The Effect of Immigration on Natives' Earnings in Finland

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Abstract

Using a shift-share instrument, I study inflows of foreign-background immigrants to Finland and estimate how immigration affects natives' earnings at various points of the income distribution. My point estimates for the earnings effects of immigration are negative and statistically significant for natives below the 40th earnings percentile, near zero for those between the 40th and 55th percentiles, and positive and statistically significant for those in and above the 60th percentile. Notably, the intensity of the estimated effects tends to increase further away from the median. These results indicate that immigrants are closer substitutes for low-skilled Finns and more complementary to high-skilled native workers.

Keywords: *Immigration, Natives' earnings, Income distributions*
JEL codes: *J31, J61*

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1 Introduction

Immigration to Finland has increased rapidly since the early 2000s with the share of immigrants in the population increasing from 2.2% in 2000 to 9.1% in 2022 (Statistics Finland, 2023b). While immigration is often viewed as a necessary response to declining birth rates and population aging, it has also raised concerns about its economic consequences, particularly for native workers. Textbook economic models with heterogeneous labor types argue that the impact of immigration depends on the differences between the skill compositions of native and immigrant labor. An influx of immigrants of a certain skill profile will cause natives with equivalent skills to face stiffer competition in the labor market and hence slower wage growth. On the other hand, natives who are more complementary to immigrants can expect higher earnings as demand for their now relatively more scarce skills increases (Borjas, 2010). Identifying which natives belong in which category is ultimately an empirical question, and one that my results help answer in the case of Finland.

I examine the effect of immigration on the earnings of native Finns between the years 2001 and 2018. Following the technique of Dustmann et al. (2013), I use regional variation in immigration rates to estimate the impact of a change in the ratio of foreign-background immigrants to natives in the local labor force on the earnings of the local x^{th} percentile native earner. Essentially, I compare changes in the annual earnings of natives of a specific percentile in a municipality or travel-to-work area (TTWA[1]) with large flows of immigrants to changes in the earnings of an equivalent group in locations with low immigration. Estimates for the lower percentiles measure how immigration affects relatively low-skilled natives, while the greater percentiles reflect effects on high-skilled natives. The advantage of this approach is that it does not require a pre-allocation of natives and immigrants into skill-cells based on educational attainment and experience, instead allowing individuals to take their true spot in the earnings distribution. This enables the measurement of total effects of immigration, as opposed to skill-cell relative effects, while still shedding light on treatment effect heterogeneity along the native skill distribution (Dustmann et al., 2016).

As immigrants' location choices are likely to be influenced by local labor conditions, I instrument for them with an immigrant enclave shift-share instrument, which allocates immigrant inflows from different countries of origin across locations in proportion to the local shares of immigrants from that country in the reference year of 1993. Thus, my core identifying assumption is that locations with different stocks of immigrants in 1993, both in volume and country of origin composition, would have evolved similarly in the estimation period in the absence of immigration. While this is not directly testable, I argue that it is plausible given the limited role of work-based migration to Finland in the early 1990s. At that time only 10% of immigrants cited work as their primary reason for moving (Martikainen et al., 2013), implying that their location choices are not likely to have been greatly influenced by local labor conditions. Furthermore, even if locations of high and low-immigration in the 1990s do follow divergent paths in the 2000s, so long as these trends are shared across the native earnings distribution, a comparison of estimated effects for high vs low-earnings natives can still give insight into the distributional consequences of immigration.

The point estimates of the earnings effects of immigration from my preferred specification are negative and statistically significant for natives below the 40^{th} earnings percentile, near zero for those between the 40^{th} and 55^{th} percentiles, and positive and statistically significant for those in and above the 60^{th} percentile. My estimates suggest that moving from the 10th to the 90^{th} percentile of the TTWA distribution of the immigrant-to-native ratio decreases earnings growth of the 10^{th} percentile native earner by 1660€[2]. The equivalent estimate for the 90^{th} percentile earner is an increase of about 2400€. These results indicate that the skill composition of immigrants to Finland makes them closer substitutes for low-skill natives and more complementary to high-skilled natives. This suggests that immigration increases earnings disparities among the native population, at least in the short run. However, several limitations temper the interpretation of these point estimates. Most notably, it is difficult to establish the validity of the parallel trends assumption, and the concentration of immigrant flows into Helsinki may disproportionately influence the estimates. As a result, the effect sizes should be

[1] *A TTWA is defined by workplace commute patterns. A municipality is included in a TTWA if at least 10% of its labor travels daily into the central municipality of that area (Statistics Finland, 2018). See section 3 for further details.*

[2] *Constant 2018 Euros*

interpreted with caution. Nonetheless, the overarching pattern of negative effects on low-earning natives and positive effects on high-earning natives remains consistent across various specifications and robustness checks.

Given that I broadly follow the method of Dustmann et al. (2013), it is also the most immediate point of comparison for my results. They estimate earnings percentile specific wage effects of immigration in the UK and find negative and statistically significant effects on natives below the 20th earnings percentile and positive and significant effects on those above the 40th percentile. Although our specific results are rather different, likely speaking to labor market differences between Finland and the UK, both indicate that the effects of immigration vary greatly across the native income distribution. The best point of comparison in terms of setting is provided by Kuosmanen and Meriläinen (2022) who study how immigration that followed the eastern enlargement of the EU in 2004 affected Finnish construction workers. They find that the increased number of posted workers to Finland resulted in a 9% decrease in the annual earnings of more exposed natives relative to less exposed ones. This result is in part driven by younger and therefore less experienced workers, who the authors conjecture are better substitutes for immigrant workers. My findings are somewhat compatible with this observation, as in my dataset, sub 30-year-old construction workers are on average in the 41st earnings percentile, for which I find negative but not statistically significant estimates. My findings are also compatible with the work of Edo and Özgüzel (2023) who use the 1990 distribution of immigrants as the basis of a shift-share instrument in studying the effects of immigration on native employment in a cross-European sample. They find that immigration has adverse short-run effects on employment that dissipate over time, and that low educated natives are more likely to experience negative employment effects while high educated natives can benefit. As those with low education are more likely to be in the lower earnings percentiles, my findings are in line with these observations.

However, several recent works find immigration to have zero or even positive effects on natives. The most broad of such studies is by Caiumi and Peri (2024), who use a shift-share instrument to study the effects of immigration across national level skill-cells in the US in the period 2000-2019. They find that, owing to the relatively large in-flow of college educated labor and complementarities between native and immigrant workers, immigration led to mild positive wage effects for low educated natives and generally positive employment effects. Closely related, Peri et al. (2015) study variation in the American H-1B visa policy and find that such immigration increased wages for all natives, with the greatest gains made by skilled natives, implying that highly educated immigrants working in STEM occupations increased total productivity in the US. Monras (2020) studies the wave of low-skilled immigration to the US following the Mexican peso crisis in 1995 and finds negative short-run effects on natives that eventually dissipate over space and time. Similar studies have also been conducted with European data. Mitaritonna et al. (2017) use a shift-share instrument to study the effects of immigration on firm performance in France and find that immigration increased firm productivity and had positive effects on natives' wages within firms that hired many immigrants. The authors note that immigrants to France are relatively high-skilled. Signorelli (2024) also study the French context, focusing on a 2008 reform that facilitated hiring of non-European workers into certain (high-skill) occupations deemed to have tight labor markets. The resultant increase in immigrant workers, which crucially did not allow for occupational downgrading, had no discernible effect on native employment and slight negative effects on natives' wages, with the brunt of wage effects borne by earlier immigrants. Beerli et al. (2021) study a policy change that increased the number of cross border workers in Switzerland in the 2000s and find that immigration increased firm size and productivity, and boosted earnings of highly educated natives while leaving average employment and wages untouched. They conjecture that these positive effects were a result of the relatively high-skill composition of immigrant workers, the readiness of firms to adopt immigrant labor, and the policy change occurring at a time when demand for skilled labor was. Finally, Foged and Peri (2016) study the impacts of low-skilled immigrant labor, in the form of refugees, on low-skilled Danish workers and find positive effects on wages and employment as the increased competition pushed natives to less manual-intensive occupations.

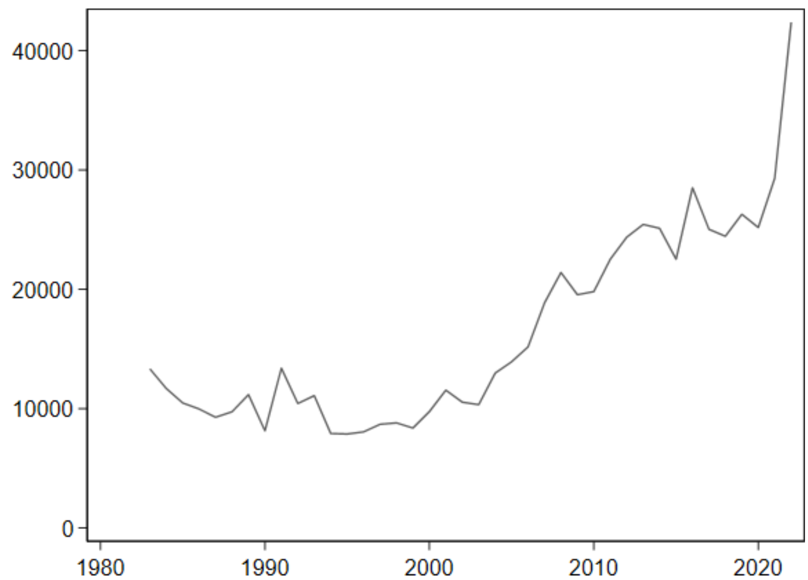
Given this heterogeneity in estimated effects across research settings it appears to be important from a policy perspective to empirically evaluate immigration separately in each context. To this end, my contribution is to estimate the Finland specific impact of immigration on natives. Furthermore, by studying total immigrant flows and the entire labor market with an approach that mirrors Dustmann et al. (2013), rather than specific inflows caused by a policy change, I provide as broad and natural of a perspective on the matter as possible.

The next section provides background information on immigration to Finland. It is followed by a description of my data and method in sections 3 and 4, which enable the subsequent discussion of results in section 5. Section 6 presents robustness checks and section 7 concludes.

2 Immigration to Finland

For most of its history Finland was a country of net emigration, and until the mid 2000s immigration to Finland was rather small in volume (Martikainen et al., 2013). Since then immigrant flows have increased greatly, as can be seen in Figure 1, and in 2022 immigrants made up roughly 9.1% of the population (Statistics Finland, 2023b).

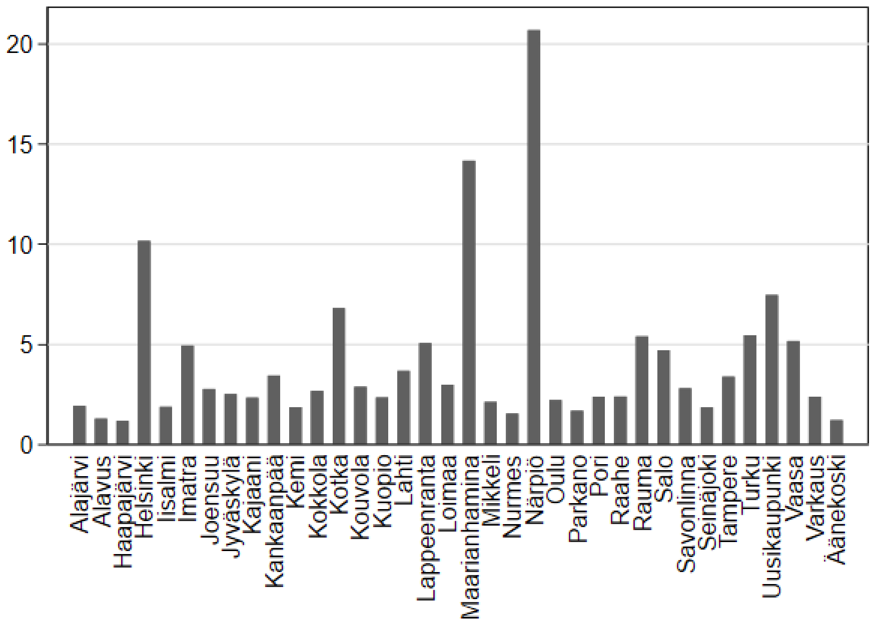
Figure 1: *Annual flow of foreign-born immigrants to Finland*



However, this growth has not been experienced equally across the country and the proportion of immigrants in local populations varies greatly across Finnish regions. This is depicted in Figure 2 which shows how the number of immigrants per 100 natives in the labor force of each TTWA changed between 2001 and 2018. For example, during this period the number of immigrants per 100 natives in the Helsinki TTWA increased by about 10 while the equivalent figure for the Äänekoski TTWA is less than 2. I employ this spatial variation to identify the effects of immigration on natives.

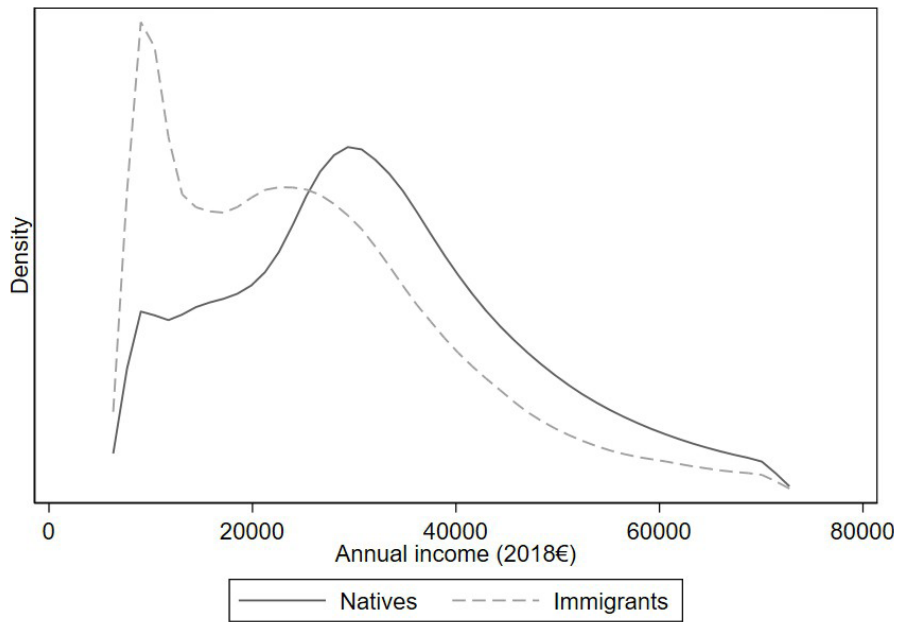
The most common countries of origin of immigrants to Finland are Sweden, Russia, and Estonia although recently inflows from Somalia, Iraq and India have increased significantly (Martikainen et al., 2013; Statistics Finland, 2023a). Compared to these countries, excluding Sweden, Finland has relatively low income inequality (WID, 2018). Hence, immigrants to Finland are likely to be negatively selected (Borjas, 1987). This prediction is supported empirically as the earnings distribution of immigrants appears to be systematically to the left of the natives', as shown in Figure 3. Research on educational attainment of the immigrant population indicates that the proportion with high education is similar among Finnish natives and immigrants (Sutela & Larja, 2015), but this does not necessarily imply that immigrants are represented equally in high-skilled professions. Indeed, some of the concentration of immigrants into low-skill occupations may be due to barriers to entry such as language, rather than strictly skill (Forsander, 2013), resulting in immigrants downgrading their occupation relative to their ability (Dustmann et al., 2013). Nevertheless, it appears that low-earning Finnish natives are likely to face more competition from immigrant labor, while higher earning natives are predicted to be more complementary to immigration. My results test these predictions.

Figure 2: *TTWA change in immigrants per 100 natives 2001-2018*



The figure represents the TTWA specific change from 2001 to 2018 in the number of immigrants per 100 natives in the local labor force. The X-axis names refer to the central municipality of each TTWA. Source: author's calculations.

Figure 3: *Annual earned income distributions of natives and immigrants*



The figure represents the earned income distributions of natives and immigrants in the labor force between 2001 and 2018. The unit of measurement is constant 2018 euros. Source: author's calculations.

3 Data

I construct my dataset using the FOLK Basic, Income, Employment, and Migration modules collected and maintained by Statistics Finland (Statistics Finland, 2024a, 2024b, 2024c, 2024d). These modules contain information from a plethora of registers and datasets maintained by various government bodies. The most relevant sources for this study are the Digital and Population Data Services Agency's Population Information System (Väestö tietojärjestelmä), the National Tax Authority's Incomes register (Tulorekisteri), the Ministry of Economic Affairs and Employment's register of job seekers (Työnhakijarekisteri), and Statistics Finland's degrees register (Tutkintorekisteri). I use data from 1983 to 2018 but focus my analysis between the years of 2001 and 2018.

I restrict my sample to working age (15-74) individuals who are in the labor force. Specifically, I include those whose primary economic activity is employment or unemployment. Hence, part-time workers are included, but students who work a few months of a year are not. Additionally, the FOLK modules only contain information on individuals that are permanent residents of Finland on the final day of the year (Statistics Finland, 2024a). Seasonal immigrant workers who depart before year's end are therefore not observed.

I aggregate the individual level data by location (municipality or TTWA) and year to enable spatial and temporal comparisons of the effects of immigration. A municipality is included in a TTWA if at least 10% of its labor travels daily into the central municipality of that area (Statistics Finland, 2018). Such areas cover typical workplace commutes and can therefore be more appropriate spatial representations of an individual's labor market than their municipality of residence - it is relatively easy to live in one municipality and work in another, but not so for TTWAs. I use the 2018 definitions of TTWAs as it is the final year of my dataset. Additionally, some municipalities that exist in earlier time periods later merge with others. To adjust to this, any municipality that eventually merges is treated as having always been merged. Finally, while the data contains 309 municipalities in total, only 212 are covered by the 36 TTWAs from 2018. A municipality is not part of any TTWA if there is no nearby municipality to which at least 10% of its labor force commutes and if they do not receive many commuters from elsewhere. They tend to be small and rural, and face little immigration. To harmonize analysis between different location definitions, I drop all non-TTWA municipalities from the data set.

In the literature immigrants are typically defined as anyone foreign-born. However, immigrants with Finnish backgrounds are likely to be more substitutable with natives than immigrants with foreign backgrounds, resulting in noise in estimated effects. As such I focus my analysis on foreign-background immigrants, defined as foreign-born individuals whose parents are both also foreign-born. Additionally, the availability of country of birth information from immigrants is crucial for constructing the instrument. Therefore, individuals of unknown origin are excluded from the dataset. Natives are defined as anyone born in Finland.

Table 1: *Means of control variables*

	Natives		Immigrants	
Variable	**2001**	**2018**	**2001**	**2018**
Age	40.8	42.2	37.0	39.6
%Female	49.0	49.7	44.6	44.8
%High education	17.4	32.9		
%Medium education	60.1	57.7		
%Low education	22.4	9.3		

Average age, share of women, and educational attainment shares of working age natives and immigrants in the labor force.

Annual real earned incomes of natives in the labor force form the response variable. Earned income is defined as the sum of salary and entrepreneurial earnings, benefits and capital gains are omitted. I adjust the data to inflation so that they are measured in constant 2018 euros (Statistics Finland, 2024e). From this raw data, the annual earnings placing an individual in the 5th, 10th, 25th, 50th, 75th, 90th and 95th percentiles of native earners in a specific location[3] are calculated for each year. These values represent the annual earnings of the x^{th} percentile native earner in location l and year t, and can include zeros. Most prior literature focuses on the impact of immigration on hourly wages (Dustmann et al., 2016), but due to features of the Finnish collective bargaining based wage-setting system (Asplund, 2007), it is possible that the earnings effects of immigration will be felt in reduced working hours and employment rather than wages. Annual earned income is a product of both wages and working hours, and hence captures the effects of interest better than wage information, especially when unemployed individuals are included in the analysis. It is noteworthy that the mass of these long-term unemployed and part-time workers makes average earnings at each percentile smaller than perhaps expected. They can also contribute larger effect sizes, for immigration may cause a native to become unemployed, pushing their annual earnings to zero.

The key explanatory variable is the ratio of immigrants to natives in the labor force of a location at a certain time. The immigrant-to-native ratio is preferred to simple inflows of immigrants as normalizing by native population size produces results that are more comparable across locations. However, changes in this ratio capture not just immigrant inflows but also potential native out-migration. Fortunately, regional labor mobility in Finland is relatively modest (Poghosyan, 2018) implying that the majority of estimated effects ought to be attributable to immigrant inflows.

The location and time specific control variables are: average age and sex ratio of the native and immigrant labor forces, and the shares of the native population with high, medium, and low educational attainment. High educational attainment is defined as having a bachelor-level degree or above, medium as having completed secondary education and low as anything less. As education information is unreliable for immigrants (Sutela & Larja, 2015), I omit it. These variables are summarized in Table 1.

4 Method

In this section I first present my baseline empirical strategy, and then introduce my immigrant enclave instrument, describe its identifying assumptions, and evaluate its relevance and validity empirically.

4.1 Baseline specification

Following Dustmann et al. (2013) I estimate the effects of immigration on natives' earnings using a first-differenced fixed effects model, presented in Equation 1.

$$\Delta I_{plt} = \alpha_t + \beta_p \Delta R_{lt} + \Delta \mathbf{X}_{lt} + \Delta \varepsilon_{plt} \tag{1}$$

The dependent variable is the change in annual earned income I of the p^{th} percentile native earner in the labor force of a specific location l (municipality or TTWA) at a specific time t. The coefficient of interest β_p captures the percentile specific effect of a change in the ratio of immigrants to natives in the labor force R on annual earned income, averaged over location and time. A vector of control variables X containing information on the age and sex composition of natives and immigrants and the educational attainment of natives is included in the model. Location specific fixed effects are eliminated by estimating the model in first-differences, while the term α consists of yearly dummies that control for the overall time trend. Error is represented by ε. Additionally, locations are weighted by the size of their labor force in all specifications.

[3] *I use the word location when the argument is applicable to both municipalities and TTWAs.*

At its core this approach compares changes in earnings between natives in high and immigration locations that are not explained by developments in the control variables. If immigrants were to be randomly assigned across Finland, any differences in natives' earnings between locations found by this model could be attributed to immigration. However, immigrants' location choices are non-random and indeed are likely to be correlated with local labor market prospects, causing a simultaneity problem. I address this using a shift-share instrument based on the immigrant enclaving tendency, which is presented in detail below.

4.2 The immigrant enclave instrument

The immigrant enclave instrument has a shift-share[4] structure wherein regional variation in immigrant flows is derived from national level variation in the country of origin distribution of incoming migrants. It is founded on the enclaving tendency of immigrants - on balance immigrants tend to prefer locations where a community of migrants from the same country of origin already exists (Bartel, 1989; Munshi, 2020). Hence, pre-existing enclaves can be used to predict the settlement of later migrants by allocating immigrants from a certain country of origin across locations in similar proportions to how they were distributed at some earlier reference date. This is the 'share' component of the instrument. Under this approach, variation in immigrant flows between locations is driven by changes in the country of origin composition of national inflows - the 'shift'. The version of the instrument that I use is defined in Equation 2 (Altonji & Card, 1991; Jaeger et al., 2018).

$$z_{lt} = \sum_{o} \frac{M_{olt^0}}{M_{ot^0}} \frac{\Delta M_{ot}}{N_{lt-1}} \tag{2}$$

The first term in the sum is the share of immigrants from country of origin o in location l at reference date t^0, which predates t. The national inflow of migrants from country o at time t is represented by ΔMot, and N_{lt-1} is the total population of location l in the previous period. In constructing the instrument I use data on total populations of natives and immigrants, not just the labor force, to provide independence from local labor conditions.

Equations 3 and 4 present the two stage least squares (2SLS) procedure I use in estimating the IV models, where γ_t are time fixed effects in the first stage and u_{lt} is the first stage error term.

$$\Delta \hat{R}_{lt} = \gamma_t + z_{lt} + \Delta \mathbf{X}_{lt} + \Delta u_{lt} \tag{3}$$

$$\Delta I_{plt} = \alpha_t + \beta_p \Delta \hat{R}_{lt} + \Delta \mathbf{X}_{lt} + \Delta \varepsilon_{plt} \tag{4}$$

Recent work by Borusyak et al. (2022) and Goldsmith-Pinkham et al. (2020) highlights that in most cases of shift-share instrumentation it is either the regional shares or external shocks that introduce exogeneity into the system, but rarely both. Adapting these ideas to the immigrant enclave instrument, the shocks view implies a setting where the country of origin composition of immigrants changes over time due to many exogenous push factors in the various departure countries. Finnish locations then experience different levels of this newfound immigration in proportion to the size of the enclave there. Importantly, because the shocks are exogenous to Finnish labor markets, the local shares do not need to be (Borusyak et al., 2022). Under the shares view, the story is inverted. Assuming that local shares of immigrants from each country of origin at the reference date are independent of later labor market developments, exogenous regional variation in immigration can be derived from differential exposure to common and possibly endogenous inflows. Indeed each share can be used as a separate instrument. It is noteworthy that local shares need not be exogenous to the levels of native earnings at the time that the enclaves are formed, but only subsequent changes in these (Goldsmith-Pinkham et al., 2020).

While there is some merit to both views in this context, I choose to take the exogenous shares perspective. The shocks view requires a large number of exogenous push factors in immigrant flows from different countries of origin to function.

[4] *Shift-share instruments are also often called Bartik or Bartik-like instruments, after Bartik (1991)*

While some such flows certainly exist, with refugees being a particularly good example, lots of immigration to Finland will certainly be motivated more by pull factors that are less likely to be independent of local labor conditions. Instead, noting that only about 10% of immigrants to Finland in the early 1990s cited work as their primary reason for moving (Martikainen et al., 2013), I argue that the enclaves they formed were largely independent of local labor conditions at the time and are therefore also exogenous to later developments. Thus, the identifying assumption I rely on is that locations with different immigrant stocks in 1993, both in volume and country of origin composition, would have evolved similarly in the estimation period in the absence of immigration. To the extent that enclaves were formed by immigration decisions related more to family than choosing the best labor market, this assumption is plausible. It is also supported by the minimum 8-year gap between the reference date and estimation period, which mitigates effects of serial correlation in local labor demand. Furthermore, if there are divergent trends between high and low-immigration areas, so long as these trends affect natives of all rungs in a similar way, comparison between estimates for different percentiles can still give insight into the distributional consequences of immigration in Finland.

I evaluate this assumption and the strength of my instrument below by calculating Rotemberg weights for the country of origin shares, inspecting pre-trends, discussing slow adjustment of labor markets, and presenting first stage results.

4.2.1 Rotemberg weights

Following the advice of Goldsmith-Pinkham et al. (2020) I calculate Rotemberg weights for the country of origin shares. These weights decompose the identifying variation in shift-share instruments to show how much each country of origin share contributes to the final IV estimate.[5] They are presented in Table 2.

As shown in Panel A, the mean weight of the shares is very small. This is likely a result of there being many countries of origin from which migrant inflows are minor. Indeed the top ten countries account for 58% (0.613/1.059) of the positive weight. Furthermore, Estonia is a clear outlier as it alone accounts for 22% of the explanatory power of the instrument, and possibly more as some immigrants born in the Soviet Union are likely to be Estonians. The importance of Estonian inflows is also reflected in the sum of annual weights increasing significantly after 2004, the year in which Estonia joined the EU. This large weight is likely a direct result of the large flow of Estonian immigrants to Finland during the period of study, and it indicates that the overall estimates are the most sensitive to misspecification in the Estonian shares.

Given Estonia's large weight, part of the identifying variation in the instrument can be interpreted as a comparison between locations with high vs low initial stocks of Estonians. Before the fall of the Soviet Union, migration from Estonia to Finland was heavily restricted and small in volume. Therefore, the enclaves formed by 1993 will largely have been due to return migration of Ingrian Finns, which was allowed after 1990, and marriages between Finns and Estonians (Alho & Kumer-Haukanõmm, 2020). Such immigration is by nature different from the later flows of posted workers, lending some credence to exogeneity.

However, Estonia's proximity and EU accession in 2004 led to large labor-motivated inflows, especially into Helsinki. Since Helsinki already hosted a significant Estonian enclave, this raises the risk that enclave size is correlated with later economic trends. In response, Section 6.1 presents robustness checks that omit both Helsinki and the smallest TTWA to assess the sensitivity of results to this concern.

[5] *I calculate the weights at the TTWA level, as it is the primary focus of my analysis. The municipal level versions, included in section A of the online appendix, produce broadly the same conclusions.*

Table 2: *Rotemberg weights*

Panel A: Negative and positive weights	**Sum**	**Mean**	**Share**
Negative	-0.059	-0.003	0.053
Positive	1.059	0.008	0.947
Panel B: Variance in α over years	**Sum**	**Mean**	
2002	0.017	0.000	
2003	0.005	0.000	
2004	0.009	0.000	
2005	0.024	0.000	
2006	0.043	0.000	
2007	0.065	0.000	
2008	0.068	0.000	
2009	0.045	0.000	
2010	0.048	0.000	
2011	0.084	0.001	
2012	0.084	0.001	
2013	0.104	0.001	
2014	0.095	0.001	
2015	0.088	0.001	
2016	0.079	0.001	
2017	0.067	0.000	
2018	0.075	0.000	
Panel C: Top ten countries	α	$\hat{\beta}$	g
Estonia	0.238	7725	3357
USSR	0.070	11334	2028
China	0.053	12152	766
Afghanistan	0.050	10976	684
Somalia	0.048	12653	617
India	0.037	15259	629
Thailand	0.036	9458	599
Russia	0.029	15642	876
USA	0.028	14548	383
Sweden	0.024	-6369	754

The table presents Rotemberg weights of the country of origin shares from the TTWA level IV model with mean earnings of natives as the dependent variable. The α represents the country specific Rotemberg weight, $\hat{\beta}$ the estimated coefficient when only shares from that country of origin are used as an instrument, and g the total shock from a specific country.

4.2.2 Pre-trends

By using the immigrant enclave instrument from an exogenous shares perspective, the identifying assumption underpinning my empirical strategy is that locations with different immigrant stocks in 1993 would have evolved similarly in the absence of immigration. One way of evaluating this is to check the data for pre-trends and examine whether locations with different shares develop differently in the pre-period between 1993 and 2000. To this end, I provide an empirical evaluation of pre-trends by regressing changes in mean, 90th, and 10th percentile native earnings in three year groups against the 1993 total share of immigrants in a TTWA[6] (Goldsmith-Pinkham et al., 2020). Estimated coefficients from these regressions are plotted in figure 4. If the identifying assumption holds, there should be no systematic relationship between baseline immigrant shares and pre-period earnings growth. Hence, estimates from the regressions studying 1994-1996 and 1997-1999 are the most relevant for analyzing pretrends. I also investigate the existence of pre-trends after dropping the Helsinki and Parkano TTWAs (largest and smallest) from the analysis.

The results of the test raise concern. In panel a of Figure 4, there is evidence of a positive pre-trend - before the estimation period natives in TTWAs with higher shares of immigrants in 1993 seem to have experienced faster earnings growth than those in low-immigration TTWAs. This pattern is most pronounced at the 90th earnings percentile but appears across other percentiles as well. Such differences challenge the common trends assumption and suggest that part of the estimated effects may reflect pre-existing growth differentials rather than causal impacts of immigration. As a result, the estimates are likely biased upward and should be interpreted with caution.

Panel b of the figure shows that trimming Helsinki and Parkano from the sample eliminates the statistical significance of these pre-trends, though largely due to reduced precision. Given that the regressions are weighted by the labor force size, this change is attributable to the removal of Helsinki. Thus, it is possible that the pre-trends in panel a are mostly due to Helsinki trending differently than other locations in the pre-estimation period and that removing Helsinki may reduce bias. However, trimming Helsinki makes the relationship between immigration share and earnings growth noisier. This may reflect greater heterogeneity or weaker earnings trends in smaller TTWAs. While excluding Helsinki improves the credibility of the identifying assumption, it does not fully eliminate the possibility of underlying growth differentials. This reinforces the importance of conducting robustness checks with and without Helsinki, as is done in Section 6.2.

4.2.3 Slow adjustment

The validity of this shift-share instrument is jeopardized by slow adjustment of labor markets to shocks. If the shock that past immigrants caused on a local economy is still being absorbed today, then the short-term effects of current migrants can be confounded by long-term effects of past ones. This is particularly true if the country of origin composition of migrants is relatively stable over time (Jaeger et al., 2018). As short and long-term effects likely have opposite directions, this causes point estimates to be biased toward zero. I investigate the relevance of this issue in my setting by following the multiple-instrumentation procedure designed by Jaeger et al. (2018) to correct for slow dynamic adjustment. In this technique, the basic model is modified to include lagged values of the immigrant-to-native-ratio intended to capture long-term effects of migration. Each of these lagged terms is instrumented for using equivalent lags of the instrument to address the same endogeneity issues that plague present immigrant flows. Thus the system contains two endogenous regressors and two instruments. The lagged terms should thus control for any ongoing adjustment to older shocks, isolating short-run effects to the contemporary term. Specifically, I use the tenth lag of the immigrant-to-native ratio as a control for long-run adjustment, the same gap as employed by Jaeger et al. (2018). The two first stages and the reduced form are represented in Equations 5, 6 and 7, where η_p is the percentile specific long-term effect of immigration on earnings.

[6] *Equivalent estimates at the municipal level are in section B of the online appendix.*

$$\Delta\hat{R}_{lt} = \gamma_t + z_{lt} + z_{lt-10} + \Delta\mathbf{X}_{lt} + \Delta u_{lt} \tag{5}$$

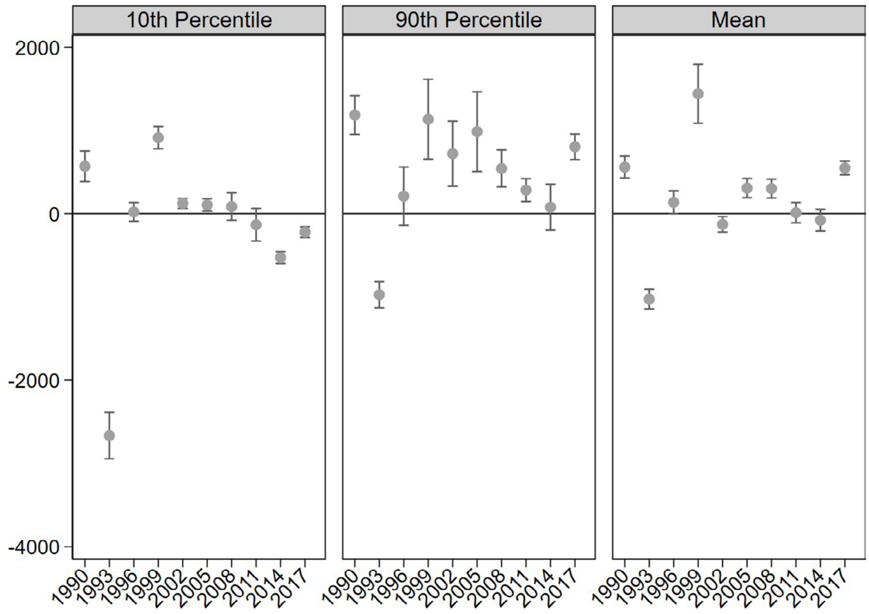
$$\Delta\hat{R}_{lt-10} = \gamma_t + z_{lt} + z_{lt-10} + \Delta\mathbf{X}_{lt} + \Delta u_{lt} \tag{6}$$

$$\Delta I_{plt} = \alpha_t + \beta_p\Delta\hat{R}_{lt} + \eta_p\Delta\hat{R}_{lt-10} + \Delta\mathbf{X}_{plt} + \Delta\varepsilon_{plt} \tag{7}$$

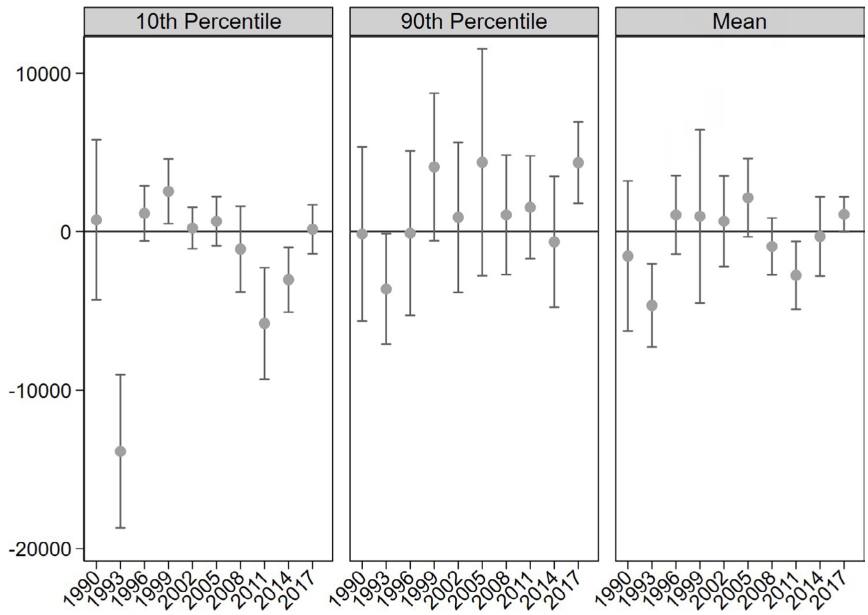
The results from this procedure are presented in section 5.2.

Figure 4: *Pre-trend regression coefficients*

(a) Full sample



(b) Helsinki and Parkano omitted



The figure presents estimated coefficients from a regression of annual change in the earnings of the mean, 90th, and 10th percentile natives against the aggregate share of immigration in a TTWA in 1993. The regressions are done in three-year groups, with the year labels corresponding to the final year of the group. Controls are fixed to their differenced 1993 values and interacted with a year dummy. Year fixed effects are included. TTWAs are weighted by the size of their labor force. Standard errors are clustered by TTWA.

4.2.4 First stages

The first stage specification is presented in Equation 8 where ΔR_{lt-i} is the i^{th} lag ($i = 0, 10$) of the change in the immigrant-to-native ratio in location l at time t, z is the instrument, γ captures time fixed effects and X is the vector of controls described earlier.

$$\Delta R_{lt-i} = \gamma_{t-i} + z_{lt} + z_{lt-10} + \Delta \mathbf{X}_{lt-i} + \Delta u_{lt-i} \tag{8}$$

I examine the strength of my instrument using Kleibergen-Paap Wald rk F-statistics[7], which are robust to heteroskedasticity, autocorrelation, and clustering, and the Kleibergen-Paap rk LM test of underidentification (Kleibergen & Paap, 2006). Test statistics from these tests are presented in Table 3 alongside first stage regression coefficients.

Weak instruments cause three issues in 2SLS estimation: the estimates are biased toward ordinary least squares (OLS), the t-test suffers from size inflation leading to overrejection, and the t-test exhibits power asymmetry wherein standard errors are artificially small when 2SLS estimates are near OLS estimates, and artificially large when they are far (Keane & Neal, 2024). Generally, the first two issues are negated when (robust) F-statistics are greater than critical values outlined by Stock and Yogo (2005). For my single-IV models which have only one endogenous regressor the relevant critical value is 16.38, for the multi-IV models with two endogenous regressors the critical value is 7.03. However, power asymmetry requires much stronger instruments with F statistics over 50 to be overcome (Keane & Neal, 2024).

Table 3: *First stage estimates*

	Municipal			Travel-to-work area		
	R	***R***	**L10.*R***	***R***	***R***	**L10.*R***
Instrument	0.609	0.215	-0.161	1.012	0.589	-0.164
	(0.129)	(0.174)	(0.095)	(0.038)	(0.157)	(0.095)
L10.Instrument		0.520	0.948		0.625	1.115
		(0.472)	(0.227)		(0.184)	(0.138)
N	3619	1489		612	252	
Robust F	22	13		724	10	
KP $\chi 2$	8.804	3.367		4.172	3.004	
	[0.003]	[0.067]		[0.041]	[0.083]	

The table presents first stage estimation results. R refers to the ratio of immigrants to natives in the labor force while $L10$ indicates the 10^{th} lag of the variable. The model is estimated in first differences, locations are weighted by the size of their labor force, and controls and time fixed effects are included in each specification. Location clustered standard errors in parentheses. The robust Fs are Kleibergen-Paap Wald rk F-statistics, while the $KP\ \chi^2$ are from a Kleibergen-Paap rk LM test for underidentification, with corresponding p-values in square brackets (Kleibergen & Paap, 2006).

Against this backdrop, my instrument appears sufficiently strong in the TTWA single-IV specification. For the municipal single-IV it is strong enough to avoid issues of size inflation, but the risk of power asymmetry remains. Thus, it will be important to compare the municipal 2SLS estimates to their OLS counterparts in order to draw conclusions about to which direction the standard errors may be biased. For the multiple-IV specifications the instruments exceed the critical values for avoiding size inflation but fall short of the cutoff for power asymmetry. Furthermore, the Kleibergen-Paap LM test does not reject underidentification at the 5% significance level, although it is rather close. Thus, the TTWA single-IV

[7] *In cases with one endogenous regressor and one excluded instrument, this statistic corresponds to the effective F of Olea and Pflueger (2013), which is generally the preferred statistic for evaluating instrument strength. However, the effective F cannot be calculated in cases with multiple endogenous regressors (Andrews et al., 2019). Thus, I choose to use the Kleibergen-Paap Wald F-statistics throughout.*

model is the most robust to weak instrument concerns. The municipal and multiple-IV specifications remain informative but require caution in interpreting standard errors and statistical significance.

I use 1993 as the reference year in the instrument, but the results and instrument strength are not sensitive to this choice, as can be seen in section D of the online appendix.

5 Results

In this section I present and discuss my empirical findings. The core empirical results from the single-IV procedure are considered in the first subsection. The second subsection focuses on multiple-IV and the slow dynamic adjustment critique.

5.1 OLS and IV results

Table 4 summarizes my OLS and IV regression results. To make interpretation more intuitive I scale the point estimates and standard errors to represent the change in earnings caused by a location with the 10th percentile immigrant-to-native ratio in 2018 experiencing an exogenous inflow that brings them to the 90th percentile.[8] For municipalities this translates to moving from 1.3 immigrants per 100 natives to 9.8, for TTWAs these figures are 1.7 to 8.1. For example, the scaled TTWA IV point estimate for the 10th native earnings percentile is -1661. This implies that, all else held constant, if a TTWA with 1.7 immigrants per 100 natives faced immigration bringing this number to 8.1, the expected annual earnings of the 10th percentile native would be 1661€ smaller than without immigration. This same scaling is used in all remaining results, with the scaling factor calculated separately for each sample. The TTWA sample mean earnings for each percentile are also included in Table 4 to enable comparisons of relative effect strengths.

It is useful to start by comparing the OLS and IV estimates as this helps contextualize the chosen method. The OLS estimates are generally closer to zero than their IV counterparts. This indicates the positive simultaneity bias caused by immigrants preferring locations with favorable labor market prospects. The existence of this bias leads me to focus primarily on the IV results in subsequent analysis.

Given that TTWAs more accurately reflect functional labor markets than municipalities and due to the stronger first stages, I focus primarily on the TTWA-level estimates. Municipal level results serve as a point of reference where appropriate.

With these observations in mind, the estimates can now be used to evaluate how immigration impacts native workers at different points in the income distribution. The TTWA IV estimates for natives in the 5th, 10th, and 25th percentiles of the native earnings distribution are negative and statistically significant. For median earners the estimates are near zero and not statistically significant. Above this the estimates are positive and statistically significant. The magnitude of the coefficients tends to increase the further one moves from the median, a pattern that holds even when normalized by mean earnings. The exception to this trend is the difference between estimated coefficients at the 5th and 10th percentiles, as the former is smaller in absolute value than the latter.

[8] *I scale estimates using percentile differences rather than standard deviations due to the skewed distribution of the immigrant-to-native ratio across regions (see Figure 3). In such settings, standard deviation-based scaling can overstate the extent of typical variation due to sensitivity to extreme values. Percentile-based scaling, by contrast, is more robust to skewness and provides a clearer interpretation of effect sizes across the central range of the distribution. Additionally, comparing the 10th and 90th percentiles offers a more intuitive contrast between regions with low and high immigrant shares, which may be less transparent when expressed in terms of standard deviation units.*

Table 4: *The effect of immigration on natives' earnings*

		Municipality		Travel-to-work-area	
	Mean earnings	**OLS**	**IV**	**OLS**	**IV**
Mean	35286	165 (265)	-182 (365)	429 (190)	645 (225)
5th percentile	7702	-497 (102)	-865 (169)	-450 (133)	-592 (126)
10th percentile	11072	-1113 (135)	-3039 (472)	-1116 (228)	-1661 (252)
25th percentile	21192	-970 (176)	-2748 (426)	-972 (248)	-1423 (329)
50th percentile	31748	-13 (171)	-713 (285)	201 (106)	194 (163)
75th percentile	43435	608 (306)	282 (370)	935 (222)	1301 (223)
90th percentile	59473	1074 (407)	1274 (490)	1543 (350)	2398 (354)
95th percentile	73214	1727 (773)	2770 (620)	1899 (577)	3279 (396)
Year dummies		YES	YES	YES	YES
Controls		YES	YES	YES	YES
N		3619	3619	612	612
Robust F		-	22	-	724
KP χ^2		-	8.804	-	4.172
			[0.003]		[0.041]

The models are estimated in first differences, with locations weighted by the size of their labor force. The control variables are the average age and sex ratio of the native and immigrant populations in the labor force, as well as the educational attainment of the native labor force, and year dummies. Location clustered standard errors are in parentheses. The robust Fs are Kleibergen-Paap Wald rk F-statistics, while the KP χ^2 are from a Kleibergen-Paap rk LM test for underidentification, with corresponding p-values in square brackets (Kleibergen & Paap, 2006). The estimates are scaled to reflect the effect of the immigrant-to-native ratio increasing from the 10th to the 90th percentile. The scaling factors are 0.085 for municipal level estimates and 0.063 TTWA estimates.

Two mechanisms may explain this irregularity. First, workers at the bottom of the earnings distribution may be more likely to exit the labor force in response to increased competition from immigration, resulting in their outcomes being underrepresented in annual earnings data. Second, some empirical evidence exists of immigration pushing low-skilled native workers from manual labor to more communication based occupations as linguistic and cultural differences imply that native workers have a comparative advantage in the latter (Foged & Peri, 2016; Peri & Sparber, 2009). Natives who change occupations like this can increase their earnings, resulting in average estimated effects of immigration being closer to zero. It is possible that natives in the 5th percentile undergo such profession upgrades more readily than those in the 10th.

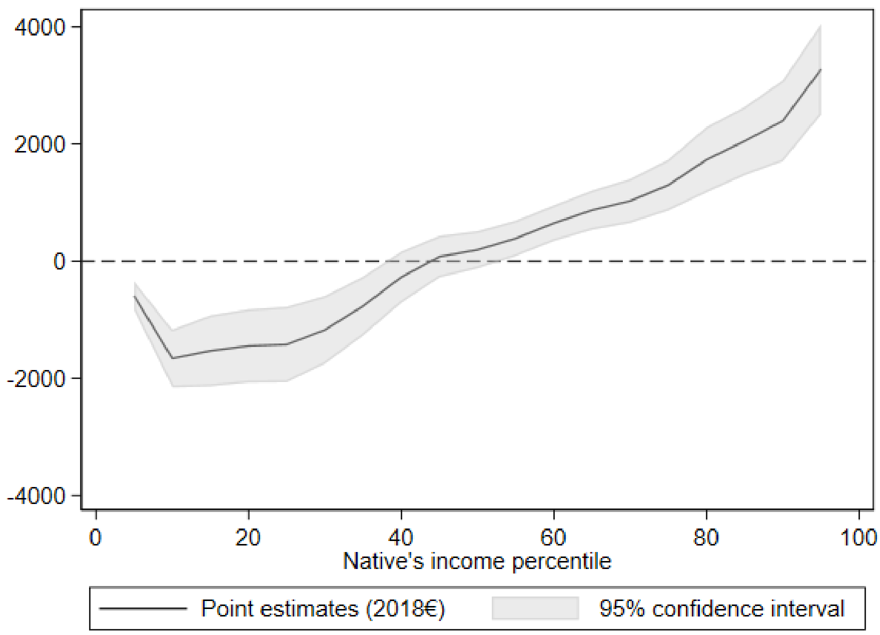
Similar patterns are evident at the municipal level, although all of the estimates are smaller than at the TTWA level. As a result, the point where immigration's effects shift from negative to positive occurs higher in the distribution at around the 75th percentile. Regarding power asymmetry caused by the weaker instrument, the IV estimates at the municipal level are generally rather far from their OLS counterparts, suggesting that standard errors are unlikely to be artificially small.

Overall, the results imply that foreign-background immigrants to Finland are closer substitutes for low-earning natives and more complementary to high-earning ones. An influx of migrants therefore represents increased competition in the local labor market for low-earning natives, which places downward pressure on their average earnings relative to unaffected locations. These effects do not necessarily reflect absolute income losses, but rather slower earnings growth relative to a counterfactual without immigration. Importantly, I cannot say how exactly these negative effects manifest, whether as slower wage growth or reduced working hours. Simultaneously, high-earning natives appear to benefit from these same inflows as demand for their skills increases as a result of complementarities between low-skill immigrants and high-skill natives. These observations suggest that immigration has contributed to widening earnings disparities among natives, with high-income Finns benefiting the most. Further, this implies that high-immigration areas are likely to become more unequal than low-immigration areas.

These key takeaways are summarized in Figure 5, which plots scaled TTWA IV point estimates for every fifth earnings percentile. The Figure depicts the trend suggested in the regression tables - immigration is estimated to have adverse effects on below 40th percentile native earners, and positive effects on above 55th percentile earners.

As noted in Section 4.2.2, there is some evidence of positive pre-trends in native earnings. This suggests that the IV estimates may be biased upward, potentially overstating the beneficial effects of immigration for high-earners. As such, the curve in Figure 5 may be shifted above its true position. Nonetheless, the overall trend of negative effects for low-earners and positive effects for high-earners remains consistent and is unlikely to be a spurious result of pre-trends alone.

Figure 5: *Scaled travel-to-work-area IV point estimates*



The figure presents earnings percentile specific IV estimates of the expected change in a native's annual earnings caused by the number of immigrants per 100 natives in their TTWA's labor force increasing from 1.7 to 8.1. The estimates are calculated for every fifth native earnings percentile starting at the 5th percentile.

5.2 Multiple-IV results

Overall, the results of the single-IV estimation indicate that immigration has a negative impact on native earners below the 40th earnings percentile and positive effects on earners above the 55th percentile. In this section, I evaluate this finding against estimates from the multiple-IV specification, which are presented in Table 5, to help understand whether slow adjustment to past immigration is biasing the baseline results in the way envisioned by Jaeger et al. (2018).

At both the municipal and TTWA levels, the multiple-IV coefficients for the current term are mostly smaller than their single-IV counterparts, while the coefficients for the lagged term are often positive. For example, the scaled TTWA level current and lagged estimates for the 10^{th} percentile are -4860 and 4082 respectively, while the single IV estimate is -1661. This implies that the short term effects of immigration on natives of that percentile may be more severe than captured by the baseline model, but that some of this adverse impact is later compensated for by positive long-run adjustment.

The estimates are, however, rather noisy with large standard errors and a general lack of statistical significance. This makes it difficult to draw overall conclusions about the potential biasing effects of slow adjustment from the results, but taken together it seems that the single-IV estimates are more likely to be biased upward than down.

6 Robustness checks

The key observations drawn from the baseline results are that immigration appears to have negative effects on low-earning natives and positive effects on high-earning natives. Additionally, the intensity of these effects typically increases further away from the median.

This section evaluates whether these observations are robust to various changes in the sample population. More specifically, I reproduce the TTWA level single-IV results using data constructed with one of the five following deviations from the baseline sample and examine the impact on the estimates:

1. The largest and smallest TTWAs by population are removed from the sample

2. Working age changed from 15-74 to 20-59

3. Only men are included in the sample

4. Only women are included in the sample

5. Only salaried workers (not self-employed) are included in the sample

The new population definitions are applied to both natives and immigrants.

The results of these robustness checks are presented in Table 6. I focus solely on the TTWA level results, but equivalent checks at the municipal level can be found in section C of the online appendix.[9]

[9] *Broadly, the municipality-level robustness checks reveal that point estimates are less stable than those at the TTWA level, further supporting the choice to focus on the latter. Nevertheless, all specifications consistently show a pattern of negative effects for low-earning natives and positive effects for high-earners, reinforcing the central finding.*

Table 5: *Multiple-IV results*

	Municipality			Travel-to-work-area		
	IV	Multi-IV		IV	Multi-IV	
	R	*R*	L10.*R*	*R*	*R*	L10.*R*
Mean	-182	-2702	4185	645	616	288
	(365)	(1145)	(1219)	(225)	(724)	(1045)
5th percentile	-865	2476	-3836	-592	824	-3100
	(169)	(1297)	(1376)	(126)	(728)	(1233)
10th percentile	-3039	-1018	-3626	-1661	-4860	4082
	(472)	(1739)	(2383)	(252)	(883)	(1282)
25th percentile	-2748	-4253	1931	-1423	-468	-1400
	(426)	(1430)	(2073)	(329)	(1022)	(1649)
50th percentile	-713	-4131	4301	194	-696	975
	(285)	(1278)	(1650)	(163)	(693)	(1080)
75th percentile	282	-5857	8299	1301	-639	2769
	(370)	(1719)	(1956)	(223)	(894)	(1418)
90th percentile	1274	-6680	11421	2398	1029	2772
	(490)	(2627)	(2867)	(354)	(1660)	(2710)
95th percentile	2770	-13107	21085	3279	-3609	12286
	(620)	(4118)	(4578)	(396)	(2461)	(4315)
N	3619	1489		612	252	
Robust *F*	22	13		724	10	
KP χ^2	8.804	3.367		4.172	3.004	
	[0.003]	[0.067]		[0.041]	[0.083]	

The table presents the estimated coefficients for the ratio of immigrants to natives (R) and its tenth lag (L10.R). The multiple-IV estimates for the current and lagged term are estimated simultaneously. The models are estimated in first differences, with locations weighted by the size of their labor force. The control variables are the average age and sex ratio of the native and immigrant populations in the labor force, as well as the educational attainment of the native labor force, as well as year dummies. Location clustered standard errors in parentheses. The robust Fs are Kleibergen-Paap Wald rk F-statistics and they represent the joint explanatory power of both the current and past instruments. KP χ^2 statistics are from a Kleibergen-Paap rk LM test for underidentification, with corresponding p-values in square brackets. The estimates are scaled to reflect the effect of the immigrant-to-native ratio increasing from the 10th to the 90th percentile. The scaling factors is 0.085 for municipal level estimates and 0.063 TTWA estimates.

Table 6: *TTWA robustness checks*

	Baseline	Trimmed	Age	Male	Female	Salaried
Mean	645	-1565	672	818	617	974
	(225)	(839)	(246)	(311)	(226)	(235)
5th percentile	-592	-1170	-1157	-451	-987	-940
	(126)	(358)	(173)	(129)	(126)	(136)
10th percentile	-1661	-3749	-1551	-1442	-2439	-1600
	(252)	(1016)	(259)	(238)	(336)	(310)
25th percentile	-1423	-4095	-1381	-1212	-1997	-1260
	(329)	(1424)	(332)	(346)	(417)	(350)
50th percentile	194	-1652	206	462	246	390
	(163)	(840)	(152)	(168)	(221)	(169)
75th percentile	1301	-692	1275	1691	1954	1618
	(223)	(773)	(214)	(225)	(285)	(238)
90th percentile	2398	1883	2412	2720	3840	3052
	(354)	(850)	(299)	(287)	(525)	(334)
95th percentile	3279	2575	3370	3526	5049	4035
	(396)	(1289)	(476)	(466)	(623)	(413)
Year dummies	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
N	612	578	612	612	612	612
Robust F	724	20	770	709	619	655
KP χ^2	4.172	4.316	4.312	4.024	3.455	3.957
	[0.041]	[0.038]	[0.038]	[0.045]	[0.063]	[0.047]

The name at the top identifies what aspect of the population has been changed to test robustness. 'Baseline' refers to the single-IV results presented earlier. Under the 'Trimmed' model the baseline is re-estimated after dropping the largest and smallest TTWAs (Helsinki and Parkano, respectively). In the 'Age' model the age range of the population is decreased from 15-74 to 20-59. Columns 'Male' and 'Female' consider men and women separately. Model 'Salaried' excludes self-employed workers. Other specification details are the same, except the controls for sex ratio are omitted in the 'Male' and 'Female' models. The estimates are scaled to reflect the effect of the share of immigrants to natives in the labor force going from the 10th to the 90th percentile of each sample population. The scaling factors are, in order: 0.063, 0.060, 0.068, 0.082, 0.064, 0.065.

6.1 Removal of Helsinki and Parkano

Arguably the most important robustness check is the removal of the largest and smallest TTWAs (Helsinki and Parkano). As mentioned earlier, Helsinki stands out due to its large population share, its disproportionately high share of immigrants, and evidence of stronger earnings pre-trends. Further, since TTWAs are weighted by labor force size in the main regressions, Helsinki exerts significant leverage on the estimates. Therefore it is crucial to evaluate whether the key findings depend on its inclusion. While Parkano is also removed for symmetry, due to weighting by the size of the labor force, most of the changes in the results are attributable to Helsinki's exclusion.

There are three major differences between the baseline and trimmed results. First, the instrument is significantly weaker after trimming, as indicated by the F statistic. However, it remains above the Stock and Yogo (2005) critical value of 16.38, suggesting that size distortion in inference is unlikely, though the risk of power asymmetry remains. The decline in instrument strength likely stems from Finland's concentrated immigration pattern. Helsinki accounts for a large share of both historical and recent inflows boosting the correlation between the instrument and immigration. Once Helsinki is excluded, this channel weakens. That said, the instrument may become more exogenous to the extent immigrants settling outside the capital region are more likely driven by enclave dynamics than by local labor market conditions.

Second, standard errors increase substantially after removing Helsinki. This may partly reflect greater heterogeneity in immigration effects outside the capital, due to variation in the skill composition of native and immigrant populations across TTWAs. These differences can increase sampling variability in estimated effects. When Helsinki is included, its weight dominates the average, dampening such heterogeneity. Additionally, the large errors may also be a consequence of the weak instrument and related power asymmetry.

Third, the point estimates are smaller across the board, and they turn positive only at the 90th percentile. This suggests that immigration's effects on natives were more positive in Helsinki than elsewhere. This aligns with the stronger positive pre-trends observed when Helsinki is included. If those trends reflect underlying upward momentum unrelated to immigration, their inclusion would shift estimates upward. Removing Helsinki may therefore reduce this source of bias.

However, despite the changes in magnitude and precision, the core pattern remains: immigration has negative effects on earnings at the lower end of the native earnings distribution and positive effects at the upper end. While Helsinki meaningfully influences the size of the estimated effects, this overall trend does not hinge on its inclusion. This lends credibility to my central findings, particularly in light of the improved pre-trend balance and reduced concentration of immigrant inflows in the trimmed sample.

6.2 Varying the sample population

The population-characteristic related robustness checks primarily serve to ensure that the key findings are not specific to the types of individuals included in the baseline population. The interpretation of the results of these checks is more straightforward than the trimming of Helsinki and Parkano, as the core observations generally survive all of them. In all specifications the point estimates at the TTWA level are negative for low-earning natives and then rise with earnings, again with the exception of estimated effects for the 5th percentile, becoming positive at around the median. Somewhat curiously, the estimates between the male and female regressions are quite different, with the effects of immigration seemingly more intense for women at both ends of the income distribution. Nevertheless, it seems that the results of the baseline model are broadly robust to reasonable deviations in the sample population.

Thus, the core result of immigration having negative effects on low-earning natives and positive effects on high-earning natives, with the intensity of these effects increasing further away from the median broadly survives all of the robustness checks. Though omitting Helsinki and Parkano greatly increases the noisiness of the estimates it does not remove the overarching pattern. The other checks indicate that the main findings are not driven by specific population definitions.

7 Conclusion

I investigate the effects of foreign-background immigration on the earnings of native Finns. Using individual level register data from 2001 to 2018 I estimate a fixed-effects model to compare changes in natives' earnings across municipalities and travel-to-work areas that experience varying flows of immigrants over time. I employ a shift-share instrument to enable causal identification.

Point estimates for the earnings effects of immigration from my preferred specification are negative and statistically significant for natives below the 40th earnings percentile, near zero for those between the 40th and 55th percentiles, and positive and statistically significant for those in and above the 60th percentile. The estimated impacts generally intensify further away from the median and, as indicated by the multiple-IV results, may be even stronger in the short-term for low-earning natives.

These findings indicate that the skill-distribution of foreign-background immigrants to Finland is more complementary to high-skilled natives and more substitutable with that of low-skilled natives implying that immigration increases earnings disparity between these groups. This finding is largely robust to variation in the underlying population, changes in instrument reference date, and remains even after large locations are omitted from analysis. Moreover, it is broadly consistent with research by Dustmann et al. (2013), who study the effects of immigration on natives' wages in the UK, Kuosmanen and Meriläinen (2022) who analyze the impact of posted workers on Finnish construction workers, and Edo and Özgüzel (2023) who evaluate the effects of immigration on native employment with a cross-European sample.

There are limitations in my work, primarily related to methodology and scope. Foremost, it is difficult to establish the validity of the parallel trends assumption, and the concentration of immigrant flows into Helsinki appears to disproportionately influence my estimates. This complicates efforts to precisely identify effect sizes at each percentile, and I cannot entirely rule out that the estimates capture divergent development between locations that would have occurred regardless of immigration. However, the robustness check of removing large and small locations somewhat abates this concern - the finding of negative effects on low-earning natives and positive effects on high-earners persists, albeit with the point at which effects turn positive shifted higher in the distribution.

Additionally, as I aim to provide as broad of a view as possible, I study all immigrant flows across the entire labor market, rather than a specific context with a well defined exogenous shock. This makes the logic underlying the identifying assumptions less clear than in related research that focuses on the effects of specific policy changes (Beerli et al., 2021; Peri et al., 2015; Signorelli, 2024). A study focusing specifically on inflows of refugees, which are caused by exogenous push factors in the departure country rather than by Finnish pull factors, to similar locations with relatively small immigrant enclaves could provide more clear identification. However, this approach is less general than mine.

Finally, my analysis focuses on short-term, individual-level impacts. I cannot comment on longer-term or macroeconomic consequences which are also highly relevant in the Finnish context.

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Online Appendix for: ″The Effect of Immigration on Natives′ Earnings in Finland″

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A Rotemberg weights in the municipality level specifications

Table A1 presents Rotemberg weights derived at the municipal level. The conclusions are broadly the same as with their TTWA level counterparts, although notably Sweden is dropped from the list of top ten countries and is replaced by Turkey.

Table A1: *Municipality level Rotemberg weights*

Panel A: Negative and positive weights			
	Sum	**Mean**	**Share**
Negative	-0.038	-0.002	0.035
Positive	1.038	0.008	0.965
Panel B: Variance in α over years			
	Sum	**Mean**	
2002	0.017	0.000	
2003	0.013	0.000	
2004	0.019	0.000	
2005	0.035	0.000	
2006	0.063	0.000	
2007	0.079	0.001	
2008	0.064	0.000	
2009	0.066	0.000	
2010	0.057	0.000	
2011	0.088	0.001	
2012	0.079	0.001	
2013	0.098	0.001	
2014	0.100	0.001	
2015	0.066	0.000	
2016	0.055	0.000	
2017	0.061	0.000	
2018	0.039	0.000	
Panel C: Top ten countries			
	α	β	g
Estonia	0.189	-6243	3290
USSR	0.078	-7830	2058
China	0.068	679	747
Somalia	0.058	-143	628
India	0.046	2065	600
Russia	0.037	13578	871
Thailand	0.034	-2232	601
Afghanistan	0.031	8265	516
USA	0.031	7094	375
Turkey	0.026	-8499	365

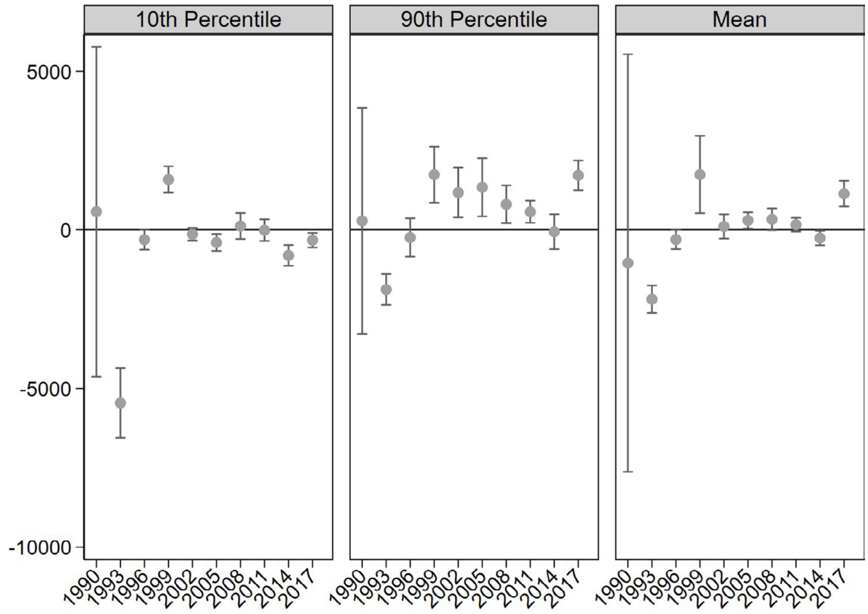
The table presents Rotemberg weights of the country of origin shares from the municipal level IV model with mean earnings of natives as the dependent variable. The α represents the country specific Rotem- berg weight, $\hat{\beta}$ the estimated coefficient from an IV regression where only shares from that country of origin are used as an instrument, and g the total shock from a specific country.

B Pretrend regression coefficients from the municipal level

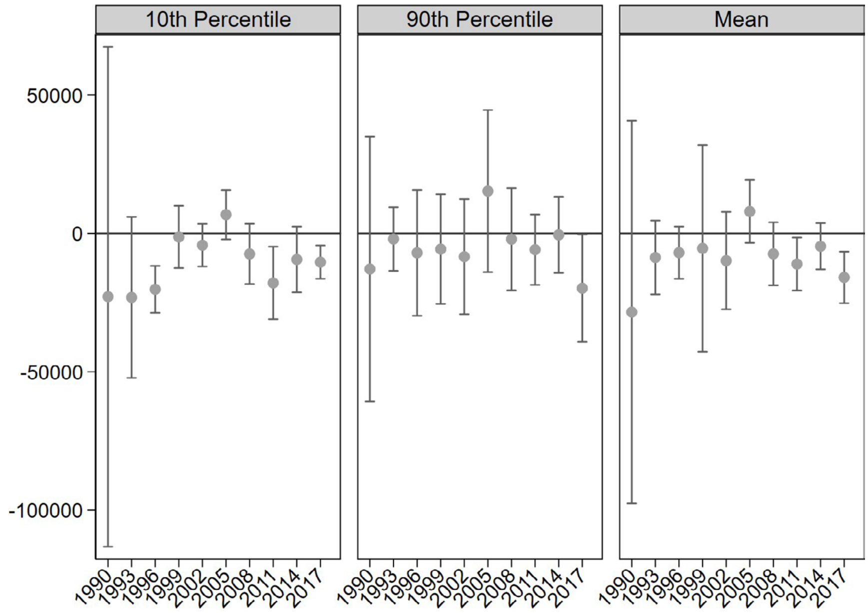
Figure B1 presents pretrend regression coefficients as in section 4.2.2 of the main paper, but now estimated at the municipal level. The conclusions are largely the same as at the TKA level.

Figure B1: *Pretrend regression coefficients at the municipal level*

(a) Full sample



(b) Top and bottom 5% of municipalities by labor force omitted



The figure presents estimated coefficients from a regression of annual change in natives' mean earnings against the aggregate share of immigration in a TTWA in 1993. Controls are fixed to their differenced 1993 values and interacted with a year dummy. Year fixed effects are included. Standard errors are clustered by TTWA.

C Municipal level robustness checks

This section presents robustness checks of the single-IV results at the municipal level. The checks are equivalent to the TTWA level ones presented in section 6 of the paper, except that instead of dropping the largest and smallest TTWAs I remove the the largest and smallest 5% of municipalities[10]. The results are presented in Table C1.

In the 'trimmed' specification the instrument is now too weak for reliable inference. The other checks show relatively stable estimates below the 50th percentile but disperse above this. These observations indicate that the exact level of point estimates in the baseline are less stable and reliable than at the TTWA level, further justifying using the latter as the primary focus. However, all specifications exhibit the trend of negative effects on low- earning natives and positive effects on high earners, supporting the central finding.

D Robustness to changes in the instrument reference date

Tables D1 and D2 re-estimate the IV models with alternate reference years for the instrument.

Overall, the estimates are rather insensitive to changing the instrument's reference date. The trend of negative and statistically significant point estimates for low-income natives, near zero estimates for median natives, and positive but statistically insignificant estimates for high-income natives holds across all specifications. Effect in- tensity increases with distance from the median in all the models as well. Furthermore, point estimates below the median are rather similar to each other in value. Therefore, it appears my findings are not specific to the choice of instrument reference year.

[10] From largest to smallest, the dropped municipalities are: Helsinki, Espoo, Tampere, Vantaa, Oulu, Turku, Jyväskylä, Lahti, Kuopio, Kouvola, Pori, Eckerö , Kustavi, Luhanka, Fö glö , Brändö , Geta, Vårdö , Lumparland, Kumlige, Kö kar, and Sottunga.

Table C1: *Municipal robustness checks*

	Baseline	Trimmed	Age	Male	Female	Salaried
Mean	-182 (365)	-1567 (1001)	-654 (396)	-565 (576)	127 (280)	243 (446)
5th percentile	-865 (169)	-757 (435)	-1658 (173)	-958 (273)	-1540 (327)	-1062 (300)
10th percentile	-3039 (472)	-2760 (721)	-3724 (527)	-2934 (421)	-3663 (613)	-3030 (458)
25th percentile	-2748 (426)	-3339 (930)	-3375 (511)	-2658 (542)	-3856 (633)	-2451 (420)
50th percentile	-713 (285)	-2329 (892)	-1022 (319)	-1422 (439)	-225 (251)	-440 (307)
75th percentile	282 (370)	-1253 (882)	-109 (413)	-618 (543)	1705 (283)	589 (403)
90th percentile	1274 (490)	-647 (944)	916 (550)	-137 (698)	3595 (494)	2000 (549)
95th percentile	2770 (620)	662 (1734)	2267 (685)	1670 (1352)	6863 (689)	3639 (736)
Year dummies	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
N	3619	3245	3604	3179	3485	3570
Robust F	22	12	21	27	19	23
KP χ^2	8.804 [0.003]	12.503 [0.000]	8.136 [0.004]	8.132 [0.004]	7.068 [0.008]	8.259 [0.004]

The name at the top identifies what aspect of the population has been changed to test robustness. 'Baseline' refers to the single-IV results presented earlier. Under the 'Trimmed' model the baseline is re-estimated after dropping the largest and smallest TTWAs (Helsinki and Parkano, respectively). In the 'Age' model the age range of the population is decreased from 15-74 to 20-59. Columns 'Male' and 'Female' consider men and women separately. Model 'Salaried' excludes self-employed workers. Other specification details are the same, except the controls for sex ratio are omitted in the 'Male' and 'Female' models. The estimates are scaled to reflect the effect of the immigrant-to-native ratio increasing from the 10th to the 90th percentile of each sample population. The scaling factors are, in order: 0.085, 0.062, 0.100, 0.101, 0.085, 0.093.

Table D1: *Municipal level reference year robustness checks*

	1991	1992	1993	1994	1995
Mean	-310 (391)	-326 (389)	-182 (365)	-282 (356)	-267 (358)
5^{th} percentile	-934 (161)	-876 (170)	-865 (169)	-840 (167)	-898 (162)
10^{th} percentile	-3047 (458)	-3104 (431)	-3039 (472)	-2915 (470)	-2924 (489)
25^{th} percentile	-2814 (404)	-2826 (409)	-2748 (426)	-2691 (417)	-2671 (438)
50^{th} percentile	-725 (279)	-806 (304)	-713 (285)	-725 (269)	-685 (265)
75^{th} percentile	254 (370)	199 (390)	282 (370)	198 (345)	225 (335)
90^{th} percentile	1203 (500)	1175 (513)	1274 (490)	1105 (476)	1119 (471)
95^{th} percentile	2568 (674)	2577 (644)	2770 (620)	2572 (620)	2497 (647)
Year dummies	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
N	3619	3619	3619	3619	3619
Robust F	25	30	22	22	20
KP χ^2	9.224 [0.002]	8.472 [0.004]	8.804 [0.003]	9.295 [0.002]	9.622 [0.002]

The year at the top identifies the reference year used to construct the instrument. The 1993 estimates are the baseline. Other specification details, including scaling, and the data are the same as in the baseline IV model.

Table D2: *TTWA level reference year robustness checks*

	1991	1992	1993	1994	1995
Mean	590 (238)	606 (226)	645 (225)	644 (228)	627 (241)
5th percentile	-616 (134)	-574 (136)	-592 (126)	-583 (132)	-634 (140)
10th percentile	-1616 (237)	-1654 (256)	-1661 (252)	-1616 (240)	-1686 (257)
25th percentile	-1446 (321)	-1389 (329)	-1423 (329)	-1405 (333)	-1447 (344)
50th percentile	204 (161)	196 (172)	194 (163)	210 (166)	195 (167)
75th percentile	1293 (215)	1299 (220)	1301 (223)	1343 (227)	1305 (242)
90th percentile	2354 (337)	2432 (346)	2398 (354)	2416 (359)	2373 (375)
95th percentile	3236 (387)	3277 (388)	3279 (396)	3275 (400)	3305 (402)
Year dummies	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
N	612	612	612	612	612
Robust F	644	367	724	535	556
KP χ^2	4.357 [0.037]	4.357 [0.037]	4.172 [0.041]	4.209 [0.040]	4.219 [0.040]

The year at the top identifies the reference year used to construct the instrument. The 1993 estimates are the baseline. Other specification details, including scaling, and the data are the same as in the baseline IV model.

E AKM standard errors

Adao et al. (2019) show that when a model contains regions with highly correlated share components, conventional standard errors can lead to overrejection if there are shift-share terms in the residual that are not accounted for. They devise two estimation methods that produce standard errors robust to such correlation in residuals.

However, this insight is derived under the 'shocks' view of exogeneity, where there are many idiosyncratic shock factors (Adao et al., 2019; Borusyak et al., 2022). I take the 'shares' perspective under which the shares are exogenous by assumption, and via Rotemberg weights show that most of the variation in immigration is derived from only 10 countries. Hence, I believe there are insufficient shocks to justify the AKM approach. Furthermore, for the kind of correlation in residuals that (Adao et al., 2019) are concerned with to exist in my research context, there would have to be an unobserved shift-share term that affects locations differentially via shares similar to the country of origin shares used in the instrument. It is hard to imagine what such a factor could be.

Nevertheless, for completeness, I have calculated AKM and AKM0 confidence intervals for my IV point estimates and present them in Table E1.

The AKM and AKM0 confidence intervals are enormous at the municipal level, making inference impossible. This is likely a result of the large number of small municipalities with very low immigration that have highly correlated near-zero shares.

At the TTWA level, the AKM confidence intervals are wider than their cluster-robust counterparts, but inference remains the same everywhere except for estimated effects on mean earners, where statistical significance is lost. The AKM0 intervals are again extremely large, and the only statistically significant estimate is for the effect on 25th percentile earners. However, as Adão et al. (2018) discuss in section F of their online appendix, the AKM0 method tends to be too conservative in immigrant enclave shift-share specifications like mine. Their proposed solution is to remove the countries of origin with the largest volumes of immigration at the reference date. However, as I take the shares approach in which various locations are differentially exposed exactly to these large inflows from a few countries, I feel it is inappropriate to remove them from the analysis.

Overall, because I take the 'shares' view and am using an immigrant enclave instrument instead of an industry share one, I believe the issues that Adao et al. (2019) attempt to address are not relevant in my setting. However, even if they were, the TTWA level results and inferences remain unchanged, at least under the AKM procedure.

Table E1: *AKM standard errors*

	First stage	Mean	5th	10th	25th	50th	75th	90th	95th
Panel A: Municipal									
$\hat{\beta}$	0.609	-182	-865	-3039	-2748	-713	282	1274	2770
Cluster	[0.356, 0.862]	[-898, 533]	[-1196, -535]	[-3963, -2115]	[-3583, -1912]	[-1272, -154]	[-442, 1007]	[314, 2235]	[1555, 3984]
AKM	[-4.769, 5.986]	[-13822, 13455]	[-7079, 5351]	[-22723, 16657]	[-24354, 18858]	[-19971, 18541]	[-21737, 22298]	[-21440, 23979]	[-31375, 36898]
AKM0	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]
Panel B: TTWAs									
$\hat{\beta}$	1.012	645	-592	-1661	-1423	194	1301	2398	3279
Cluster	[0.938, 1.086]	[204, 1086]	[-839, -344]	[-2154, -1167]	[-2067, -778]	[-125, 513]	[865, 1738]	[1704, 3091]	[2503, 4055]
AKM	[0.792, 1.231]	[-48, 1338]	[-895, -288]	[-2401, -920]	[-2131, -714]	[-434, 822]	[304, 2299]	[1095, 3701]	[1389, 5169]
AKM0	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]	(-∞, -1302]	(-∞, -932]	(-∞, 516]	[-∞, ∞]	[-∞, ∞]	[-∞, ∞]
				∪ [104, ∞)	∪ [-242, ∞)	∪ [1610, ∞)			

The table presents the estimated first stage coefficients from Table 3 and the IV point estimates from Table 4 of the main paper with cluster-robust, AKM and AKM0 confidence intervals.

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