



# Immigration and the Tower of Babel: Using language barriers to identify individual labor market effects of immigration<sup>☆</sup>

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## ABSTRACT

This paper introduces a novel approach to estimating immigration impacts on natives' labor market outcomes. Differential language requirements across occupations serve as an arguably exogenous source of variation during the large and sudden immigration surge to Norway after the enlargements of the European labor market in 2004 and 2007. Migrant inflow into occupations is instrumented with occupations' required level of (Norwegian) language skills. Administrative register data allow for a rich set of individual-level outcomes. Comparing workers in occupations with different language requirements, I find that a one percentage point increase in the share of Eastern European workers reduced native workers' labor earnings by 0.75 percent. I further find adverse employment effects and evidence of skill-upgrading, but largely no other form of worker mobility among treated individuals. In particular, young workers were hit in the wage dimension and old workers in the employment dimension. The results are highly robust.

## 1. Introduction

What happens to the labor-market careers of native workers after a sudden inflow of migrants into their occupations? The question is difficult to answer empirically because labor-demand changes affect migration flows, causing a simultaneity problem. In this paper, I provide an answer based on individual-level administrative register data for Norway and novel exogenous immigration variation at the occupational level. The variation arises from occupation-specific requirements for language skills combined with the enlargement of the common European labor market.

The expansions of the European Union (EU) eastward in 2004 and 2007 with, in total, 12 new countries (EU12<sup>1</sup>) led to an immigration surge into Norway. Labor immigration from the new member states increased strongly and contributed to more than a quarter of the net increase in the Norwegian workforce between 2005 and 2011. Prior to the EU accession, EU12 immigration had been limited to seasonal workers and specialists with work permits via their Norwegian employ-

ers. As EU citizens, however, they were free to enter employment in Norway.

The arriving EU12 migrants sorted into less language-intensive occupations because of limited Norwegian language skills. Norwegian is linguistically distant from Eastern European languages and hardly spoken or taught outside Norway. Consequently, natives and immigrants with otherwise identical formal qualifications became employed in different occupations. The left panel of Fig. 1 illustrates the handicap of EU12 migrants. The vertical axis measures the percentage point change in occupations' share of EU12 workers from 2005 to 2011, and the horizontal axis ranks all occupations by the required level of Norwegian language skills. I measure language requirements with a standardized index based on O\*NET data adapted to Norwegian occupations.<sup>2</sup> Each circle represents an occupation, weighted by (total) 2005 employment. Occupations with above-average language requirements received substantially less EU12 immigration than did occupations with below-average.

Fig. 1 illustrates that occupation-specific language requirements are well suited to instrument for EU12 immigration into the Norwegian

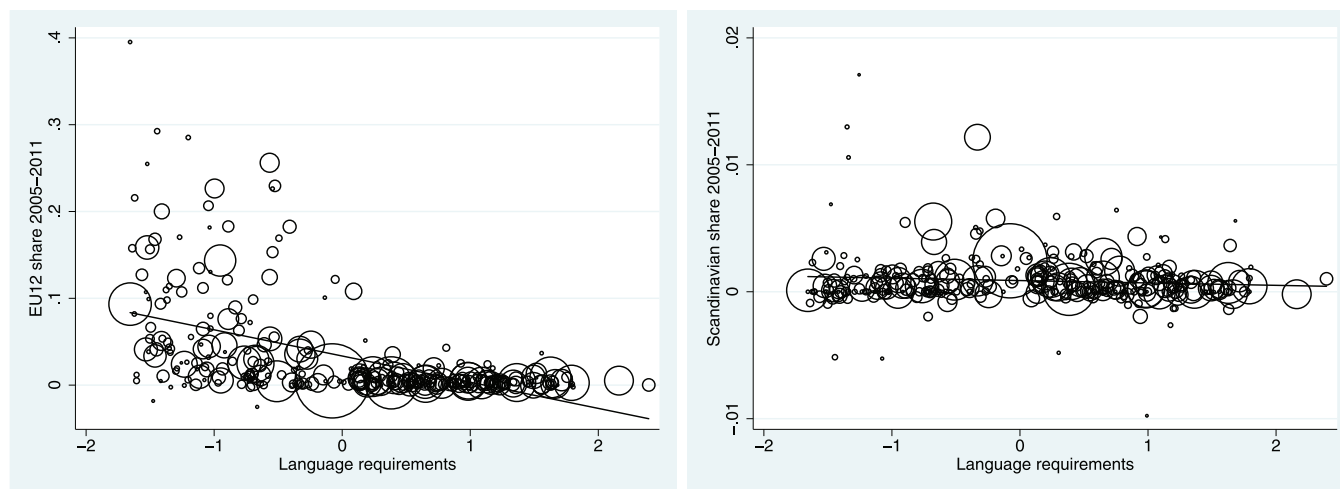
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<sup>1</sup> Bulgaria, Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Romania, Slovakia and Slovenia.

<sup>2</sup> U.S. Department of Labor, [www.onetcenter.org](http://www.onetcenter.org).



**Fig. 1.** Immigration into occupations and language requirements. *Note.* The y-axis measures the percentage point change in occupations' share of EU12 immigrants (left panel) and recent Scandinavian immigrants (right panel) from 2005 to 2011. The x-axes measure occupation-specific requirements for language skills, given by a standardized index retrieved from O\*NET data. Each circle is an occupation, weighted by its size in 2005. The solid line is a linear fit. Scandinavian immigrants are newly arrived Swedes and Danes to exclude those with already acquired Norwegian language skills.

labor market, but less so for immigration from countries with linguistically proximate majority languages, for instance, Scandinavia. The Scandinavian languages—Swedish, Danish, and Norwegian—are so similar that Swedes and Danes in principle can enter most Norwegian occupations. As visible in the right panel of Fig. 1, Scandinavian migrants entered occupations along the complete language-skill distribution.<sup>3</sup> In parallel, language requirements are less suited to instrument for immigration to countries with a world language as the majority language, where such requirements generally form no barriers.

I exploit the relationship in the left panel of Fig. 1 to estimate the causal effect of the historically large labor immigration surge to Norway on natives' occupation-specific outcomes—that is, labor earnings, several employment outcomes, and various mobility forms. I instrument for the possibly endogenous inflow of EU12 workers into occupations with the language requirement index. The baseline model gives the effect of EU12 immigration into (unprotected) natives' initial occupation (fixed in 2005) on the change in their cumulative labor earnings from before to after the onset of the immigration surge.

The results show that a one percentage point increase in an occupation's EU12 share reduced the earnings of natives initially employed in that occupation by 0.75 percent relative to language-protected natives. The estimate incorporates all medium-run changes in labor-market activity that possibly affect labor earnings, including occupational mobility. I find evidence of strong adverse impacts on wages and unemployment insurance receipt for young workers, and on work hours, full-time employment, and disability program participation for all workers, and in particular for old workers. Surprisingly, I find no effect on mobility across areas, industries, sectors, or firms for workers exposed to EU12 immigration. However, the probability of re-educating and entering occupations with higher language requirements increased, indicating skill-upgrading.

To interpret the estimated earnings effect causally, I must assume parallel earnings trends in absence of migration—that is, that the earnings of workers in occupations with varying language requirements would have developed equally without increased inflow of EU12 workers, conditional on the control variables. Section 5 gives credibility to the assumption both graphically and with numerous robustness checks, and shows that EU12 immigration cannot predict previous earnings. Further, estimating on sample strata and adding and altering various

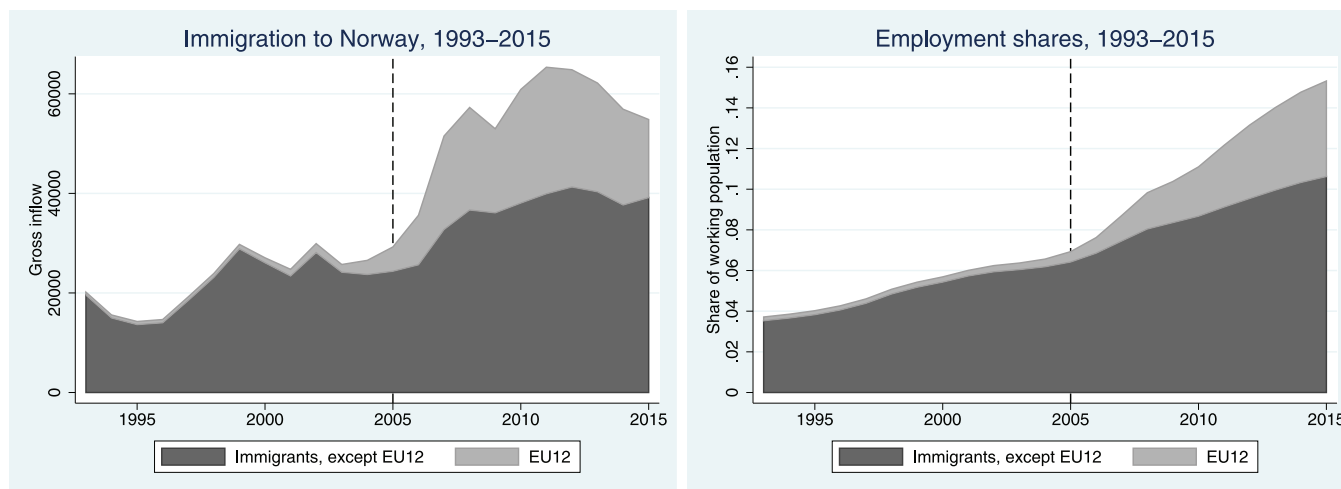
individual- and occupation-level controls, such as intelligence, industry, region, and requirements for several cognitive ability measures, do not change the main conclusion. Neither does the inclusion of fixed effects for nine aggregated occupation groups, which alters the identification level from across all occupations to across occupations *within* each group. All checks prove that the estimated immigration coefficient is highly robust.

This paper introduces language requirements as a novel instrument for the endogenous allocation of immigrants at the occupational level. Rather than estimating impacts on the skill content of natives' occupations, as in Foged and Peri (2016), Peri and Sparber (2009), and Peri and Sparber (2011), I use the specific (language) skill content of each initial occupation to predict migrant inflow and thereby the heterogeneous immigration exposure of native workers with unequal language protection. The identified effect is the *relative effect* on natives employed in occupations with different language requirements, and thereby with different immigrant exposure, and not the *total effect* on (all) natives. I estimate the earnings differentials resulting from increases in the supply of low-language workers.

I cannot identify the underlying theoretical mechanisms, which may include high substitutability between natives and immigrants within occupations and complementarities across occupations. Increased demand for high-language workers or for language skills *per se* in response to increased low-language labor supply would amplify the effect. Opposite, natives' selective entry into more language-intensive occupations would reduce the wages in those occupations, dampening the (relative) effect. The total effect is likely to be less negative due to cross-occupational complementarities.

Two related Norwegian studies by Bratsberg and Raaum (2012) and Finseraas et al. (2019) exploit differential licensing requirements among occupations in the construction sector. Both estimate almost identical wage effects as my earnings estimate. To my knowledge, other occupation-level studies estimate total wage effects and therefore are not directly comparable. The comparability is also low due to the setting-specific nature of immigration impacts and because different estimation strategies identify different parameters (Dustmann et al., 2016). Nevertheless, my results align with the negative effects on manual laborers in Orrenius and Zavodny (2007); the large negative effects on service occupations in Steinhardt (2011); and the negative effects at the bottom, the insignificant at the middle, and the positive at the top of the communicative-to-manual task intensity distribution in Bollinger and Sharpe (2019). Because I estimate relative effects, my results are

<sup>3</sup> Since 1954, labor has moved freely within the Nordic (herein, *Scandinavian*) countries.



**Fig. 2.** Immigration to Norway from EU12 and all other countries, 1993–2015. Left: annual gross immigration. Right: immigrant employment shares. *Notes.* Counts include first-time moves only. The working population includes all residents of Norway aged 18–70 years with annual labor earnings above 1.5 G.

consistent with the (insignificant) large positive total earnings effects in [Friedberg \(2001\)](#), as well.<sup>4</sup>

The present estimation strategy falls into the broader “skill-cell approach,” in which workers are divided into experience-education cells. Commonly, skill-cell studies estimate negative (short-run) wage effects on (low-skill) natives relative to more experienced natives ([Aydemir and Borjas, 2011](#); [Borjas, 2003](#); [Llull, 2018](#)). My results align with these studies, as well, although the identified effect is relative to workers in occupations with different language requirements rather than different experience levels within education cells.

Exploiting occupation-level immigration variation has several advantages, including reduced bias resulting from misplacement of immigrants into skill cells (“downgrading bias”) and from low substitutability of immigrants and natives within skill cells. The substitutability is likely higher within occupations than skill cells ([Card, 2001](#)).<sup>5</sup> Further, when comparing repeated cross-sections over time—as in both skill-cell studies and “spatial studies”—mobile workers may cause bias by altering the group compositions. I avoid potential attenuation bias arising from such endogenous mobility by following individuals and keeping their occupation fixed.<sup>6</sup>

The rest of the paper is organized as follows. [Section 2](#) describes the immigration surge; [Section 3](#) explains the identification strategy and data used; [Section 4](#) presents the estimation setup and baseline results; [Section 5](#) tests for validity and robustness; [Section 6](#) examines possible mechanisms behind the estimated earnings impact; and [Section 7](#) concludes.

## 2. Eastern European migration to Norway

I exploit the large and sudden immigration surge to Norway after the eastward EU enlargements as a “natural experiment” in estimating immigration impacts on the receiving labor market. Because Norway is not a formal EU member, the policy decision to enlarge the EU was unrelated to Norwegian economic conditions. Norway is nevertheless part

of the common European labor market (European Economic Area, EEA). Economic upturns and increasing labor demand, combined with a compressed wage structure and high relative compensation to low-skilled labor, attracted many new EU citizens into Norway. In fear of social dumping and welfare tourism, in 2004 many existing EEA countries implemented transitional restrictions on free movement of people from the new member states. Norway required contracted full-time employment with conditions according to Norwegian rules and standards in order to immigrate. After termination of the restrictions in 2009, EU12 citizens were free to migrate within the EEA. Prior to the EU enlargement, difficulties obtaining work permits had greatly limited labor immigration to Norway for other than specialists.

[Fig. 2](#)’s left panel depicts the gross inflow of migrants to Norway between 1993 and 2015 separately for EU12 (light gray) and all other origins (dark gray). EU12 immigration rose from nearly none (roughly 1000 per year) pre-enlargement to more than 25,000 in 2011—and gradually decreased thereafter. The financial crisis caused a small dip in 2009. Over the period with the largest immigration increase (2005–2011), EU12 workers accounted for more than a third of the total immigration to Norway and increased the working population by nearly 3 percent. The EU12 employment share rose from 0.4 to 3.0 percent—and up to 5.0 percent in 2015 ([Fig. 2](#), right panel). However, the degree of migrant competition faced by natives was highly unequal due to heterogeneous flows into areas, industries, and—most important in this study—occupations.

Basically, all EU12 immigrants were labor migrants and therefore entered directly into an occupation. They were, however, confined to occupations without language barriers. The largest EU12-receiving occupations between 2005 and 2011 had well-below-average language requirements. The top five were cleaners, carpenters, construction workers, cabinet makers, and fish-processing machine operators. The top receiving sectors were construction, service, manufacturing, agriculture, and forestry.

The Norwegian language skills of EU12 immigrants are widely known to be poor. Norwegian is hardly spoken or taught outside Norway and is linguistically distant from Eastern European languages. Further, unlike humanitarian immigrants, EEA labor migrants are not entitled to state-financed language training or integration programs. Only 21 percent of employers with Eastern European laborers offer some form of language training ([Friberg and Tyldum, 2007](#)). In general, immigrants’ language skills are correlated with their age at immigration, education, and perceived affiliation in the host country ([Vrålstad and Wiggen, 2017](#)), all of which further reduce the likelihood of such skills among EU12 workers.

<sup>4</sup> [Borjas and Monras \(2017\)](#)’s findings contradict [Friedberg’s \(2001\)](#) results with significant negative earnings effects on (high-skill) Israelis, but are disputed by [Clemens and Hunt \(2017\)](#).

<sup>5</sup> More recent studies have relaxed the assumption of perfect substitutability within cells (e.g., [Manacorda et al., 2012](#); [Ottaviano and Peri, 2008](#); [Peri and Sparber, 2009](#)).

<sup>6</sup> I ignore possible factor mobility across occupations. Because I examine only 6 years, this mobility is probably limited. Further, industry and area fixed effects reflect time-invariant differences in existing capital stock.

The typical EU12 immigrant to Norway is a 30-year-old man (70 percent men) from Poland without higher education or any particular language skill (Dølvik and Eldring, 2006).<sup>7</sup> The selection in the type of Polish immigrants to Norway is likely linked to language barriers in the Norwegian labor market (Friberg, 2013).<sup>8</sup> In a 2010 survey of Polish workers in the Oslo region, as many as 70 percent of male respondents, including those with several years of residency, had no or very limited Norwegian language skills (Friberg and Eldring, 2011). Nevertheless, the crucial point for the present identification strategy is that EU12 workers did not speak Norwegian upon arrival and therefore entered occupations without language barriers. Section 4 details this point. Similarly, Chiswick and Taengnoi (2007) find that in the United States, immigrants with linguistically distant mother tongues are employed more often in occupations with low English-language requirements.

### 3. Data

This study is based on high-quality, individual-level administrative register data of all residents of Norway. The panel structure of the data allows me to follow individuals over time. The main source is the Register of Employers and Employees, with information on cash payments, duration, industry, and municipality of all employment spells each year. Since 2003, it also includes occupation and contracted work hours. I keep each worker's main occupation, defined as the best paid (full- or part-time) occupation or the unique full-time occupation at end of the base year (2005).

Data on annual labor earnings come from the Norwegian Tax Register. Earnings include income from employment and self-employment, taxable in-kind earnings, and sickness- and parental-leave benefits. I censor negative wage earnings to 0 and top income to 10 million NOK annually. I deflate earnings with the overall wage growth, approximated by the growth in the Social Security system's basic amount (G), such that monetary variables are measured in 2005 value.<sup>9</sup> Employment is defined as annual labor earnings above 1.5 G.

Immigrant and demographic data are drawn from the Central Population Register. "Immigrants" are persons born abroad by two foreign-born parents. Occupations' immigrant shares are based on the universe of residents aged 18 to 70 years with labor earnings above 1.5 G, excluding temporary and seasonal migrants.

Data on natives' highest completed education follow the Norwegian Standard Classification of Education with six-digit codes for each educational attainment (Barrabés and Østli, 2015). I use three-digit codes, capturing both level and main fields. Industry data follow two-digit codes of the Standard Industrial Classification of 2007. I use alternative specifications in the robustness analyses. Firms' sectors are given by 33 codes and localization by 46 commuting zones, following Bhuller (2009).

Table A.1, Column 1, describes the estimation sample, consisting of 772,310 native residents aged 23 to 62 years between 2005 and 2011. To ensure comparability of workers across occupations and of their earnings developments in the outcome period, I limit the sample to full-time employees with annual earnings above 1.5 G in the 4 years prior to the immigration surge (2002–2005) and not in education in 2005. For this group, the outcome variables can be constructed consistently. All outcomes measure changes from before to after the onset of the surge—that is, cumulative outcomes over the period 2006 to 2011 relative to 2002 to 2005, except weekly work hours and hourly wages, which are instead

<sup>7</sup> Education data for immigrants are based on survey data and available for less than 40 percent of the EU12 immigrants who arrived between 2005 and 2011 and were 23 to 62 years old. Of those with data, 64 percent have secondary education as the highest completed level.

<sup>8</sup> Baba and Dahl-Jørgensen (2013) further discuss the role of language and language policies in the migration from Poland to Norway.

<sup>9</sup> One G was equal to 60,699 Norwegian kroner (NOK), roughly USD 7,500 in 2005.

relative to 2003 to 2005 due to poor data quality in 2002. Immigration exposure is defined as the growth in occupations' EU12 employment shares from 2005 to 2011.<sup>10</sup> I exclude the Armed Forces due to lack of O\*NET data and occupations with less than 100 workers because of the potentially volatile immigrant-share measure. This leaves 318 of 349 occupations.<sup>11</sup>

The O\*NET database provides data on worker and occupational characteristics, from which I construct an index that measures occupations' language requirements.<sup>12</sup> The index is a standardized average over four "worker requirements": speaking skills, writing skills, knowledge of English, and (the inverse of) foreign language knowledge. The latter two account for majority language requirements (i.e., English in the U.S., and Norwegian in Norway) rather than general communication skills. Requirements for foreign languages enter inversely because they may correlate negatively with majority language requirements, and hence positively with the inflow of EU12 workers. Thus, they would weaken the correlation between language requirements and EU12 immigration into Norwegian occupations.<sup>13</sup> I average over the four requirements and standardize to mean 0 and standard deviation of 1.<sup>14</sup>

To map the language index to Norwegian occupations, I manually construct a mapping between the occupational classification systems in Norway ("Standard yrkesklassifisering NOS C521," in Statistics Norway, 1998) and the U.S. Standard Occupational Classification System as of the 2000 Census of Population and Housing.<sup>15,16</sup> I thoroughly inspect that the resulting language skill ranking of Norwegian occupations is sensible. However, an imperfect ranking would weaken only the correlation between language and immigration—reducing the first stage—but not invalidate the identification strategy. Section 4 shows that the first stage is indeed strong.

Unfortunately, data on workers' occupations are scarce in the beginning of the estimation period. The Employer and Employee Register includes information on occupations for a selection of workers from 2003. The quantity and quality of the occupation data gradually increase over time, as more employers register their employees' occupations (correctly). By 2010, basically all workers have occupation data recorded. In 2005, occupation is available for 66 percent of workers.<sup>17</sup> I increase that share to 85 percent by adding occupation data for workers included in the Wage Statistics Survey from Statistics Norway and by extrapolating

<sup>10</sup> The share is defined as the number of EU12 workers divided by the total number of workers in an occupation in year  $t$ . This measure may lead to a bias if natives selectively change occupation and thereby alter the immigrant share for a constant number of immigrants (Card and Peri, 2016). Nevertheless, due to improvements in occupation data over the period, I still use the annual number of workers in the denominator rather than fixing it in 2005. My assessment is that the more correct immigrant-share measure surpasses the possible bias. Furthermore, it minimizes possible attenuation bias (Aydemir and Borjas, 2011). Result with the denominator fixed in 2005 indicates robustness to the choice (Table A.6, Panel D).

<sup>11</sup> Examples of excluded occupations include hunters; pawnbrokers; handicraft workers; fashion models; and historians, archaeologists, and philosophers.

<sup>12</sup> I use Version 9.0 from 2005.

<sup>13</sup> The first stage, as well as the point estimate, is indeed reduced without foreign language requirements in the index (Table A.6, Panel B).

<sup>14</sup> O\*NET includes information on both the required level and the importance of each requirement. I rescale each requirement level to the importance scale (1–5) and multiply them together before averaging over the four requirements.

<sup>15</sup> <https://www.bls.gov/soc/2000/socguide.htm>

<sup>16</sup> The technical report Hoen (2016) provides the complete mapping and details on the O\*NET data used.

<sup>17</sup> Better occupation data, which increase the sample size and reduce the measurement error in the immigrant shares, drive the decision to use 2005 rather than 2004 (when the EU expanded) as the base year. The choice represents a compromise between data quality and natives' endogenous occupation choices. Natives may have started adjusting to increased immigration in 2005, but Fig. 2 shows little immigration the first year. Thus, the endogeneity issue in 2005 is probably small.

occupation back one year for workers employed within the same firm for two successive years.

Table A.1 compares the estimation sample to the sample of workers who would have been included if they had occupation data in 2005. By far, the largest difference is in the public sector share—91 percent of the “lost” workers are in that sector. Because public sector workers usually earn less, are better educated, and are mostly (70 percent) women, the samples differ in these aspects, as well, but are otherwise comparable.

Initially, large privately owned firms were required to register their workers’ occupations, whereas most small firms and public sector employers were exempted. All occupations over-represented in the beginning of the data period are hence in the private sector. They have mainly below-average language requirements. Many public employers instead reported “positions” but gradually changed to reporting occupations.<sup>18</sup> Thus, public sector occupations—in particular within health and education—with well-above-average language requirements, are under-represented. This reduces the (control) group with limited immigration exposure, but is, however, unlikely to bias the results by mismeasuring the EU12 shares due to the limited number of EU12 immigrants in the public sector. At a minimum, the estimate represents a lower bound (fewer public sector workers in 2005 attenuates the increase in the EU12 shares in language-intensive occupations). Moreover, the results are robust to excluding public sector employees, as well as estimating within only the largest (10 percent) private firms and occupations with below-median language requirements (Table A.5).

The estimation sample may not represent the Norwegian workforce perfectly due to scarce occupation data. However, because I estimate individual-level impacts rather than occupation averages, the non-representativeness should not bias the results. I fix workers’ occupations and follow changes in their labor-market careers that result from changes in the initial occupation’s EU12 employment share. As long as the improved registration of workers’ occupations do not differ systematically between natives and immigrants, the immigrant shares will not be biased. Such a systematic difference is unlikely because firms had to register all employees’ occupations, regardless of migrant status. Further, the results are insensitive to exchanging the changes in the EU12 shares from 2005 to 2011 with the levels in 2011, when the shares are measured perfectly (Table A.6, Panel C). In all, limited occupation data are unlikely to bias the results but may reduce the external validity.

#### 4. Identification and estimation

Estimating the impact of immigration on natives’ earnings by simply comparing workers in occupations with different migrant inflows would yield a biased estimate. Arguably, immigrants select into booming occupations, upward biasing the estimate. On the other hand, they may be confined to declining occupations due to, for instance, discrimination, and thereby downward bias the estimate. To circumvent these selection issues, I exploit occupation-specific language requirements as identifying variation. The variation is arguably exogenous to labor-demand changes, conditional on the control variables, as it is caused by occupations’ distinctive features.

I instrument for the possibly endogenous inflow of EU12 migrants into natives’ (initial) occupations by occupations’ language requirements. The baseline outcome is the change in cumulative labor earnings from a 4-year pre-immigration period to a 6-year outcome period. It includes all factors that possibly affect total earnings (e.g., wages, work hours, unemployment, self-employment, occupational mobility). The long outcome period captures sluggish responses to immigration and accounts for the gradual nature of the immigration surge and the uncertain timing of immigration impacts.

<sup>18</sup> “Positions” are linked to wage payments and tenure rather than tasks and work content, and therefore not directly relatable to occupations. To increase the public sector share, I nevertheless translate positions into occupations whenever possible. (See Hoen (2016) for further details on the data issues.)

To be a valid instrument, language requirements must satisfy two properties. First, they must predict the inflow of EU12 workers into occupations—that is, the first stage in the IV estimation must be strong. I verify this graphically and in the estimation. Second, language requirements can have no independent effect on changes in natives’ earnings, and no variable correlated with both language and earnings can be omitted from the estimation. Although the latter cannot be tested directly, Fig. 3 provides graphical evidence and Section 5 presents numerous robustness checks.

The baseline estimation model is as follows. Eq. (1) is the first-stage regression of the immigration variable on the instrument and the vector of control variables and Eq. (2) is the second stage regression of the outcome variable on the predicted immigrant share from the first stage and the same set of controls.<sup>19</sup>

$$\Delta EU12_{j,05-11} = \gamma_0 + \gamma_1 lang_j + X'_{i,05} \gamma_2 + \gamma_3 \tilde{Y}_{i,02-05} + \gamma_4 soc_j + \xi_{ij} \quad (1)$$

$$y_{ij} = \beta_0 + \beta_1^{IV} \widehat{\Delta EU12}_{j,05-11} + X'_{i,05} \beta_2 + \beta_3 \tilde{Y}_{i,02-05} + \beta_4 soc_j + \epsilon_{ij} \quad (2)$$

$\Delta EU12_{j,05-11}$  is the change in occupation  $j$ ’s EU12 employment share from the base year (2005) to the immigration peak (2011), and  $lang_j$  is the standardized language index explained in Section 3. The earnings measure,  $y_{ij} = \ln(\sum_{2006}^{2011} Y_{ijt}) - \ln(\sum_{2002}^{2005} Y_{ijt})$ , is the logarithm of cumulative annual (real) labor earnings from 2006 to 2011 for worker  $i$  in occupation  $j$ , minus the same lagged four years (2002–2005). It is specified in terms of changes to account for differences in occupation-specific labor demand and individuals’ earnings potential. The parameter of interest,  $\beta_1^{IV}$ , gives the earnings effect of increased EU12 immigration into occupations, measured relative to natives protected by language barriers.

I fix natives’ occupations in 2005 to avoid bias caused by endogenous occupation shifts. The left hand side follows workers’ annual labor earnings, regardless of labor market activity, whereas the immigration measure captures inflow of EU12 workers into the initial occupation.  $X_{i,05}$  is a vector of control variables possibly correlated with the error term. They include fixed effects for sex, birth year, highest education completed, industry, commuting zone, and years of tenure, and a linear control for labor-market experience, all fixed in 2005.  $\tilde{Y}_{i,02-05} = (Y_{i,05} - Y_{i,02})/Y_{i,02}$  is earnings growth from 2002 to 2005. Finally,  $soc_j$  is occupations’ requirements for social skills.<sup>20</sup> Such skills might cause omitted variable bias if they were increasingly rewarded in the labor market during the estimation period (Deming, 2017; Edin et al., 2017) and positively correlated with language requirements. I cluster standard errors at the occupational level, and check for sensitivity with higher level clustering.

Table 1, Column 1 shows the main result. A one percentage point higher EU12 share led to 0.75 percent lower earnings in the period from 2006 to 2011 relative to 2002 to 2005 for natives initially employed in occupations without language barriers. The first stage (Column 2) is significant at any conventional level and has a robust  $F$ -statistic of 33. One standard deviation higher language index reduces an occupation’s EU12 share by 3.4 percentage points. Column 3 shows the ordinary least squares (OLS) results from an (uninstrumented) estimation with the EU12 share directly inserted into Eq. (2). A one percentage point higher share is associated with 0.36 percent lower earnings. The upward-biased OLS estimate reveals that EU12 workers sorted into occupations with increasing demand.

To illustrate the economic magnitude of the effect, the overall real wage in Norway grew around 17 percent from 2006 to 2011.<sup>21</sup> The EU12 share grew on average 3.8 percentage points more in occupations

<sup>19</sup> The estimation was conducted with Correia (2016)’s Stata module.

<sup>20</sup> A standardized average of the required level of social perceptiveness, coordination, persuasion, and negotiation from O\*NET, following Deming (2017).

<sup>21</sup> Wage per standard person-year, deflated by the consumer price index. Numbers from Statistics Norway’s StatBank, Table 311464.

**Table 1**  
Main estimation results.

	(1)	(2)	(3)
Growth in total labor earnings from 2002–2005 to 2006–2011	IV b/se	First stage b/se	OLS b/se
EU12 share	-0.745*** (0.156)		-0.359*** (0.048)
Language		-0.034*** (0.006)	
Occupations' social skills	YES	YES	YES
Earnings growth 2002–2005	YES	YES	YES
Education fixed effects	YES	YES	YES
Industry fixed effects	YES	YES	YES
Region fixed effects	YES	YES	YES
Previous labor market history	YES	YES	YES
Demographics	YES	YES	YES
R <sup>2</sup>	0.070		0.071
F-statistic		33.06	
R <sup>2</sup> adj		0.488	
N	772,310	772,310	772,310

Estimation of baseline model (Eqs. (1) and (2)). Outcome is the logarithm of total labor earnings in 2006–2011 minus the same in 2002–2005. "EU12 share" is the change in occupations' share of EU12 workers from 2005 to 2011. The share is instrumented with occupations' language requirements in column 2. Workers' occupations and all controls are fixed in 2005. Controls include three-digit education, two-digit industry, commuting zone, birth year, years of tenure, and sex fixed effects, as well as labor-market experience, earnings growth from 2002 to 2005 and occupations' social-skill requirements. Baseline sample includes native residents born 1949–1982 who were employed full-time, had annual labor earnings above 1.5 G in 2002–2005, and were not in education in 2005. Standard errors clustered at the occupational level are given in parentheses. Reported F-statistic is Kleibergen-Paap rk Wald F-statistic. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

with below-median language requirements than above. The average relative earnings loss between workers with and without language protection was thus nearly 3 percent, or roughly one-sixth of the real-wage growth (although the earnings estimate also includes employment effects). Similarly, Bratsberg and Raaum (2012) find identical point estimates and Finseraas et al. (2019) find 1 to 2 percent reduced wage

growth for workers unprotected by occupational licensing requirements in the Norwegian construction sector.

The Introduction raised caveats of comparing estimates across studies. With the caveats in mind, my estimate is nevertheless close to the immigration coefficients in Borjas (2003)'s seminal study: -0.60 on log weekly earnings and -0.92 on log annual earnings. Aydemir and Borjas (2011) find somewhat weaker wage effects (around -0.5 for both Canada and the U.S.). Llull (2018) reports wage estimates of more than double the size of mine but OLS estimates that are closer. Finally, Card (2001) estimates a relative wage effect on low-skilled natives of -0.15 percent. Thus, my result seems to align with the literature.

**5. Robustness**

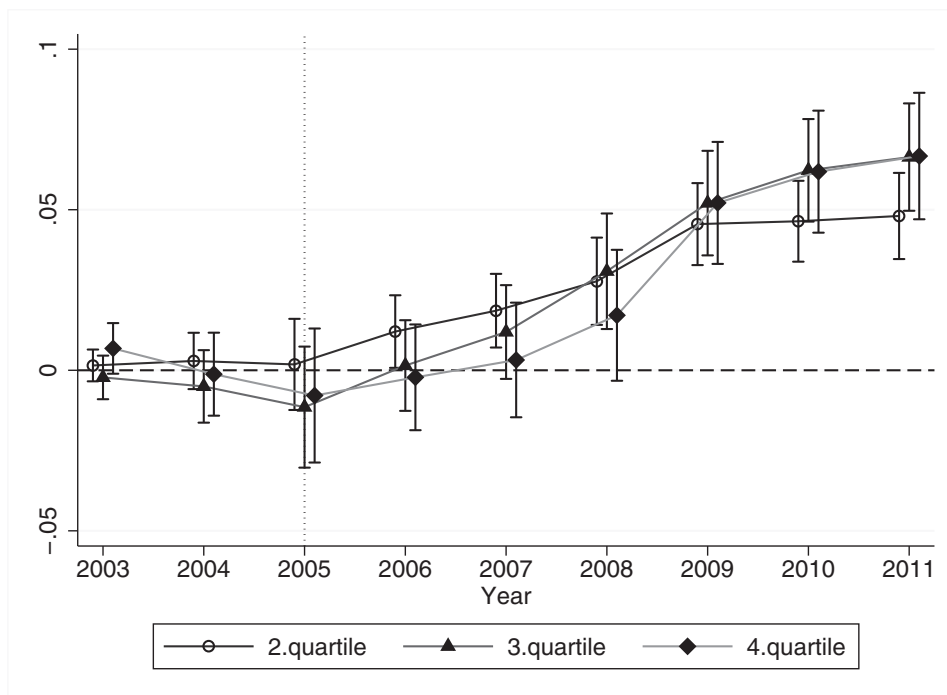
**5.1. Parallel earnings trends**

The crucial assumption for a causal interpretation of the estimated earnings effect is that the earnings trends in occupations with varying language requirements would have been equal in the absence of immigration, conditional on the control variables. If the model does not control for all factors that possibly correlate with both earnings developments and language requirements, the estimate is biased. Several tests of this parallel earnings-trend assumption follow.

First, I show graphically that the earnings developments in quartiles of the language-requirement distribution were similar pre-immigration. In a panel data setup, see Eq. (3) below, I regress log annual (real) earnings of individual  $i$ 's 2005-occupation  $j$  on year fixed effects ( $T_t$ ) interacted with the language quartile ( $Q_{j,05}$ ) of  $i$ 's occupation. I include all baseline controls ( $X_{it}$ ) except earnings growth, as well as fixed effects for year, quartile, and year-by-group for nine aggregated occupation groups ( $J_{j,05}$ ). The nine groups are defined by the first digit of the occupation codes and roughly represent skill levels (Statistics Norway, 1998).

$$y_{ijt} = \alpha + \alpha_1 T_t \times Q_{j,05} + X_{it}' \alpha_2 + T_t + Q_{j,05} + T_t \times J_{j,05} + \epsilon_{ijt} \quad (3)$$

Fig. 3 plots the coefficients on the quartile-by-year interactions ( $\alpha_1$ ) together with 95 percent confidence intervals. They measure average (residual) earnings in each language quartile relative to the lowest quartile in the base year 2002. I keep workers' occupation fixed in 2005. Fig. A.1 shows the exact same figure but with time-varying



**Fig. 3.** Average (residual) earnings in language quartiles, relative to the lowest quartile. Note. The figure shows the  $\alpha_1$ -coefficients from Eq. (3) on quartile-by-year fixed effects together with 95 percent confidence intervals. The coefficients are relative to the lowest language quartile in 2002. To minimize compositional changes in the quartiles over time, I limit the sample each year to full-time employees with annual earnings above 1.5 G, not in education, and of age 23–62 years. I fix workers' occupation and the language quartiles in 2005, but let all controls vary over time. The largest occupations in each quartile are 1) carpenters, cleaners, and plumbers; 2) shop salespersons, clerical officers, and stock clerks; 3) nursing assistants, sales representatives, and childcare workers; and 4) teachers, chief executives, and secretaries.

**Table 2**  
Previous earnings as outcome.

	(1)	(2)	(3)
	Log	Log	Growth
	b/se	b/se	b/se
EU12 share	−0.928 (0.643)	−0.922 (0.641)	−0.401* (0.169)
Earnings growth 02–05	No	Control	Outcome
R <sup>2</sup>	0.525	0.525	0.061
N	772,310	772,310	772,310

Estimation of baseline model with previous earnings as outcome variable. Outcome in (1) and (2) is log of total earnings over 2002–2005. Earnings growth from 2002 to 2005 is included as control in (2) and as outcome variable in (3). \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

occupations. Reassuringly, earnings differ insignificantly across quartiles pre-immigration, lending support to the parallel earnings-trend assumption. Furthermore, earnings in the top three quartiles grow significantly more towards the end of the period. As expected, the trends do not diverge immediately after 2005 due to the gradual nature of both the immigration surge and its impact. However, Fig. 3 excludes the unemployment component of the earnings decline because the underlying sample includes only employed individuals (see Fig. 3 notes).

Second, I check whether EU12 immigration can explain earnings backward in time, which would reduce the credibility of the parallel trend assumption. I estimate the baseline model with earnings prior to the immigration surge as the dependent variable, keeping everything else equal except from (re-)moving the earnings growth control variable. The outcomes in Table 2, Columns 1 and 2 are the logarithm of cumulative earnings from 2002 to 2005 (equal to the denominator of the baseline outcome variable).<sup>22</sup> Column 2 controls for earnings growth from 2002 to 2005 (the baseline earnings growth control variable), as well. This growth variable is the outcome in Column 3—that is, it is moved from the right to the left side of the baseline model. The only (weakly) significant estimate is in Column 3, but both its magnitude and significance are much smaller than the baseline. Table A.6, Panel D repeats the same specifications for the main outcome period, showing that the result is robust to alternative specifications and that the models of the “placebo tests” in Table 2 are not chosen to pass the test.

Third, I split the sample along characteristics that may be associated with differential labor-demand trends. More than one-third of the EU12 migrants settled in the Oslo region (called East). The three largest immigrant commuting zones, Oslo, Bergen, and Stavanger, together received 56 percent of the net EU12 immigration between 2005 and 2011. I therefore drop the 3 and 10 largest EU12-commuting zones, respectively, and, as shown in Table A.5, estimate separately within Norway’s five major regions (Bhuller, 2009). The estimated immigration impact is similar across areas but somewhat larger in the densest EU12-migrant region. The estimate is similar across sectors, as well, although not statistically significant in the public sector (Table A.5). The construction sector estimate is slightly smaller than Bratsberg and Raaum (2012)’s wage estimate—possibly because of the different periods (i.e., they end when I start).

Finally, I add and alter potentially omitted control variables (Table A.6, Panel A). I add fixed effects for the nine aggregated occupation groups. The level of identifying variation changes from across all occupations to across occupations *within* each group. The baseline estimate survives controlling for the groups, although the significance and first stage diminish. I further allow for region-specific labor-demand changes within industries and include more detailed controls for industry and

<sup>22</sup> Cumulative earnings are not relative to previous earnings, as in the baseline model, because they would yield a highly selected sample, namely those employed for 7 years prior to the immigration surge. The estimate then would be less comparable across time and models.

education than in the baseline, as well as for years of completed education. The estimated immigration effect is highly robust to alternative controls. Table A.6, Panel E shows robustness to successively excluding each fixed effect of the baseline model. The immigration coefficient is unaltered when I drop industry but decreases when I drop commuting zone, revealing immigrants’ endogenous geographical allocation. The exercise further reveals education as an important control variable, as it likely positively correlates with both language requirements and earnings (at least in levels). In the same spirit, I control for intelligence in the subsample with ability scores from military examinations—with no change in the immigration coefficient (Panel A).

Motivated by Deming (2017), who finds that the labor market has increasingly remunerated social skills *in combination* with high cognitive abilities, I add several cognitive-ability measures (Table 3). Each column includes a cognitive measure, based on O\*NET data, separately and interacted with social skills, in addition to the baseline controls. In Column 5, I drop four verbal abilities from the cognitive composite because they closely resemble the baseline language index.<sup>23</sup> The EU12 coefficient is stable across all specifications but, interestingly, no interaction and only one separate coefficient (*reasoning*) is significant.

## 5.2. Heterogeneity

The estimated earnings effect represents an average over all occupations, but the true effect may be heterogeneous. In particular, institutional settings, such as licensing requirements, union arrangements, and minimum wages, can cause differential labor-supply elasticities and wage rigidities across occupations. The presence of heterogeneity further complicates interpretation of the results (Dustmann et al., 2016). Systematic differences among occupations with unequal language requirements could drive the estimate up or down. However, both minimum wages and licensing are present at all language-skill levels. For instance, both cleaners and salespersons have minimum wages; both plumbers and doctors need licenses. Minimum wages are nevertheless more common in low-language occupations and therefore likely to impose downward wage rigidity at the bottom of the language-skill distribution, reducing the earnings effect.

To investigate the role of minimum wages, I drop occupations they typically protect. Norway has no general minimum wage, but certain sectors have minimum wages through collective agreements.<sup>24</sup> Table A.5 shows that the point estimate is indeed larger without the most common occupations in these sectors.

Labor-supply elasticities are likely less heterogeneous within groups of similar workers. I therefore split the sample along several characteristics (Table A.5) and show that the estimate is insignificantly different for male and female workers. I group workers by education level in 2005: no/primary education, high school (12 years), or college/university degree. The effect increases with education level, although insignificant among top-skilled workers, who were either little exposed to migrant competition or able to counteract the effect, as in Peri and Sparber (2011). Workers with high-school education (more than half of the sample) in occupations without language barriers are most affected relative to other workers with high-school educations. The point estimates for low- and medium-education levels, as well as the insignificant for the top, are reassuringly close to Bratsberg and Raaum (2012).

<sup>23</sup> Peri and Sparber (2009) and Ottaviano et al. (2013), among others, use indeed these four abilities as a language measure. Table A.6 shows that the result is robust to employing an instrument based on the four abilities, as well as a dichotomous instrument for below-median language requirements.

<sup>24</sup> Generally applicable collective agreements are agreements concerning pay and working conditions that apply to everyone in a specific sector, regardless of unionization. Sectors/occupations with minimum wages are construction; maritime construction; agriculture and horticulture; cleaning workers; fish-processing enterprises; electricians; freight transport by road; passenger transport by tour bus; and hotel, restaurant, and catering.

**Table 3**  
Additional controls for occupational requirements.

	(1)	(2)	(3)	(4)	(5)
	b/se	b/se	b/se	b/se	b/se
EU12 share	−0.709*** (0.16)	−0.660*** (0.18)	−0.552** (0.18)	−0.695*** (0.18)	−0.738*** (0.17)
Social	0.007* (0.00)	0.002 (0.01)	0.001 (0.00)	0.005 (0.00)	0.006 (0.00)
Non-routine	0.003 (0.00)				
Social × non-routine	0.003 (0.00)				
Abstract		0.008 (0.01)			
Social × abstract		0.000 (0.00)			
Reasoning			0.012** (0.00)		
Social × reasoning			0.002 (0.00)		
Cognitive				0.005 (0.00)	
Social × cognitive				0.002 (0.00)	
Cognitive wo. verbal					0.003 (0.00)
Social × cognitive wo. verbal					0.002 (0.00)
R <sup>2</sup>	0.071	0.071	0.071	0.071	0.070
N	772,310	772,310	772,310	772,310	772,310

Each column is a separate estimation of the baseline model with additional control variables as follows: (1) "non-routine abilities" from Deming (2017), capturing mathematical competence; (2) "abstract tasks" (non-routine cognitive) from Acemoglu and Autor (2011); (3) 4 reasoning abilities, capturing non-routine abilities from O\*NET's content model; and (4) a composite of the 21 "cognitive abilities" from O\*NET. (5) equals (4) without four verbal abilities. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

Following Orrenius and Zavodny (2007), I group occupations by skill level and find a significant negative effect among manual laborers and no effect among professionals (Table A.5). The point estimate for manual occupations is identical to Orrenius and Zavodny (2007)—although they estimate total effects and I relative effects among manual laborers with unequal language protection. The different nature of the estimates may explain why I find a substantial effect among service workers and they none. Steinhart (2011), however, estimates negative total wage effects in service occupations.

As discussed in the Introduction, cross-elasticities of labor demand across cells may amplify the estimate (Dustmann et al., 2016). With a large number of cells, many potential cross-elasticities are at play. Therefore, in Table A.2, I aggregate occupations by the first two digits (Columns 1 and 2) and three digits (Columns 4 and 5) of the four-digit occupation codes, respectively providing 30 and 107 groups. Columns 2 and 5 are weighted by the size of the four-digit occupations within each group. The estimates for two-digit occupations are negative but insignificant—as expected, with only 30 groups. The estimates for three-digit occupations, both weighted and unweighted, are similar to the baseline. Columns 3 and 6 show robustness of the baseline model to higher level clustering—although arguably, 30 two-digit occupations are too few upon which to cluster.

In all, the checks in this section confirm that the adverse earnings impact of EU12 immigration is robust to alternative specifications and sample strata. They lend credence to the parallel earnings-trend assumption, suggesting that the estimation identifies the causal effect of immigration. Further tests are available upon request.

## 6. Mechanisms

To examine possible mechanisms behind the earnings effect, I estimate a set of employment and adjustment response outcomes in the

following 2SLS estimation.

$$\Delta EU12_{j,05-11} = \gamma_0 + \gamma_1 lang_j + X'_{i,05} \gamma_2 + \gamma_3 \tilde{Y}_{i,02-05} + \gamma_4 soc_j + \gamma_5 Y_{i,02-05} + \xi_{ij} \quad (4)$$

$$q_{ij} = \beta_0 + \beta_1^{IV} \widehat{\Delta EU12}_{j,05-11} + X'_{i,05} \beta_2 + \beta_3 \tilde{Y}_{i,02-05} + \beta_4 soc_j + \gamma_5 Y_{i,02-05} + \epsilon_{ij} \quad (5)$$

$q_{ij}$  measures changes from before (2002–2005) to after (2006–2011) the onset of the immigration surge in the following labor market outcomes: contracted weekly work hours, (approximated) hourly wages, full-time employment, (un)employment, and disability program participation, as well as change of employer, commuting zone, industry, sector, education, and occupation.<sup>25</sup> I include all control variables from the baseline model plus the logarithm of cumulative earnings from 2002 to 2005 (the denominator in the earnings outcome variable). In the wage estimation, the latter is substituted with wage growth from 2003 to 2005.<sup>26</sup>

### 6.1. Employment

Table 4 reveals adverse effects in all employment dimensions on natives less protected by language barriers relative to more protected natives. A one percentage point increase in the EU12 share reduces contracted weekly work hours (Column 1) by 0.12 percent (mean of 37 hours) and wages (conditional on employment) by 0.42 percent (Column 2).<sup>27</sup> The wage effect equals roughly half of the earnings effect.

<sup>25</sup> Hours and wages are measured relative to 2003 to 2005 due to poor data quality in 2002.

<sup>26</sup> Results are similar with earnings growth as the control.

<sup>27</sup> The coefficient on hourly wages should be interpreted with caution because the measure is a rough approximation based on contracted weekly work hours, duration, and annual cash payments of the main employment spell. Further, contracted and actual hours worked are not necessarily identical.



**Table 4**  
Employment outcomes.

	(1) Weekly hours b/se	(2) Hourly wages b/se	(3) Full-time employment b/se	(4) Employment b/se	(5) Unemployment insurance b/se	(6) Disability insurance b/se
EU12 share	-0.118* (0.058)	-0.424** (0.147)	-0.967*** (0.257)	-0.471*** (0.123)	0.446** (0.167)	0.563*** (0.084)
R <sup>2</sup>	0.028	0.029	0.053	0.043	0.114	0.164
N	757,815	747,537	772,310	772,310	772,310	772,310

Each column is a separate estimation of Eqs. (4) and (5) with outcome variables as follows: changes in log of average contracted weekly work hours (1) and hourly wages (2) in the main job from 2003–2005 to 2006–2011, the probability of full-time employment (3) and earnings above the employment threshold (4) each year in 2006–2011, and the probability of receiving unemployment (5) and disability (6) insurance in 2006–2011, controlling for receipt in 2002–2005. Baseline sample. Samples in (1) and (2) are somewhat reduced due to limited data on work hours. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table 5**  
Employment outcomes, by age groups.

	(1) Young b/se	(2) Middle aged b/se	(3) Old b/se
<i>Earnings</i>			
EU12 share	-0.972*** (0.166)	-0.569*** (0.145)	-0.701*** (0.206)
<i>Hourly wages</i>			
EU12 share	-0.635*** (0.187)	-0.249 (0.133)	-0.187 (0.154)
<i>Weekly hours</i>			
EU12 share	-0.100* (0.042)	-0.124* (0.063)	-0.166* (0.072)
<i>Full-time employment</i>			
EU12 share	-0.798*** (0.231)	-0.991*** (0.247)	-1.278*** (0.320)
<i>Employment</i>			
EU12 share	-0.385*** (0.112)	-0.340** (0.104)	-0.714*** (0.194)
<i>Unemployment insurance</i>			
EU12 share	0.533*** (0.147)	0.451** (0.169)	0.320 (0.199)
<i>Disability insurance</i>			
EU12 share	0.455*** (0.070)	0.457*** (0.079)	0.722*** (0.129)
N (earnings outcome)	252,114	271,572	248,600

Each cell is a separate estimation of Eqs. (4) and (5) for three age groups of workers in 2005: (1) 23–36 years, (2) 37–46 years, and (3) 47–53 years. See Table 4 notes for definition of the outcomes. Baseline sample. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Correspondingly, the probabilities of full-time employment (Column 3) and labor earnings above the employment threshold (Column 4) are reduced by 0.97 (mean of 80 percent) and 0.47 percentage points (mean of 92 percent). The employment effect is somewhat smaller than in Dustmann et al. (2017).

Finally, the probabilities of receiving unemployment insurance (Table 4, Column 5) and disability insurance (Column 6) for at least 3 months within a year are respectively 0.45 and 0.56 percentage points higher for each percentage point increase in the EU12 share. Multiplying them by the average difference in the increase in EU12 share (3.8 percentage points), yields respectively 1.7 and 2.1 percentage points higher unemployment and disability rates among occupations with below- than above-median language requirements. For comparison, the average unemployment rate over the period 2006 to 2011 was 3.4 percent and the disability rate was 8.0 percent in the (full) estimation sample. Similarly, Bratsberg and Raaum (2012) estimate substantial employment effects from immigration to the Norwegian construction sector, and Balsvik et al. (2015) from increased import competition from China.

Table 5 displays the heterogeneity of the employment effects across age groups. Not surprisingly, young workers (Column 1) are strongly affected relative to their (language-protected) peers in the earnings and

wage dimensions. This aligns with the literature, wherein immigrants commonly are found to compete more with young and low-skilled native workers (e.g., Borjas, 2003; Card, 2001). The wage impacts are insignificant among both middle-aged and older workers. The former (Column 2) are generally the least affected, whereas the latter (Column 3) are strongly affected in all employment dimensions except unemployment insurance receipt. This is expected because older workers tend to enter early retirement or (permanent) disability, rather than unemployment, as a consequence of the design of the Norwegian Social Security System. Dustmann et al. (2017) also find strong wage effects for young workers and strong employment effects for old workers.

## 6.2. Mobility

From standard economic theory, we expect that natives competing with immigrants respond by moving geographically or changing to less exposed jobs, for instance by upgrading skills. Surprisingly, the estimated effects on native mobility along most dimensions are insignificant. However, I find that low-language natives more often re-educate and change to occupations with higher language requirements. Table 6 shows that the (unconditional) probabilities of changing commuting zone, firm, industry, sector, and occupation differ insignificantly between workers with unequal immigrant exposure. All outcome variables equal 1 for individuals who change state at least once during the outcome period and 0 otherwise (including for workers who leave employment). I control for the same variable lagged.

The mobility results capture outflows and shifts of working natives, but not inflows or changes in occupational employment, which are potential important drivers of total immigration effects (e.g., Autor and Dorn, 2009; Dustmann et al., 2017). Because of limited occupation data (see Section 3) and because the estimation sample consists of only initially employed natives, changes in inflows or employment stocks cannot be detected. Improved registration of workers' occupations over the period cannot be separated from actual growth in occupations.

The insignificant occupational mobility results in Table 6, Column 5 must be interpreted with caution due to scarce occupation data.<sup>28</sup> I can not properly control for mobility rates prior to the immigration surge which should be included if they systematically differed along the language-requirement distribution for reasons unrelated to immigration. I nevertheless include a rough control for the share of workers who changed (non-missing) occupation between 2003 and 2005, based on the (non-representative) subset of workers with occupation data in 2003 and 2004. Table A.3 demonstrates that the immigration coefficient decreases in both magnitude and significance as the control for previous mobility improves (but it never reaches perfect). As evident from the negative coefficient, previous mobility was lower in occupations without language barriers (conditional on the controls).

<sup>28</sup> However, Bratsberg and Raaum (2012) also find no effect on job changes.

**Table 6**  
Mobility results.

	(1) CZ b/se	(2) Firm b/se	(3) Industry b/se	(4) Sector b/se	(5) Occupation b/se	(6) Education b/se
EU12 share	0.050 (0.144)	-0.407 (0.306)	0.083 (0.428)	0.124 (0.310)	-0.795 (0.720)	0.324** (0.107)
R <sup>2</sup>	0.120	0.110	0.147	0.113	0.087	0.036
N	772,310	772,310	772,310	772,310	772,310	772,310

Each column is a separate estimation of Eqs. (4) and (5) with outcome variables as follows: the probability of changing commuting zone (1), firm (2), industry (3), sector (4), occupation (5), and education (6) at least once during 2006–2011, controlling for the same in 2002–2005. In addition, (5) controls for the share of new workers in occupations from 2003 to 2005. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table 7**  
Probability of changing language requirements.

	(1) Higher req. b/se	(2) Lower req. b/se	(3) Earnings b/se
EU12 share	1.859* (0.913)	-2.875*** (0.735)	-0.560 (0.619)
Higher			0.022*** (0.004)
Lower			-0.034*** (0.006)
R <sup>2</sup>	0.027	-0.032	0.543
N	772,310	772,310	287,962

Column (1) and (2) are separate estimations of Eqs. (4) and (5) with the probability of changing to an occupation with higher (1) and lower (2) language requirements as outcome. Column (3) is the earnings effect (baseline model) among those who changed occupation, controlling for whether they faced higher or lower requirements in the new occupation. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Despite no effect on the occupational mobility of workers initially employed in less language intensive occupations, their probability of changing to an occupation with higher (*lower*) language requirements increased (*decreased*) significantly (Table 7, Columns 1 and 2). Furthermore, among workers who changed occupation (roughly 37 percent), the earnings effect of higher (*lower*) language requirements in the new occupation is positive (*negative*), as revealed by the coefficients in Rows 2 and 3 of Column 3.<sup>29</sup> This aligns with Peri and Sparber (2009) and Foged and Peri (2016), who find that in response to immigration, low-skilled natives specialize into less manual-intensive and more communication-intensive occupations with higher wages. The (relative) earnings effect among occupation switchers (without the “bad controls”) is much smaller than in the full sample, albeit insignificant (Table A.5). Occupation stayers have the largest point estimate, although both it and the estimate for occupation leavers (including those with occupation data gaps) are insignificantly different from 0, as well. The insignificance indicates that the earnings effect margin is *across* subsets, for example, between stayers and language upgraders or between non-employed and occupation switchers.<sup>30</sup>

A second indication of natives’ ongoing adjustment responses is the decreasing earnings effect over time (Table A.4). However, adjustments may be in the form of reduced inflow into occupations (which I cannot detect) as opposed to increased outflows (which I find to be insignifi-

cant). The sharp decrease in the wage effect and the relatively stable employment effect over time points to the importance of the employment component of the (medium-run) earnings effect. As expected, wages are the most important adjustment mechanism in the short run, whereas employment is more important in the long run. Both effects tend to 0 in the very long run.

To summarize, I find that young workers employed in occupations without language barriers suffer most in the earnings and wage dimension, and older workers in the (intensive and extensive margin) employment dimension, particularly through increased disability program participation. This parallels Dustmann et al. (2017)’s conclusions. Inhabitants of the capital region, which had the largest EU12 migrant inflow, are as expected strongly affected, as are workers in service occupations and occupations without minimum wages. In contrast, public sector and high-skilled workers are not significantly affected—possibly because those occupations require other formal qualifications that hinder most EU12 immigrants from entry regardless of language requirements.

Affected native workers experience losses in both wages and work time, when employed, as well as reduced employment. To some extent, they re-educate or advance in the occupational language requirements, dampening the adverse effects. Surprisingly, I find no other mobility response among workers exposed to immigration, possibly because both the immigration surge and its impacts were sluggish. Stickiness in workers’ mobility (and capital) is indeed crucial for heterogeneity of immigration impacts. With perfect mobility, the effect would be identical for all, and my estimates would be 0.

## 7. Conclusion

To empirically identify immigration impacts on receiving labor markets is challenging because of simultaneity issues. Immigrants do not take jobs at random and natives may respond to increased migrant competition. I introduce a novel source of immigration variation at the occupational level that is arguably exogenous to labor-demand changes, namely occupations’ requirements for language skills. Language requirements vary substantially among occupations and hinder non-Norwegian speaking immigrants from entering language-intensive occupations.

I show that language barriers distributed EU12 immigrants unevenly across occupations during the historically large immigration surge to Norway after the EU enlargement in 2004. The EU12 immigrants’ poor language skills make language requirements a particularly well-suited instrument for the endogenous allocation of this group in the Norwegian labor market. The results show that relative to protected natives, natives without language protection experience a substantial earnings loss from increased migrant competition. Identified channels for the earnings effect include reduced wages and employment on both the intensive and extensive margin, as well as increased disability program participation. I find evidence of skill-upgrading through re-education and advancements in language requirements, but no other adjustment responses.

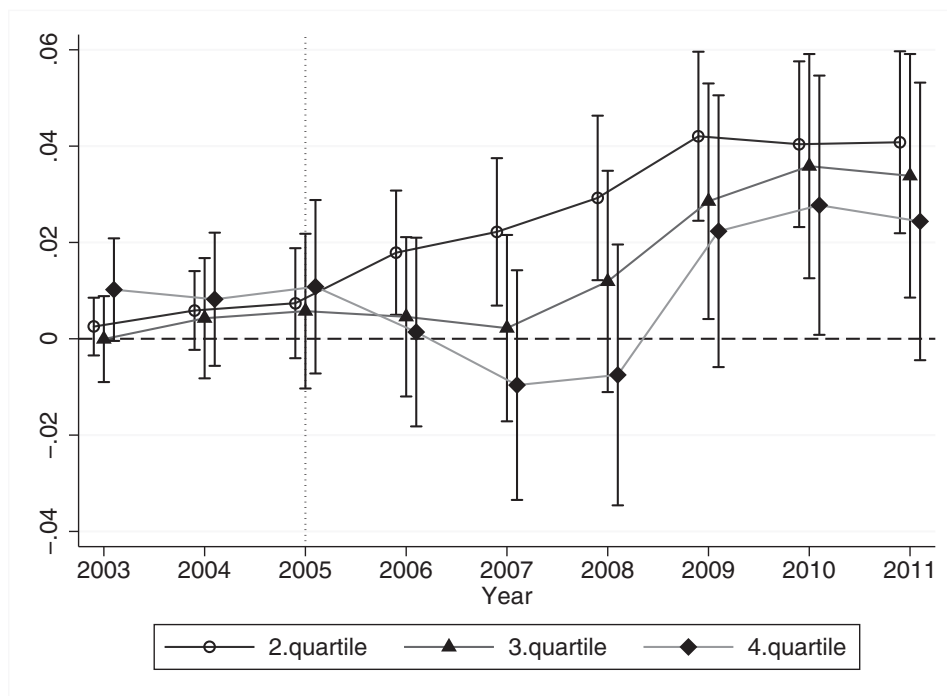
<sup>29</sup> The variables *higher* and *lower* are endogenous and thereby “bad controls” (Angrist and Pischke, 2008), possibly biasing the earnings estimate. Table A.5 shows the reduced estimate when they are dropped.

<sup>30</sup> The result may be biased as I stratify on an endogenous variable (Abadie et al., 2018).

Unprotected workers are affected because poor language skills force EU12 migrants to enter occupations without language barriers. Hence, possible policy recommendations include providing language courses and integration programs for EU12 immigrants to improve their own employment possibilities and thereby benefit natives employed in less language-intensive occupations. Increased employment protection and minimum wages through generally applicable collective agreements could furthermore enhance overall welfare because the earnings effect is smaller among occupations typically covered by such agreements and

because immigrants are commonly less unionized. However, more research is needed to conclude on the total effects of applying collective agreements to more sectors. Finally, these recommendations may or may not change the *total* earnings impact of immigration, but improved integration arguably benefits both the immigrants and the Norwegian society as a whole.

## Appendix



**Fig. A.1.** Average (residual) earnings in language quartiles, relative to the lowest quartile. *Note.* The figure shows the  $\alpha_1$ -coefficients from Eq. (3) on quartile-by-year fixed effects together with 95 percent confidence intervals. The coefficients are relative to the lowest language quartile in 2002. To minimize compositional changes in the quartiles over time, I limit the sample each year to full-time employees with annual earnings above 1.5 G, not in education, and of age 23–62 years. Workers' occupation as well as all controls vary over time, but the occupations constituting each quartile are constant.

**Table A.1**  
Descriptives of estimation sample in 2005.

	Estimation sample Mean (SD)	Missing occupation data Mean (SD)
Earnings (1,000 NOK)	411.9 (234.9)	364.6 (158.6)
Earnings growth 2002–2005	6.3 (38.3)	6.3 (41.1)
Male share	65.3 (47.6)	45.9 (49.8)
Age	41.3 (8.7)	42.6 (8.7)
Experience (years)	20.3 (8.7)	20.2 (8.6)
Tenure (years)	5.7 (4.3)	4.6 (4.0)
Education (years)	13.3 (3.0)	14.5 (2.9)
Public sector share	29.6 (45.6)	90.7 (29.0)
Weekly work hours	37.0 (2.0)	36.8 (2.9)
EU12 share	0.4 (0.4)	N.A.
$\Delta$ EU12 2005–2011	2.3 (4.3)	N.A.
N	772,310	82,069

Estimation sample includes native residents born 1949–1982, not in education in 2005, and employed full-time with annual labor earnings above 1.5 G during 2002–2005. Column 3 shows the sample of workers fulfilling these criteria but with missing occupation data in 2005.

**Table A.2**  
Aggregating occupations.

	(1) (2) (3) Two-digit occupation groups			(4) (5) (6) Three-digit occupation groups		
	b/se	Weighted b/se	Clustered b/se	b/se	Weighted b/se	Clustered b/se
EU12 share	-0.252 (0.201)	-0.246 (0.214)	-0.745** (0.207)	-0.601** (0.205)	-0.576* (0.271)	-0.745*** (0.174)
R <sup>2</sup>	0.071	0.065	0.070	0.071	0.065	0.070
F (first stage)	11.77	15.45	11.26	28.86	28.31	19.98
Number of groups	30	30	30	107	107	107

Estimations of baseline model with occupations aggregated into groups by the first two (1–3) and three digits (4–6) of the occupation codes. (2) and (5) are weighted with the size of the four-digit occupations within each group. (3) and (6) are estimated with four-digit occupations and standard errors clustered at two- and three-digit levels, respectively. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A.3**  
Occupational mobility with alternative controls.

	(1) No b/se	(2) Individual b/se	(3) Share 04–05 b/se	(4) Share 03–05 b/se	(5) Individual + share 03–05 b/se
EU12 share	-1.928** (0.740)	-1.738* (0.713)	-1.374* (0.693)	-0.805 (0.721)	-0.795 (0.720)
R <sup>2</sup>	0.065	0.072	0.081	0.085	0.087
N	772,310	772,310	772,310	772,310	772,310

Each column is a separate estimation of Eqs. (4) and (5) with the probability of changing occupation from 2005 to 2011 as outcome and different controls for previous mobility: no control (1), indicator for individual occupation shift from 2003 to 2005 (2), and the share of new workers in occupations over 2004–2005 (3) and 2003–2005 (4). (5) combines (2) and (4). \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A.4**  
Alternative outcome periods.

	(1) 2005–2007 b/se	(2) 2005–2008 b/se	(3) 2005–2009 b/se	(4) 2005–2010 b/se	(5) 2005–2012 b/se	(6) 2005–2013 b/se
<i>Earnings</i>						
EU12 share	-2.098** (0.776)	-1.391*** (0.415)	-1.075*** (0.252)	-0.913*** (0.199)	-0.659*** (0.134)	-0.626*** (0.123)
<i>Hourly wages</i>						
EU12 share	-2.443** (0.936)	-1.263** (0.475)	-0.811** (0.270)	-0.593** (0.201)	-0.350** (0.121)	-0.302** (0.104)
<i>Employment</i>						
EU12 share	-0.622* (0.308)	-0.505* (0.201)	-0.504** (0.162)	-0.506*** (0.137)	-0.443*** (0.110)	-0.429*** (0.104)
R <sup>2</sup>	0.056	0.057	0.061	0.064	0.078	0.088
N	771,958	772,130	772,214	772,263	772,299	772,292

Estimations of Eqs. (4) and (5) with earnings, hourly wages, and employment as outcome and with varying outcome periods. Changes in earnings and EU12 shares are measured over the specified period above each column. Variables as defined in Tables 1 and 4. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A.5**  
Subset estimation.

Subset	EU12 Share	SE	N
Without top 3 EU12 CZ	-0.591***	0.152	379,488
Without top 10 EU12 CZ	-0.471**	0.143	224,353
East region	-1.037***	0.215	378,555
South region	-0.527*	0.237	58,782
West region	-0.520***	0.130	195,863
Midregion	-0.743***	0.211	71,562
North region	-0.554**	0.207	56,130
Public sector	-0.739	0.460	228,330
Private sector	-0.773***	0.125	543,924
Construction sector	-0.594***	0.078	65,598
Ten percent largest firms	-0.910***	0.147	345,607
Below language median	-0.551***	0.101	349,212
Excl. min.wage occupations	-1.204**	0.267	665,671
Men	-0.770***	0.119	504,522
Women	-0.816*	0.358	267,777
No education	-0.518***	0.137	122,974
High school	-0.829***	0.138	400,065
College/University	-1.474	1.206	249,271
Manual occupations	-0.312**	0.112	221,320
Service occupations	-3.686*	1.540	173,668
Professional occupations	2.485	6.092	377,287
Occupation stayers	-1.312	0.682	360,748
Occupation leavers	-0.895	0.577	123,573
Occupation switchers	-0.196	0.658	287,962
Higher language	-0.232	0.452	132,261
Lower language	-1.530	0.805	95,301

Each row is a separate estimation of the baseline model within subsets of the baseline estimation sample. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A.6**  
Robustness checks of the earnings effect.

Panel A	Control variable	EU12 share	SE	F-statistic
	Aggregate occupation groups	-0.778*	0.381	9.9
	Industry $\times$ region	-0.760***	0.154	34.8
	Three-digit industry	-0.751***	0.169	37.4
	Six-digit education	-0.749***	0.153	34.9
	Years of education	-0.770***	0.175	32.0
	Intelligence	-0.708***	0.121	30.5
Panel B	Instrument	EU12 share	SE	F-statistic
	Without foreign language	-0.630***	0.147	22.8
	Verbal abilities	-1.004***	0.194	22.7
	Binary (below median)	-1.112**	0.391	6.9
Panel C	Immigrant share measure	EU12 share	SE	F-statistic
	Change rel. to 2005-count	-0.647***	0.140	32.0
	Share in 2011	-0.707***	0.149	33.7
Panel D	Outcome variable	EU12 share	SE	R <sup>2</sup>
	Log earnings	-1.719*	0.696	0.333
	Log earnings, growth control	-1.667*	0.682	0.341
	Earnings growth	27.29	25.79	0.000
Panel E	Excluded fixed effects	EU12 share	SE	R <sup>2</sup>
	Industry	-0.662**	0.206	0.065
	Commuting zone	-0.285***	0.154	0.068
	Education	-1.189***	0.222	0.059
	Demographics	-0.589***	0.123	0.062
	Experience	-0.796***	0.154	0.060
	Earnings growth	-0.791***	0.163	0.057

Each row is a separate estimation of the baseline model with an additional or altered control variable (Panel A), an alternative instrument (Panel B), immigration measure (Panel C), or outcome variable (Panel D). Panel E successively excludes the baseline model's fixed effects. Reported F-statistics are Kleibergen-Paap rk Wald F statistics. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

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