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Immigration, Work, and Welfare

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Table of contents

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Summary of results

The first five chapters of this thesis deal with the effects of immigration on the labor market outcomes of previous residents of a country, i.e., natives and previous immigrants. Chapter 1 estimates the effects of Swedish refugee immigration on previous residents' unemployment rates. No effects can be identified for natives or previous immigrants from high-income countries. Yet large effects are identified for previous immigrants from low- and middle-income countries.

Chapter 2 shows that previous influential results concerning limited substitutability between natives and immigrants on the labor market disappear when the composition of the immigrant group is controlled for.

Chapter 3 picks up the recent hypothesis that the impact of immigration on the types of jobs that are performed by natives is limited in sectors of the economy (i.e., manufacturing) where offshoring is important, as immigrant workers would then rather be substitutes for offshore workers than for native workers. The analysis finds empirical support for more immigration causing natives to perform more communication-intensive jobs also in the manufacturing sector.

A combined conclusion from the first three chapters is that more attention ought to be paid to migrants' origin in the literature on the labor market impact of immigration. Migrants of different origins – even at similar education levels – perform very differently on the labor market. Hence we should expect them to also affect other workers differently. The empirical results in these chapters confirm that accounting for migrant origin may substantially increase the clarity of the results obtained.

In Chapter 4 I construct a new structural model of the US economy that builds on insights from the extensive literature on individual returns to education. This model is used to simulate the impact of the last 20-30 years of US immigration on the structure of wages. In contrast to previous similar studies, I do not find that high school dropouts have been main losers from immigration; possibly they have even been large winners.

Chapter 5 provides a structural model of the average wage effect of immigration when the accumulation rate and international distribution of the capital stock are assumed to adjust smoothly to international migration.

Chapter 6 deals with the hypothesis that large differences in migrants' estimated returns to education between countries of origin are due to large differences in education quality between countries. The results indicate that these differences are instead mainly due to differences in how migrants are selected on unobserved abilities depending on differences in migration costs.

Chapter 7 analyzes the link between the recently worsening macroeconomic conditions and increasing public resistance to immigration in Europe. It finds a significant link, i.e., resistance to immigration has increased more in the countries with the worst economic trends and especially in the countries with the largest increases in public debt.

Chapter 8 analyzes the net fiscal contributions of post-EU-enlargement immigrants from the new member states in Sweden and finds that these contributions are marginally positive. The results are mainly interesting because Sweden was the only country that did not initially limit these migrants' access neither to the labor market nor to social security benefits. Hence the results indicate that fears of "welfare tourism" may have been exaggerated.

Abstracts

Chapter 1: The labor market impact of refugee immigration in Sweden 1999-2007

This study estimates labor market effects of refugee immigration in Sweden 1999–2007. The setting is particularly suitable for using spatial variation within the country to estimate labor market effects of immigration. Bias from endogenous immigrant settlement is likely to be smaller when estimating the effect of only refugee immigration. Bias from internal migration of previous inhabitants is eliminated by using data where individuals can be observed before the immigrant inflow happens. No significant effect of refugee immigration on total unemployment is found, but there is a large effect on the unemployment of previous immigrants from low- and middle-income countries. This indicates that newly arrived refugee immigrants are more easily substituted for this group than for natives in production.

Chapter 2: Immigrant-native wage gaps in time-series: Complementarities or composition effects?

This study investigates the role of immigrant composition in creating the observed negative correlation between immigrant supply and immigrant wages in US time series, which has recently been interpreted as evidence of immigrant-native complementarities in production. The main finding is that compositional effects fully explain this empirical pattern. Newly-arrived immigrants, notably Latin Americans, earn less than previous immigrants. Hence their presence decreases average immigrant wages, mechanically. Controlling for this, no negative relation between immigrant supply and immigrant wages remains. More generally, the findings highlight problems in structural estimation of wage effects of immigration, and also the need to distinguish between effects of immigrants of different origins.

Chapter 3: Native and immigrant comparative advantage in manufacturing

This study investigates how immigration affects the types of manufacturing jobs that are performed by natives. Previous research has shown that immigration induces natives to specialize in more communication-intensive jobs, yet that this is not so in the manufacturing sector, possibly due to immigration being primarily a substitute for offshoring in this sector. Using geographical variation in immigrant shares of the workforce by industry the present study finds that immigration does induce native specialization in – and increasing returns to – communication-intensive jobs also in manufacturing. These effects are driven by Latin American immigration. Latin Americans are the only immigrants that specialize in less communication-intensive manufacturing jobs than natives. Like natives, non-Latin American immigrants also specialize further in communication-intensive jobs when the presence of Latin American immigrants is higher.

Chapter 4: Immigration, education, and earnings

I simulate relative wage effects of immigration in a partial equilibrium framework derived directly from a human capital earnings function. While previous literature has found immigration to substantially decrease the wages of the least educated workers, I find that immigration either has had a neutral effect on wages, or has favored the least skilled workers, possibly by several percent. The supply-side predictor used in the model has significant power to explain historical trends in relative wages since the 1960s, whereas predictors of the kind used in previous literature have no significant explanatory power when the two are used simultaneously.

Chapter 5: Average wage effects of immigration with endogenous saving and international capital mobility

This study estimates average wage effects of immigration, accounting for the interplay between factor supply and factor prices. While immigration decreases native wages, it increases capital returns. Hence both domestic saving and capital inflows from abroad and in turn foreign saving increase. This in turn eliminates some of the wage effect. Yet there is a negative effect of immigration on average wages as long as immigration persists. At the current US immigration rate, this effect is estimated around 0.9%, i.e., a substantially smaller effect than in previous analyses treating capital supply as inelastic. However, total gains of immigration are substantially larger than losses to native workers. A tax reform is considered where immigrant workers would compensate native workers for their losses. The required tax surcharges for immigrants are found to be around 10% of gross income for between 20 and 30 years. Although these surcharges are quite large, most migrants who pay them would still make substantial income gains from migration.

Chapter 6: Why is the payoff to education smaller for some migrants than others? The role of migrant selection

Previous literature has estimated large differences in migrants' returns to education depending on their countries of origin. These differences have often been interpreted as evidence of large differences in education quality between their home countries, with implications for analyses of global income differences and for development policy. The present study investigates if and how the observed patterns could instead have been generated by migrant selection on unobserved abilities depending on migration costs. It finds that migrant selection can account for at least two-thirds of the observed variation between 19 main countries and regions of US immigrant origin. Indicators for education quality do not explain any of the remaining variation.

Chapter 7: The macroeconomic factors behind rising public resistance to immigration in Europe

This article shows that the recent increase in public resistance to immigration in Europe has been importantly shaped by the deteriorating macroeconomic context. While the link between economic circumstances and resistance to immigration may appear obvious, previous cross-national research has generally not been able to identify it. The main advantage of the present study in this respect is in its use of within-country instead of cross-country variation. Also in contrast to previous research, perceived economic threat is found to be substantially more important than perceived cultural threat in explaining resistance to immigration. And perceived cultural threat is itself found to be significantly influenced by the macroeconomic context.

Chapter 8: Free immigration and welfare access: The Swedish experience

With the expansion of the European Union from 15 to 25 member countries in 2004, fears of migrants' excessive welfare use lead 14 of the 15 older member countries to impose restrictions on access of citizens of the new member countries – the A10 countries – to their welfare systems. Sweden was the only exception. This paper evaluates the net contributions of post-enlargement A10 immigrants on Swedish public finances in 2007. A10 immigrants make a small but significant positive net contribution. On average, they generate less public revenue than the population on average, but also cost less, in the end yielding a positive net result. A10 immigrants do not benefit more from basic social welfare than the population on average.

Paper I

The labor market impact of refugee immigration in Sweden 1999-2007

Abstract

This study estimates labor market effects of refugee immigration in Sweden 1999–2007. The setting is particularly suitable for using spatial variation within the country to estimate labor market effects of immigration. Bias from endogenous immigrant settlement is likely to be smaller when estimating the effect of only refugee immigration. Bias from internal migration of previous inhabitants is eliminated by using data where individuals can be observed before the immigrant inflow happens. No significant effect of refugee immigration on total unemployment is found, but there is a large effect on the unemployment of previous immigrants from low- and middle-income countries. This indicates that newly arrived refugee immigrants are more easily substituted for this group than for natives in production.

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1 – Introduction

This study exploits within-country spatial variation in immigrant inflows and labor market outcomes to estimate the labor market impact of Sweden's large refugee immigration 1999-2007. The primary reason for analyzing the impact of refugee immigration only is the push-driven nature of these individuals' migration decisions. A main problem with using spatial variation to make inference about the impact of immigration is otherwise that endogenous immigrant settlements with respect to labor market opportunities may bias the estimates in a positive direction (Borjas, 2003). Yet this should be less of an issue when analyzing refugee immigration. In contrast to other migrants refugees do not migrate to find work and hence their settlement decisions in the immigration country should be less sensitive to labor market opportunities compared with other migrants. This prediction is also supported empirically in this study.

The focus on the impact of a narrower group of immigrants is also motivated by the fact that immigrants of different origin and different reasons for migration perform very differently on the labor market (Borjas, 1995; Bratsberg, Barth, and Raaum, 2006). Hence we should also expect them to affect other workers differently. However, while several influential studies of the labor market effects of immigration (Borjas, 2003; Card, 2005, 2009; Ottaviano and Peri, 2012) analyze differential effects of immigration depending on the immigrants' educational attainments, differential effects depending on immigrant origin and reason for migration have received little attention in previous literature. In the Swedish case refugees are a large immigrant group, making up one-fourth of all immigrants who arrived in 1998-2007 and remained in the country in 2007.

The data set used in the present study covers all Swedish working-age individuals 1998-2007. It has the important advantage that individuals are followed over time. This implies that the empirical analysis can be made insensitive to bias from relocation of resident workers, which is normally a potential problem in similar studies (Borjas, 2003). If an immigrant inflow is offset by an outflow (or non-inflow) of resident workers any estimated impact of immigration will be biased towards zero. In the present case such bias is eliminated by observing the locations of resident individuals before the immigrant inflow happens, i.e., before they have any possibility to observe and react to the inflow.

Labor market effects of immigration are measured as effects on unemployment rates. This choice of outcome variable is obvious due to the highly centralized nature of Swedish wage-setting, which implies that short-term responses to any kind of shocks are forced to happen primarily as changes in unemployment rates rather than wages. The total time interval covered by the study is nine years long, i.e., plausibly too long to measure short term effects. The interval is therefore divided in two. The middle point in time is set to 2003 rather than 2002, as this makes the refugee inflows of the two sub-periods more equal. Further division of the time interval is avoided to still allow sufficient time for the refugee inflow to potentially affect the labor market, remembering that refugee immigrants are on average quite slow to enter the labor market.

Unemployment rate effects of refugee immigration are measured on all resident workers, as well as separately on more narrow subgroups that are expected to be different in terms of substitutability for the refugees. In rising order of expected substitutability these are native workers, previous immigrants from high-income countries, and previous immigrants from low- and middle-income countries. Ideally, the last group should have been divided further between refugees and non-refugees, yet this is impossible since refugee status is not appropriately coded for immigrants arriving in Sweden earlier than 1997.

The empirical analysis identifies no impact of refugee immigration on the total unemployment rate or on the unemployment rates of natives or high-income country immigrants. Yet there is a large and significant effect on previous low- and middle-income country immigrants, supporting the expectation that the newly arrived refugees are most easily substituted for this group in production.

2 – Swedish refugee immigration

By now Sweden has a long tradition of relatively generous refugee immigration policies and of large refugee immigration. During the first decade of the new century it has been particularly large, compared with both historical Swedish numbers and numbers for other countries during the same period. Comparable data that would enable exact comparisons of numbers of actual refugee immigrants between countries do not exist. However the total number of *asylum applications* submitted in Sweden – which should be a reasonable proxy – is very similar to the numbers in the important European immigration countries Germany, France, and the UK, whose total populations are 7–10 times that of Sweden. If the numbers of applications are divided by home country populations only the small Mediterranean islands of Cyprus and Malta, which in many cases act as migrant gateways to other parts of the European Union, received as many applications as Sweden or more in the industrialized world (UNHCR, various years).

The present study focuses on refugee immigration 1999–2007, with data availability limiting the total length of the period. The data set used is from the STATIV database of Statistics Sweden, and contains all individuals registered as present in Sweden and 25-64 years old in all three years 1998, 2003, and 2007. Information on immigrants includes source country, year of immigration, and reason for immigration, including refugee status. The definition of a refugee includes relatives of refugees, as well as other immigrants with "refugee-like" reasons for immigration, as defined by the Swedish Migration Board. These are individuals who are not granted refugee status according to the UN refugee convention, yet still granted residence permits for humanitarian reasons. This group makes up 1.5% of all newly arrived refugee immigrants in the data. Relatives of refugees make up another 17.4%.

Of all refugees immigrating in 1999–2007 and who are still present and of working age in 2007, almost half are from Iraq, followed by former Yugoslavian countries, Somalia, and Afghanistan. This distribution is shown in Table 1. The total number, 82,460 individuals, corresponds to 1.5% of the total working-age population in 2007.

The average labor market performance of refugee immigrants is poor. For example, five years after immigrating, the employment ratio of working-age refugees who immigrated in 2002 and were of working age in 2007 was merely 39%, compared to 76% for the rest of the Swedish population. The quality of jobs taken by refugees is also often substantially below what would correspond to their level of education (Ekberg and Rooth, 2005).

2.1 – Refugees' settlement patterns

Refugee migration is per definition push-driven. Since finding work is not the reason for refugee migration we would expect refugees to be less informed and concerned about where to settle in the immigration country to maximize expected income or the probability of finding work compared with other migrants. In a study of settlement choices of US immigrants, Dodson (2001) shows that the tendency of immigrants to settle close to previous immigrants of the same ethnicity is particularly strong for refugee immigrants, while it is insignificant for labor immigrants.

Variation in refugees' settlement locations and previous residents' labor market outcomes will be used in this paper to identify a causal relation between the two. The settlement patterns of newly arrived refugees are shown in the graphical appendix. The settlement locality for immigrants arriving in 1999–2003 is identified by where they reside in 2003, and for arrivals in 2004–2007 by where they reside in 2007. Crucially for the identification strategy to be applied, settlement patterns must not be significantly predicted by initial labor market opportunities. In the Swedish setting, with highly centralized wage-setting, the obvious variable of choice for measuring initial labor market opportunities is the initial unemployment rate. This measure also has the advantage that information on it is easily available to potential settlers, compared with information on wages or total income levels.

The finest measurement level for both refugee settlements (the dependent variable) and unemployment rates (the independent variable) is the municipality. Sweden has 289 municipalities,[1] with widely varying geographical areas between the more and less densely populated parts of the country. Although the vast majority of municipalities have a clearly defined main city or town dominating the municipality, the municipality does in several cases not well represent the local labor market, especially for municipalities in densely populated regions in the vicinities of larger cities. To arrive at a measure of unemployment that is relevant also in municipalities near the larger cities, unemployment rates in surrounding municipalities must be taken into account. The size N_m of the local labor market relevant to a worker in municipality m is specified as:

$$N_m = \sum_{i=1}^{289} \left(\frac{n_i}{D_{im}^2} \right) \tag{1}$$

where n_i is the number of workers in municipality i, and D_{im} is the distance in kilometers, as the crow flies, between municipalities i and m. For the more than 90% of municipalities with an easily defined main city/town, distances are measured from this town. When an obvious main city/town does not exist, they are measured from a central location in the municipality. The distance is set to 5 km when $i=m$, or when (one instance) two municipalities are less than 5 km apart. The distance is squared to capture the quickly decreasing "gravitational force" between two municipalities as the distance between them increases. Due to this quickly declining influence, it is not necessary to specify an outer geographical bound for the local labor market. This specification implies, e.g., that in a municipality that is situated 15 km from a municipality ten times as large, it is about equally probable that workers residing in the smaller municipality will find their workplace in either of the two, while if the larger municipality is 30 km away, the probability of working in the own municipality is 3-4 times as high as that of working in the larger municipality. These magnitudes are roughly consistent with out-commuting population shares in municipalities in the vicinities of the larger cities.

To arrive at a relevant unemployment rate in a local labor market, the total number of unemployed U_m in labor market m is given by substituting total numbers of unemployed workers for total numbers of workers in Equation (1), and the unemployment rate u_m is equal to U_m/N_m. All individuals registered at the Swedish Public Employment Service on Dec 31 each year are counted as unemployed, including if they have part-time, temporary, or subsidized employment. The weighted local labor market unemployment rates will be similar to the original municipal rates yet with smaller variance, both along any vector across the map

[1] The number of municipalities changed from 289 to 290 in 2003, when Knivsta municipality was separated from Uppsala municipality. To maintain a consistent data set, this study will treat Knivsta as still being part of Uppsala.

and in total. The latter is confirmed in Table 2, which shows summary statistics of the two rates. It shows that the two measures are highly correlated and also that unemployment is strongly decreasing over the period. The geographical patterns of municipal unemployment rates in 1998 and 2003 are shown in the graphical appendix. It shows in particular that unemployment is consistently higher in the northern part of the country.

2.2 – Regression analysis of settlement patterns

To investigate whether unemployment rates predict refugee settlement decisions, the number of newly arrived refugees in a municipality over a period – measured as a share of the municipality's initial total population – is regressed on the weighted unemployment rate u_m in the relevant labor market at the start of the period, while controlling for latitude, total initial labor market size, a vector of distances – raised to minus two – to the three largest cities Stockholm, Gothenburg, and Malmö, a vector of dummies for ten municipality groups, as defined by the Swedish Association of Municipalities and Regions (*Sveriges Kommuner och Landsting*),[2] and a second-period dummy that is also interacted with the other control variables. The fact that unemployment is significantly higher in northern Sweden is thus controlled for both through the latitude variable and through the municipality groups, since municipalities belonging to three of the groups (8,9,10) are mostly clustered in this region. Summary statistics of the refugee inflow rates are shown in the tope panel of Table 3.

Estimates of the parameter of interest – for the pooled sample and for each period separately – are shown in column (1) of Table 4. They vary in sign and are far from significant, both with standard errors robust to heteroscedasticity and standard errors robust to spatial correlation (Conley method). Spatial correlation in the independent variable is also accounted for by the construction of the local labor market rate, which decreases variance between municipalities close to each other.

For comparison, column (2) of Table 4 shows similar parameter estimates from similar regressions for newly arrived non-refugee immigrants from low- and middle-income countries.[3] Here, the parameter of interest always has the expected negative sign, and it is significant at the 5% level in the first period and almost significant in the pooled sample. This indicates that this immigrant group to some extent chooses labor markets with lower unemployment rates. An increase in the unemployment rate by one percentage point reduces the migrant inflow by about 0.01–0.02 percentage points, i.e., a one standard deviation increase in the former implies a 0.1-0.2 standard deviation increase in the latter. The estimates in column (3) are less directly comparable with those of columns (1) and (2), but show that internally migrating natives significantly – and strongly – take unemployment rates into account. Here the dependent variable is the relative change in the native working-age population over the time period. The coefficient estimates imply that an increase in the unemployment rate by one percentage point reduces the size of the native population 4-5 years later by around 0.3%.

[2] These groups are 1) Larger cities (n=3), 2) Suburbs of larger cities (n=38), 3) Larger towns (n=31), 4) Suburbs of larger towns (n=21), 5) Commuter municipalities (n=51), 6) Tourism municipalities (n=20), 7) Goods-producing municipalities (n=54), 8) Sparsely populated municipalities (n=20), 9) Municipalities in densely populated regions (n=35), and 10) Municipalities in sparsely populated regions (n=16).

[3] "Low- and middle-income countries" refers to all countries except Andorra, Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Liechtenstein, Luxemburg, Monaco, the Netherlands, New Zeeland, Norway, Portugal, San Marino, Spain, Switzerland, the UK, and the US, which will be referred to as "high-income countries."

In sum, there is no empirical support at all in Table 4 for refugee immigrants significantly taking unemployment rates into account in their settlement decisions, which would have invalidated a crucial identifying assumption of this study. Moreover, since the estimates at least indicate that other low- and middle-income immigrants *do* take these rates into account, the results give some support to the claim made in this paper that settlement endogeneity is less of a problem when estimating the labor market impact of refugee immigration compared with other immigration.

3 – Unemployment effects of immigration

When estimating the unemployment effect of a refugee inflow in this study, the dependent variable is the change in unemployment status of an individual over a time period. It can take the values 0 (no change), 1 (enters unemployment), or –1 (quits unemployment). Unemployment status is measured on December 31 in each of the three years. A main limitation is that it is not clear in the data whether an individual moves between unemployment and employment or between unemployment and being out of the labor force. Hence the credibility of the estimates hinges on the assumption that the first of the two dominates the second. Importantly, this crucial assumption will be supported in Section 3.1, where restricting the sample to workers who are seemingly present in the labor force in all three years does not significantly affect parameter estimates.

The independent variable of interest is the inflow rate of refugees over the time period into the labor market where the individual resides. This inflow rate is constructed as a weighted sum of all newly arrived refugees in the country, in the same way as the labor market size and unemployment rate variables in Section 2. Summary statistics of the weighted refugee inflow rates in the two sample periods are shown in the bottom panel of Table 3.

The change in unemployment status during the time period is regressed on the weighted refugee inflow during the same period, while controlling for a second-degree polynomial in age, a dummy for individuals who moved between municipalities during the period, and the control variables used in Section 2: initial labor market size, latitude, distances to Stockholm, Gothenburg, and Malmö raised to minus two, municipality group dummies, and a second-period dummy. The second-period dummy is interacted with all other control variables except the dummy for switching municipalities, the effect of which is assumed to be the same in both periods.

A main advantage of the data set is that we can follow individuals who migrate between municipalities during the time period, since they may possibly do so in response to immigration. This is potentially important, as in the first and second sample periods 12% and 8% of all individuals in the data set moved between two municipalities, respectively. Hence if these internal migrations are to some extent caused by the immigrant inflows they could importantly bias the estimates of the parameter of interest towards zero. To eliminate this bias resident individuals are observed at the start of each period rather than at the end. Hence they are not given the possibility to react to the immigrant inflow.

Unemployment effects are evaluated in samples including all workers, all natives, all previous immigrants from low- and middle-income countries, and all previous immigrants from high-income countries. No distinction can be made between the unemployment of earlier refugee immigrants and other low- and middle-income immigrants, as refugee status is not appropriately coded for immigrants who arrived earlier than 1997. Refugee inflows are not disaggregated by educational status, as newly arrived highly educated Swedish refugee immigrants seldom take up jobs corresponding to their education levels (Ekberg and Rooth, 2005).

Estimates of the parameter of interest from the pooled sample of the two time periods and for each period separately are shown in Panel A of Table 5. The marginal effects are effects of inflow rates on the individual's probability of being unemployed, i.e., effects on unemployment *rates*. As seen in column (1), the analysis finds no significant effect of refugee immigration on the unemployment rate of the total working-age population. In columns (2)-(4), effects are measured separately for natives, low- and middle-income country immigrants, and high-income country immigrants. While there are no consistently significant effects on natives or high-income country immigrants, large and significant effects are found on previous immigrants from low- and middle-income countries, with very similar magnitudes across the two periods. These coefficients imply that a one percent increase in the total working-age population due to refugee immigration increases the unemployment rate of previous immigrants in this group by 2.0 percentage points. Reinterpreting this estimate as a crowding-out effect, i.e., how many previous immigrants who lose or do not find a job for every newly arrived immigrant who finds one, we find that this effect is as large as 0.8, with a 95% confidence interval between 0.4 and 1.2.[4]

Averages of residuals from the pooled sample regressions of columns (1) and (3), per municipality and year, are shown in the graphical appendix. It shows that municipalities with positive and negative residuals are quite evenly distributed across the country, indicating that the analysis has managed to control for confounding spatial trends.

Panel B of Table 5 shows parameter estimates similar to in Panel A, obtained when resident individuals are instead observed at the end of the period, i.e., when their behavior in response to the immigrant inflow can potentially bias the coefficient estimate of interest towards zero. There are no significant differences between the results reported in Panels A and B though. Hence in this setting internal migration would not have importantly biased any parameter estimates if we would not have been able to control for it. This observation may also to some extent serve as support for the results obtained by other studies from other countries that could not effectively control for internal migration.

In Table 6 the effects of refugee immigration are evaluated separately for workers at different levels of educational attainment, in the pooled sample of two periods. "No high school" means that the individual does not report any high school (Swedish gymnasium) education, with or without degree; "No university" means no reported university education, with or without degree; "University degree" means having a degree requiring at least three years of university studies. There are still no significant effects among natives or high-income country immigrant workers, and also not among the best educated low- and middle-income country

[4] Denoting the number of old jobs lost (or not found, that would otherwise have been found) by previous immigrants ΔJ_P, and the number of jobs obtained by the newly arrived refugees ΔJ_R, the crowding out effect is $\Delta J_P / \Delta J_R$. A positive coefficient in Table 5 can be interpreted as $\frac{\Delta J_P / n_P}{\Delta n_R / n_{TOT}}$, where the numerator is the change in the unemployment rate of previous immigrants and the denominator is the relative increase in the population due to immigration of new refugees. So to move from the estimated coefficient to a crowding-out effect, the coefficient needs to be multiplied by n_P / n_{TOT}, which is the share of previous immigrants in the population (slightly larger than 6%), and divided by $\Delta J_R / \Delta n_R$, which is the employment rate of the newly arrived refugees (around 15%). In sum, this amounts to a division by about 2.5.

immigrants. This is consistent with the earlier finding (Ekberg and Rooth, 2005) that the newly arrived mostly compete for lower skilled jobs.

3.1 – Sensitivity analysis

In this subsection the results reported in Table 5 are subjected to various modifications in sample selection and regression specification. All results reported henceforth refer to the pooled sample of two periods.

A weakness of the identification strategy employed is that it is not possible to properly distinguish between a worker who moves between unemployment and employment and one who moves between unemployment and being out of the labor force, as there is no point measure of employment or labor force participation in the data. What exists is a measure, created by Statistics Sweden, that combines information from different sources into a binary measure of being employed or not in a specific year. While crude, this variable enables an alternative identification strategy that discards some information yet provides more robustness against effects of movements in and out of the labor force. This is to run the regressions only in the sample of workers who are clearly in the labor force in all three years, i.e., they are either classified as employed or they have been registered as unemployed some time during the year (not necessarily on Dec 31). The results of this analysis are shown in row (1) of Table 7. The estimated effect on previous low- and middle-income country immigrants is very similar to that in the original results and is still highly significant, and the effects estimated in other samples are still insignificant.

Row (2) reports the results obtained when a more strict definition of refugees – excluding the 19% who are either relatives or individuals in "refugee-like" situations – is applied when measuring the refugee inflow. These results are highly similar to the benchmark results.

While the maps in the graphical appendix are quite reassuring on the point that no importantly confounding spatial trends have been omitted from the analysis, they do reveal some visible amount of spatial clustering of residuals. The Moran's I statistics for these patterns also reject the null hypothesis of no spatial correlation in the residuals. One way to eliminate this correlation is to increase the level of spatial aggregation from the municipality to the *län*, i.e., an administrative region encompassing several municipalities (14 on average). In one instance, in the capital region, what is presumably one labor market consists of three *län*: Stockholm, Södermanland, and Uppsala Län. Therefore these three are merged into one. Estimation results with standard errors clustered at the level of the *län* are reported in row (3) of Table 7. Remarkably, the coefficient of interest in the previous low- and middle-income country immigrant sample has a p value of 0.000 also in this case, although the number of *län* (after merging three of them) is only 19. If the merged capital region *län* is removed from the sample the coefficient falls to 1.80 with p = 0.001. With any other individual *län* removed the coefficient is always higher than so and with p = 0.000.

Row (4) reports results obtained after removing outlier municipalities. Outlier municipalities are identified in Figure 1, which plots changes in total unemployment rates against refugee inflow rates for all municipalities and both periods. The figure shows five municipalities that could potentially have strong influence on the regression results. These are Salem (128), Södertälje (181), Strömstad (1486), Sorsele (2422), and Dorotea (2425). Four of these are small municipalities with populations below 15,000 and hence limited influence on the regression results. Södertälje, the only municipality that is a visible outlier in both periods, is larger, with a population around 80,000. Södertälje is also well-known for having attracted very large numbers of Iraqi refugee migrants since 2005, due to the historical presence of

Assyrians in the city.[5] As shown in row (4), removing outlier municipalities brings the estimated coefficient in the low- and middle-income country immigrant sample down by about 30%, and the associated crowding-out effect between 50% and 60% may be seen as more reasonable than the original 80%.

From the decrease in the coefficient of interest in the low- and middle-income country immigrant sample when outliers are removed, one might suspect that the unemployment effect of refugee immigration is increasing rather than linear. This is also what is indicated if a squared term in the refugee inflow is added to the regression specification: it dominates the linear term, i.e., makes the coefficient on the linear term negative. Yet this conclusion may not be correct. When Södertälje is removed from the sample, it is instead the coefficient on the squared term that is negative, and unemployment in Södertälje has increased partly due to substantial layoffs and not only due to an increased supply of workers. In 2007, the year when Södertälje received particularly large numbers of refugees, AstraZeneca, one of the two strongly dominating private-sector employers in Södertälje laid off large numbers of workers (on request they have refused to disclose exact numbers). Hence concluding that unemployment effects of immigration are increasing based on the Södertälje case could be erroneous.

Row (5) of Table 7 shows the results from analyzing a reweighted sample where each labor market rather than each individual is given the same weight, to reduce the influence of the larger labor markets. To this end each individual is weighted in the regressions by the inverse of labor market size. This modification has negligible impact on the regression results.

In row (6), the northern part of the country, which has a lower population density, higher unemployment, and lower immigrant inflow, is deleted from the sample. The northern part is delimited here by what is conventionally referred to as the Norrland region, i.e., all municipalities with Statistics Sweden numbers larger than 2100. In row (7), the capital region is deleted. The capital region is defined as the sum of all municipalities with centers within 100 km of that of Stockholm. Deleting the Norrland region does not affect any of the conclusions. Deleting the capital region makes the estimated coefficient in the low- and middle-income country immigrant sample somewhat smaller and less significant, yet far from significantly smaller than the benchmark estimate. This result is strongly driven by the deletion of Södertälje.

3.2 – Further analysis of effects on low- and middle-income country immigrants

The results obtained thus far are much in line with those of recent studies from the US (Ottaviano and Peri, 2012; Card, 2009), Germany (D'Amuri, Ottaviano, and Peri, 2010), and the UK (Manacorda, Manning, and Wadsworth, 2012), which conclude that newly arrived immigrants are substantially more easily substituted for previous immigrants than for natives in production. In this study this result is confined to immigrants from low- and middle-income countries though. However, the large estimated effect on the unemployment rate of previous low- and middle-income country immigrants does not imply a significant effect on the total unemployment rate. Hence this negative effect on one minority group may be balanced by positive effects on other subgroups that complement newly arrived immigrants in production

[5] There are well-known explanations also for the other outliers: Sorsele and Dorotea, which are remotely situated municipalities with declining and aging populations, have actively tried to attract immigrants to turn their demographic trends. Strömstad is situated on the Norwegian border close to the Norwegian capital Oslo and benefits from flourishing cross-border trade as a result of the large price differences between Sweden and Norway. Salem is a smaller neighbor of Södertälje.

yet have not been identified and analyzed separately. Although not very different from comparable effects estimated in other countries in the studies referred to above, the derived crowding-out effect on the low- and middle-income country immigrant group of around 80% is substantial, motivating further analysis of the robustness and drivers of this result.

One theoretically possible reason for an inflated estimate of the effect of newly arrived refugees could be a positive correlation between the numbers of newly arrived refugees and of newly arrived non-refugees. If the settlement patterns of the two immigrant groups are positively correlated and they both affect unemployment of previous immigrants, the effect of the non-refugees will, at least partly, go into the coefficient on the refugees when no measure of the inflow of non-refugees is included in the regressions. Yet, when including the immigration rate of low- and middle-income country non-refugee immigrants besides the refugee measure its coefficient is very small and far from significant. This is shown in column (1) of Table 8. Although large, it is not even significantly different from zero when the refugee inflow measure is not included, as shown in column (2). This result may also be interpreted in support of the identification strategy of this paper, i.e., to only look for the effects of the push-driven refugee inflow. A possible reason why the refugee inflow gives a significant effect and the non-refugee inflow does not may be an endogenous settlement pattern of the latter, biasing its coefficient towards zero.

A further attempt to shed light on the large effect of refugee immigration on the unemployment rate of earlier low- and middle-income country immigrants is to further disaggregate the affected subsample. Estimated effects on low- and middle-income country immigrant subsamples that are disaggregated along likely refugee status, time since immigration, gender, marital status, and age are shown in Table 9. While refugee status is not properly coded for immigrants arriving earlier than 1997, combinations of country of origin and year of arrival can be used to identify groups of immigrants who are highly likely to be refugees.[6] The estimated effect on this group is shown in row (1) of Table 9. It is smaller, yet not significantly smaller than that for all low- and middle-income country immigrants.

Time since immigration is another plausible candidate covariate that could influence the effect of immigration on the individual's probability of being unemployed, since immigrants' initial labor market attachment is poor but improves over time. The median immigration year of immigrants in the sample is 1989. Rows (2) and (3) of Table 9 show the effects on immigrants arriving before and after (and including) the median year, respectively. The difference between the estimates has the expected sign, with the estimate on later arrivals being larger, yet the difference is not significant.

In rows (4)–(7), the low- and middle-income country immigrant sample is disaggregated along gender and marital status, where marital status is measured in 1998. There is a negative and insignificant estimate for unmarried men, which may be a group that more easily adjusts to changing circumstances. The estimate for unmarried women is also smaller than that for married women and men, yet these differences are not significant. Rows (8) and (9) show results disaggregated by the midpoint in the age interval, 40 years, in 1998. As expected, the effects are stronger on younger workers, but also in this case the difference is not significant.

[6] The combinations of country of origin and year of immigration used to identify refugees are: Afghanistan 1980–1998, Bosnia-Hercegovina 1993–1998, Bulgaria –1989, Chile 1973–1980, Croatia 1993–1998, Czechoslovakia –1989, Cuba, Eritrea, Ethiopia 1974–1998, Guatemala, Hungary –1989, Iran 1979–1989, Iraq 1980–1989, Liberia 1989–1998, Libanon 1975–1991, Poland –1989, Romania –1989, Sierra Leone 1992–1998, Somalia 1991–1998, Sudan, and Yugoslavia 1993–1998.

In sum, the results in Table 9 do not reveal any striking differences between different subgroups of the low- and middle-income country immigrant sample. The estimated differences between subgroups always have the expected signs, yet the estimated coefficient is significantly different from the benchmark estimate +1.98 only in the subsample of unmarried men. Analyses not reported have also included interaction terms between the refugee inflow and the previous unemployment rate, with far from significant coefficients.

3.3 – Effects on other groups with low labor market attachment

In line with earlier research the results obtained thus far, with large effects of refugee immigration on earlier low- and middle-income country immigrants' unemployment yet no significant effects on native or total unemployment, have been interpreted in terms of substitutes and complements. New immigrants are more easily substituted for previous immigrants than for natives, presumably because immigrants, who often lack linguistic and cultural skills, concentrate disproportionately in certain occupations where these skills are less required (Peri and Sparber, 2009). Another possible interpretation of the results of this study is that refugee inflows have large negative effects on workers with low labor market attachment, of whom many are earlier immigrants. This interpretation is partly different from the substitutability interpretation, as lack of linguistic and cultural skills may be – yet is not necessarily – the reason for low labor market attachment. Hence, it is informative to analyze whether refugee inflows have significant unemployment effects on native workers with low labor market attachment.

Besides immigrants, the group commonly referred to in the Swedish public debate as one with low labor market attachment is young natives, and especially young natives with low education. Table 10 reports estimates of the effect of refugee immigration on the unemployment probabilities of six groups of young natives: the rows are for those aged 25–29 and 25–34 in 1998 and the columns are for all workers, those with no university education, and those with no high school education, respectively. The groups are thus cumulative in terms of both age and education. Table 10 reports a significant effect on unemployment where it was most expected, i.e., for the very youngest and least educated group. The estimate is about 60% in magnitude of that on previous immigrants from low- and middle-income countries yet the difference is not significant. Hence both of the interpretations mentioned in the beginning of this subsection seem to have some validity. A refugee inflow has an effect also on the unemployment rate of the small native group with the weakest attachment to the labor market, yet the effect on previous immigrants is probably stronger. The point estimates in the other cells of Table 10 are, although not significant, quite large. Yet these are quite strongly driven by the group with the significant estimate, which is part of all the other groups.

The point estimate on the youngest and least educated natives implies that each refugee who actually finds work crowds out about 0.06 of these natives. The difference between the estimated crowding-out effects on this group and on immigrants from low- and middle-income countries, 6% versus 80%, is substantially larger than the difference between the estimated unemployment rate effects. This is because the previous immigrant sample is 7-8 times as large as the native sample in question, i.e., there are simply more immigrants to crowd out. It is worth reiterating that although these estimated crowding-out effects sum to almost 100%, there is no significant effect on the total unemployment rate. And although the point estimate for all workers in Table 5 corresponds to a quite substantial total crowding-out effect of around 35%, several of the robustness checks reported in Table 7 even make this point estimate negative. The absence of a significant total unemployment effect of immigration, in spite of the substantial effects on smaller groups, indicates that plausibly there are other subgroups of the workforce – which the analysis has not managed to identify – that complement the newly arrived refugees in production, i.e., whose unemployment rates may decrease with a refugee inflow.

4 – Discussion

This study has estimated unemployment effects of Swedish refugee immigration using spatial variation in immigrant inflows and labor market outcomes. Refugee immigration is found to have a substantial negative impact on earlier immigrants from low- and middle-income countries, yet no significant impact on natives or immigrants from high-income countries. The estimated impact on the unemployment rate of previous low- and middle-income country immigrants is high, and translates into a crowding-out effect on this group as high as 0.8 for each refugee who finds work. The effect is quite constant across different subgroups of the low- and middle-income country immigrant population, yet it does not shine through in a significant effect on the total unemployment rate. This indicates that there may be positive effects on other subgroups of the workforce who complement these immigrants in production.

The results of this study are well in line with earlier results from other countries, both qualitatively and in magnitudes. Importantly though, this study derives this result using spatial variation for direct identification of effects. With the exception of Card (2009), the earlier studies referred to use time series data to estimate parameter values in assumed national-level production functions and then use these functions to simulate the impact of immigration. The results of these studies depend strongly on the assumed functional forms and on stable trends in changes in parameter values and that these in turn are not affected by the immigration patterns over the decades. The generally low robustness of results thus derived are shown by Ottaviano and Peri's (2012) analysis of the results obtained by Borjas (2003), and in turn by Borjas, Grogger, and Hanson's (2012) analysis of the results of Ottaviano and Peri. Further methodological issues are highlighted by Dustmann and Preston (2012). In light of these question marks on the validity of the structural framework technique, it is important to see that similar results are obtained when using a different method that does not rely on any functional form assumptions.

Another difference between this study and all previous studies that find imperfect substitutability between immigrants and natives is that in this study this finding only applies to immigrants from low- and middle-income countries, while high-income country immigrants seem to be more similar to natives. Most studies of wage effects of immigration implicitly or explicitly assume that immigrants from all countries affect the labor market in the same way. The results presented here should serve as motivation to move away from this assumption in future research.

The time interval covered by this study is one of increasingly good labor market conditions. Between 1998 and 2003, the unemployment rate in the average municipality fell by 4.8 percentage points, and in 2007 it had fallen by another 2.4 percentage points. The effects of immigration are not necessarily the same in business cycle downturns. An analysis of possible differential effects of immigration depending on business-cycle movements should be enabled by similar data from only a few years later, when Sweden, along with most of the Western world, entered a recession.

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Tables

Table 1. Distribution of refugee immigrants by source country

Country	Count
Iraq	37,460
Former Yugoslavia	10,939
Somalia	5,760
Afghanistan	4,466
Iran	3,461
Syria	1,587
Burundi	1,320
Russia	1,283
Eritrea	1,268
Others	14,916
Total	82,460

Notes: Numbers include refugees immigrating in 1999 – 2007, still present, and 18 – 64 years of age in 2007. Former Yugoslavian countries are counted as one, as the data do not enable proper distinction between them.

Table 2. Summary statistics of unemployment rate measures

Year	Mean %	St. dev. p.p.	Min %	Max %	Corr A,B %
			Municipal rates (A)		
1998	18.0	5.2	5.2	31.9	-
2003	13.2	4.3	4.3	31.2	-
2007	10.8	3.8	2.6	26.1	-
			Weighted local labor market rates (B)		
1998	17.6	4.1	8.6	30.4	96.9
2003	12.9	3.5	6.5	29.0	97.5
2007	10.7	3.0	5.2	25.0	96.9

Notes: n=289. Rates are measured on Dec. 31 each year and refer to numbers of unemployed over total populations aged 18–64. All individuals registered at the Swedish Public Employment Service on Dec. 31 each year are counted as unemployed, including if they have part-time, temporary, or subsidized employment.

Table 3. Summary statistics of municipal refugee inflow rates

Period	Mean %	St. dev. p.p.	Min %	Max %
		Municipal rates		
1999 – 2003	0.44	0.47	0	3.0
2004 – 2007	0.63	0.49	0	3.9
		Weighted local labor market rates		
1999 – 2003	0.56	0.31	0.03	2.1
2004 – 2007	0.73	0.31	0.07	2.4

Note: n = 289.

Table 4. Partial effects of initial unemployment rates on settlements

	Sample		
Period	(1) New refugees	(2) New low- and middle-income country non-refugees	(3) Internally migrating natives
Pooled (n=578)	–0.001	–0.014	–0.324
	(0.008)	(0.006)	(0.050)
	[0.009]	[0.008]	[0.075]
1999-2003 (n=289)	–0.012	–0.019	–0.374
	(0.010)	(0.009)	(0.073)
	[0.009]	[0.009]	[0.010]
2004-2007 (n=289)	0.011	–0.007	–0.268
	(0.012)	(0.009)	(0.064)
	[0.011]	[0.009]	[0.071]

Note: Each cell contains the parameter of interest from a separate regression. Initial unemployment rates are measured on Dec. 31 in 1998 and 2003 respectively. Heteroscedasticity-robust standard errors in parentheses; spatial correlation-robust standard errors (Conley method) in brackets.

Table 5: Estimated unemployment effects of refugee immigration

Period	Sample – origin			
	(1) All workers	(2) Natives	(3) Low- and middle-income country immigrants	(4) High-income country immigrants
	Panel A: Resident workers observed at start of period			
Pooled	0.053	0.152	1.98*	0.304
	(0.864)	(0.604)	(0.000)	(0.472)
	[7,296]	[6,337]	[590]	[368]
1999-2003	–0.223	0.144	1.93*	–0.107
	(0.655)	(0.785)	(0.019)	(0.889)
	[3,648]	[3,169]	[295]	[184]
2004-2007	0.250	0.154	2.02*	0.591*
	(0.318)	(0.478)	(0.001)	(0.035)
	[3,648]	[3,169]	[295]	[184]
	Panel B: Resident workers observed at end of period			
Pooled	–0.085	–0.013	2.07*	0.230
	(0.743)	(0.957)	(0.000)	(0.494)
	[7,296]	[6,337]	[590]	[368]
1999-2003	–0.526	–0.167	1.66*	–0.355
	(0.178)	(0.694)	(0.017)	(0.537)
	[3,648]	[3,169]	[295]	[184]
2004-2007	0.236	–0.099	2.35*	0.645*
	(0.338)	(0.654)	(0.000)	(0.018)
	[3,648]	[3,169]	[295]	[184]

Note: Each cell contains the parameter of interest from a separate regression. Standard errors are clustered at the municipality level. P values in parentheses and n values in thousands in square brackets. A * denotes significance at the 5% level.

Table 6. Estimated effects by level of education

	Sample – origin			
Sample – education	(1) All workers	(2) Natives	(3) Low- and middle-income country immigrants	(4) High-income country immigrants
No high school	0.163	0.085	2.51*	–0.211
	(0.455)	(0.700)	(0.000)	(0.522)
	[1,275]	[1,041]	[142]	[90]
No university	0.136	0.142	2.74*	0.451
	(0.692)	(0.666)	(0.000)	(0.269)
	[4,864]	[4,202]	[400]	[260]
University degree	0.066	0.335	–0.929	0.252
	(0.778)	(0.114)	(0.253)	(0.708)
	[1,180]	[1,055]	[76]	[48]
Benchmark results	0.053	0.152	1.98*	0.304
	(0.864)	(0.604)	(0.000)	(0.472)
	[7,296]	[6,337]	[590]	[368]

Note: Each cell contains the parameter of interest from a separate regression. Standard errors are clustered at the municipality level. P values in parentheses and n values in thousands in square brackets. A * denotes significance at the 5% level.

Table 7: Sensitivity analysis

	Sample – origin			
Specification	(1) All workers	(2) Natives	(3) Low- and middle-income country immigrants	(4) High-income country immigrants
(1) Strong attachment	0.139	0.292	1.81*	0.661
	(0.653)	(0.314)	(0.016)	(0.116)
	[5,875]	[5,242]	[372]	[260]
(2) "Proper" refugees	0.097	0.184	2.50*	0.368
	(0.782)	(0.560)	(0.000)	(0.432)
(3) S.e. clustered at *län*	0.053	0.152	1.98*	0.304
	(0.830)	(0.607)	(0.000)	(0.484)
(4) Deleting outliers	–0.265	–0.111	1.37*	–0.363
	(0.354)	(0.693)	(0.045)	(0.300)
	[7,208]	[6,271]	[578]	[357]
(5) Weights	0.144	0.195	1.74*	0.252
	(0.613)	(0.513)	(0.006)	(0.605)
(6) Deleting Norrland	0.134	0.223	2.20*	0.227
	(0.701)	(0.495)	(0.000)	(0.635)
	[6,343]	[5,444]	[565]	[333]
(7) Deleting Stockholm	–0.026	0.071	1.68	–0.483
	(0.931)	(0.809)	(0.085)	(0.369)
	[5,228]	[4,660]	[353]	[214]
Benchmark results	0.053	0.152	1.98*	0.304
	(0.864)	(0.604)	(0.000)	(0.472)
	[7,296]	[6,337]	[590]	[368]

Note: Each cell contains the parameter of interest from a separate regression. Standard errors are clustered at the municipality level. P values in parentheses and n values in thousands in square brackets (only shown when deviating from original sample). A * denotes significance at the 5% level.

Table 8: Effects of refugee and non-refugee inflows

Independent variable	(1)	(2)	(3) (original)
Refugee inflow	2.03*		1.98*
	(0.002)		(0.000)
Non-refugee inflow	–0.084	0.812	
	(0.876)	(0.195)	

Notes: The sample consists of previous low- and middle-income country immigrants only. Non-refugee inflows include only low- and middle-income country immigrants. Each cell contains the parameter of interest from a separate regression. Standard errors are clustered at the municipality level. P values in parentheses. n>590,000. A * denotes significance at the 5% level.

Table 9: Effects on subgroups of low- and middle-income country immigrants

Subsample	Estimate
(1) Refugees	1.57
	(0.075)
	[220]
(2) Year of immigration < 1989	1.39*
	(0.012)
	[280]
(3) Year of immigration ≥ 1989	2.42*
	(0.001)
	[310]
(4) Married men	2.59*
	(0.000)
	[176]
(5) Unmarried men	–0.322
	(0.666)
	[113]
(6) Married women	2.62*
	(0.000)
	[197]
(7) Unmarried women	1.80*
	(0.032)
	[103]
(8) Age < 40 in 1998	2.40*
	(0.000)
	[337]
(9) Age ≥ 40 in 1998	1.40*
	(0.024)
	[253]
Benchmark result	1.98*
	(0.000)
	[590]

Notes: Each cell contains the parameter of interest from a separate regression. Standard errors are clustered at the municipality level. P values in parentheses and n values in thousands in brackets. A * denotes significance at the 5% level.

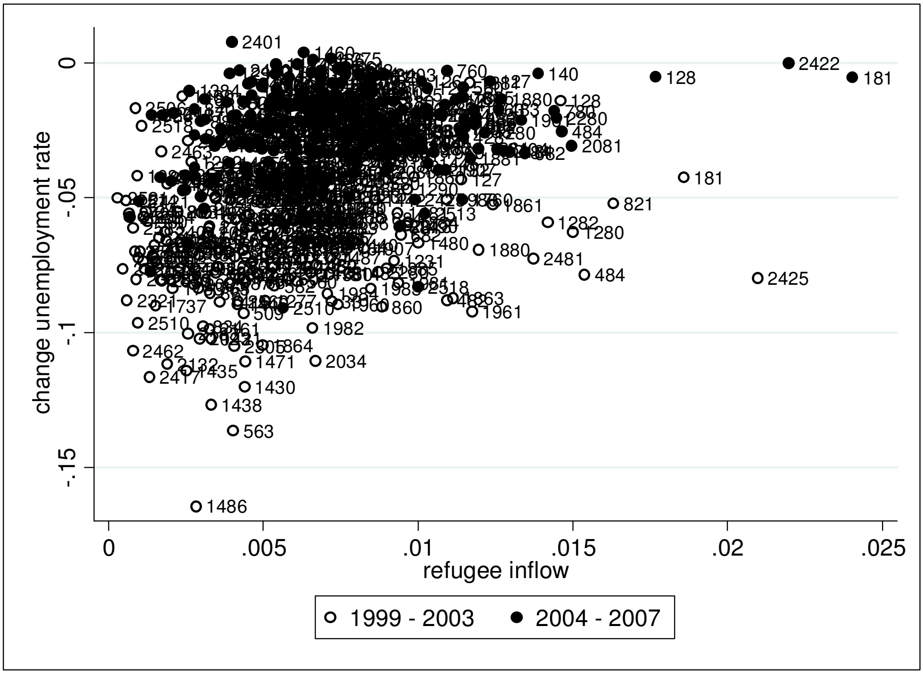
Table 10. Estimated effects on young natives' unemployment

Age in 1998	All workers	No university	No high school
25–29	0.442	0.869	1.21*
	(0.470)	(0.208)	(0.043)
	[1,004]	[598]	[79]
25–34	0.260	0.473	0.621
	(0.597)	(0.388)	(0.136)
	[2,100]	[1,306]	[180]

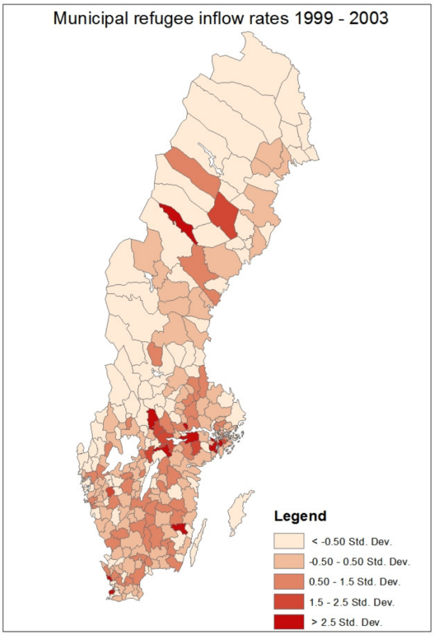
Notes: Each cell contains the parameter of interest from a separate regression. Standard errors are clustered at the municipality level. P values in parentheses and n values in thousands in brackets. A * denotes significance at the 5% level.

Figures

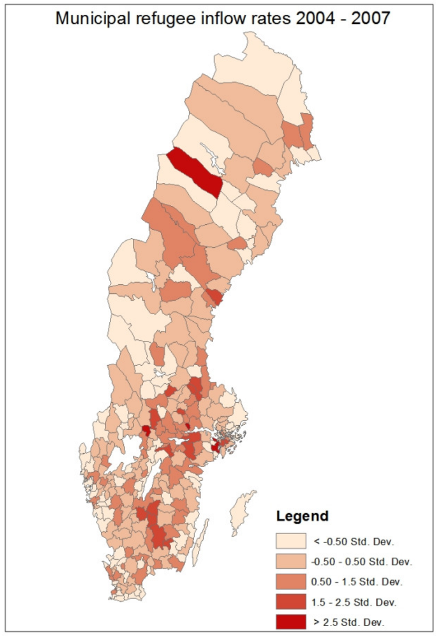
Figure 1. Outlier municipalities



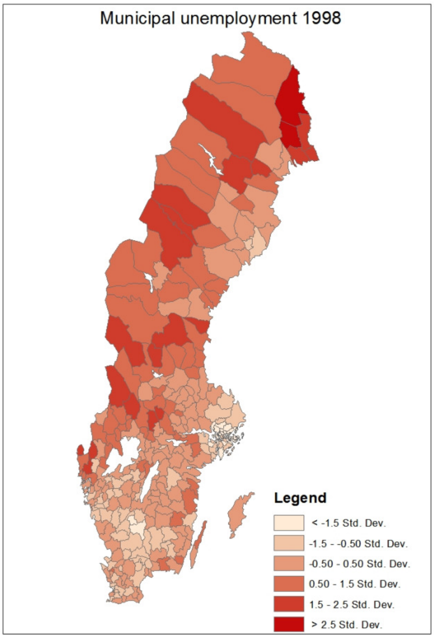
Graphical appendix: maps



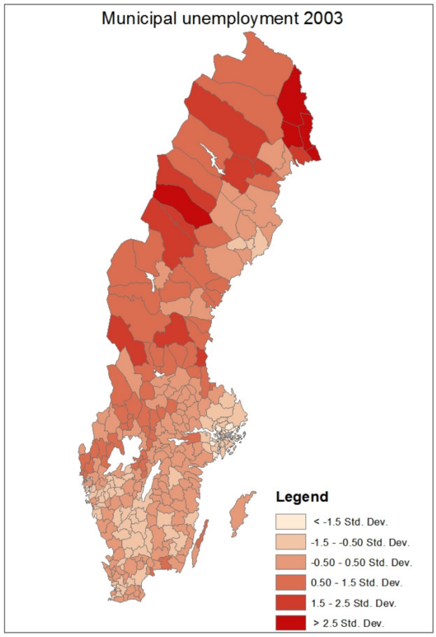
Mean: 0.44%
Std. Dev.: 0.47%



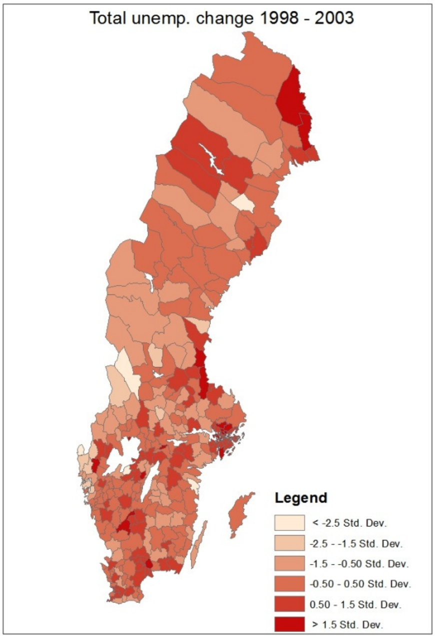
Mean: 0.63%
Std. Dev.: 0.49%



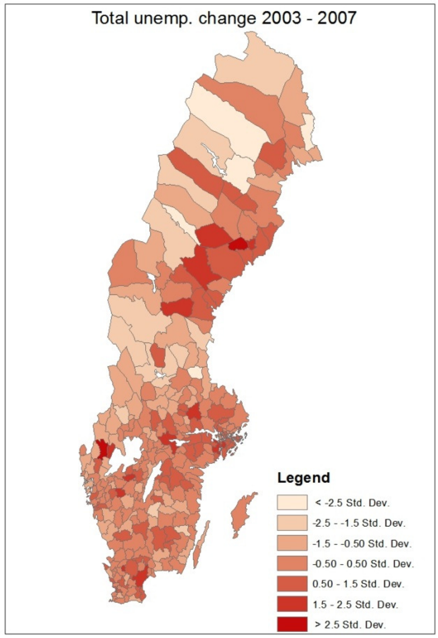
Mean: 18.0%
Std. dev.: 5.2%



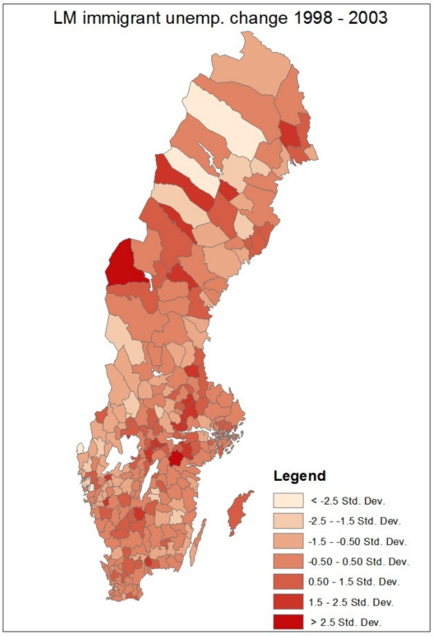
Mean: 13.1%
Std. dev.: 4.3%



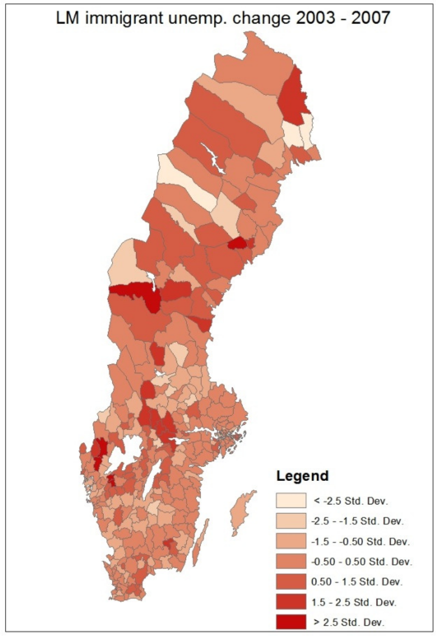
Mean: –4.8%
Std. Dev.: 2.2%



Mean: –2.3%
Std. Dev.: 1.6%

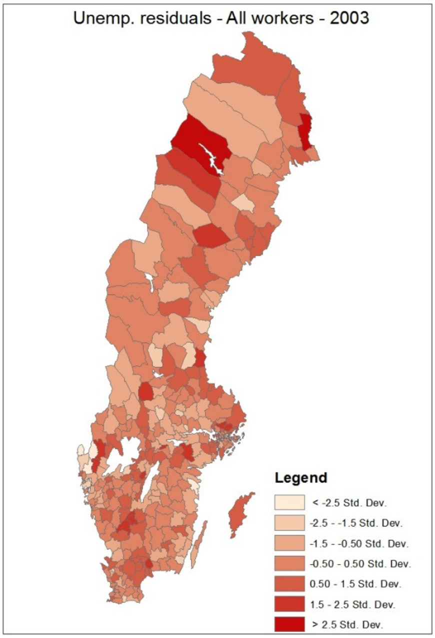


Mean: –9.4%
Std. Dev.: 6.7%

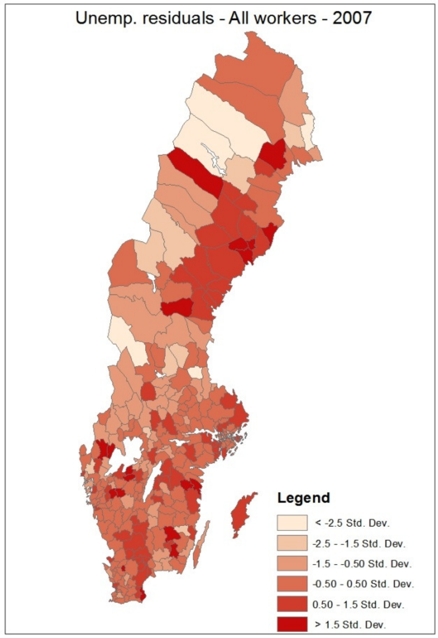


Mean: 0.2%
Std. Dev.: 6.7%

Note: “LM immigrant” refers to low- and middle-income country immigrants.

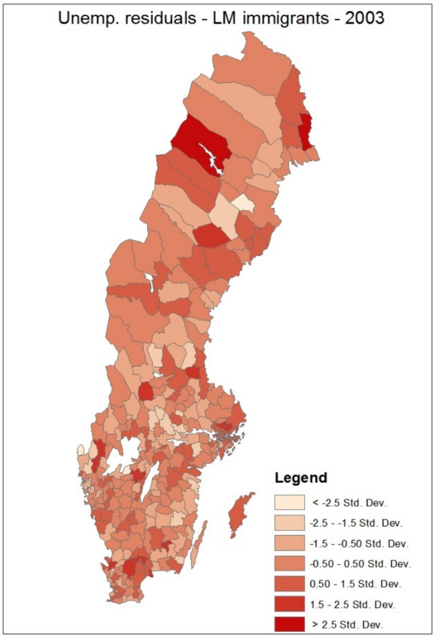


Mean: –0.000
Std. Dev.: 0.021

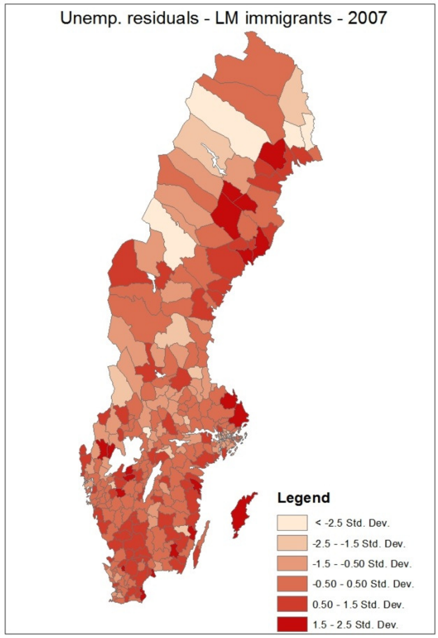


Mean: –0.001
Std. Dev.: 0.013

Note: Graphs show municipal averages of residuals from regressions reported in Table 5, column (1), first row, separately by year of observation.



Mean: 0.072
Std. Dev.: 0.024



Mean: 0.019
Std. Dev.:0.016

Notes: Graphs show municipal averages of residuals from regressions reported in Table 5, column (3), first row, separately by year of observation. “LM immigrant” refers to low- and middle-income country immigrants.

Paper II

Immigrant-native wage gaps in time series: Complementarities or composition effects?

Abstract

This study investigates the role of immigrant composition in creating the observed negative correlation between immigrant supply and immigrant wages in US time series, which has recently been interpreted as evidence of immigrant-native complementarities in production. The main finding is that compositional effects fully explain this empirical pattern. Newly-arrived immigrants, notably Latin Americans, earn less than previous immigrants. Hence their presence decreases average immigrant wages, mechanically. Controlling for this, no negative relation between immigrant supply and immigrant wages remains. More generally, the findings highlight problems in structural estimation of wage effects of immigration, and also the need to distinguish between effects of immigrants of different origins.

1 – Introduction

One of the more contentious issues in the recent academic literature on immigration and the labor market is that of the substitutability between immigrants and natives when these are similar in terms of education and work experience. Whether they are perfect substitutes or not is of first-order importance for how we expect immigration to affect the labor market outcomes of natives and previous immigrants respectively. Recent influential studies from the US (Ottaviano and Peri, 2012), the UK (Manacorda, Manning, and Wadsworth, 2012), and Germany (D'Amuri, Ottaviano, and Peri, 2010) have shown in time series that the immigrant-native wage gap is significantly higher in the parts of the workforce – defined by education and work experience – where immigrants are more numerous. These results have been interpreted as evidence of immigrant-native complementarities in production.

However, an alternative interpretation that cannot yet be ruled out is that these results may be mechanical effects of changing immigrant composition. It is known from previous literature that immigrant earnings vary substantially by region of origin and by the number of years since immigration, also when controlling for education and experience (Borjas, 1995; Bratsberg, Barth, and Raaum, 2007). If this variation is not itself due to complementarities but rather to, e.g., differences in human capital transferability to the immigration country, then increasing immigration from certain regions could create the results observed as a purely mechanical compositional effect also in the absence of immigrant-native complementarities.

The study by Ottaviano and Peri (hereafter OP) has been particularly influential. Their estimated degree of immigrant-native complementarity implies that US immigration 1990-2006 decreased previous immigrants' wages by more than 6% in their simulations, yet increased natives' wages by about 0.6%. Manacorda, Manning, and Wadsworth estimated even stronger complementarities. Due to the magnitudes and influence of these estimates it is important to investigate to what extent they are due to complementarities and compositional effects respectively. The present study investigates this issue on US data. The data underlying the results from the UK and Germany were not of sufficient quality to enable similar investigation.

2 – Data and method

Following OP this study analyzes supply and wages of immigrants and natives at four levels of education and eight levels of work experience. The data used is from the US Census/American Community Surveys 1960-2006 (Ruggles et al., 2010). See Ottaviano and Peri (2012) for details on sampling and data treatment.

2.1 – Immigrants' earnings heterogeneity

Table 1 documents the variation over time in US immigrant stocks by origin and in immigrants' wages by origin. It documents a steady increase in the shares of immigrants from the low- and middle income regions. The increase in the Latin American (includes the Caribbean) share is particularly strong, amounting to 37 percentage points of the immigrant workforce from 1960 to 2006. Looking at wages, high-income region immigrants' residual wages are consistently significantly higher than natives'. All other immigrant groups consistently earn less than natives, almost always with significant differences. Latin American immigrants have the lowest residual wages in all six years, and their difference to natives is always significant. The immigrant group that has increased the most over time is thus the one that consistently has the lowest wages.

Time spent in the US is also an important predictor of immigrant wages, at least for immigrants from low- and middle income countries. For example, in the year 2000 the average recently-arrived Mexican immigrant worker with exactly high school education and 21-25 years of potential work experience earned only slightly more than half of what the average native worker with similar education and potential experience did. Yet the average similarly educated and experienced Mexican immigrant that arrived 21-25 years earlier earned about the same as the native worker.

2.2 – Estimation strategy

This study investigates the role of immigrant earnings heterogeneity and compositional effects of immigration in creating the observed negative correlation in time series between supply of and returns to immigrated labor. This is done by controlling for compositional factors in regressions that are similar to those performed by OP, i.e., by adding control variables and by analyzing subsamples of immigrants separately. Following a long tradition, OP estimated the elasticity of substitution between immigrant and native workers using the equation

$$\ln\left(\frac{W_I}{W_N}\right) = edex + year - \frac{1}{\sigma}\ln\left(\frac{H_I}{H_N}\right). \qquad (1)$$

Equation (1) expresses the average wage (W) ratio of immigrants (I) and natives (N) in the education-experience cell as a function of dummies for each education-experience cell (*edex*), and for each year, and of the ratio between total hours worked (H) by immigrants and natives in the cell. The regression coefficient on relative supply (hours worked) is interpretable as minus one over the elasticity of substitution between immigrant and native workers, given similar education and experience. The expected sign of the regression coefficient is thus negative. A crucial assumption is that relative supply only affects relative wages in the same cell, with no effects across cells.

In the regressions reported by OP, the coefficient on the relative supply of immigrant workers in Equation (1) has the expected negative sign. This is interpreted as evidence of less than perfect substitutability between immigrant and native workers. It is clear that the average immigrant wage is lower when immigrants are more numerous. What it yet is to be answered is whether this implies lower expected wages for actual individual immigrants or that when immigrants have been more numerous, larger shares of them have been from lower-earnings categories, driving down the averages. If the regression coefficient of interest stays negative and significant when immigrant composition is controlled for, this further supports the previous conclusion that the coefficient does measure the degree of substitutability. If it does not, compositional effects are found to be the driver of the previous result. It then remains to be analyzed whether the important immigrant earnings heterogeneities are themselves partly due to complementarities.

3 – Results

OP's estimation results in the pooled sample of men and women are replicated using data and program code from Giovanni Peri's website. These results are shown in row (1) of Table 2. The remaining rows add control variables for average years since immigration (YSM) among the immigrants in the cell, the fraction of immigrants who are from high income regions (Anglo America, Europe, and Oceania), and the fraction of immigrants who are from Latin

America.[1] We see that controlling for years since migration or shares of immigrants from the high-income regions has virtually no effect on the estimated coefficient of interest, which remains negative and significantly different from zero. Yet, when controlling for shares of immigrants being from Latin America, the estimated coefficients on relative immigrant supply are either insignificant or significant with the wrong sign. All indications of a functional negative correlation between relative supply and relative wages of immigrants disappear. These patterns also look very similar if the sample is restricted to the lower education groups, for which OP identified the most important immigrant-native complementarities.

These results indicate that the share of immigrants in the education-experience cell has served as a proxy for the share of Latin American immigrants among the immigrants in the cell, since the share of Latin Americans among immigrants has been strongly correlated with the share of immigrants in the workforce. The residual (regressed on dummies for all education-experience cells, and all years) correlation between log relative immigrant supply and share of Latin Americans among immigrants is between 0.60 and 0.90 for each of the four education groups.

Another way to arrive at a similar conclusion is to substitute smaller subgroups of immigrants for all immigrants on the left hand side, yet not on the right hand side, of Equation (1). The coefficient of interest is then a measure of the effect of the share of immigrants in the workforce on the average wage of the smaller immigrant subgroup relative to natives. This measure is not interpretable as an elasticity of substitution, yet it serves another purpose. If there are immigrant-native complementarities in production, then the expected wage of the individual immigrant – relative to the average native – decreases with the share of immigrants in the workforce. If the total stock of immigrants is then partitioned into a number of subgroups, this decrease must by construction be visible for at least one of these subgroups. Otherwise the negative effect on the average immigrant wage must be due to changing weights of the subgroups on the immigrant average.

Rows (5) – (8) of Table 2 report the results of these modified regressions for four subgroups of immigrants defined by region of origin. We see that two point estimates are positive and two are negative, and nothing is significant. With no significant effect in any subsample, the conclusion is that the significant negative effect in the total sample in row (1) is rather due to changing weights of the four subsamples, i.e., a compositional and not a complementarity effect.

The crucial question that remains to be answered is then whether the low earnings of the Latin Americans are themselves due to complementarities. Possibly, it could be the case that although the average immigrant is a perfect substitute for the average native in production, the average Latin American immigrant is not. This question is easily answered by substituting Latin American immigrants for all immigrants, and non-Latin American immigrants and natives for natives in Equation (1). The resulting coefficient of interest is reported in row (9) of Table 2. It is very small, and far from providing any significant support for the hypothesis of complementarities between Latin American immigrants and other workers.

[1] While the measure of relative immigrant supply was expressed in logs, for the regression coefficient to be interpretable as minus one over the (constant) elasticity of substitution, the fractions of high-income region or Latin American immigrants are not logged as they reflect compositional effects on the averages of immigrant wages.

4 – Discussion

The results presented in this study do not imply that immigrant and native workers are indeed perfect substitutes. The opposite still seems to be a plausible hypothesis, as immigrants are substantially more concentrated in some occupations than in others (Peri and Sparber, 2009). Yet the existence or magnitude of immigrant-native complementarities should not be seen as established by the time series evidence presented in previous studies. A plausible reason for why this empirical strategy fails to identify complementarities is likely to be found in the construction of the aggregates. By the empirical specification, each of the 32 education-experience cells is in fact treated as an isolated labor market. This has strong implications: for example wages of high school dropouts with 21-25 years of experience are *completely* insulated from supply of high school dropouts with 15-20, or 26-30, years of experience. A more plausible empirical setup would have to allow for a richer variety of cross-effects.

The method of estimating/simulating wage effects of immigration in structural frameworks, with assumed national-level production functions and parameters estimated from time series data, was originally used by Borjas (2003). It was an alternative to earlier more direct estimation methods, which suffered from potentially serious endogeneity issues. OP later highlighted the lack of robustness of Borjas' results. In turn, Borjas, Grogger, and Hanson (2012) highlighted the lack of robustness of OP's results. Dustmann and Preston (2012) have also questioned the construction of the education-experience cells. The structural method is also based on quite strong assumptions regarding stability in parameter trends in the production functions, and crucially that parameters do not respond to immigrant inflows (evidence that production technology does respond to immigration is presented by Lewis, 2003). Adding the findings of this study, i.e., that compositional effects can importantly bias the results, it does seem questionable that this new method has indeed produced any more trustworthy results.

The findings in this study also more generally highlight a weakness in previous literature on immigration and labor markets: too often ignoring that since immigrants of different origin perform very differently on the labor market their presence should be expected to affect other workers differently. This issue ought to be better accounted for in future research on the subject.

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Tables

Table 1. Immigrant shares and wages by origin

	Share of immigrant workforce %					
Origin	1960	1970	1980	1990	2000	2006
AA, Eu, Oc	81	63	40	26	19	18
Latin America	13	25	37	45	52	50
Asia	6	11	21	27	27	29
Africa	0	1	2	2	3	4
	Residual wage relative to natives %					
AA, Eu, Oc	107	107	103	108	107	107
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Latin America	77	83	81	85	85	80
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Asia	85	86	85	92	96	96
	(0.00)	(0.00)	(0.00)	(0.00)	(0.08)	(0.06)
Africa	88	93	88	93	91	85
	(0.06)	(0.20)	(0.00)	(0.00)	(0.00)	(0.00)

Notes: "AA, Eu, Oc" refers to Anglo America, Europe, and Oceania. Latin America includes the Caribbean. Residual wages relative to native wages are given by one minus the coefficients on dummies for each region in a log weekly wage regression – for full time workers only – that includes a dummy for each education-by-experience cell. P values in parentheses refer to the significance of the estimated dummy parameter. Standard errors are clustered at the education-experience cell.

Table 2. Effects of relative immigrant supply on relative immigrant wages

Specification	(1) All workers	(2) Full time workers only
(1) Original	–0.026*	–0.039***
	(0.015)	(0.013)
(2) + YSM	–0.041**	–0.054***
	(0.017)	(0.015)
(3) + AA, Eu, Oc	–0.033***	–0.045***
	(0.011)	(0.009)
(4) + Latin	0.047***	0.037**
	(0.016)	(0.015)
(5) AA, Eu, Oc subsample	0.011	0.005
	(0.014)	(0.010)
(6) Latin subsample	–0.004	–0.007
	(0.024)	(0.020)
(7) Asia subsample	0.021	0.018
	(0.027)	(0.019)
(8) Africa subsample	–0.069	–0.086
	(0.046)	(0.060)
(9) Latin complementarity	–0.004	–0.004
	(0.026)	(0.020)

Notes: Each cell reports the estimate of the parameter of interest from a separate regression. Robust standard errors in parentheses. n=160 when YSM is included (no data from 1960); n=192 otherwise. *, **, and *** refer to significance at the 10%, 5%, and 1% levels respectively. Observations are education-experience cells. Fixed effects for education-experience cells and for years are included. AA, Eu, Oc refer to Anglo America, Europe, and Oceania.

Paper III

Native and immigrant comparative advantage in manufacturing

Abstract

This study investigates how immigration affects the types of manufacturing jobs that are performed by natives. Previous research has shown that immigration induces natives to specialize in more communication-intensive jobs, yet that this is not so in the manufacturing sector, possibly due to immigration being primarily a substitute for offshoring in this sector. Using geographical variation in immigrant shares of the workforce by industry the present study finds that immigration does induce native specialization in – and increasing returns to – communication-intensive jobs also in manufacturing. These effects are driven by Latin American immigration. Latin Americans are the only immigrants that specialize in less communication-intensive manufacturing jobs than natives. Like natives, non-Latin American immigrants also specialize further in communication-intensive jobs when the presence of Latin American immigrants is higher.

I am grateful to Arne Bigsten for helpful comments.

1 – Introduction

The average immigrant in any high-income country is most plausibly less skilled than the average native in communicating in the country's dominant language. Comparative advantage thus predicts that immigrants will be employed in occupations that require less language and communication skills to a larger extent than natives, while natives will specialize in more language- and communication-intensive occupations. Furthermore the degree of native specialization, i.e., the communication-intensity of the average native job will increase with the share of immigrants in the workforce. This mechanism is most plausibly a main vehicle for reducing competition between immigrants and natives on the labor market and hence the negative wage effects of immigration (Peri and Sparber, 2009).

These two predictions have been verified empirically by Peri and Sparber. However, Ottaviano, Peri, and Wright (forthcoming) predict that the same should not happen in manufacturing industries. This is because of the high prevalence of offshoring in the manufacturing sector. Offshored jobs – which cannot be directly observed – are assumed to be more communication-intensive than immigrant jobs, yet less so than native jobs. Hence the presence of offshoring insulates the job distribution of native workers from being affected by the presence of immigrant workers. In line with this prediction the authors find no empirical support for any effect of immigration on the within-industry yearly variation 2000-2007 in native job composition in US manufacturing industries.

The conclusion that immigration has been primarily a substitute for offshoring in the manufacturing sector is of first-order importance for how we should expect immigration to affect natives' task specialization and wages more generally in the future when offshoring will presumably be more prevalent in most sectors of the economy. It is therefore important to investigate if a different choice of empirical strategy to test the theoretical predictions leads to the same conclusion. The present study tests the predictions of Ottaviano, Peri, and Wright using cross-sectional geographical within-industry variation in the presence of immigrants and native job composition instead of within-industry variation over time. This strategy has the advantage of providing substantially more variation in the independent variable. On the other hand it raises well-known questions about estimation bias from possible endogeneity of immigrants' settlement patterns and/or firms' choices of production technology (Borjas, 2003; Card, 2005). Yet the results reported in this study generally point in the opposite direction. Hence if any such bias is present it implies that the true effects are even larger than those that will be reported, and the issue of endogeneities cannot change the qualitative conclusions of the study.

2 – Data and method

This study uses individual-level data from the US 2000 census (Ruggles et al., 2010), which contains information on the individual's country of birth, occupation, industry, education, and income. Data on the importance of language and communication skills in performing each occupation is sampled from cycles 1-12 of the O*NET Analyst Occupational Abilities Ratings reports (the most recent cycle is used when an occupation was included in multiple cycles). These reports code the oral comprehension, written comprehension, oral expression and written expression requirements (not actual worker skills) for 433 occupations that are found in the census. The remaining 46 occupations found are recoded as the most similar occupation that is included in the O*NET reports. The measure of language and communication requirements used in this study is the average of the four codes for each occupation.

Individuals are coded as immigrants if they were born outside the US. Further they are coded as Latin American immigrants if they were born in a Latin American or Caribbean country. Effects of Latin American immigration will be analyzed separately since Latin American immigrants on average take jobs with substantially lower communication skill requirements than other immigrants. Average communication skill requirements for all workers, natives, all immigrants, Latin American and non-Latin American immigrants are shown in Table 1, with columns for all industries and only manufacturing industries.

Table 1 shows that the communication requirement of the average immigrant job is about one-fourth of a standard deviation below that of the average native job. We also find that this difference is entirely due to the Latin American immigrants. The requirement of the average Latin American immigrant job is about half a standard deviation below that of the average native job, while that of the average non-Latin American immigrant job is actually slightly above that of the average native job. These results are highly similar in both columns. The fact that non-Latin American immigrants on average have jobs that are as communication-intensive as natives' should not be surprising. It corresponds well to the common finding in the literature of positive selection of immigrants (Chiswick; 1978, 1999).

The task of this study is to investigate the effect of immigration – and Latin American immigration in particular – on native specialization in communication-intensive jobs. To investigate the effect of the share of immigrants on native specialization workers are grouped into cells defined by industry at the four-digit level and state, excluding Alaska, Hawaii, and the District of Columbia. The estimation equation is:

com=share+metro+industry4

The unit of observation is an industry-by-state cell, *com* is the average communication-intensity index, *metro* is the share of workers living in a metropolitan area, and *industry4* is a vector of industry dummies. The use of geographical variation in the presence of immigrants to identify effects of immigration hinges on the assumption of the exogeneity of immigrant settlements to the dependent variable being studied (Borjas, 2003; Card, 2005). In the present context, failure of this assumption to hold would imply that immigrants would concentrate more in states where industries use less communication-intensive technologies, and/or that industries choose less communication-intensive technologies in states with more immigrants. Such endogeneities would create a negative correlation between the share of immigrants and the communication-intensity of the average job, averaged over all workers. This hypothesis is easily tested. Yet in the present case we are searching for a *positive* correlation between the share of immigrants and the communication-intensity of the average *native* job. Hence to the extent that such a positive correlation is identified, it will represent a lower bound of the true effect.

3 – Results

Effects of immigrant shares and Latin American immigrant shares of the workforce in a cell on communication-intensities of jobs of different groups of workers are presented in Panel A of Table 2. Cells are weighted by the numbers of observed individuals in the cells. As expected, columns (1) and (2) show negative correlations between the shares of immigrants and communication-intensity, when communication-intensity is averaged over all workers. Plausibly this is due to settlement and/or technology endogeneities. However, the interesting result is that columns (3) and (4) show positive effects of immigrant shares on *native* average communication intensity. Hence in spite of the fact that a given industry uses less communication-intensive technologies overall in the presence of more immigrants, natives have more communication-intensive jobs. The magnitudes of the estimates imply that an increase in the immigrant share or Latin-American immigrant share by ten percentage points of the workforce increases the native communication-intensity index by slightly more than 0.02. This magnitude can be compared, e.g., to the standard deviation of average communication-intensities in manufacturing across states, which is 0.07.

Columns (5) and (6) show effects on the communication-intensity of the average non-Latin American immigrant. There is no significant effect of the total immigrant share, which is reasonable as the non-Latin Americans are themselves part of that share. Yet there is a positive effect of the share of Latin American immigrants that is highly significant and of about the same magnitude as their effect on natives. Hence as Latin American immigrants take the less communication-intensive jobs they induce specialization in more communication-intensive jobs among both natives and other immigrants.

Columns (7) and (8) show effects on the least educated natives, i.e., those with less than a high school degree. Even these columns contain significantly positive estimates, i.e., even the least educated natives specialize in more communication-intensive jobs when the share of immigrants in the workforce is higher.

Positive effects on communication-intensities are only to some extent accompanied by positive effects on wages. Effects of immigrant shares on average log weekly wages for fulltime workers (those working at least 40 weeks per year and 35 hours per week) are shown in Panel B of Table 2. An increase in the share of immigrants by ten percentage points implies a two per cent increase in average native wages. Yet no significant effect of Latin American immigration is identified. Estimated effects on non-Latin American immigrants' wages are negative and partly significant, and in one case the effect on the least educated natives' wages is negative and almost significant. It is not possible to distinguish whether these negative coefficients are due to settlement and/or technology endogeneities or represent actual effects. The latter is not impossible or necessarily in conflict with the results in Panel A. Even though Latin American immigration implies that non-Latin American immigrants and the least educated natives take more communication-intensive jobs, the competition from the former group could still imply that they suffer a wage decrease.

The results reported here change very little with the exclusion of California, which is by far the state with the largest share of immigrants. They also change very little if the communication-intensity index is recoded as the maximum rather than the average of its four component values. One may argue that the maximum could be a better measure of requirements as it might matter little, e.g., if the written expression requirements of a job are low if the oral expression requirements are still high.

3.1 – Effects on natives' returns to communication intensity

We have thus established that the presence of immigrants increases native specialization in communication-intensive jobs and also increases natives' average wages, yet not the wages of the least educated natives. According to theory the mechanism behind these effects is an increase in natives' returns to communication, which induces specialization. Effects of the presence of immigrants on natives' returns to education are investigated by estimating the equation:

$$ln(w)=com+edu+metro+share+com*share+edu*share+com*metro+edu*metro+industry4$$

over all fulltime native manufacturing workers. *ln(w)* is the logged weekly wage, *com* is the communication-intensity index, *edu* is years of education, *metro* is a dummy for living in a metropolitan area, *share* is the share of immigrants in the four-digit industry and state where the individual works, and *industry4* is a vector of industry dummies. The coefficient of interest is that on the interaction between communication intensity and the immigrant share, which is expected to be positive. The remaining interaction terms are included to ensure that the coefficient of interest does not pick up effects of education or metropolitan status, since education is positively correlated with communication-intensity and metropolitan status is positively correlated with the share of immigrants.

Results are shown in Table 3, with columns for whether the presence of immigrants is measured by all immigrants or Latin American immigrants only. As expected we see that returns to communication-intensity rise with the presence of immigrants in both columns. These results also change very little with the exclusion of California or with recoding of the communication-intensity index.

4 – Discussion

This study has shown that the language- and communication-intensity of native manufacturing jobs and natives' returns to communication-intensity rise with the presence of immigrants. These results are in line with those of Peri and Sparber (2009) for all US workers, yet not with those of Ottaviano, Peri, and Wright (forthcoming) for the manufacturing sector. The identification strategies used to produce these contrasting results are quite different. Ottaviano, Peri, and Wright relate year-to-year differences in native communication-intensity in 2000-2007 to year-to-year variation in the predicted (instrumented) presence of immigrants by industry and find no significant effect, positive or negative. This might be because year-to-year variation in the presence of immigrants over seven years is not that large. The variation in immigrant shares in the present study is substantially larger, which may explain why the expected positive effects are still found in this study. There are even signs that settlement and/or technology endogeneities bias the results in the negative direction, i.e., that the coefficients that are estimated in the present study understate the true effects.

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Tables

Table 1. Average communication skill requirements by worker category

	All industries	Manufacturing industries
All workers	3.47	3.28
Native workers	3.49	3.30
Immigrant workers	3.36	3.16
Latin American immigrants	3.20	3.03
Non-Latin American immigrants	3.52	3.31

Note: The standard deviation of the communication skill index for all workers is 0.50 in both columns. The minimum value is 2.2225 and the maximum value is 4.44.

Table 2. Effects of immigrant shares on communication-intensity and wages

	Affected group							
	(1) All workers	(2) All workers	(3) Natives	(4) Natives	(5) Non-Latin	(6) Non-Latin	(7) Least educated natives	(8) Least educated natives
	Panel A: Communication intensity							
Immigrant share	–0.045*		0.262**		0.039		0.114**	
	(0.035)		(0.000)		(0.485)		(0.000)	
Latin share		–0.155**		0.209**		0.267**		0.083*
		(0.000)		(0.000)		(0.000)		(0.039)
Metropolitan share	0.127**	0.138**	0.127**	0.160**	–0.038	–0.070**	0.028*	0.045**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.143)	(0.004)	(0.043)	(0.001)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,491	2,491	2,487	2,487	1,942	1,942	2,257	2,247
	Panel B: Wages							
Immigrant share	–0.119**		0.239**		–0.198*		0.006	
	(0.001)		(0.000)		(0.013)		(0.910)	
Latin share		–0.387**		0.024		–0.139		–0.132
		(0.000)		(0.613)		(0.197)		(0.056)
Metropolitan share	0.286**	0.312**	0.291**	0.346**	0.255**	0.227**	0.169**	0.191**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,456	2,456	2,445	2,445	1,815	1,815	2,090	2,090

Note: * and ** denote significance at the 5% and 1% levels. Observations (cells) are weighted by the number of individuals observed in the cell.

Table 3. Effects of presence of immigrants on natives' returns to communication-intensity

	Measure of immigrant share	
	All immigrants	Latin American immigrants
com	0.27**	0.28**
	(0.000)	(0.000)
edu	0.05**	0.06**
	(0.000)	(0.000)
metro	–0.23**	–0.29**
	(0.000)	(0.000)
share	–1.02**	–0.81**
	(0.000)	(0.000)
com*share	**0.21****	**0.27****
	(0.000)	**(0.000)**
*edu*share*	0.04**	0.006
	(0.000)	(0.536)
*com*metro*	0.02**	0.03**
	(0.010)	(0.001)
*edu*metro*	0.02**	0.02**
	(0.000)	(0.000)
industry4	Yes	Yes

Notes: N=447,692. * and ** denote significance at the 5% and 1% levels. The sample consists of all native individuals in the manufacturing industries who work at least 40 weeks per year and 35 hours per week. Standard errors are clustered at the state level.

Paper IV

Immigration, education, and earnings

Abstract

I simulate relative wage effects of immigration in a partial equilibrium framework derived directly from a human capital earnings function. While previous literature has found immigration to substantially decrease the wages of the least educated workers, I find that immigration either has had a neutral effect on wages, or has favored the least skilled workers, possibly by several percent. The supply-side predictor used in the model has significant power to explain historical trends in relative wages since the 1960s, whereas predictors of the kind used in previous literature have no significant explanatory power when the two are used simultaneously.

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1 – Introduction

A recent influential strand of literature estimates relative wage effects of US immigration in structural frameworks. The method was pioneered by Borjas (2003) and further developed by Ottaviano and Peri (2012), and it was motivated as a way of avoiding the endogeneity bias that was potentially present in studies using variation across cities to identify wage effects of immigration. It relies on assuming a functional form for an aggregate production function, where the factors of production are workers with different levels of education and the functional parameters are estimated from time series data.

In contrast to the larger literature estimating wage effects of immigration from cross-sectional immigration and wage data, the structural studies conclude that immigration has substantially affected the distribution of earnings across workers with different amounts of education, especially hurting high school dropout workers. Dropout workers are most negatively affected as the relative inflow of immigrants has been largest in this group. However, the magnitudes of the estimates produced in the different studies vary a lot, with the effects estimated by Borjas being several times larger than those estimated by Ottaviano and Peri.

The differences in estimated outcomes resulting from different structural models highlight the importance of appropriately specifying the functional relation between a worker's level of education and marginal productivity, where the latter is equal to the wage. Most of the literature on the relation between education and earnings is based on a human capital earnings function, describing a very simple functional relation between education and average earnings (Card, 1999). Yet, in studies explicitly focusing on *differences* in earnings, such as the literature on wage effects of immigration or on skill-biased technological change (Katz and Murphy, 1992; Autor, Katz and Kearney, 2008), it has been common to use structural frameworks where workers at two or four levels of education constitute distinct inputs in the production process. A fundamental difference between these two approaches is that in a (partial) equilibrium version of the former, differences in earnings between two groups that differ in amounts of education are determined by the total distribution of human capital supply in the economy, while in the latter case they are primarily determined by the supply relation of the two groups.

In this study, I first derive a structural framework from a human capital earnings function. I then examine the performance of the two approaches by relating variation in earnings gaps between eight different pairs of skill groups simultaneously to variation in supply within the pair and to aggregate human capital supply variation. The results profoundly support the human capital measure as the relevant explanatory factor. For all eight pairs examined, variation in human capital supply is strongly correlated with earnings gap variation, while, after controlling for human capital variation, supply variation within the pair does not have significant prediction power with the expected sign in any of the eight cases. The results thus strongly indicate that correlations between supply variation and earnings gap variation within skill group pairs, as estimated in previous literature, can be regarded as proxies for the aggregate human capital supply variation in the economy, and that aggregate human capital supply constitutes a better foundation for a structural model of how workers with different amounts of education are combined in the production process.

These findings have strong implications for the outcomes of simulations of relative wage effects of immigration. As the distributions of immigrants and natives over skill groups are quite different, with immigrants most of all being more concentrated among high school dropouts, earlier studies using models that are more sensitive to supply peaks in the education spectrum have estimated important negative effects of immigration on dropouts' relative earnings. Yet, under the assumption that immigrants and natives with the same level of education supply the same amount of human capital, and since immigrants are more concentrated than natives also among the best educated workers, the average amounts of human capital per immigrant and native workers are almost equal, implying hardly any effects at all of immigration on relative wages. This aggregated result masks important differences in effects of immigration by origin, with Latin American immigration being less skilled and thus favoring the wages of the more skilled, while the opposite holds for immigrants from all other regions. Allowing human capital per level of education to differ between natives and immigrants in the analysis, it is estimated that immigration has instead increased the relative wages of high school dropouts by 1-4%, depending on the time frame chosen. Thus, the group found in earlier studies to lose the most are here found not to be losers, and possibly even large winners.

2 – Studying education and earnings

Studies of the relation between education and earnings form a particularly large subfield of economics. In this literature, the large majority of studies apply the human capital earnings function, due to Mincer (1974), where the logarithm of the wage is a linear function of years of education:

$$ln(y) = a + bS \; (+ \; control \; variables) + e \qquad (1)$$

This specification has proved successful in describing the relationship between education and earnings for each year of the last five decades.

An alternative specification, which since Katz and Murphy (1992) has been popular in studies of the earnings gap between workers with different amounts of education, is to consider two groups of workers, e.g., “high skilled” (college degree) and “low skilled” (no college degree), forming a labor aggregate through a constant elasticity of substitution (CES) function, which produces output together with physical capital:

$$Y = K^{\alpha} X^{1-\alpha} \qquad (2)$$

$$X_t = \left(\theta_t H_t^{\frac{\sigma-1}{\sigma}} + (1-\theta_t) L_t^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \qquad (3)$$

where H is “high skilled” workers, L is “low skilled” workers, θ is a technology parameter, and σ is the time-invariant elasticity of substitution. In this framework, the relative wages of the two skill groups are given by their relative supplies, the elasticity of substitution, and the technology parameter, according to

$$\ln\left(\frac{w_H}{w_L}\right)_t = const + \ln\left(\frac{\theta_t}{1-\theta_t}\right) - \frac{1}{\sigma}\ln\left(\frac{H}{L}\right)_t + e_t \qquad (4)$$

which can be estimated from time series data with chosen restrictions on the behavior of the technology parameter, normally that the term is linear, or piecewise linear, in time.

It is clear that Equation (3) discards much of the earnings variation compared with the human capital earnings function in Equation (1). However, at this cost it gains a simple structural framework that can be directly applied in analyzing the behavior of the technology parameter, such as in the skill-biased technological change literature, or in simulating wage effects of counterfactual supply, such as counterfactual immigration levels, by inserting counterfactual values of H and L into Equation (3) or (4).

2-1 – Immigration, education, and earnings

In the context of analyzing the wage effects of immigration, Equation (3) may represent a too high level of aggregation. A persistent feature of US immigration is that immigrants, particularly those from Latin America, are overrepresented among workers without a high school degree. This motivates a structural framework that disaggregates group L into workers with and without a high school degree. Such a framework is used by Borjas, in a framework where four groups of workers, i.e., high school dropout workers (henceforth D), high school graduate workers (G), workers with some college education but no degree (S), and college graduates (C), are combined in a CES structure to form the labor aggregate X:

$$X = \sum_{i=D,G,S,C} \left(\theta_i X_i^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}, \ \sum \theta_i = 1 \tag{5}$$

Borjas estimates the parameters of this structure from time series data, and simulates that the US immigrant inflow 1980-2000 decreased the wages of high school dropouts by a whopping 8.7% in the central specification, of which 5.5% is a relative effect due to relative supply of different skill groups in Equation (5) and 3.2% is due to the assumption of completely inelastic capital supply in Equation (2).

A major issue in Borjas' approach is the identical elasticity of substitution between all pairs of skill groups, which implies, e.g., that high school dropouts are no more easily substituted for high school graduates than for college graduates. More flexibility is allowed in a later study by Ottaviano and Peri (2012), who start from the specification in Equation (3), yet split each of the two aggregates in two. Thus, groups D and G are combined in one separate CES structure to form the "low skilled" group and groups S and C are combined in another to form the "high skilled group." Hence, Ottaviano and Peri estimate three different elasticities of substitution and three different technology parameters, and as expected, the elasticity of substitution between the "low skilled" and "high skilled" aggregates is a lot smaller than those within the aggregates, the former being around 2 and the latter around 10. Card (2009) also estimates similar parameter values in the same structural form, from cross-sectional data on metropolitan areas in the year 2000. Eventually, wage effects of immigration simulated in the framework of Ottaviano and Peri are substantially smaller than those obtained by Borjas. The negative relative effect on the wages of high school dropouts is around 1%, to be compared with the 5.5% in Borjas' study. The differences between the results highlight the importance of correct specification of functional forms and the sensitivity of the results to the chosen specifications.

3 – Human capital and earnings gaps

While building aggregate production functions with distinct skill groups as factors of production is a simple way to obtain a model, it imposes sharp limits on the amount of complexity allowed. The specification of Katz and Murphy, in Equations (3) and (4), proved highly effective in explaining the evolution of the earnings gap between "high skilled" and "low skilled," at least between the 1960s and the 1980s, although its later performance is slightly poorer. This is potentially due to the fact that its high degree of simplification is unable to capture differing supply and returns trends within the "high skilled" group, namely between workers with post-college degrees and those with "only" college degrees (Autor, Katz, and Kearney, 2008). Yet with more than two skill groups it becomes a lot more complicated to define an appropriate structural form, with potentially important consequences as indicated by the different wage effects of immigration simulated by Borjas and Ottaviano and Peri.

On the other hand, while the human capital earnings function performs very well in relating individual variation in earnings to individual variation in education, it does not specify the production technology that causes this relationship. Without a specification of the production side, it cannot in itself be used in counterfactual analysis. In this section, I outline a partial equilibrium framework, constructed explicitly to be in accordance with a human capital earnings function (henceforth HCEF). I show that this model consistently outperforms production frameworks built on distinct skill groups in explaining historical trends in relative wages of different skill groups. After controlling for variation in the proposed measure of human capital supply, variation in supply within a pair of skill groups is never significantly correlated with the expected sign with variation in earnings gaps between the skill groups.

3.1 – A partial equilibrium model of human capital and output

Several forms of the HCEF exist in the literature, but the most commonly applied version is

$$ln(y) = a + bS + e$$

The simple linear coefficient on years of education has been doing well for many years. Yet, on data for each single year from the 1960s until today, the model performs even better if allowing a kink at eleven years of education. For many years, the difference is not large, but since around 1990 it is more pronounced. This is visible in Figure 1, which shows logarithmic hourly wages by years of education in 1980, 1990, and 2010. The data is from the March Current Population Surveys (King et al., 2010) and includes all workers aged 18-64 who are in the labor force but are not self-employed and who report positive earnings and that they work at least 35 hours per week and 40 weeks of the year. The kink at 11 years of education is very evident in the 1990 and 2010 lines. Yet, although a kink at 11 years is not as visually obvious from the 1980s and back, in an augmented HCEF of the form

$$ln(y) = a + bS + c(max[S-11,0]) + e \qquad (6)$$

the coefficient c is positive and significant with $p < 0.001$ in each year from 1964 to 2010.

The choice of a logarithmic transformation of the dependent variable in the HCEF reflects partly the theoretical foundations proposed by Mincer (1974), partly the normal distribution of the dependent variable thus obtained, and partly other factors as well (Card, 1999). However, the difference in performance between using and not using the logarithmic transformation is very small. This is shown in Figure 2, which displays the R^2 statistics obtained from yearly regressions of Equation (6) from CPS 1965 to 2011, with and without the logarithmic transformation. Obtaining R^2 values from the regression without logs that are directly comparable to those from the regression with logs is done in two steps. First, Equation (6) is estimated without the logarithmic transformation of y, and predicted values are stored. Second, actual log wages are regressed on the logarithms of the predicted values from the first regression, and R^2 is obtained from the second regression. As seen in Figure 2, the difference between the R^2 values of the HCEFs with and without the logarithmic transformation is in most cases too small to be discernible in the graph. For the purpose of building the model applied in this paper, not applying the logarithmic transformation greatly simplifies matters and, as indicated by Figure 2, this departure from convention does not imply any significant loss in precision.

Average hourly wages by education in 1980, 1990, and 2010, without the logarithmic transformation, are shown in Figure 3. Two important observations can be made. First, the kink at 11 years of education is again clearly visible in the 1990 and 2010 lines. Thus, at least in later years, we cannot properly summarize returns to education by one number only, but rather by one number for the first 11 years and another number thereafter. Note that the latter number is very close to what would come out of a simple regression with only a linear education term as in Equation (1). This is because in later years there are relatively few workers with less than 11 years of education and hence the regression line is predominantly fit to the slope at >11 years.

In the present case, the importance of the kink is not its existence per se, but the fact that slopes on the left and right sides of the kink move in different directions over time. This is shown in Figure 4, which shows the evolution of the slopes over time, i.e., of the coefficients b and c in

$$y = a + bS + c(max[S-11,0]) + e \qquad (7)$$

As Figure 4 shows, a large difference between returns to the first 11 years of education and to later years is present already in the 1960s, and it increases sharply from around 1990. This variation is not so easily reconciled with the theory that students only acquire one kind of human capital in school, unless the quality of higher education would have increased tremendously over time compared to lower education. The facts are more easily explained by considering two kinds of human capital: low level human capital (L), which is acquired in the first 11 years at school, and high level human capital (H), acquired in later years. Adopting this view, the differing trends in Figure 4 reflect changes in relative demand and supply of the two versions of human capital, i.e., increasing demand for skills.

The two kinds of human capital form the labor aggregate X, according to some production function f(H,L), and finally produce output together with physical capital:

$$Y = K^{\alpha} f(H,L)^{1-\alpha}$$

Returns to H are equal to the wage increase from an additional year of education for years above 11, which is estimated by the coefficient c in Equation (7). This simple matter is the main virtue of discarding the logarithmic transformation of y, and it in turn implies that each individual worker supplies $H = \gamma(max[S-11,0])$ per hour of work. As we have no interest in units of measurement, we can set $\gamma = 1$.

Returns to and supply of L is treated differently. Returns to the first 11 years of education are not only small, they are also difficult to measure adequately in later years. This is seen for example in the 1990 line in Figure 3. For low years of education, average wages do not rise monotonically with education in the data, implying large standard errors and most of all less robust results in the subsequent analysis. For these reasons, the variation in earnings and years of education for workers with less than 12 years of education, which is identical to group D (high school dropouts), will henceforth be discarded, and supply of L per hour of work is set equal to one for all workers, regardless of years of education. This implies the HCEF for estimation of returns to L and H:

$$y = a + c(max[S-11,0]) + e \qquad (8)$$

where returns to L are given by the intercept *a* and returns to H are given by *c*, without having yet specified the functional form of f(H,L). Thus, the model incorporates the information from a HCEF by construction. The evolution of returns to H and L over time is shown in Figure 5, where both returns in the first year are normalized to one. The gap in returns was quite stable in the 1960s and 1970s, increased in the 1980s and even more in the 1990s, and was stable again in the 00s.

3.2 – Evaluating supply-side predictors of earnings gaps

CES production functions have been popular in the literature on earnings gaps. In a CES function with two or more skill groups, such as Equation (3) or (5), relative earnings of two groups, A and B, are given by

$$\ln\left(\frac{w_A}{w_B}\right)_t = const + \ln\left(\frac{\theta_A}{\theta_B}\right) - \frac{1}{\sigma}\ln\left(\frac{A}{B}\right)_t + e_t \qquad (9)$$

of which Equation (4) is the two-group case, where $\theta_B = 1 - \theta_A$. In a nested CES function, such as the one used by Ottaviano and Peri, Equation (9) applies between and within the respective nests. With more than two skill groups, whether the function is nested, as by Ottaviano and Peri, or not, as by Borjas, a crucial assumption underlying Equation (9) is that supply of skill groups other than A and B is unimportant for relative earnings of A and B. So if A and B represent, e.g., high school dropouts and high school graduates, these are not allowed to be differentially substitutable for, say, college graduates. With only two skill groups, as in the study by Katz and Murphy, this problem does not exist as no group is left out, although at the cost of more information being discarded than with four groups.

In the human capital partial equilibrium framework outlined in the previous subsection, labor's contribution to output and relative earnings between groups of workers are defined only by aggregate supplies of low level and high level human capital, while the distribution of those aggregate supplies over more distinct worker groups is not important. It does not matter if a supply of say 6 units of H is provided by one worker alone or by two workers supplying 3 units each. This framework and that represented in Equation (9) thus represent two different assumptions about what is most important for relative wages. With four skill groups, the two views are clearly conflicting, as the human capital framework implies that groups other than A and B matter for the relative earnings of A and B. With two skill groups, there is not so much conflict between the two views. The difference is instead that the human capital framework accounts for more of the variation in the data, and also that it does not have to deal with where to draw the sharp dividing line between two distinct aggregates in a continuous distribution of years of education, where there is some variation in previous literature. Katz and Murphy divide group S between the aggregates, with more weight in the low skilled aggregate, while Ottaviano and Peri place it in the high skilled aggregate.

The performance of the two frameworks can be tested by adding the term $ln(H/L)_t$ to Equation (9), and thus regressing relative wages of groups A and B simultaneously on their own relative supplies and the relative supplies of the human capital aggregates H and L. This is done on March CPS data of 1965-2011 (King et al., 2010), for all six pairs that can be formed by the groups D, G, S, and C, and for two pairs representing "high skilled" and "low skilled" aggregates as in Equation (4). In the first version, group S belongs to the "high skilled" group, while in the second it belongs to the "low skilled." Following much of the earlier literature, average wages per skill group are calculated in the sample of workers aged 18-64 who are in the labor force but are not self-employed and who report positive earnings and that they work at least 35 hours per week and 40 weeks of the year, while average hours of work is calculated in a larger sample of all workers who are in the labor force and report positive earnings. All technology terms are assumed to be linear in time.

The results are shown in Table 1. They strongly support the human capital framework in all eight cases. The coefficient on relative supply of H and L has the expected sign and p values of 0.01 or lower in all eight regressions. It is also consistently larger in magnitude when the two skill groups are further apart in the education distribution, as is easily seen by comparing columns (1) – (3), all of which have the dropout wage in the denominator, or columns (3), (5), and (6), all of which have the college graduate wage in the numerator. The coefficients on relative supply within the skill group pair in question are always small, never significant with the expected sign, and in three regressions even significant with the wrong sign. As shown in Table 6, this coefficient is significant with the expected sign in six out of eight regressions when the human capital supply term is not included in the regressions. Thus, when these coefficients are significant, they are rather proxies for the aggregate variation in human capital supply, and the aggregate human capital supply matters more for relative wages between any two groups than do more limited measures of their relative supplies.

3.3 – Functional form and calibration

To be able to proceed to an analysis of wage effects of immigration, we must specify and calibrate the function *X=f(H,L)*, which describes how the supplies of H and L form an aggregate that produces output together with capital. I use two different functional forms: one CES and one generalized Leontief. The CES function is

$$X_t = \left(\theta_t H_t^{\frac{\sigma-1}{\sigma}} + (1-\theta_t) L_t^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$

It is estimated by Equation (9), assuming that the technology term is linear in time. The resulting estimates are

$$\ln\left(\frac{w_H}{w_L}\right)_t = -1.50 + 0.053 time - 1.89 \ln\left(\frac{H}{L}\right)_t + e_t$$

with all p values < 0.01 (Newey-West standard errors, one lag). The estimates imply that $\sigma = 1/1.89 = 0.53$, and θ_t is recovered from the constant and the time trend. The generalized Leontief function is

$$X_t = b_{H,t} H_t + b_{L,t} L_t + c\sqrt{H_t L_t}$$

which is estimated by the marginal productivity conditions

$$w_H = b_{H,t} + c\sqrt{\frac{L_t}{H_t}}$$

$$w_L = b_{L,t} + c\sqrt{\frac{H_t}{L_t}}$$

These are estimated as seemingly unrelated regressions, with the constraint that *c* must be equal in both regressions. Similar to the CES structure, technology shifts of the *b* parameters are assumed to be linear in time, while the parameter *c*, which plays a role similar to σ in the CES function, is assumed not to change over time. The resulting estimates are

$$w_H = -4.5 + 0.07time + 7.2\sqrt{\frac{L_t}{H_t}}$$

$$w_L = 4.7 - 0.18time + 7.2\sqrt{\frac{H_t}{L_t}}$$

with all p values < 0.01 (Newey-West standard errors, one lag).

4 – Effects of immigration on relative wages

In this section I use the functions calibrated in the last section to simulate effects of immigration on relative wages. Specifically, I compare the actual structure of wages in year T with the simulated counterfactual structure that would have been had immigration between T-20 and T not occurred. The effect on the wage w_A of group A is

$$\frac{w_{A,T} - w^{-}_{A,T}}{w^{-}_{A,T}}$$

where w^{-} is the simulated counterfactual with no immigration. Effects are evaluated in T=2000, of immigration in 1980-2000, and T=2010, of immigration in 1990-2010. The data is from the 1980-2000 censuses and the 2010 American Community Survey (Ruggles et al., 2010). Hours of work are summed over all individuals who are in the labor force and report positive earnings. Table 2 reports the effects of immigration on the supplies of and returns to skills H and L, as well as wage effects on skill groups D, G, S, and C, in the CES and the generalized Leontief functions. The effects may be thought of as effects on natives and earlier immigrants, as they do not account for composition effects within the groups, which are relevant to groups S and C that encompass more than one number of years of education.

While recent immigrants and the rest of the population have substantially different distributions across the four skill groups D, G, S, and C, their aggregate ratios of high and low level human capital are very similar, as shown in the top rows of Table 2. Hence, the simulated relative wage effects of immigration are very small, in fact never larger than 0.15% in absolute magnitude. These small effects look very different when studying immigration by origin. Table 3 shows results similar to those in Table 2, but for only 2000 and by immigrants' origin. The wage effects are given by the CES structure. The effects in the generalized Leontief structure are always 74-75% of the CES effects, and the effects in 2010 are higher than in 2000 for some origins and lower for others.

Table 3 reveals large differences by origin. At one extreme, Mexican immigration has been distinctly low skilled and has lowered the wages of dropouts by 1.4% but raised those of college graduates by 0.7%. At the other end, Asian immigration, where, e.g., India and the Philippines are important source countries, was distinctly high skilled, raising dropout wages by 1%, and lowering college graduate wages by 0.5%. European, Anglo-American, and African immigration were also predominantly high skilled, while Latin American was more low skilled also when not including Mexico. Immigration from Oceania was also more high skilled, yet was very small.

4.1 – Human capital differences between immigrants and natives

The results presented so far assume that immigrants and natives with the same number of years of education supply equal amounts of human capital in the labor market. Although this assumption is useful as a benchmark and for its robustness, it is likely to be incorrect for several reasons. Immigrants are no random samples of the populations in their countries of origin. For almost all countries in the world, emigrants are on average more skilled than the average inhabitant (Docquier and Marfouk, 2006). There are reasons to believe that emigrants are more industrious, and also less risk-averse, than others, simply because they are the ones who chose to migrate. The composition of immigrants is also affected by immigration policy. Importantly, immigrants were in most cases educated in their home countries and do not speak English fluently, which may imply that their human capital is less useful in the US than it was in their home countries. On the other hand, however, those who choose to migrate to the US may be those whose human capital is most useful there. Recent empirical studies also conclude that immigrants and natives with the same years of education are not perfect substitutes in production (Ottaviano and Peri, 2012, and Card, 2009, for the US; D'Amuri, Ottaviano, and Peri, 2010, for Germany; Manacorda, Manning, and Wadsworth, 2012, for the UK).

All these reasons justify an analysis of differences between natives and immigrants in human capital at given numbers of years of education, as well as of what those differences imply for the simulated wage effects of immigration. To do so, I estimate Equation (9) separately for immigrants and natives, obtaining the coefficients a_{native}, $a_{immigrant}$, c_{native}, and $c_{immigrant}$, which are used to calculate efficiency units of immigrants' human capital, as $EU_L=a_{immigrant}/a_{native}$, and $EU_H=c_{immigrant}/c_{native}$, respectively. There is no a priori hypothesis about whether these ratios are smaller or larger than one, as some of the factors listed above would make them smaller and others larger. Neither do they need to be either both smaller or both larger, as these factors are likely to be of different importance in different parts of the education spectrum.

Table 4 shows the estimated ratios in 1980, 1990, 2000, and 2010. They are estimated with high precision, with all p values < 0.0001. With quite small variation in magnitudes between the years, the results consistently indicate that migrants carry less low level human capital L, and more high level human capital H, than do natives, given years of education. This result is consistent with several different explanations. Push factors may plausibly be more important than pull factors in driving less skilled migration, while for the more skilled, pull factors may be more important, and those migrating may be those with the more appropriate skills to use in the US labor market. Also, the fact that EU_L is estimated smaller than one may simply reflect that immigrants on average have fewer years of education than natives, given that the number is smaller than 12. This is relevant and not a problem in itself, but could potentially bias the EU_H estimates upwards. However, estimating the same equations after deleting all workers with less than 11 years of education changes the estimates reported in Table 4 very little, indicating that this is not a problematic source of bias.

Table 5 shows simulated effects, similar to Table 2, allowing for efficiency units of immigrants' human capital. The simulated wage effects are substantially different. The wages of high school dropouts have increased and those of college graduates decreased by about one percent each due to immigration in 1980-2000 and even several percent each due to immigration in 1990-2010. These results stand in very sharp contrast to those of the earlier literature, which identified high school dropouts as the losers from immigration. In the present analysis, high school dropouts are not losers. Instead, they may in fact be winners, and possibly even large winners.

5 – Conclusion

In this paper, I have simulated relative wage effects of US immigration in a partial equilibrium framework derived from a human capital earnings function. The results indicate that either relative wages have been quite unaffected or the least educated workers have been the winners from immigration. These results greatly differ from previous literature that has concluded that immigration has mostly hurt the least educated workers. The key reason for the difference lies in the fact that effects simulated in the HCEF framework depend on the aggregate supply of human capital, whereas results in models previously used are sensitive to supply peaks in the spectrum of years of education, such as has been the case with late US immigration.

Comparisons of the human capital framework and previous frameworks used in the literature in terms of performance, in explaining historical trends in relative wages between skill groups, consistently support the human capital framework, and hence the credibility of the effects of immigration simulated therein.

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Tables

Table 1: Predictors of earnings gaps

	Earnings gap							
	Four groups						Two groups	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	w_G/w_D	w_S/w_D	w_C/w_D	w_S/w_G	w_C/w_G	w_C/w_S	S high	S low
x_A/x_B	0.15	0.12	0.05	0.03	-0.13	-0.05	-0.002	-0.12
	(0.00)	(0.00)	(0.66)	(0.03)	(0.30)	(0.09)	(0.97)	(0.15)
H/L	-0.66	-0.76	-1.03	-0.20	-0.55	-0.35	-0.55	-0.44
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Time	0.011	0.011	0.026	0.003	0.020	0.012	0.014	0.016
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Const	0.36	0.70	0.90	0.26	0.43	0.37	0.54	0.35
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.07)

Notes: The dependent variable is the log of the wage ratio of two skill groups. x_A/x_B refers to the individual (logged) supply ratio within the skill group pair. Standard errors are Newey-West, with one lag. P values in parentheses.

Table 2: Supply and wage effects of 20 years of immigration, in 2000 and 2010

Effect on	2000		2010	
H	9.00%		10.64%	
L	8.88%		10.77%	
	CES	Gen. Leontief	CES	Gen. Leontief
wage H	–0.15%	–0.11%	0.14%	0.11%
wage L	0.09%	0.08%	–0.14%	–0.11%
wage D	0.09%	0.08%	-0.14%	-0.11%
wage G	0.05%	0.04%	-0.07%	-0.05%
wage S	0.01%	0.01%	-0.02%	-0.01%
wage C	-0.05%	-0.04%	0.05%	0.04%

Table 3: Supply and wage effects of immigration in 2000, by origin

Effect on	Africa	Anglo America	Asia	Europe	Latin America	Mexico	Oceania
H	0.49%	0.26%	3.97%	1.50%	1.46%	0.71%	0.07%
L	0.34%	0.08%	2.74%	0.65%	1.94%	2.54%	0.05%
wage H	-0.18%	-0.20%	-1.45%	-0.99%	0.56%	2.17%	-0.02%
wage L	0.11%	0.13%	0.95%	0.64%	-0.36%	-1.36%	0.01%
wage D	0.11%	0.13%	0.95%	0.64%	-0.36%	-1.36%	0.01%
wage G	0.06%	0.07%	0.47%	0.32%	-0.18%	-0.69%	0.01%
wage S	0.01%	0.01%	0.08%	0.05%	-0.03%	-0.12%	0.00%
wage C	-0.06%	-0.07%	-0.48%	-0.32%	0.18%	0.69%	-0.01%

Notes: Effects are evaluated in the CES structure. Latin America does not include Mexico.

Table 4: Estimated efficiency units of immigrant human capital

	EU_L	EU_H
1980	91%	114%
1990	91%	106%
2000	89%	107%
2010	83%	113%

Table 5: Supply and wage effects, adjusted for efficiency units of immigrant human capital

Effect on	2000		2010	
H	9.26%		12.51%	
L	7.80%		8.39%	
	CES	Gen. Leontief	CES	Gen. Leontief
wage H	–1.82%	–1.40%	-4.31%	-3.20%
wage L	1.17%	0.94%	4.39%	3.27%
wage D	1.17%	0.94%	4.39%	3.27%
wage G	0.59%	0.47%	2.20%	1.64%
wage S	0.11%	0.09%	0.59%	0.44%
wage C	-0.58%	-0.45%	-1.40%	-1.04%

Table 6: Regression results of Equation (9) without the $ln(H/L)_t$ term, similar to Table 1:

	Earnings gap							
	Four groups						Two groups	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	w_G/w_D	w_S/w_D	w_C/w_D	w_S/w_G	w_C/w_G	w_C/w_S	S high	S low
x_A/x_B	-0.09	-0.07	-0.27	0.06	-0.71	-0.10	-0.33	-0.35
	(0.00)	(0.10)	(0.00)	(0.01)	(0.00)	(0.02)	(0.00)	(0.00)
time	0.007	0.010	0.027	-0.00	0.026	0.006	0.018	0.014
	(0.00)	(0.00)	(0.00)	(0.21)	(0.00)	(0.00)	(0.00)	(0.00)
const	0.11	0.11	0.19	0.20	-0.36	0.24	-0.07	-0.22
	(0.00)	(0.04)	(0.00)	(0.00)	(0.00)	(0.00)	(0.24)	(0.00)

Notes: The dependent variable is the log of the wage ratio of two skill groups. x_A/x_B refers to the individual (logged) supply ratio within the skill group pair. Standard errors are Newey-West, with one lag. P values in parentheses.

Figures

Figure 1: Log wages by schooling, 1980, 1990, and 2010

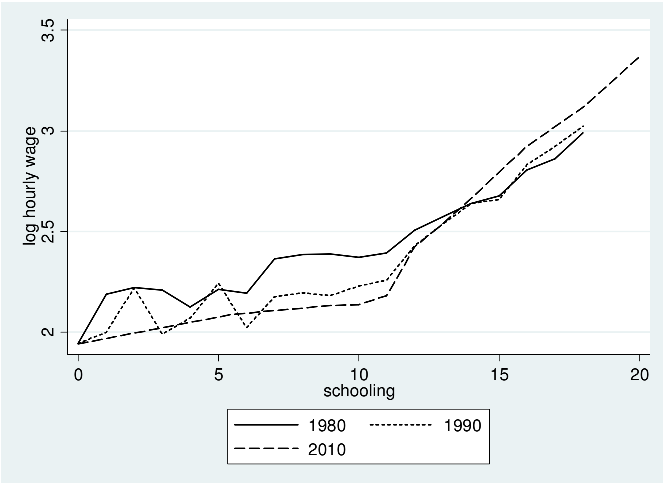


Figure 2: Degree of fit over time for HCEF with and without logs

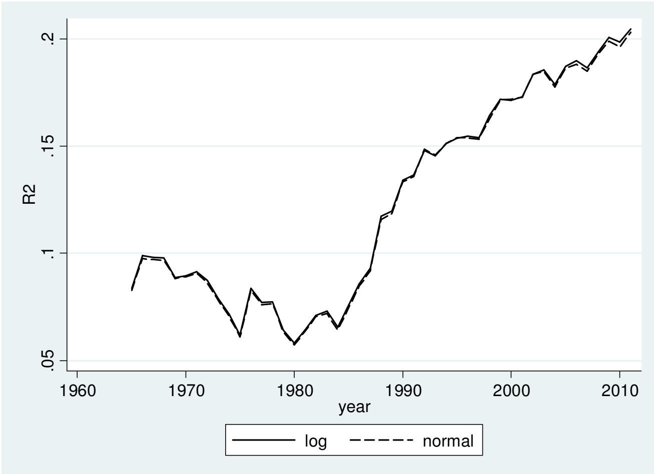


Figure 3: Wages by schooling, 1980, 1990, and 2010

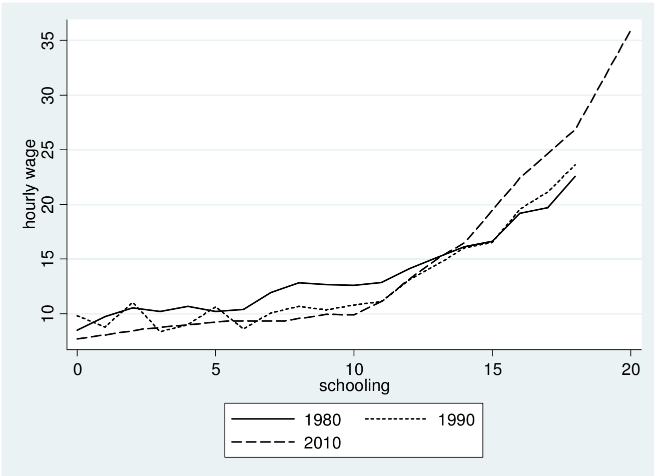


Figure 4: Returns to early and late schooling years over time

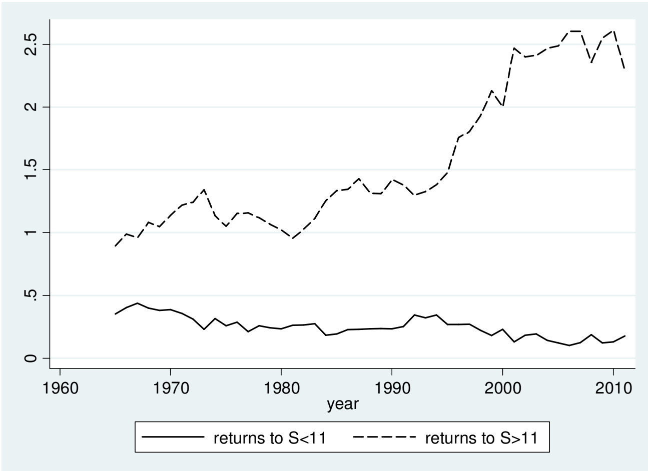
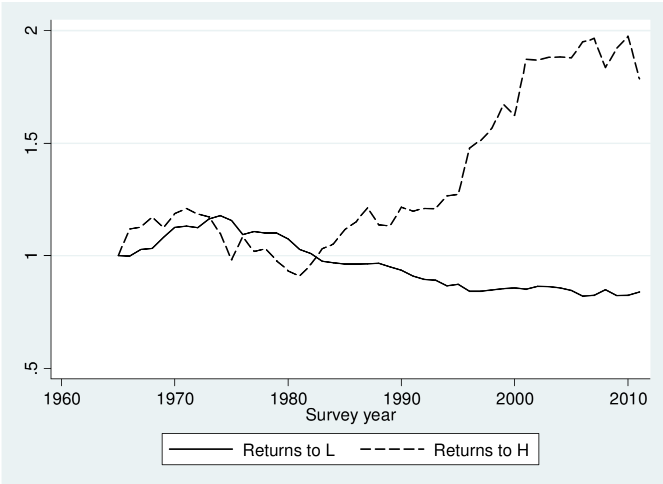


Figure 5: Returns to low and high level human capital over time (normalized)



Paper V

Average wage effects of immigration with endogenous saving and international capital mobility

Abstract

This study estimates average wage effects of immigration, accounting for the interplay between factor supply and factor prices. While immigration decreases native wages, it increases capital returns. Hence both domestic saving and capital inflows from abroad and in turn foreign saving increase. This in turn eliminates some of the wage effect. Yet there is a negative effect of immigration on average wages as long as immigration persists. At the current US immigration rate, this effect is estimated around 0.9%, i.e., a substantially smaller effect than in previous analyses treating capital supply as inelastic. However, total gains of immigration are substantially larger than losses to native workers. A tax reform is considered where immigrant workers would compensate native workers for their losses. The required tax surcharges for immigrants are found to be around 10% of gross income for between 20 and 30 years. Although these surcharges are quite large, most migrants who pay them would still make substantial income gains from migration.

I am grateful to Arne Bigsten for helpful comments.

1 – Introduction

Several influential studies from the last decade use structural models to estimate wage effects of immigration, focusing predominantly on *relative* wages: how wages of different groups move in relation to each other when the skill distribution of immigrants differs from that of natives. Although *average* wage effects should be no less important when evaluating the impact of immigration, less attention has been given to these.

In economic theory, the primary reason for negative effects of immigration on average wages is that immigration alters, i.e., decreases, the capital-labor ratio. In the most influential recent studies on wage effects of immigration, the extent to which this happens is determined by simple assumptions. The results of Borjas (2003) feature large average effects, due to the extreme assumption that capital does not adjust at all, over 20 years, to an immigrant inflow approximating 10% of the population. Another highly influential study, by Ottaviano and Peri (2012), instead uses the opposite assumption that capital eventually adjusts fully to an immigrant inflow, implying no long-term average effects at all, although possibly this adjustment takes time and short-term negative effects may appear. Since Ottaviano and Peri's objective is to analyze wage effects of a specific immigrant inflow over 16 years, i.e., from 1990 to 2006, their assumption should be reasonable; yet, importantly, it also implies that their results are not interpretable as effects of continuous inflows of migrants. Still, continuous and quite stable inflows of migrants is what we see in most high-income countries today, so to properly evaluate average wage effects of current immigration we should estimate the impact of these continuous inflow rates.

The objective of this study is to provide a modeling framework for estimation of average wage effects of stable migration flows. Core features of the framework are the extent of international capital mobility, and the link between the immigration rate and optimal saving decisions. Optimal saving is analyzed in a basic neoclassical growth model, where capital is treated as completely mobile between Anglo American and Western European countries and completely immobile between all other countries. In this setup, average wage effects of immigration are negligible in the smallest of the countries with mobile capital and largest in the US, which is the largest economy. Average wage effects of current US immigration rates are estimated at around 0.9%, so the effects are significant yet a lot smaller than what they would have been with fixed capital stocks. These effects are solely due to the effects of immigration on the capital-labor ratio and, hence, do not account for possible positive average wage effects of immigration due to potential productivity increases, or possible positive effects on average *native* wages due to potential immigrant-native complementarities in production (Ottaviano and Peri, 2012). In percentage terms, the average effect of US immigration also applies in all other Anglo American and Western European countries, whose capital stocks adjust equally to immigration in the dominating country. Thus, the average wages in small Western European countries may be more affected by US immigration than by their own immigration.

While native workers lose from immigration, total gains in the immigration country are substantially larger than total losses, implying scope for redistribution. In the last section of this article, a revenue-neutral tax reform in the immigration country is analyzed, where native workers are given tax-deductions to compensate for the negative wage effects of immigration, and these deductions are financed by tax surcharges for immigrant workers. The required tax surcharges for immigrants are found to be around 10% of gross income for between 20 and 30 years. Although these surcharges are quite large, most migrants who pay them would still make substantial income gains from migration due to the large income differences between the US and the lower income countries that send the majority of its immigrants.

2 – The model

The model is a standard neoclassical growth model, with endogenous capital accumulation and a certain degree of capital mobility. The simple treatment of the degree of international capital mobility is to treat capital as perfectly mobile between the high-income countries of Anglo America (the US and Canada) and Western Europe (EU15 plus Switzerland and Norway) and completely immobile between all other countries. Although sharp, this distinction should be a reasonable approximation to what empirical evidence on international capital market integration tells us. Formal controls on capital mobility between high-income countries were largely dismantled in the 1980s. Influential papers from a few years later on the degree of actual capital mobility between these countries following the dismantling (Frankel, 1992; Obstfeld, 1995) concluded that the degree of international mobility was high, although not perfect. More recent research (Lane and Milesi-Ferretti, 2007) has shown that since then, international financial integration between industrial countries has continued to increase strongly, in contrast to low- and middle-income countries, where also formal capital controls often remain. Gollin and Lange (2007) provide empirical estimates of how international capital flows respond to migration flows.

Capital is treated as being owned by a distinct group of capitalists. Capitalists do not work, and workers do not save. Since capital is allowed to move freely between certain countries, it is unimportant in which of these countries the capitalists live. The important point is that capitalists only care about their own utility, so changes in countries' populations due to migration only affect the production sides of their economies, not the numbers or distributions of agents making saving decisions. This setup is not only significantly easier to analyze than the alternative setup of Weil (1989), where capital is owned by workers and immigrants' capital ownership patterns converge to natives', but arguably also more relevant in that it represents the highly concentrated capital ownership structure in Western countries. What is to be analyzed is thus immigration of workers; the number of capitalists is constant.

Output in country i at time t is produced by

$$Y_{it} = K_{it}^{\alpha}\left(A_{it}L_{it}\right)^{1-\alpha},$$

where labor is homogenous and A_i is the country-specific level of labor-enhancing technology. Capital's share of income α is constant across all Anglo American and Western European (henceforth AE) countries. When capital is mobile between these countries, they have the same marginal capital return, implying that capital per effective worker k is at all times equal between all countries with mobile capital:

$$k_t = \left(\frac{K}{AL}\right)_{it} = \left(\frac{K}{AL}\right)_{jt}, i \neq j$$

The composition of the capitalist group is static, so capitalists optimize their behavior considering aggregated values of capital stocks, income, and consumption, represented by capital letters. They maximize utility with constant relative risk aversion (CRRA) subject to the capital accumulation constraint:

$$U = \int_{t=0}^{\infty} \frac{\left(C_t^K\right)^{1-\lambda}}{1-\lambda} e^{-\rho t} dt \quad , \tag{1}$$

$$\dot{K} = \varphi(K) - C^K \quad , \tag{2}$$

where C^K refers to total consumption of all capitalists, λ is the intertemporal elasticity of substitution, ρ is a discount factor, K is the total capital stock summed over all AE countries, and $\varphi(K)$ is total capitalist income from the same countries net of depreciation:

$$\varphi(K_t) = \alpha K_t^{\alpha} (AL)_t^{1-\alpha} - \delta K_t \, .$$

Maximizing (1) subject to (2) gives the optimal consumption path for capitalists:

$$\frac{\dot{C}^K}{C^K} = \frac{\varphi'(K) - \rho}{\lambda} \, . \tag{3}$$

The growth rate x of the total stock of effective workers in AE countries is the sum of three components: technology growth, population growth, and immigration. Technology growth and population growth in country i, as well as migration to country i from a non-AE country, increase the AE stock of effective workers by a factor equal to country i's share of that stock. By construction, this share is equal to the country's share of AE output, since α is the same across all AE countries. Migration between AE countries also affects the total stock of effective workers, since labor-enhancing technology levels differ between AE countries. Migration from country j to country i affects the total AE stock of effective workers by the difference (positive or negative) between the increase it implies in country i and the decrease it implies in country j. In sum, the growth rate of the total AE stock of effective workers is:

$$x = \frac{\sum_i (g + n + m)_i Y_i}{Y_{AE}} \tag{4}$$

summing over all AE countries. The parameter g is the technological growth rate, n is the crude population growth rate (births minus deaths), and m is the net immigration rate, which can be positive or negative. For a more convenient way to estimate the effect of immigration in country i on the total stock of effective workers, the effects of migration between AE countries can be incorporated by summing (4) only over countries with positive immigration rates, and adjusting the immigration rates according to:

$$m_i = \sum_j m_{ij} \left(\frac{A_i - A_j}{A_i} \right) , \tag{5}$$

where m_{ij} is the immigration rate in i originating in j. The parameters A_i and A_j are given by GDP per capita for AE countries, and A_j=0 for non-AE countries, since the capital stocks of these countries are immobile.

In steady state, (3) must be constant, implying that capital per effective worker is constant and thus that the total capital stock and capitalists' total consumption grow at the same rate as the total number of effective workers, x, which is given by (4). This implies expressions for steady state returns to capital, net of depreciation, and for capital per effective worker:

$$\varphi'(K) = \varphi'(k) = \lambda x + \rho$$

$$\varphi'(k) = \alpha^2 k^{\alpha-1} - \delta = \lambda x + \rho$$

$$k = \left(\frac{\lambda x + \rho + \delta}{\alpha^2}\right)^{1/\alpha-1} \qquad (6)$$

Labor income per effective worker is denoted w^*, and like capital returns per effective worker it is equalized between AE countries. The actual wage is different in different countries though, as:

$$w_i = A_i w^*.$$

Solving for labor income per effective worker:

$$w^* = (1-\alpha)\left(\frac{\lambda x + \rho + \delta}{\alpha^2}\right)^{\alpha/\alpha-1}. \qquad (7)$$

The wage effect in relative terms of immigration in country *i* is the same in all AE countries, and is given by inserting (4) into (7):

$$\frac{w_{i,m_i}}{w_{i,m_0}} = \frac{w^*_{m_i}}{w_{m_0}} = \left(\frac{\lambda x + \rho + \delta}{\lambda\left(x - m_i \dfrac{Y_i}{Y_{AE}}\right) + \rho + \delta}\right)^{\alpha/\alpha-1}, \qquad (8)$$

where m_i is the US-adjusted net immigration rate, given by (5), and m_0 is a counterfactual zero immigration rate. Obviously, the effect is increasing in the size of the immigration country relative to other AE countries. Average wage effects of immigration in small AE countries like Sweden, Norway, Switzerland, and Austria will be negligible, although these countries have high immigration rates. Wage effects of US immigration will be the highest, since the US economy is so much larger than those of other AE countries. The rest of this article therefore focuses on estimating wage effects of US immigration.

2.1 – Calibrating the model

The parameter *x*, the growth rate of the AE stock of effective workers, is given by a geometric mean of total AE output growth rates from 2001 to 2009, with no need to disentangle the effects of technology improvements, population growth, and migration. This implies x=1.3%. The parameter (Y_i/Y_{AE}) is the US's nominal share of AE output. These nominal output shares vary quite a lot due to exchange rate movements, yet nominal rather than purchasing power-adjusted shares must be used, since it is nominal capital returns that are equalized across countries. Using the average shares of the years 2000-2009 gives a US share of AE output of 48%. Central estimates and plausible intervals for the remaining parameters are given in Table 1. Note that the limit of the CRRA utility function as $\lambda \rightarrow 1$ is the logarithmic utility function.

The geometric averages of immigration-induced annual increases in hours worked, by immigrant origin, between 2000 and 2010 are shown in the first column of Table 2 (calculated from Ruggles et al., 2010). The second column shows the values of the weighting parameter $(A_i-A_j)/A_i$, which are used to calculate immigration rates of effective workers by origin. These are calculated from GDP/capita figures from the World Bank's World Development Indicators, but are only different from one for regions with mobile capital.

2.2 – Simulated average wage effects of immigration

Table 3 shows the simulated steady state wage effect of current US immigration levels. The effect is one minus the relative effect given by (8). It is calculated for the central parameter estimates of Table 1, and also when varying these parameters within their respective intervals so as to achieve the smallest possible and largest possible effects, besides the central estimate. The wage effect increases with increasing α, β, and λ and decreases with increasing δ.

The central estimate of the average wage effect of immigration is 0.89%. The "Smallest" and "Largest" effects provide extreme intervals, as they contain estimates resulting from synchronized variation of all four parameters, so as to achieve the smallest and largest possible estimates. A sensible conclusion may be that the present steady state wage effect of US immigration is a negative effect of somewhere between 0.8% and 1%. Although the relative wage effect given by (8) is not actually a linear function, it is approximately so for reasonable immigration rates, indeed even for rates of several percent of the work force per year, with the elasticity of the wage with respect to the immigration rate being approximately –1.5.

The earlier literature does not really provide estimates with which this estimate can be compared. Borjas (2003) assumes that capital never adjusts to immigration, in which case the negative average wage effect of immigration would be constantly increasing by about 0.2% per year, accumulating to about 4% after 20 years. Ottaviano and Peri (2012) only consider long-term effects, after the capital stock has fully adjusted to immigration. Yet, importantly, according to the neoclassical model, for the capital stock per effective worker to return to a no-immigration level, immigration must first cease. The capital stock per effective worker will be affected as long as immigration persists.

3 – A tax reform to compensate native workers for wage decreases

Migration from a low-income country to a high-income country typically implies large economic gains for the migrant; in many cases, the wage income will increase more than tenfold. By affecting factor supply relations, migration also implies gains and losses for other factors – typically gains to capital owners and losses to competing workers in the immigration country. Yet, migration is not a zero sum game. There are global gains from migration when the migrant moves to a technologically more advanced country, hence increasing global productivity. When the capital stock increases in response to immigration, there are also substantial total gains in the immigration country.

These total gains imply scope for redistribution to leave native workers economically unaffected by immigration. Consider an income tax decrease applied to all workers to compensate for the negative wage effect of immigration, as well as a tax surcharge imposed on the newly arrived immigrants to finance the rebate and make the reform revenue-neutral for the government. The idea is basically that immigrants pay a fee to immigrate, yet since the fee would be very high if it had to be paid in full on arrival, it is distributed over a number of years to make it more payable. The distribution of tax payments over the years could take different functional forms. In addition to a benchmark case where the entire fee is paid on arrival, I will analyze a form with a constant annual fee over a limited number of years.

Effects of the tax on immigrants' labor supply are ignored. While such effects may exist, they are actually already accounted for in the measure of the immigration rate that affects wages. This is because the rate was measured over hours worked, and immigrants would only affect wages to the extent that they work.

The immigration fee needs to cover the total discounted loss, denoted *L,* to all workers residing in the country and their future descendants:

$$L = -\%\Delta w \int_{t=0}^{\infty} e^{(n+m-r)t} dt = \frac{-\%\Delta w}{n+m-r},$$

where $\%\Delta w$ is the relative effect of immigration rate m on wages, n is the crude population growth rate caused by births over deaths, and r is the interest rate. The unit of accounting is the wage level corresponding to zero immigration, so the measured wage effect is equal to the relative wage effect. The immigration rate appears in the exponential expression because the simplest way to formulate the tax reform would probably be to let the tax rebate apply to all workers, including all immigrants, with the tax surcharge for immigrants levied on top of that. Taxes paid by immigrants sum to the discounted tax revenue:

$$R = \int_{a=0}^{\infty} me^{(n+m)t} \left(\int_{t=a}^{a+Y} T(t-a) e^{-rt} dt \right) da$$

where a is arrival year, $T(t\text{-}a)$ expresses the tax payment T as a function of the number of years since the arrival year a, and Y is the number of years during which each immigrant pays a tax surcharge. The appropriate level of the tax T is found by setting $R=L$, given Y, and solving for T. The integral from zero to infinity of the population growth term $e^{(n+m)t}$ appears in the expressions for both losses and revenues – in the loss expression because the population to compensate grows, and in the revenue expression because the immigration rate is a constant fraction of the growing population. Hence, when equaling the two expressions, the population growth term cancels out:

$$R = L \Leftrightarrow m \int_{a=0}^{\infty} \left(\int_{t=a}^{a+Y} T(t-a) e^{-rt} dt \right) da = -\%\Delta w \int_{t=0}^{\infty} e^{-rt} dt = \frac{\Delta w}{r}.$$

Case 1: Payment of immigration fee upon arrival

The first case, serving as a benchmark, is trivial. The entire fee is paid upon arrival, so:

$T(t-a) = T$ for $t\text{-}a=0$

$T(t-a) = 0$ for $t\text{-}a>0$.

Setting $R=L$ and solving for T:

$$\frac{\%\Delta w}{r} = m \int_{a=0}^{\infty} Te^{-ra} da = \frac{mT}{r}$$

$$T = \frac{\%\Delta w}{m}.$$

Trivially, the immigration fee, as share of the wage, is equal to the wage decrease, as share of the wage, divided by the number of immigrant workers, as share of the total number of workers. Simply put, the immigrants need to pay an immigration fee that is equal to their first annual wage times the elasticity of the wage with respect to the immigration rate, which we saw in Section 2.2 was about 1.5, regardless of the immigration rate. For most potential immigrants, paying 1.5 annual native wages on arrival would not be possible, and if the government is free to borrow at the market interest rate, nor is it necessary. Instead, immigrants' payments of their fees can be extended over a larger number of years, and the government can cover the initial deficit that arises when the reform is launched by borrowing, leaving its discounted revenues unaffected.

Case 2: Payment of constant immigration fee for *Y+1* years

In the second case, a constant fee is paid for *Y+1* years:

$T(t-a) = T_0$ for $t\text{-}a \leq Y$

$T(t-a) = 0$ for $t\text{-}a > Y$.

Setting $L=R$ and solving for T:

$$\frac{\%\Delta w}{r} = m\int_{a=0}^{\infty}\left(\int_{t=a}^{a+Y} T_0 e^{-rt}\,dt\right)da = \frac{Tm}{-r}\int_{a=0}^{\infty}\left(e^{-r(a+Y)} - e^{-ra}\right)da = \frac{Tm}{r^2}\left(1-e^{-rY}\right)$$

$$T = \frac{\%\Delta w r}{m\left(1-e^{-rY}\right)}.$$

The share of the wage to be paid for *Y+1* years is increasing in the interest rate and decreasing in *Y*. Again, due to the almost constant value of $\%\Delta w/m$, this share hardly changes with the immigration rate. The estimated numbers of *T* as a function of *Y* are given in Table 4. Immigrants would pay 9% of their income for 30 years or 11% for 20 years.

4 – Conclusion

This paper has presented a structural model for estimating average equilibrium wage effects of immigration when capital stocks are endogenous and capital is mobile between Anglo American and Western European countries. The effects are largest in the largest economy – the US – where the average negative wage effect in percentage terms is estimated at around 1.5 times the immigration rate. In percentage terms, this effect of US immigration also applies to all other Anglo American and Western European countries, which do not receive the immigrants but whose capital-labor ratios are nonetheless affected. Hence, average wages in a small Western European country are more affected by US immigration than by the country's own immigration, as the former has a larger impact on the international distribution of capital.

The negative average wage effect of current US immigration is thus around 0.9%. This is substantially lower than if assuming no capital adjustment to immigration, and substantially higher than if disregarding that also when savings are endogenous, immigration will lower the capital-labor ratio as long as it persists. If immigration ceases, there will be no remaining wage effect of previous immigration in equilibrium though. Also, if immigration increases productivity, average wage effects will be more positive, and if there are immigrant-native complementarities in production, they will be more positive for natives but more negative for previous immigrants.

While some agents lose from immigration, total gains are substantially larger than total losses, implying scope for redistribution. This is important from a policy perspective, as it could imply that no actor in the immigration country would have any economic reason to oppose immigration. In the US context, such redistribution from the most important winners, i.e., the immigrant workers themselves, to the most important losers, i.e., native workers, is found to require that immigrants pay a tax surcharge of around 10% of their annual income for 20-30 years following immigration. Regardless of whether such a surcharge is considered appropriate or not, most immigrants who would have to pay it would still make large income gains when moving to the US, since the majority of US immigration is from countries whose wages are merely fractions of US wages. Thus, most would-be migrants would still find it worth migrating.

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Tables

Table 1. Parameter estimates and intervals

Parameter	Description	Estimate	Interval
α	Capital's share of output	0.3	0.25 – 0.35
ρ	Time discount rate	0.04	0.02 – 0.06
δ	Capital depreciation rate	0.08	0.05 – 0.11
λ	Constant relative risk-aversion	1	0.75 – 1.25

Table 2. Immigration rates and technology weights by origin

	Immigration rate %	$(A_i-A_j)/A_i$
Africa	0.032	1
Anglo America (Canada)	0.005	0.272
Asia	0.201	1
Europe Western	0.004	0.137
Europe other	0.031	1
Mexico	0.150	1
Latin America & Caribbean	0.156	1
Oceania	0.003	1
Total	0.582	–

Note: Latin America & Caribbean does not include Mexico.

Table 3. Simulated wage effects of US immigration

	Effect %
Central estimate	0.89
Smallest effect	0.50
Largest effect	1.48

Note: The table shows absolute values; the wage effects are negative.

Table 4. Immigrant tax surcharges as functions of the number of payment years

	Share of wage
$Y = 20$	0.11
$Y = 30$	0.09

Paper VI

Why is the payoff to education smaller for some migrants than others? The role of migrant selection

Abstract

Previous literature has estimated large differences in migrants' returns to education depending on their countries of origin. These differences have often been interpreted as evidence of large differences in education quality between their home countries, with implications for analyses of global income differences and for development policy. The present study investigates if and how the observed patterns could instead have been generated by migrant selection on unobserved abilities depending on migration costs. It finds that migrant selection can account for at least two-thirds of the observed variation between 19 main countries and regions of US immigrant origin. Indicators for education quality do not explain any of the remaining variation.

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1 – Introduction

Decades of research – starting with Chiswick (1978) – have concluded that the returns to foreign education are lower than the returns to domestic education on the US labor market. This conclusion is inferred from comparisons of immigrants' and natives' returns to education in Mincerian wage regressions. There is also large variation in returns to education between migrants of different origins. Candidate explanations for these results have been differences between countries in the quality of education (Bratsberg and Ragan, 2002; Bratsberg and Terrell, 2002; Schoellman, 2012), limited transferability of education across countries, and migrant selection (Chiswick and Miller, 2008). Several studies have found empirical support for education quality being an important factor, since the estimated returns are higher for immigrants from richer countries that devote more resources to education.

The conclusion that the differences in estimated returns to education by country of origin reflect education quality has strong policy implications. Schoellman (2012) concludes that if education quality differences are responsible for all of his estimated differences in returns to foreign education, then they also explain an important share of global income differences. This result implies obvious policy recommendations for poor country governments to prioritize improvements in their educational systems for their economies to grow.

However, no previous study has tried to empirically quantify the importance of migrant selection in generating the observed differences in estimated returns to education between countries of origin. The closest resemblance is that of a study by Mattoo et al. (2008), who investigate the importance of selection for the related question of why there is variation by immigrant origin in the prevalence of highly educated US immigrants doing low-skilled jobs. They find that the most important explanatory variable also in that case is home country education quality, although they also find a significant yet smaller role for migrant selection.

Against this background, the present study investigates if and how migrant selection may generate the differences in migrants' estimated returns to education. The main conclusion is that migrant selection can easily explain the lion's share – around 70% – of the differences between 19 main countries and regions of migrant origin. Indicators for education quality do not explain any of the remaining variation. In fact they have no explanatory power at all once nonlinearities in returns to education – which are substantial – are accounted for. Hence differences in education quality between countries might not be as large as previous studies of migrants' returns to education have suggested, and on the whole these returns are probably not good indicators of education quality.

2 – Migrants' returns to home country education

This study estimates earnings equations for natives and immigrants using the iPUMS 5% sample of the US 2000 census (Ruggles et al., 2010). The sample is restricted to males aged 25-64, who are not in school, work at least 30 weeks per year and 30 hours per week, and are not self-employed. The census provides information on educational achievements, yet not on whether education was obtained in the home country or in the US. Information on educational achievement, age, and year of migration is therefore used to infer where the education was most likely obtained. Individuals aged six plus the number of years of education or less when migrating are thus expected to have received at least some education in the US and are therefore excluded from the sample. Some previous studies have imposed an extra buffer restriction – e.g., excluding those aged ten or twelve plus the number of years of education or less when migrating – since individuals may have had breaks in their education. However, such restrictions may induce the potentially serious unwanted selection effect that the most efficient individuals – those who promptly finished their education and then migrated – are excluded from the sample. However, all analyses reported in this article have also been tried – with very similar results – on a sample selected with an extra four years buffer restriction.

Returns to education are estimated using the equation:

$$wage=\alpha_0+\alpha_1 education+\alpha_2 age+\alpha_3 age^2+\alpha_4 ysm+\alpha_5 ysm^2+\alpha_6 region+\alpha_7 metro \qquad (1)$$

where *wage* is the logged hourly wage, *education* is the number of years of education, *ysm* is the number of years since migration, *region* is a vector of dummies for nine US regions, and *metro* is a dummy for living in a metropolitan area. For natives, Equation (1) is estimated without the *ysm* polynomial.

Estimated on the respective samples, the coefficient α_1 on the number of years of education is 0.104 for natives and only 0.065 for immigrants, as seen in Table 1. These coefficients are similar to those reported by earlier studies on the same data (Chiswick and Miller, 2008), although the coefficient for immigrants is somewhat sensitive to sample selection criteria, choice of control variables, and choice of hourly or weekly wages in the dependent variable. More importantly one crucial fact – which was recognized by Bratsberg and Ragan (2002) and their study of US 1990 census data – yet not accounted for in later studies that use 2000 census data is that the difference between natives' and immigrants' coefficients is to a large extent explained by the fact that returns to education increase substantially after 11 years of education. This is seen in Figure 1, which shows average logged hourly wages by years of education separately for natives and immigrants. It shows two lines with very similar slopes along the distribution. Still the corresponding regression coefficients in Table 1 are highly different. This is because while 96% of natives have 11 years of education or more, the same is true only for 65% of immigrants. Hence the high-return range above 11 years is given substantially more weight in the native regression than in the immigrant regression, resulting in a larger coefficient in the former. If Equation (1) is estimated separately for those with less than 11 years of education and those with at least 11 years, the resulting estimates are more similar between natives and immigrants. This is shown in the last two rows of Table 1.

2.1 – Returns to education by home country/region

After accounting for the kink at 11 years of education, estimated returns to education in the sample of all immigrants appeared quite similar to those for natives. In the higher-education spectrum the results reported in Table 1 even indicated that they were slightly (yet significantly) higher. This is also a misleading result though, as will be seen when we estimate returns to education separately by home country/region. The analysis is henceforth restricted to individuals with 11 years of education and more. When the analysis is done separately by origin estimates in the lower education spectrum are no longer useful, due to few observations and far from monotonic relations between reported years of education and average wages, resulting in large standard errors of the estimated returns to education, several point estimates that are negative, and a few that are very high.

Results for 19 major home countries and regions are reported in Table 2. Numbers of observations by country within each region are reported in the appendix. To further control for confounding effects of nonlinear returns, each individual has been weighted in the regression by the inverse of the total number of observations by origin and years of education. These weights cause the difference between the coefficients obtained for natives in Tables 1 and 2. Table 2 shows that while the estimated returns for all immigrants were higher than for natives, estimated returns for most individual migrant origins are lower. The high estimate for immigrants in Table 1 was thus partly due to different distributions across the education spectrum of immigrants of different origins.

In contrast to what is obtained when regressions are run across the entire education spectrum and hence strongly influenced by variation in shares of observations in the high and low returns to education regions (Chiswick and Miller, 2008; Schoellman, 2012), the coefficient variation reported in Table 2 is not easily explained by education quality or transferability. Looking first at the top origins: Assuming that university rankings (e.g., that of *Times Higher Education*) are not completely uninformative of quality in higher education, we would have expected US natives in the top position, followed by immigrants from the UK and Canada. The same would be true if transferability were the main story, since transferability is not an issue for natives and should be a smaller issue for migrants from the UK and Canada due to these countries' shared linguistic and cultural origins with the US. Yet instead the top three positions are occupied by Asian low- and middle-income regions: China, South Asia (dominated by India), and East Asia (Korea, Taiwan, and Hong Kong). The top two estimates – which are held by two of the poorest origins on the list – are significantly higher than each of those for natives and migrants from the UK and Canada.

What we see at the bottom of Table 2 is probably even more difficult to reconcile with education quality. Instead of by any of the poorest countries and regions on the list, the bottom four positions are occupied by the four relatively richer Central American and Caribbean countries and regions. The estimates for each of these four origins are significantly lower than those for each of the five poorest origins (South Asia, China, Sub-Saharan Africa, Southeast Asia, and the Philippines). And at the very bottom, significantly below all other estimates, we find Cuba, which is known for high spending (confirmed in Figure 2) and high quality in higher education by low- and middle-income country standards.

Figure 2 plots estimated returns to education against home country government spending on tertiary education, which should be the best proxy available for quality in tertiary education and is also used by Mattoo et al. (2008). Data on government spending per student in tertiary education as share of GDP is from the World Bank's World Development Indicators. Observations are quite scarce, and therefore the average share per country over the decade 1990-1999 is used. It is then multiplied by PPP-adjusted GDP per capita in the year 2000. Spending is measured by country and averaged over all immigrants to obtain the relevant values for regions encompassing several countries. The implied linear regression line in Figure 2 is nearly flat with a T value of 0.28. Substituting PPP-adjusted GDP/capita for spending in tertiary education on the horizontal axis results in an equally insignificant correlation. If these two independent variables are instead expressed in logs – which is common – the coefficients are no more significant and for GDP/capita it even becomes negative.

While the estimated returns to education in Table 2 did not show any correlation with measures that were indicative of quality in higher education, they have strong geographical patterns that are indicative of migrant selection playing an important role in their generation. In the top we find Asian origins and Oceania, i.e., far-away origins that send relatively few migrants. In the bottom we find all four Central American and Caribbean origins, i.e., nearby origins that send relatively many migrants. Earlier research (Chiswick, 1978, 1999) has suggested that migration distance is an important predictor of migrant selection on unobserved ability, i.e., that selection is more positive when the migration distance is longer and hence monetary and non-monetary costs of migration are higher. This suggestion seems consistent with the estimates in Table 2 and will be explored in more detail in the rest of this article.

3 – Theoretical model of migrant selection and returns to education

Chiswick (1978, 1999) suggested models of how migrant selection on unobserved abilities is influenced by migration costs such as those that are due to migration distance. This section presents a modified version that is similar to these models yet aimed at explicitly delivering predictions of how selection affects estimated returns to education in the destination country depending on the characteristics of the country of origin.

The logarithm of the wage w of an individual working in country j is given by a human capital earnings function:

$$\log(w) = \alpha_j x + \theta\beta xS\,. \qquad (2)$$

The variable S measures the number of years of education, x denotes individual unobserved ability, and α_j, β, and θ are parameters. Unobserved ability x influences both the wage that the individual could earn without education and her individual return to education. It can only take positive values. For expositional simplicity its mean is equal to one and it is uncorrelated with x. A positive correlation between S and x can be added to the model without qualitatively changing any of what follows from here as long as this correlation is constant across countries. The parameter θ captures imperfect transferability of education between countries. In each country we have $\theta = 1$ for natives and $0 \leq \theta \leq 1$ for immigrants.

Countries are only different regarding one parameter of Equation (2): they have different income levels, which are measured by the parameter α_j. Education quality and transferability, the returns to education and ability, and the distribution of ability are all identical across countries, since the focus of the model is to see how migrant selection can generate the differences in migrants' estimated returns to education between home countries.

Utility is logarithmic, so utility for a non-migrant is equal to the right hand side of Equation (2). Migration to a country with a higher income level α_j may increase the wage, yet will increase utility only if the log-wage increase is larger than the utility costs of migration. Here the wage w may most appropriately be thought of as a measure of discounted lifetime wage income. Yet since the simple model is not meant to account for differential selection by age, for pre- or post-migration human capital investments, or for return-migration decisions, discounted lifetime wage income is simply proportional to the wage. The utility costs of migration include both monetary costs such as travel costs, and the non-monetary costs of uprooting and living further away from family and friends.

The change in utility for a worker migrating from country $j \neq$ US to the US is thus:

$$\Delta U = (\alpha_{US} - \alpha_j)x - (1-\theta)\beta xS - C_j + \varepsilon$$

where C_j represents country-specific migration costs and the mean-zero independently distributed error term ε represents individual migration preferences. Several country-specific factors are likely to contribute to the value of C_j. Both monetary and non-monetary costs of migration increase with a longer migration distance. Non-monetary costs also decrease when staying home is a worse option, such as most obviously for refugee migrants. Home country emigration policies and destination country immigration policies toward the respective home countries are also important determinants of C_j.

For each country there exists a threshold level of individual ability above which the individual will migrate. We denote this threshold x_j and it is given by:

$$x_j = \frac{C_j}{\alpha_{US} - \alpha_j - (1-\theta)\beta S} \geq 0 . \qquad (3)$$

A higher value of x_j thus implies that the emigrant population will be smaller and more positively selected. The first-order derivatives of x_j with respect to C_j, α_j, and S are all positive. The fact that selection is more positive at higher levels of education does not imply that estimated returns to education for migrants must be higher than for natives in the destination country though, since $\theta \leq 1$ for immigrants. Without further restrictions, estimated returns to education can be higher for natives or higher for immigrants depending on the values of the variables and parameters. Estimated returns to education will be given by the derivative of the right hand side of Equation (2) with respect to S. For natives this derivative is simply equal to $\theta\beta x = \beta$. Yet for immigrants – who are a selected sample – the distribution of x is a function of the other variables and parameters and the resulting derivative can be smaller or larger than β. This is most easily seen by looking at extreme cases. If, e.g., $\theta = 1$ transferability is perfect and migrant selection is independent of the level of education (see Equation (3)). Hence the distribution of x is the same at all values of S also for migrants and

$$\frac{d(\alpha_j x + \theta\beta x S)}{dS} = \beta x .$$

In this case positive migration costs imply that the mean of x is larger for immigrants than for natives, i.e., larger than one, implying $\beta x > \beta$. Hence estimated returns to education will be higher for immigrants than for natives. If instead, e.g., $\theta = 0$ and Var(x) = 0 there is no transferability of education and no variation in unobserved ability and

$$\frac{d(\alpha_j x + \theta\beta x S)}{dS} = 0 .$$

Hence estimated returns to education will be lower for immigrants than for natives. Obviously, between these extremes there are variable ranges for which estimated returns to education are higher than for natives for immigrants from some countries, yet lower than for natives for immigrants from other countries, i.e., consistent with the estimated returns in Table 2.

Equation (3) also delivers explicit predictions for the variation by origin in migrants' estimated returns to education in the destination country. The first-order derivatives of x_j with respect to C_j and α_j are both positive, i.e., migrant selection on ability will be more positive for countries with higher migration costs and for countries with higher income levels.[1] The second-order derivatives $\partial^2 x_j/\partial C_j \partial S$ and $\partial^2 x_j/\partial \alpha_j \partial S$ are also positive, i.e., positive selection is more strongly increasing with education for the same countries. Hence emigrants' estimated returns to education in the destination country will be higher for countries with higher migration costs and for countries with higher income levels.

[1] This is more complicated for the one country with higher PPP-adjusted income in 2000 than the US, which is Norway. According to the model, no one should migrate from Norway to the US, but if we think instead of the alphas as income levels within more narrow sectors of the economy, it is likely that most Norwegian migrants to the US belong to those sectors where income levels are higher in the US than in Norway.

4 – Empirical tests of selection effects

We can test the theoretical predictions about the drivers of estimated returns to education empirically with data on home-country income levels and proxies for country-specific migration costs. However, while we may expect factors like migration distance, political instability, and migration policies to constitute important elements of the variation in C_j, we cannot expect to obtain a full account of all its elements. This may also in turn potentially bias estimated effects of α_j to the extent that α_j and C_j are correlated.

A potentially more fruitful strategy that bypasses the need to completely specify C_j is to build on the result that a high value of x_j on the left hand side of Equation (3) indicates both that country *j* has high estimated returns to education and that it has a small emigrated share of the population. This allows us to treat the right hand side of Equation (3) as a black box. For a country with few emigrants relative to the total population, these emigrants will to a larger extent be drawn from the top of the ability distribution and their abilities will also be more strongly increasing with education. Hence immigrants from countries with small emigrant populations will have larger estimated returns to education.

This prediction is strongly supported by the data. We measure the share of US emigrants by the total emigrant population present in the US according to the 2000 census, divided by the sum of the same migrant population and the total home country population. Emigrant populations in other countries than the US are ignored, as their sizes depend on migration costs to those destinations. The home country population is given by the World Development Indicators. Values for regions encompassing several countries are averaged over US immigrant populations. Figure 3 plots estimated returns to education against the share of US emigrants. The regression line – which is reported in the first column of Table 3 – has a slope of –0.50. It is highly significant and R^2 is 0.53. As we have no prediction at hand for the actual shape of the correlation between the independent and dependent variables, the second column of Table 3 uses a second-degree polynomial in the emigrant share, increasing R^2 to 0.58. Hence this simple black-box measure explains well above half the variation in estimated returns to education between the 19 origins.

4.1 – English skills as a measure of selection

A complementary black-box method to investigate the role of migrant selection behind the estimated returns to education is to note that – since we are analyzing international migrants – a plausibly very important part of what we treat as unobserved ability in x_j is actually to some extent observable, namely English language skills. The census data contains a self-reported measure of English skills, where individuals report any of the following five skill levels: “Speaks only English”, “Speaks very well”, “Speaks well”, “Speaks, but not well”, and “Does not speak English”. However, including a measure of language skills as a covariate in wage regressions or in regression sample selection is problematic, since these skills are to some extent themselves measures of migrants’ labor market success (Dustmann and Glitz, 2011). Therefore we want to restrict our attention to studying variation in English skills by origin *at migration*, as a measure of migrant selection. Hence we measure the share of English speaking migrants by country as the fraction of immigrants with at least 11 years of education – who arrived during the last five years[2] – who report the skill level “Speaks well” or better.

[2] The interpretation of “recent” can be stricter without noticeably affecting the results. The correlation between values obtained with a five-year and a two-year limit respectively is 0.98.

However, even when they are measured at the time of migration, using English skills as a measure of positive selection deserves further motivation. Its usefulness is clearly limited for the four countries/regions of origin where English is a native language for large shares of the population, i.e., the UK, Canada, Oceania and the Caribbean. For the remaining origins the measure could also well reflect variation in English speaking shares in the home populations. There is no accurate data on these shares to enable us to express English speaking shares among migrants relative to shares in the home populations. The shares could also reflect variation in the use of English in tertiary education between home countries, and would then rather represent variation in education transferability, i.e., a competing hypothesis in explaining variation in estimated returns to education.

Yet there is one strong empirical argument for using English speaking shares as a selection measure, although admittedly a less than perfect one. This is the strong negative (as expected) correlation between these shares and the other selection measure: US emigrant shares. In the sample of 19 origins this correlation is –0.48, and excluding the four origins with many native speakers it is as high as –0.78. The important point is that these correlations should have been positive if the variation in English speaking shares indeed primarily represented home country fundamentals. This is because in countries where larger shares of the population speak English larger shares would have the skills required to find it worth migrating to the US. This point is also valid concerning skill transferability hypotheses more generally: better transferability should imply more migrants. No similar point can be made concerning education quality though, as higher quality of education would imply greater returns to education regardless of where the individual chose to live.

Estimated returns to education are plotted in Figure 4 against the share of recent arrivals who speak English "well" or better. We may first note the many high English speaking shares in the figure. Again, while it is not a surprise – and does not say much about selection – that more than 99% of all immigrants from the UK speak English well, it is interesting that the corresponding shares are as high as 88-96% for migrants from Sub-Saharan Africa, South Asia, the Philippines and the Middle East.[3] At the other end of the spectrum the shares are below 50% for Cuba, Mexico, and Central America, i.e., the three origins with the largest US emigrant shares apart from the Caribbean, which has a large share of native English speakers. However, also these three lowest shares are plausibly a lot higher than the corresponding shares in their home countries, i.e., also these migrants are positively selected on English skills, although to a smaller extent than migrants with higher net costs of migration.

Figure 4 shows a strong positive correlation between the English speaking share of recent migrants and estimated returns to education. As shown in column (3) of Table 3, the implied regression line for 19 origins has a slope of 0.069 and explains 40% of the variation in the dependent variable. Column (4) considers a second-degree polynomial in the English speaking share and raises R^2 to 0.65. Finally, column (5) maximizes the fit by including second-degree polynomials in both the emigrant and English speaking shares, reaching an R^2 value of 0.68. The bottom panel of Table 3 shows the corresponding results in the limited sample of 15 origins without large shares of native English speakers. As expected, the R^2 values are substantially higher in columns (3) and (4) in this case. Yet in column (5) when the US emigrant share polynomial is also included the degree of fit is similar between the two panels.

[3] In further support against these values being generated by home country fundamentals, we may note that two of these regions contain French-speaking countries for which the shares are very close to the respective regional averages (Algeria and Morocco in the Middle East / North Africa, and Senegal in Sub-Saharan Africa).

In sum, the two black-box measures of selection are able to explain a large majority of the variation in estimated returns to education between home countries and regions. The first and most obvious measure – the US emigrant share – alone explained almost 60% of the variation. Including the English speaking share explained another 10% and this subsections has argued that this improvement indeed represents improved measurement of selection effects.

4.2 – Specific components influencing selection

The black-box approach to migrant selection was highly successful in explaining the variation in migrants' estimated returns to education. In this subsection we open this box and see how close to this performance we can get with components that we can identify. The model in Section 3 predicted that estimates would rise with home country GDP per capita, rise with migration distance, and fall with factors indicating political instability in the home country. We measure migration distance by the distance – in thousands of kilometers – as the crow flies between the two closest points of the US and the migrant's home country. Various measures of the degree of political instability exist. Yet there is considerable variation among home countries in the extent to which instability at home has led to migration flows to the US. For this reason the measure of choice is the share of refugees among US immigrants from the home country. The numbers of refugees are given by the online refugee database of the UNHCR, and the total numbers of immigrants are given by the census data.

The coefficients from bivariate regressions of estimated returns to education on each of these three selection measures are shown in the first three columns of Table 4. All three coefficients have the expected signs, yet the only variable with a significant bivariate correlation is migration distance. Returns to education are plotted against migration distance in Figure 5. R^2 is a fairly high 0.40, and with a squared term it rises to 0.47, as shown in column (4) of Table 4. Hence migration distance alone can account for half the total variation in estimated returns to education. Column (5) of Table 4 includes GDP per capita and the refugee share besides the distance polynomial, raising R^2 to a full 0.61, i.e., not so far from the degree of fit obtained with the black-box approach. The degree of fit is the same if GDP/capita is removed from the specification. Hence the fit is entirely due to the two measures that unambiguously measure selection effects.

4.3 – The (remaining) role for education quality

It was mentioned in Section 2 that the bivariate correlations between proxies for education quality and estimated returns to education were far from significant. These bivariate coefficients on GDP per capita and government spending on tertiary education are reported in columns (1) and (2) of Table 5. A remaining question is if these variables might explain any of the remaining 30-40% of the total variation after controlling for measures of selection. The answer turns out to be negative. Columns (3) and (4) of Table 5 add one each of these measures to the black-box specification from Table 3. In none of these cases are the resulting estimates significantly different from zero and R^2 remains equal to 0.68.

The investigation into components of selection in Table 4 already included GDP per capita as a potential measure of selection, which makes it more difficult to add education quality proxies to this specification. Yet in an attempt to do this, column (5) of Table 5 substitutes government spending in tertiary education – which should be a better measure yet is likely to contain more measurement error – for GDP per capita, and column (6) adds it besides GDP per capita. None of these specifications result in any significant estimates, although R^2 rises a few percentage points.

In sum, controlling for selection does not change the result that no important role can be identified for proxies for education quality in explaining variation in immigrants' estimated returns to education by origin.

5 – Conclusion

Previous research has estimated large differences in US immigrants' returns to education in Mincerian wage regressions depending on the migrants' origin. Differences have often been attributed to – and interpreted as measures of – large education quality differences between the home countries. If education quality differences are indeed responsible for the differences in estimated returns to education, they are also – according to Schoellman (2012) – responsible for an important share of global income differences.

At the same time, the differences in returns to education have strongly visible geographical patterns. They are highest for Asian origins with relatively low emigration to the US, and lowest for Central American and Caribbean origins with high US emigration. This suggests that positive ability selection on migration costs could play a role in explaining the differences in returns to education. The present study has investigated this role and found that migrant selection can explain a full two-thirds of the differences between 19 major countries and regions of migrant origin. Most of this – around half the total variation – is due to selection on migration distance.

There is no significant role for indicators for home country education quality to explain any of the remaining variation in estimated returns to education. Hence inference from these estimates about education quality is probably not advisable.

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Tables

Table 1. Estimated returns to education for natives and immigrants

Sample education	Natives	Immigrants	T value for difference
All	0.104	0.065	112
	(0.0002)	(0.0003)	
	[1,887,362]	[194,010]	
Years of education < 11	0.026	0.010	13.9
	(0.0009)	(0.0007)	
	[80,280]	[70,190]	
Years of education ≥ 11	0.110	0.116	8.2
	(0.0002)	(0.0007)	
	[1,807,082]	[123,820]	

Note: Each cell shows the estimated returns to education from a separate regression. Standard errors in parentheses. Sample sizes in brackets.

Table 2. Estimated returns to education by home country / region

	Estimate	Standard error	Number of obs.
China	0.133	0.003	4,665
South Asia	0.118	0.002	10,462
East Asia	0.107	0.004	5,060
Oceania	0.106	0.007	1,072
Middle East / North Africa	0.105	0.003	5,306
Western Europe	0.100	0.003	7,194
UK	0.097	0.004	3,975
Sub-Saharan Africa	0.095	0.003	4,663
Canada	0.092	0.004	3,404
South America	0.091	0.002	9,961
Japan	0.089	0.006	2,003
Former USSR	0.088	0.003	4,029
Southeast Asia	0.087	0.002	7,295
Philippines	0.082	0.002	7,965
Eastern Europe	0.069	0.002	6,266
Caribbean	0.067	0.002	8,320
Central America	0.062	0.002	6,846
Mexico	0.053	0.001	22,284
Cuba	0.039	0.004	2,932
US (natives)	0.105	0.0001	1,807,082

Note: Estimated returns to education are obtained in separate regressions for each country / region of origin for individuals with at least 11 years of education.

Table 3. Black-box determinants of estimated returns to education

	(1)	(2)	(3)	(4)	(5)
			N = 19		
Emigrant share	–0.50**	–1.01*			–0.38
	(4.39)	(2.49)			(0.75)
Emigrant share ^{2}		5.01			0.56
		(1.31)			(0.13)
English speaking share			0.069**	0.40*	0.24
			(3.35)	(2.67)	(1.37)
English speaking share ^{2}				–0.23*	–0.14
				(2.22)	(1.17)
R^2	0.53	0.58	0.40	0.54	0.68
			N = 15		
Emigrant share	–0.68**	–0.98			–0.87
	(4.77)	(1.39)			(1.17)
Emigrant share ^{2}		3.6			7.6
		(0.44)			(0.83)
English speaking share			0.084**	0.48*	0.36
			(3.38)	(2.94)	(1.19)
English speaking share ^{2}				–0.28*	–0.22
				(2.45)	(1.13)
R^2	0.64	0.64	0.47	0.65	0.69

Notes: The bottom panel excludes the UK, Canada, Oceania, and the Caribbean from the sample. T values in parentheses. * and ** denote significance at the 5% and 1% levels respectively.

Table 4. Specific determinants of estimated returns to education

	(1)	(2)	(3)	(4)	(5)
GDP / capita	2.7e-7				1.6e-7
	(0.49)				(0.18)
Refugee share		–2.6			–7.8
		(0.50)			(1.82)
Distance			0.0037**	0.0094*	0.012*
			(3.34)	(2.42)	(2.92)
Distance ^{2}				–5.4e-6	–7.2e-6
				(1.52)	(1.94)
R^2	0.01	0.01	0.40	0.47	0.61

Notes: N = 19. T values in parentheses. * and ** denote significance at the 5% and 1% levels respectively.

Table 5. Education quality and selection determinants of estimated returns to education

	(1)	(2)	(3)	(4)	(5)	(6)
GDP / capita	2.7e-7		–9.2e-8			–4.6e-7
	(0.49)		(0.23)			(0.66)
Spending tertiary educ.		5.0e-7		3.7e-7	1.3e-6	2.4e-6
		(0.28)		(0.24)	(1.03)	(1.15)
Emigrant share			–0.34	–0.41		
			(0.62)	(0.76)		
Emigrant share ^{2}			0.22	0.71		
			(0.05)	(0.16)		
English speaking share			0.24	0.25		
			(1.34)	(1.35)		
English speaking share ^{2}			–0.14	–0.14		
			(1.14)	(1.16)		
Refugee share					–7.4	–8.4
					(1.91)	(1.98)
Distance					0.012**	0.013**
					(3.20)	(3.14)
Distance ^{2}					–7.1e-6	–8.0e-6
					(2.08)	(2.14)
R^2	0.01	0.00	0.68	0.68	0.63	0.64

Notes: N = 19. T values in parentheses. * and ** denote significance at the 5% and 1% levels respectively.

Figures

Figure 1. Average wages by years of education

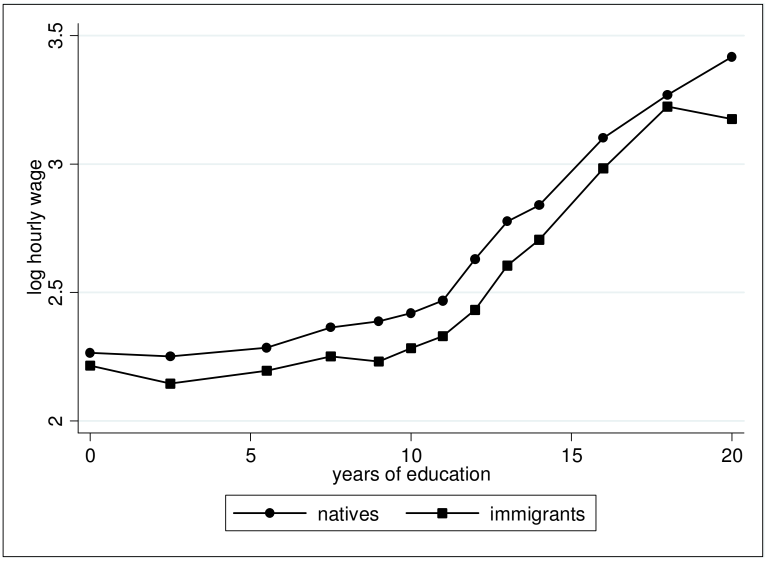


Figure 2. Returns to education and government spending on tertiary education

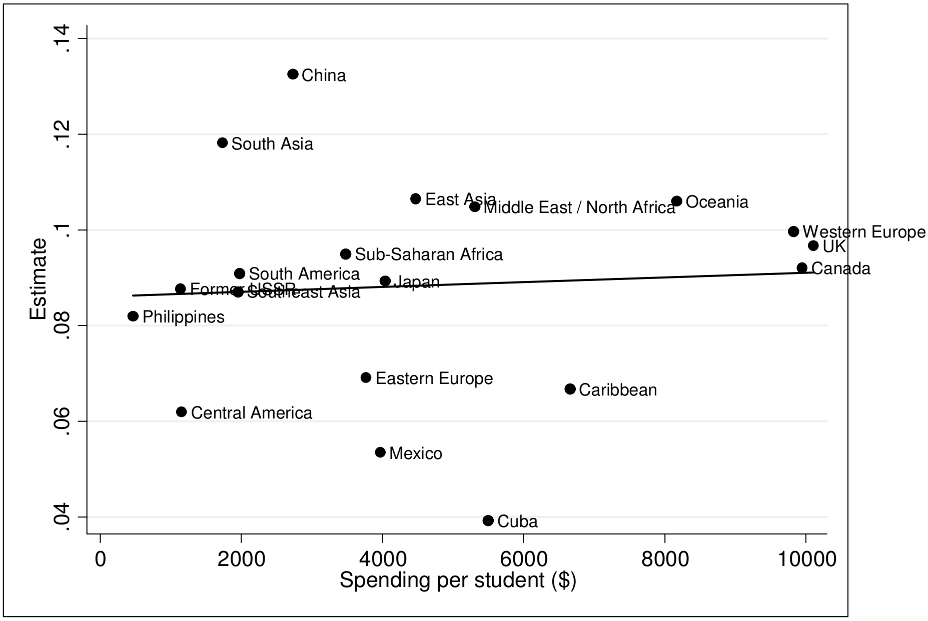


Figure 3. Returns to education and US emigrants shares

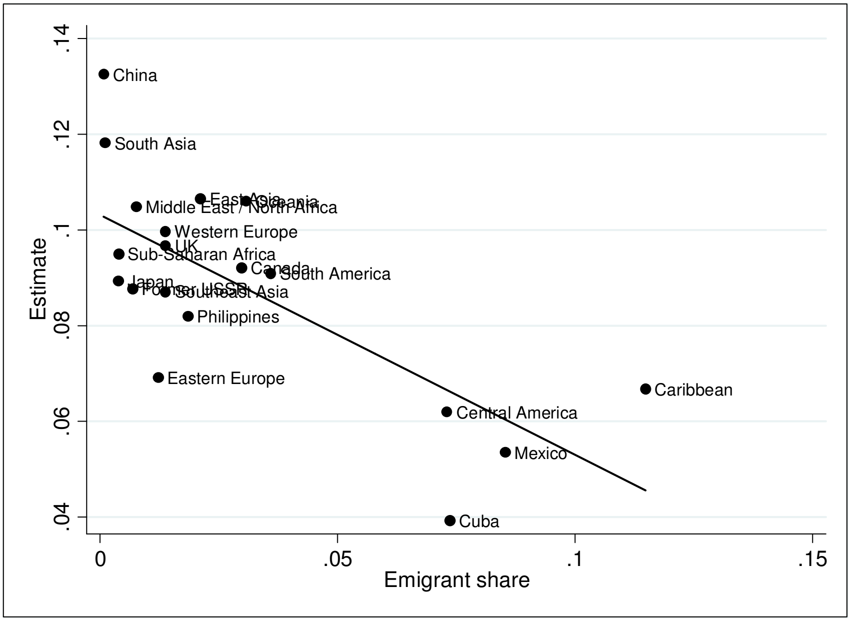


Figure 4. Returns to education and recent migrants' English skills

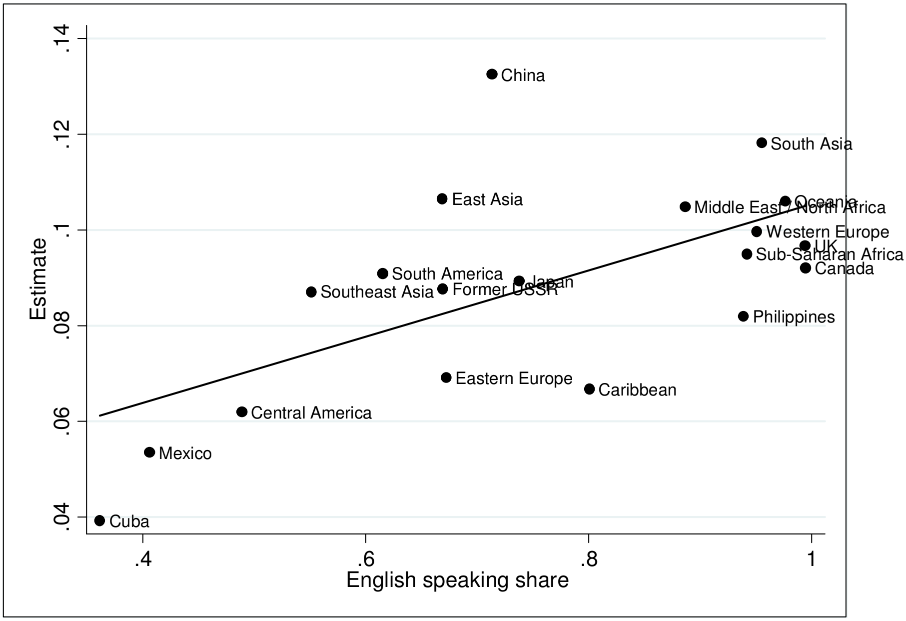
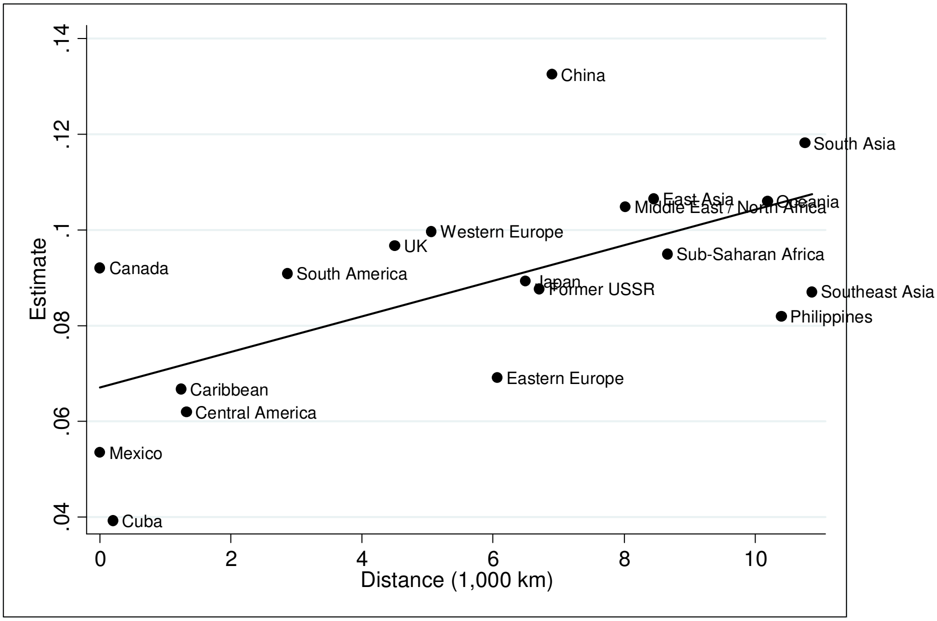


Figure 5. Returns to education and migration distance



Appendix: Observations by country within country groups

Canada	**3,404**	**Western Europe**	**7,194**
Mexico	**22,284**	--Denmark	162
Central America	**6,846**	--Finland	111
--El Salvador	2,490	--Norway	125
--Belize/British Honduras	158	--Sweden	258
--Costa Rica	306	--Ireland	843
--Guatemala	1,643	--Belgium	134
--Honduras	904	--France	793
--Nicaragua	941	--Netherlands	392
--Panama	404	--Switzerland	256
Cuba	**2,932**	--Italy	1,015
Caribbean	**8,320**	--Portugal	552
--Bermuda	43	--Spain	368
--Dominican Republic	2,094	--Austria	157
--Haiti	2,013	--Germany	2,028
--Jamaica	2,311	**Eastern Europe**	**6,266**
--Antigua-Barbuda	84	--Albania	216
--Bahamas	90	--Greece	433
--Barbados	276	--Macedonia	104
--Dominica	68	--Bulgaria	240
--Grenada	124	--Czechoslovakia	150
--St. Kitts-Nevis	64	--Slovakia	81
--St. Lucia	64	--Czech Republic	116
--St. Vincent	95	--Hungary	256
--Trinidad and Tobago	866	--Poland	2,608
--Other	128	--Romania	758
South America	**9,961**	--Yugoslavia	379
--Argentina	627	--Croatia	184
--Bolivia	321	--Serbia	58
--Brazil	1,110	--Bosnia	656
--Chile	476	--Kosovo	27
--Colombia	2,331	**Former USSR**	**4,029**
--Ecuador	1,415	--Latvia	55
--Guyana/British Guiana	1,146	--Lithuania	71
--Paraguay	52	--Byelorussia	208
--Peru	1,778	--Moldavia	97
--Uruguay	135	--Ukraine	1,214
--Venezuela	499	--Armenia	221
--Other	71	--Azerbaijan	77
United Kingdom	**3,975**	--Georgia	47
		--Uzbekistan	90
		--Other	1,949

China	**4,665**	**Sub-Saharan Africa**	**4,663**
Japan	**2,003**	--Cape Verde	86
East Asia	**5,060**	--Sudan	104
--Hong Kong	688	--Ghana	555
--Taiwan	1,580	--Liberia	219
--Korea	2,792	--Nigeria	947
Philippines	**7,965**	--Senegal	84
Southeast Asia	**7,295**	--Sierra Leone	158
--Cambodia	477	--Ethiopia	438
--Indonesia	289	--Kenya	206
--Laos	744	--Somalia	121
--Malaysia	212	--Tanzania	73
--Singapore	92	--Uganda	91
--Thailand	413	--Zimbabwe	75
--Vietnam	4,558	--Eritrea	114
--Myanmar	211	--Cameroon	80
--Other	299	--South Africa	437
South Asia	**10,462**	--Other	875
--Afghanistan	249	**Oceania**	**1,072**
--India	7,845	--Australia	418
--Bangladesh	657	--New Zealand	209
--Pakistan	1,493	--Fiji	178
--Sri Lanka	218	--Tonga	87
Middle East / North Africa	**5,306**	--Western Samoa	88
--Iran	1,254	--Other	92
--Nepal	62		
--Cyprus	44		
--Iraq	429		
--Israel/Palestine	627		
--Jordan	241		
--Kuwait	71		
--Lebanon	468		
--Saudi Arabia	68		
--Syria	259		
--Turkey	386		
--Yemen	85		
--Algeria	101		
--Egypt	922		
--Morocco	289		

Paper VII

The macroeconomic factors behind rising public resistance to immigration in Europe

Abstract

This article shows that the recent increase in public resistance to immigration in Europe has been importantly shaped by the deteriorating macroeconomic context. While the link between economic circumstances and resistance to immigration may appear obvious, previous cross-national research has generally not been able to identify it. The main advantage of the present study in this respect is in its use of within-country instead of cross-country variation. Also in contrast to previous research, perceived economic threat is found to be substantially more important than perceived cultural threat in explaining resistance to immigration. And perceived cultural threat is itself found to be significantly influenced by the macroeconomic context.

I am grateful to Alpaslan Akay and Arne Bigsten for useful comments and suggestions.

1 – Introduction

Over the last few years, Europe has seen a substantial increase in public resistance to immigration. Anti-immigration movements have become increasingly visible – and increasingly violent – in several countries and have also been successful in elections to several national assemblies and to the European Parliament (Fekete, 2012; Goodwin, 2011). In public debate this development is commonly interpreted as an effect of the European economic crisis (*Economist*, 2012). The aim of this paper is to empirically investigate this link.

Knowing how anti-immigration opinions are affected by macroeconomic circumstances is important both to predict their future development and to devise proper strategies to counter them, if they are to be countered. Is it likely that public resistance to immigration will decrease when the European economy improves? Would it decrease with better popular knowledge about the limited effects of immigration on labor markets (Card, 2005, 2009) and public finances (Rowthorn, 2008) that are estimated in most empirical studies?

There already exists a large empirical quantitative literature on the determinants of anti-immigration opinions. There is largely a consensus in this literature about the positive link between the size of the immigrant population in a country and the prevalence of anti-immigration opinions (Semyonov, Raijman, and Gorodzeisky, 2006). Yet there is surprisingly little empirical support for the existence of any link between macroeconomic contextual factors and anti-immigration opinions. Quillian (1995) investigates the correlation between GDP per capita and anti-foreigner sentiments and finds no significant correlation. Semyonov et al. (2006) find a significant correlation in only one sample year out of four. Scheepers, Gijsberts, and Coenders (2002) do not find any significant correlation between unemployment rates and anti-immigrant sentiments. Furthermore, in a recent study Card, Dustmann, and Preston (2012) find that the importance of individual perceptions about the economic effects of immigration in shaping individual opinions on immigration policy is relatively small. The substantially more important determinant of these policy opinions is individual preferences for cultural homogeneity. Goodwin (2011) reaches the related conclusion that the recently increasing support for anti-immigration movements in Europe is little motivated by economic concerns and more by perceived cultural threat.

In the theoretical literature on the subject, negative attitudes towards foreigners, immigrants, or minorities are generally expected to increase in more difficult macroeconomic circumstances (Semyonov et al., 2006). Hence the list of empirical results that indicate only a modest role for macroeconomic factors in explaining anti-immigration opinions and support for anti-immigration movements, and the absence of studies that robustly identify such links are quite surprising. Against this background the motivation of the present study is twofold. First, it covers the recent period of concurrent sharply deteriorating economic conditions and rising public resistance to immigration, where we may expect any link between the two to possibly be more easily identified. Second – and more importantly – it offers the first comprehensive comparative analysis of the determinants of variation over time in public resistance to immigration *within* European countries. It is argued that the within-country design has important advantages over cross-sectional designs. Due to the small number of countries available, cross-sectional designs are vulnerable to omitting important explanatory variables and also to some extent to survey questions being differently interpreted in different countries and languages. Within-country analysis has obvious benefits in being substantially less sensitive to these sources of bias.

To my knowledge, the only previous study that explicitly compares national trends in anti-immigration opinions is by Meuleman, Davidov, and Billiet (2009).[1] They relate changes in attitudes to immigration expressed in the European Social Survey between the 2002 and 2006 rounds by country to changes in GDP, unemployment and immigration for 17 countries. In this small data set they find indicative empirical support for negative views on immigration increasing with both unemployment and immigration levels. The results are not significant at the 5% level though, and only bivariate regressions are tried although the independent variables are correlated with each other.

The present study provides a comprehensive multivariate analysis of a larger data set that enables more thorough investigation of possible links. It compares variation over time in anti-immigration opinions from all five rounds of the European Social Survey 2002-2010 for all countries for which data was available from at least four rounds. Anti-immigration opinions are found to increase significantly both with higher immigration and with deteriorating macroeconomic conditions. The most important economic variable during this period is public debt, which increased substantially in several countries over the later part of the period. Furthermore, and in contrast to the results of Card et al (2012), negative views on immigration policy are found to be far more strongly correlated with individuals' economic concerns than with their cultural concerns. This result even holds also in the pre-crisis period. Cultural concerns are in turn found to be significantly influenced by macroeconomic circumstances, although as expected to a lesser extent than economic concerns.

2 – Data and descriptive overview

This study uses data from rounds 1-5 of the European Social Survey, which was performed biannually 2002-2010. In all five rounds respondents to the survey were asked questions about their views on immigration. These include whether immigration worsens or improves the economic and cultural situations in the country and whether respondents think the country should allow many or few immigrants from other countries and of other ethnicities. All countries that participated in at least four out of five survey rounds are sampled except Ukraine, which is not considered part of the European core. This gives a sample of 20 countries. Only native individuals are sampled, so this is a study of native views on immigration. The total sample consists of 164,542 individuals.

The aim of the study is to analyze the determinants of shares of negative responses to the survey question:

> **Q1:** *To what extent do you think [country] should allow people from the poorer countries outside Europe to come and live there?* [2]

Question Q1 had four possible responses: 1) "Allow many to come and live here", 2) "Allow some", 3) "Allow a few", and 4) "Allow none". The distribution of responses by country for all rounds pooled is shown in Figure 1. The analysis in this study will focus on the prevalence of the most negative response (4), i.e., that the country should allow no immigrants, as the

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[1] Part of the analysis by Semyonov et al. (2006) pool the four sample years and allow both cross-sectional variation and variation over time to influence the results. Their study contains no analysis of the time dimension only though.

[2] The wording "to come and live there" was chosen instead of "to immigrate" because it was expected to be interpreted more similarly across countries, whereas "immigrate" may in some cases be interpreted more narrowly in terms of obtaining citizenship (Card et al., 2012).

objective is to study *resistance* to immigration. The shares of such responses vary greatly from 3% of respondents in the most positive country to 39% in the most negative country. The most positive countries are Sweden, Norway, Switzerland, and Poland; the most negative countries are Hungary, Estonia, Greece, and Portugal.

The shares of sampled respondents who think that their country should allow no immigrants from the poorer counties outside Europe have increased substantially over time. Respondent shares with this preference by country for the years 2002 and 2010 and the T values of the differences between the two years are shown in Table 1. The share preferring no immigration in the average country has increased from 10.8% of respondents in 2002 to 17.1% in 2010. Within countries there are significant increases in 14 countries out of 18 and no significant decrease. The increases in some countries are really striking: 13 percentage points in the Czech Republic, 16 in Hungary, and a full 20 percentage points in Greece over a period of only eight years.

Section 3 of this study investigates correlations between within-country variation in anti-immigration opinions and in three variables that measure macroeconomic fluctuations: economic growth rates, unemployment rates, and public debt. A fourth explanatory variable is the share of immigrants in the population. Data on the macroeconomic variables is from the World Bank's World Development Indicators. Data on shares of immigrants is from the OECD International Migration Database. Data for all variables for at least four years was available for 16 countries (not Greece, Poland, the Slovak Republic, and Slovenia), and for all five years for 12 countries (not the Czech Republic, Estonia, France, and Switzerland).

The general developments of the four explanatory variables over time are summarized in Table 2. Table 2 contains variable averages per year for the 12 countries from which data was available for all five years. The business cycle movements can be seen in the growth variable, which rises up to 2006, falls sharply in 2008, and increases again in 2010. The unemployment rate follows the growth rate (inversely) with approximately a two-year lag. Public debt (as share of GDP) is quite stable between 2002 and 2008, yet substantially higher in 2010. The immigrant stock is steadily increasing.

Section 4 investigates correlations between resistance to immigration and strongly negative views on economic and cultural effects of immigration respectively. Respondents have been asked the following two questions:

> **Q2:** *Would you say that it is generally bad or good for [country]'s economy that people come and live here from other countries?*
>
> **Q3:** *Would you say that [country]'s cultural life is generally undermined or enriched by people coming to live here from other countries?*

Answers were coded from 0 to 10, with 0 being the most negative reply. The share of respondents in the average country that participated in both years who gave one of the three most negative replies (0-2) increased from 16.0% in 2002 to 17.9% in 2010 regarding the economy and from 10.8% to 12.8% regarding cultural life.

3 – Contextual determinants of anti-immigration opinions

How anti-immigration opinions are influenced by contextual factors is analyzed in a within-country framework using the estimation equation:

$$allownone=\alpha_0+\alpha_1 growth+\alpha_2 unemp+\alpha_3 debt+\alpha_4 immigrant\_share+\alpha_5 country+\alpha_6 year$$

The dependent variable is the share of respondents in country *i* in year *t* who respond that no immigrants from the poorer countries outside Europe should be allowed to come and live in their country. Country fixed effects are included to account for country-specific factors. Year fixed effects account for common European trends in the dependent variable, to avoid spurious correlations due to increasing trends. Only the 16 countries with all data available for at least four out of five years are included. Four observations are missing from the resulting data panel: the Czech Republic and Estonia were missing from the survey for one year each, and all 2010 data was not available for Switzerland and France.

Regression results are shown in columns (1) and (2) of Table 3. The unemployment rate and the share of immigrants in the country are logged in the first specification. This is because an increase of a given number of percentage points in one of these variables is assumed to be more noticeable to the population if the previous value was low than if it was high. The logarithmic specification (1) performs better than specification (2). It has higher joint significance of the economic variables, and higher individual significance of the immigrant share. All coefficients have the expected signs, yet the coefficient on growth is not significantly different from zero. The effects in Table 3 are also reasonably large. Everything else equal, a one standard deviation increase in the logged unemployment rate increases the share of the population preferring no immigration by one-fifth of a standard deviation. A one standard deviation increase in public debt increases it by two-fifths of a standard deviation, and a one standard deviation increase in the logged immigrant share increases it by a full two-thirds of a standard deviation.

Other specifications that were tried but are not reported included interaction terms between the immigrant share and the unemployment rate or public debt. These terms were not significant though. Neither were interactions between the immigrant share and measures of immigrants' labor market success (employment and unemployment rates of immigrants or non-EU immigrants relative to those of total populations, using the EU Labour Force Survey) or shares of refugees among immigrants (using UNHCR data on asylum applications as proxies for numbers of refugees due to lack of better data). Removing any individual country or any of the first four sample years has no noteworthy effect on any of the estimated coefficients. Removing the last sample year (2010) decreases the coefficient on public debt to 0.08 and raises its p value to 0.250. This should not be surprising due to the relatively small variation in public debt between the first four sample years. It also highlights the advantage of studying the effects of the macroeconomic context on anti-immigration opinions in times of large fluctuations of the former.

3.1 – Within-country versus cross-country estimation

This study focuses explicitly on within-country variation to establish causality and argues that this strategy is more robust than cross-country strategies to omitting country-specific factors. For comparison, in column (3) of Table 3 the country fixed effects are excluded from the regressions and hence the cross-sectional dimension of the data is also allowed to influence the results. The coefficient on the share of immigrants is then *negative* and significant: anti-immigration opinions among natives are *less* prevalent when the share of immigrants is higher. While this result is the opposite of what previous cross-country studies have obtained with data from earlier years, it should not be a surprising result from a cross-country analysis. Plausibly it reflects reverse causality: countries with more immigration-friendly natives have more immigration.

The strengths of the negative cross-country and positive within-country (bivariate) relationships between the logged share of immigrants and the prevalence of anti-immigration opinions – for the thirteen countries with data available on both variables for all five years – are shown in Figure 2. The outlier observation in the dependent variable for Hungary represents the year 2002. None of the results reported in this study is sensitive to the inclusion of Hungary though.

Although sampled countries differ slightly between the present study and previous ones, this does not create the difference in estimated coefficients on the share of immigrants. The coefficient in column (3) is still significantly negative if countries that were not included in the studies by Semyonov et al. (2006) or by Scheepers et al. (2002) are excluded from the present sample. It is also significantly negative in the specification where the explanatory variables are not logged (not reported).

3.2 – Effects in subgroups

The reported effects of contextual factors on anti-immigration opinions are quite constant across various subgroups of the population. Estimates obtained with the same specification as in column (1) of Table 3 are reported for different subgroups in Table 4. The subgroups are males, females, young individuals (35 or less), old individuals (above 65), less educated (10 years of schooling or less; sample restricted to individuals that are at least 20 years old and hence plausibly not in school), highly educated (15 years of schooling or more), employed individuals (refers to last week), individuals in difficult economic circumstances (who respond that the situation on the present household income is “difficult” or “very difficult” on a four-point scale), and individuals with left-leaning and right-leaning political sympathies (answer codes 0-3 and 7-10 respectively on a 0-10 scale).

Dependent variable means by subgroup – evaluated across the 76 country-year observations in the regressions – are shown in the bottom row of Table 4. The old, the less educated, those in difficult economic circumstances, and those with right-leaning sympathies are the most negative groups. The young, the highly educated, the employed, and the left-leaning are the most positive groups. There is no significant difference between males and females.

The estimated coefficients on debt and the log immigrant share that are reported in Table 4 are quite stable across these ten subgroups. The coefficients on growth are still mostly negative but insignificant, yet the three economic variables are always jointly significant. Unsurprisingly, the unemployment rate is less important for individuals that are themselves employed. Perhaps less expectedly it is also less important for individuals with left-leaning political opinions.

3.3 – Effects on other measures of anti-immigration opinions

The main dependent variable analyzed in this study is the share of respondents per country and year who prefer that their country should allow no immigrants from the poorer countries outside Europe. For comparison this subsection reports corresponding results for two related questions:

> **Q4:** *To what extent do you think [country] should allow people of the same race or ethnic group as most [country] people to come and live there?*

> **Q5:** *How about people of a different race or ethnic group from most [country] people?*

The same four response alternatives apply as to Q1. Overall the total shares of respondents giving the most negative reply were slightly lower for these two questions: 8% for people of the same ethnicity, and 14% for people of different ethnicities, versus 16% for people from the poorer countries outside Europe.

Regression results for the shares of most negative responses to these two questions are reported in Table 5. They are very similar in structure to the effects reported in Table 3. Magnitudes of estimated effects can be directly compared. As expected the effects are smaller when the question concerns people of the same ethnicity. Concerning people of different ethnicities the magnitudes are very similar to those for the poorer countries outside Europe.

4 – Economic and cultural concerns behind resistance to immigration

The previous section estimated reasonably large effects of macroeconomic variables on the prevalence of anti-immigration opinions. However Card et al. (2012) – using more detailed data that was collected only in the 2002 round of the European Social Survey – reported that concerns about cultural composition were substantially more important than economic concerns in shaping respondents' attitudes to immigration. It is thus of interest to see – to the extent that this is possible with the data that was collected in all five rounds – if their conclusion also holds in within-country trend analysis. To this end the same dependent variable as before is now related to within-country variation over time in negative perceptions of the economic and cultural effects of immigration respectively in the estimation equation:

$$allownone=\beta_0+\beta_1 bad\_for\_economy+\beta_2 bad\_for\_culture+\beta_3 country+\beta_4 year$$

where the independent variables indicate response codes 0-2 on the 0-10 scales to the questions Q2 and Q3 on whether immigration is bad or good for the economy and whether it undermines or enriches cultural life. While these two questions do not probe individual attitudes to the same depth as the larger set of questions used by Card et al., results reported by these authors strongly suggest that they are good representatives (see their Table 2 on cultural life, and their Table 3 on the economy).

Results are reported in Table 6. Column (1) shows that the opinion that no immigration should be allowed is strongly related to the opinion that immigration is bad for the economy. An increase in one percentage point in the latter share implies a 0.75 percentage point increase in the former. On the other hand, the estimated partial correlation with the opinion that immigration undermines cultural life is small and not significantly different from zero. It is large and significant though when negative economic opinions are not controlled for as shown in column (2). The result that economic threat is far more important than cultural threat stands in stark contrast to the results obtained by Card et al. (2012). Interestingly it is not driven by the economic crisis. This is shown in column (3), where the sample is restricted to the pre-crisis years 2002-2006 and the difference between the two coefficients is even larger. Neither can it be explained by differences in variance between the two independent variables, as their standard errors are identical at 7.8 percentage points. Furthermore, the results do not change substantially if any individual sample year or country is excluded from the analysis or if the contextual variables from Section 3 are added as control variables (not reported).

The regressions behind column (1) in Table 6 have also been repeated in the same ten subsamples that were analyzed in Section 3. These results are not reported and contain little variance. All estimated coefficients on economic concerns are in the interval 0.55-0.80 with $p < 0.001$. The coefficient on cultural concerns is 0.52 and significant for the highly educated sample and otherwise far smaller and not significant.

4.1 – Effects of contextual variables on economic and cultural concerns

The results presented in the previous subsection indicate that economic concerns are far more important than cultural concerns in shaping negative opinions on immigration. In this subsection contextual macroeconomic factors are also found to significantly influence not only the opinion that immigration is bad for the economy but also the opinion that it undermines cultural life. This amounts to additional evidence in favor of economic factors really being important in shaping public resistance to immigration.

Regressions are performed where the dependent variables are the shares of respondents who think immigration is bad for the economy and undermines cultural life respectively. The independent variables are the same contextual variables as in Section 3. Results are reported in Table 7, with effects on economic concerns in column (1) and on cultural concerns in column (2). The F values at the bottom rows show that the three economic variables significantly predict both dependent variables. Unsurprisingly they do so more strongly for economic than for cultural concerns. As with negative policy opinions in Section 3, the most important economic predictor in both cases is public debt. The share of immigrants also significantly predicts both dependent variables.

5 – Conclusions

Public resistance to immigration in Europe has increased substantially over the last decade, concurrently with sharply deteriorating macroeconomic conditions in several countries. Understanding to what extent these phenomena are linked is crucial to understanding if this shift in public opinion is here to stay or if we should expect a reversed trend in coming years, if the European economy improves. Also, to the extent that we look upon rising resistance to immigration – and the associated rise in violence directed at immigrants – as a problem, it is vital to understand its roots to be able to devise strategies to counter it. There is, e.g., little reason to make an effort to educate the public about the limited empirical support for the proposition that immigrants importantly harm the economic prospects of natives, if what people are really worried about is that immigration threatens their cultural identity and way of life.

The main conclusion from the present study is that macroeconomic factors are indeed important in explaining public resistance to immigration. This result holds for men and women, for young and old, for more educated and less educated, for employed and unemployed, and for left-wing and right-wing supporters alike. Negative perceptions on the economic effects of immigration are also found to be more important than negative perceptions on the cultural effects in explaining the prevalence of the opinion that there ought to be no immigration. In addition to these results, contextual economic factors are found to significantly influence also concerns about the cultural effects of immigration, i.e., cultural concerns have increased more in countries with worse macroeconomic trends. Hence also what the individual respondent expresses as a cultural concern may to some extent have an economic foundation.

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Tables

Table 1. Percentages preferring no immigration by country in 2002 and 2010

Country:	2002 %	2010 %	T value for difference
Belgium	12.7	15.6	2.4*
Czech Rep.	12.3	25.4	9.4*
Denmark	6.8	10.1	3.2*
Finland	8.7	15.8	6.6*
France	13.4	13.6	0.2
Germany	7.8	10.1	2.9*
Greece	24.9	45.4	15.1*
Hungary	22.3	38.7	10.2*
Ireland	6.5	23.2	15.1*
Netherlands	9.0	13.0	4.0*
Norway	4.7	5.0	0.4
Poland	6.5	5.3	–1.5
Portugal	20.9	24.7	2.6*
Slovenia	10.2	13.7	2.8*
Spain	7.6	14.1	6.1*
Sweden	2.3	1.9	–0.8
Switzerland	2.4	8.8	7.9*
UK	15.9	24.0	6.5*
Average	10.8	17.1	4.2*

Note: A * denotes significance at the 5% level. Estonia and the Slovak Republic, which were not part of the 2002 round, are excluded from the table. The bottom row represents averages across countries, not individuals.

Table 2. Average independent variable trends

Year	Growth (%)	Unemployment (%)	Debt (% of GDP)	Immigrant stock (%)
2002	1.4	6.1	52	8.2
2004	2.6	6.8	52	8.8
2006	2.8	6.5	49	9.4
2008	–0.6	6.1	53	10.4
2010	1.5	9.3	65	10.9

Note: Values are averages for the 12 countries with all data available for all years: Belgium, Denmark, Finland, Germany, Hungary, Ireland, the Netherlands, Norway, Portugal, Spain, Sweden, and the UK.

Table 3. Effects of contextual factors on anti-immigration opinions

	(1) FE	(2) FE	(3) OLS	Mean	St. Dev.
Growth	–0.22	–0.35	0.72	0.017	0.022
	(0.334)	(0.142)	(0.24)		
Log unemployment	0.046*		0.074*	–2.7	0.43
	(0.015)		(0.007)		
Unemployment		0.30		0.070	0.031
		(0.184)			
Debt	0.12*	0.13*	0.034	0.49	0.22
	(0.006)	(0.004)	(0.510)		
Log immigrant share	0.12*		–0.054*	–2.39	0.52
	(0.007)		(0.012)		
Immigrant share		0.72		0.10	0.05
		(0.073)			
Country fixed effects	Yes	Yes	No		
Year fixed effects	Yes	Yes	Yes		
F (economic variables)	8.8	6.4	3.5		

Notes: N=76. The number of countries is 16 (not Greece, Poland, the Slovak Republic, and Slovenia). P values in parentheses. A * denotes significance at the 5% level. The F value refers to the joint significance of the three economic variables (not the immigrant share). The mean of the dependent variable is 0.155, with a standard deviation of 0.099.

Table 4. Effects of contextual factors on anti-immigration opinions in subgroups

	(1) Male	(2) Fem.	(3) Age ≤35	(4) Age >65	(5) Educ ≤10	(6) Educ ≥15	(7) Has work	(8) Diff. econ.	(9) Left	(10) Right
Growth	-0.24	-0.21	-0.23	-0.14	-0.18	0.02	-0.32	-0.12	-0.25	-0.38
	(0.34)	(0.37)	(0.38)	(0.93)	(0.58)	(0.92)	(0.20)	(0.69)	(0.35)	(0.19)
Log unemp	0.05*	0.05*	0.04	0.07*	0.08*	0.03	0.02	0.05*	0.02	0.05*
	(0.02)	(0.02)	(0.08)	(0.01)	(0.00)	(0.10)	(0.25)	(0.03)	(0.40)	(0.05)
Debt	0.14*	0.11*	0.09	0.17*	0.13*	0.11*	0.13*	0.14*	0.17*	0.13*
	(0.01)	(0.01)	(0.05)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.00)	(0.02)
Log imm share	0.14*	0.10*	0.10*	0.10	0.10	0.11*	0.12*	0.10	0.08	0.12*
	(0.00)	(0.02)	(0.05)	(0.10)	(0.10)	(0.01)	(0.01)	(0.07)	(0.09)	(0.02)
Country fixed eff.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed eff.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F (econ)	8.7	7.7	4.5	7.6	8.0	4.6	4.1	6.2	6.6	7.1
Sample share %	47	53	29	20	30	27	53	20	21	25
Mean Dep.	15.4	15.4	10.9	21.4	23.0	9.1	13.1	19.9	13.5	18.6

Notes. N=76. The number of countries is 16 (not Greece, Poland, the Slovak Republic, and Slovenia). P values in parentheses. A * denotes significance at the 5% level. The F value refers to the joint significance of the three economic variables (not the immigrant share).

Table 5. Effects of contextual factors on other measures of anti-immigration opinions

	(1) Same ethnicity	(2) Different ethnicity
Growth	–0.10	0.032
	(0.585)	(0.879)
Log unemployment	0.024	0.033*
	(0.098)	(0.048)
Debt	0.080*	0.11*
	(0.021)	(0.007)
Log immigrant share	0.084*	0.12*
	(0.014)	(0.002)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
F (economic variables)	4.9	6.2
Dependent variable mean	0.076	0.135

Notes: N=76. The number of countries is 16 (not Greece, Poland, the Slovak Republic, and Slovenia). P values in parentheses. A * denotes significance at the 5% level. The F value refers to the joint significance of the three economic variables (not the immigrant share).

Table 6. Correlations between negative economic, cultural, and policy opinions

	(1) Full sample	(2) Full sample	(3) Rounds 1-3
Bad for economy	0.75*		0.81*
	(0.000)		(0.000)
Undermines cultural life	0.10	0.85*	–0.25
	(0.552)	(0.000)	(0.388)
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
N	96	96	56
Dependent variable mean	15.6	15.6	14.5

Notes: The number of countries is 20. The number of “refugee countries” is 5. P values in parentheses. A * denotes significance at the 5% level.

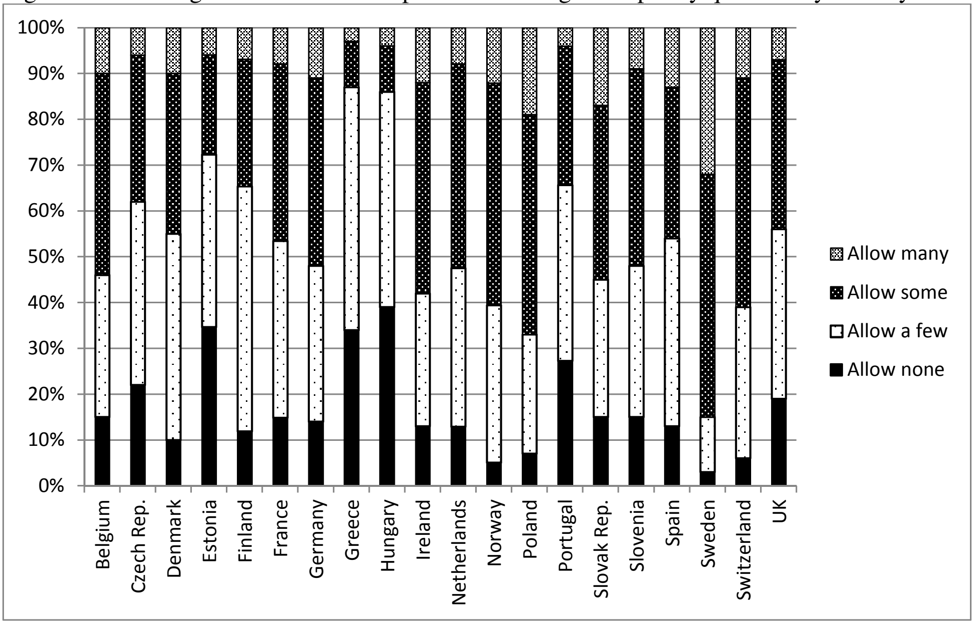
Table 7. Effects of contextual circumstances on negative economic and cultural opinions

	(1) Bad for economy	(2) Undermines cultural life
Growth	–0.24	0.20
	(0.295)	(0.220)
Log unemployment	0.031	–0.011
	(0.089)	(0.386)
Debt	0.16*	0.087*
	(0.000)	(0.004)
Log immigrant share	0.090*	0.11*
	(0.034)	(0.000)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
F (economic variables)	10.3	3.3

Notes: N=76. The number of countries is 16 (not Greece, Poland, the Slovak Republic, and Slovenia). P values in parentheses. A * denotes significance at the 5% level. The F value refers to the joint significance of the three economic variables (not the immigrant share).

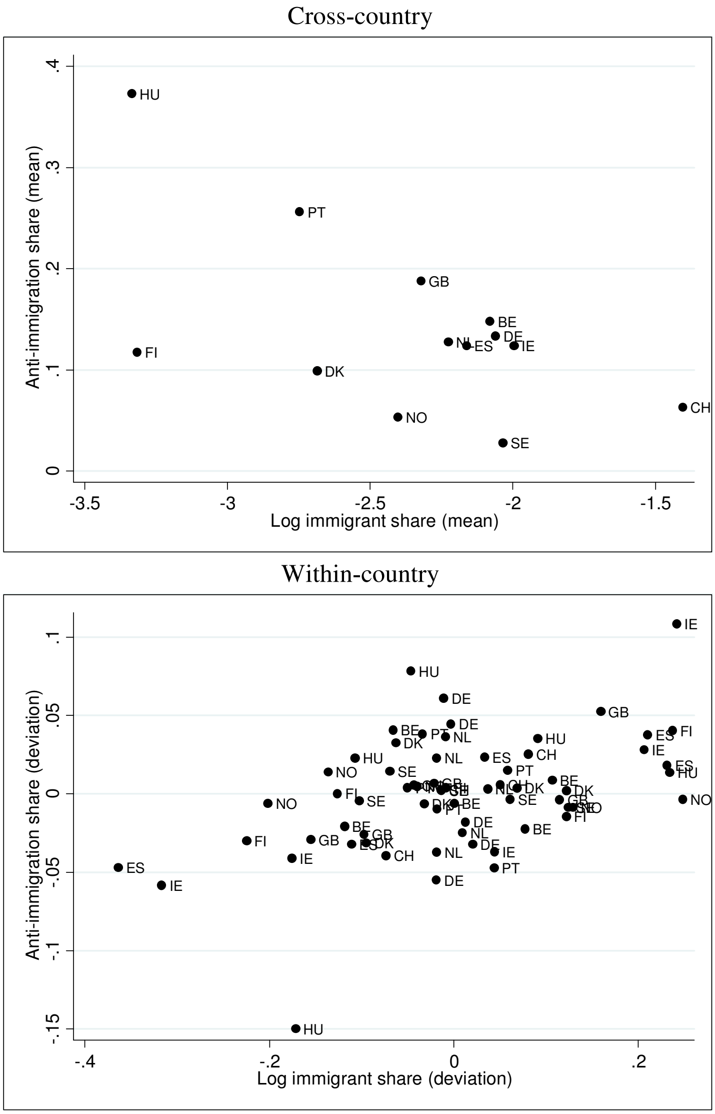
Figures

Figure 1. Percentage distribution of responses to immigration policy question by country



Note: The figure shows the distribution of responses to question Q1. Shares are averages per country across all individuals and all survey rounds. 16 countries participated in all five rounds. Estonia and the Slovak Republic did not participate in the 2002 round, and the Czech Republic and Greece did not participate in the 2006 round.

Figure 2. Relationships between immigrant shares and anti-immigration opinions



Notes: Sampled countries are those with data available for both variables in all five years. The anti-immigration share is the share of respondents preferring no immigration. Variable values in the cross-country panel are averages across the five sample years. Values in the within-country panel are deviations from these averages.

Paper VIII

Free immigration and welfare access: The Swedish experience

Abstract

With the expansion of the European Union from 15 to 25 member countries in 2004, fears of migrants' excessive welfare use lead 14 of the 15 older member countries to impose restrictions on access of citizens of the new member countries – the A10 countries – to their welfare systems. Sweden was the only exception. This paper evaluates the net contributions of post-enlargement A10 immigrants on Swedish public finances in 2007. A10 immigrants make a small but significant positive net contribution. On average, they generate less public revenue than the population on average, but also cost less, in the end yielding a positive net result. A10 immigrants do not benefit more from basic social welfare than the population on average.

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1 – Introduction

A contentious issue in the immigration debate in many high-income countries is immigrants' actual or potential use of the host country's welfare systems. The risk of immigrants arriving with the intent of benefiting from welfare systems and thus draining public finances is a common argument in favor of restricting immigration, as in the debate preceding the EU enlargement in 2004. The same argument is supported by economic theory in the writings of, e.g., Friedman (1977), Borjas (1999), and Razin et al. (2011). Empirical studies of whether welfare benefits attract migrants to any substantial extent yield mixed results. Studies of immigrants' net contributions to public finances show that these are generally close to zero, but are confined to cases of restricted immigration, since until recently there has not been any case of a modern welfare state allowing free immigration from poorer countries. A recent exception is Sweden since the EU enlargement in 2004, when the EU expanded from 15 member countries (henceforth referred to as EU15) with high and relatively similar income levels by adding another 10 countries (referred to as the A10 countries) with substantially lower income levels on average. Sweden was the only EU15 country that did not impose restrictions on access of the new EU citizens to its welfare systems.

The Swedish case thus provides a possibility to study empirically the public finance contributions of immigrants arriving under a regime of unrestricted immigration and equal access to welfare systems. Two questions are of major importance. The first is the question of the overall net contribution of A10 immigrants to Swedish public finances, revenues and costs taken together. The second is whether A10 immigrants are overrepresented as beneficiaries of welfare systems, relative to the total Swedish population or to immigrants from the richer EU15 countries. This study provides answers to both questions, using detailed individual data from 2007 on tax payments, welfare receipts, and age structure of a sample of 3,000 A10 immigrants who moved to Sweden in 2004-2006. The estimated net contribution to public finances is small but significantly positive, and A10 immigrants' use of basic social welfare (minimum level of subsistence) is similar to the total Swedish population on average.

2 – The EU enlargement in 2004 and Swedish immigration

The ten countries that acceded to the EU in 2004, the A10 countries, had on average substantially lower income levels and higher unemployment rates than the EU15 countries. Purchasing power adjusted income per capita and unemployment rates for the two blocks of countries in 2003, the year before the enlargement, are shown in Table 1. The enlargement was preceded by extensive debate in the richer EU countries about the possible consequences of free labor mobility between dissimilar countries. There were fears that A10 citizens would migrate to the richer EU15 countries and use their social welfare systems excessively. Eventually, most of the EU15 countries imposed various restrictions on access of A10 citizens to their labor markets and welfare systems. Fewer restrictions were imposed by the UK, Ireland, and Sweden, where Sweden was the only country that did not impose any restrictions at all (Gerdes and Wadensjö, 2010).

During the first years after the EU enlargement, the UK and Ireland received more immigrants than Sweden from the A10 countries, relative to their total populations. According to data from the UK Office of national statistics, the UK immigration rate from the new EU member states in 2004-2006 was about three times as high as the Swedish rate, and according to less exact estimates in Barrett (2010), the Irish rate may have been about six times as high as the Swedish. Fewer migrants than initially expected arrived in Sweden, yet the rate increased rapidly, as seen in Table 2.

2.1 – Immigrant characteristics

A10 immigrants differ from the total Swedish population in respects that are of first order importance for their net contribution to public finances. Most importantly, they have lower incomes and thus pay less tax, and very few of them are old, which implies low public costs related to old age. This section reviews these background characteristics in detail.

To describe the characteristics of A10 immigrants, I use micro data from the *Linda* database. The *Linda* database is managed by Statistics Sweden and contains detailed information from public authorities on two large samples of the Swedish population: the general sample and the immigrant sample. The general sample comprises a random 3% of the total population (referred to as sampled individuals), as well as all individuals belonging to the same households as those 3%. The immigrant sample comprises a random 20% of the Swedish immigrant population, plus all those who belong to their households. The database is longitudinal: it contains the same individuals each year, and each year the sample is adjusted through the addition of some new individuals to maintain its representativeness of the whole population. See Edin and Fredriksson (2000) for a detailed description of the data source.

The 2007 *Linda* immigrant sample contains information on 3,392 sampled individuals who immigrated to Sweden from the A10 countries in 2004-2006 and did not register emigration through 2007. It also contains data on 5,779 sampled individuals who immigrated to Sweden from the rest of EU15 during the same period.[1] Figures relating to the latter group are included in this paper to highlight the differences in net contributions of immigrants of different origin to public finances. The general *Linda* sample contains 323,418 sampled individuals. The samples together form the dataset used in this study. Only sampled individuals are included in order to maintain the randomness of the sample.

One important difference between A10 immigrants and the total Swedish population lies in their age distributions, which are shown in Figure 1. As the figure shows, A10 immigrants are heavily concentrated in the younger half of the working ages, and there are almost no individuals above retirement age. The distribution of EU15 immigrants is similar but not quite as uneven.

Table 3 shows the distributions of income from work and business activity for ages 25-64. When obtaining reliable information on income distributions for recent immigrants, we are confronted with the question of which immigrants still remain in the country. While data on immigration from these countries are very reliable, re-emigration data are not. There is no real incentive for emigrants to register re-emigration. In fact, some emigrants may even have an incentive *not* to register re-emigration, because one may then lose one's entitlements to sickness, parental leave, or unemployment support. A clear indication of immigrants having re-emigrated without registering is that out of all A10 immigrants in *Linda* who arrived in 2004-2006 and have not registered re-emigration, 11% have exactly zero household disposable income in 2007, which is not at all in parity with the rest of the population, but corresponds well to the estimate in Gerdes and Wadensjö (2010) that official statistics may contain around 10% of recent A10 immigrants who are no longer in Sweden. I deal with this flaw by deleting all individuals with household disposable income of zero or less from the sample. The smaller number with a registered disposable income below zero may not be as likely to have left Sweden, but there are probably other important errors in the data on these

[1] One may note the large difference between the ratio of these numbers and that of the corresponding numbers in Table 2. This is due to a much higher re-emigration rate of EU15 immigrants.

individuals. They are few enough to make their inclusion or exclusion irrelevant for the analysis.

As shown in Table 3, most recent immigrants from EU15 and A10 countries earn less than the total population across the distribution, although the top segment of EU15 immigrants earn relatively more. Substantial shares of the recent immigrant populations do not earn any income, and this is after deleting individuals for whom the reported *household* disposable income, including welfare income, is non-positive. While some of these individuals may in fact also have left the country, their households all have registered positive income, so it is plausible that the majority of them are supported by other household members.

The statistics on 2004-2006 A10 immigrants presented so far indicate both positive and negative factors concerning Swedish public finances. Their age structure is obviously favorable, as it implies low costs for elderly care, while the fact that they earn less income from work should imply that they contribute less to public finances through taxes. The rest of this paper presents a detailed analysis of public revenues and costs in order to estimate the net contributions of A10 immigrants to Swedish public finances.

3 – Method

The method used in this study is to ascribe, as far as possible, all Swedish public revenues and costs to the proper individuals or groups of individuals in the population, and thus to estimate the net contribution of A10 immigrants. Although some parts of public services, such as infrastructure and defense, are more or less equally distributed, the majority of public costs can be attributed to specific individuals or groups of individuals. On the public revenue side, an even larger share relates to specific individuals, although there is a certain share for which the connection is more far-fetched here as well, such as revenues derived directly from larger corporations.

The *Linda* dataset contains detailed information on all tax payments to and all individual receipts from the public finances for all individuals in the data. These detailed data correspond to about one-third of all public sector revenues and one-third of all public sector costs per individual. In addition, the income data in *Linda* can be used to estimate payroll taxes with high precision, adding a detailed breakdown of another third of public revenues. Income data can also be used to estimate VAT payments, which will not be very credible per individual, but arguably so when averaging over large groups and using aggregated data on the relation between income and consumption. In total, *Linda* data can be used to ascribe with high credibility 78% of public sector revenues in 2007 to different groups of individuals.

On the cost side, about one-third of all public sector costs are costs of schooling and care, for which there are no detailed data on individual use of services. Yet there are detailed data on these costs by age group, and when averaging over larger groups of people these can be used to ascribe costs to different groups with high precision. This results in 62% of public sector costs in 2007 being ascribed to different groups. The share is thus lower on the public cost side than on the revenue side, which is mostly a reflection of the fact that a substantial part of public sector costs are counted equally for all.[2]

[2] The variables collected in this study represent important public revenues and costs that are directly related to specific individuals. Different methods are used in the literature to distribute the remaining elements of the public economy between immigrants and natives. Some costs may be more immigrants specific, while at the same time one may argue as Lee and Miller (1998) that on the margin immigrants don't add proportionately (if at all) to expenses on public goods such as infrastructure and defense, and therefore shouldn't be assigned such

Having ascribed all the public revenues and costs included in the study to different individuals, calculating the net contribution of A10 immigrants to public finances amounts to a simple estimate of difference in means between these immigrants and the total population. It could be argued that the proper reference value to set to zero would be the total population average *less the immigrant group in question*. However, since the group of interest in this paper amounts to less than 0.2% of the total population, this would not affect the results. The difference in means correctly estimates the net contribution of the group to the public sector if the public sector runs a balanced budget. Yet in 2007 the public sector ran a surplus of 11,400 SEK per individual, so to correctly estimate the net contribution, this surplus is added to the difference in means.

The method used is borrowed from a set of studies estimating the net contributions of immigrant stocks to public finances in various high-income countries. These studies were surveyed recently by Rowthorn (2008); Swedish studies were surveyed by Ekberg (2009). The resulting estimates of immigrants' net contributions to public finances are generally between +1% and -1% of GDP, yet estimates are of limited usefulness for immigration policy evaluation since the immigrant stocks at hand are the consequence of generations of immigration policies. In the present study, the method is used for more direct policy evaluation, focusing only on one group of immigrants that arrived under one specific policy regime.

3.1 – Data treatment

Direct tax payment data is reported from the tax agency to Statistics Sweden, making the values in the dataset highly reliable. Earnings data are also directly reported, and payroll taxes are estimated as 32.42% of earnings, which was the payroll tax rate in 2007. When estimating value-added taxes (VAT) from earnings data, I take into account that VAT payments are a highly concave function of earnings. Statistics Sweden publishes VAT payment estimates per disposable income decile of the population. I use these data to ascribe to each individual in the dataset VAT payments equal to the estimated mean of his/her income decile.

Similarly, all data on transfers to individuals, as well as student loan repayments, are directly reported to Statistics Sweden and are thus very reliable. The estimates of individual costs of education and care on the other hand are made using age (and gender) group means. Child care, schooling, elderly care, and disability care are municipality responsibilities, and all municipalities report average costs, per age group where relevant. These data are published by the Swedish Association of Municipalities and Regions (Sveriges Kommuner och Landsting, 2007). Schooling cost estimates per child are taken directly from these data. Child care cost estimates per child are adjusted for female labor force participation, as all immigrant groups have substantially lower female labor force participation than the Swedish population mean. Childcare costs for each immigrant group and immigration year are multiplied by the share of females aged 20-49 who report positive labor income in that group, and divided by the same share in the population. For A10 immigrants, this implies a multiplication by about 0.9.

The data on elderly care are adjusted for a more detailed data breakdown published by the National Board of Health and Welfare (Socialstyrelsen, 2008) of elderly care costs by five year age interval and gender. The data on aggregate disability care costs from the Swedish

costs in the analysis. This study adds no new perspective to those issues and assigns those elements equally to everyone.

Association of Municipalities and Regions are made individual by distributing them evenly across those individuals who received an individual disability support transfer (in *Linda*).

Hospital care is a regional and not a municipal responsibility. Thus, hospital care costs cannot be estimated using the same dataset as other care-related costs. Instead I rely on a study by Borgquist et al. (2010), who estimate hospital care costs by age group in 2007 in the county of Östergötland, which is deemed representative of Sweden as it includes both rural areas and two larger cities (neither one is among the country's four largest), and both a university environment and basic industry.

One problem in the data concerns calculating the number of children born to immigrants after they arrived to Sweden, since these children are not defined as immigrants. Hence, when identifying them in the data they may be mixed up with children who entered the household because the whole household composition changed. However, since the number of children born to immigrants up to three years after their arrival is not very large, any estimation errors do not affect the results to any large degree.

The data elements used in the study and their importance for the Swedish public sector are summarized in Table 4. The value of 36,024 toward the bottom is the net position per Swedish inhabitant vis-à-vis the public sector that is left when all the differently ascribed elements are accounted for. Subtracting this number from the total revenues minus costs for an individual or group, and adding the public sector surplus of 11,400 SEK, gives the estimated net public sector contribution of that individual or group. How this value varies with age is shown by the solid line in Figure 2.

4 – Results

The net contributions of the average A10 and EU15 immigrants to Swedish public finances are shown in Table 5. A positive sign indicates larger revenue or smaller cost, compared to the population at large, and a negative sign smaller revenue or larger cost. The net contributions of both immigrant groups are positive and significantly different from zero. Due to their age structures, both groups cost the public sector substantially less than the population average with respect to pensions and elderly care. For A10 immigrants, this smaller cost is balanced by smaller tax revenues, as they earn less and thus pay less tax than the population on average. Hence, the Revenues Total minus Costs Total row in Table 5 shows that the difference between A10 immigrants and the total population is not significantly different from zero, so the positive net contribution of the average immigrant to public finances closely corresponds to the public sector surplus. For EU15 immigrants, the negative difference in tax payments is smaller and the net contribution is more positive yet still economically unimportant. Multiplied by the number of immigrants, the total net contribution of all EU15 immigrants is only about 1/1,500 of the public sector turnover.

A10 immigrants' low hospital care costs are also due to the age structure. Notably, A10 immigrants also cost substantially less in disability care and early retirement than the population average. The differences on the remaining rows of Table 5 are smaller in absolute numbers, and are sometimes positive and sometimes negative. As regards whether welfare systems attract immigrants to any large extent, notably the difference between A10 immigrants' and the total population's average use of basic social welfare (minimum level of subsistence) is small and not significantly different from zero. While not amounting to any proof against welfare systems attracting low skilled migrants, it is important to note that the migrant group is not overrepresented in the use of basic welfare. The conclusion from Table 5 is that in the case of Sweden since the EU enlargement, free immigration and generous welfare access have managed to coexist.

On three rows, the numbers of Table 5 may be affected by relatively low immigrant eligibility. These are sickness and parental leave support, which require eight months of working for eligibility, and unemployment support, where full eligibility is reached after twelve months of working. Thus, not all immigrants who arrived in 2006 were fully eligible for these benefits from January 1st 2007. Table 6 contains the values that correspond to those in Table 5, but for 2004-2005 and for A10 immigrants only. It shows that the immigrants who arrived during this period indeed used more of these systems in 2007. Substituting Table 6 values for the corresponding values in Table 5 changes the sum of revenues minus costs per A10 immigrant from +1,288 to -791 SEK, which is still not significantly different from zero.

The standard errors used in calculations of the T values in Table 5 are based only on (finite sample corrected) variance in assigned values, and thus do not reflect uncertainty in the value assignment itself, i.e., they do not capture the unknown variation in age-related costs within age groups or in consumption within income deciles. Most importantly, there are no data that enable identification of whether A10 immigrants are on average different from the total population in their consumption behavior after controlling for income, or in their use of hospital care after controlling for age. Under the hypothesis that those differences are no larger than 10%, the estimates in Table 5 may be wrong by at most about 2,000 SEK for VAT and at most 1,000 SEK for hospital care.

It would have been informative to split the results in Table 6 by educational attainment, yet the validity of such an analysis would be too hampered by the large share of missing values on educational attainment (23%) and the strong correlation between a missing value on educational attainment and some of the parameters under study.

5 – Discussion

The only existing study whose results are directly comparable with the ones obtained in this paper is the one by Dustmann et al. (2010), who estimate the contribution of post EU-enlargement immigrants from the “A8” countries, i.e., the A10 countries less Cyprus and Malta, on UK finances. The contribution they find is more positive than the one identified in this paper. I find three possible explanations for this difference: (1) Immigrants did not obtain access to welfare systems in the UK to the same extent that they did in Sweden (Gerdes and Wadensjö, 2010; Dustmann et al., 2010). Immigrants more likely to be eligible for welfare (in Sweden) may thus have had relatively higher incentives to choose Sweden than the UK. (2) Welfare systems are on average more generous in Sweden than in the UK. According to OECD statistics, social expenditure in the UK in 2005 was 20.6% of GDP, while in Sweden it was 29.1%. Again, immigrants who are more likely to be eligible for welfare may have had relatively higher incentives to choose Sweden. (3) More skilled emigrants in terms of English language proficiency should have had relatively higher incentives to go to the UK, and we would expect more skilled immigrants to have a more positive impact on public finances. In generalizing the results presented in this paper to a prediction of the effects of free migration from poorer to richer countries in general, points (2) and (3) indicate that certain other high-income countries may have reason to expect more positive results. The other important issue is income levels in the emigration countries: if they are lower than in the emigration countries included in this study, there may be reason to expect more negative results.

The starting point for this paper was the statement that it is not possible for a welfare state to have free immigration from poorer countries. The analysis has shown that in the case of

Swedish post EU-enlargement immigration, at least in the short term this statement has not been correct: free immigration and welfare access have coexisted, and so far the migrants have contributed positively to public finances. We cannot yet say what the longer-term consequences of this immigration will be. Immigrants will become older and thus more costly for the welfare state. On the other hand, the vast majority of them arrived in the younger half of the working ages, implying that they will have plenty of time to contribute to public finances before that, and in the end the most decisive factor for long-term outcomes will almost certainly be to what extent they re-emigrate before they become old, which is entirely unknown today. At any rate, whether the long-term outcomes will be slightly better or slightly worse than the short-term ones, they are unlikely to be devastating. Had immigrants arrived with the intent of receiving welfare benefits, there is no reason why we should not have been able to identify their excessive welfare use already in the first years. The analysis of this paper shows that they are not on average very different from the rest of the Swedish population. They might fare a bit better or a bit worse as they remain longer in Sweden, but according to what is found in the data they do not seem to have arrived with an intention to live on welfare.

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Tables

Table 1. Purchasing power adjusted GNI per capita and unemployment rates in 2003

Country group	GNI per capita (PPP $)	Unemployment rate
Old EU member countries (EU15)	27,800	8.0%
Accession countries (A10)	13,300	14.9%

Notes: Numbers are averaged over total populations, not countries. Data source: World Development Indicators (the World Bank).

Table 2. Swedish yearly immigration from A10 and other EU15 countries 2003-2007

Year	No. A10 immigrants	‰ of home population	No. EU 15 immigrants	‰ of home population
2003	2,381	0.03	19,005	0.05
2004	4,232	0.06	18,661	0.05
2005	5,559	0.07	19,403	0.05
2006	9,178	0.12	23,690	0.06
2007	10,767	0.14	25,065	0.07

Notes: Data source: Statistics Sweden. A10 immigration numbers prior to 2003 were similar to the 2003 numbers.

Table 3. Distributions of income (in SEK) from work and business, ages 25-64

	mean	10th percentile	25th percentile	median	75th percentile	90th percentile
Total population	267,000	85,000	171,000	249,000	327,000	435,000
EU15 immigrants	245,000	0	19,000	169,000	306,000	508,000
A10 immigrants	152,000	0	39,000	139,000	226,000	300,000

Table 4. Data elements and their importance for public finances

	Average value per Swedish inhabitant (SEK)	Total value (billion SEK)	Percent of total public sector spending
Revenues			
--Direct taxes	59,144	542.6	35.3%
--Payroll taxes	58,205	534.0	34.8%
--Value-added taxes	21,577	198.0	12.9%
--Student support repayment	1,192	10.9	0.7%
Revenues total	**140,117**	**1,285.5**	**83.7%**
Transfers			
--Sickness support	3,020	27.7	1.8%
--Public pensions	32,332	296.6	19.3%
--Parental leave support	2,680	24.6	1.6%
--Unemployment support	2,772	25.4	1.7%
--Early retirement	6,140	56.3	3.7%
--Basic social assistance	929	8.5	0.6%
--Other family support	4,706	43.2	2.8%
--Other transfers	302	2.8	0.2%
--Student support	2,436	22.3	1.5%
-Transfers total	**55,318**	**507.5**	**33.1%**
Education and care			
--Child care	5,947	54.6	3.6%
--Schooling	11,938	109.5	7.1%
--Hospital care	15,717	144.2	9.4%
--Elderly care	10,992	100.8	6.6%
--Disability care	4,181	38.4	2.5%
-Education and care total	**48,775**	**447.5**	**29.2%**
Costs total	**104,093**	**955.0**	**62.2%**
Revenues total minus Costs total	**36,024**		
Difference	24,624		
Public sector surplus	**11,400**		

Note: N=309,502. Numbers are averaged over the total population of all ages.

Table 5. Net contributions of A10 and EU15 immigration on public finances

	Contribution of average A10 immigrant (SEK)	Absolute T value	Contribution of average EU15 immigrant (SEK)	Absolute T value
Revenues				
--Direct taxes	-30,220*	35.0	-622	0.3
--Payroll taxes	-20,985*	29.2	-3,124	2.2
--Value-added taxes	-3,774*	26.1	-961*	6.0
--Student support repayment	-1,150*	73.9	-1,060*	45.3
Revenues total	**-56,129***	**35.1**	**-5,768**	**1.6**
Transfers				
--Sickness support	+1,745*	9.8	+1,665*	9.6
--Public pensions	+31,975*	180.6	+28,942*	73.8
--Parental leave support	-1,638*	6.1	-365	1.8
--Unemployment support	+1,233*	7.0	+1,470*	11.4
--Early retirement	+5,916*	67.0	+4,954*	31.2
--Basic social assistance	-273	1.8	-6	0.0
--Other family support	+272	1.5	+1,610*	13.2
--Other transfers	+104	1.4	+252*	8.8
--Student support	-117	0.6	-46	0.2
-Transfers total	**+39,217***	**71.8**	**+38,477***	**64.1**
Education and care				
--Child care	-1,233*	3.4	-3,704*	11.0
--Schooling	-170	0.4	+283	0.7
--Hospital care	+5,239*	97.3	+4,322*	59.3
--Elderly care	+10,772*	117.0	+9,985*	68.6
--Disability care	+3,592*	11.3	+3,762*	16.1
-Education and care total	**+18,200***	**27.2**	**+14,649***	**26.0**
Costs total	**+57,417***	**71.5**	**+53,126***	**67.6**
Revenues total minus Costs total	**+1,288**	**0.7**	**+47,359***	**12.2**
Public sector surplus	+11,400	-	+11,400	-
Net contribution	**+12,688***	**6.4**	**+58,759***	**15.1**

Notes: The table shows difference in means estimates of A10 and total population values. Numbers are averaged over the total populations of all ages. A positive sign indicates either larger public revenue or smaller public cost. Significance at 1% level indicated by *. Number of A10 immigrants in sample = 3,057. Number of EU15 immigrants in sample = 4,306.

Table 6. Estimated values for 2004-2005 A10 immigrants only

	Contribution of average A10 immigrant (SEK)	Absolute T value
--Sickness support	+1,132*	3.6
--Parental leave support	-2,923*	6.4
--Unemployment support	+264	0.8

Notes: The table shows difference in means estimates of A10 and total population values. Numbers are averaged over the total populations of all ages. A positive sign indicates either larger public revenue or smaller public cost. Significance at 1% level indicated by *. Number of A10 immigrants in sample = 1,539.

Figures

Figure 1. Age distributions of total population and immigrant groups

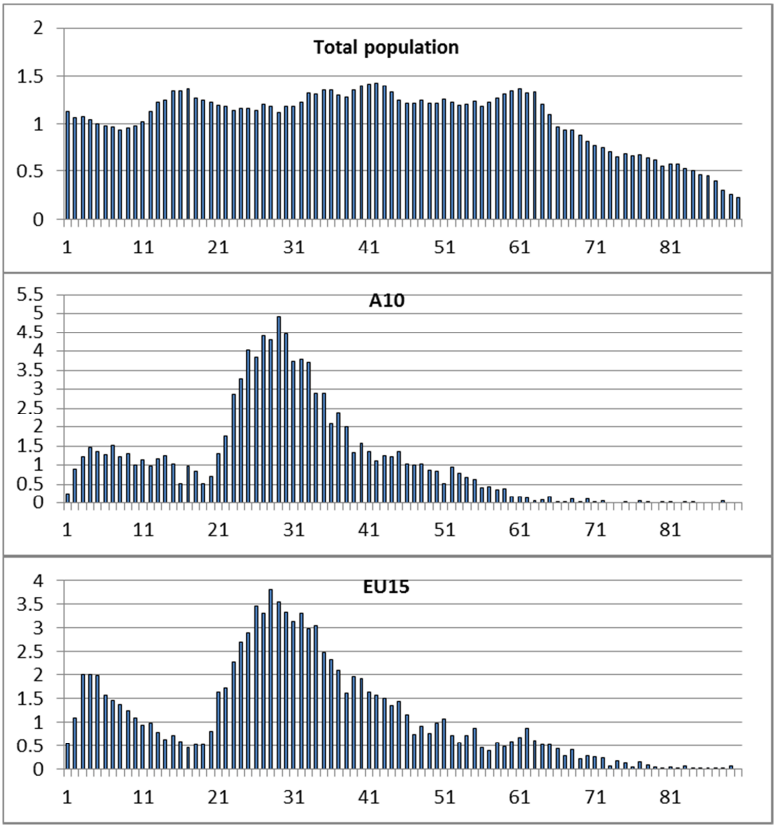
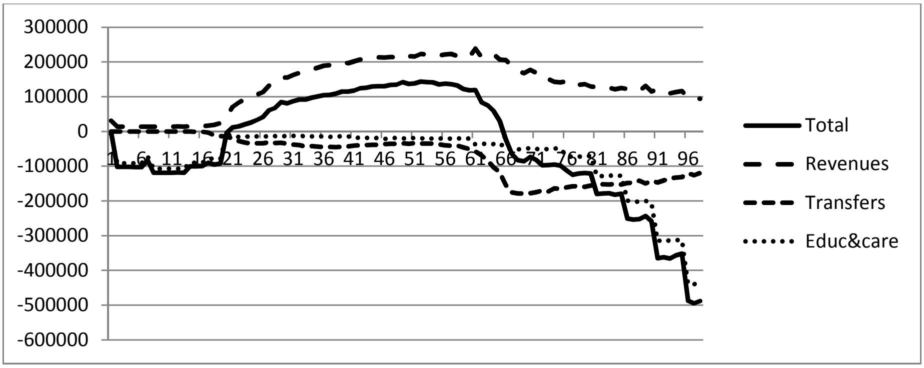


Figure 2. Variation in revenues and costs with age



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