



Immigration and economic mobility

Maria F. Hoen¹ · Simen Markussen¹ · Knut Røed¹

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Abstract

We examine how immigration affects natives' relative prime-age labor market outcomes by economic class background, with class background established on the basis of parents' earnings rank. Exploiting alternative sources of variation in immigration patterns across time and space, we find that immigration from low-income countries reduces intergenerational mobility and thus steepens the social gradient in natives' labor market outcomes, whereas immigration from high-income countries levels it. These findings are robust with respect to a wide range of identifying assumptions. The analysis is based on high-quality population-wide administrative data from Norway, which is one of the rich-world countries with the most rapid rise in the immigrant population share over the past two decades. Our findings suggest that immigration can explain a considerable part of the observed relative decline in economic performance among natives with a lower-class background.

Keywords Immigration · Intergenerational mobility

JEL Classification J62 · J15 · J24

1 Introduction

This paper provides an empirical analysis of the relationship between immigration and intergenerational economic mobility within a native population. While there is a large literature on the impacts of immigration on natives' average labor market

Responsible editor: Klaus F. Zimmermann

✉ Knut Røed
knut.roed@frisch.uio.no

Maria F. Hoen
m.f.hoen@samfunnsforskning.no

Simen Markussen
simen.markussen@frisch.uio.no

¹ The Ragnar Frisch Centre for Economic Research, Oslo, Norway

outcomes as well as on the outcomes of particular skill groups, this paper is, to our knowledge, the first to examine its impacts on natives' intergenerational mobility. More specifically, we study how exposure to immigration from different types of source countries affects natives' relative prime-age employment and earnings outcomes by economic class background. The latter is specified in terms of the parents' earnings rank within a generation, as in Dahl and DeLeire (2008), Chetty et al. (2014), Corak et al. (2014), Bratberg et al. (2017), and Pekkarinen et al. (2017).¹ We refer to the relationship between class background and own adult outcomes as the social gradient. The steeper the social gradient, the lower the economic mobility.

The basic idea behind our empirical approach is that economic class background is highly correlated with ultimate skill levels, such that heterogeneity in the impacts of immigration on natives' economic performance can be traced in its impact on the social gradient. Economic theory suggests that natives with qualifications that are complementary to the qualifications of immigrants gain from higher immigration, at least in relative terms, whereas natives with qualifications similar to those of immigrants may lose.² Since natives with a disadvantaged family background have a disproportionally high probability of competing in the low-skill segment of the labor market, we expect immigration from countries with a large low-skill component to steepen the social gradient in economic outcomes. Conversely, we expect immigration from countries with a large high-skill component to level the social gradient.

Our empirical analysis builds on data from Norway, which is one of the rich-world countries with the most rapid rise in the immigrant population share, particularly after the expansion of the European labor market in 2004. The overall immigrant share of the adult population (age 25–66) quadrupled between 1992 and 2017—from 5 to 20%—and the vast majority of the new immigrants came from countries with much lower earnings levels and living standards than Norway, i.e., from less developed countries (LDC) and Eastern Europe. At the same time, recent empirical evidence indicates that economic mobility declined considerably for natives born into the poorest families. With economic class background defined in terms of parents' earnings rank, Markussen and Røed (2020) examine economic mobility among native offspring born between 1952 and 1975 and show that the bottom decile has fallen systematically behind in terms of own adult earnings rank as well as employment propensity.

The question we address in this paper is whether—and to which extent—there is a causal relationship between the rise in immigration and the decline in intergenerational mobility among natives. We use administrative register data covering all individuals born in Norway from 1960 through 1980. The identifying variation in immigration exposure comes from commuting-zone-birth-cohort cells, where each individual is assigned the commuting zone inhabited during adolescence (by age 15/16). Immigrant exposure is measured as the immigrant population share in the respective commuting zone by age 32, whereas adult economic

¹ In the [appendix](#), we also present results based on models where class background is determined from parents' educational attainment and from the earnings rank of grandfathers.

² See, e.g., Borjas et al. (1996) or Borjas (1999; 2014).

outcomes are measured during age 33–36. To take into account that immigrants from different source countries compete in different skill segments of the labor market, we estimate separate effects of exposure to immigration originating in high-income and low-income source countries.

We focus on three outcomes: earnings rank within own birth cohort and commuting zone, earnings level relative to the cohort and commuting zone average, and employment (relative to a reference class background). Although we think of contemporary labor market competition as the primary causal channel linking immigration to these outcomes, we also examine the case for effects operating via educational attainment, labor market entry, and regional migration. The explanatory variables of interest are interaction terms between measures of exposure to immigration and class background (indicators of parental earnings rank). Our analysis faces a number of identification challenges related to reverse causality (e.g., that relatively favorable prospects for low-skill workers attract a disproportionately large number of low-skill immigrants), as well as to local factors affecting both immigration and economic mobility (e.g., changes in industrial or occupational structure of local labor markets and business cycle fluctuations). Although we do not have access to experimental (random-assignment-like) variation in natives' exposure to immigration, our empirical analysis aims to build a solid case for a causal interpretation of our key findings. To this end, we apply a wide range of identification strategies that exploit different sources of variation in immigrant exposure. In essence, we seek to annihilate simultaneity problems by using alternative combinations of fixed effects to absorb causal factors potentially correlated with both immigration and outcomes.

In our baseline model, we control for class-by-cohort fixed effects. This implies that we control for all countrywide changes in the different classes' relative outcomes. In alternative models, we control for the same types of class-specific changes in relative outcomes at *regional* levels, where "regions" are defined either by geographic proximity or by the degree of urbanity. We also exploit other dimensions of variation in immigrant exposure, such as the pure idiosyncratic variation within commuting zones. Based on the non-experimental data used in our analysis, we of course cannot eliminate simultaneity concerns completely. Yet, the main takeaway from the many models estimated in this paper is that the conclusions regarding the impact of immigration on economic mobility are robust with respect to different identifying assumptions.

In an auxiliary analysis, we examine directly the determinants of immigration patterns, including the extent to which the spatial distribution of immigrants from low- and high-income countries responds to the local relative economic performance of natives with different class background. Depending on the margin of variation in immigration patterns, we indeed find some evidence that immigrants from low-income countries are attracted to commuting zones where natives from lower classes perform relatively well. Such a sorting process will induce a positive bias in the estimated influence of immigration from low-income countries on economic mobility. As our results are going to point in the opposite direction, it does not undermine the causal interpretation of the findings reported in this paper but may instead imply that some of the estimated effects should be interpreted as lower bounds.

Our empirical strategy circumvents a number of the most common challenges in the empirical literature on the impacts of immigration on native outcomes (reviewed in the next section). First, by using family background to characterize natives rather than their actual education, experience, or occupation, we avoid simultaneity problems related to endogenous choices of these skill characteristics. Second, since family background is observed for complete birth cohorts, we can examine impacts on *all* natives and thus include prime-age employment status as an outcome of interest. This is potentially important, as recent research has indicated much larger immigration effects on employment than on wages in a setting similar to the one analyzed in the present paper (Dustmann et al. 2017). Third, by categorizing immigration flows in terms of origin country rather than formal skills, we avoid missing observations due to lack of data on immigrants' human capital and yet obtain a grouping that corresponds closely to the skill segments of the labor market. At the same time, we circumvent the skills downgrading problem discussed by Dustmann et al. (2016), i.e., that immigrants' formal skills are "downgraded" upon arrival in the host country. Finally, by examining the outcomes of individuals rather than of cell averages and by assigning each individual a fixed predetermined commuting zone, we steer clear of simultaneity problems related to endogenous native migration across commuting zones.

While the focus on predetermined family background as the key distinguishing feature of natives solves some methodological problems, it also provides the most direct route toward answering the research question addressed in this paper: Can recent immigration patterns explain why lower class individuals have fallen systematically behind in economic outcomes over the past few decades? Our findings suggest that the answer to this question is yes: Immigration has indeed played an important role in the steepening of the social gradient in labor market outcomes among natives. We present robust evidence showing that immigration from low-income countries has significantly reduced economic mobility and thus steepened the social gradient, whereas immigration from high-income countries has levelled it. A probable explanation is that immigration affects economic mobility by changing the skill-composition of the labor force. As the immigration to Norway over the past decades predominantly has been of the low-skill type, it is likely to have raised the economic returns to skills. And since lower-class individuals on average accumulate less skills than those from higher classes, a larger skill premium implies, *ceteris paribus*, a steeper social gradient in economic outcomes. One could hypothesize that this mechanism would be offset by low-skill individuals accumulating more education in response to the increased returns to skills. However, when we examine this empirically, we find that immigration from low-income countries has contributed to a steepening of the social gradient also in educational attainment. If anything, the choice of educational attainment appears to be less sensitive with respect to immigration patterns in the lower than in the higher classes.

Our findings can rationalize the apparent polarization of attitudes toward immigration, and in particular the rise of anti-immigrant sentiments among lower-class natives. Existing evidence from many different countries shows that higher education and higher labor market skills invariably mean more support for liberal immigration policies (Scheve and Slaughter 2001; Mayda 2006; Hainmueller and Hiscox

2007), and some studies suggest a direct causal relationship between fears of labor market competition and attitudes toward immigration (Malhotra et al. 2013; Dan-cygier and Donnelly 2013; Finseraas et al. 2017). There is also evidence indicating that low-skill immigration tends to raise support for right-wing anti-immigrant political parties, whereas high-skill immigration reduces it (Halla et al. 2017; Harmon 2018; Moriconi et al. 2018; Edo et al. 2019) and that the former of these effects is particularly strong among low-skilled natives (Moriconi et al. 2018; Edo et al. 2019). Relatedly, recent empirical evidence demonstrates that job-threatening import competition from China has led to more political polarization and increased support for nationalist populism in both the USA (Autor et al. 2020) and Europe (Colantone and Stanig 2018). While our study does not examine the impacts of immigration on attitudes or voting behavior directly, it arguably indicates that the historically large influx of migrants from low- to high-income countries, by reducing economic mobility and steepening the social gradient in native outcomes, has also laid the foundation for a more polarized political environment in the host countries.

2 Related literature

Our paper relates to a large empirical literature on the impacts of immigration on natives' earnings and employment patterns; see, e.g., Longhi et al. (2010), Peri (2014), Borjas (2014), Card and Peri (2016), and Dustmann et al. (2016) for recent overviews and discussions about the literature's somewhat conflicting results. Empirical specifications differ along a number of dimensions, such as the margin of variation in immigration flows (e.g., by skill cells, by geographical cells, or by a combination of the two), the way immigration flows are measured (e.g., relative to the current or the past labor force), and the selection of endogenous outcome (e.g., wages versus employment). For the papers relying on spatial variation in immigrant exposure, there is also a major identification issue caused by endogenous migration patterns: Immigrants do not randomly sort into locations, but are instead disproportionately attracted to areas with favorable labor demand conditions. Many of the empirical contributions to the literature rely on a shift-share identification strategy, whereby actual immigration flows to each labor market region are instrumented by the corresponding national flows interacted with each region's initial fractions of the respective immigrant groups. However, the literature does not provide a consistent picture of the causal effects. Jaeger et al. (2018) argue that this ambiguity is related to fundamental methodological problems with the shift-share strategy: If regional labor demand shocks are serially correlated, the exclusion restriction does not hold, and even if it holds, the inclusion of past settlement patterns in the instrument may conflate the short- and long-term effects of immigration.

The literature focusing more directly on differential impacts by skill or experience categories is also characterized by conflicting results. While some empirical studies claim to have found evidence of large adverse impacts of immigration on similarly skilled native workers (e.g., Borjas 2003; Jaeger 2007; Borjas and Katz 2007), others report small or insignificant effects (e.g., Card 2001; Friedberg 2001; Dustmann et al. 2005; Carrasco et al. 2008; Ottaviano and Peri 2008; 2012; Manacorda et al.

2012). A contribution to this literature based on Norwegian data is Bratsberg and Raam (2012), who use occupational licensing requirements within a particular industry (the construction sector) as a source of exogenous variation in immigrant employment across occupations, and find evidence of a significant negative causal relationship between an occupation's exposure to immigration and its wage level. Similar results are obtained by Hoen (2020), based on the usage of each occupation's language requirements as an indicator for exposure to immigrant competition. These findings imply that natives in exposed occupations have an incentive to move towards less exposed, and presumably better paid, occupations in response to higher immigration. Based on a random-assignment-like dispersion of refugees (and family migrants) across municipalities in Denmark, Foged and Peri (2015) find evidence of such effects for low-skilled native workers.

A common feature of existing research into the differential impacts of immigration is that the natives' skill level is wholly or partly interpreted as exogenous, such that the analysis is conditioned on educational attainment, occupation, or experience, and in many cases also on (initial) employment. These studies cannot tell the whole story, however, as educational and occupational choices are likely to be affected by immigration. Studies conditioning on occupation—and thereby also on employment—also miss out on one of the most plausible channels for impact of immigration, namely on the inflow of natives into work (Dustmann et al. 2017). To our knowledge, the present paper is the first to examine differential impacts of immigration based on a native characteristic that is highly correlated with ultimate skills, yet not affected by own behavior, and observed for everyone. This allows for an unconditional analysis with respect to the individuals' own educational or labor market achievements, facilitating employment as an important outcome.

3 Immigration to Norway

Figure 1 illustrates the rapidly growing share of immigrants among adult (age 25–55) residents and workers in Norway: from approximately 6% in 1992 to 22% in 2017. An immigrant is in this paper defined as a person who was born outside Norway by non-Norwegian parents and who settled in Norway after the school-starting age of 6 years. We have divided the immigrant population into three groups, i.e., immigrants from other high-income countries (Members of the European Economic Area (EEA) prior to 2004, North America, Japan, and Oceania), from Eastern Europe (countries that entered the European Union in 2004 and 2007 and European countries outside EEA), and less developed countries (rest of the world).

Norway has since 1975 had a general freeze on non-humanitarian immigration, with an important exception for Nordic citizens, as well as for labor with qualifications considered to be in short supply. After the EU enlargement in 2004, there has been a massive flow of work-related immigration from Eastern Europe, and in particular from Poland and Lithuania. Throughout the period since 1975, immigration from less-developed countries has been dominated by humanitarian motives, i.e., refugees, asylum seekers, and subsequent family (re)unifications.

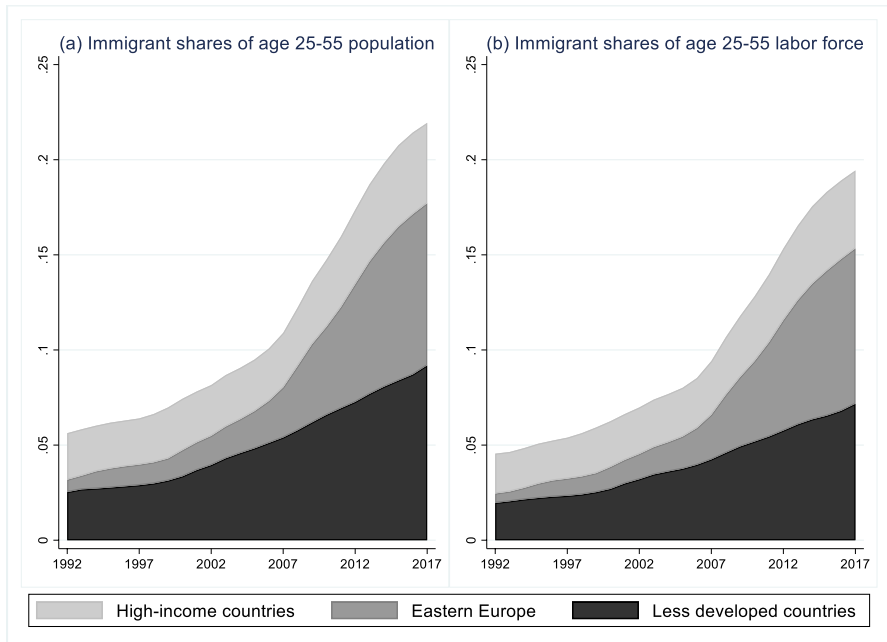


Fig. 1 Immigrant population and labor force participation shares in Norway 1993–2017 by type of origin country. Note: The figure shows the shares of immigrants in the adult (age 25–55) population (panel (a)) and in the labor force (panel (b)). The labor force is defined as all natives and immigrants with work-related earnings exceeding approximately one third of average full-time-full-year earnings in Norway and/or registration as an active job seeker at the employment office

Work-related immigration from developing countries has been limited to certain “specialist” or “seasonal worker” programs.

Consequently, we see from Fig. 1 that immigrants from less-developed countries constitute a larger share of the working age population than of the labor force. Yet, annual labor force participation shares may underrate the influence that immigrants from developing countries have had on the Norwegian labor market. Although participation rates for humanitarian immigrants tend to be very low during the first years after entry, they pick up considerably after 5–10 years (Bratsberg et al. 2017). Based on our own data reported in Fig. 1, we note that as much as 94.7% of the immigrants from less developed countries have at least 1 year with active participation in the labor market during their first 10 years of residency. However, as they tend to end up in precarious and marginal jobs, employment spells are typically much shorter than for natives (Bratsberg et al. 2018).

Labor market integration of immigrants in Norway does not stand out as very different from other European countries; see, e.g., Dumont et al. (2016), OECD (2019), and Brell et al. (2020). The employment rates of both refugees and other non-EU born residents in Norway are in line with the EU average, at approximately 55 and 60%, respectively (Dumont et al. 2016).

4 Data and definitions

Our data include all natives born between 1960 and 1980 who are still residents in Norway by age 32, and the purpose of our analysis is to examine how exposure to immigration during adolescence and young adulthood has affected relative prime age (33–36) earnings and employment outcomes (measured 1993–2016) by economic class background. Since we are going to use the variation in immigration patterns across commuting zones (travel-to-work areas) as the key source of identification, we need to assign each person to a commuting zone. Our definition of commuting zones follows Bhuller (2009), which divides Norway into 46 such zones. As we return to below, the size distribution of these zones is highly skewed. The largest city area around the capital alone accounts for 22.6% of the observations, whereas the four largest city zones taken together account for 44.2%. Since the commuting zone of actual residence in adulthood may have responded endogenously to immigration patterns, we assign each person to the strictly predetermined commuting zone inhabited at age 15/16. This information is obtained from educational registers containing addresses at the time of primary school completion.

4.1 Class background

Each person is assigned a class background on the basis of the average of his/her parents' labor earnings (including self-employment earnings). To do this in practice, choices have to be made both with respect to *which* and *how many* earnings years to include in order to appropriately capture “class background.” These choices involve tradeoffs between concerns related to lifecycle bias (Solon 1999; Grawe 2006), attenuation bias (Solon 1992), consistency over time (Markussen and Røed 2020), and—in our case—possible simultaneity problems related to effects of immigration on parental ranks. Our limitation is that earnings data are observed from 1967 and onwards, while most of the parents used in our analysis are born between 1925 and 1960. In the main part of the paper, we apply four alternative ranking algorithms, all based on the sum of parental (wage-inflation-adjusted) earnings over a given period:

- i. Parental earnings during their age 52–58
- ii. Parental earnings during their age 33–36³
- iii. Parental earnings during the offspring's age 7–15
- iv. Parental earnings during 1980–1992

Earnings obtained during the age of mid 50's have been shown by Markussen and Røed (2020) to yield the highest correlation with lifetime earnings. For our purpose, they also have the advantage of being observed for almost all parents to the cohorts included in our analysis. A disadvantage is that they to some extent are measured in the same period as we measure offspring outcomes and well

³ For parents born before 1934, we have to measure earnings at higher ages.

into the period with large immigration flows; hence, ages 52–58 measurement of parental earnings may entail some simultaneity problems. If immigration has effected the rank outcomes of offspring, it may as well have affected the earning rank of parents, making the interpretation of results regarding economic mobility less clear. By measuring parents at earlier ages or at a calendar time strictly prior to the immigration patterns used to identify the effects on offspring (starting in 1993), we reduce this potential source of simultaneity but at the cost of weaker association with lifetime earnings.

Given that immigration to Norway started before 1993 and that the spatial pattern of immigration may be persistent, none of the ranking criteria presented above can rule out completely that the migration patterns used to identify the causal impact on offspring's economic mobility exhibit some statistical association with the sorting of parents into the different ranks. In the [Appendix](#), we present results based on two additional ranking criteria: parents' educational attainment (measured in 1990) and grandfathers' earnings, respectively. These ranking criteria substantially reduce or (in the case of grandfather earnings) almost eliminate concerns that the class background itself has been influenced by immigration. However, as we explain in the [appendix](#), the grandfather-based ranking criterion can be computed only for a subset of the data.

The ranks are in all cases computed from the earning distribution within each commuting zone. We use commuting zones (rather than the whole country) as the foundation for ranking in this paper to ensure that we compare offspring who, conditional on parental rank, have been exposed to similar overall economic and labor market developments. Had we used a national ranking algorithm, the distribution of classes would have varied considerably across commuting zones, implying that geographically differentiated economic trends could have affected different classes differently. For each annual birth cohort, the ranking is made separately for sons and daughters based on earning comparisons with all other parents to offspring born in the same year and living in the same commuting zone. Administrative registers ensure that 93–99% of all native birth cohorts are included in the dataset with appropriate information on both class background and commuting zone. In total, we have 1,116,827 observations that can be used in the empirical analysis.

In most of our empirical analysis, we divide the native population into five parental earnings classes (quintiles), with the aim of identifying separate immigration effects for the first, second, fourth, and fifth quintile relative to the third (middle) quintile. The choice of five classes in this context represents a compromise between the aim of allowing for sufficient variation in impacts across the class distribution and the need for including a wide range of class-specific fixed effects, which soak up more degrees of freedom the larger is the number of distinct classes.

4.2 Economic outcomes

For the offspring generation, we focus on three prime-age labor market outcomes:

Fig. 2 Average economic outcomes at prime age (33–36) for the bottom and top parental earnings class quintiles by year of birth. Note: Parental earnings quintile is calculated on the basis of the parents' age 52–58 earnings. Panels (a) and (b) show average earnings percentile rank within own birth cohort, by class. Panels (c) and (d) show average earnings by class, normalized by average earnings for the whole birth cohort. Panels (e) and (f) show the average employment rate by class minus the corresponding employment rate for the whole birth cohort. All ranks/shares/differentials are gender-specific. The solid lines in panels (d)–(f) are linear regression lines. The scales on the graphs are chosen such that the ranges on the vertical axes are the same for the bottom and top classes

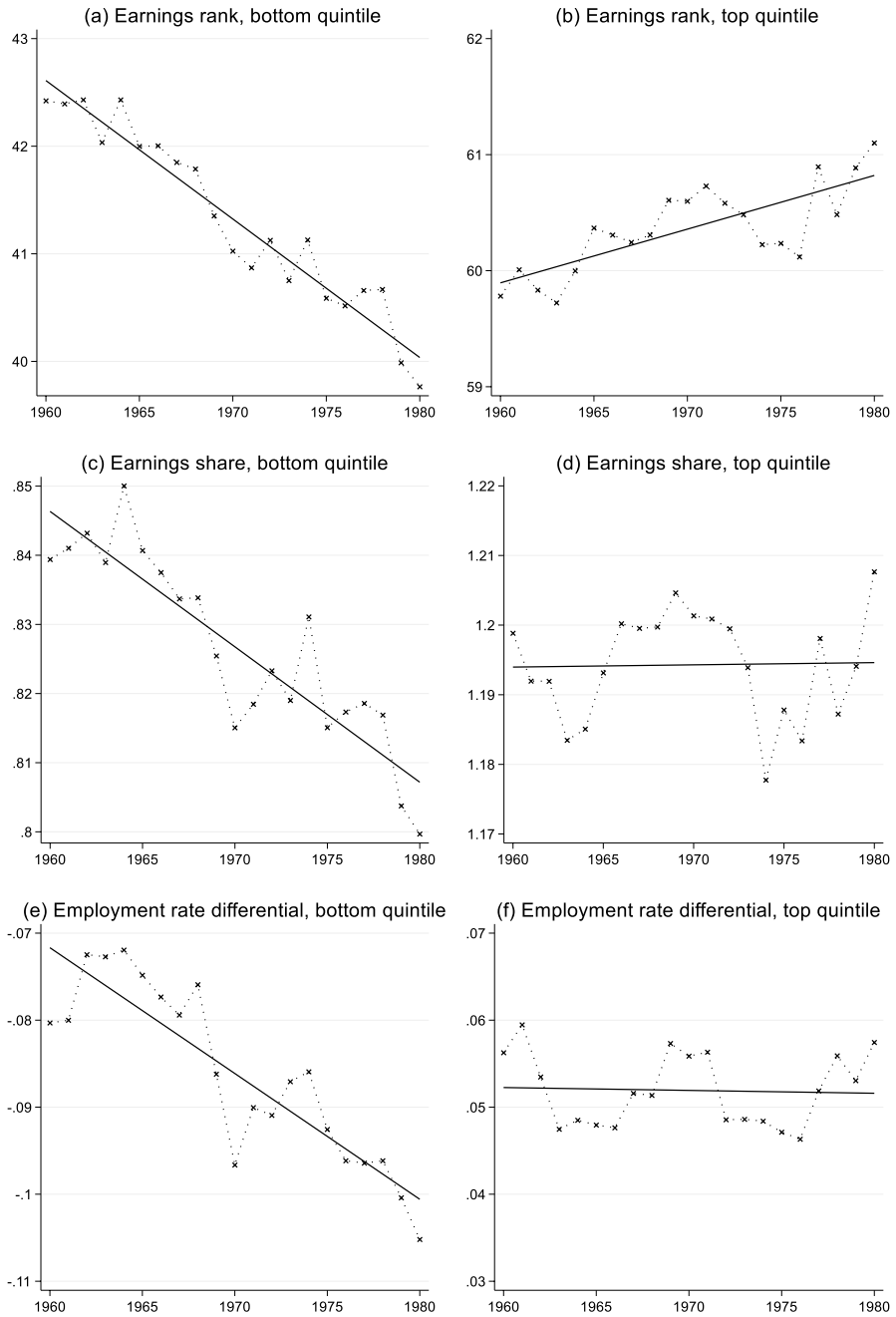
- i. Earnings rank: The rank position in the gender- and cohort-specific distribution of ages 33–36 earnings within the commuting zone, measured in percentiles⁴
- ii. Earnings share: The total earnings obtained in the age 33–36 period divided by the gender- and cohort-specific average in the commuting zone
- iii. Employment: A dummy variable equal to one if average annual labor earnings obtained in the age 33–36 period exceeded approximately one third of average full-time-full-year earnings in Norway⁵

Measurement at age 33–36 is chosen as a compromise between the purpose of minimizing lifecycle bias (which calls for measurement at higher ages) and the ambition of being able to examine the more recent and immigration-exposed birth cohorts (which calls for measurement at younger ages). The earnings measure consists of all labor-related earnings, including business income from self-employment (but not capital income). Figure 2 shows how the bottom and top class quintiles have performed according to these outcomes, for offspring born from 1960 to 1980. Since a stable interpretation of class background across cohorts is a key concern when we wish to characterize changes in mobility over time, we use parental earnings obtained during age 52–58 as the ranking criterion in this figure.

Figure 2 confirms the finding in Markussen and Røed (2020) that people born into the lowest socioeconomic classes have fallen systematically behind over time. In particular, their average earning rank outcomes have dropped by approximately 2 percentiles, their relative earnings have declined by approximately 3%, and their employment propensity has fallen by approximately 2 percentage point relative to the cohort average. For the top quintile, the trends are less clear, although their earnings rank outcomes appear to have trended upwards.

⁴ In cases where more than one percent have zero earnings, we have randomized these cases into the relevant bottom percentiles, such that there is always exactly one percent in each percentile.

⁵ More precisely, we require average annual earnings during these 4 years to exceed 2 times the so-called Basic Amount (BA) in the Norwegian pension system, which is adjusted each year approximately in line with the general wage growth. This definition of employment implies that it can be satisfied by having a very weak attachment to the labor market over 3–4 years or by having a strong attachment over just 1–2 years.



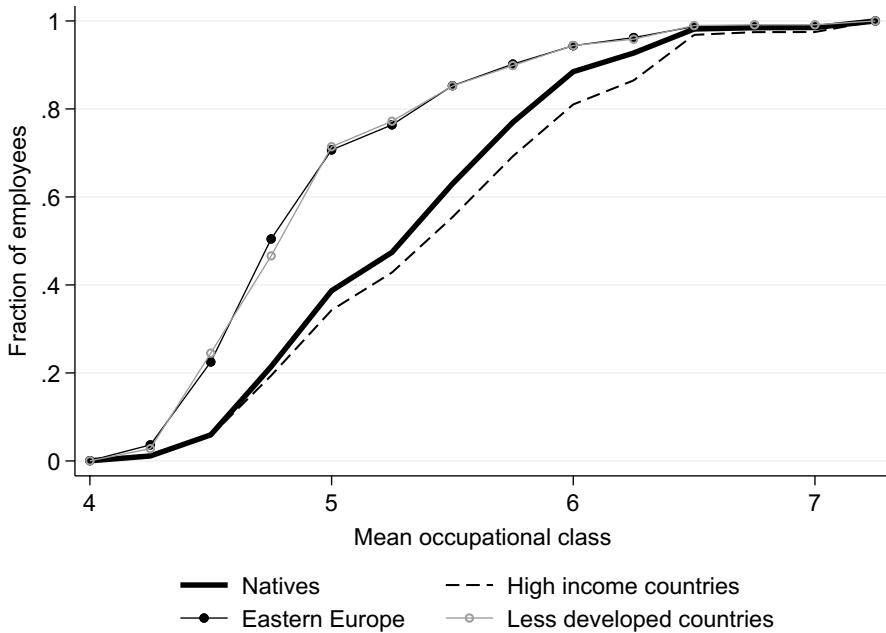


Fig. 3 The socioeconomic rank of occupations held by immigrants and natives. Note: The figure shows cumulative distribution functions for the socioeconomic rank of all occupations held at age 32 by natives and three different immigrant groups. An occupation's socioeconomic rank is computed as the average class background of all natives in the occupation in 2005, where each employee's class is computed as the parents' decile position in their generations' earnings distribution

4.3 Occupational class structure of native and immigrant employees

Before we examine the relationship between natives' labor market outcomes and their exposure to immigration, we take a closer look at the kind of jobs that immigrants actually take in order to see which groups of natives they compete with in the labor market. To do this, we first need to characterize jobs in terms of their class status. From 2003, the Norwegian employee register contains detailed occupational codes, based on the International Standard Classification of Occupations (ISCO 88).⁶ We use these auxiliary data to assess the class-structure of all occupations observed in our data (344 different occupations). This assessment is based on the population of employed adult natives, for which we have data on class background, i.e., we characterize each occupation's socioeconomic status by computing the average class background of its native employees, again based on parental earnings during age 52–58. The parental classes are here defined in terms of earning decile rank (running from 1 to 10), with mean equal to 5.5; hence, the occupational status codes are also defined on this scale. Equipped with these occupational status codes, we

⁶ The application of this standard on Norwegian data is described in Statistics Norway (1998), which also include a list of all occupations.

compare the distribution of employees across occupational statuses for natives and immigrants. As the occupational structure varies considerably between immigrants from different origin countries, we start out by dividing the immigrant population into the three groups described in Section 3; i.e., (i) other rich countries, (ii) less developed countries (LDC), and (iii) Eastern Europe. We then compare the occupational class structure observed for natives and the three immigrant groups.

Figure 3 shows distribution functions for the resultant occupational class structure. Although individual class backgrounds vary from 1 to 10, the averages taken over occupations essentially vary between 4 and 7. A first point to note from Fig. 3 is that immigrant workers from less-developed countries (LDC) and Eastern Europe are heavily overrepresented in occupations typically held by natives from the lower classes, whereas immigrants from high-income countries are overrepresented in occupations held by natives with a high rank. A second point to note is that the class structures of the jobs held by immigrants from less-developed countries and from Eastern Europe are hardly distinguishable. Hence, in a social class context, immigrants from these two country groups compete in exactly the same segments of the labor market. Based on this observation, we aggregate these two immigrant groups into a single one. In our empirical analysis, we thus divide the immigrant population into two groups:

- Low-income countries: Eastern Europe plus less-developed countries (LDC). The quantitatively most important countries in this group are Poland (2.8% of the adult population in Norway by 2016), Lithuania (1.1%), Somalia, Iraq, and the Philippines (all with 0.6% of the population)
- High-income countries: Rest of the world. The most important countries in this group are Sweden (1.0% of the adult population by 2016), Germany (0.6%), Denmark (0.4%), Great Britain (0.3%), and the USA (0.2% of the population).

Based on the occupational structure described in Fig. 3, we expect immigrants from low-income countries to offer labor services of a type that primarily is a substitute for low-class native workers and a complement for high-class native workers, whereas immigrants from high-income countries offer services of a type that is a substitute for high-class and a complement for low-class native workers. The distinction between immigrants from high-income and low-income countries is also clear from the perspective of economic incentives for migration resulting in a low-pay job in Norway. While immigrants from high-income countries face a similar wage level in Norway as in their home country, immigrants from the low-income countries can obtain much higher earnings in Norway. For example, Bratsberg et al. (2020) show that average hourly wages in Norway exceeded those in Poland and Lithuania by factors of 4 and 6, respectively, in 2010.

4.4 Exposure to immigration

We measure the degree of exposure to immigration as the immigrant adult (age 25–55) population shares (from high-income and low-income countries,

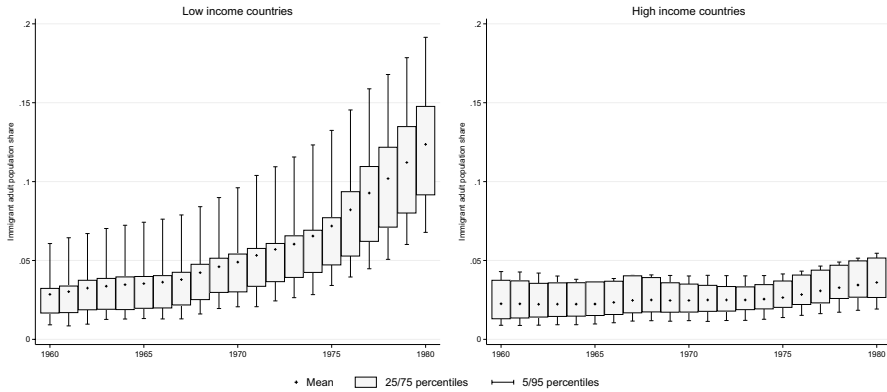


Fig. 4 Longitudinal and cross section variation in exposure to immigration. By birth cohort. Note: The figures show, for each birth cohort, selected statistics describing the variation in immigrant adult (ages 25–55) population shares by age 32 across commuting zones. The reported statistics are percentiles and means in the respective distributions of all individuals

respectively) in each offspring's childhood commuting zone by age 32. Given that the first birth cohort in our dataset is born in 1960 and that residential information for immigrants is available from 1992, age 32 is the lowest age at which we can measure the immigrant shares precisely for all the cohorts. However, as the immigration shares are stock variables, with moderate variation from year to year, these shares will to some extent pick up the overall exposure to immigrant labor market competition through adolescence and young adulthood. For more recent cohorts, we also have data for immigrant exposure earlier than at age 32, and these data show that there is a very high correlation between exposure at different ages. For example, for birth cohorts born after 1969, the correlation coefficients between immigrant exposure at age 22 and age 32 are 0.91 for immigration from both low-income and high-income countries.

Figure 4 illustrates that there has been considerable variation in exposure to immigrant population shares, both over the longitudinal and the cross-sectional dimensions, particularly for immigration from low-income countries. Mean exposure to immigration from low-income countries has varied from approximately 3–4% for the cohorts born in the early 1960s to 13% for the 1980 cohort, whereas exposure to immigration from high-income countries has remained fairly stable around 3%, yet with a slight increase for cohorts born after the mid-1970s.

5 Econometric model and identification strategy

Before we describe our empirical approach in detail, it is useful to take a closer look at the different sources of variation in immigrant exposure that potentially can be used to identify the causal effects on native outcomes. Figure 5 illustrates this by showing immigration shares in each of the 46 different commuting zones in the beginning and at the end of our observation period, with and

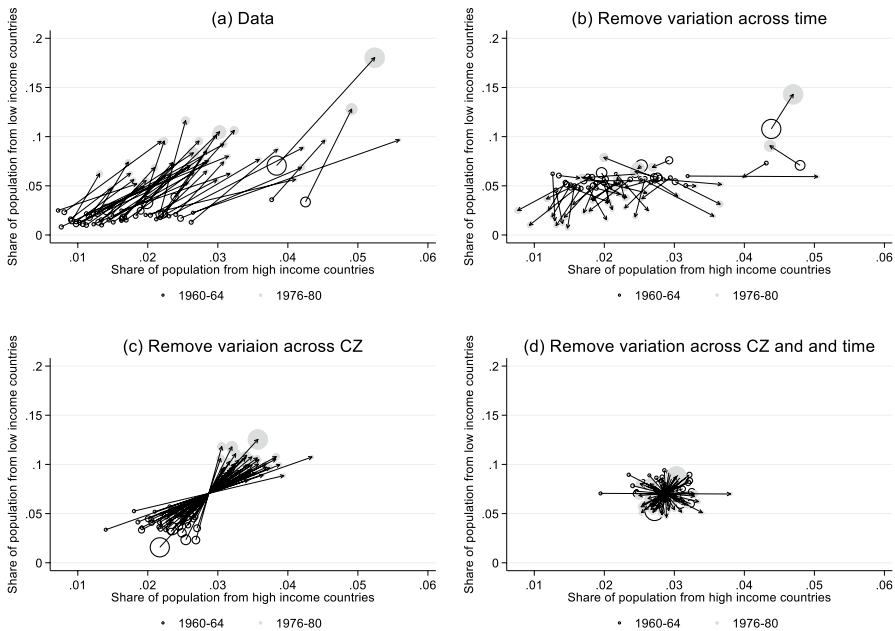


Fig. 5 Exposure to immigration from low- and high-income countries by birth cohort and commuting zone. Note: The black circles show a cross-plot of low-income and high-income immigration shares relevant for the first five birth cohorts across the 46 different commuting zones, whereas the grey circles show the same plot for the last five cohorts. The arrows bind the two observations from each commuting zone together. The sizes of the circles are proportional to the sizes of the respective commuting zones. The data points are scaled such that the average immigrant exposure is the same in all panels

without particular sources of variation removed. The sizes of the data-points are proportional to the population sizes of the respective commuting zones in each period. Panel (a) first illustrates the total variation in the two immigrant shares. Arrows pointing upwards indicate a rise in immigration from low-income countries, whereas arrows moving to the right indicate higher immigration from high-income countries. The position of the line segments illustrates the cross-sectional variation. It is clear that both immigrant shares have risen in virtually all commuting zones, yet with a considerable variation in magnitudes as well as composition. In panel (b), we have removed the common time trend from the data, such that the arrows now indicate the extent to which the changes in the immigrant shares in each commuting zone have been larger or smaller than the national changes. In panel (c), we have instead removed the cross-sectional variation, such that the arrows again indicate the actual time-change in each commuting zone, but the arrows' positions are aligned such that average exposure over the two time periods is the same for all commuting zones. Finally, in panel (d), we have removed both the common time trend and the average exposure in

each commuting zone, such that the only variation left is that of idiosyncratic time trends across commuting zones.

5.1 Specification of a baseline model

A number of factors other than immigration may have contributed to the overall time trends observed in economic mobility; see, e.g., Markussen and Røed (2020). These include changes in technology, trade patterns, childcare institutions, and educational opportunities. In order to identify the causal effects of immigrant exposure, it is thus necessary to control for common (national) trends in economic mobility. In the baseline version of our model, we thus exploit the cross-sectional and idiosyncratic time variation only, i.e., the variation illustrated in Fig. 5, panel (b). In subsequent robustness analyses, we use the same variation within separate regions (defined by geographic proximity or the degree of urbanity), and we exploit the much more narrow idiosyncratic variation illustrated in panel (d).

We model the three outcomes described in the previous section; i.e., prime-age earnings rank and earnings share within cohort-gender-commuting-zone cells and employment, all measured at ages 33–36. To facilitate a flexible analysis allowing us to control for a range of class-fixed effects, we divide the population into five parental classes only; i.e., class quintiles. Let y_{ctzg} be one of the outcomes measured for an individual with class background c (defined as quintile in the parental earnings distribution), of gender g , belonging to commuting zone z , and born in year t . The baseline model can then be described as follows:

$$y_{ctzg} = \alpha_{ctg} + \delta_{tzg} + \sum_{c \neq 3} [(\beta_c^H IMM_{tz}^H + \beta_c^L IMM_{tz}^L) I_c] + \varepsilon_{ctzg}, \quad (1)$$

$c = 1, \dots, 5; t = 1960, \dots, 1980, z = 1, \dots, 46; g = \text{male}, \text{female}$

where α_{ctg} is a class-by-cohort-by-gender fixed effect (represented in the model by 210 dummy variables), δ_{tzg} is a cohort-by-commuting-zone-by-gender fixed effect (represented in the model by 1,932 dummy variables), I_c is a dummy equal to 1 for persons belonging to class (quintile) c (and otherwise 0), and $\{IMM_{tz}^H, IMM_{tz}^L\}$ are the two immigrant population shares (originating in high-income and low-income countries, respectively) in commuting zone z when a person born in year t reached 32 years. This specification implies that any effects of immigration on the outcomes of middle class individuals (quintile 3) will be absorbed by the commuting-zone-by-gender-by-cohort fixed effect. The β coefficient captures the additional effects on the lower and upper classes and thus reflects the impacts on the social gradient in offspring outcomes. If, say, immigration from low-income countries disproportionately hurts low-class natives and benefits high-class natives relative to its impact on the middle classes, β_1^L and β_2^L will be negative and β_4^L and β_5^L will be positive. Conversely, if immigration from high-income countries benefits low-class natives and hurts high-class natives, β_1^H and β_2^H will be positive and β_4^H and β_5^H will be negative.

5.2 Threats against identification

In our baseline specification Eq. (1), the coefficients of interest are identified by the cross-sectional variation in immigrant shares and its changes over time. However, the allocation of immigrants across commuting zones is clearly not a result of random assignment. With a possible exception for quota (resettlement) refugees and their families (Godøy 2017), immigrants have themselves chosen the commuting zone in which they live, and for many of them (particularly labor migrants from Eastern Europe), labor market conditions are likely to have played an important role. We therefore expect immigrants from both high-income and low-income countries to be overrepresented in thriving commuting zones. This is not in itself a threat against identification. The identifying assumption behind our econometric approach is that immigration patterns are exogenous with respect to the *relative* economic performance of natives with different class backgrounds, conditional on the specified fixed effects, i.e., that the residual in the estimating equation is uncorrelated with the immigrant shares. This assumption is also debatable, however. We see three types of threats against identification.

The first comes from the possible existence of a systematic relationship between the overall local labor market conditions and the steepness of the social gradient. The social gradient may be different in thriving economic zones with growing employment and earnings as compared with more economically depressed areas. At the same time, it is likely that immigrants (of both types) are attracted to commuting zones with favorable labor demand conditions and thus high employment and earnings levels. We provide empirical evidence below that, if unaccounted for, this will bias the estimated impacts of both low-income and high-income immigration towards levelling the social gradients. In a robustness analysis, we show that inclusion of controls for class-specific influences of the average *levels* of local employment and earnings makes the estimated effects of immigration on the social gradients larger. The reason why we nevertheless do not incorporate these controls into the baseline model is that we cannot rule out that these averages are causally influenced by immigration, in which case they will absorb parts of the influence of immigration patterns on the social gradients, and thus, in the language of Angrist and Pischke (2009), become “bad controls.”

A second threat against identification is that settlement patterns are partly driven by the relative demand for different skills, such that immigrants from low-income countries are disproportionately attracted to commuting zones with particularly high demand for low-skill labor normally recruited from the lower classes, whereas immigrants from high-income countries are attracted to zones with high demand for high-skill labor. If unaccounted for, this mechanism will induce a positive spurious correlation between immigration from low-income countries and the economic performance of the lower classes and, hence, bias the estimated impacts of low-income immigration towards levelling the social gradient and the impacts of high-income immigration towards steepening it. We examine the empirical relevance of this mechanism in Appendix A by regressing the annual changes in the commuting zones’ two immigrant shares on the lagged relative economic performance of the different classes. Overall,

there is no evidence of a systematic correlation between these variables in the data used to identify the baseline model (Fig. 5, panel (b)). However, when we zoom in on the idiosyncratic time-variation within commuting zones only (Fig. 5, panel (d)), our findings suggest that the share of immigrants from low-income countries increases significantly in response to relatively favorable economic conditions for the bottom class. To the extent that this sorting process contaminates our estimates, it will imply a positive correlation between (unobserved) relative job prospects for lower class people and exposure to low-income country immigration and, hence, bias the effects of low-income country immigrant exposure toward levelling the social gradient.

The third possible threat against identification comes from any local factors that influence both economic mobility and immigration patterns. This could, for example, be related to the industrial and occupational structure of economic regions or to their degree of urbanity. In the robustness analysis, we address the identification challenges arising from such factors by dividing the country into separate entities, based on either geographical proximity or the degree of urbanity, and estimate Eq. (1) with the class-by-cohort-controls specified separately for each entity. The purpose of this exercise is to study the effects of immigration within areas that arguably have been exposed to similar labor market trends and shocks along dimensions other than immigration. Moreover, we investigate how alternative sources of variation in immigrant shares contribute to identification, as reflected in the different panels of Fig. 5.

6 Estimation results

We first present and discuss the estimation results from the baseline model (Eq. (1)), with controls for all national trends and fluctuations in class-specific outcomes. To assess the potential importance of the parental rank algorithm, we estimate this model based on all the four alternative ranking criteria discussed in Section 4.1. We then select a “preferred ranking algorithm” and move on to a battery of robustness analyses where we change the set of identifying information along different dimensions.

6.1 Results from the baseline model

The main results are presented in Table 1, and the point estimates are illustrated graphically in Fig. 6 with 95% confidence intervals.⁷ In line with expectations built on economic theory, our results show that exposure to immigration from low-income countries steepens the social gradients in all three outcomes, whereas exposure to immigration from high-income countries levels them. This pattern is remarkably consistent across the class distribution, regardless of which parental earnings rank algorithm we use to define class background. The estimated effects are significant

⁷ In Appendix B, we show estimated impacts by gender, based on separate regressions. They indicate similar effects for men and women.

Table 1 Main estimation results—effects of immigrant shares at age 32 on native outcomes at age 33–36. By immigrants' origin country and parental earnings quintile

Class background rank criterion:												
Parents' earnings ages 52–58			Parents' earnings ages 33–36			Parents' earnings offspring ages 7–15			Parents' earnings 1980–1992			
1	2	3	4	5	6	7	8	9	10	11	12	
Earnings rank	Earnings share	Employment	Earnings rank	Earnings share	Employment	Earnings rank	Earnings share	Employment	Earnings rank	Earnings share	Employment	
<i>Immigration from low-income countries</i>												
Quintile 1	–26.01*** (5.91)	–0.45*** (0.11)	–29.67*** (5.59)	–0.61*** (0.10)	–0.23*** (0.07)	–26.04*** (5.33)	–0.55*** (0.10)	–0.24*** (0.08)	–32.74*** (4.87)	–0.64*** (0.09)	–0.32*** (0.07)	
Quintile 2	–13.79*** (4.77)	–0.21*** (0.10)	–7.37 (4.95)	–0.16* (0.09)	0.06 (0.06)	–9.67* (5.42)	–0.19* (0.11)	–0.01 (0.07)	–16.68*** (5.21)	–0.33*** (0.10)	–0.07 (0.06)	
Quintile 3	Ref	Ref										
Quintile 4	13.56*** (4.11)	–0.03 (0.05)	2.186 (5.82)	0.07 (0.11)	–0.03 (0.05)	9.46** (4.28)	0.19** (0.08)	–0.03 (0.06)	4.66 (5.03)	0.10 (0.09)	–0.04 (0.05)	
Quintile 5	22.84*** (5.12)	0.67*** (0.12)	8.44 (6.55)	0.29** (0.13)	–0.03 (0.07)	19.28*** (5.17)	0.54*** (0.11)	–0.08 (0.06)	13.00*** (4.88)	0.38*** (0.10)	–0.15*** (0.06)	
<i>Immigration from high-income countries</i>												
Quintile 1	34.64* (18.45)	0.45 (0.35)	18.71 (16.10)	0.24 (0.29)	0.20 (0.22)	16.88 (15.18)	0.33 (0.28)	0.35 (0.23)	37.89*** (13.98)	0.70*** (0.26)	0.53*** (0.20)	
Quintile 2	31.15** (14.29)	0.37 (0.28)	3.72 (14.48)	–0.04 (0.27)	–0.01 (0.18)	9.53 (16.43)	0.03 (0.32)	0.04 (0.20)	21.21 (15.40)	0.34 (0.28)	0.17 (0.18)	
Quintile 3	Ref	Ref										
Quintile 4	–6.16 (13.35)	–0.25 (0.27)	–3.36 (19.41)	0.10 (0.41)	–0.11 (0.20)	–5.31 (12.86)	0.06 (0.26)	0.09 (0.17)	8.66 (15.49)	0.29 (0.29)	0.04 (0.15)	
Quintile 5	–30.77** (15.01)	–0.45 (0.31)	–57.43 (18.67)	–1.13*** (0.37)	–0.33 (0.21)	–30.02* (16.24)	–0.32 (0.31)	–0.08 (0.17)	–21.45 (14.30)	–0.04 (0.29)	–0.08 (0.16)	
R-squared	0.051	0.049	0.027	0.027	0.044	0.042	0.041	0.049	0.044	0.043	0.051	

The number of observations is 1,116,827. All regressions include 210 class-by-cohort-by-gender dummies and 1932 commuting-zone-by-cohort-by-gender dummies. Earnings include all gross labor-related earnings (including self-employment earnings) obtained over the age 33–36 period. Earnings rank is defined within each of 1932 cohort-commuting-zone-gender cells and measured in percentiles. Earning share is defined as own labor earnings divided by the cell average. Employment is equal to 1 when average annual earnings exceed 2 Basic amounts in the Norwegian pension system (approximately one third of average full-time-full-year earnings), and otherwise zero. Immigrant shares are defined as the fractions of the adult (ages 25–55) population in the commuting zone that have emigrated from low-income and high-income countries, respectively. The regression model is described in Eq. (1). Standard errors (clustered at labor-market-region-by-cohort-cells) are reported in parentheses. The division into regions follows Bhuller (2009) and the five regions are Øst-Norge (Capital region), Sør-Norge (Southern region), Vest-Norge (Western region), Midt-Norge (Central region), and Nord-Norge (Northern Region). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ indicates statistical significance at the 10/5/1% level

from both a statistical and a substantive viewpoint. For example, our estimates indicate that relative to the (unidentified) impact on middle class natives (the third quintile), a 10 percentage point increase in the share of immigrants from low-income countries (i.e., the actual increase observed in our data period) reduces the expected rank outcome for a low-class (first quintile) native by somewhere between 2.6 and 3.3 percentiles, reduces expected labor earnings relative to the cohort average by between 4.5 and 6.4%, and reduces the expected employment rate relative to the third quintile by 2.3–3.6 percentage points. For a top-class native, on the other hand, it raises the expected rank outcome by 0.8–2.3 percentiles and raises the expected labor earnings relative to the cohort average by 2.9–6.7%, but has no, or even negative, influence on relative employment. The asymmetry with respect to the influence on employment suggests that while increased supply of low-skill labor primarily affects low-class natives by pushing some of them out of the labor market, it affects high-class natives by raising their expected earnings. This pattern is consistent with downward rigidity in native low-skill wages. We show in Section 7 that the possibly negative effect on relative employment for the top class can be explained by a disproportionately large positive effect on educational attainment in this group, implying that a larger fraction is in education when we measure earnings outcomes at age 33–36.⁸

The estimated impacts of immigration from high-income countries are more or less mirror images of the impacts identified for immigration from low-income countries. Relative to the middle classes, high-income immigration benefits low-class natives and hurts high-class natives. The magnitudes of the effects are even larger than for low-income immigration. One possible interpretation of that finding is that immigrants from high-income countries on average participate more actively in the Norwegian labor market and therefore exert a larger overall influence on native labor market outcomes; confer Fig. 1. However, the statistical uncertainty is also much larger, and it is important to keep in mind that the changes over time in the immigrant share from high-income countries are almost negligible compared with the changes in the immigrant share from low-income countries, i.e., 1 percentage point increase over the data period instead of 10, confer Fig. 4.

In Appendix C, we present results from models where class background is based on the educational attainment of parents and on the earning rank of grandfathers, respectively. In the latter exercise, more than half of the estimation sample is lost (due to missing information about grandfathers). Yet the results based on both these alternative rankings are remarkably similar to those presented in Table 1, particularly for the bottom and top quintiles. Hence, although we cannot rule out that also

⁸ We have also estimated models where we have split immigration from low-income countries into immigration from less-developed countries (LDC) and Eastern Europe (EE) and estimated separate effects for all three origin-country groups. What happens then is that the estimates for LDC immigration become similar to those reported in Table 1 for low-income countries, whereas all estimates for EE immigration become statistically insignificant, with large standard errors. Our reading of this result is that the identifying variation in immigration from EE is too small to facilitate a meaningful separate analysis. The estimated effects of immigration from high-income countries are (as expected) largely unaffected.

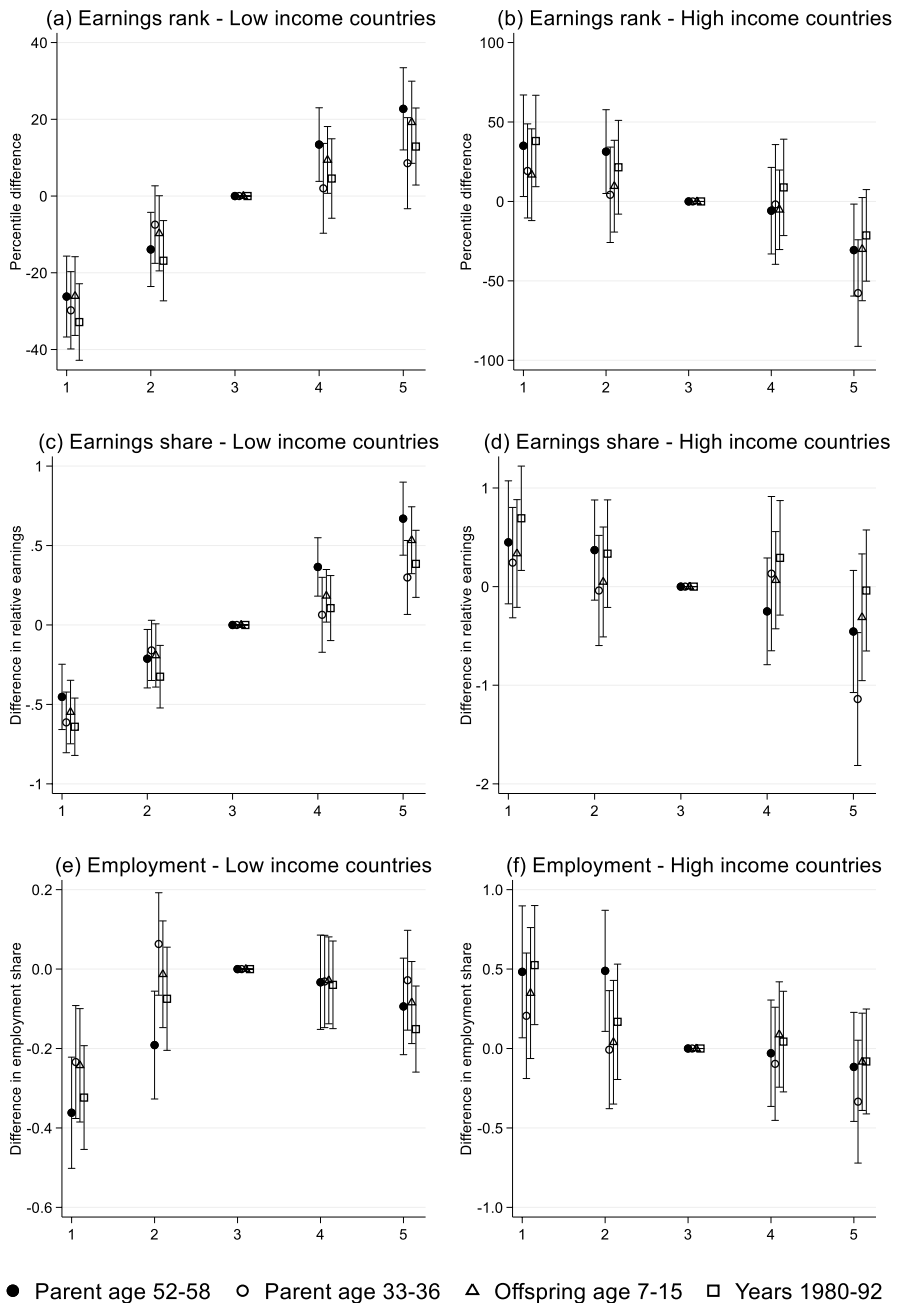


Fig. 6 Main estimation results for effects of immigrant shares on relative native outcomes by class background based on alternative ranking criteria for parental earnings. Note: Reference is class 3 (offspring with middle class background). Point estimates are shown with 95% confidence intervals, with standard errors clustered at labor-market-region-by-cohort-cells. See also notes to Table 1

earnings in the parent generation have been affected by immigration, we can safely reject that this is the primary mechanism behind our baseline results.

6.2 Robustness

Based on previous evidence reported in Markussen and Røed (2020), we consider the class background definition based on parental age-52–58-earnings to provide the best and most stable indicator for the parents' permanent income and the offspring's socioeconomic class background. This is also confirmed by the results in Table 1, as the model based on ages 52–58 ranking yields a considerably higher R-square than the other three rank algorithms. However, as discussed in Section 4.1, the ranking of parents at this high age involves a potential simultaneity problem in our model, as it implies that the assignment of class background may have been influenced by the same factors that affect offspring outcomes, including immigrant shares. In our robustness analysis, we therefore continue with a parental ranking algorithm that does not involve data from the outcome period. Of the three alternatives reported in Table 1, we choose the one with the highest explanatory power, i.e., the model with parents ranked based on their 1980–1992 earnings. This implies that the earnings rank will have a stronger association with lifetime earnings and thus presumably be a better indicator for socioeconomic class background, for earlier than for later cohorts. However, as we are going to control non-parametrically for class-by-cohort-by-gender throughout the analysis, this is unlikely to bias our results.⁹

Figure 7 presents results for four different models, distinguished by their source of identifying information. The first set of coefficient estimates—indicated by black filled circles—repeats the baseline results from Fig. 5, whereas the others represent estimates from various robustness exercises (denoted R1–R3). The estimates indicated by open circles (R1) are built on a model where we have divided the country into five separate geographical regions, each consisting of commuting zones characterized by a more homogenous industry composition and labor market development. We have then replaced the class-by-cohort-by-gender fixed effects in Eq. (1) with class-by-cohort-by-region-by-gender fixed effects. The division into regions follows Bhuller (2009), and the five regions are Øst-Norge (Capital region), Sør-Norge (Southern region), Vest-Norge (Western region), Midt-Norge (Central region), and Nord-Norge (Northern Region). The estimates indicated by open triangles (R2) come from a similar model, but with the commuting zones divided into three separate entities based on the degree of urbanity instead of geographical proximity (larger city zones, smaller city zones, and rural zones). For both these divisions of the country, we have also estimated completely separate models for each entity

⁹ A complete robustness analysis based on the age 52–58 ranking criterion is available in a previous working paper version of this article (Hoen et al. 2018). Note that the different raking algorithms produce rank orders that are highly (but far from perfectly) correlated. The highest correlation coefficient (0.79) is obtained for the 1980–1992 and the offspring age 7–15 criteria, and the lowest correlation (0.45) is obtained for the age 52–58 and the age 33–36 criteria. Looking at the data in which the earnings of grandfathers are observable, we note that the intergenerational rank correlation (between grandfathers and parents) is 0.22 when we use the age 52–58 measurement for parents (and approximately 0.20 for the other parental rank criteria).

based on Eq. (1), and the results from these exercises are provided in Appendix D. Although point estimates vary, and confidence intervals in some cases become much larger, inspection of Appendix Figures 10 a and b suggests that the qualitative findings regarding the influence of migration on economic mobility are similar irrespective of region and urbanity. Finally, the estimates indicated by x-es in Fig. 7 (R3) come from a model where we have included both class-by-commuting-zone-by-gender fixed effects and class-by-cohort-by-gender fixed effects, such that the only variation left is the small idiosyncratic variation illustrated in Fig. 5, panel (d).

The takeaway from Fig. 7 is that the main results are robust with respect to the different sources of identifying variation, with a possible exception for the last model (R3) which is estimated with very wide confidence intervals. It is worth noting that there is some evidence of reverse causality within this narrow information set, in particular such that immigration from low-income countries respond positively to favorable economic conditions for the lower classes; see Appendix A. As noted in Section 5.2, the economic outcomes of the lower classes are likely to be more affected by overall labor market conditions than the outcomes of the higher classes.

Figure 8 presents results for the models shown in Fig. 7 but this time extended with class-specific controls for overall local economic conditions, as reflected in the male/female average earnings and employment levels in the commuting zone. This is done in the following fashion: for each cohort, commuting zone, and gender, we compute the average earnings level and employment rate and then interact them with a class scalar variable (running from 1 to 5) and a vector of cohort dummy variables. The resultant variables are then added as controls to Eq. (1), which with the baseline identifying set can be written:

$$y_{xtzg} = \alpha_{ctg} + \delta_{t zg} + \sum_{c \neq 3} [(\beta_c^H IMM_{tzc}^H + \beta_c^L IMM_{tzc}^L) I_c] + \lambda_t^{EMP} EMP_{t zg} \times Class + \lambda_t^{EARN} EARN_{t zg} \times Class + \varepsilon_{ctzg}, \quad (2)$$

where *Class* is a scalar variable describing the class quintile (1–5) and $\{EMP_{gzt}, EARN_{gzt}\}$ are average employment rate and average earnings level during the outcome period, for gender *g* in commuting zone *z* and cohort *t*.

As expected, the estimated influence of immigration from low-income countries on the social gradients in economic outcomes become larger when we include these additional class-specific controls, reflecting that high employment and earnings tend to induce both more immigration and improved relative outcomes for the lower classes. It is also notable that the class-specific controls for aggregate labor market conditions make the estimates based on different sources of identification more similar.

7 Mechanisms

The causal relationship between the immigrant shares in the commuting zone of young adulthood and intergenerational economic mobility may arise from a number of mechanisms, related to, e.g., educational choice, migration across commuting zones, and direct competition in the labor market. As our data do not allow us to identify, in a consistent fashion, the geographical distribution of immigrant

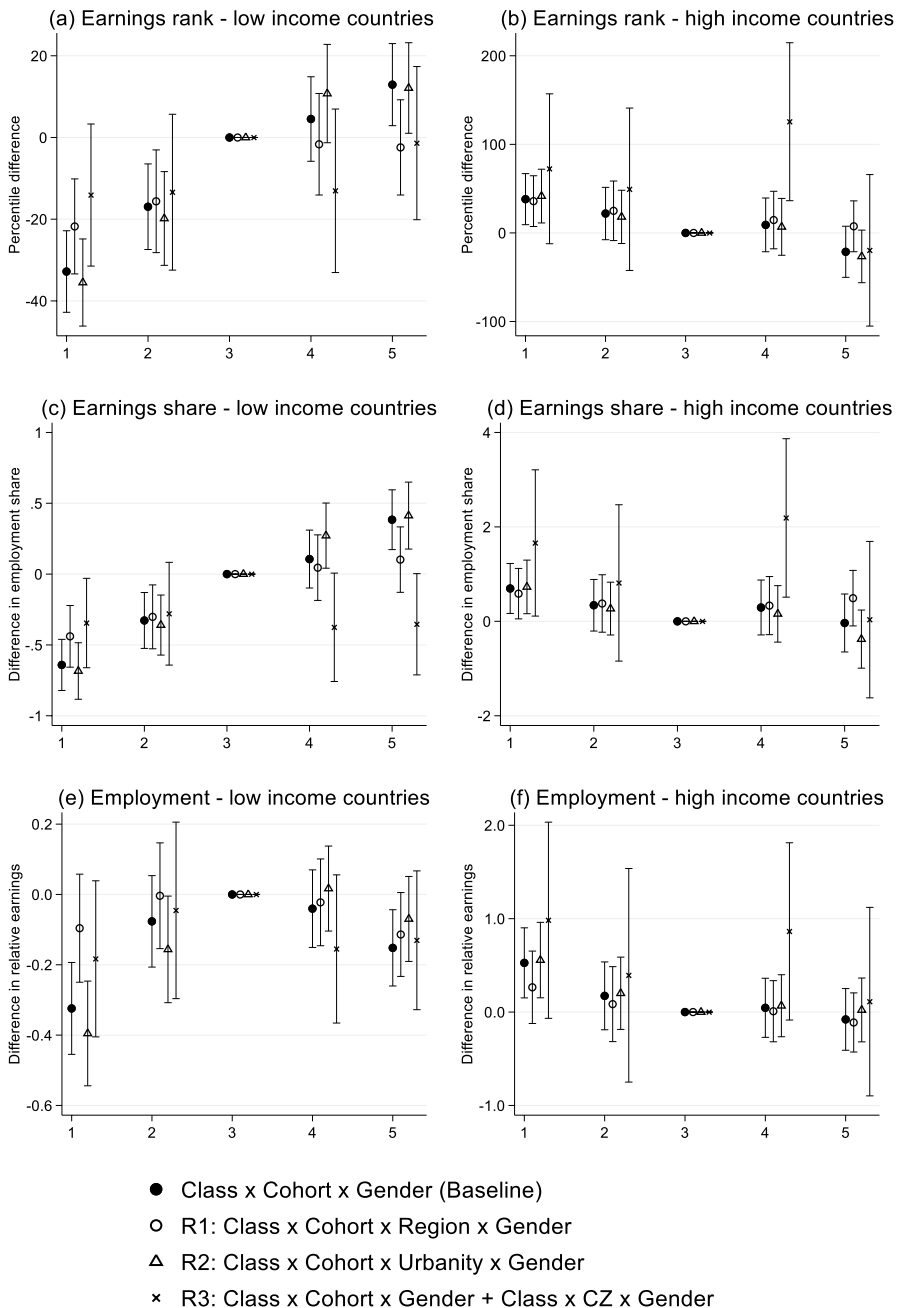


Fig. 7 Estimated effects of immigrant shares at age 32 on relative native outcomes by class background, alternative sources of identification, without class-specific controls for local economic conditions. Note: Reference is class 3 (offspring with middle class background). Each set of coefficient estimates is based on a version of the baseline model (Eq. (1)). The type of class-specific fixed effects is reported in the legend. The graphs show the point estimates with 95% confidence intervals based on standard errors clustered at labor-market-region-by-cohort-cells

exposure for ages lower than 32, we cannot examine directly the influence of immigration at different stages of the lifecycle based on the complete dataset. However, given the cumulative nature of the commuting zones' immigrant shares, we can think of their values observed by age 32 as proxies for immigrant exposure over a longer time period leading up to this age, and examine how they have influenced earlier decisions and outcomes. We can also, obviously with some measurement error, infer immigrant shares at younger ages by assuming that immigrants observed in a particular commuting zone in 1992 have lived in that zone since the year of entry.

As immigration alters the skill composition of the labor force, it is also likely to affect the returns to education. While we expect immigration from low-income countries to increase the returns to education (because it raises the relative supply of low-skill workers), we expect immigration from high-income countries to reduce the returns (because it raises the relative supply of high-skill workers). Since there is a strong positive association between educational attainment and parental earnings rank (Markussen and Røed 2020), this may be a central mechanism behind the effects of immigration on economic mobility identified in this paper. However, by affecting the returns to education, immigration is also likely to affect the natives' choice of educational attainment. In particular, we may expect that low-skilled natives respond to the increased competition in the low-skill segment of the labor market by upgrading their skills; see, e.g., Foged and Peri (2015) for empirical evidence pointing in that direction. This may again offset the relative decline in economic outcomes for the lower classes. However, although offspring of parents with low earnings rank does tend to have low education, it is not necessarily the case that their choices of attainment are particularly responsive with respect to changes in the returns. There is ample empirical evidence indicating that family support and encouragement are essential inputs to educational achievement, and that such support and encouragement are provided less by lower class families (Guryan et al. 2008; Kalil et al. 2012; Mayer et al. 2015). This may imply that the marginal (effort) cost of obtaining additional education in response to, say, increased returns is larger for low-class than for middle- or high-class offspring, despite their lower average attainment. If so, it is possible that increased returns to education steepens the social gradient in educational attainment, rather than leveling it.

To examine the influence of immigration on the social gradient in educational attainment, we use years of non-compulsory education attained by age 32 as an alternative outcome in the baseline regression model Eq. (1). As educational choices are typically made in the early 20 s or before, it is problematic to use immigrant shares observed by age 32 as explanatory variables in this model, despite their cumulative nature. Hence, we also estimate a model where we use shares by age 22, although, for cohorts born before 1970, these are inferred from the immigrant population observed in 1992. The results are presented in Table 2, Columns 1 and 2. They show that immigration from low-income countries indeed steepens the social gradient in educational attainment, and this effect appears to be stronger when we focus in immigration shares by age 22. Hence, instead of offsetting the influences of immigration on

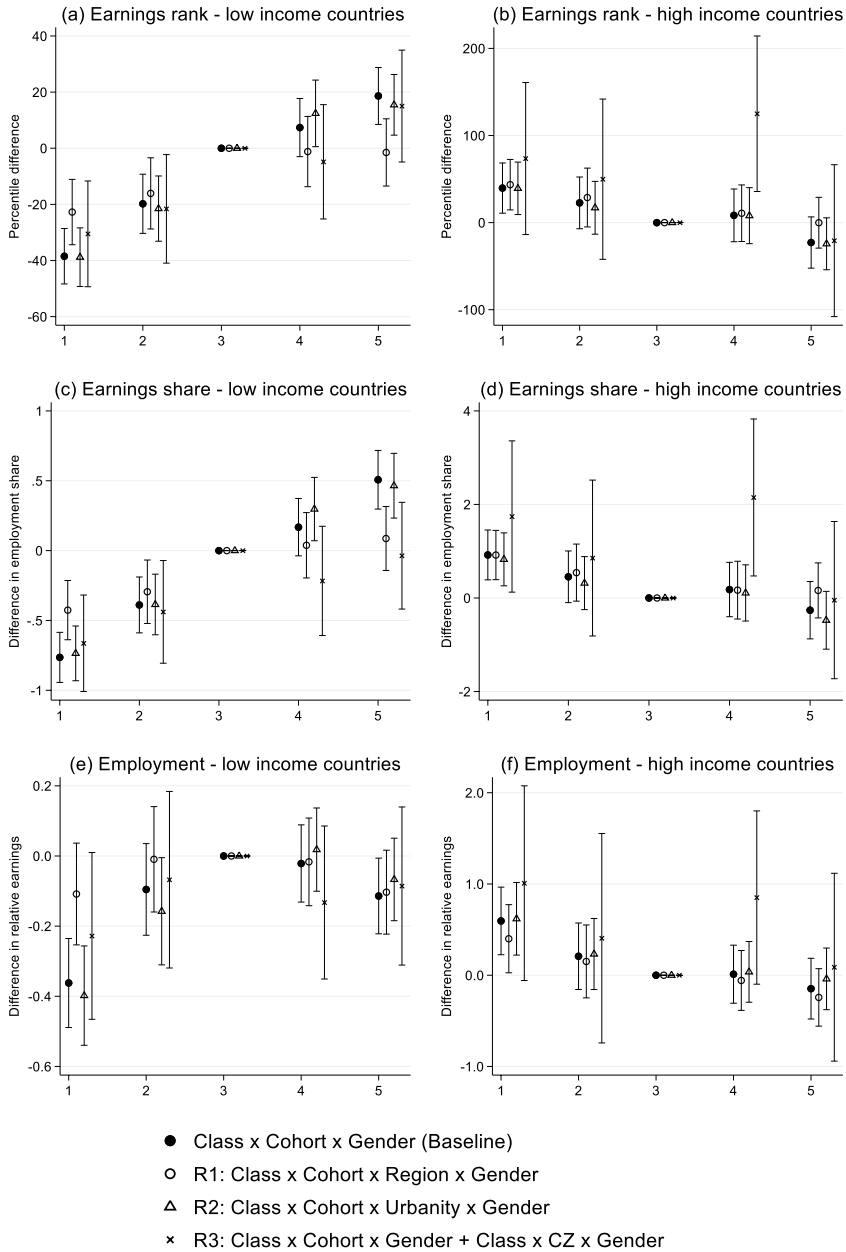


Fig. 8 Estimated effects of immigrant shares at age 32 on relative native outcomes by class background, alternative sources of identification, with class-specific controls for local economic conditions. Note: Reference is class 3 (offspring with middle class background). Each set of coefficient estimates is based on a version of the baseline model extended with class-specific controls for local economic conditions (Eq. (2)). The type of class-specific fixed effects are reported in the legend. The graphs show the point estimates with 95% confidence intervals based on standard errors clustered at labor-market-region-by-cohort-cells

economic mobility, it appears that endogenous educational choices tend to reinforce them.¹⁰ For immigration from high-income countries, the results are less clear.

Throughout the analysis, we have used the immigrant shares observed in the commuting zone of adolescence as the key explanatory variables, both to ensure exogeneity and to capture potential effects through the whole process of educational choice and labor market entry. This implies that many of the examined individuals (26%) have migrated to another commuting zone at the time of outcome measurement at ages 33–36. To the extent that the immigrant shares affect job opportunities within a given commuting zone, migration across commuting zones is clearly a potential offsetting mechanism. If so, we expect immigration from low-income countries to trigger relatively more outmigration within the lower classes and immigration from high-income countries to trigger relatively more outmigration within the higher classes. To examine this hypothesis empirically, we have used as an additional outcome a dichotomous variable indicating that a person has migrated to another commuting zone by age 32. The result is provided in Table 2, Column 3. As expected, we find that immigration from low-income countries does trigger relatively more outmigration for the lower than the higher classes. However, also immigration from high-income countries appears to reduce the relative migration propensity in the top classes. This suggests that other forces are at play here.¹¹

It appears plausible that geographically mobile natives may be less affected by immigration into their childhood commuting zone; hence, although we have shown that the decision to move is endogenous with respect to immigration patterns, it may be of some interest to examine immigration effects separately for movers and stayers. In Appendix E we do this and show that the effects indeed appear to be somewhat larger for stayers than for movers.

Finally, while we have focused on labor market performance by ages 33–36 as indicators for labor market success in this paper, we can obtain more insight to the underlying mechanisms by examining the ultimate outcome of the school-to-work transition in terms of employment/non-employment. To do this, we use as an outcome a dichotomous variable indicating that a person has *never* been employed before age 32. The results show that an important part of the effects of immigration on economic mobility arises through its influence on the probability of ever obtaining a job; see Table 2, Column 4.

¹⁰ Higher educational attainment among upper-class offspring also partly explains the apparent negative employment effect of low-income country immigration on employment in the top class quintile. In an auxiliary regression (not reported), we use the combined outcome of not being employed *and* still being in education at the age of 33 years, and we then obtain a borderline statistically significant coefficient for the interaction of low-income country immigration share and belonging to the upper quintile (point estimate 0.05; standard error 0.3).

¹¹ A possible explanation is that higher immigration from high-income countries pushes property prices upwards, such that people with less financial resources (overrepresented in the lower and middle classes) leave the commuting zone, which again implies that the *relative* migration propensity for the upper classes declines. It is however beyond the scope of our paper to investigate this further.

Table 2 Estimation results—effects of immigrant shares on native outcomes by age 32

	1	2	3	4
	Years of non-compulsory education	Years of non-compulsory education	Migrated to another CZ	Never employed
<i>Immigration from low-income countries by parental earnings rank</i>				
Quintile 1	− 3.40*** (0.41)	− 3.78*** (0.63)	0.36*** (0.09)	0.09*** (0.03)
Quintile 2	− 1.11*** (0.43)	− 1.07 (0.66)	0.24*** (0.07)	− 0.00 (0.02)
Quintile 3	Ref	Ref	Ref	Ref
Quintile 4	2.43*** (0.47)	3.86*** (0.72)	− 0.32*** (0.08)	− 0.00 (0.02)
Quintile 5	3.45*** (0.70)	6.02*** (1.07)	− 1.38*** (0.09)	0.01 (0.02)
<i>Immigration from high-income countries by parental earnings rank</i>				
Quintile 1	0.95 (1.28)	− 2.20* (1.28)	0.23 (0.22)	− 0.36*** (0.10)
Quintile 2	− 1.51 (1.14)	− 3.02*** (1.15)	0.09 (0.20)	− 0.05*** (0.06)
Quintile 3	Ref	Ref	Ref	Ref
Quintile 4	− 1.64 (1.13)	− 1.57 (1.28)	− 0.69*** (0.21)	0.09 (0.06)
Quintile 5	0.16 (1.54)	− 0.50 (1.90)	− 1.95*** (0.27)	0.16*** (0.05)
Immigration shares measured by age	32	22	32	32
R-squared	0.136	0.0958	0.096	0.015
Mean outcome (standard deviation)	3.625 (2.630)	3.625 (2.630)	0.261	0.039

The numbers of observations are 1,116,827 in all the regressions. All regressions include 210 class-by-cohort-by-gender fixed effects and 1932 commuting-zone-by-cohort-by-gender fixed effects. Years of non-compulsory education are based on the stipulated number of years required to obtain the highest completed education. Migration to another commuting zone is defined as living in another commuting zone by age 33 than by age 15/16. Never employed is defined as never having had annual labor earnings exceeding 2 basic amounts (see note to Table 1). The regression model is described in Eq. (1). Standard errors (clustered at labor-market-region-by-cohort-cells) are reported in parentheses. *, **, *** indicates statistical significance at the 10/5/1% level

8 Concluding remarks

One of the starting points of this paper was the observation that the social gradients in natives' economic outcomes have become steeper over time, and in particular that the intergenerational mobility out of the lower economic classes has declined. We have examined whether this development is attributable to skill-biased changes in labor supply caused by the rise in the number of immigrants from less-developed countries and Eastern Europe. As these immigrants have been disproportionately recruited to jobs typically held by lower-class natives, the hypothesis is that they have crowded out lower-class natives in the labor market.

Our findings suggest that immigration patterns have indeed been a major force behind changes in the social gradients in adult economic outcomes for natives born in Norway between 1960 and 1980. While immigration from low-income countries has steepened the social gradients in native employment and earnings outcomes, immigration from high-income countries has levelled them. And since immigration from low-income countries has been much larger than immigration from high-income countries, the net effect of the actual immigration to Norway over the past decades has been to reinforce the influence of family background on economic outcomes and thus to reduce economic mobility. Taken at face value, our estimation results imply that the observed 10% point increase in the low-income-country immigrant population share between 1992 and 2012, together with the 1 percentage point increase in the high-income-country immigration share, explains a 2.3 percentile drop in the average earnings rank outcome for the bottom class quintile for the cohorts examined in this paper (born 1960–1980), roughly corresponding to the observed decline. Hence, according to this measure, immigration patterns can explain the entire observed drop in economic mobility for the bottom class quintile.¹²

Given the identified effects of immigration on the economic mobility of natives, it appears plausible that the inflow of new immigrants also to some extent affect the relative outcomes of previous immigrants. It has been beyond the scope of our paper to investigate this directly, but existing empirical evidence from Norway indicates that labor market outcomes of existing immigrants are highly responsive to new immigration (Bratsberg et al. 2014) and that the wage effects for all groups of immigrant workers are similar to those for low- and semi-skilled natives (Bratsberg and Raaum 2012).

Whereas our empirical analysis cannot say anything about the aggregate effects of recent immigration patterns on native outcomes, it strongly suggests that immigration has skewed relative economic success away from the lower and toward the higher economic classes. Hence, it rationalizes the apparent polarization of sentiments regarding immigrants and immigration policies. To the extent that people's political opinions are influenced by own economic interests, it may explain why the higher classes tend to favor a more liberal immigration policy than the lower classes.

¹² Here, we have used the model based on parental age 52–58 earnings for illustration, since this provides the best foundation for comparisons across birth cohorts.

Our findings imply that high immigration, *ceteris paribus*, may make it more challenging to achieve goals of equality of opportunities and high economic mobility among natives. This does not necessarily imply a case for more restrictive immigration policies. Our analysis says nothing about overall welfare effects, neither for the native population nor for the combined population of natives and immigrants. However, if equality of opportunities is considered an important policy aim in its own right, our findings suggest that high immigration from low-income countries calls for other policies aimed at improving economic outcomes for natives growing up in economically disadvantaged families and thus securing high mobility.

Appendix A: The allocation of immigrants across commuting zones

To see how migration patterns have correlated with already existing geographical differences in economic mobility, we model the changes in each of the two immigrant shares from one year to another as functions of the initial class structure of economic outcomes. We estimate regression models with the following structure:

$$IMM_{zt}^R - IMM_{zt-1}^R = \sigma_t^R + \sum_{c \neq 3} (\phi_c^R (\bar{y}_{czt-1} - \bar{y}_{3zt-1})) + \zeta_{zt}^R, \quad (3)$$

$$R = H, L, \quad z = 1, \dots, 46, \quad t = 1993, \dots, 2012,$$

where $IMM_{zt}^R - IMM_{zt-1}^R$ is the change in the share of immigrants originating in country type R from year $t-1$ to t in commuting zone z , \bar{y}_{czt-1} is a measure of an average class-specific outcome (earnings rank, earnings share, or employment rate) in year $t-1$, and σ_t^R is a year-fixed effect. In an alternative specification, we add 46 commuting-zone fixed effects into the model.

We estimate separate models for immigration from high-income and low-income countries, with three different class-specific measures of economic success included as explanatory variables. The estimation results are presented in Table 3. Consider first the estimated influence of class-specific rank and earnings share outcomes in the table's panels A and B. Without the inclusion of commuting-zone fixed effects (columns 1 and 3), there is no systematic pattern between the class structure of economic outcomes and immigration patterns. However, once we include commuting-zone fixed effects into the model (columns 2 and 4), we find that immigration from low-income countries do respond positively to relatively favorable labor market outcomes for low-class natives. Together, these findings suggest that the overall cross-sectional distribution of annual net immigration does not correlate strongly with the existing intergenerational mobility patterns, but that the variation within commuting zones to some extent is affected by fluctuations in relative labor demand. This sorting mechanism is likely to attenuate the estimated effects of immigration from low-income countries on economic mobility, particularly in models where the idiosyncratic variation within commuting zones is used to identify them.

Table. 3 Estimated effects of class-specific economic outcomes among natives on the annual changes in local immigrant shares

	Change in migrant share from low-income countries		Change in migrant share from high-income countries	
	1	2	3	4
	Without CZ fixed effects	With CZ fixed effects	Without CZ fixed effects	With CZ fixed effects
<i>A. Lagged average rank outcome by class (divided by 100)</i>				
Quintile 1	-0.0019(0.0081)	0.0103*** (0.0029)	0.0002(0.0016)	-0.0013(0.0010)
Quintile 2	-0.0072(0.0049)	-0.0051(0.0032)	0.0008(0.0009)	0.0006(0.0011)
Quintile 4	0.0079(0.0068)	-0.0015(0.0030)	-0.0006(0.0016)	-0.0002(0.0016)
Quintile 5	0.0005(0.0051)	-0.0024(0.0031)	-0.0006(0.0016)	0.0008(0.0018)
R squared	0.7090	0.8043	0.5005	0.5474
# Observations	920	920	920	920
<i>B. Lagged earnings share by class</i>				
Quintile 1	-0.0014(0.0039)	0.0057*** (0.0019)	0.0003(0.0008)	-0.0006(0.0005)
Quintile 2	-0.0048(0.0029)	0.0022(0.0018)	0.0007(0.0005)	0.0006(0.0006)
Quintile 4	0.0038(0.0031)	-0.0013(0.0015)	-0.0004(0.0007)	-0.0003(0.0007)
Quintile 5	0.0035(0.0032)	0.0007(0.0013)	-0.0003(0.0007)	0.0005(0.0008)
R squared	0.7123	0.8042	0.5013	0.5478
# Observations	920	920	920	920
<i>C. Lagged employment rate by class</i>				
Quintile 1	0.0018(0.0031)	0.0076*** (0.0027)	0.0015(0.0009)	0.0002(0.0012)
Quintile 2	0.0005(0.0027)	0.0011(0.0026)	0.0024(0.0010)	0.0013(0.0014)
Quintile 4	0.0030(0.0047)	0.0008(0.0034)	0.0000(0.0009)	0.0004(0.0013)
Quintile 5	-0.0111** (0.0052)	0.0003(0.0027)	-0.0002(0.0018)	0.0012(0.0018)
R squared	0.7097	0.8045	0.5116	0.5482
# Observations	920	920	920	920

Note: All models include year-fixed effects. The results in columns 1 and 3 are based on Equation (A1), whereas the results in columns 2 and 4 are based on Equation (A1) augmented with 46 commuting-zone-fixed effects. The model in panel C also includes the employment rate for Quintile 3. Standard errors (clustered on commuting zone) are reported in parentheses. ***/**/* indicates statistical significance at the 10/5/1 % level

Moving on to the impact of class-specific employment rates in panel C, we find the same type of immigrant sorting as for the rank and earnings share outcomes within commuting zones; see columns 2 and 4. For the model without commuting-zone fixed effects (columns 1 and 3), the picture is a bit noisy. While the estimates indicate that immigration from low-income countries respond negatively to

improved relative employment prospects for the top class, as expected given a sorting process driven by labor demand, they indicate that immigration from high-income countries respond positively to improved prospects for the bottom class. This is hard to reconcile with a demand-driven explanation. Yet, given the large number of reported coefficients in Table 3 (48 in total), it is probable that some become statistically significant by coincidence. Viewed as a whole, there is in the model without commuting-zone fixed effects no systematic sorting pattern in any particular direction, whereas the sorting within commuting zones induces a positive correlation between immigration from low-income countries and economic mobility.

Appendix B: Baseline results estimated separately for men and women

In this [appendix](#), we present separate baseline results for men and women, based on the four different parental earnings rank criteria. Given the gender differences in occupational structure as well as the different trends in male and female educational attainment and labor force participation rates, one could perhaps expect gender differences in the impacts of immigration on economic outcomes also.¹³ However, the results shown in Figs. 9a and b indicate that the mechanisms examined in this paper are similar for men and women.

Appendix C: Alternative rankings based on parents' educational attainment and grandfathers' earnings

To rule out any risk that class background has been causally affected by immigration, we have employed two alternative ranking criteria based on parents' educational attainment and on the earnings of grandfathers, respectively.¹⁴

The education-based ranking is derived from parents' observed educational attainment in 1990. We first add up years of non-compulsory education for the two parents and then rank within offspring's birth-cohort-by-gender-by-commuting-zone cells (as before), such that we again end up with class background quintiles. In cases where there are "ties" (equal parental education on both sides of a threshold), we randomly assign offspring to the two relevant quintiles to ensure exactly 20% in each bin, thereby maximizing comparability with the results in Table 1.

¹³ In our data, the employment age 33–36 employment rate for women rose from 71 to 83% from the 1960 to 1980 birth cohort, whereas it declined from 90 to 88% for men.

¹⁴ Very few women were in paid work in these generations; hence, we focus exclusively on earnings obtained by grandfathers in this exercise.

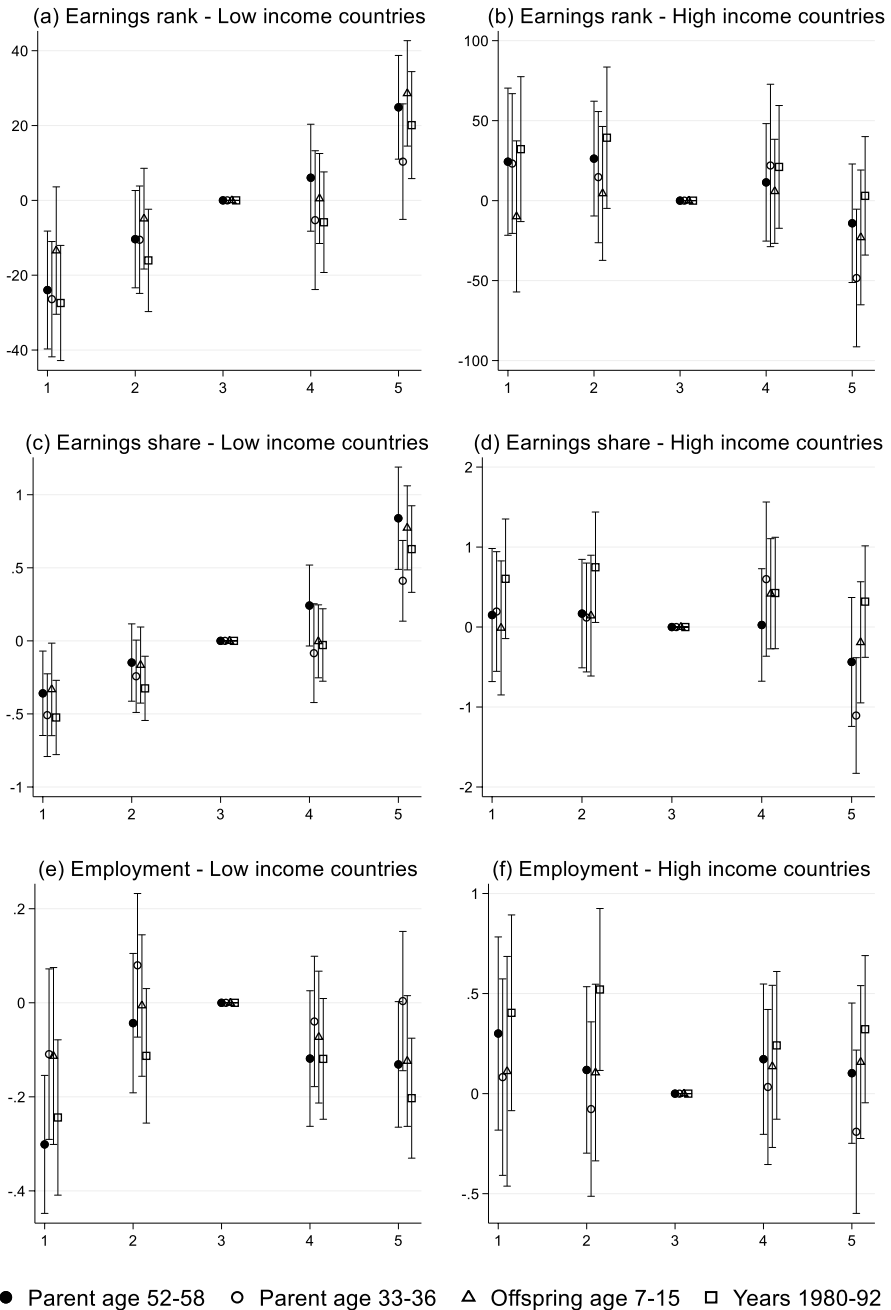
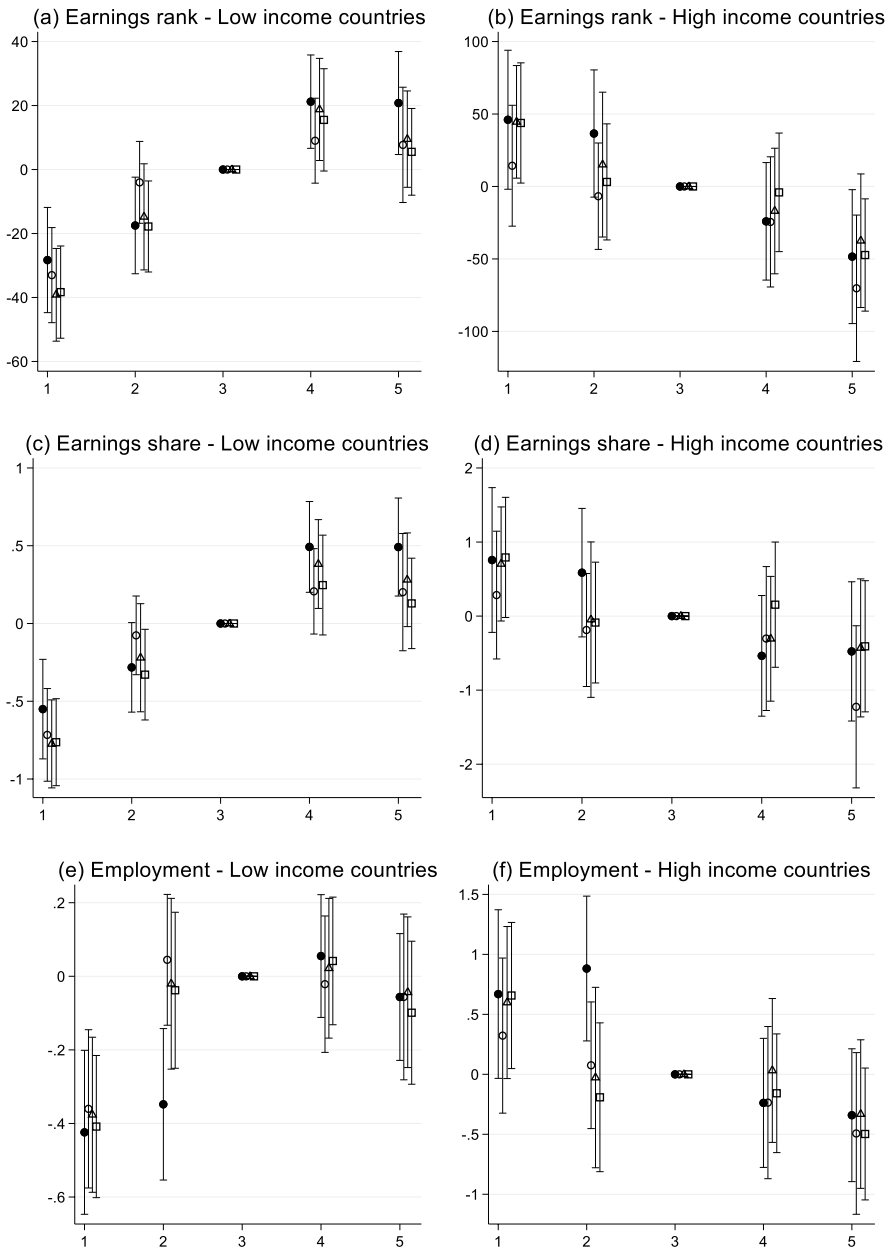


Fig. 9 **a** Main estimation results for men. Effects of immigrant shares on native outcomes by class background. Note: The estimates are based on Eq. (1). Point estimates are shown with 95% confidence intervals, with standard errors clustered at labor-market-region-by-cohort-cells. **b** Main estimation results for women. Effects of immigrant shares on native outcomes by class background. Note: The estimates are based on Eq. (1). Point estimates are shown with 95% confidence intervals, with standard errors clustered at labor-market-region-by-cohort-cells



● Parent age 52-58 ○ Parent age 33-36 △ Offspring age 7-15 □ Years 1980-92

Fig. 9 (continued)

Table. 4 Estimation results based on alternative class background ranking criteria. Effects of immigrant shares at age 32 on native outcomes at age 33–36. By immigrants' origin country and natives' class quintile

	Parent's education 1990			Grandfather's earnings age 52–58 (or later)		
	1 Earnings rank	2 Earnings share	3 Employment	4 Earnings rank	5 Earnings share	6 Employment
<i>Immigration from low income countries</i>						
Quintile 1	-34.58*** (4.84)	-0.69*** (0.09)	-0.37*** (0.06)	-27.35*** (7.92)	-0.48*** (0.15)	-0.37*** (0.09)
Quintile 2	- 0.10 (5.16)	-0.04 (0.10)	-0.00 (0.06)	-7.04 (6.50)	-0.15 (0.12)	-0.12 (0.08)
Quintile 3	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Quintile 4	28.67*** (5.34)	0.58*** (0.10)	0.04 (0.06)	18.33** (7.37)	0.35** (0.14)	0.02 (0.08)
Quintile 5	21.21*** (4.75)	0.50*** (0.10)	-0.09 (0.06)	43.26*** (8.05)	0.96*** (0.16)	0.16* (0.09)
<i>Immigration from high-income countries</i>						
Quintile 1	0.55 14.74)	0.07 (0.28)	0.54*** (0.19)	68.17** (27.88)	1.13** (0.51)	0.75** (0.31)
Quintile 2	-8.36 (14.79)	-0.15 (0.28)	0.25 (0.19)	10.42 (22.75)	0.12 (0.42)	0.18 (0.28)
Quintile 3	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Quintile 4	15.80 (15.14)	-0.07 (0.28)	-0.07 (0.18)	-25.03 (24.01)	-0.56 (0.47)	-0.03 (0.26)
Quintile 5	50.50*** (13.99)	-0.52* (0.30)	-0.09 (0.18)	-41.08 (27.64)	-0.78 (0.54)	-0.37 (0.31)
R-squared	0.039	0.040	0.046	0.011	0.012	0.020

The number of observations are 1,116,827 in columns 1–3 and 428,752 in columns 4–6. Standard errors (clustered at labor-market-region-by-cohort-cells) are reported in parentheses. See also note to Table 1.

*/**/** indicates statistical significance at the 10/5/1 % level.

The ranking based on grandfathers can only be implemented for a subset of the data. The fact that we cannot observe earnings prior to 1967 implies that the earnings of grandfathers born before around 1905 cannot be properly observed during active age. This part of the analysis is therefore restricted to people born between 1970 and 1980 who have at least one male grandparent born after 1904. For each grandfather, we compute (a proxy for) age X earnings by picking the observed earnings for the calendar year as $\max(1967, \text{birthyear} + X)$, and calculate average earnings for age 52–58 as the basis for ranking. In practice, this implies that for parents whose earnings are not observed until after age 52, we substitute earnings obtained at somewhat higher ages.

Table 4 presents our main results based on the two alternative ranking criteria, in both cases using the baseline model in Eq. (1). They follow the same pattern as the main results in Table 1.

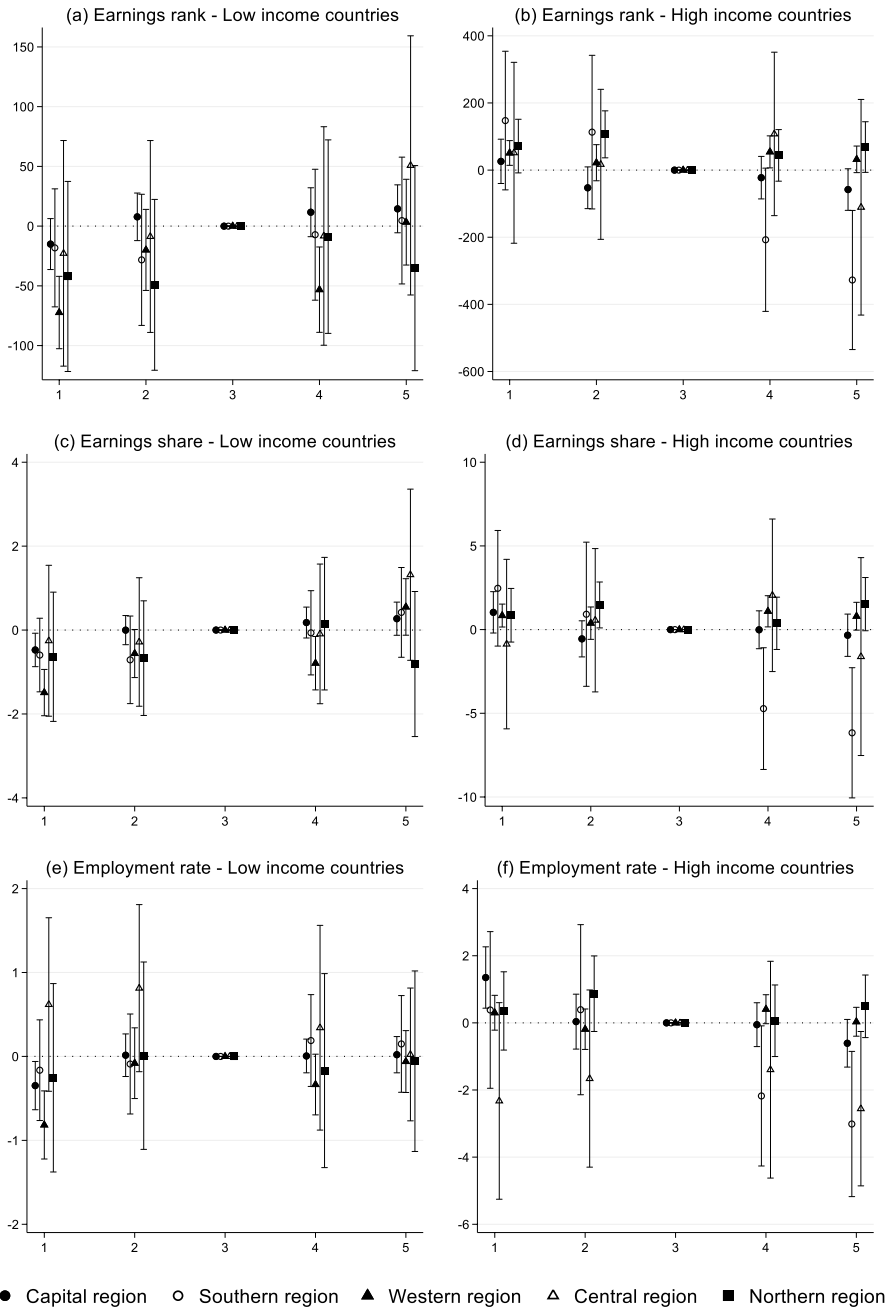
Fig. 10 a Separate estimation results for each geographical region in Norway. Effects of immigrant shares on native outcomes by class background. Note: The graphs show the point estimates with 95% confidence intervals. The numbers of observations (commuting zones) in the various regressions are as follows: The capital region: 451,781 observations (13 commuting zones); the Southern region: 111,269 (6); the Western region 312,065 (12); the Central region: 130,091 (5); and the Northern region: 111,629 (10). See also notes to Table 1. **b** Separate estimation results for larger city zones and rural areas. Effects of immigrant shares on native outcomes by class background. Note: The graphs show the point estimates with 95% confidence intervals. The numbers of observations (commuting zones) are as follows “Rural and smaller city zones”: 623,410 observations (42 commuting zones), “Larger city zones” (4 commuting zones around Oslo, Bergen, Stavanger and Trondheim): 493,425 observations

Appendix D: Additional robustness analyses—separate estimates by geographic region or urbanity

In Section 5.2, we presented models with controls for class-by-cohort-by-gender fixed effects estimated separately by geographical region or by the commuting zone’s degree of urbanity. In this [appendix](#), we present results for models where we have split the sample into separate datasets, and estimate Eq. (1) separately for each entity. The results are presented in Figs. 10a and b. Although there are variations in point estimates across the different datasets, the main pattern is robust: Immigration from low-income countries steepens the social gradient in all parts of the country, and in urban as well as rural areas. A particularly robust result is that relative labor market outcomes for the bottom class are negatively affected by immigration from low-income countries and positively affected by immigration from high-income countries. The effects on the social gradient across the middle and upper classes are less robust.

Appendix E: Separate estimates for movers and stayers

Table 5 report separate baseline results for movers (offspring living in another commuting zone at age 32 than at age 15/16) and stayers.



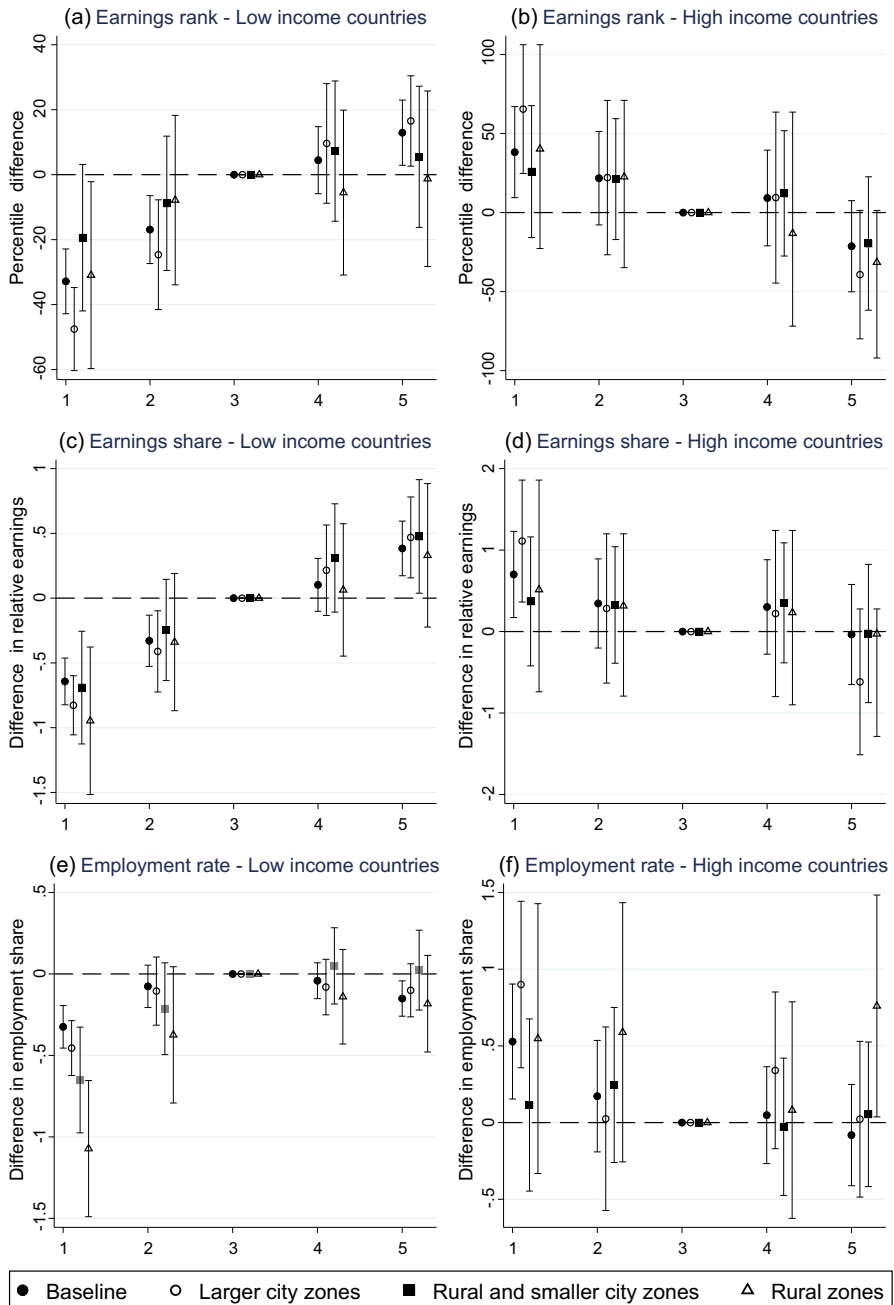


Fig. 10 (continued)

Table. 5 Estimation results for movers and stayers. Effects of immigrant shares at age 32 on native outcomes at ages 33–36. By immigrants' origin country and natives' class quintile

	Movers			Stayers		
	1 Earnings rank	2 Earnings share	3 Employment	4 Earnings rank	5 Earnings share	6 Employment
<i>Immigration from low income countries</i>						
Quintile 1	-42.19*** (11.48)	-0.87*** (0.22)	-0.65*** (0.14)	-36.36*** (5.15)	-0.71*** (0.092)	-0.29*** (0.08)
Quintile 2	-10.42 (12.67)	-0.22 (0.24)	-0.13 (0.14)	-20.22*** (5.84)	-0.40*** (0.11)	-0.063 (0.074)
Quintile 3	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Quintile 4	-12.13 (12.14)	-0.05 (0.24)	-0.14 (0.12)	13.71** (5.57)	0.23** (0.11)	0.00 (0.07)
Quintile 5	8.22 (11.65)	0.23 (0.22)	-0.13 (0.11)	28.12*** (5.86)	0.72*** (0.13)	-0.05 (0.07)
<i>Immigration from high-income countries</i>						
Quintile 1	39.80 (31.42)	0.78 (0.60)	0.07 (0.39)	38.39** (15.23)	0.67** (0.27)	0.76*** (0.22)
Quintile 2	6.26 (31.93)	0.17 (0.64)	0.26 (0.38)	20.64 (16.80)	0.29 (0.30)	0.16 (0.21)
Quintile 3	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Quintile 4	49.50 (31.50)	0.75 (0.62)	0.73 (0.32)	12.73 (16.75)	0.53* (0.31)	-0.06 (0.19)
Quintile 5	59.17** (28.26)	0.63 (0.58)	1.08*** (0.29)	21.27 (16.75)	1.41*** (0.35)	-0.20 (0.20)
R-squared	0.070	0.067	0.060	0.045	0.044	0.053

The number of observations are 291,863 in columns 1–3 and 824,964 in columns 4–6. Standard errors (clustered at labor-market-region-by-cohort-cells) are reported in parentheses. See also note to Table 1. */**/***/*** indicates statistical significance at the 10/5/1 % level

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Declarations

Conflict of interest The authors declare that they have no conflict of interest.

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