010)	(0.012)	(0.010)
N	11454	11454	75626	75626	45909	45909
Annual earnings in retail	0.402***	0.179	0.473***	0.263**	0.509***	0.295**
	(0.102)	(0.096)	(0.088)	(0.083)	(0.090)	(0.085)
N	11076	11076	70708	70708	43615	43615
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table B3. Labor market outcomes – dynamic model without lags

	Log (Number of employees in retail)		Share employed in retail		Log (Annual earnings in retail)	
Real exchange rate	0.291**	0.200**	0.024***	0.012	0.306***	0.178
	(0.067)	(0.063)	(0.005)	(0.005)	(0.066)	(0.072)
y_{it-1}	0.655***	0.615***	0.650***	0.649***	0.304***	0.303***
	(0.010)	(0.009)	(0.012)	(0.012)	(0.009)	(0.009)
N	26082	26082	27400	27400	25964	25964
Linear trend		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table B4. Labor market outcomes – controlling for Norwegian real GDP

	Log (Number of employees in retail)		Share employed in retail		Log (Annual earnings in retail)	
Real exchange rate	0.174	0.170	0.018**	0.018**	0.182*	0.181*
	(0.094)	(0.090)	(0.007)	(0.007)	(0.078)	(0.078)
GDP Norway	0.234***	0.275**	0.017***	0.020**	0.110**	0.148*
	(0.066)	(0.103)	(0.003)	(0.007)	(0.035)	(0.070)
N	27398	27398	28550	28550	27304	27304
Linear trends		X		X		X

Notes: Robust standard errors clustered on SAMS within parentheses. Regressions are weighted by the number of underlying observations. Sample includes 18-64 year olds residing in SAMS-areas within 150 km from the nearest border crossing. Controls include age, sex, immigrant status, SAMS-area fixed effects and year fixed effects. The linear trend is distance specific.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Appendix C. Figures

Figure C1. Treatment and control

Sweden

0-49 km
50-149 km
150+ km
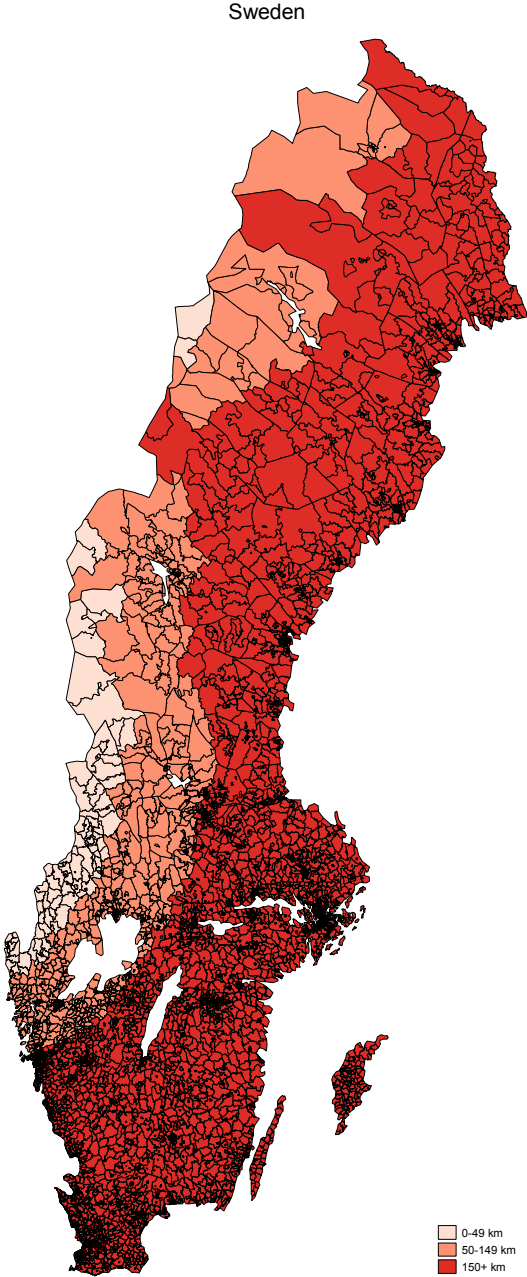
Figure C2. Labor market outcomes and population characteristics
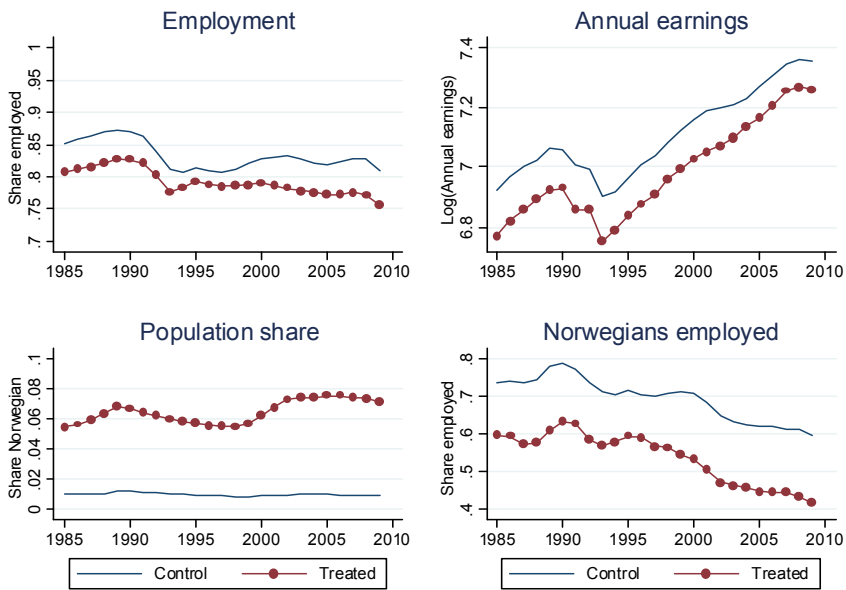
Notes: All individuals residing within 50 km from the nearest border crossing are included in the treated group. The figures are based on the full population aged 18-64 years old residing within 150 km from the nearest border crossing and that are observed at least once between 1985-2009. Employment is defined as having positive earnings from labor. The two upper panels and the lower left panel include the full population. The lower right panel is restricted to the Norwegian born population.

Essay 3. The Value of Earning for Learning: Performance Bonuses in Immigrant Language Training◆

Co-authored with Olof Åslund

1. Introduction

A central idea in economics is that we act on economic incentives: increasing the rewards for a certain type of behavior will make us more likely to behave in that way. Despite the theoretical predictions, the empirical evidence from education research is mixed. While a number of randomized trials suggest that financial incentives do improve student achievement (e.g. Angrist, Lang and Oreopoulos 2009, Angrist et al. 2002, Kremer, Miguel and Thornton 2009, Angrist and Lavy 2009, Dearden et al. 2009, Dee 2009 and Pallais 2009) there is also evidence of more limited or no effects (e.g. Angrist, Oreopoulos and Williams 2010, Fryer 2011, Bettinger 2008 and Sharma 2010).

This paper evaluates a pay for performance scheme from a setting not studied before: performance bonuses in language training for immigrants. The experiment is unique in the sense that it tests how financial rewards affect basic human capital accumulation among adults and because it is an incentive program targeted at immigrants. The lack of economic and social integration among the immigrant population is a major concern for policy makers throughout most of the industrialized world and host country language proficiency is generally considered a key factor in promoting economic as well as political and social inclusion. There is plenty of evidence that the labor market rewards such skills[1] and many countries spend substantial resources on language training for immigrants. Whether costs are considered too high or the perceived benefits are seen as too low is hard to tell, but it is a fact that many immigrants never come to master the host country language (Tubergen and Kalmijn 2005; Rooth and Åslund 2006).[2]

Our data come from a Swedish policy pilot implemented in 2009–2010 within the already existing Swedish language tuition program for immigrants. Municipalities which had expressed an interest in participating in the pilot were matched into pairs, and then allocated to treatment and control by pair-wise randomization. In treated municipalities, migrants passing a bonus-rewarding course within fifteen months after immigration but no later than a year after the course start were awarded up to SEK 12,000 (about 1,350 Euros). The data used include all immigrants since 2006 and we are able to investigate both performance and enrollment effects, as well as heterogeneous effects in terms of student and course characteristics.

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[1] See, for example, Bleakley and Chin (2004), Chiswick and Miller (2002), Carnevale and Lowell (2001), Chiswick and Miller (2010), and Carliner (1999) for the US, Dustmann and Fabbri (2003) for the UK, Dustmann (1994) and Dustmann and Van Soest (2002) for Germany, Chiswick and Miller (1995) for Australia, Chiswick (1998) for Israel. The list is not exhaustive. Early work includes McManus, Gould and Welch (1983), Borjas (1984), Carliner (1980), Tainer (1988) and Kossoudji (1988). Swedish evidence is given in Rooth and Åslund (2006).

[2] This is also true for migrant youth; see for example OECD (2012).

The analysis shows that the introduction of performance bonuses had a substantial positive average effect on student achievement. However, the effects are concentrated to the metropolitan areas in the sample; in the other participating municipalities, performance was not affected. In the metropolitan areas, student achievement improved both for courses qualifying directly for a bonus, i.e. continuation courses, and for beginner's courses, even though the relative impact was higher for the bonus-awarding courses. The only clear impact on enrollment is found for bonus-awarding courses in metropolitan areas.

The previous literature suggests that economic incentives may or may not be an effective tool in education. Our analysis gives several indications that perceived feasibility and learning costs may be important. The performance criteria for earning the bonus were quite strict relative to outcomes in the system. We show that the probability of meeting them depends strongly on starting early after arriving, and that the institutions of the metropolitan areas (where effects are found) were more beneficial to early starts already before the trial. The impact is also clearer in the study paths containing the more skilled students and exhibiting the shortest average completion times throughout the observation period. Furthermore, the impact is particularly strong for EEA (European Economic Area) migrants, whose native language and family situation are likely to make learning costs lower. Similarly, we see greater effects among the young, where costs are likely to be lower and financial rewards arguably higher valued.

The paper proceeds as follows: in Section 2 we discuss theoretical arguments and expected impacts, empirical evidence on financial incentives in education, and the link between economic integration of immigrants and language skills. Section 3 describes the Swedish tuition system for immigrants in which the experiment was conducted. In Section 4 we present the policy pilot and in Section 5 we discuss the data and some initial descriptive statistics. Section 6 first presents the empirical strategy and specification, and then turns to the empirical results and robustness checks. Section 7 concludes.

2. Theoretical Considerations and Previous Evidence

2.1. Financial Awards in Education – Motivations and Expectations

In a stylized world, one could argue that there are essentially two reasons for why policy wants to consider influencing individual decisions and actions: (i) people do not know their own good (i.e. paternalism); (ii) there are externalities that are not internalized by individual decisions.

Regarding the first argument, people may underinvest in education, including language skills, if they have time inconsistent preferences or misperceive education costs to be too high or the returns to be too low (Rodriguez-Planas 2010). Under such circumstances, influencing an individual to invest more in education will increase his/her utility (ex post).

The second argument considers the utilities of others, or society at large. There are numerous possible spillover effects from having a more well-educated population. For example, it has been suggested that it positively affects economic growth, innovation, democratic stability, and so forth (e.g. Moretti 2004 and Krueger and Lindahl 2001). Given such effects, it may be optimal for society to stimulate investments in education. In a welfare state such as Sweden's, there are of course also more mundane external effects; immigrants who learn the host country language are more likely to be employed and pay taxes and less likely to depend on social benefits (more on this link below).

Whether behavior is actually influenced by the introduction of new forms of economic incentives, depends on the comparison between costs and gains, which is likely to vary across individuals and groups. A potential drawback of performance bonuses is that there may be deadweight losses, if not only those who change their behavior earn the bonus. Both these arguments consider the possibilities of designing the bonus scheme in an appropriate way to maximize the incentive effect while minimizing deadweight costs. There may also be directly negative performance effects of a bonus, if it, for example, causes stress or affects people to opt for more risky study strategies. It has, for example, been suggested that economic rewards crowd out self-motivation and curiosity among other things (e.g. Deci et al. 2001).[3]

So far the arguments circle around individual behavior and decisions. But it can also be argued that implementing a bonus in one setting and (as in our example) for some participants can have positive or negative effects on others. Positive peer effects can arise if students aiming for a bonus encourage their peers to also work harder. Opposite effects can arise, for example, due to jealousy or crowding-out of teaching resources. If the bonus is seen as unfair, it is also possible that it would trigger negative sentiments at an aggregate level.

2.2. Empirical Evidence on Economic Incentives in Education

A number of randomized trials have been carried out to test whether financial awards alone or in combination with educational support services

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[3] There is a long debate in psychology on whether extrinsic motivation crowds out intrinsic motivation; see, for example, Deci et al. (2001) and Cameron and Pierce (2002) for opposing views, and Rodríguez-Planas (2010) for an overview of mechanisms. Moreover, see Leuven et al. (2010) for evidence of financial awards crowding out intrinsic motivation for weak student groups.

can improve student achievement or lower dropout rates.[4] The collected evidence is mixed.

Positive effects on student achievement and/or dropout rates of introducing financial awards to students in elementary schools, secondary schools and colleges are found in studies from various countries (e.g. Angrist, Lang and Oreopoulos 2009, Angrist et al. 2002, Kremer, Miguel and Thornton 2009, Angrist and Lavy 2009, Dearden et al. 2009, Dee 2009, Jackson 2010, and Pallais 2009). For example, in an attempt to improve student performance at a Canadian college, academic support services and/or financial incentives were assigned by randomization to new students (Angrist, Lang and Oreopoulos 2009). The results show that the take-up for services was higher for women and that the combination of economic rewards and support services had a positive effect on study results for this group. Furthermore, the effects were not limited to the year of the intervention.

In contrast, there is also evidence from randomized experiments from the US, Canada and elsewhere suggesting that financial incentives play a limited role or are not effective (Angrist, Oreopoulos and Williams 2010, Fryer 2011, Bettinger 2008, and Sharma 2010). Probably the largest experiment up to date was carried out on more than 200 elementary schools in three different metropolitan areas in the US (see Fryer 2011). Interventions were randomized on the school level and included financial incentives for reading books, improved classroom grades and awards for interim assessments. Overall, the study gives little support for this type of interventions.[5]

Thus, what to expect a priori from the experiment analyzed in this paper is not obvious. Furthermore, none of the experiments discussed in this section were directed towards newly arrived immigrants or adults, nor did they focus on language proficiency. Financial incentives appear to have an effect on achievement in some settings, but the evidence also suggests that it is no panacea working in all cases.

2.3. Immigrant Language Skills and Labor Market Outcomes

A prime reason for the political interest in promoting host country language acquisition is its expected impact on labor market outcomes. Like many other OECD countries, Sweden exhibits major native-immigrant differences in the labor market (e.g. Sébastien et al. 2010). In 2009, employment in the foreign-born population was 62.5 percent. This could be compared to 75.7 percent among natives (Eriksson 2011). There are naturally also big

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[4] For a more extensive overview see Rodriguez-Planas (2010).
[5] In an attempt to summarize the literature, Angrist, Oreopoulos and Williams (2010) draw the conclusion that if incentives work they appear to have larger effects in elementary and secondary schools than on the university level.

discrepancies within the group of foreign-born. In general, immigrants arriving for humanitarian reasons and succeeding family members perform substantially worse than immigrants arriving from OECD countries.[6]

Language proficiency is arguably a key component of the skill acquisition often assumed to explain much of the relatively sharp increase in earnings among recently arrived migrants (see e.g. Borjas 1999, LaLonde and Topel 1997 for overviews, or Friedberg 2000, Bratsberg and Ragan 2002, and Berman, Lang and Siniver 2003). The literature contains abundant evidence that migrants who master the dominant language of the destination country have higher earnings than migrants who lack such skills. Moreover, destination country language skills have been suggested to be associated with lower unemployment rates and higher employment levels as well as decreasing consumption costs (Chiswick and Miller 1998). Better language skills could, for example, lower the search cost for housing or other particular goods.

An inherent problem in the literature is to establish whether the acquisition of host country language skills has an effect on labor market outcomes or whether it is merely an association. If language proficiency is correlated with individual ability this unobserved heterogeneity is likely to bias the estimates of simple correlation studies. It has also been pointed out that measurement errors are common when measuring language skills (e.g. Dustmann and van Soest, 2001 and 2002). The more recent literature tries to address these issues (e.g. Bleakley and Chin 2004, Dustmann and Fabbri, 2003, and Dustmann and Van Soest, 2002) and to no surprise the overall message stands; i.e. the acquisition of destination country language skills is of importance for the labor market success of immigrants.[7]

Despite the potential benefits, many immigrants never become proficient in the language of the host country (e.g. Tubergen and Kalmijn 2005 and Rooth and Åslund 2006, (for results on Sweden)). Other factors that may be important for fluency is exposure to the language of the host country and efficiency (e.g. Chiswick and Miller 1995 and Chiswick and Miller 1998). Some studies find that host country language proficiency is inversely related to the size of the individual's linguistic community (Lazear 1999; Borjas 2013). Efficiency refers to the process of translating exposure into actual

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[6] The inflow of immigrants to Sweden has been relatively large the last decades and in the end of 2011 about fifteen percent of the Swedish population was born abroad. This corresponds to an increase of four percentage points (423,000 individuals) since the year 2000 (SCB, 2012). The five largest source countries among immigrants arriving in 2011 were Iraq, Poland, Afghanistan, Somalia and China (SCB, 2012). During the last decade, on average, 15 percent of all residence permits were granted to refugees, 36 percent to family members of earlier immigrants or Swedish born residents and 37 percent to labor immigrants including immigrants from the European Economic Area (EEA) and the rest of the world (Migrationsverket, 2012).

[7] One exception is Hayfron (2001) who does not find a link between language training for Thirld world immigrants in Norway and earnings. See footnote 1 for addtional references.

skills (Chiswick and Miller 1998) and, for example, age at the time of migration appears to be of importance; young individuals are more likely to become more fluent in a second language (Long 1990). The educational background of immigrants similarly appears to affect the possibilities of acquiring language skills. In general, immigrants with more schooling pick up language skills more easily (Chiswick and Miller 1998). The linguistic distance between the native language and the dominant language probably also affects the pace at which language skills are acquired.

3. The Swedish Language Tuition System for Immigrants

The bonus scheme under study was implemented within the Swedish tuition system for immigrants. Immigrants to Sweden have been offered Swedish tuition in one form or another since the 1960s (Kennerberg and Sibbmark 2005). The aim of this educational program is to provide adult immigrants with basic Swedish language skills (Skollag 2010:800)[8]. The scheme known as Sfi (Swedish for immigrants) is free of charge and also aims to provide basic reading and writing training to immigrants lacking such skills.

It is an ambitious program. In 2011 around 102,400 people were enrolled. About 66 percent of the long-term migrants between 1994 and 2003 aged 20 to 55 years old (excluding immigrants from Norway, Denmark and Finland) started Sfi within a year after immigration (Kennerberg and Åslund 2010). The number of immigrants enrolling into Sfi for the first time is naturally related to the number of immigrants to Sweden a particular year.

It is the responsibility of the municipalities to provide language training programs but the municipalities can contract other providers. In 2010 about 35 percent of the students were enrolled in courses offered by private institutions, adult educational associations (studieförbund) and folk high schools (folkhögskolor) (Swedish National Agency for Education 2010A).[9] The municipalities finance the basic language training by a combination of grants from the state and local taxes. The state compensates municipalities that receive refugees and this compensation normally covers the cost of providing language courses, among other things. For other groups of immigrants the municipalities finance the program through the tax system (Kennerberg and Sibbmark 2005). Since 2007 municipalities also receive a lump sum from the central government for refugees who have either passed a Sfi course within 12 months after immigration, or if a refugee has worked

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[8] This law from 2010 replaced Skollag 1985:1100 that had been in place since 1985 but the purpose of the scheme however remained the same.
[9] All providers participated in the experiment studied here.

or had an internship for at least 5 months during the first 12 months in Sweden (SFS 2007 and SFS 2009).[10]

Immigrants interested in enrolling into Sfi should be offered a place within three months after fulfilling the requirements to participate. Apart from lacking basic Swedish skills the only additional criteria for admission is that the immigrant should be registered as a resident in a municipality and be at least sixteen years old.[11] In general immigrants that apply to participate can be divided into two groups: refugees and other immigrants. About one third belong to the former group (Swedish Schools Inspectorate 2010) and this group is often assigned to language training through the introduction programs organized for refugees. The second group is more heterogeneous; some students are directed to Swedish tuition through the social insurance system or the employment services, others enroll voluntarily.

The bonus program was thus implemented in a setting where there are already strong incentives to participate (in addition to those provided by the expected gains from learning the Swedish language). The conditions vary depending on immigrant category and individual characteristics and situation, but for a large share of the immigrants going to the language courses can be considered mandatory for receiving financial and other forms of support from society.

The Swedish tuition system for immigrants is regulated in more detail by SKOLFS 2009:2 (replaced in 2012 by SKOLFS 2012:13). It is a regulation that describes the purpose and aim of the educational program as well as the structure of the training programs. Detailed goals for the courses are also specified. The structure of the educational scheme is roughly sketched in Figure 1.

It contains three study paths: Sfi1, Sfi2, and Sfi3. The different study paths are targeted to groups that differ in their educational background. On average the language courses should include at least 15 classroom hours per week. The length of a course could vary depending on the educational background of the participants, but there is a target (although not a limit) of 525 hours. Standardized tests are used as a tool for grading on course B, C and D indifferent of the study path and the tests are given throughout the year.

The student group is heterogeneous, which is partly reflected by the differences in schooling from the home country. For example, the average years of schooling for students following path 1 is only 4.5 years which could be compared to fourteen years for students on study path 3 (Figure 1). Each study path contains two courses. A course is either a beginner's course

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[10] In principle, this would be an interesting topic for an(other) evaluation.
[11] A few exceptions exist. Norwegian and Danish citizens are in general not eligible to the scheme. Furthermore, Finnish citizens residing in Finland but working in Sweden are under certain criteria eligible to language training programs.

or a more advanced course depending on the study path. One example is course B in study path 1 and 2 that has the same goals indifferent of the study path, but different structures to better fit the educational background of the students and their previous knowledge of Swedish. After finishing a course the student can make progress by starting a new course. All students have the right to progress up to the most advanced course, i.e. course D on study path 3.

Figure 1. The Swedish language tuition system for immigrants
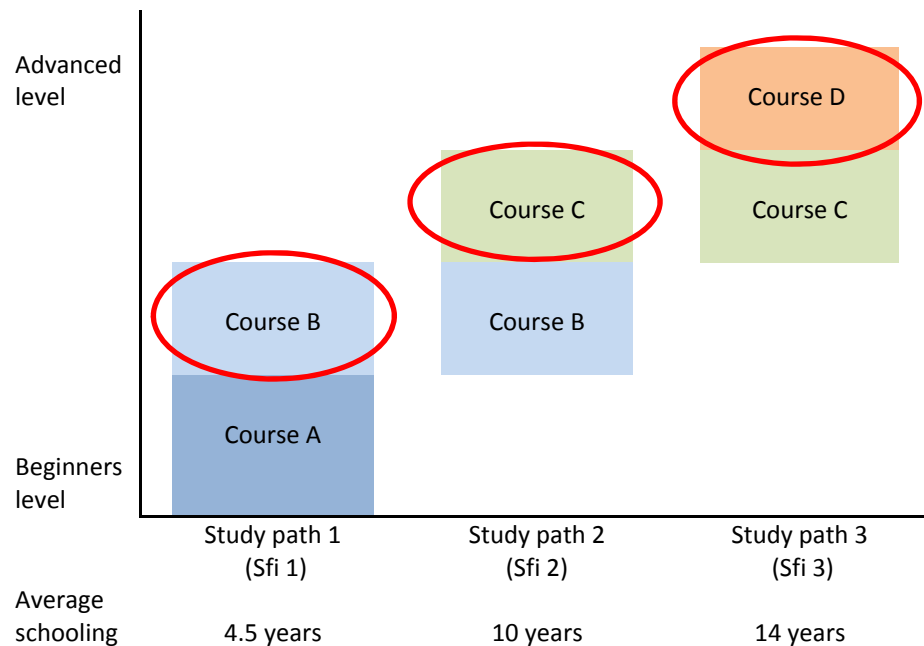
Notes: The bottom row shows mean years of schooling in the source country. The average is calculated for beginners of Sfi between 2009-07-01 and 2010-06-30. A circle indicates that the course entitles for a bonus payment under certain criteria (see Section 4.1).

3.1. Previous Studies of the Sfi Tuition System

Deficiencies of the tuition system have been stressed in a number of reports (e.g. Statskontoret 2009, Riksrevisionen 2008 and Eriksson 2007, Swedish Schools Inspectorate 2010). For example, the share of dropouts and the number of students still enrolled after three years in the system has been highlighted as potential problems (Eriksson 2007 and Statskontoret 2009). Official statistics show that of those enrolling in 2009, 60 percent had completed at least one course by 2011, 29 percent had dropped out temporarily or permanently, and 11 percent were still enrolled. Outcomes were somewhat better among women than among men, and the recent

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figures compare favorably to statistics from earlier years (Swedish National Agency for Education 2012).

Whether the results of Sfi are satisfying or not does not only depend on the actual pass rate of the courses but also on the alternatives to taking the courses. If a person drops out because he or she finds a job it is questionable whether that should be considered a failure of the system. Non-participants work to a larger extent than participants. On the other hand there is a considerable group that has not enrolled into Sfi that has a rather weak position on the labor market or depend on social assistance (Kennerberg 2009).

Despite the scope of the program, its labor market effects have received little scholarly attention. Kennerberg and Åslund (2010) studied the correlation between participation in Sfi and the later success on the labor market by the use of a matching procedure. They concluded that immigrants who enrolled into Sfi initially had lower employment rates and earnings relative to "comparable" non-participants, but that earnings converged after ten years in the country and that employment rates were surpassed by around five percentage points. A strict causal interpretation of the results is not possible, however, due to methodological constraints. A thorough discussion and various approaches to an empirical analysis can also be found in Riksrevisionen (2008).

4. The Design of the Policy Pilot

The intention of the policy pilot was to test whether providing economic incentives improve student achievement and/or attract more immigrants to language training courses. Increased language skills were in turn hoped to ease the transition to the labor market (Prop. 2008/09:156). To this end a performance bonus (in Sweden known as Sfi-bonus) was introduced in a limited number of municipalities. The policy pilot was run by the central government, which cooperated with the Institute for Evaluation of Labour Market and Education Policy (IFAU) in designing the experiment.

A selected group of fifty municipalities, based on a "sufficiently" high number of students in the municipality (given the even number of inquiries one may suspect that the cut-off was somewhat arbitrary), were inquired about their willingness to participate in the policy pilot by the Central Government Offices. The municipalities were informed that participants would receive additional funds for quality improvements within Sfi and that a maximum of fifteen municipalities would be entitled the right to pay out performance bonuses funded by the central government. Indicating an interest the municipality agreed to participate, regardless of whether it was assigned to the treatment or to the control group. Thirty-five of the fifty

municipalities that received the proposal indicated their interest to participate.

Given this, there were five types of municipalities: (i) municipalities that would be able to pay out performance bonuses and receive additional funds for quality improvements; (ii) municipalities that would receive additional funds; (iii) municipalities that had shown interest in participating, but were not included in the pilot; (iv) municipalities that rejected the proposal; and (v) municipalities that were not offered to participate. By comparing group (i) and (ii) it is possible to evaluate the effect of the bonus.[12]

The next step was to divide the 35 municipalities that had accepted the proposal to participate into group (i), (ii) or (iii). Five broad criteria were used. First, a maximum of 15 municipalities were allowed to pay out performance bonuses. Second, group (i) and (ii) should include a high and similar number of expected participants. Third, group (i) and (ii) should be roughly comparable in terms of population size, labor market conditions and geography. Fourth, the whole country should be represented, and fifth, the major cities of Stockholm, Göteborg and Malmö (who were all interested in the pilot) should be represented in each of (i), (ii) and (iii).

Following these criteria, pools of pairs of municipalities were constructed. One exception to the sample criteria was that the municipality pair including Stockholm and Göteborg was balanced by adding Uppsala and Södertälje (who both belong to the Stockholm local labor market region) to Göteborg. To meet the criteria of the large cities being represented in group (i) – (iii), Malmö was chosen not to be included in the pilot. Thus, 13 pools of municipalities were constructed to not exceed the maximum limit of 15 municipalities getting the right to pay out the performance bonus. Out of the pool of the 35 municipalities that showed interest in participating, seven municipalities were assigned to group (iii).[13] Finally, within the pairs treatment was assigned randomly (the outcome of the randomization process is discussed below).

4.1. The Performance Bonus

The municipalities belonging to group (i), the experimental group, were permitted to pay out a performance bonus to students within the Swedish tuition system for immigrants under certain conditions. To be eligible for a performance bonus the following rules applied:

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[12] By comparing outcomes in (ii) with (iii) and (iv) the effect of additional funds can also be evaluated to some extent (however, without the random component). Åslund and Engdahl (2012) present results from such an analysis, suggesting no impact on performance but a possible faster enrollment from the additional funds.
[13] These municipalities were excluded as no suitable municipality pair was found and because the Government had set the maximum number of municipalities that could implement the bonus scheme to fifteen.

i.   The immigrant must have registered as a resident in any munici-pality for the first time between the 1st of July 2009 and 30th of June 2010 and should have revived a residence permit in accord-ance with Aliens Act, Chapter 5, paragraph 1, 2, 3, 3a, 4, 5 or 6.

ii.   The participant in the Swedish tuition system for immigrants should be between 18 and 64 years old.

iii. The participant should have received a pass or a pass with distinc-tion on one of the following courses: study path 1, course B; study path 2, course C; or study path 3, course D in one of the treated municipalities.

iv.   The grade should have been received within 12 months after the course start but no later than 15 months after immigration.

The types of residence permits that were required include, in general, permits granted to refugees and family immigrants. Thus, labor migrants and guest students were not eligible. Consequently, most immigrants from EEA/EU were not eligible while the majority of the immigrants from other parts of the world fulfilled the requirement.[14] Municipalities that introduced the bonus system were also required to inform newly arrived immigrants about the conditions and requirements surrounding the bonus system.

Regarding the time frame, the average number of weeks between course start and completion varies across study paths and courses. For immigrants enrolling into Sfi for the first time in 2008, the average number of weeks before receiving at least a pass on any course was 59 weeks. For beginners on the bonus courses, i.e. course B, study path 1, the average was 57 weeks; course C, study path 2, the average was 50 weeks; and for course D, study path 3, 19 weeks (Swedish National Agency for Education 2010B). Note that these averages are not restricted to newly arrived immigrants.

If fulfilling these requirements an immigrant could apply for a bonus payment from the municipality.[15] The application should have been handed in no later than three months after the completion of the course. The size of the performance bonus depended on the course. Bonus courses are highlighted by a circle in Figure 1.Course B, study path 1, yielded a bonus of SEK 6,000; course C, study path 2, gave SEK 8,000; and course D, study path 3, gave SEK 12,000. As a student had the right to progress until course D on study path 3, more than one bonus payment is possible per student. The total amount that a student could receive was however set to SEK 12,000.

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[14] Within the group of immigrants from EEA/EU, migrants granted residence permits were eligible, including family migrants applying for permits following the national legislation, but not immigrants with the "right of residence" (uppehållsrätt) that follow EU rules. The latter group dominates the group.

[15] The municipalities were reimbursed by the Swedish National Agency for Education for the payment of the bonuses and its related administrative costs.

4.2. Limitations of the Design

From a scientific perspective, there are endless ways of designing a bonus experiment. For practical implementation, however, it has to be politically feasible. While the bonus scheme provided a rare opportunity of a controlled large-scale design in a politically important and sensitive issue, it had its limitations.

In the process of setting up the pilot, a common objection was that it was unfair to offer the bonus in some locations but not in others. For this reason and for practical purposes, varying treatment across individuals in the same location was not an option. While the relatively few municipalities inquired covered a very large share of the Sfi students, it also means that randomization will only be across a limited number of units. The within-pair randomization was an attempt to make sure that the treatment and control group did not end up being too different. But while the ambition was to create similar matches with respect to the number of Sfi participants, geography and labor market characteristics, the small number of alternatives necessarily made some matches rather poor.

In section 6 we will discuss what the design did and did not bring, and how this affected the choice of empirical specification. First we turn to our data sources.

5. Data Sources and Descriptive Statistics

5.1. Data on the Foreign-Born Population and the Participants in Language Training

The database used for the evaluation contains a rich set of individual demographic variables as well as information on earnings, employment, and other labor market indicators. It covers the total Swedish population between 18 and 64 years old during 2006 to 2010. The content was mainly collected by Statistics Sweden (SCB). We also have access to information on country (group) of birth and to a table including the date of immigration of all individuals that have immigrated since 1985. We also make use of a register on participants in Swedish tuition for immigrants (Sfi) containing, among other things, individual information on enrollment into courses, course starts, the completion date, reasons for dropping out, and grades received. All registers are linked with a personal identifier.

The sample used in the main empirical analysis is restricted to immigrants who arrived to municipalities that were affected by the reform, i.e. introduced the bonus scheme, or belonged to the control group. We include foreign-born people that immigrated for the first time (since 1985) between July 1, 2006 and June 30, 2010, but apply a few exceptions. Following the

criteria for receiving a bonus we restrict the sample to those 18–64 years old. Immigrants from Norway, Finland and Denmark are also excluded as they in general are not eligible for language training courses.[16]

The Stockholm–Göteborg/Södertälje/Uppsala group dominates the sample, making up about sixty percent of the total number of the observations. We will therefore present three sets of estimates throughout: for the group containing Stockholm (hereafter referred to as metropolitan areas), other municipalities, and all municipalities. In some instances we will also split the sample in other dimensions, for example, region of origin, gender, and age. All demographic and labor market related characteristics are measured in the year of arrival.

Table 1 presents some characteristics of immigrants to the treated and non-treated areas.[17] The statistics refer to the pre-reform period to give a picture of how comparable the groups were prior to the intervention. In metropolitan areas, the immigrants were on average 31 years old, slightly more than half were men, half were married, and a little bit less than a quarter had children under 18 living at home. These demographics are well-balanced across treatment and control. People in the latter group, however, had a weaker economic position upon arrival: a larger fraction received social assistance, and earnings were lower on average. In terms of region-of-origin there are also differences. For example, immigrants from the Horn of Africa and Sudan are overrepresented in the treated areas and immigrants from Iraq are overrepresented in the control group (see columns 2 and 3).

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[16] Immigrants from Iceland are also excluded as they are grouped together with Norwegians for confidentiality reasons (see discussion above). This is likely to be a small concern as this is a small group of migrants.

[17] The full regression sample is found in Table A1 in the appendix.

Table 1. Pre-reform characteristics

	Metropolitan areas		Other municipalities		All municipalities	
	Treated	Control	Treated	Control	Treated	Control
Age	31.26	31.06	31.68	30.91	31.41	30.99
Gender (0=women)	0.54	0.54	0.53	0.51	0.53	0.53
Married or partner	0.46	0.48	0.53	0.52	0.48	0.50
Children	0.21	0.23	0.28	0.26	0.24	0.24
Social assistance	0.13	0.26	0.27	0.27	0.18	0.27
Annual earnings	430.3	268.7	284.9	299.5	378.0	282.3
Bosnia & Herzegovina	0.00	0.01	0.01	0.01	0.01	0.01
Former Yugoslavia	0.01	0.03	0.05	0.05	0.03	0.04
Poland	0.10	0.08	0.11	0.10	0.11	0.09
Ireland & UK	0.03	0.02	0.02	0.02	0.03	0.02
Germany	0.00	0.00	0.00	0.00	0.00	0.00
Mediterranean countries	0.05	0.03	0.02	0.03	0.04	0.03
The Baltics	0.03	0.02	0.03	0.02	0.03	0.02
East. Europe & fmr Soviet	0.09	0.08	0.09	0.07	0.09	0.08
Central Europe	0.01	0.02	0.02	0.02	0.02	0.02
France & Benelux	0.04	0.04	0.02	0.02	0.03	0.03
US & Canada	0.02	0.02	0.01	0.01	0.02	0.02
Central America	0.01	0.01	0.01	0.01	0.01	0.01
Chile	0.01	0.01	0.01	0.01	0.01	0.01
South America	0.04	0.02	0.03	0.03	0.04	0.02
Horn of Africa & Sudan	0.08	0.05	0.05	0.06	0.07	0.06
North Africa & Midd. East	0.06	0.06	0.06	0.06	0.06	0.06
Sub-Saha. Africa & Egypt	0.03	0.04	0.03	0.03	0.03	0.04
Iran	0.03	0.05	0.04	0.03	0.03	0.04
Iraq	0.08	0.18	0.16	0.16	0.11	0.17
Turkey	0.02	0.02	0.03	0.03	0.02	0.03
East Asia	0.06	0.07	0.04	0.07	0.05	0.07
South East Asia	0.04	0.04	0.06	0.06	0.05	0.05
South Asia	0.08	0.06	0.07	0.05	0.08	0.06
Australia and the Pacific	0.01	0.01	0.00	0.01	0.01	0.01
Not classified	0.00	0.00	0.00	0.00	0.00	0.00
Course start 3 months	0.29	0.21	0.29	0.25	0.29	0.23
Course start 6 months	0.41	0.39	0.44	0.43	0.42	0.41
Course start 12 months	0.48	0.51	0.54	0.53	0.50	0.51
Passed any course	0.16	0.20	0.23	0.23	0.18	0.21
Passed a bonus course	0.06	0.10	0.09	0.11	0.07	0.10
Passed other course	0.14	0.19	0.21	0.20	0.17	0.19
N	24910	21218	14016	16916	38926	38134

Notes: The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2009-06-30, i.e. before the reform, aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Finland and Denmark are excluded as they normally are not eligible for language training programs. All demographic and labor market characteristics are measured the year of arrival. Children refer to the presence of children under 18 in the household. Annual earnings are measured in 1000s of SEK. The outcome variables course start within 3/6/12 months refers to time before the first course start within Sfi after immigration. The outcome variables passed any course/a bonus course/other course are set to unity if an individual have completed a course within 15 months after immigration but no longer than 12 months after immigration, i.e. the bonus requirement was fulfilled.

Regarding "other municipalities" the differences between the group that implemented the bonus scheme and the control municipalities are small in terms of demographics and labor market outcomes (columns 4 and 5).

In the empirical analysis we focus primarily on study performance, specifically course completion, varying some of the criteria, for example time frames and types of courses). Table 1 reveals that for metropolitan areas, there are differences between the treatment and the control group in favor of the control group. In the next section we will address how these differences affect the empirical analysis. For other municipalities the differences in outcomes are small.

We will also present results on enrollment. The statistics shown in Table 1 here show that prior to the bonus pilot there was a substantial difference between the treatment and the control group in the fraction starting a course rapidly in metropolitan areas. Six months after immigration, however, the numbers had evened (see also figures B1-B3 in the appendix for a graphical presentation of this pattern).

While the data are quite rich, they lack two components of interest: (i) residence permit classification that would enable us to more accurately identify bonus eligibility at the individual level; (ii) information on whether the individual applied for and received a bonus. The first restriction means that to the extent that we wish to identify only those eligible, we would need to use some proxy based on country of birth and other variables (in addition to immigration date). This is perhaps not a great concern as one could argue that an evaluation should capture the overall effect of the reform, i.e. allowing for crowding-out and spillover effects. The second restriction mostly matters for descriptive purposes and is handled through another data source not linked to the main data (see immediately below).

5.2. Bonus Payments

The Swedish National Agency for Education constructed a database containing information on all the performance bonuses that have been paid out since the start of the experiment, and a limited set of individual characteristics. Using this data source in combination with database used for the evaluation, Table 2 shows that during our observation period a total of 1,005 bonuses were awarded. This corresponds to around seven percent of the immigrants that arrived to the municipalities that introduced the bonus scheme receiving a bonus. The variation between municipalities is large. More than half of the bonuses were paid out to students on course D on study path 3, i.e. the most advanced course within the tuition system (see Figure 1). The mean age of the individuals receiving a bonus was 30 years and the gender distribution was fairly equal. The table also shows that the number of bonus payments varies considerably across municipalities.

Table 2. Characteristics of the recipients of the bonus

Municipality	Number of bonuses	Share of migrants receiving the bonus	Men	Age	Study Path 1, Course B	Study Path 2, Course C	Study Path 3, Course D
Borås	81	0.13	0.58	28.4	0.16	0.46	0.38
Halmstad	52	0.12	0.48	28.1	0.02	0.17	0.81
Huddinge	57	0.05	0.33	30.1	0.02	0.16	0.82
Karlstad	55	0.15	0.49	28.8	0.11	0.16	0.73
Katrineholm	27	0.15	0.56	30.0	0.00	0.00	1.00
Nacka	51	0.09	0.49	28.1	0.04	0.18	0.78
Sandviken	20	0.12	0.35	29.4	0.10	0.05	0.85
Sollentuna	93	0.20	0.51	30.7	0.25	0.44	0.31
Stockholm	361	0.04	0.50	29.9	0.03	0.25	0.72
Trelleborg	9	0.05	0.78	25.4	0.00	0.00	1.00
Uddevalla	18	0.11	0.39	29.4	0.17	0.06	0.78
Växjö	148	0.25	0.49	28.4	0.33	0.44	0.23
Örnskölds-vik	33	0.24	0.64	29.7	0.00	0.09	0.91
Total	1005	0.07	0.50	29.3	0.11	0.27	0.62

Notes: Sample includes all migrants that received a bonus payment and that immigrated between 2009-07-01 and 2010-06-30. The share of migrants receiving the bonus is the share of immigrants receiving the bonus following data from Skolverket divided by the number of immigrants arriving to the treated municipalities during the treatment window. Source: Skolverkets Sfi-bonusdatabas and dataset used for the main analysis (see Section 5), own tabulations.

6. Empirical Analysis

Below we first discuss the choice of empirical strategy, based on the design of the pilot and descriptive statistics. This leads to an econometric specification used to retrieve the estimates on student achievement. After presenting the baseline results together with an investigation of heterogeneous impacts, we discuss a number of specification checks and also present some evidence on enrollment and achievement conditional on enrollment.

6.1. Choosing an Empirical Strategy

The design of the pilot in combination with the data available gives several options for the empirical strategy. The aim of this section is to outline the arguments guiding this choice.

In a randomized experiment with a sufficiently large number of observation units over which randomization is done, one could simply compare the mean outcomes of interest in the treatment and control groups

to get the treatment effect. Table 3 presents statistics for student achievement in the municipal pairs studied (see Table A2 in the appendix for a corresponding presentation of course starts). The pair-wise comparisons in the pilot period show significantly positive differences in four cases, negative in three cases, and no significant difference in six of the municipal pairs. The population-weighted difference presented in the top row of the table suggests no difference in the outcomes.

If one believed strongly in the outcome of the randomization, this could be it. But making the same comparison in the pre-period casts doubt on such a belief: outcomes were in fact significantly better in the control group prior to the reform. As for the pairs, some of the differences we saw in the pilot period existed already before. There are also examples of substantial changes within control municipalities over time, suggesting that outcomes in Sfi are affected by several factors outside the reform, and probably also includes substantial random variation.

A second alternative is thus to use a regression-discontinuity approach (see Lee and Lemieux 2010), exploiting the fact that treatment (in the sense of being eligible for a bonus) switches from one day to another based on the date of registration. This would mean largely ignoring the control group (and the randomization) and focusing on the potential shift in the treated regions just around the introduction of the bonus. While there may also be principal caveats to such an analysis (treatment effects could, for example, be gradual, and there are possible spillover effects or general equilibrium effects affecting also those arriving shortly prior to the reform), inspection of the data suggests that important conditions for an RD analysis are not met. In our context, the "running variable" along which the RD uses a discontinuity in treatment would be time. From Figure 2 and 3 below it is clear that there is a lot of seasonal variation in the outcome variable along this dimension (this is true also for course starts; see figures B1-B3 in the appendix).[18]

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[18] Immigrants arriving during the summer months on average have poorer Sfi outcomes. This is likely to depend on both the composition of the arriving migrants as well on institutional features. More labor migrants arrive during the summer for seasonal work. This group is less likely to be interested in starting Sfi. Similarly, migrants arriving during the summer holidays are more likely to miss the course starts in the autumn as it takes time to screen and place new migrants into Sfi. Furthermore, there is a sharp discontinuity starting in August, which for 2009 means the second month of treatment. Further inspection shows that the number of immigrants as well as their characteristics (gender, age, children) appear to change non-smoothly around the discontinuity. There is a peak in the number of residence permits that are granted in August/September in comparison with June, and immigrants arriving in the former months are on average younger and a larger share is male. Even though there are techniques for handling some of these problems, our interpretation is that the setting is not appropriate for an RD analysis.

Table 3. Student achievement, t-test

	Pilot period				Before pilot			
	Mean pass rate treated group	Mean pass rate control group	Diff. (Treated-Control)	P-value	Mean pass rate treated group	Mean pass rate control group	Diff. (Treated-Control)	P-value
All treated-all untreated	0.203	0.211	-0.008	0.110	0.185	0.214	-0.029	0.000
Stockholm-Göteborg	0.180	0.191	-0.012	0.055	0.157	0.200	-0.043	0.000
Huddinge-Haninge	0.171	0.219	-0.048	0.011	0.195	0.233	-0.037	0.001
Borås-Jönköping	0.297	0.320	-0.023	0.398	0.270	0.159	0.111	0.000
Växjö-Kalmar	0.276	0.155	0.121	0.000	0.250	0.187	0.063	0.001
Sandviken-Gävle	0.298	0.252	0.046	0.227	0.351	0.325	0.026	0.281
Nacka-Täby	0.088	0.177	-0.089	0.000	0.101	0.189	-0.088	0.000
Sollentuna-Solna	0.279	0.119	0.159	0.000	0.186	0.122	0.064	0.000
Halmstad-Helsingborg	0.266	0.215	0.050	0.042	0.294	0.281	0.013	0.405
Karlstad-Västerås	0.225	0.372	-0.147	0.000	0.306	0.322	-0.016	0.405
Trelleborg-Landskrona	0.144	0.111	0.032	0.265	0.127	0.133	-0.006	0.742
Örnsköldsvik-Härnösand	0.421	0.429	-0.007	0.917	0.416	0.307	0.109	0.003
Uddevalla-Trollhättan	0.453	0.289	0.164	0.000	0.380	0.247	0.133	0.000
Katrineholm-Nyköping	0.506	0.412	0.094	0.059	0.223	0.405	-0.182	0.000

Notes: The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30, aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Finland and Denmark are excluded as they normally are not eligible for language training programs. The outcome is defined as having passed a course within 15 months after immigration but no longer than 12 months after the course start, i.e. the requirement for receiving a bonus.

A natural alternative is then to consider a difference-in-differences (DD) approach, i.e. assuming that in absence of the bonus, the average development of outcomes over time is expected to be similar in the two groups. The identifying assumption is then that absent the reform, the development over time around the reform would have been the same in the treated and non-treated areas. While treatment is simply a before-after, the approach allows for controlling for general time effects in a flexible manner (for example, dummies for month of immigration). Given the patterns of Figure 2 and Figure 3, this is important. As will be discussed below, we will investigate the plausibility of the assumptions of the DD approach using several specification checks.[19]

Figure 2. Metropolitan areas
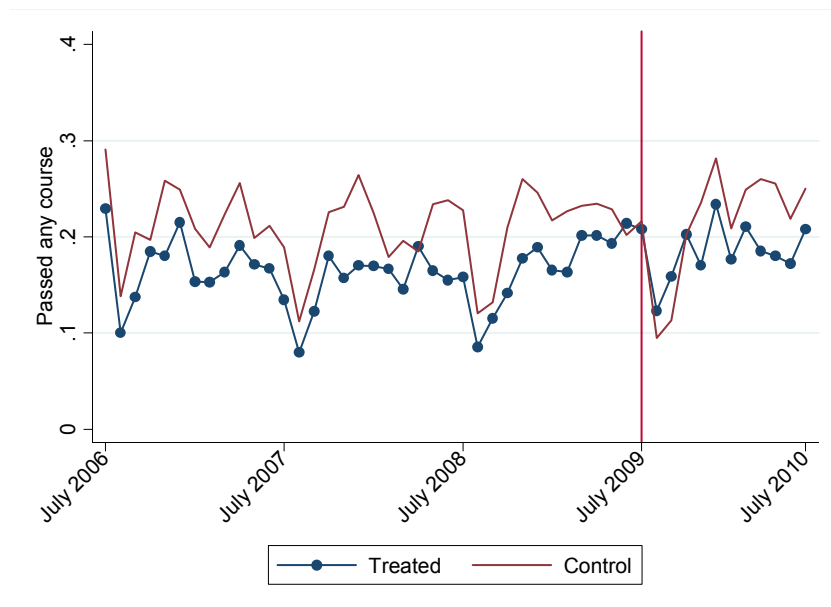
Notes: The figure shows the fraction of the immigrant population, by month of immigration, arriving between 2006-07-01 and 2010-06-30 that completes any course within 15 months from immigration but no more than 12 months after the course start unconditional on enrollment. The vertical line represents the introduction of the bonus scheme that occurred on the 1st of July 2009.

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[19] Note that since there is by definition no observed history for the studied population (the clock starts ticking when they register in Sweden for the first time), we are unable to use many of the more flexible estimators (e.g. conditional difference-in-differences) discussed in the literature (e.g. Heckman et al. 1999, and Bergemann et al. 2009).

Figure 3. Other municipalities
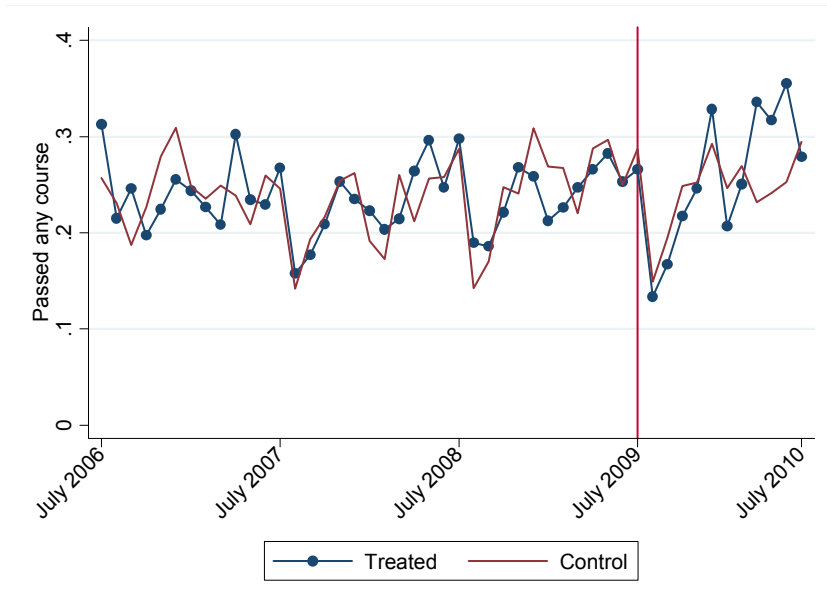
Note: See note Figure 2.

Given these considerations it is relevant to ask what the randomization process brings in terms of benefits for the empirical evaluation. First, a major advantage is that selection into the pilot was similar in the treatment and the comparison group; all the municipalities stated their willingness to participate in the trial under the same expectations. Second, within this group it was a random draw that decided treatment status – it was not the most interested among the interested that eventually got to try the bonus. Thus, even though the number of municipalities was not large enough to perfectly balance pre-reform outcomes and covariates, we may arguably be less concerned of selection on potential future outcomes; by definition, there is no self-selection.

6.2. Empirical Specification
Based on the discussion above, we specify the baseline model in the following way:

$$y_{ijt} = a + X_{it}\beta + \emptyset_t + \theta_j + D_{jt}\gamma + \varepsilon_{ijt} \qquad (1)$$

y_{ijt} is the outcome of interest (completion of a course with at least a pass, course starts). X_{it} is a vector of control variables including age, age squared, gender (0 if a woman), civil status, the presence of children in the household,

and country or region of birth. $\emptyset_t$ is a vector of immigration month fixed effects, $\boldsymbol{\theta_j}$ is a set of municipality fixed effects. Finally $\boldsymbol{D_{jt}}$ is an indicator taking the value one for immigrants settling on or after July 1, 2009, in municipalities that were included in the treatment group. $\boldsymbol{\gamma}$ is thus the average treatment effect of the reform.

Even though the specification above is quite flexible in terms of allowing for general time and location effects as well as individual covariates, it builds on some leap of faith. A somewhat formal way of testing the plausibility of the model is to run "placebo" regressions pretending that the bonus scheme was implemented on July 1, 2008, i.e. one year prior to the actual reform.[20] The idea is that if we see "effects" where there should be none, one should be cautious when interpreting the main estimates. We will also try augmenting the model with a group-specific linear time trend (thus allowing for a gradual divergence starting before the reform and assumed to be continuing after).

An always-present question in this type of analysis is whether and how to cluster the standard errors. We choose to cluster the baseline analysis on municipality interacted by immigration month. At this level some sort of dependence appears plausible, e.g. due to people being in the same class and meeting the same labor market opportunities. An alternative would be to use municipalities per se, i.e. the units over which randomization was done. This is a common choice in the literature (e.g. Angrist and Lavy 2009). For this reason the appendix presents estimates using this level of clustering (Table A3 and A10). [21]

On an opposite view, one can in fact argue that clustering leading to greater standard errors could be problematic in our setting. As discussed below we will to some extent lean on "placebo" regressions in interpreting the results. Here, a procedure resulting in larger standard errors means a higher risk of disregarding problematic pre-reform patterns. Åslund and Engdahl (2012) present the results using robust but not clustered standard errors in the baseline setting; the picture is the same as the one presented here.

In section 6.5 we discuss several robustness checks, including alternative definitions of outcomes and covariates, and restrictions of the sample.

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[20] We have also run placebo regressions pretending that treatment occurred on July 1st 2007, i.e. two year before the real reform limiting the observation window to +/- 12 months around the fictive reform. The results are in line with the placebo estimates discussed below.

[21] With this alternative the number of clusters (28) is then smaller than the level recommended by e.g. Angrist and Pischke (2009) to achieve the asymptotic properties. This problem becomes acute when we run separate regressions for metropolitan areas (4 clusters).

6.3. Effects on Student Achievement

We now turn to investigate the effects on student achievement. Our baseline empirical setup is chosen to allow for the reform to affect outcomes through any channel. By starting at immigration, we allow for the possibility that outcomes improve because more/other people register in the courses (although we find no strong support for the latter notion; see below). We also include all immigrants since also the non-eligible individuals in the treated areas may be positively or negatively affected by the existence of a bonus. The performance criterion for receiving a bonus was that one finished a bonus-granting course no more than 15 months after immigration and within a year from the course start. The introduction of the bonus may have influenced also other courses (e.g. through people being motivated by the bonus to advance faster within the system). It is thus relevant to investigate effects at different levels of aggregation. Table 4 therefore begins by showing the impact on any course completion, and then proceeds to look at different (types of) courses separately.

The estimates suggest that the effect on the probability of completing any course is statistically and economically significant in the metropolitan areas, but zero in other treated areas. Since metropolitan areas dominate the sample, the average effect for the overall treatment group is also positive and significant. The third row of estimates shows that courses not qualifying for the bonus were also affected. This is likely to be because most individuals start out in non-qualifying courses; working for the prize of a bonus then more or less requires passing the first course. It could however also be taken to indicate spillover through peer effects or through overall changes in teaching or local institutions. The effect on bonus courses is however twice as large as the effect on other courses evaluated at their respective pass rates.

Moving down the table to the estimates for individual courses, it is clear that the effects are greater for the more advanced tracks. For 3D, there is also a significant positive impact in treated municipalities outside the metropolitan areas. For 1B—the bonus course for the least advanced track— the estimate is actually negative and significant, although limited in economic terms. The same pattern arises in the analysis conditional on enrollment (see Section 6.5.2), and can partly be due to selection with people opting for paths where the pace and thus the probability of completing on time for the bonus are higher (although it is uncertain to which degree students can choose track; see below). The pattern signals that institutional features matter for the impact; we find effects where historic performance suggests that the bonus is more feasible. We will return to this issue.

Table 4. Effects on student achievement

Passed the following course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.032**	(0.010)	0.005	(0.012)	0.020***	(0.008)
Bonus courses (1B, 2C, 3D)	0.034***	(0.006)	0.007	(0.009)	0.022***	(0.005)
Other courses (1A, 2B, 3C)	0.031***	(0.009)	0.005	(0.012)	0.019*	(0.008)
1A	-0.001	(0.003)	0.000	(0.004)	0.000	(0.003)
1B	-0.004*	(0.002)	-0.007*	(0.003)	-0.005**	(0.002)
2B	0.023***	(0.004)	0.001	(0.008)	0.013**	(0.004)
2C	0.015***	(0.003)	-0.006	(0.005)	0.005	(0.003)
3C	0.011	(0.006)	0.006	(0.008)	0.008**	(0.005)
3D	0.028***	(0.005)	0.016**	(0.006)	0.022***	(0.004)
N	62589		41918		104507	
	Mean pass rates					
Any course	0.18		0.23		0.20	
Bonus courses	0.08		0.10		0.09	
Other courses	0.16		0.21		0.18	
1A	0.01		0.03		0.02	
1B	0.00		0.01		0.01	
2B	0.06		0.09		0.07	
2C	0.02		0.03		0.03	
3C	0.09		0.10		0.10	
3D	0.06		0.07		0.06	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses (see Table A3 for clustered standard errors on the municipality level). The outcomes are defined as having passed a course within 15 months after immigration and 12 months after the course start. The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30 aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Finland and Denmark are excluded as they normally are not eligible for language training programs. Treatment is defined as the interaction between residing in a bonus-municipality and the immigration period (pre- or post-2009-07-01, i.e. before or after the reform). Controls include the immigration month fixed effects, age, age squared, gender, civil status, children under 18 in household and country of birth. Each cell represents separate regressions.
* <0.05 ** <0.01 *** <0.001.

It is quite likely that the effects of a bonus may vary depending on individual characteristics. Table 5 shows estimates for subgroups defined by region of origin, age, gender, income and receipt of social benefits. Splitting the sample by broad region of origin (EEA vs. Non-EEA) provides a crude indicator of eligibility as well as expected socioeconomic position; most in the former are not in a residence permit category covered by the bonus, most in the latter are.[22] As can be seen in the table below, the estimated effects for the two origin groups are similar in metropolitan areas. For the other municipalities there is a tendency to an effect for the EEA immigrants, but the estimate is not statistically significant. The placebo estimate for the EEA migrants (Table A6), however, suggests that we should be somewhat cautious in interpreting these differences.

The mean of the dependent variable is twice as high among the Non-EEA migrants, making effects smaller in the relative sense. This may be surprising considering that a greater fraction in this group were eligible. On the other hand, it can be argued that EEA immigrants have on average lower costs for learning the language (for example, a native language closer to Swedish, see also the discussion in section 2.1). Furthermore, the most common type of residence permits for those eligible in this group is based on family ties and it is likely that this group has a particular advantage as they immigrate to someone already living in the country. Worth noting is also that the share of this group in the total sample is relatively small (25 percent of the full sample) and even smaller when considering only those that have enrolled in Sfi (about 15 percent of all enrolled students).[23] A back-of-the-envelope calculation suggests that if the EEA effect is only driven by individuals eligible for the bonus[24], it is required that one in ten respond to the incentives provided by the reform. This is indeed a large effect that potentially reflects the particular advantages of this group (see above).

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[22] Due to the fact that some countries are grouped together in the data our classification of EEA does not follow the actual definition. The following countries are included: Poland, Ireland, the UK, Germany, Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain, Estonia, Latvia, Lithuania, Slovak Republic, Czech Republic, Hungary, Andorra, Belgium, France, Liechtenstein, Luxemburg, the Netherlands, Switzerland, Austria.

[23] Dividing the sample into finer regions of origin (or even separate source countries) largely confirms the baseline picture of a positive impact in Stockholm but not in the other areas. It should however be noted that statistical precision becomes a concern and that there are examples of substantial negative but insignificant point estimates.

[24] Around one third of the EEA migrants fulfilled the requirements to participate in the experiment. The estimate is based on information on the types of residence permits that were granted by the Swedish Migration Board during the time period.

Table 5. Student achievement – Heterogeneous effects

Passed any course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
EEA	0.034***	(0.010)	0.024	(0.015)	0.031***	(0.009)
Non-EEA	0.029*	(0.011)	-0.000	(0.015)	0.016	(0.010)
Women	0.033**	(0.011)	-0.001	(0.017)	0.019	(0.010)
Men	0.031**	(0.011)	0.011	(0.014)	0.021*	(0.009)
18-29 years old	0.045***	(0.010)	0.002	(0.015)	0.026**	(0.010)
30-64 years old	0.017	(0.012)	0.004	(0.014)	0.012	(0.009)
Controlling for income	0.032**	(0.010)	0.004	(0.012)	0.020*	(0.008)
Household on SA	0.039	(0.022)	0.025	(0.027)	0.035	(0.018)
Household not on SA	0.029***	(0.008)	0.002	(0.011)	0.017*	(0.007)

	Mean pass rates		
EEA	0.10	0.13	0.11
Non-EEA	0.21	0.27	0.23
Women	0.22	0.28	0.25
Men	0.14	0.19	0.16
18-29 years old	0.19	0.24	0.21
30-64 years old	0.17	0.22	0.20
Controlling for income	0.18	0.23	0.20
Household on SA	0.31	0.37	0.34
Household not on SA	0.15	0.19	0.16

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table 5 further shows that the impact is larger among younger migrants in metropolitan areas. The young are only slightly more likely than the old(er) to complete courses, but appear to be substantially more responsive to the bonus.[25] For men and women, the results are very similar (the estimated relative effect is however greater for men). As we pointed out there are notable differences in labor market outcomes measured the year of arrival between the treatment and control group in metropolitan areas. Splitting the sample depending on whether a household is a recipient of social assistance (SA) shows that the point estimate is larger for individuals that receive social benefits. In relative terms the size of the effect of the bonus is similar in the two groups when evaluated at their respective pass rates. We also tried including a control for income from labor; this did not have any effect on the results.[26]

6.4. Variations and Robustness Checks

6.4.1. Placebo Regressions and Linear Trends

Table A4 in the appendix presents placebo regressions for the baseline specification, "moving" the treatment one year back in time (as discussed above). The estimates do in general not question the plausibility of the baseline model. Only a few estimates are statistically significant, and overall the magnitude is clearly smaller than in the actual analysis. If anything, the negative placebo estimates outside the metropolitan areas could be indicating that we would underestimate the impact of the bonus.[27] For example, assuming a DDD setting, one could argue that zero estimates for the reform in non-metropolitan areas combined with negative placebo estimates, actually suggest a positive impact of the bonus reform also in these areas.[28] As for linear trends, it is reassuring to see in Table A5 that the results are not affected by the inclusion of them.

6.4.2. Time Frame for Completion

It is relevant to ask whether students only moved the completion date to just before the time limit for eligibility or whether effects are present also for more long-run outcomes. Changing the outcome to a pass or pass with

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[25] In our baseline sample we include immigrants aged 18-64 years old, i.e. the age group that was affected by the implementation of the bonus scheme. Some municipalities however require that participants in Sfi should be at least 20 years old. Otherwise, they are offered language training within high school education programs. To this end we have re-run our analysis setting the age requirement to at least 20 years at time of immigration. This exercise has little effect on our results.

[26] Placebo estimates are found in Table A6 in the appendix.

[27] Although insignificant, some of the point estimates in the disaggregated placebo analysis of Table A6 urge caution.

[28] An alternative view is of course that negative placebo estimates indicate poor handling of factors relevant to the outcomes.

distinction within 18 months after arrival gives support for the latter notion (see Table A7), i.e. that it was not just a matter of changing the completion date to fulfill the bonus requirement (18 months is the maximum follow-up for the latest cohorts studied).

6.4.3 Time-Varying Effects

We have also investigated the possibility that the impact varies over time. One hypothesis is that the effect would increase with time since implementation as the bonus becomes more known. An opposite idea would say that the immediate impact is bigger than the long-run, as the existence of the bonus becomes an established part of the system, not receiving that much attention. Table A8 shows estimates for bonus courses, where we allow the estimate to vary by month of immigration. The results show that the impact in the metropolitan areas is present in nine out of the twelve months that the experiment lasted, but that the effects tend to be weaker the last months, thus lending support to the latter explanation. On the other hand, for the other treatment areas, where the baseline estimates are zero, the estimates become somewhat more positive over time although only one of the estimates can statistically be distinguished from zero.

One way to view these estimates is to say that since there is no impact in all of the months, we cannot believe in the baseline estimate.[29] On the other hand, if there was no treatment effect and the significant estimates were just outcomes of major but random swings over time, we would expect to see significant differences going in the other direction as well. But we do not.

If one instead is willing to accept the existence of an impact, the question of how to explain the observed time pattern arises. This necessarily becomes speculative. It could be that what we see in the metropolitan areas is an effect of some people responding to the news of the bonus and possibly also to feeling "chosen" relative to their peers being in the same classes but non-eligible due to their earlier arrival. To reconcile the pattern in the other locations one could think of a situation where information and institutions respond slower, perhaps in combination with peer effects spurring performance in some narrowly defined cohorts but not in others.

Our reading of the time-varying results is that they underscore the fact that even though there appears to be some effect of the bonus on study achievement, there is a lot we do not understand regarding the mechanisms.

6.4.4 Strategic Behavior – Relocation and Course Choice

The setup of the bonus aimed at granting eligibility to people immigrating to some municipalities but not to others. Only the participating municipalities were instructed to inform about the bonus, and the information stated the

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[29] As shown in Åslund and Engdahl (2012), this ambiguity is even greater if one include also non-bonus courses.

requirements similar to section 4.1. Yet, the fine prints of the regulations opened up for gaming the system in the sense that those who moved to a course in a participating municipality were not to be excluded from the bonus. We have therefore checked whether changing to a participating municipality became more common with the reform, which it did not. Neither do the data suggest that people chose to immigrate to bonus municipalities to a greater extent (results available on request).

Another possibility is that the bonus affected course choice. It is hard to tell to what extent students can affect this decision. Many municipalities use tests or conversations to classify the students, but in other cases the procedure is unclear and one cannot rule out the possibility that some individuals acted strategically. To the extent that our outcome variables capture the aims of the bonus in a reasonable way (and thus we ignore the possibility that completing some courses may not be as valuable as completing others), one could argue that the baseline approach is robust to such compositional effects since it includes the entire population of migrants and studies impacts at all types of courses.

6.4.5 Additional Specification Checks

Even though we control for background characteristics, one could worry that if some groups are more affected by general time effects, imbalances across treatment and control may create a false impression of a treatment effect. This should arguably to some extent show up in the placebo analysis, but we have nevertheless tried re-weighting the sample according to country of origin. The estimates confirmed the baseline results (which are in line with the above-mentioned analysis on finer subgroups based on region of origin) (see Table A9).

Another test performed to confirm our results is to re-run our analysis by excluding covariates altogether. This is a common variation, based on the idea that if the results are unaffected this would to some extent confirm how successful the randomization was. The outcome of this exercise indicates that there are limited imbalances in covariates as the point estimates become slightly smaller, but that the qualitative results are not affected (results available on request). One should, however, note that this type of sensitivity check is less needed as well as less informative, given the randomization that by definition excludes self-selection on unobservables. The fact that pre-reform outcomes and covariates are not perfectly balanced through the randomized allocation does in our setting not raise a concern that, for example, locations which are more ambitious in raising performance are also more likely to exert effort which makes them more likely to participate.

6.5. Enrollment and Achievement Conditional on Enrollment

Enrollment is interesting as an outcome in itself, but investigating enrollment can also aid the interpretation of the results on student performance. This section first presents estimates on enrollment, and then turns to an analysis of achievement conditional on enrollment.

6.5.1. Enrollment

Table 6 shows the baseline estimates for course starts. We examine respectively the probability of starting a course within 3, 6, and 12 months after immigration. The estimates are in general positive and significant for metropolitan areas, but insignificant and closer to zero for the other municipalities. Taken at face value these estimates indicate a non-trivial positive impact on enrollment in the metropolitan areas.

However, this interpretation is questioned by several placebo regressions as well as the inclusion of trends in the specifications. For metropolitan areas there are indications of an on average more positive development over time than in the comparison areas, while the opposite holds true for other municipalities (see Table A11 in the appendix), which casts doubt on the baseline enrollment estimates. Furthermore, including a group-specific linear time trend, removes also the significance and the size of the estimates for Stockholm (see Table A12).

All in all, these findings suggest that we need to be very cautious in interpreting the enrollment estimates as causal effects of the reform. In fact, there is only one enrollment estimate that comes out of the sensitivity checks more or less unscattered: the positive impact on starting a bonus course within 12 months. It makes sense that an impact at this level is delayed, given that most people do not take bonus courses as their first course. It also seems reasonable that the possibility of earning a bonus may have affected some students to progress in the system. In sum, it is hard to argue strongly that the implementation of the bonus increased or speeded up overall enrollment. At the same time the overall estimates contain too much uncertainty to fully rule out an impact on enrollment.

Table 6. Effects on course starts

Started within:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
			Any course			
3 months	0.035**	(0.013)	-0.009	(0.014)	0.020	(0.011)
6 months	0.026	(0.018)	0.012	(0.015)	0.021	(0.013)
12 months	0.032*	(0.013)	-0.011	(0.015)	0.015	(0.011)
			Bonus course			
3 months	0.001	(0.003)	-0.004	(0.005)	-0.000	(0.003)
6 months	0.006	(0.006)	-0.006	(0.008)	0.002	(0.005)
12 months	0.020*	(0.009)	0.002	(0.012)	0.012	(0.008)
			Other course			
3 months	0.039**	(0.014)	0.002	(0.013)	0.026*	(0.011)
6 months	0.035*	(0.017)	0.017	(0.014)	0.028*	(0.012)
12 months	0.041***	(0.012)	-0.009	(0.015)	0.021	(0.011)
N	62589		41918		104507	
			Enrollment rate any course			
3 months	0.26		0.26		0.26	
6 months	0.40		0.43		0.41	
12 months	0.50		0.52		0.51	
			Enrollment rate bonus courses			
3 months	0.02		0.04		0.03	
6 months	0.05		0.08		0.07	
12 months	0.16		0.21		0.18	
			Enrollment rate other courses			
3 months	0.27		0.27		0.27	
6 months	0.38		0.39		0.39	
12 months	0.46		0.49		0.47	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses (see Table A10 for clustered standard errors on the municipality level). The outcomes are defined as either having started a course within a quarter of a year (91 days) /half a year (182 days) /a year (365 days) after arrival to Sweden. See also note Table 4. * <0.05 ** <0.01 *** <0.001.

6.5.2. Achievement Conditional on Enrollment

There are good reasons for focusing on the overall population of recent immigrants, and not just on those enrolling in Sfi. First, the policy aimed to increase language learning in general, which partly could be achieved through increased enrollment. Second, conditioning on enrollment may create sample selection problems if the reform affects who enrolls. However, a conditional analysis has the merit of excluding those who under no circumstances are interested in Sfi.

Table A13 shows that the basic conclusions from the performance analysis above are not altered by restricting the sample to those who actually participated. Point estimates are somewhat larger in Stockholm, and particularly so for bonus courses, but there are no aggregate effects in the other regions, as in the unconditional analysis. Moving down the table it can be noted that, just as in the unconditional analysis, there is a negative effect on results on course 1B in metropolitan areas (see Section 6.3). Regarding the other bonus courses the effects are positive. Further, for other municipalities, taking the placebo results into account, there are indications of a positive impact on some of the courses.[30]

Performing the conditional analysis by subgroups reveals some patterns worth noting (Table A14). The estimated positive impact is more marked among EEA immigrants who actually enrolled, compared to the effect among non-EEA students. This pattern signals the potential importance of how feasible the bonus threshold appears considering the individual's characteristics.

A (political) objection to the bonus scheme has been that it favors the highly educated and those who for other reasons may find it easier to accomplish the goals of the bonus program. If not generally, so at least in terms of the higher amounts given for more advanced courses. Table A14, however, does not suggest that the impact was greater among the highest educated.[31] The point estimates suggest similar effects for people with 0–6 and 7–12 years of schooling, but no significant impact on those with 13 or more years of education. Even though this does not necessarily say anything about fairness, it indicates that it was not the case that only the most educated were affected by the policy. It should however be noted that some of the placebo estimates suggest that one should be very cautious in drawing firm conclusions in this part of the analysis. It is also quite possible that the selection into participation varies by level of education.

Throughout the analysis, the results for metropolitan areas differ substantially from other participating municipalities. One could speculate

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[30] The instability of the estimates however urges caution.
[31] Data on individual education for all cohorts under study are only available through the Sfi register and can thus not be used in the unconditional analysis.

that Stockholm's comparatively quick course starts make bonuses more feasible. One way of investigating this possibility is to include controls for time until course start in a conditional analysis. Doing so strongly confirms the idea that an early start is important for completing in time for a (possible) bonus; the pass rates fall sharply month-by-month. Thus, the regulations can be seen as quite strict. The point estimate on treatment for Stockholm is however more or less unchanged for all courses (Table A15). Thus, it is not the case that the effects can be explained by people in Stockholm enrolling quicker after the reform compared to before. To the extent that the shorter enrollment times in Stockholm do play a role, it is more likely to be as a time-constant institutional feature promoting the impact of the bonus.

7. Conclusions

This paper evaluates the effects of a pay-for-performance scheme within the Swedish language tuition system for adult immigrants. The use of economic incentives to improve student achievement has become increasingly popular among policy makers, practitioners and researchers around the world but this is the first time financial awards are used in the type of setting studied here. A policy pilot run in 2009–2010 gave a randomly assigned group of municipalities the right to grant substantial cash bonuses to recently arrived immigrants meeting certain performance criteria. In short, to qualify for the bonus a student should have passed a bonus qualifying language course within 15 months after arriving to Sweden but no longer than a year after the course start.

The average estimated impact on student achievement is substantial, but driven alone by the metropolitan areas included in the pilot. Effects are greater for bonus-awarding courses, and in particular in more advanced tracks. For the metropolitan areas the effect of the bonus scheme appears to have had a comparable effect across regions of origin. In relative terms the effect is however larger for younger students and for men. The relative estimated impact of the bonus scheme is also greater for immigrants from the EEA/EU in comparison with immigrants born elsewhere. Similar effects are present for groups of different socioeconomic status. A large number of specification tests and robustness checks, which by and large support the baseline conclusion, are also discussed.

Although the results may seem quite disparate, there is actually a common thread of finding effects where they are more likely to occur. First, effects are greater for bonus-awarding courses. Second, we find a clear impact on tracks where the (historical) average duration is shorter, and none where the odds of completing in time for a bonus are poor. Similarly, Stockholm (where effects are found) has a record of people starting more quickly and a quick start is strongly associated with the probability of

completing in time. It is of course also possible that there are institutional advantages in larger cities, making it possible to provide teaching and classes in a manner suitable for the bonus requirements. Third, some findings suggest greater effects among migrants with arguably more favorable characteristics in terms of acquiring the Swedish language: the young, EEA migrants, and those judged suited for the advanced tracks.

One should however acknowledge the fact that the risk of there being something else than the bonus scheme driving the result is higher when effects are concentrated to one area, than if we had seen a similar pattern in all the regions. A similar concern is that the impact only seems to have been present in some periods. Taken together there are clearly still much to be understood regarding the mechanisms at work. On the other hand, such puzzles are perhaps not that surprising given the very mixed empirical evidence in previous studies. Since this is also the first study of performance bonuses for adult education and also for the immigrant population, it is hard to tell what to expect. Our estimates point to potentially very large effects in relative terms. But this is partly because outcomes are not very good to begin with. It does not seem entirely unlikely that the potential of gaining about Euro 1,350 would make three or four out of a hundred students complete their studies faster.

Taking the baseline estimates at face value, a back-of-the-envelope calculation arrives at a cost of about SEK 14,500 (Euro 1,600) per added course completion in Stockholm.[32] We do not have a good estimate of the exact value of completing Sfi, neither in terms of actual language skills nor on its labor market value. However, if one attaches any value to it in terms of shortening welfare dependence and promoting labor market integration (which, e.g. the results in Kennerberg and Åslund 2010 tend to do), the order of magnitude of the cost is relatively modest. This suggests that one would at least like to further understand the mechanisms at work before dismissing the bonus as an irrelevant policy tool.

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[32] The number of people entering Stockholm in the pilot period times the estimated increase in the probability: (33690-24910)*.032 = 280.96. The total paid bonuses plus administrative fees is 4,082,800, which divided by 280.96 yields a cost of 14,532 per added completion.

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Appendix A. Additional Results

Table A1. Regression sample (page 1/2)

	Metropolitan areas		Other municipalities		All municipalities	
	Treated	Control	Treated	Control	Treated	Control
Age	31.28	30.85	31.53	30.86	31.37	30.86
Gender (0=women)	0.54	0.54	0.53	0.51	0.53	0.53
Married or partner	0.46	0.47	0.52	0.51	0.48	0.49
Children under 18 in household	0.21	0.22	0.27	0.25	0.23	0.23
Social assistance	0.13	0.25	0.25	0.26	0.17	0.25
Annual earnings (100's of SEK)	434.05	261.20	274.28	295.51	376.24	276.34
Bosnia and Herzegovina	0.00	0.01	0.01	0.01	0.01	0.01
Former Yugoslavia	0.02	0.03	0.05	0.05	0.03	0.04
Poland	0.09	0.07	0.10	0.09	0.10	0.08
Ireland and UK	0.03	0.02	0.02	0.02	0.03	0.02
Germany	0.00	0.00	0.00	0.00	0.00	0.00
Mediterranean countries	0.05	0.03	0.02	0.03	0.04	0.03
The Baltics	0.03	0.02	0.03	0.03	0.03	0.02
Eastern Europe and former Soviet	0.09	0.08	0.09	0.07	0.09	0.08
Central Europe	0.01	0.02	0.02	0.02	0.01	0.02
France and Benelux	0.04	0.03	0.02	0.02	0.03	0.03
US and Canada	0.03	0.02	0.01	0.02	0.02	0.02
Central America	0.01	0.01	0.01	0.01	0.01	0.01
Chile	0.01	0.01	0.01	0.00	0.01	0.01
South America	0.04	0.02	0.03	0.03	0.04	0.02
Horn of Africa and Sudan	0.09	0.06	0.07	0.07	0.08	0.07

Table A1. Cont'd (page 2/2)

	33690	28899	19105	22813	52795	51712
North Africa and Middle East	0.06	0.06	0.06	0.06	0.06	0.06
Sub-Saharan Africa and Egypt	0.03	0.04	0.03	0.03	0.03	0.04
Iran	0.03	0.05	0.04	0.03	0.04	0.04
Iraq	0.07	0.16	0.14	0.14	0.10	0.15
Turkey	0.03	0.03	0.03	0.03	0.03	0.03
East Asia	0.07	0.07	0.04	0.07	0.06	0.07
South East Asia	0.04	0.04	0.06	0.06	0.05	0.05
South Asia	0.08	0.06	0.08	0.06	0.08	0.06
Australia and the Pacific	0.01	0.01	0.00	0.01	0.01	0.01
Not classified	0.00	0.00	0.00	0.00	0.00	0.00
Course start within 3 months	0.30	0.21	0.28	0.25	0.29	0.23
Course start within 6 months	0.42	0.39	0.43	0.42	0.42	0.40
Course start within 12 months	0.49	0.50	0.53	0.52	0.50	0.51
Passed any course	0.16	0.20	0.24	0.23	0.19	0.21
Passed a bonus course	0.07	0.09	0.10	0.11	0.08	0.10
Passed other course	0.15	0.18	0.21	0.20	0.17	0.19
N	33690	28899	19105	22813	52795	51712

Notes: Sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30, i.e. the full regression sample, aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Iceland, Finland and Denmark are excluded. All demographic and labor market characteristics are measured the year of arrival. The outcome variables course start within 3/6/12 months refers to time before the first course start within Sfi. The outcome variables passed any course/a bonus course/other course are set to unity if an individual have completed a course within 15 months after immigration but no longer than 12 months after immigration, i.e. the bonus requirement was fulfilled.

Table A2. Course starts within 6 months after immigration, t-test

	Pilot period				Before Pilot			
	Mean pass rate treated group	Mean pass rate control group	Diff. (Treated-Control)	P-value	Mean pass rate treated group	Mean pass rate control group	Diff. (Treated-Control)	P-value
All treated-all untreated	0.421	0.386	0.035	0.000	0.422	0.410	0.012	0.001
Stockholm-Göteborg	0.427	0.383	0.043	0.000	0.411	0.392	0.020	0.000
Huddinge-Haninge	0.393	0.418	-0.025	0.291	0.443	0.415	0.029	0.045
Borås-Jönköping	0.413	0.472	-0.059	0.039	0.394	0.440	-0.046	0.004
Växjö-Kalmar	0.297	0.235	0.062	0.085	0.378	0.404	-0.026	0.228
Sandviken-Gävle	0.444	0.406	0.039	0.365	0.597	0.568	0.029	0.252
Nacka-Täby	0.245	0.430	-0.185	0.000	0.282	0.345	-0.063	0.002
Sollentuna-Solna	0.475	0.293	0.182	0.000	0.414	0.282	0.132	0.000
Halmstad-Helsingborg	0.522	0.339	0.184	0.000	0.525	0.483	0.042	0.015
Karlstad-Västerås	0.406	0.504	-0.098	0.002	0.473	0.505	-0.032	0.111
Trelleborg-Landskrona	0.270	0.216	0.054	0.151	0.321	0.333	-0.012	0.628
Örnsköldsvik-Härnösand	0.600	0.655	-0.055	0.416	0.687	0.593	0.094	0.010
Uddevalla-Trollhättan	0.553	0.405	0.148	0.002	0.595	0.429	0.165	0.000
Katrineholm-Nyköping	0.794	0.566	0.228	0.000	0.692	0.560	0.132	0.000

Notes: The outcome is defined as having started a course within 6 months after immigration. The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30 aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality excluding immigrants from Norway, Iceland, Finland and Denmark.

Table A3. Effects on student achievement – clustered standard errors

Passed the following course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.032	(0.021)	0.005	(0.025)	0.020	(0.016)
Bonus courses (1B, 2C, 3D)	0.034	(0.011)	0.007	(0.019)	0.022	(0.011)
Other courses (1A, 2B, 3C)	0.031	(0.018)	0.005	(0.026)	0.019	(0.015)

Notes: Robust standard errors clustered on municipalities in parentheses. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A4. Placebo regressions – student achievement

Passed the following course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.007	(0.008)	-0.012	(0.013)	-0.002	(0.008)
Bonus courses (1B, 2C, 3D)	-0.003	(0.005)	-0.027**	(0.009)	-0.013*	(0.005)
Other courses (1A, 2B, 3C)	0.003	(0.008)	-0.005	(0.013)	-0.001	(0.008)
N	31284		21294		52578	
	Mean pass rates					
Any course	0.17		0.23		0.20	
Bonus courses	0.08		0.10		0.08	
Other courses	0.16		0.20		0.18	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Includes migrants that arrived between 2007-07-01 and 2009-06-30. Treatment defined to take place 1 of July 2008. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

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Table A5. Effects on student achievement – adding linear time trends

Passed the following course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.034*	(0.013)	0.003	(0.018)	0.020	(0.012)
Bonus courses (1B, 2C, 3D)	0.024**	(0.009)	0.025*	(0.012)	0.024**	(0.008)
Other courses (1A, 2B, 3C)	0.042***	(0.012)	-0.002	(0.018)	0.023*	(0.011)
N	62589		41918		104507	
	Mean pass rates					
Any course	0.18		0.23		0.20	
Bonus courses	0.08		0.10		0.09	
Other courses	0.16		0.21		0.18	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A6. Student achievement – Heterogeneous effects – Placebo regressions

	Metropolitan areas		Other municipalities		All municipalities	
Passed any course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
EEA	0.020	(0.011)	-0.019	(0.019)	0.004	(0.010)
Non-EEA	0.001	(0.010)	-0.009	(0.016)	-0.005	(0.010)
Women	-0.008	(0.011)	-0.024	(0.020)	-0.016	(0.011)
Men	0.017	(0.009)	0.003	(0.015)	0.010	(0.009)
18-29 years old	0.008	(0.010)	-0.031	(0.017)	-0.009	(0.010)
30-64 years old	0.007	(0.012)	0.008	(0.017)	0.007	(0.010)
Controlling for income	0.006	(0.008)	-0.012	(0.013)	-0.002	(0.008)
Household on SA	-0.001	(0.024)	-0.058[*]	(0.027)	-0.026	(0.020)
Household not on SA	0.005	(0.007)	0.007	(0.013)	0.004	(0.007)
	Mean pass rates					
EEA	0.10		0.13		0.11	
Non-EEA	0.20		0.26		0.23	
Women	0.22		0.28		0.24	
Men	0.13		0.18		0.15	
18-29 years old	0.18		0.23		0.20	
30-64 years old	0.17		0.23		0.19	
Controlling for income	0.17		0.23		0.20	
Household on SA	0.30		0.36		0.33	
Household not on SA	0.14		0.18		0.16	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed any course within 15 months after immigration and 12 months after the course start. Treatment is defined to take place on 1st of July 2008. Immigrants immigrating between 2007-07-01 – 2009-06-30 included. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A7. Effects on student achievement – pass within 18 months after immigration

Passed the following course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.032**	(0.010)	0.001	(0.012)	0.018*	(0.008)
Bonus courses (1B, 2C, 3D)	0.031***	(0.006)	0.007	(0.009)	0.020***	(0.006)
Other courses (1A, 2B, 3C)	0.034***	(0.009)	0.001	(0.012)	0.020**	(0.008)
N	62589		41918		104507	
	Mean pass rates					
Any course	0.21		0.27		0.24	
Bonus courses	0.10		0.13		0.12	
Other courses	0.19		0.24		0.21	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 18 months after immigration. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A8. Time-Varying effects on student achievement

Passed:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Bonus courses (1B, 2C, 3D)						
July 2009	0.030*	(0.012)	0.001	(0.024)	0.016	(0.013)
August 2009	0.063***	(0.004)	-0.014	(0.015)	0.033**	(0.012)
September 2009	0.065***	(0.009)	0.004	(0.017)	0.041***	(0.012)
October 2009	0.025*	(0.011)	-0.010	(0.027)	0.009	(0.012)
November 2009	0.047**	(0.017)	0.013	(0.022)	0.031*	(0.015)
December 2009	0.024*	(0.011)	0.019	(0.041)	0.021	(0.019)
January 2010	0.051***	(0.006)	-0.033	(0.033)	0.013	(0.019)
February 2010	0.023*	(0.010)	-0.008	(0.035)	0.010	(0.016)
March 2010	-0.015	(0.010)	0.074**	(0.026)	0.020	(0.020)
April 2010	0.008	(0.008)	0.024	(0.027)	0.014	(0.012)
May 2010	0.004	(0.009)	0.037	(0.025)	0.013	(0.021)
June 2010	0.023**	(0.008)	0.004	(0.029)	0.013	(0.014)
	62589		41918		104507	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001

Table A9. Effects on student achievement – weights defined by country/region of birth

Passed the following course:	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.028***	(0.009)	0.006	(0.012)	0.017*	(0.008)
Bonus courses (1B, 2C, 3D)	0.033***	(0.006)	0.008	(0.009)	0.020***	(0.005)
Other courses (1A, 2B, 3C)	0.027**	(0.008)	0.006	(0.012)	0.017*	(0.007)
N	62589		41918		104507	
	Mean pass rates					
Any course	0.18		0.23		0.20	
Bonus courses	0.08		0.10		0.09	
Other courses	0.16		0.21		0.18	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A10. Effects on course starts – clustered standard errors

	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Started within:	Any course					
3 months	0.035	(0.025)	-0.009	(0.031)	0.020	(0.024)
6 months	0.026	(0.045)	0.012	(0.026)	0.021	(0.026)
12 months	0.032	(0.022)	-0.011	(0.025)	0.015	(0.020)
	Bonus course					
3 months	0.001	(0.008)	-0.004	(0.011)	-0.000	(0.007)
6 months	0.006	(0.018)	-0.006	(0.017)	0.002	(0.012)
12 months	0.020	(0.024)	0.002	(0.022)	0.012	(0.016)
	All municipalities					
3 months	0.039	(0.018)	0.002	(0.029)	0.026	(0.021)
6 months	0.035	(0.030)	0.017	(0.027)	0.028	(0.021)
12 months	0.041	(0.012)	-0.009	(0.028)	0.021	(0.019)
N	62589		41918		104507	

Notes: Robust standard errors clustered on municipalities in parentheses. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A11. Placebo regressions – course starts

	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Started within:	Any course					
3 months	0.028	(0.017)	-0.019	(0.017)	0.013	(0.013)
6 months	0.008	(0.020)	-0.032	(0.019)	-0.006	(0.015)
12 months	0.046**	(0.016)	-0.011	(0.018)	0.023	(0.013)
	Bonus course					
3 months	-0.001	(0.003)	0.012	(0.007)	0.005	(0.004)
6 months	-0.004	(0.006)	0.006	(0.011)	0.000	(0.006)
12 months	-0.008	(0.008)	0.004	(0.014)	-0.004	(0.008)
	Other course					
3 months	0.035*	(0.016)	-0.018	(0.016)	0.016	(0.012)
6 months	0.017	(0.018)	-0.038*	(0.017)	-0.003	(0.014)
12 months	0.048**	(0.016)	-0.011	(0.017)	0.024	(0.013)
N	31284		21294		52578	
	Enrollment rate any course					
3 months	0.26		0.27		0.26	
6 months	0.39		0.42		0.41	
12 months	0.48		0.52		0.50	
	Enrollment rate bonus courses					
3 months	0.02		0.04		0.02	
6 months	0.05		0.08		0.06	
12 months	0.16		0.21		0.18	
	Enrollment rate other courses					
3 months	0.26		0.28		0.27	
6 months	0.37		0.39		0.38	
12 months	0.46		0.48		0.47	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Sample includes all immigrants to Sweden that arrived to Sweden between 2007-07-01 and 2009-06-30. Treatment is defined to take place on 2008-07-01. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A12. Effects on course starts – adding linear time trends

	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Started within:	Any course					
3 months	-0.014	(0.022)	0.005	(0.022)	-0.008	(0.018)
6 months	0.001	(0.024)	0.014	(0.024)	0.004	(0.019)
12 months	0.002	(0.020)	-0.023	(0.023)	-0.009	(0.017)
	Bonus course					
3 months	-0.003	(0.004)	0.021**	(0.008)	0.006	(0.005)
6 months	0.001	(0.008)	0.029*	(0.013)	0.010	(0.008)
12 months	0.026*	(0.012)	0.026	(0.018)	0.024**	(0.011)
	Other course					
3 months	-0.019	(0.024)	-0.006	(0.020)	-0.015	(0.018)
6 months	0.004	(0.023)	-0.001	(0.023)	-0.000	(0.018)
12 months	0.008	(0.020)	-0.039	(0.023)	-0.012	(0.017)
N	62589		41918		104507	

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. See also note Table 4.
 * <0.05 ** <0.01 *** <0.001.

Table A13. Effect on student achievement conditional on enrollment within 6 months and placebo (page 1/2)

Passed the following course:	Baseline estimates			Placebo estimates		
	Metropolitan areas ATE (S.E.)	Other municipalities ATE (S.E.)	All municipalities ATE (S.E.)	Metropolitan areas ATE (S.E.)	Other municipalities ATE (S.E.)	All municipalities ATE (S.E.)
Any course (1A-3D)	0.045** (0.016)	-0.005 (0.020)	0.020 (0.014)	0.002 (0.017)	0.015 (0.023)	0.002 (0.016)
Bonus courses (1B, 2C, 3D)	0.096*** (0.006)	0.028 (0.022)	0.059*** (0.006)	-0.003 (0.019)	-0.052* (0.024)	-0.026 (0.016)
Other courses (1A, 2B, 3C)	0.036* (0.018)	-0.006 (0.022)	0.014 (0.014)	-0.005 (0.019)	0.028 (0.024)	0.004 (0.017)
1A	-0.004 (0.006)	-0.003 (0.009)	-0.002 (0.006)	0.008 (0.006)	0.020* (0.010)	0.014* (0.006)
1B	-0.007* (0.003)	-0.019* (0.007)	-0.012** (0.004)	-0.003 (0.003)	-0.012* (0.005)	-0.006 (0.003)
2B	0.047** (0.009)	-0.007 (0.016)	0.021* (0.009)	0.003 (0.011)	-0.012 (0.018)	-0.008 (0.011)
2C	0.034*** (0.006)	-0.023 (0.011)	0.008 (0.007)	0.004 (0.008)	-0.043*** (0.012)	-0.018* (0.007)
3C	-0.000 (0.009)	0.023 (0.016)	0.007 (0.009)	-0.011 (0.011)	0.032 (0.019)	0.005 (0.011)
3D	0.055*** (0.009)	0.030 (0.013)	0.042*** (0.008)	-0.020 (0.038)	-0.076 (0.075)	-0.003 (0.010)

Table A13. Cont'd (page 2/2)

	Mean pass rate				Mean pass rate		
Any course	0.40	0.49	0.44		0.39	0.48	0.43
Bonus courses	0.18	0.22	0.20		0.18	0.21	0.19
Other courses	0.36	0.44	0.40		0.36	0.42	0.39
1A	0.03	0.05	0.04		0.02	0.05	0.03
1B	0.01	0.02	0.01		0.01	0.01	0.01
2B	0.14	0.19	0.16		0.13	0.18	0.15
2C	0.05	0.07	0.06		0.05	0.07	0.05
3C	0.21	0.22	0.21		0.21	0.22	0.21
3D	0.14	0.16	0.14		0.14	0.15	0.14

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. In the placebo regressions migrants immigrating between 2007-07-01 – 2009-06-30 are included and the reform is defined to take place on the 2008-07-01. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A14. Heterogeneous results – conditional on enrollment within 6 months after immigration and placebo estimates (page 1/2)

	Metropolitan areas		Other municipalities		All municipalities	
Passed any course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
EEA	0.056	(0.031)	0.031	(0.049)	0.040	(0.026)
Non-EEA	0.039[*]	(0.017)	-0.006	(0.022)	0.015	(0.015)
Women	0.035	(0.018)	-0.010	(0.025)	0.013	(0.016)
Men	0.057[**]	(0.021)	0.011	(0.028)	0.031	(0.018)
18-29 years old	0.058[**]	(0.018)	-0.007	(0.026)	0.026	(0.016)
30-64 years old	0.032	(0.020)	0.002	(0.026)	0.015	(0.017)
Controlling for income	0.046[**]	(0.016)	-0.006	(0.020)	0.020	(0.014)
Household on SA	0.068[**]	(0.024)	0.008	(0.031)	0.044[*]	(0.021)
Household not on SA	0.033	(0.018)	-0.008	(0.023)	0.013	(0.014)
0-6 years of education	0.058	(0.030)	0.016	(0.040)	0.033	(0.027)
7-12 years of education	0.065[***]	(0.018)	0.025	(0.028)	0.044[*]	(0.017)
13+ years of education	0.002	(0.021)	-0.015	(0.030)	-0.007	(0.018)
			Placebo estimates			
EEA	0.064	(0.035)	-0.014	(0.058)	0.036	(0.031)
Non-EEA	-0.008	(0.020)	0.018	(0.024)	-0.003	(0.018)
Women	-0.011	(0.024)	0.016	(0.030)	-0.002	(0.020)
Men	0.020	(0.021)	0.017	(0.030)	0.010	(0.020)
18-29 years old	0.007	(0.021)	-0.001	(0.031)	-0.002	(0.020)
30-64 years old	-0.001	(0.026)	0.023	(0.029)	0.006	(0.021)
20 years old and older	0.004	(0.018)	0.017	(0.024)	0.005	(0.016)
Controlling for income	0.002	(0.017)	0.017	(0.023)	0.003	(0.016)
Household on SA	0.031	(0.027)	-0.040	(0.032)	-0.005	(0.024)
Household not on SA	-0.008	(0.018)	0.053	(0.028)	0.011	(0.017)
0-6 years of education	0.113[**]	(0.040)	0.010	(0.049)	0.065[*]	(0.031)
7-12 years of education	-0.034	(0.024)	0.033	(0.032)	-0.006	(0.023)
13+ years of education	-0.015	(0.023)	-0.008	(0.036)	-0.014	(0.022)

Table A14. Cont´d (page 2/2)

	Mean pass rates (full period)		
EEA	0.39	0.45	0.42
Non-EEA	0.40	0.49	0.44
Women	0.44	0.52	0.47
Men	0.35	0.45	0.39
18-29 years old	0.42	0.52	0.46
30-64 years old	0.38	0.46	0.41
Controlling for income	0.40	0.49	0.44
Household on SA	0.43	0.50	0.46
Household not on SA	0.39	0.48	0.42
0-6 years of education	0.42	0.41	0.42
7-12 years of education	0.36	0.48	0.41
13+ years of education	0.42	0.54	0.46

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. In the placebo regressions treatment is defined to take place on 1st of July 2008 and migrants immigrating between 2007-07-01 – 2009-06-30 are included. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A15. Effect on student achievement conditional on enrollment within 6 months – controlling for time before course start and placebo

	Baseline estimates			Placebo estimates		
	Metropolitan areas	Other municipalities	All municipalities	Metropolitan areas	Other municipalities	All municipalities
	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)
Passed any course (1A-3D)	0.039	-0.005	0.016	-0.006	0.017	-0.001
	(0.016)	(0.020)	(0.014)	(0.018)	(0.023)	(0.016)
Started a course within (ref <1 month):						
1-2 month	-0.062***	-0.020	-0.042***	-0.057***	0.003	-0.029*
	(0.012)	(0.014)	(0.009)	(0.016)	(0.019)	(0.013)
2-3 months	-0.097***	-0.049***	-0.073***	-0.112***	-0.026	-0.072***
	(0.013)	(0.014)	(0.010)	(0.017)	(0.020)	(0.014)
3-4 months	-0.130***	-0.090***	-0.109***	-0.152***	-0.071**	-0.113***
	(0.014)	(0.015)	(0.010)	(0.019)	(0.021)	(0.015)
4-5 months	-0.188***	-0.127***	-0.157***	-0.211***	-0.114***	-0.166***
	(0.017)	(0.016)	(0.012)	(0.024)	(0.022)	(0.017)
5+ months	-0.249***	-0.177***	-0.214***	-0.261***	-0.151***	-0.210***
	(0.014)	(0.018)	(0.011)	(0.018)	(0.026)	(0.016)
	Mean pass rate			Mean pass rate		
Any course	0.40	0.49	0.44	0.39	0.48	0.43

Notes: Robust standard errors clustered on municipality interacted by immigration month within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. In the placebo regressions migrants immigrating between 2007-07-01 – 2009-06-30 are included and the reform is defined to take place on the 2008-07-01. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Appendix B. Figures

Figure B1. Course starts within 3 months
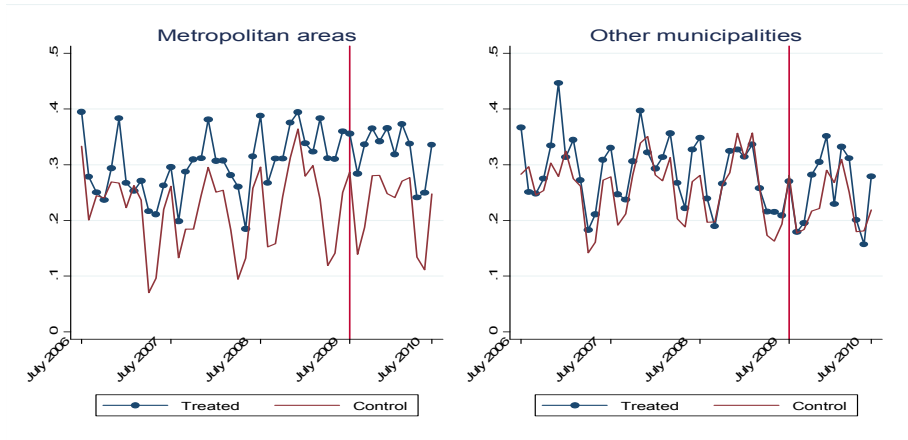
Notes: The figure shows the fraction of the immigrant population, by month of immigration, arriving between 2006-07-01 and 2010-06-30 that starts any course within 3 months after immigration. The vertical line represents the introduction of the bonus scheme that occurred on the 1st of July 2009.

Figure B2. Course starts within 6 months
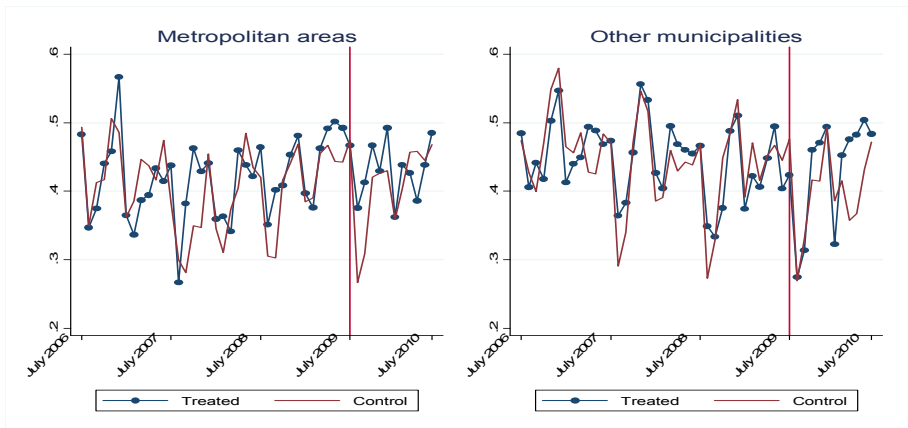
 Notes: The figure shows the share of migrants starting any course within 6 months after immigration. See also note Figure A1.

Figure B3. Course starts within 12 months
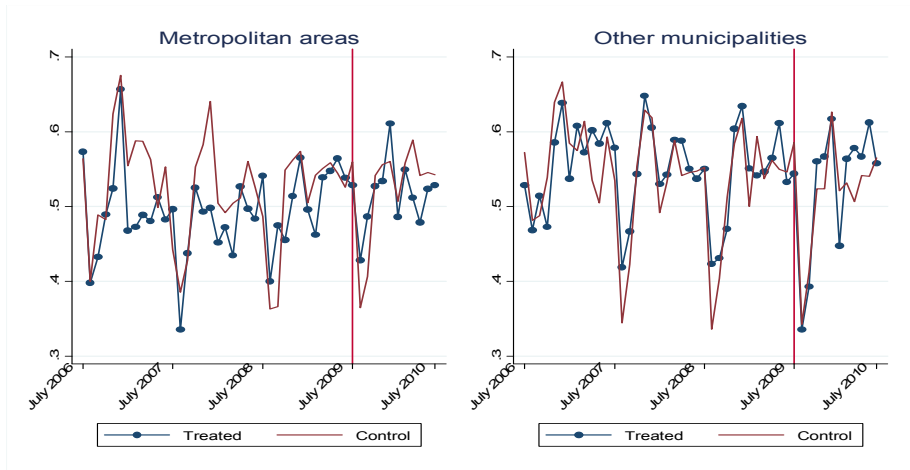
Notes: The figure shows the share of migrants starting any course within 12 months after immigration. See also note Figure A1.

Essay 4. Naturalizations and the Economic and Social Integration of Immigrants•

1. Introduction

Finding ways of better integrating migrants into host societies is high on the policy agenda in most western countries. Evidence from the US, Germany and France has shown that labor market outcomes of migrants improve following naturalizations (Bratsberg et al. 2002, Steinhardt 2012, Fougère and Safi 2009). One proposed explanation to why naturalized citizens experience these positive outcomes is that they move into better jobs (Bratsberg et al. 2002). For example, before naturalization jobs that require citizenship are off limits. In addition, if employers perceive the choice to naturalize as a positive signal this could potentially also enhance labour market opportunities (e.g. OECD 2011). Recent evidence from Norway raises doubt about whether the observed impacts are causal effects or merely correlations (Bratsberg and Raaum 2011). A wish to better integrate into the labor market could, for example, affect both labor market outcomes and the decision to naturalize.

More generally, citizenship is a legal status that formally regulates whom has the right to live in a country, enter the country freely and not to be deported. In some countries citizenship also determines the access to welfare, health and education services. Thus, a better understanding of the implications of acquiring citizenship should be of relevance for policy.

This paper provides new insights into this stream of research. The main emphasis is on effects on labor market outcomes but it also contributes with unique evidence on the timing of the formation of families. This is potentially important as other shocks that coincide in time with naturalizations could bias estimates of the so called citizenship premium. It also casts new light on why immigrants decide to apply for citizenship. For example, anecdotal evidence suggests that migrants planning to have children might naturalize since they, in some instances, have a preference for their children growing up as citizens (Szabó 1997).[1] Being married to a citizen also shortens the waiting time before it is possible to apply for citizenship. This potentially affects the timing of marriages. Further, as shown by Bratsberg and Raaum (2011), in Norway, naturalized citizens spend more time abroad. Thus, it is not certain that naturalizations improve labor market outcomes as behavior not necessarily associated with success on the labor market could be affected.

This study focuses on Sweden. Contrary to many other countries there is no language requirement in place, or any other test of civic knowledge that has to be fulfilled to be able to naturalize. This is likely to undermine the potential signaling value of naturalizations as employers are likely to be aware of the fact that it is relatively easy to become a citizen. Likewise,

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[1] Children to citizens automatically become citizens in countries where the citizenship legislation follow the ius sanguinis tradition.

whereas in some countries many jobs are restricted to nationals (e.g. the US and Germany) other countries, including Sweden, have gone further to equalize the rights of citizens and foreigners (SOU 1999). In the latter case it is reasonable to expect a more modest impact of naturalizations. If we, despite this, find an impact of naturalizations on labor market outcomes there are reasons to believe that naturalizations have an effect in other contexts as well.

Cross-sectional data on outcomes of foreign citizens and naturalized immigrants is not sufficient to determine whether differences in labor market outcomes depend on selection into citizenship or whether naturalizations causes labor market outcomes to improve. Furthermore, it is not possible to explore the timing of any potential effects. Accordingly, the analysis in this paper is based on population wide data covering the years 1990 to 2009. This allows me to some extent to deal with this issue.[2]

The analysis focuses on migrants from outside the OECD as they on average face substantial difficulties in integrating into the labor market. The results can be summarized as follows: on average the labor market outcomes improve following naturalizations. To some extent the results vary in magnitude across country groups and across gender. For most groups however the likelihood of finding a job increases and positive earnings growth is experienced.

The analysis also shows that for some country groups naturalizations are positively correlated with the likelihood of getting married and having children. These hypotheses have never been tried out empirically before, but illustrates that naturalizations could be correlated with a multitude of factors that coincide in time. This, of course, casts doubt on interpretations of estimates in the previous literature. That is, these findings suggests that, if it is not possible to take all possible confounders into account (observed as well as unobserved factors) estimates of the so-called citizenship premium are likely to be biased.

Further, the fact that decisions to naturalize precede the actual change of nationality complicates matters. What we observe in the data is when the application was approved. Thus, it is possible that part of the effect of the decision or the possibility to apply for citizenship precedes the naturalization event. This makes the interpretation of the results less straightforward. A strict causal interpretation would only be possible if all effects occur following naturalizations/the decision to naturalize (of course in the absence of other correlated shocks). Also, we cannot rule out that it is, e.g. the long-term plan to stay in the country (which potentially has an independent effect on labor market outcomes) that drives both naturalization decisions and the

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[2] There is only a handful of paper using longitudinal data, see e.g. Bratsberg et al. (2002), Bratsberg and Raaum (2011), Steinhardt (2012), Fougère and Safi (2009), Scott (2008) and Ohlsson (2008). These papers are reviewed in Section 2.

improvement in labor market outcomes. Obviously, the long-term plan to stay in a country could also affect other outcomes. I will return to this issue later.

The paper proceeds as follows: Section 2 outlines a general framework of how to understand the benefits and costs of acquiring citizenship. The section also contains a description of the relevant institutions and a summary of the previous literature. In section 3 I proceed by describing the data. This section also discusses the general pattern of citizenship acquisitions in Sweden. Section 4 describes the empirical strategy, followed by a presentation the main results in Section 5. Finally, in section 6, I summarize my findings and discuss how they relate to other studies and policy.

2. Background and Institutions

2.1. Effects of Citizenship

Naturalizations have formal as well as informal implications. In most countries only citizens have full access to the labor market. Thus, some jobs are off limits to foreigners. These often include jobs within the police, the military, the judiciary system, the government, but sometimes also other types of jobs. The restrictions vary across countries. Furthermore, because of visa restrictions jobs that require cross-border travels might be difficult to obtain depending on the nationality of the migrant. There could also be administrative costs related to hiring foreign citizens (OECD 2011). Altogether, this suggests that naturalizations can ease the labor market integration of migrants as they gain full access to the labor market.

Apart from the legal aspects naturalizations also have a more informal side. Barriers to employment potentially diminish if employers are more willing to hire citizens than foreigners (OECD 2011). It could be the case if naturalizations are perceived as a positive signal. One of the requirements for naturalizations is an interrupted period of stay in the host country, normally a number of years. This is a period normally long enough to at least attain some country-specific skills valued at the labor market. It is therefore natural to believe that naturalizations could function as a proxy for these types of characteristics.[3] Furthermore, we cannot exclude the possibility of naturalized immigrants facing lower levels of statistical discrimination than foreign citizens.

Lower barriers to employment, or perceived lower barriers, could also affect the search intensity for jobs. Similarly it could encourage investments in higher education as the return to education potentially rises. Incentives for family formation and childbearing could likewise change as citizenship, in

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[3] The question whether employers actually care about the citizenship of their employees is an open question, at least in the Swedish context.

many countries, is passed on from parents to children. Thus, if foreign residents perceive it as beneficial for their children to grow up as citizens they might apply for citizenship before having children. Anecdotal evidence from Sweden suggests that this might be true (Szabó 1997). On the other hand, it is possible to apply for citizenship at a later point in time as well. Similarly, the decision to naturalize might be correlated with marriages. The reason is that marriages with citizens in some countries shorten the waiting time before it is possible to apply for citizenship. Further, one of the requirements to apply for naturalizations in most countries is an interrupted period of stay. This imply that migrants following naturalizations potentially spend more time abroad (visiting family or due to other reasons).[4]

2.2. Previous Literature

The literature on impacts of naturalizations is limited. Most studies focus on labor market outcomes. This topic will consequently receive most of the attention in this review.[5] The majority of the studies on labor market outcomes rely on cross-sectional data.[6] From this evidence it is difficult to establish whether naturalizations actually have a causal impact on labour market outcomes. The observed differences between naturalized citizens and foreign citizens could be driven solely by the selection processes into citizenship. For this reason longitudinal data is essential as it to some extent allows us to deal with this issue. The existing evidence based on the later type of data can easily be summarized. Fougère and Safi (2009) presents evidence of rising employment probabilities of immigrants that become French citizens. Bratsberg et al. (2002) explore the NLSY and show that naturalizations have a positive impact on the wage growth of male immigrants to the United States the years following naturalizations. Steinhardt (2012), furthermore, examined the situation in Germany and

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[4] There are a large number of studies that explores the correlation between immigrant characteristics and whom that naturalizes. This falls outside the scope of this paper but, in general, both individual characteristics and features of the country of origin and the host country are potentially important explanations (see e.g. Chiswick and Miller, 2009, for a good overview of the literature). Some examples include the age of the migrant, gender, the reason for immigration, years since immigration and country of birth. Other factors include the cost of returning (e.g. distance to the home country), the level of income, political freedom, civil and economic freedom in the country of origin. Institutional factors surely also play a role. Mazzolari (2009) has e.g. shown that rules restricting dual citizenship rights in migrants' source countries diminish the likelihood of naturalizing in the US.

[5] There is also a large literature on social inclusion with contributions from primarily political scientist and sociologist (see e.g. Kesler and Demireva (2011) for an overview of the literature). Political economists have long focused on the size of government and how it relates to the voter base (see e.g. Meltzer and Richard, 1981). This literature has been less explicit about naturalizations and how it changes the voter base. One exception is Ortega (2010).

[6] See Liebig and Von Haaren (2011) for an overview. A few examples include Chiswick (1978), Devoretz and Pivnenko (2004) and Bevelander and Veenman (2006).

found an immediate effect of naturalizations on male wages. His analysis also shows that naturalizing leads to increased wage growth the years following naturalizations. A similar pattern is found for Switzerland (Steinhardt and Wedemeier 2011).

The evidence from other countries is more mixed. For example, for Sweden, there is some evidence of positive earnings growth of immigrants already prior to the naturalization event (Ohlsson 2008). There is also a study showing mixed results on the impact of naturalizations on wages and employment (Scott 2008). For Norway, there is further evidence of the relation between naturalizations and labor market outcomes being an association rather than a causal relation (Bratsberg and Raaum 2011). Furthermore, the authors show that for some immigrant groups labor market outcomes deteriorate following naturalizations.

Little is known about why positive effects are found for some countries. Bratsberg et al. (2002) proposes that the positive outcome for the US partly can be explained by changes in the job distribution. They show that there is a tendency of naturalized migrants to move into better-paying sectors and/or sectors where job restrictions for foreign citizens exist.

Concerning other types of outcomes the collected evidence is even scarcer. The effects of granting birth-right citizenship in Germany on the social integration of immigrant parents has been evaluated by Avitabile, Clots-Figueras and Masella (2010). They show that parents whose children are born as German citizens following the reform are more likely to establish contacts with native Germans. In a follow up paper they also demonstrate that parents invest more in children that were born as citizens (Avitabile, Clots-Figueras and Masella 2012).

2.3. Institutions

In Sweden, the rules regulating citizenship acquisitions and losses are laid out in the Citizenship Act of 2001 (e.g. Björk and Sandesjö 2009). The nationality law is built on two main principles: the principle of ius sanguinis and avoidance of statelessness (Lokrantz Bernitz and Bernitz 2006). The first principle refers to the practice of determining an individual's nationality according to the citizenship of a parent or an ancestor. The second principle can be traced back to the 'Universal Declaration of Human Rights' from 1948 that affirms that everyone has the right to a nationality.

There are three main ways of acquiring citizenship in Sweden: automatically, by notification and by naturalization. Children to citizens belong to the group that receives citizenship automatically. The notification procedure is a simplified procedure for foreign citizens aged 18 to 19 years old who have had a permit for residence since the age of 13. A simplified

procedure also applies for Nordic citizens.[7] All individuals that do not receive citizenship automatically or cannot make use of the notification procedure will have to apply for naturalization. To naturalize an individual must fulfil the following requirements: the applicant must be able to identify him- or herself, be at least eighteen years old, have a permanent residence permit, have resided in Sweden for five years and fulfil the good conduct requirement (Björk and Sandesjö 2005).[8] In comparison with the practice in many other countries the required residence period is relatively short. Further, there is no language requirement in place, nor any test of civic knowledge or other requirements that have to be fulfilled to be able to naturalize.[9] The current requirements have at large been the same since the late 1970s.

2.4. Rights and Duties of Citizenship

A guiding principle to equalize the rights and duties of foreign and Swedish citizens has existed since the late 1960s. Over time permanent residents have gained most of the rights that citizens have (SOU 1999). This includes, e.g. full access to the social insurance system and other welfare systems.[10] As a result of this policy the formal significance of being a citizen has decreased (see e.g. Lokrantz Bernitz and Bernitz 2006, SOU 2000 and Ds A 1984).

Some rights (and duties) are however reserved to citizens. These can be categorized into four groups: restrictions on political participation, labour market restrictions, the permanent right to reside in Sweden, and mobility restrictions. With regards to the first group, only Swedish citizens are allowed to vote in national elections and get elected into parliament. The second group includes the restriction of certain occupations to citizens. Jobs exclusive for nationals include a number of government posts, posts in the judiciary sector and certain occupations within the military and police services (SOU 1999). The third restriction denotes that the ever-lasting right to reside in Sweden is limited to citizens. Finally, the last group of restrictions is conditional on the earlier citizenship. E.g. having a Swedish passport potentially increases international mobility. This could be of importance for jobs that require cross-country travels. One example is that for citizens from outside the European Union, Swedish citizenship guarantees full mobility within the European Union (SOU 2000).

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[7] There is a long tradition of collaboration between the Nordic countries regarding citizenship law that started to develop in the 1890's. See Lokrants Bernitz (2009) for a short overview.

[8] For refugees the residence requirement is four years. Individuals married to or cohabiting with a Swedish citizen can apply for citizenship after three years.

[9] See Guimezanes (2011) for an overview of nationality laws in the European Union and selected OECD countries.

[10] Another example of the ambition to equalize rights and duties between Swedish and foreign citizens is the right to vote in municipal elections. This is a legal right for foreign citizens since 1975 (see Ds A 1984:6).

Duties or obligations limited to citizens are few. One example is the previous mandatory military service for men[11], another is that Swedish citizens are subject to Swedish law when spending time abroad (SOU 2000).

To summarize, the Swedish institutions are suggestive of a modest impact of naturalizations given the fact that it is relatively easy to become a citizen and that only a few jobs are restricted to nationals.[12]

3. Data and Descriptive Statistics

The analysis is built on register data from Louise. It is a database administrated by Statistics Sweden. The database covers the full population in working age between 1990 and 2009. It includes a rich set of human capital and sociodemographic characteristics including; e.g. the latest year of immigration, country of birth, educational background as well as information on labour market outcomes. This dataset is linked, through a personal identifier, to a population register (Historiska FBR) that contains information on dates of naturalizations.

The analysis focuses on non-western migrants as this group faces substantial difficulties in integrating into the labor market (Lemaître 2007).[13] The sample is split into different subgroups based on birth regions. I divide the sample as the reasons for immigration and the propensity to naturalize to some extent differ between different source regions. The variation within each group is of course also large but this is something I abstract from. Further, the sample is divided by gender.

In Table 1 I present the studied cohorts.[14] They include all immigrants aged 20 to 64 years old that have arrived from a selected number of countries (see Table A1 in the appendix) between 1985 and 2009. The sample is restricted to individuals between 20 and 55 years at the time of immigration. The upper age limit is set to 55 as the primary outcomes include labor market outcomes. It implies that all individuals in the sample have at least ten years left before they reach the normal retirement age. The reason why immigrants close to the retirement age are excluded is that they are likely to face different incentives to integrate into the labor market then younger migrants. The lower age limit is set as there is a simplified procedure for naturalizations for individuals younger than 20 (Section 2.3).

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[11] The system with compulsory enrollment to the army was abolished in 2010.

[12] In a comparison between the US and Norway, Bratsberg and Raaum (2011) argues that relatively few jobs are exclusive for nationals in Norway. They also put forward the argument that since most migrants naturalize after a relatively short time period in Norway, the signaling value of naturalizations is likely to be low. Thus, the same types of arguments that are likely to hold also for Sweden.

[13] See Table A1 in Appendix A for a list of the countries included in the sample.

[14] The sample restrictions are similar to Bratsberg and Raaum (2011).

The table displays some interesting patterns. The largest source regions are the Balkans and Eastern Europe and the Middle East and North Africa. Further, a large share of the migrants from all source regions have naturalized by the end of the observation period. Women are somewhat more likely to have naturalized than men. Moreover, the table also shows that the mean age at the time of arrival is around 30 years for all groups and that there is some variation in the time of arrival to Sweden.

Table 1. Cohorts studied

	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Women					
Individuals	65 792	80 671	26 769	41 432	21 685
Mean year of arrival	1995.25	1994.23	1995.60	1996.37	1993.42
Mean age of arrival	32.52	30.45	28.31	30.01	31.74
Fraction naturalised by end of 2009	0.84	0.85	0.71	0.64	0.70
Men					
Individuals	59 060	103 024	29 628	28 181	19 265
Mean year of arrival	1994.91	1993.59	1994.92	1996.67	1993.01
Mean age of arrival	32.82	30.79	30.06	30.15	31.47
Fraction naturalised by end of 2009	0.81	0.81	0.63	0.56	0.62

Notes: The sample includes all immigrants arriving to Sweden between 1985 and 2009 from selected birth regions, aged 20-64 years old the year of observation, and that were between 20-55 years old at the time of immigration. Further, to be included the migrant must be observed at least once between 1990 and 2009.

Individual decisions to naturalize are likely not to be determined solely by labor market considerations or expectations of faster economic integration but rather an interplay of factors (see Section 2). That the country of origin matters is clearly illustrated in Figure 1. Migrants from low- and middle-income countries are much more likely to naturalize than migrants born in high-income countries. This pattern is consistent with the general pattern in the OECD-countries. In comparison with immigrants to other OECD countries a relatively large share of the immigrants to Sweden however naturalizes (OECD 2011 and Lokrantz Bernitz 2009).

The regression sample is presented in Table 2. It shows substantial variation in labor market outcomes across birth regions and gender. In terms

of labor market outcomes women from the Balkans, Eastern Europe, Asia and Latin America are somewhat better off in comparison with other women. For example, fewer than half of the women from the Middle East, North Africa and Sub-Saharan Africa had any income from labor during the observation period. Similarly, almost fifty percent belonged to households that received income support (social assistance). The outcomes of men are, on average, better. Further, the majority of men and women from all countries are married and at least sixty percent of the women have children present in the household.

Figure 1. Naturalizations by birth region
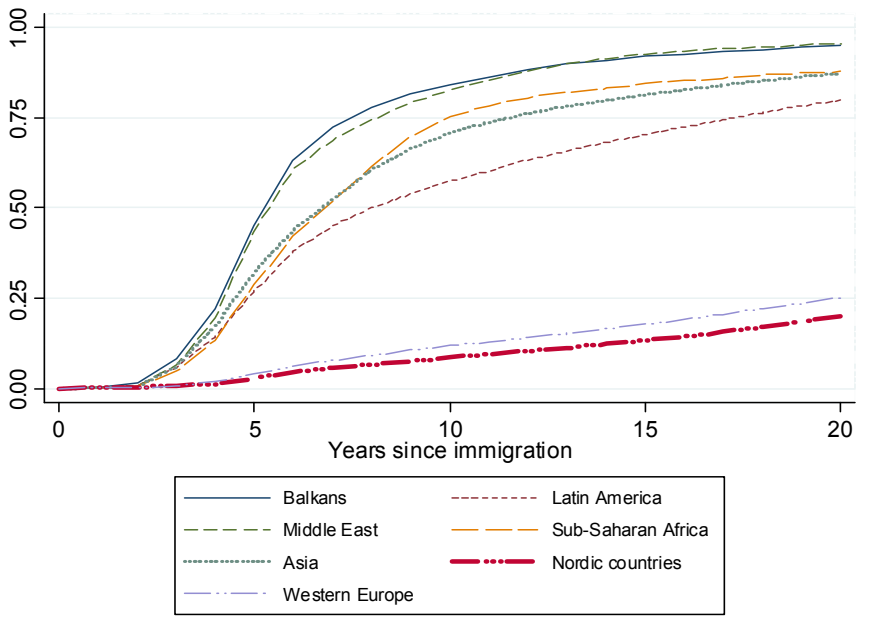
Notes: The y-axis is the share of immigrants that have naturalized. Immigrants born in the Nordic countries (Norway, Denmark, Finland and Iceland) and Western Europe are added to the sample as a reference. For sample restrictions see Table 1.

As a comparison, but also to be able to identify business cycle effects (this is discussed in the following section), a sample of 10 percent of the Swedish born population without a high school diploma is added to the baseline sample.

Table 2. Regression sample

	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America	Native born
	Women					
Observations (in 1000's)	691	793	212	323	218	1 076
Any work-related income	0.54	0.37	0.47	0.54	0.64	0.66
Annual earnings from labor unconditional on employment	889.57	497.99	633.44	719.09	937.68	958.02
Social Assistance	0.35	0.46	0.46	0.20	0.24	0.09
Age	39.35	37.99	34.89	36.33	39.60	43.85
Married	0.70	0.75	0.60	0.64	0.53	0.45
Child in household	0.60	0.69	0.65	0.60	0.60	0.30
College degree	0.39	0.34	0.23	0.32	0.38	N/A
Years since immigration	6.83	7.54	6.58	6.32	7.86	N/A
Naturalized	0.51	0.52	0.41	0.37	0.41	N/A
	Men					
Observations (in 1000's)	621	1 039	235	178	195	1 347
Any work-related income	0.62	0.48	0.57	0.56	0.73	0.69
Annual earnings from labor unconditional on employment	1279.07	784.78	947.04	964.84	1407.15	1484.64
Social Assistance	0.34	0.41	0.40	0.28	0.22	0.08
Age	39.79	38.69	36.90	36.16	39.52	44.10
Married	0.69	0.64	0.55	0.57	0.47	0.40
Child in household	0.54	0.51	0.37	0.42	0.42	0.25
College degree	0.34	0.36	0.35	0.44	0.37	N/A
Years since immigration	6.97	7.90	6.85	6.01	8.04	N/A
Naturalized	0.50	0.53	0.40	0.34	0.37	N/A

Notes: The native born sample includes a 10 percent sample of the Swedish born population without a high school diploma. For more information on the population studied see Table 1. Having any work-related income is defined as having annual earnings from work larger than zero; social assistance refers to social assistance receipts at the household level.

The group was chosen as this is the group that most resembles the foreign born population in terms of labor market outcomes. Nevertheless, for all

migrant groups outcomes are worse than among the Swedish born (Table 2).[15]

The characteristics of those that naturalize are likely to differ from those that do not. Figure 2 and 3 illustrates that this is probably a relevant observation for both women and men from all country groups. Migrants that naturalize within the observation period experience a faster increase in the probability having any work-related income than migrants that do not naturalize. Another interesting observation is that the increase is smooth for migrants around the time period when most migrants naturalize, i.e. between 5 and 10 years after immigration (cf. Figure 1). This is suggestive of a modest impact of naturalizations.[16] Similar observations can be made for annual earnings and also for the likelihood of having a child and getting married (see Figures C1 – C6 in the appendix).

Figure 2. Share of women with any work-related income
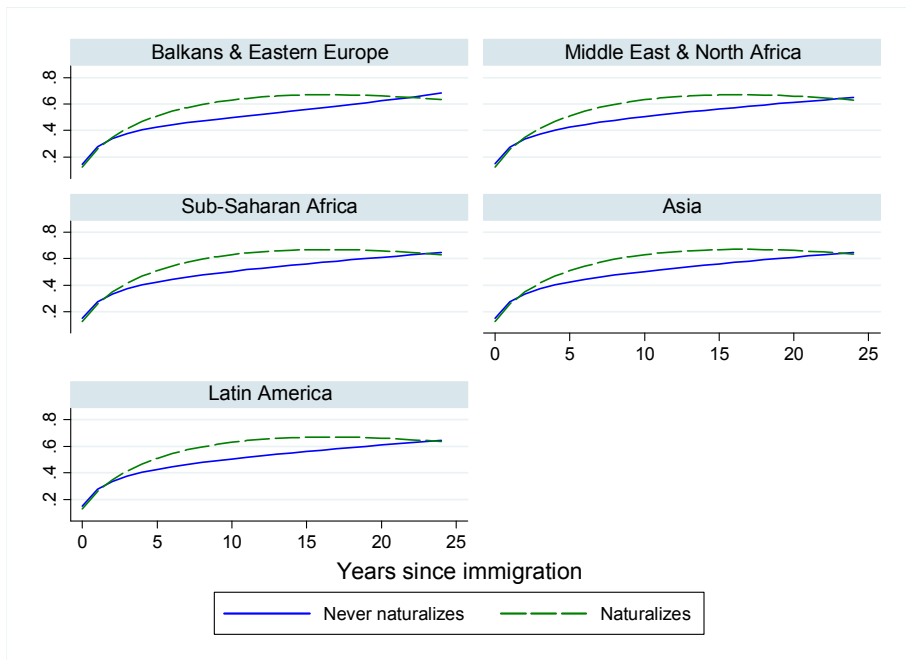

Notes: The figure is predicted from a regression of a dummy indicating whether the individual had any income from labor on a quartic function of years since immigration. For sample restrictions see Table 1.

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[15] See Eriksson (2011) for an overview of studies on labor market outcomes of immigrants to Sweden.
[16] The same pattern is observed in Norway (Bratsberg and Raaum 2011).

Figure 3. Share of men with any work-related income
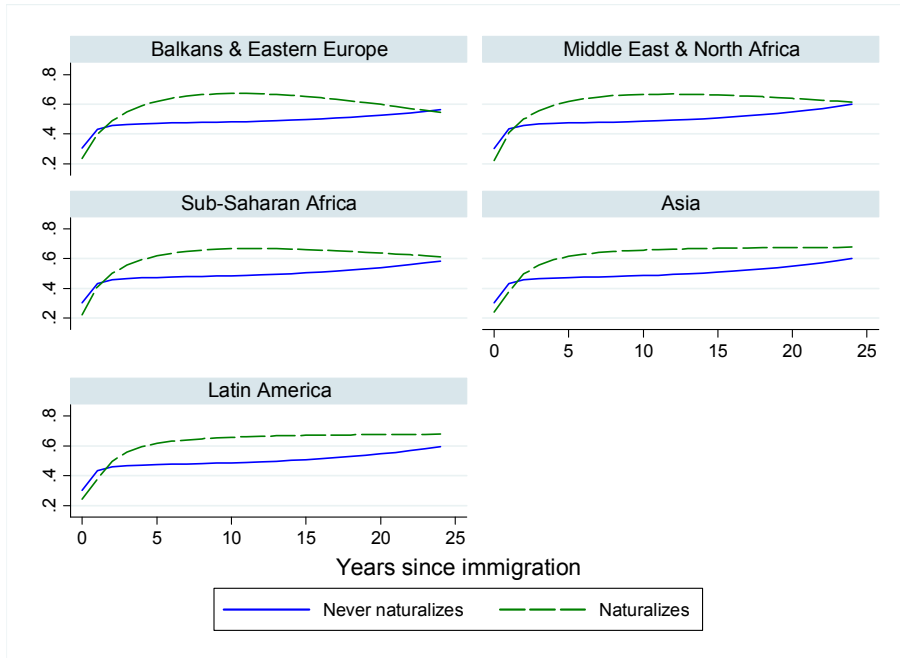
Notes: See Figure 2.

4. Empirical Model

The relationship between naturalizations and labor market outcomes and family formation is modeled in the following fashion:

$$y_{it} = a_0 + a_1 N_{it} + a_2 D_i X_{it} + \delta X_{it} + \gamma Z_{it} + \mu_i + \vartheta_t + \varepsilon_{it} \quad (1)$$

It follows the modeling approach in Bratsberg et al. (2002) and Bratsberg and Raaum (2011). The discussion below follows the latter paper closely. y_{it} is the outcome of interest of individual i at time t. N_{it} is an indicator of naturalization status that is set to unity the naturalization year and all subsequent years. a_1, thus captures the immediate impact of becoming a citizen. X_{it} is labor market experience from the Swedish labor market approximated by years since immigration as actual experience is not observed in the data. D_i is a time-constant dummy set to unity for individuals that naturalize during the observation period and a_2 thus captures potential differences in the experience profiles of individuals that naturalize during the observation period and individuals that do not. Z_{it} is a vector of controls including age. ϑ_t is the observation year to control for business cycle effects. For identification of these effects a ten percent sample of the Swedish born

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population without a high school diploma is included (see also Section 3).[17] μ_i is an individual fixed effect and ε_{it} is a common error term. Standard errors are clustered within individuals.

I also estimate an augmented model that allows for a gradual change in outcomes the years following naturalizations:

$$y_{it} = a_0 + a_1 N_{it} + a_2 N_{it}(X_{it} - X_{iN}) + a_3 D_i X_{it} + \delta X_{it} + \gamma Z_{it} + \mu_i + \vartheta_t + \varepsilon_{it} \qquad (2)$$

X_{iN} is the labor market experience at the time of naturalization. a_2, thus captures a gradual divergence in outcomes by time since naturalization. Thus, in comparison with model (1) that only captures the average constant effect of naturalizations this model is less restrictive. When estimating the model I will also allow for changes in outcomes up to two years prior to naturalizations. This is done to explore whether any changes occur before an individual becomes a citizen. If such effects are observed this would be an indication of the estimated effects of naturalizations not being purely causal.

Depending on the model restrictions, identification arises from different sources. In a model where $D_i = 0$ those that eventually naturalize and those that do not are assumed to have similar returns to experience. In addition, where individual fixed effects are not controlled for, identification of the parameters a_1 and a_2 arises from the fact that immigrants naturalize at different points in time and that some never do. A model where D_i is allowed to vary captures the within group effect of naturalizations. Thus, it takes into account differences in the experience profile of migrants that naturalize and those that do not. Adding individual fixed effects means that the parameter a_1 and a_2 are identified by changes in outcomes "within" the individual the period before and after the change of citizenship.

All models, of course, control for time since immigration. Thus, any observed changes are on top of the effect arising from more time spent in Sweden. Finally, for the identification strategy to be valid no other individual shocks can be correlated with the decision to naturalize. It is a strong assumption. I will return to this in Section 5 as the evidence presented there suggests that, e.g. decisions regarding childbearing and marriages are associated with naturalizations.

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[17] The results are not sensitive to these sample restrictions. The use of a 10 percent sample of the Swedish born population unconditional on education gives similar results.

5. Results

This section contains two parts: Section 5.1 discusses the effects of naturalizations on labor market outcomes. It starts by an exploration of potential employment effects and continues with an analysis on the impact of acquiring citizenship on annual earnings. In Section 5.2 the potential association between family formation and naturalizations is discussed.

5.1. Labor Market Attachment

In Table 4 the effects of acquiring citizenship on immigrant's attachment to the labor market are presented. The table includes estimates from three different models based on equation (1).

The first model (model I) is a simple comparison of outcomes between immigrants that has naturalized during the observation period and immigrants that eventually naturalizes and those that do not. Time spent in Sweden is taken into account. Thus, any observed effects are on top of the return to experience from the Swedish labor market. For women, the table shows that naturalizations are associated with an, on average, 8 percentage point higher likelihood of having any work-related income. It also shows that there is substantial variation across country groups. The point estimates range between 6 and 17 percentage points.

Model II allows for the possibility that the experience from the Swedish labor market differ for immigrants that naturalizes and those that do not (as suggested by Figure 2 and 3). The data support this notion. For most country groups the estimated association is weaker. This shows that naturalized citizens are positively selected, i.e. immigrants that chose to naturalize have more favorable labor market outcomes already to start with.

The last model, which takes into account overtime fixed individual characteristics such as e.g. inherent ability, the age at time of immigration, education obtained in the home country, demonstrates that immigrants that decide to apply for citizenship indeed are selected group (model III). Thus, if we do not take this into account we would overestimate the naturalization premium. For all groups except for women from Latin America the association between naturalizations and positive earnings nevertheless remains positive. The point estimates shows that the likelihood of having any income labor increases by between 1 percentage point and 2.5 percentage points depending on the country group.

For men, a similar pattern is found. The first model shows that, on average, naturalized men are more likely to have any work-related income in comparison with the non-naturalized. Further, taking differences in labor market experience into account significantly decrease the estimated effects (model II). Lastly, Model (III) demonstrate that, for immigrant men on average, naturalizations are followed by a positive increase in the likelihood

of having any income from labor. The analysis also shows that there is substantial variation across the country groups. Men from the Balkans and Eastern Europe and Middle East and North Africa appear to be driving the results. For the other groups the point estimates are negative but cannot statistically be distinguished from zero.

Table 4. Effect of naturalizations on having any work-related income

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
I: Common ysm profile	0.079***	0.097***	0.092***	0.170***	0.063***	0.114***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)
II: Separate ysm profile	0.060***	0.058***	0.077***	0.160***	0.060***	0.065***
	(0.002)	(0.004)	(0.003)	(0.007)	(0.006)	(0.005)
III: Separate ysm-profile and individual fixed effects	0.016***	0.009*	0.015***	0.024***	0.010*	0.008
	(0.002)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)
Men						
I: Common ysm profile	0.107***	0.139***	0.115***	0.159***	0.145***	0.102***
	(0.002)	(0.004)	(0.003)	(0.006)	(0.008)	(0.005)
II: Separate ysm profile	0.041***	0.066***	0.063***	0.055***	0.061***	0.021***
	(0.002)	(0.004)	(0.003)	(0.006)	(0.008)	(0.005)
III: Separate ysm-profile and individual fixed effects	0.011***	0.013***	0.013***	-0.009	-0.009	-0.002
	(0.002)	(0.003)	(0.002)	(0.005)	(0.006)	(0.005)

Notes: The results are estimated by OLS/LPM. Standard errors clustered within individuals in parentheses. Each cell represents a separate regression. In all models controls for years since immigration, age, the interaction between age and whether the individual was born in Sweden or not, and the observation year, are included (all controls are introduced as dummies). Model I includes a joint experience profile for those that naturalize and those that do not, model II relaxes this restriction and in model III individual fixed effects are included. Sample sizes for women by birth region are for All (3 405 750), Balkans & Eastern Europe (1 767 799), Middle East & North Africa (1 869 406), Sub-Saharan Africa (1 288 485), Asia (1 399 225), Latin America (1 294 450) and for men (3 658 803); (1 958 351); (2 365 512); (1 571 915); (1 514 991); (1 532 204). The female sample includes a ten percent sample of Swedish born women without a high school diploma which correspond to 1 076 494 observations and for men 1 336 944. For additional information on the size of cohorts see Section 4.
* p<0.05, ** p<0.01, *** p<0.001

The estimates reported in Table 4 should be interpreted as the constant effect of naturalizations on the likelihood of having any income from labor. This type of model is restrictive as it is possible that outcomes changes

already prior to naturalizations. To explore that possibility a model that allows outcomes to change the years before and after the acquisition of citizenship is needed.

In Table 5 I present the results from this type of specification. It is an extended model based on equation (2). The models are estimated using separate experience profiles and include controls for individual fixed effects (model III). For women, the results show that, on average, the likelihood of having any income from labor is around 4 percentage points higher the naturalization year. For most country groups this change appears to be persistent, i.e. the observed association between naturalizations and the likelihood of having any income do not return to zero the years following naturalizations. The only exception is women from Latin America, where the effects cling off following the third year after naturalizations.

Further, for all groups the estimates show that the positive increase in the likelihood of having any income precedes the naturalization event. This indicates that a causal interpretation of the results in Table 4 potentially would be incorrect. That is, if naturalizations e.g. function as a door-opener into the labor market we would not expect any effects in the pre-period. Thus, on the one hand the evidence is suggestive of naturalizations being associated with improved labor market outcomes rather than being the driving force behind the positive outcomes. On the other hand, it could be that the immigrants are affected by the mere possibility to apply for citizenship, i.e. that the institutional "right" to apply for citizenship causes individual behavior to change.

For men, a similar pattern is found. The estimated effects are however larger. Persistent effects are found for men from the Balkans and Eastern Europe as well as for men from the Middle East and North Africa. Just as for women the positive correlation is observed prior to the naturalization event. For the other groups the association becomes weaker the years following naturalizations. Moreover, the point estimates eventually turns negative (but not significant), except for men from Sub-Saharan Africa.

The outcome, to have any income at all from labor, is probably a poor indicator of successful integration of immigrants into the labor market. An income just above this threshold is not enough for self-sufficiency. To this end, an alternative employment measure is used. It is defined as having annual earnings higher than 3 basic amounts.[18] This corresponds to 133 500 SEK in 2013 (or approximately 14 600 Euro).

Table B1 in the appendix presents the results using this definition. It shows that the likelihood of employment increases for women from most

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[18] Is an amount based on the consumer price index, CPI. It is adjusted annually by the government and is used e.g. within the social insurance system to set benefit levels.

country groups following naturalizations.[19] Further, the estimated effects are larger than the effects reported in Table 4 and 5.

Table 5. Effect of naturalizations on having any work-related income (alternative specification)

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Two years before	0.027***	0.027***	0.023***	0.041***	0.026***	0.044***
	(0.001)	(0.003)	(0.002)	(0.005)	(0.004)	(0.005)
One year before	0.036***	0.036***	0.028***	0.064***	0.035***	0.049***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.005)	(0.006)
Naturalization year	0.040***	0.037***	0.033***	0.067***	0.036***	0.046***
	(0.002)	(0.004)	(0.004)	(0.007)	(0.006)	(0.006)
Year after	0.040***	0.032***	0.036***	0.064***	0.031***	0.039***
	(0.002)	(0.005)	(0.004)	(0.008)	(0.007)	(0.007)
Two years after	0.036***	0.026***	0.030***	0.057***	0.026***	0.028***
	(0.003)	(0.005)	(0.005)	(0.009)	(0.007)	(0.008)
Three years after	0.039***	0.024***	0.035***	0.053***	0.033***	0.025**
	(0.003)	(0.006)	(0.005)	(0.010)	(0.008)	(0.008)
Four years+	0.045***	0.028***	0.036***	0.061***	0.039***	0.012
	(0.004)	(0.006)	(0.006)	(0.011)	(0.009)	(0.009)
Men						
Two years before	0.047***	0.049***	0.043***	0.073***	0.060***	0.038***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)
One year before	0.060***	0.063***	0.056***	0.077***	0.072***	0.044***
	(0.002)	(0.004)	(0.003)	(0.006)	(0.007)	(0.005)
Naturalization year	0.055***	0.060***	0.051***	0.072***	0.057***	0.041***
	(0.002)	(0.004)	(0.003)	(0.007)	(0.008)	(0.006)
Year after	0.045***	0.051***	0.043***	0.035***	0.037***	0.021**
	(0.003)	(0.005)	(0.004)	(0.008)	(0.009)	(0.007)
Two years after	0.038***	0.046***	0.037***	0.009	0.019	0.012
	(0.003)	(0.005)	(0.004)	(0.009)	(0.010)	(0.007)
Three years after	0.034***	0.043***	0.030***	-0.004	0.008	0.002
	(0.003)	(0.006)	(0.005)	(0.009)	(0.011)	(0.008)
Four years+	0.034***	0.046***	0.032***	-0.028**	-0.017	-0.009
	(0.004)	(0.007)	(0.005)	(0.010)	(0.012)	(0.009)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 4.
* p<0.05, ** p<0.01, *** p<0.001

For men, the previous measure indicated a significant association for two groups only (men form the Balkans and Eastern Europe and the Middle East and North Africa). The latter estimate however indicates that effects are

[19] Employment defined as annual earnings larger than two basic amounts yield similar results.

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present for all groups except for men from Latin America. The more flexible specification also supports this notion (Table B2). Thus, naturalizations appear to be associated with immigrants, on average, finding more stable sources of income.

In Table 6 the effects of becoming a citizen on annual earnings are presented. The sample is conditioned on having positive earnings from labor. The table shows that for women having naturalized is associated with, on average, 10 percent higher earnings (model I). It also shows that the correlation is positive for all groups but varies in strength.

Model (II), where differences in the experience profile of those that naturalize in comparison with those do not is taken into account, shows that this consideration is less important than for the outcomes previously discussed. This is in line with the observation that the earnings curve of those that ever naturalizes follows the one for those that never naturalizes closely (see Figure C1 and C2 in the appendix). Further, model (III) demonstrates that the effect on annual earnings is only significant for women from the Balkans and Eastern Europe and the Middle East and North Africa. The point estimates are however positive in all instances.

For men the results shows that naturalized immigrants from all country groups have higher earnings than foreign citizens (model I and II). When individual fixed effects are taken into account the positive correlation however only remains for migrants from the Balkans and Eastern Europe and the Middle East and North Africa. Thus, for both men and women there is evidence of positive selection into citizenship, although the effect varies across country groups.

Table 7 presents the estimated effects on annual earnings from the model that allows for changes already prior to naturalizations. The table shows that for women, on average, earnings start to grow the two years preceding naturalizations. It also shows that the earnings growth is persistent. That is, following naturalizations women, on top of the experience effect from being in Sweden, have higher earnings than in the preceding period. The correlation varies between the groups but tends to be the strongest the naturalization year.

For women from the Middle East and North Africa, Asia and Latin America the association is only significant the immediate years surrounding naturalizations. Again, the findings suggest that the observed effects are correlated with naturalization decisions but not caused by them. At least we cannot interpret the effects as being strictly causal.

For men the observed correlations are stronger but follow the same time pattern as for women, i.e. earnings improve already prior to naturalizations. The increase seems to be lasting for immigrants from the Balkans and Eastern Europe, the Middle East and North Africa and Latin America. For the other groups the effects decline towards zero the period following naturalizations.

Table 6. Effect of naturalizations on annual earnings

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
I: Common ysm profile	0.098***	0.124***	0.109***	0.120***	0.049***	0.144***
	(0.005)	(0.009)	(0.012)	(0.019)	(0.012)	(0.014)
II: Separate ysm profile	0.096***	0.113***	0.126***	0.110***	0.033*	0.101***
	(0.006)	(0.010)	(0.012)	(0.020)	(0.015)	(0.016)
III: Separate ysm-profile and individual fixed effects	0.040***	0.047***	0.028*	0.029	0.022	0.017
	(0.006)	(0.010)	(0.013)	(0.021)	(0.015)	(0.016)
Men	All					
I: Common ysm profile	0.172***	0.176***	0.245***	0.254***	0.068***	0.193***
	(0.005)	(0.009)	(0.009)	(0.017)	(0.020)	(0.014)
II: Separate ysm profile	0.111***	0.103***	0.186***	0.170***	0.068**	0.057***
	(0.006)	(0.010)	(0.010)	(0.019)	(0.023)	(0.016)
III: Separate ysm profile and individual fixed effects	0.037***	0.039***	0.046***	0.008	-0.010	0.020
	(0.005)	(0.009)	(0.009)	(0.019)	(0.021)	(0.015)

Notes: The outcome is defined as the log of annual earnings from labor. Sample sizes for women by birth region are for All (1 833 353), Balkans & Eastern Europe (1 087 645), Middle East & North Africa (1 003 775), Sub-Saharan Africa (812 022), Asia (886 040), Latin America (852 031) and for men (2 205 964); (1 302 767); (1 412 298); (1 051 845); (1 016 473); (1 059 863). Both female and male sample includes a ten percent sample of Swedish born women/men without a high school diploma. Standard errors clustered within individuals in parentheses. See also note Table 4.
* p<0.05, ** p<0.01, *** p<0.001

The results presented in Table 6 and 7 are not conditioned on the individual being formerly employed or having earnings above a certain threshold. Again, it is therefore relevant to explore whether naturalizations are beneficial for individuals with more stable sources of income. For this purpose I restrict the sample to individuals with earnings above 133 500 SEK (the employment measure used above). It then becomes apparent that the earnings effects are driven by immigrants with relatively low levels of income (see Table B3-B4 in the appendix). This is the case as the effect size declines substantially in magnitude for both men and women. In some instances, e.g. for women from the Middle East and North Africa, the point estimates are even found to be negative (Table B4). That is, naturalizations are associated with a decline in earnings for this group.

Table 7. Effect of naturalizations on annual earnings (alternative specification)

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Two years before	0.036***	0.052***	0.007	0.076**	0.018	0.037*
	(0.007)	(0.012)	(0.015)	(0.024)	(0.017)	(0.018)
One year before	0.085***	0.119***	0.033	0.159***	0.040*	0.069***
	(0.008)	(0.014)	(0.017)	(0.027)	(0.019)	(0.020)
Naturalization year	0.100***	0.125***	0.061**	0.164***	0.061**	0.078***
	(0.009)	(0.016)	(0.019)	(0.030)	(0.021)	(0.022)
Year after	0.080***	0.121***	0.030	0.098**	0.046	0.041
	(0.010)	(0.018)	(0.021)	(0.034)	(0.024)	(0.024)
Two years after	0.083***	0.113***	0.039	0.109**	0.027	0.042
	(0.011)	(0.020)	(0.023)	(0.037)	(0.026)	(0.025)
Three years after	0.074***	0.104***	0.020	0.097*	0.028	0.027
	(0.012)	(0.022)	(0.024)	(0.041)	(0.029)	(0.028)
Four years+	0.093***	0.109***	0.047	0.094*	0.065*	0.046
	(0.014)	(0.024)	(0.027)	(0.043)	(0.032)	(0.030)
Men						
Two years before	0.092***	0.079***	0.079***	0.110***	0.175***	0.090***
	(0.006)	(0.011)	(0.011)	(0.020)	(0.022)	(0.017)
One year before	0.132***	0.124***	0.118***	0.154***	0.193***	0.129***
	(0.007)	(0.012)	(0.012)	(0.023)	(0.025)	(0.018)
Naturalization year	0.137***	0.130***	0.124***	0.151***	0.150***	0.109***
	(0.008)	(0.014)	(0.014)	(0.026)	(0.028)	(0.020)
Year after	0.110***	0.110***	0.106***	0.073*	0.086**	0.062**
	(0.009)	(0.015)	(0.015)	(0.029)	(0.032)	(0.022)
Two years after	0.102***	0.094***	0.108***	0.015	0.070*	0.054*
	(0.010)	(0.017)	(0.017)	(0.032)	(0.035)	(0.024)
Three years after	0.104***	0.089***	0.102***	0.005	0.085*	0.080**
	(0.011)	(0.019)	(0.018)	(0.035)	(0.038)	(0.026)
Four years+	0.113***	0.089***	0.117***	0.002	0.038	0.089**
	(0.012)	(0.021)	(0.020)	(0.038)	(0.041)	(0.027)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 6.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

5.2 Childbearing and Marriage Propensities

One of the implications of being a citizen is that newborn children automatically become nationals. The timing of when children are born is thus of relevance in this context. To this end it is natural to focus on the more dynamic models. Similarly, since marriages with a citizens shortens the waiting period before one can apply for citizenship the model that allows

for changes prior to naturalizations is preferable. Hence, hereafter only estimates based on the more flexible model are presented.[20]

Table 8. Effect of naturalizations on having a child

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Two years before	0.003**	-0.001	0.008***	-0.007*	-0.002	-0.002
	(0.001)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
One year before	0.003*	-0.001	0.011***	-0.008	-0.004	0.003
	(0.001)	(0.002)	(0.002)	(0.004)	(0.003)	(0.004)
Naturalization year	0.002	-0.000	0.012***	-0.009	-0.004	0.005
	(0.001)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)
Year after	0.002	0.001	0.012***	-0.011	-0.001	0.007
	(0.002)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)
Two years after	0.002	0.001	0.013***	-0.010	0.000	0.009
	(0.002)	(0.003)	(0.003)	(0.007)	(0.005)	(0.005)
Three years after	0.001	0.001	0.012***	-0.004	0.002	0.013*
	(0.002)	(0.003)	(0.003)	(0.008)	(0.006)	(0.005)
Four years+	-0.000	0.000	0.013***	0.005	0.003	0.016*
	(0.002)	(0.004)	(0.004)	(0.009)	(0.007)	(0.006)
Men						
Two years before	0.005***	0.003	0.007***	0.004	0.007	-0.003
	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)	(0.004)
One year before	0.006***	0.007**	0.007**	0.010*	0.005	0.004
	(0.001)	(0.002)	(0.002)	(0.005)	(0.006)	(0.004)
Naturalization year	0.006***	0.008**	0.007*	0.009	0.006	0.006
	(0.002)	(0.003)	(0.003)	(0.006)	(0.007)	(0.005)
Year after	0.007***	0.009**	0.008**	0.009	0.004	0.012*
	(0.002)	(0.003)	(0.003)	(0.007)	(0.008)	(0.005)
Two years after	0.009***	0.013**	0.010**	0.015*	0.013	0.012*
	(0.002)	(0.004)	(0.003)	(0.007)	(0.009)	(0.006)
Three years after	0.012***	0.014**	0.014***	0.018*	0.022*	0.016*
	(0.002)	(0.004)	(0.004)	(0.008)	(0.009)	(0.006)
Four years+	0.015***	0.019***	0.017***	0.033***	0.033**	0.021**
	(0.003)	(0.005)	(0.004)	(0.009)	(0.010)	(0.008)

Notes: The outcome is defined in levels, i.e. it is set to one if a new child is observed within the household. Standard errors clustered within individuals in parentheses. See also note Table 4.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 8 shows that, on average, women are slightly more likely to have children the years prior to naturalizations. The time pattern of when

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[20] The more restrictive models are found in the appendix, Tables B5-6.

naturalizing migrants have children however varies substantially between different country groups. The effect appears to be driven by women from the Middle East and North Africa. This group is also more likely to have children the years following naturalizations. For the Balkans and Eastern Europe and Asia no effects are found. Further, the point estimates indicate that the association is negative but not statistically significant for women from Sub-Saharan Africa.

For men the association between childbearing and naturalizations is stronger. The association is significant for all groups, although the strength varies between the groups. For some groups the correlation is observed already prior to naturalizations. On average, it however, grows stronger the years following naturalizations.

Table 9 contains results on the correlation between marriages and naturalizations. It, once again, demonstrates that the more restrictive model is not supported by the data as changes in the propensity to get married occur prior to naturalizations. For women from all regions, except for Latin America, the likelihood of getting married increases the years prior to naturalizations. On average the increase correspond to around 1 percentage point. Further, the correlation remains positive the following years for all groups except for women from the Middle East and North Africa.

For men the same pattern is found, i.e. marriage propensities increase the years surrounding naturalizations.

Altogether, this section provides some evidence of decisions regarding childbearing and marriages being related to the naturalization process. The estimates however do not provide a clear-cut picture. For migrant women, on average, naturalizations appear to be associated with slight increase in the probability of becoming married. Further, for men, on average, there is evidence of naturalizations being associated with having children and weak evidence of an increase in the likelihood of getting married. For both groups there is large variation across the country groups.

Before closing this section it is also worth noting that the results presented here illustrate that one of the identifying assumptions for unbiased effects on labor market outcomes potentially is violated. That is, from the analysis it is not possible to rule out whether naturalizations cause the above outcomes to change or whether they are driven by other factors. As discussed the long-term plan to stay in the country could potentially have an effect on both the decision to apply for naturalizations as well as decisions regarding the family. Also other types of shocks (observable or unobservable) that are associated with the naturalization process could be of relevance. This illustrates that identifying the causal effect of naturalizations on labor market outcomes is not an easy task.

Table 9. Effect of naturalizations on becoming married

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Two years before	0.005***	0.007***	0.004**	0.010***	0.007**	-0.002
	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)
One year before	0.008***	0.009***	0.005**	0.015***	0.012***	0.003
	(0.001)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
Naturalization year	0.008***	0.011***	0.003	0.019***	0.016***	0.007
	(0.001)	(0.002)	(0.002)	(0.005)	(0.004)	(0.004)
Year after	0.008***	0.011***	0.002	0.024***	0.018***	0.008
	(0.001)	(0.002)	(0.002)	(0.005)	(0.004)	(0.004)
Two years after	0.008***	0.012***	0.002	0.029***	0.018***	0.010*
	(0.002)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)
Three years after	0.006**	0.010***	0.001	0.031***	0.019***	0.009
	(0.002)	(0.003)	(0.003)	(0.007)	(0.005)	(0.005)
Four years+	0.003	0.007	0.000	0.030***	0.021***	0.021***
	(0.002)	(0.004)	(0.003)	(0.008)	(0.006)	(0.006)
Men						
Two years before	0.005***	0.009***	0.002	0.002	0.014**	0.007*
	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)
One year before	0.003*	0.007***	-0.002	0.007	0.013*	0.009*
	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)	(0.004)
Naturalization year	0.000	0.005*	-0.007**	0.010	0.014*	0.015**
	(0.002)	(0.002)	(0.002)	(0.005)	(0.006)	(0.005)
Year after	0.003	0.005	-0.005	0.020**	0.026***	0.023***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.007)	(0.005)
Two years after	0.005*	0.005	-0.004	0.029***	0.037***	0.027***
	(0.002)	(0.003)	(0.003)	(0.007)	(0.008)	(0.006)
Three years after	0.005*	0.004	-0.005	0.037***	0.045***	0.029***
	(0.002)	(0.004)	(0.004)	(0.008)	(0.009)	(0.006)
Four years+	0.003	-0.000	-0.005	0.050***	0.058***	0.032***
	(0.003)	(0.004)	(0.004)	(0.009)	(0.010)	(0.007)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 4.
* p<0.05, ** p<0.01, *** p<0.001

6. Conclusions

In the light of declining rates of economic assimilation of migrants in most Western countries, the observation that naturalizations are associated with improving labor market outcomes in some countries is interesting. Key for policy is the perceived value of citizenship. This closely relates to the benefits associated with naturalizations, including the potential of improved labor market conditions following naturalizations studied in this paper.

One indication of the value of citizenship is the high share of migrants that naturalizes. In this light, one line of argument could be that changing the requirements for naturalizations could be an effective tool for a faster integration of the foreign-born population.[21] For example, the introduction of a language requirement could be a way of stimulating a type of human capital investment that is awarded on the labor market. The potential benefits should, of course, be weighed against the potential costs of tougher requirements. Apart from the formal status that citizenship gives, acquiring citizenship is also likely to affect the subjective identity – how you are – and social relations in society. Thus, making it harder to qualify for citizenship is likely to change the potential pool of applicants. This can cause negative sentiments among groups that know that it is hard or even impossible to fulfill the requirements. For example, it could have negative effects on the sense of closeness to the majority society.

The analysis demonstrates that it is difficult to establish whether citizenship acquisition in Sweden has a causal effect on labor market outcomes. This makes policy recommendations less straightforward. Nevertheless, we observe that the acquisition of Swedish citizenship is followed by improved labor market outcomes for some country groups. On average, the chance of finding a job rises and annual earnings from labor increases. Regarding earnings the impact is larger for low levels of income. In general, these findings are in line with the results in studies from Germany, the US and France (Bratsberg et al. 2002, Steinhardt 2012, Fougère and Safi 2009).

The evidence presented in this paper however demonstrates that the labor market outcomes start to improve already prior to naturalizations, which is consistent with previous findings from Sweden (Ohlsson 2008) and to some extent Norway (Bratsberg and Raaum 2011). Nonetheless, one difference between my findings and the findings in Bratsberg and Raaum is that there is at most weak evidence of a positive correlation between naturalizations and the economic performance of immigrants in Norway. At first this might seem strange, given the institutional similarities between the two countries. On the other hand differences do exist, for example, the waiting time before it is possible to apply for citizenship is longer in Norway. The discussion

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[21] This is in line with the arguments presented in SOU (2013).

necessarily becomes speculative but illustrates that e.g. institutional differences could be of importance. Another factor that potentially matters is the selection of immigrants to the respective countries that, to some extent, differs in terms of country of origin.

Further, the Swedish Citizenship legislation follows the ius sanguinis tradition. Accordingly, as children of citizens automatically become citizens, I hypothesized that this might create incentives to postpone childbearing decisions until after naturalizations. Similarly, marriages with citizens shorten the waiting period before one can become a citizen. This potentially affects the timing of marriages. No clear evidence is found to support these hypotheses. This might not be surprising as it is possible for parents and children to naturalize at a later point in time and because the waiting time before it is possible to apply for citizenship in the Swedish context is relatively short. The analysis does however demonstrate that marriage propensities on average, and childbearing decisions (at least for men), are more common in the years surrounding naturalizations in comparison to the period preceding this window. Hence, these results are suggestive of naturalization decisions being partly determined by family reasons.

All in all, the findings of this study indicate that naturalizations potentially are associated with several different outcomes. Thus, a focus on labor market outcomes alone will only tell part of the story as decisions regarding citizenship acquisitions are likely to be correlated with a number of separate outcomes. Lastly, the paper also clearly illustrates that modeling assumptions are of great importance. Models that are not flexible enough could lead to false claims regarding causality.

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Appendix A. Country Groups

Table A1. Country groups

Variable	Definition
Balkans and Eastern Europe	Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, former Soviet Union, Georgia, Kazakhstan, Kyrgyzstan, Moldova, Republic of Macedonia, Romania, Russia, Serbia, Montenegro, Slovenia, Tadzhikistan, Turkmenistan, Ukraine and Uzbekistan.
Middle East and North Africa	Algeria, Bahrain, Cyprus, Egypt, Gaza, Iran, Iraq, Israel, Jordan, Kuwait, Lebanon, Libya, Morocco, Palestine, Qatar, Saudi Arabia, Syria, Tunisia, Turkey, United Arab Emirates and Yemen.
Sub-Saharan Africa	Angola, Benin, Botswana, Burkina Faso, Burundi, Cameron, Cap Verde, Central African Republic, Chad, Comoros, Democratic Republic of the Congo, Djibouti, Equatorial Guinea, Eritrea, Ethiopia, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Ivory Coast, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauretania, Mauritius, Mozambique, Namibia, Niger, Nigeria, Republic of the Congo, Rwanda, Sao Tome and Principe, Senegal, Seychelles, Sierra Leone, Somalia, South Africa, Sudan, Swaziland, Tanzania, Togo, Uganda, Zambia, Zanzibar and Zimbabwe.
Asia	Afghanistan, Bangladesh, Bhutan, Brunei, Burma, Cambodia, China, Hong Kong, India, Indonesia, Japan, Laos, Malaysia, Maldives, Mongolia, Nepal, North Korea, Oman, Pakistan, Philippines, Singapore, South Korea, Sri Lanka, Thailand and Vietnam.
Latin America	Antigua and Barbuda, Argentina, Bahamas, Barbados, Belize, Bolivia, Brazil, Chile, Colombia, Costa Rica, Cuba, Dominica, Dominican Republic, Ecuador, El Salvador, Grenada, Guatemala, Guayana, Haiti, Honduras, Jamica, Mexico, Nicaragua, Panama, S:t Lucia, Paraguay, Peru, S:T Kitt and Nevis and Anguil, S:T Vincent, Surinam, Trinidad and Tobago, Uruguay and Venezuela.

Appendix B. Additional Results

Table B1. Effect of naturalizations on employment (defined as three basic amounts)

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
I: Common ysm profile	0.057***	0.083***	0.048***	0.095***	0.045***	0.093***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.005)	(0.005)
II: Separate ysm profile	0.042***	0.053***	0.040***	0.083***	0.030***	0.051***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.005)	(0.006)
III: Separate ysm profile and individual fixed effects	0.015***	0.026***	0.010***	0.023***	0.006	0.009
	(0.001)	(0.003)	(0.002)	(0.005)	(0.004)	(0.005)
Men	**All**					
I: Common ysm profile	0.104***	0.145***	0.106***	0.148***	0.113***	0.122***
	(0.002)	(0.004)	(0.002)	(0.006)	(0.008)	(0.006)
II: Separate ysm profile	0.048***	0.069***	0.069***	0.069***	0.055***	0.022**
	(0.002)	(0.004)	(0.003)	(0.006)	(0.008)	(0.007)
III: Separate ysm profile and individual fixed effects	0.023***	0.029***	0.026***	0.011*	0.009	-0.000
	(0.002)	(0.003)	(0.002)	(0.006)	(0.007)	(0.006)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 4.
* p<0.05, ** p<0.01, *** p<0.001

Table B2. Effect of naturalizations on employment (defined as three basic amounts)

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Two years before	0.007***	0.018***	0.003	0.007	0.005	0.025***
	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)	(0.005)
One year before	0.019***	0.037***	0.011***	0.026***	0.013**	0.040***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.005)	(0.006)
Naturalization year	0.025***	0.049***	0.015***	0.036***	0.019***	0.034***
	(0.002)	(0.004)	(0.003)	(0.006)	(0.006)	(0.007)
Year after	0.024***	0.048***	0.016***	0.036***	0.013*	0.028***
	(0.002)	(0.004)	(0.004)	(0.007)	(0.006)	(0.007)
Two years after	0.026***	0.048***	0.020***	0.035***	0.006	0.029***
	(0.003)	(0.005)	(0.004)	(0.008)	(0.007)	(0.008)
Three years after	0.029***	0.048***	0.025***	0.044***	0.003	0.027**
	(0.003)	(0.006)	(0.005)	(0.009)	(0.008)	(0.009)
Four years+	0.038***	0.050***	0.036***	0.059***	0.012	0.025**
	(0.003)	(0.006)	(0.005)	(0.010)	(0.009)	(0.009)
Men						
Two years before	0.030***	0.038***	0.021***	0.041***	0.053***	0.038***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.006)	(0.006)
One year before	0.050***	0.065***	0.038***	0.069***	0.073***	0.048***
	(0.002)	(0.004)	(0.003)	(0.006)	(0.007)	(0.007)
Naturalization year	0.055***	0.073***	0.045***	0.068***	0.064***	0.044***
	(0.002)	(0.004)	(0.003)	(0.007)	(0.008)	(0.007)
Year after	0.048***	0.065***	0.045***	0.046***	0.050***	0.023**
	(0.003)	(0.005)	(0.003)	(0.008)	(0.009)	(0.008)
Two years after	0.046***	0.061***	0.046***	0.023*	0.043***	0.017
	(0.003)	(0.005)	(0.004)	(0.009)	(0.010)	(0.009)
Three years after	0.047***	0.059***	0.045***	0.023*	0.045***	0.010
	(0.003)	(0.006)	(0.004)	(0.010)	(0.011)	(0.010)
Four years+	0.053***	0.065***	0.053***	0.020	0.031**	0.002
	(0.003)	(0.007)	(0.005)	(0.011)	(0.012)	(0.010)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 4.
* p<0.05, ** p<0.01, *** p<0.001

Table B3. Effect of naturalizations on annual earnings conditional on employment (defined as three basic amounts)

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
I: Common ysm profile	0.020***	0.021***	0.023***	-0.011	0.009*	0.033***
	(0.002)	(0.004)	(0.005)	(0.007)	(0.005)	(0.005)
II: Separate ysm profile	0.026***	0.021***	0.041***	0.009	0.002	0.037***
	(0.002)	(0.004)	(0.004)	(0.007)	(0.005)	(0.006)
III: Separate ysm profile and individual fixed effects	0.010***	0.013***	-0.003	0.017**	0.008*	0.008
	(0.002)	(0.003)	(0.004)	(0.006)	(0.004)	(0.004)
Men						
I: Common ysm profile	0.029***	0.025***	0.072***	0.023***	-0.028**	0.054***
	(0.002)	(0.003)	(0.004)	(0.007)	(0.009)	(0.005)
II: Separate ysm profile	0.030***	0.013***	0.069***	0.028***	-0.012	0.042***
	(0.002)	(0.003)	(0.004)	(0.007)	(0.010)	(0.006)
III: Separate ysm profile and individual fixed effects	0.010***	0.011***	0.006*	0.011*	0.015*	0.006
	(0.002)	(0.002)	(0.003)	(0.005)	(0.006)	(0.004)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 6.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table B4. Effect of naturalizations on annual earnings conditional on employment (defined as 3 basic amounts)

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
Two years before	0.001	0.001	-0.013*	0.014	0.003	-0.004
	(0.002)	(0.004)	(0.005)	(0.007)	(0.005)	(0.005)
One year before	0.004	0.007	-0.018**	0.014	0.012*	-0.005
	(0.002)	(0.004)	(0.006)	(0.008)	(0.005)	(0.005)
Naturalization year	0.011***	0.014**	-0.017**	0.023*	0.019**	0.007
	(0.003)	(0.005)	(0.006)	(0.009)	(0.006)	(0.006)
Year after	0.013***	0.018***	-0.014*	0.028**	0.007	0.011
	(0.003)	(0.005)	(0.007)	(0.010)	(0.007)	(0.006)
Two years after	0.014***	0.018***	-0.012	0.033**	0.015	0.001
	(0.003)	(0.006)	(0.007)	(0.011)	(0.008)	(0.007)
Three years after	0.015***	0.019**	-0.009	0.029*	0.013	-0.000
	(0.004)	(0.006)	(0.008)	(0.012)	(0.008)	(0.007)
Four years+	0.018***	0.017**	-0.003	0.028*	0.011	0.005
	(0.004)	(0.007)	(0.009)	(0.013)	(0.010)	(0.008)
Men						
Two years before	0.006**	0.006*	-0.007	0.011	0.022**	0.003
	(0.002)	(0.003)	(0.004)	(0.006)	(0.007)	(0.005)
One year before	0.015***	0.014***	-0.001	0.024***	0.036***	0.015**
	(0.002)	(0.003)	(0.004)	(0.007)	(0.008)	(0.005)
Naturalization year	0.018***	0.019***	0.001	0.025***	0.033***	0.016**
	(0.002)	(0.004)	(0.005)	(0.008)	(0.009)	(0.006)
Year after	0.018***	0.020***	0.002	0.026**	0.033**	0.013*
	(0.003)	(0.004)	(0.005)	(0.009)	(0.010)	(0.006)
Two years after	0.022***	0.020***	0.011	0.025**	0.049***	0.006
	(0.003)	(0.005)	(0.006)	(0.009)	(0.011)	(0.007)
Three years after	0.020***	0.017**	0.009	0.020	0.046***	0.011
	(0.003)	(0.005)	(0.006)	(0.010)	(0.012)	(0.007)
Four years+	0.023***	0.014*	0.015*	0.018	0.040**	0.027***
	(0.004)	(0.006)	(0.007)	(0.011)	(0.013)	(0.008)

Notes: Standard errors clustered within individuals in parentheses. See also note Table 6.
* p<0.05, ** p<0.01, *** p<0.001

Table B5. Effect of naturalizations on having a child

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
I: Common ysm profile	0.039***	0.011***	0.059***	0.050***	0.054***	0.046***
	(0.002)	(0.003)	(0.003)	(0.007)	(0.006)	(0.006)
II: Separate ysm profile	0.017***	0.003	0.045***	-0.000	0.021***	0.041***
	(0.002)	(0.003)	(0.003)	(0.007)	(0.006)	(0.007)
III: Separate ysm profile and individual fixed effects	0.000	0.000	0.005**	-0.007	-0.002	0.006
	(0.001)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
Men						
I: Common ysm profile	0.082***	0.041***	0.089***	0.113***	0.112***	0.033***
	(0.002)	(0.004)	(0.003)	(0.007)	(0.009)	(0.007)
II: Separate ysm profile	0.044***	0.024***	0.051***	0.047***	0.022*	0.036***
	(0.002)	(0.004)	(0.003)	(0.007)	(0.010)	(0.008)
III: Separate ysm profile and individual fixed effects	0.003*	0.004*	0.003	0.003	-0.002	0.008*
	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)	(0.004)

Notes: The outcome is defined in levels, i.e. it is set to one if a new child is observed within the household. Standard errors clustered within individuals in parentheses. See also note Table 4.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

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Table B6. Effect of naturalizations on becoming married

Women	All	Balkans & Eastern Europe	Middle East & North Africa	Sub-Saharan Africa	Asia	Latin America
I: Common ysm profile	0.008**	0.005	0.015***	0.029***	0.023***	0.050***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.005)	(0.006)
II: Separate ysm profile	0.017***	0.005	0.017***	0.029***	0.016**	0.056***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)
III: Separate ysm profile and individual fixed effects	0.003**	0.005***	-0.002	0.012***	0.009**	0.008*
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)
Men						
I: Common ysm profile	0.056***	0.002	0.060***	0.091***	0.101***	0.042***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.008)	(0.006)
II: Separate ysm profile	0.041***	0.000	0.043***	0.078***	0.061***	0.038***
	(0.002)	(0.003)	(0.003)	(0.007)	(0.009)	(0.006)
III: Separate ysm profile and individual fixed effects	-0.002	-0.003*	-0.008***	0.011**	0.009	0.014***
	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)	(0.004)

Notes: The outcome is defined in levels, i.e. it is set to one if an individual becomes married. Standard errors clustered within individuals in parentheses. See also note Table 4.
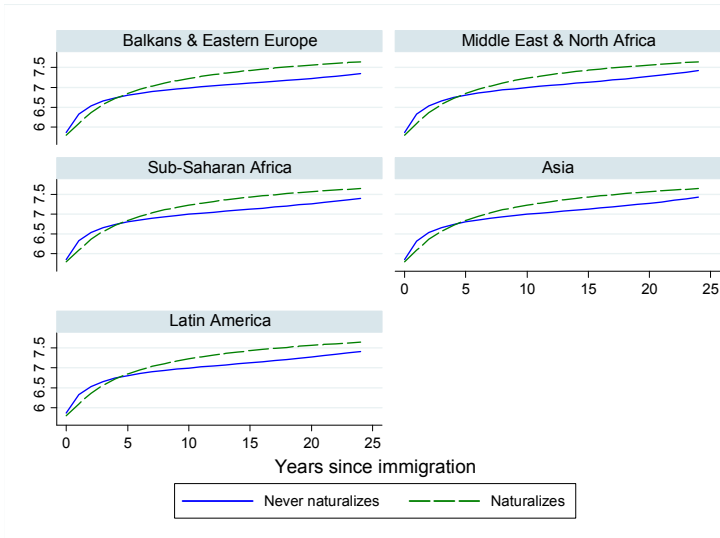* $p<0.05$, ** $p<0.01$, *** $p<0.001$

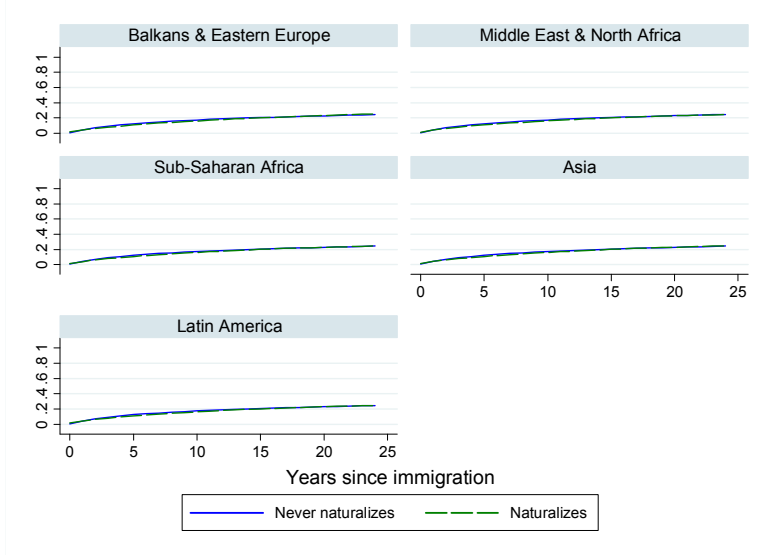# Appendix C. Figures

Figure C1. Annual earnings, women
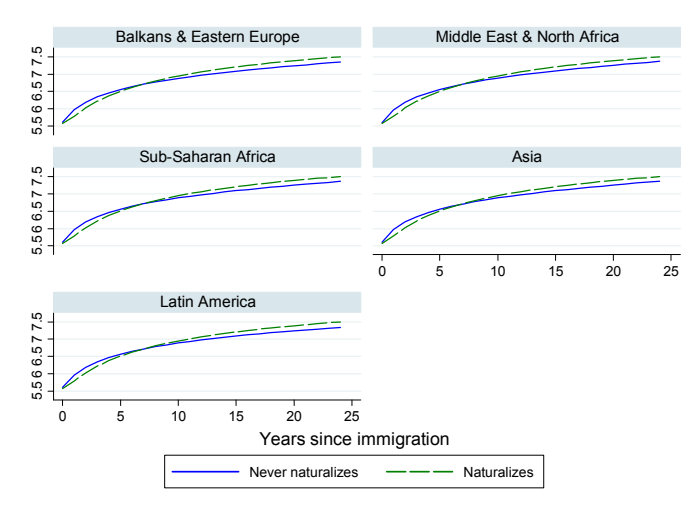
Notes: Outcome defined in logs, see also note Figure 2.
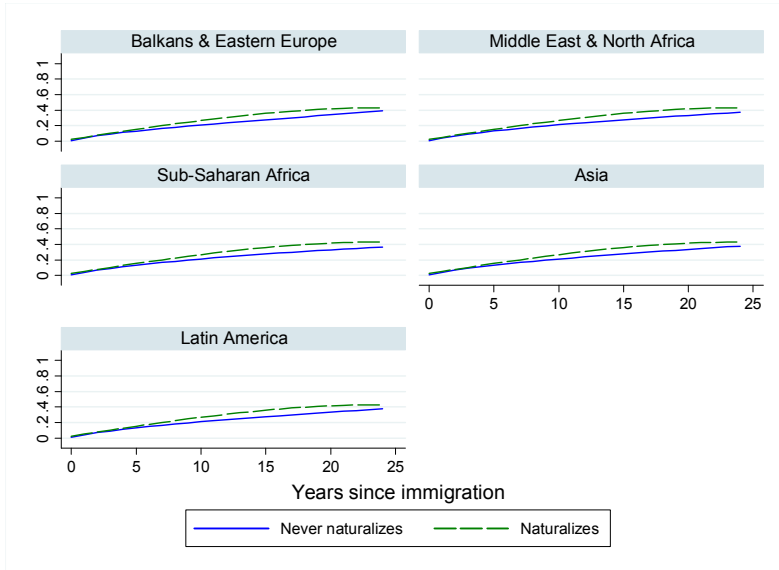
Figure C2. Annual earnings, men

Notes: Outcome defined in logs, see also note Figure 2.

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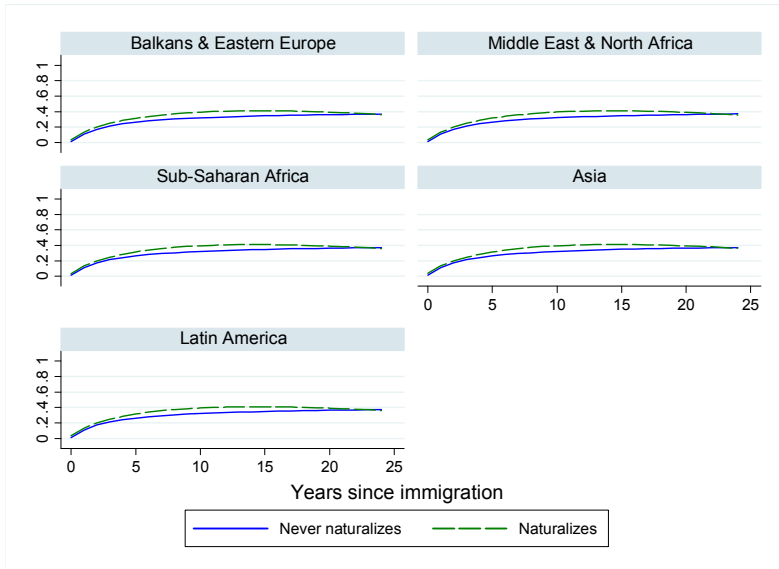## Figure C3. Getting married (after immigration), women

Notes: Outcome defined in levels, i.e. it is set to one if a woman gets married, see also note Figure 2.

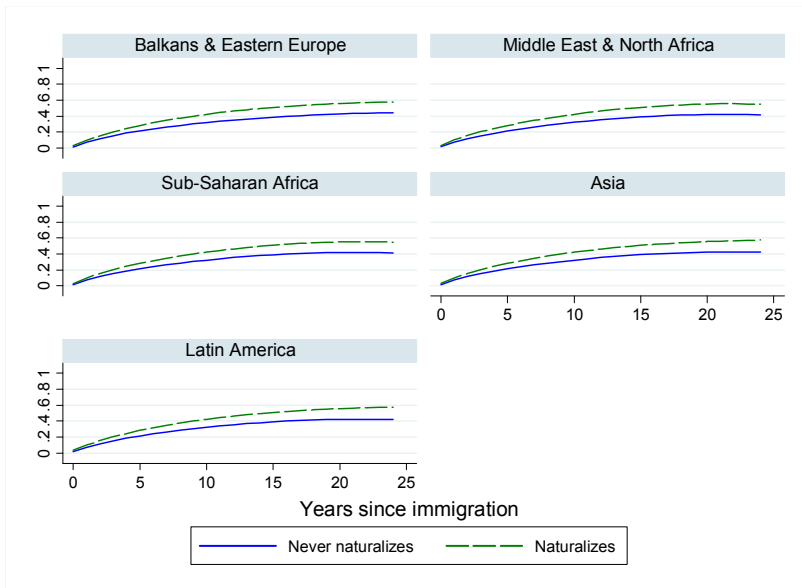## Figure C4. Getting married (after immigration), men

Notes: Outcome defined in levels, i.e. it is set to one if a man gets married, see also note Figure 2.

Figure C5. Having a child (after immigration), women

Notes: Outcome defined in levels, i.e. it is set to one if a new child is observed in the household, see also note Figure 2.

Figure C6. Having a child (after immigration), men

Notes: Outcome defined in levels, i.e. it is set to one if a new child is observed in the household, see also note Figure 2.