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# Immigration Wage Effects by Origin*

*Bernt Bratsberg*

Ragnar Frisch Centre for Economic Research, NO-0373, Oslo, Norway
bernt.bratsberg@frisch.uio.no

*Oddbjørn Raaum*

Ragnar Frisch Centre for Economic Research, NO-0373, Oslo, Norway
oddbjorn.raaum@frisch.uio.no

*Marianne Røed*

Institute for Social Research, NO-0208, Oslo, Norway
marianne.roed@samfunnsforskning.no

*Pål Schøne*

Institute for Social Research, NO-0208, Oslo, Norway
pal.schone@samfunnsforskning.no

## Abstract

We estimate the direct partial wage effects of immigrant-induced increases in labor supply, using the national skill cell approach with longitudinal records drawn from Norwegian administrative registers. The results show overall negative but heterogeneous wage effects, with larger effects on immigrant wages than on native wages and with native wages more responsive to inflows from Nordic countries than from developing countries. These patterns are consistent with natives and Nordic citizens being close substitutes, while natives and immigrants from developing countries are imperfect substitutes. Estimates are sensitive to accounting for effective immigrant experience, selective native participation, and variation in demand conditions and native labor supply.

*Keywords*: Immigration; labor-market effect; wages

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## I. Introduction

During the past decades, the immigrant share of the population has increased substantially in most high-income countries. An even more striking development is that the composition of the immigrant population

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has changed radically with increasing inflows from low-income countries (Bauer *et al*., 2000; Blau *et al*., 2011). An important question is how these labor supply shocks cause adjustments in wages and employment among residents. Solid evidence on how immigration from different origins affects the labor market is necessary for both an evaluation of immigration policy and an insight into drivers of economic development more generally.

We are interested in the wage effects of immigration. We estimate wage effects using data from Norway, where the immigrant population share has increased from 2 to 12 percent over the last 30 years. Prior to the 1980s, most immigrants came from countries that are geographically and culturally close. Today, the majority of the immigrant population originates in countries much more distant in both respects. A theory of equilibrium wages based on a standard labor demand and supply framework predicts that an inflow of immigrant labor into a certain skill group will reduce the relative wage of native workers belonging to that group. However, it also predicts that the size of the wage reduction will depend on the degree of substitutability across skill groups as well as between immigrant and native workers with similar skills. Immigration will also influence wages via product demand and prices. The negative wage effect will be mitigated by the expansion of the consumer base, a channel that is more important the less elastic product demand is (Borjas, 2009).

In this paper, we seek to identify the direct partial wage effects of immigration – the effects of immigration-induced labor supply shocks on the wages of residents (whether natives or earlier immigrants) with similar skills as the newcomers, given the supply of other factors and aggregated supply (see the discussion of partial and total wage effects by Ottaviano and Peri, 2008) – and to assess the heterogeneity of wage effects depending on the origin of the immigrant inflow. Interpreted within a labor demand framework derived from a three-level constant elasticity of substitution (CES) production technology, the combination of direct partial wage effects for natives and immigrants will identify the elasticity of substitution between immigrants and natives with similar skills. The elasticity of substitution between immigrant and native labor is a crucial determinant of how wage adjustments, following immigrant supply shocks, are distributed across groups of workers. To study wage effects, we apply the national skill cell approach (Borjas, 2003); that is, we delineate market clusters by education, work experience, and year of observation. Immigrant labor supply shocks are captured by changes in the share of foreign-born workers within each cluster, and the wages of individual native and immigrant workers are modeled as a function of the immigrant share in their skill group.

The paper contributes to European national-approach studies as well as to the general body of literature on wage effects of immigration. A novel contribution is that we estimate wage effects using a population register-based dataset with individual panel information. Although, when compared to alternative methods, the national skill cell approach by design reduces the influence of endogenous native responses, the methodology remains susceptible to bias if native labor-market participation relates to immigrant supply shocks. For example, if immigrant supply shocks affect wages and employment opportunities, a major concern for wage studies is that native attrition might be non-random. In particular, if any native displacement is dominated by low-wage workers, the within-skill cell composition of native workers will improve following an immigrant labor supply shock, rendering a positive bias in estimators that fail to account for compositional change in the data (Bratsberg and Raaum, 2012). An important advantage of the panel structure of our data is that it allows us to address any selective native employment where unobserved worker characteristics are correlated with the immigrant share within skill cells.

As in Borjas (2003) and following the national approach, we include fixed effects for education, experience, and year of observation, as well as the interactions between these variables in order to capture any differential trends in wages by education and experience and returns to experience that depend on educational attainment. The empirical model also controls for within-cell variation in native labor supply. Demographic change caused by variation in birth cohort size and expansions of the education system will mechanically affect cell-specific measures of the immigrant share. When native supply shocks also affect wages, failure to account for demographic change might induce bias in estimates of the immigration wage effect. Finally, we allow for within-cell variation in labor demand by including skill-group specific indicators for the business cycle, based on detailed individual unemployment records. If immigrant inflows are responsive to skill-group specific labor demand shocks, this is likely to impart positive bias in estimates that ignore the correlation between demand conditions and the immigrant share, leading to an understatement of the effect of immigration on native wages.

An important challenge to the national approach is to allocate immigrants to the appropriate education–experience cell. The problem will be accentuated by the high rates of non-employment among immigrants from developing countries, particularly during the first period after arrival, as observed in many European host countries. Because our data contain earnings records years back, we calculate effective experience and allocate immigrants from developing countries into experience cells on the basis of years of actual employment in Norway rather than years since arrival or potential labor-market experience.

Finally, we investigate the wage effects of disaggregated inflows from major regions of origin, such as developing countries, the neighboring Nordic countries, and other high-income countries outside the Nordic region. Immigrants from diverse source countries and cultures are expected to differ in their substitutability with native workers. While migrants from the neighboring countries share language and culture, and bring work experience and educational attainment from similar institutions, long-distance immigrants from developing countries differ along these dimensions and are therefore less likely to be (perfect) substitutes for native workers. Differences in admission categories add to cultural and linguistic factors in explaining why substitutability and labor force participation vary by origin region. Since the 1950s, immigrants from the Nordic countries have benefitted from a common labor market with no restrictions on migration. Immigrants from other high-income countries often arrive because they are actively recruited into particular jobs by domestic employers, while immigrants from developing countries are more likely to be admitted on the basis of refugee status or family reunification. Therefore, wage effects are likely to vary by immigrant origin because of differences in substitutability with native workers. Furthermore, the various immigrant groups differ greatly in terms of migration costs, which might lead to differences across groups in the importance of accounting for confounding factors. In particular, labor migrant inflows from the nearby Nordic countries might be expected to be more sensitive with respect to variation in demand conditions than inflows from developing countries.

A consequence of heterogeneity in immigrant–native substitution is that the effect of immigrant supply shocks on the wage structure will not only depend on the educational composition of immigrant inflows (Card, 2009), but also the origin mix within education groups. From a European immigration policy perspective, in particular, the assumption of a common wage effect can be misleading, because it is important to distinguish between wage effects of flows that are subject to regulation (i.e., admission of third country nationals) and those arising from free labor movements within a common labor market.

## II. Empirical Body of Literature

Wage and employment effects of immigration have typically been studied empirically by the spatial approach, in which labor-market clusters are delimited by geographical areas within the receiving country and where identification of labor-market effects of immigration draws on variation in immigrant intensity across regions. Sometimes combined with a skill dimension (e.g., Card, 2001), the spatial approach will generate substantially more cross-sectional variation in immigrant labor supply measures than approaches based on national labor-market clusters. However, because regional boundaries are easier to cross than national borders, endogenous location, whereby immigrants seek out areas with relatively favorable labor demand conditions, presents a challenge to identification in studies of local labor-market effects. Moreover, if native workers respond to high immigrant inflows by moving out of – or not into – a certain area, the wage effect will leak from the local to the national labor market. Both mechanisms predict a positive bias in estimates of the wage effect when based on variation in immigrant labor across space. To deal with this simultaneity problem, researchers have applied instrumental variable techniques and have explored natural experiment situations (Card, 1990, 2009; Hunt, 1992; Friedberg, 2001; Dustmann *et al.*, 2013). Reviews of a vast body of research literature – of which the greater part is based on US data – conclude that spatial approach studies find small and often insignificant wage effects of immigration (some examples of literature reviews are Greenwood and McDowell, 1986; Friedberg and Hunt, 1995; Longhi *et al.*, 2005; Okkerse, 2008).[1]

The national approach was introduced by Borjas (2003) in order to circumvent the problem of endogenous mobility between clusters. Individual attachment to a national skill group defined by education and experience will largely be determined by educational choice. Ignoring endogenous labor-market participation, aggregate time series reduce problems related to selective location of immigrants and endogenous native mobility. Using data from a single host country, there is however only one observation of the national labor-market cluster at each point in time. Thus, one important objection to the approach is that it might confound immigration with other skill-group specific labor supply or demand shocks that affect relative wages over time. One candidate confounding factor is skill-biased technological change that might have increased the demand for relatively young and highly skilled natives. Another candidate (on the supply side of the labor market) is caused by changing labor force participation within skill cells that might have altered the composition of individual unobserved characteristics over time.

Using the national approach to analyze immigration to the US, Borjas finds considerably more negative wage effects of immigration than preceding studies drawing on spatial variation. To interpret the skill-cell correlations and to assess the total effect of immigration on wages, Borjas applies a nested CES production function framework and concludes that

[1] Zorlu and Hartog (2005) use the spatial approach to analyze the effects of immigration on native wages in three European countries: The Netherlands, UK, and Norway. For Norway, their analyses are based on cross-sectional data from 1989 and 1996 taken from a small data extract from the Norwegian population registers (the KIRUT database). When they pool all immigrants, they find a positive correlation between immigration intensity in 19 regions and wages of low- and medium-skilled natives. Their results further indicate a positive correlation between native wages and immigration from European Union countries, but a negative correlation with immigration from Nordic countries.

the direct partial wage effect of immigration is negative and that immigration to US between 1980 and 2000 reduced the wage of the average native worker by approximately 3 percent. Aydemir and Borjas (2007) have studied immigration wage effects using the national approach and data from three countries (the US, Canada, and Mexico), and have found numerically comparable and statistically significant wage effects of immigration in each country and in the same range as the original study by Borjas (2003).

Also applying the national approach to US data, Ottaviano and Peri (2008, 2012) have concluded that the wage effect of immigration is much smaller than the effects uncovered by previous studies using the same methodology. They have extended the structural modeling approach of Borjas (2003) to allow for capital adjustments and imperfect substitutability between native and immigrant workers. Their estimate of the direct partial effect of immigration on the wage of natives is negative, but close to zero. With regard to the total effect, they have deduced that immigration to the US between 1990 and 2006 reduced the average native wage by 0.4 percent in the short run but increased the native wage by 0.6 percent in the long run. However, the corresponding effects on wages of earlier immigrants are clearly negative (Ottaviano and Peri, 2008).

European studies that build on the national approach and the CES framework include D'Amuri *et al*. (2010) and Manacorda *et al*. (2012).[2] D'Amuri *et al*. have examined the effects of immigrant inflows to Germany between the late 1980s and the early 2000s. To account for institutional frictions in the German labor market, they have investigated immigration effects on both wages and employment. They have found very small negative effects for natives, but sizable adverse effects on both wages and employment of earlier immigrants. Manacorda *et al*. have analyzed the effect of immigration on the wages of male workers in the UK, using data from the mid-1970s to the mid-2000s. As in the studies by D'Amuri *et al*. (2010) and Ottaviano and Peri (2008, 2012), they have failed to uncover discernible negative effects on native wages but have reported sizable negative effects on the wages of earlier immigrants.

Since Borjas (2003) introduced the multilevel CES production function into the national approach context, the focus in the literature has turned to estimating central parameters of the CES function and, in particular, the elasticity of substitution between native and immigrant workers with similar skills. The size of this substitution parameter has important implications. In the case of perfect substitution, the direct partial wage effect of

[2] Two recent studies that apply the national approach with labor-market cells defined by occupations are Amuedo-Dorantes and de la Rica (2013), using data from Spain, and Steinhardt (2011), using data from Germany.

immigration is negative; that is, given the composite values of all factors in the CES production function, a labor supply shock from immigration implies an unambiguous movement downward along the demand curve for native labor with similar skills as the newcomers. In the case of imperfect substitution, the negative partial effect on the native wage will be smaller, and might even be reversed by a complementarity effect on labor demand. Wage adjustments will be larger for earlier immigrants who are more exposed to labor-market competition from newly arrived immigrants. A core discrepancy across recent wage effect studies is the reported substitutability between immigrants and natives. Analyzing US data, Jaeger (2007), Borjas *et al*. (2008, 2010), and Aydemir and Borjas (2007) all conclude that the evidence points in the direction of perfect substitution, while Ottaviano and Peri (2008, 2012) uncover evidence of imperfect substitution. The results of Ottaviano and Peri (2012) are disputed by Borjas *et al*. (2012) who, based on the same data but with slightly different empirical specifications, report much higher elasticities of substitution between immigrants and natives with similar skills. Analyzing European data, both Manacorda *et al*. (2012) and D'Amuri *et al*. (2010) conclude that there is imperfect substitution between natives and immigrants with the same age and educational attainment. Dustman and Preston (2012) emphasize that pre-assignment of immigrants into skill cells based on observable characteristics such as age and education is problematic when immigrant skills are downgraded in the host-country labor market. Showing that newly arrived immigrants in the UK and the US are overrepresented in lower parts of the wage distribution than natives with the same characteristics, they argue that failure to account for downgrading of immigrant skills will induce bias in estimates of the elasticity of substitution between natives and fully integrated immigrants.

## III. Theoretical Background and Empirical Framework

According to standard neoclassical theory, an increase in the supply of one type of skill has a negative effect on the marginal product, and thus the competitive wage, of workers holding skills that are close substitutes (Borjas, 2009). At the same time, the supply shift will raise the marginal product, and the wage, of workers with skills that are complementary in production to the type that becomes more abundant. Accordingly, the skill composition of immigrants relative to the native workforce is of vital importance for the total wage effect of immigration.

It has become common in the empirical body of literature assessing wage effects of immigration to interpret reduced-form regression coefficients within a structural framework of one-output, nested, CES production technology. Ignoring capital, the total product in year $t$ ($Q_t$) depends on labor ($L_t$) and a technology parameter ($B_t$):

$$Q_t = B_t L_t^{\alpha}. \tag{1}$$

Total labor ($L_t$) is a composite of different skill groups aggregated by a nested CES technology with three (or two) levels (Card and Lemieux, 2001; Borjas, 2003; Ottaviano and Peri, 2008, 2012; Manacorda *et al.*, 2012). At the highest level, labor is the aggregate of $E$ levels of education ($L_{et}$),

$$L_t = \left[ \sum_{e=1}^{E} a_{et} L_{et}^{\rho} \right]^{1/\rho}, \tag{2}$$

where $a_{et}$ reflects the relative efficiency of education level $e$ and $L_{et}$ is the number of workers with education $e$ in year $t$. The substitution parameter, $\rho = 1 - \sigma_E^{-1}$, where $\sigma_E$ is the elasticity of substitution between labor with different levels of education. Labor input in each education group is, in turn, a CES combination of $J$ experience groups

$$L_{et} = \left[ \sum_{j=1}^{J} b_{ejt} L_{ejt}^{\tau} \right]^{1/\tau}, \tag{3}$$

where $b_{ejt}$ reflects the relative efficiency of different experience groups for each education group in year $t$. Here, $L_{ejt}$ is the number of workers with education $e$ and experience $j$ in year $t$, and $\tau = 1 - \sigma_J^{-1}$, where $\sigma_J$ is the elasticity of substitution between experience groups. Finally, each education experience group is a CES composite of immigrant ($M_{ejt}$) and native ($N_{ejt}$) workers,

$$L_{ejt} = \left[ N_{ejt}^{\lambda} + c_{ejt} M_{ejt}^{\lambda} \right]^{1/\lambda}, \tag{4}$$

where $c_{ejt}$ reflects the relative efficiency of immigrants within skill group. The parameter $\lambda = 1 - \sigma_M^{-1}$, where $\sigma_M$ is the elasticity of substitution between natives and immigrants within skill group ($e,j$).

In a competitive market, the wage of a given type of (here, native) labor equals its marginal product and

$$\ln W_{ejt}^{N} = q_{et} + \ln b_{ejt} + \left( \sigma_M^{-1} - \sigma_J^{-1} \right) \ln L_{ejt} - \sigma_M^{-1} \ln N_{ejt}, \tag{5}$$

where

$$q_{et} = \ln \left( \alpha Q_t L_t^{-\rho} a_{et} L_{et}^{\rho - \tau} \right).$$

Our focus is on the effect of an immigrant inflow on the wage paid to the same native skill group (Borjas, 2003, Part I). This is the direct partial wage effect (Ottaviano and Peri, 2008) resulting from an immigrant-induced increase in supply, holding native labor supply, aggregate supplies

($q_{et}$ in equation (5)), and capital constant (Borjas, 2009). Within the present theoretical framework, the direct partial wage effect of immigration can be expressed by the elasticity

$$\left.\frac{\partial \ln W_{ejt}^{N}}{\partial \ln M_{ejt}}\right|_{L_t, L_{et}\,\text{const}} = (\sigma_M^{-1} - \sigma_J^{-1})\eta_{ejt}, \tag{6}$$

where $\eta_{ejt}$ is the immigrant share of the wage bill in group (*e,j*) in year *t*. The scaling by the wage bill share reflects that the direct partial wage elasticity, in equilibrium, depends on the effect of the immigrant labor supply shock on the effective labor supply because $\eta_{ejt} = d\ln L_{ejt}/d\ln M_{ejt}$ (see Manacorda *et al*., 2012, p. 149).

Equation (6) demonstrates that the native wage effect of an immigration-induced labor supply shock will be negative only if $\sigma_M > \sigma_J$, or if within-group substitution dominates cross-group substitution. When $\sigma_M = \infty$ and $\sigma_M^{-1} = 0$, there is perfect substitutability between immigrants and natives within skill group and the partial elasticity in equation (6) can be interpreted as the slope of the demand curve for labor of skill group (*e,j*). In this case, the change in the immigrant share works as an instrument for an increase in labor supply within skill cell, and any resulting wage adjustment will identify the slope of the labor demand curve.

In the case of imperfect substitution within skill group (i.e., $\sigma_M^{-1} > 0$), the elasticity in equation (6) will reflect a combination of a movement down the demand curve for native workers of type *(e,j)* and a positive shift in this curve. We see from equation (6) that a lower elasticity of substitution between natives and immigrants will give a smaller (less negative) native wage effect. The intuition is that a larger part of the wage structure adjustment will be taken by immigrant labor when substitutability with natives is imperfect. This is easily seen from the first-order condition for immigrant labor:

$$\ln W_{ejt}^{M} = q_{et} + \ln b_{ejt} + \ln c_{ejt} + \left(\sigma_M^{-1} - \sigma_J^{-1}\right) \ln L_{ejt} - \sigma_M^{-1} \ln M_{ejt}. \tag{7}$$

This implies that the immigrant wage response to an immigrant supply shock is given by

$$\left.\frac{\partial \ln W_{ejt}^{M}}{\partial \ln M_{ejt}}\right|_{L_t, L_{et}\,const} = -\sigma_M^{-1}(1 - \eta_{ejt}) - \sigma_J^{-1}\eta_{ejt} < 0. \tag{8}$$

Some recent empirical studies, such as Ottaviano and Peri (2012), using US data, and Manacorda *et al*. (2012), using UK data, indicate imperfect substitutability within skill group, based on the finding that the wages of (previously arrived) immigrants relative to natives within skill group drops in response to a positive immigrant supply shock.

To estimate the direct partial wage effect for native workers, we follow Borjas (2003) and use educational attainment and work experience to classify individuals into (four levels of education $*$ eight experience groups $=$) 32 skill groups. Immigrant supply shocks are measured within skill groups. For workers with educational attainment $e$, experience level $j$, and observed in year $t$, the immigrant supply shock is defined as

$$P_{ejt} = \frac{M_{ejt}}{M_{ejt} + N_{ejt}},$$

where $M_{ejt}$ and $N_{ejt}$ denote the number of immigrants and natives in cell $(e,j,t)$. While the supply shocks are specific to the skill group, we use individual level data and the empirical set-up is the wage regression model,

$$\begin{aligned} \ln W_{iejt}^{N} &= \theta^{N} P_{ejt} + s_e + x_j + \pi_t + (s_e \cdot x_j) + (s_e \cdot \pi_t) + (x_j \cdot \pi_t) \\ &\quad + \gamma Z_{ejt} + u_{iejt}, \end{aligned} \tag{9}$$

where $W_{iejt}^{N}$ is the wage of native worker $i$ with education $e$ and experience $j$ in year $t$. The vectors of fixed effects are given by $s_e$ for education, $x_j$ for experience, and $\pi_t$ for calendar year. The interactions $s \cdot \pi$ and $x \cdot \pi$ control for any education and experience-specific wage trends and the $s \cdot x$ interaction allows for different wage–experience profiles across education groups. Any effects of changes in total and education specific labor supply summarized by $q_{et}$ in equation (5) will be captured by the education-specific year fixed effects. The interactions with differential time patterns will also account for demand shocks that are shared within education and experience levels.

Comparing the theory-based elasticity expression in equation (6) with that derived from the empirical model in equation (9), we obtain[3]

$$\left(\sigma_M^{-1} - \sigma_J^{-1}\right)\eta_{ejt} = \theta^{N} P_{ejt}(1 - P_{ejt}), \tag{10}$$

which illustrates that a given estimate of the native wage effect of immigration will be compatible with numerous combinations of substitution elasticities between and within experience groups. Estimating a wage equation for immigrants analogous to that in equation (9) provides further insight, as, from equation (8),

$$\left(\sigma_M^{-1} - \sigma_J^{-1}\right)\eta_{ejt} - \sigma_M^{-1} = \theta^{M} P_{ejt}(1 - P_{ejt}). \tag{11}$$

Combining equations (10) and (11), we obtain

$$\sigma_M^{-1} = -(\theta^{M} - \theta^{N}) P_{ejt}(1 - P_{ejt}) \tag{12}$$

[3] The expression on the right-hand side follows from differentiating equation (9) with respect to $\ln M$: $\frac{\partial \ln W}{\partial \ln M} = \frac{\partial \ln W}{\partial p}\frac{\partial p}{\partial M}\frac{\partial M}{\partial \ln M} = \theta \frac{N}{(M+N)^2} M = \theta p(1-p)$.

and

$$\sigma_J^{-1} = \left( (1 - \frac{1}{\eta_{ejt}})\theta^N - \theta^M \right) P_{ejt}(1 - P_{ejt}) \,, \tag{13}$$

which will form the basis for estimates of substitution elasticities in the empirical section. Immigrants and natives are perfect substitutes in production if and only if wage effects are the same for both groups.

The coefficients $\theta^N$ and $\theta^M$ will be consistently estimated as long as the residual unobserved components of the regression equations are orthogonal to $P_{ejt}$. Thus, the identifying assumption is the absence of any skill-group specific residual wage change that is correlated with the immigrant supply shock. In this, there are two major concerns. First, there might be outside factors that influence both (native) wages and immigrant inflows. For example, because business cycle movements and labor demand shocks (i.e., the parameters $b_{ejt}$ in equation (5) or $c_{ejt}$ in equation (7)) can be expected to affect migration flows of workers with low mobility costs and easy access to the Norwegian labor market, the concern might be that the immigrant share increases in years with favorable employment and wage conditions. In an extended empirical specification, we also include wage determinants with time variation within skill group ($Z_{ejt}$) to capture within-group labor demand and native supply shocks. We account for differential labor demand shocks within skill groups over time with the proportion of native workers within each cell who were registered unemployed or participated in active labor-market programs during the year.

The second concern is that selective attrition, whereby low-productivity native workers (within skill group) leave employment as immigrants enter, could also mask any negative effect of immigration if the composition effect works in the opposite direction of the immigrant wage effect. Unlike most previous studies, we use individual panel data that enable us to address the problem of selective native participation. We use two alternative approaches to this issue: (1) we estimate equation (9) with individual fixed effects (i.e., where $u_{iejt} = \alpha_i + v_{iejt}$); (2) we exclude from the wage sample marginal workers who move in and out of employment (i.e., workers with low attachment who will be the source of bias from any selective attrition).

Because our model specification contains a rich set of fixed effects to account for permanent and time-varying confounding factors, the remaining variation in $P_{ejt}$ will be quite limited, and even seemingly unimportant sources of classical measurement error might create substantial attenuation bias. Although sampling error, as in Aydemir and Borjas (2011), is not directly relevant because of our administrative full coverage register data, there are other potential sources of imprecise measurement of the true immigrant labor force share within skill group. First, the allocation of immigrant workers into experience groups is imprecise because exact measures of pre-migration work experience, the age at which the worker entered the labor market, or temporary withdrawals from the labor market are typically not available. Second, the generally low returns to experience for immigrants from low-income countries suggest that a common allocation rule across groups of workers based on potential years of labor-market participation might be dubious. Third, consistent educational classification across countries is fundamentally difficult because of differences in schooling structure, quality, and curriculum. The allocation of immigrants with missing information on educational attainment (see details in the Appendix) is yet another contributor to measurement error in $P_{ejt}$. While estimation with individual fixed effects accounts for selective attrition, a drawback of the fixed-effects estimator is that any attenuation bias from measurement error in $P_{ejt}$ will be greatly exacerbated. Drawing on the approach of Griliches and Hausman (1986), we examine the importance of attenuation by eliminating individual observations close in time and where regression residuals are likely to be highly autocorrelated.

Another measurement issue arises from the fact that many foreign-born employees work in Norway without being registered as permanent residents (and are thereby not counted in our measure of $P_{ejt}$).[4] Incomplete registration suggests that immigrants might be systematically undercounted. Unlike attenuation bias from classical measurement error, incomplete registration could lead to inflated estimates of the effect of immigration (scaling bias).[5] Undercounting is likely to be an issue in data on immigrant presence in other countries as well. As illustrations, Warren and Passel (1987) estimate that only one-half of the two to four million illegal immigrants living in the US in 1979 were counted in the 1980 census, and, according to Hoefer *et al*. (2010), 5.9 percent of the foreign-born population was not counted in the 2009 American Community Survey. In our comparison of empirical studies, we therefore report the elasticity of native wages with respect to the size of the immigrant labor force (rather than the share), because this metric is unaffected by any (proportional) undercounting of immigrants.

[4] In a study of the Norwegian construction sector, Bratsberg and Raaum (2012) report that about one-half of the immigrants employed in that sector are not registered permanent residents of Norway.

[5] Suppose the observed count of immigrants in cell $(e,j,t)$ is proportional to the true count by some factor $\alpha$, $\tilde{M} = \alpha M$, where $0 < \alpha <= 1$, and we use the observed immigrant share as regressor, $\tilde{p} = \alpha M/(N + \alpha M)$. Some algebra shows that the computed coefficient will overstate the true parameter as $\text{plim}\widehat{\theta} = \theta\,[(N + \alpha M)/(N + M)]^2\,/\alpha$. The elasticity of wages with respect to the immigrant stock is, however, unaffected by such proportional undercount, as $\partial \ln w/\partial \ln \tilde{M} = \partial \ln w/\partial \ln M$.

In the empirical analyses, we also split the immigrant labor force share by origin ($P_{rejt}$), where

$$P_{rejt} = \frac{M_{rejt}}{M_{ejt} + N_{ejt}}, \quad \sum M_{rejt} = M_{ejt}. \tag{14}$$

The regions ($r$) are the Nordic countries, other European countries plus North America, Australia, and New Zealand (but excluding former Yugoslavia and Turkey), and the rest of the world. This classification can be motivated from expected differences in substitutability (within skill group) between natives and immigrants by origin caused by such factors as immigration policy, economic development and school quality of the source country, and similarities of language and culture. When we estimate wage effects of immigrant supply shocks by region of origin, we simply replace the term $\theta P_{ejt}$ in equation (9) with three separate immigrant shares by origin and free coefficients.

## IV. Data

Our data are extracts of information from several administrative registers that cover all residents of Norway during the 14-year period 1993–2006. The core variables are residency, country of origin, labor force participation, educational attainment, work experience, and wage earnings. The empirical analyses are restricted to male wage earners. In this section, we provide details about the data.

### *Immigrant Labor Force*

Figure 1 depicts recent trends in immigrant shares of the male labor force. The immigrant labor force consists of foreign-born residents with two foreign-born parents, age 18–70, not enrolled in school, and with positive labor earnings, registered employment, registered unemployment, or active labor-market participation during the year.

Because of the high inactivity rates among many groups of immigrants from developing countries (OECD, 2001), we concentrate on labor supply shocks from those actively participating in the labor market rather than the complete stock of foreign-born residents. The figure shows that the immigrant labor force share has increased sharply over time, and doubled from 5 to 10 percent during our sample period. When we classify immigrants by three regions of origin (i.e., the neighboring Nordic countries, other high-income countries, and developing countries), the growth in the

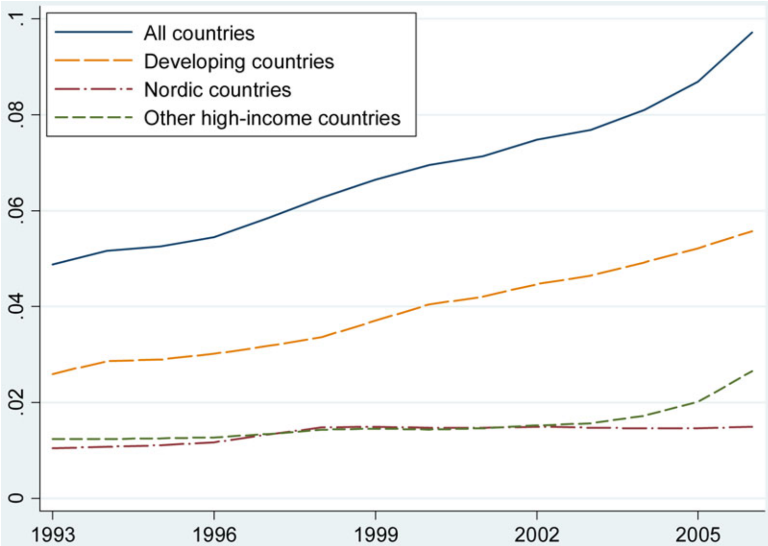


*Fig. 1.* Immigrant shares of the male resident labor force, 1993–2006

*Notes*: Immigrant labor force consists of foreign-born residents with two foreign-born parents, age 18-70, not enrolled in school, and with positive labor earnings, registered employment, registered unemployment, or active labor market participation during the year.

immigrant labor force primarily stems from a secular increase in the immigrant population from developing countries.[6]

Since 1954, the Nordic countries have constituted a common labor region. Because Nordic citizens do not need a permit to take up work or residence elsewhere in the region, their temporary cross-border mobility is often not recorded in administrative population registers. Empirical studies show that intra-Nordic migration flows have been affected by business cycle fluctuations and inter-country wage differences, with pull factors in the receiving country the main triggering device (Pedersen and Røed, 2008). The human capital of Nordic residents is highly transferable because of very similar languages, school systems, labor markets, as well as political institutions, making Nordic immigrants and native workers close substitutes in the Norwegian labor market. Empirical studies also show that, while

[6] While the exact country-of-origin composition varies somewhat over the sample period, the three countries of Pakistan, Vietnam, and Turkey figure among the top five source countries, and make up between 20 and 30 percent of the developing country bracket each year. Likewise, the UK, Germany, and Poland figure among the top five countries and make up about 50 percent of the "other high-income" bracket over the data period.

Nordic immigrants in Norway earn slightly less than natives with comparable human capital characteristics just after arrival, they catch up within a short time (Barth *et al.*, 2004).

Until the mid-1970s, most labor immigrants, regardless of country of origin, would receive a work permit if they had secured a job contract with a Norwegian employer. In 1975, this changed when Norway introduced a temporary moratorium on immigration that was followed by legislation favoring admission based on family reunification and political protection rather than work. After this immigration stop, non-Nordic citizens were granted work permits only if accepted as "specialist workers".[7] In 1994, most West Europeans gained access to the Norwegian labor market through the establishment of the common European labor market, and in 2004 citizens of the new European Union member countries in Eastern and Central Europe gained access on similar terms (with some temporary restrictions). Since 2005, the inflow of labor immigrants from this region has increased considerably.

Between 1990 and 2007, over 50 percent of immigrants from the "other high-income countries" bracket were admitted as labor immigrants, while nearly 35 percent entered because of family relations with immigrant or native residents. Among immigrants from developing countries, only 4 percent arrived on a work visa, while 57 percent were admitted as refugees and about 30 percent for family reunification (Statistics Norway, 2010). Thus, immigrant inflows from outside the Nordic and other high-income countries were less likely to be directly related to business cycle movements compared to other inflows.

### *Immigrant Supply Shocks by Skill*

We compute total labor supply as the sum of labor force participants in 32 skill groups defined by educational attainment and potential labor-market experience. Individuals with 1−40 years of potential experience are allocated into eight five-year Mincer experience intervals and four education levels based on the first digit of the six-digit education code collected from the national education database. The four education levels correspond to less than high school education, high-school graduate but no college diploma, short college/university education, and long college/university education. Our data contain information on educational attainment for (practically) all natives and we measure Mincer experience as years since

[7] To be admitted under this category the employer had to verify that the skills held by the immigrant were not available in Norway. In 2002, this requirement was replaced by a specialist quota of 5,000 per year, a limit that has not been filled to date.

leaving school, with school-leaving age computed as six plus statutory years of the individual attainment.[8]

In the baseline case, we compute potential experience for immigrants as for natives, implicitly assuming that potential work experience from abroad is comparable to experience obtained in Norway. As for natives, we collect data on attainment from the national education database, where information typically stems from Norwegian educational institutions, supplemented with decennial surveys of the immigrant population. As such, educational attainment is often missing for newly arrived immigrants. For immigrants with missing education records, we assume that their schooling distribution is similar to that observed among immigrants with the same gender, age, and origin. The Appendix offers further details on sources of education data and a detailed description of the imputation method for missing observations.

Our identification strategy hinges on the allocation of immigrants into relevant skill groups. For some immigrant groups, experience before arrival as well as years spent in the host country are not necessarily comparable to potential experience among natives. Many immigrants from distant, developing countries have both limited and very different labor-market experiences because of conflicts and high rates of unemployment. Empirical studies of immigrant earnings profiles suggest that economic returns to potential experience prior to arrival differ considerably by region of origin (Barth *et al*., 2004). While earnings profiles of immigrants from the Nordic countries are very similar to those of natives, immigrants from developing countries earn substantially less at arrival. The gap is reduced during the first 10−15 years in Norway, but there is no convergence (on average) after that. In our register data, we have access to complete earnings histories for all residents since 1967, which enables us to observe actual post-arrival experience for immigrants. Based on these records, for migrants from developing countries, we replace potential experience with the cumulative years with positive earnings in Norway, ignoring any pre-arrival experience. Constructing this "effective experience" measure, we keep the Mincer experience measure for immigrants from high-income source countries, assuming that they have worked and accumulated experiences in labor markets very similar to what they enter in Norway.[9]

Figure 2 displays how the total male immigrant labor force shares ($P_{ejt}$) evolved over the sample period. The dashed lines depict immigrant shares

[8] Specifically, the four education levels consist of education codes starting with digits 0−3, 4−5, 6, and 7−8, respectively (see Statistics Norway, 2003, for a description of the educational coding system). The procedure yields the following average school-leaving ages for the four groups in the native wage sample: 17.5, 20.1, 22.5, and 25.6.

[9] There is also evidence that educational capital is not fully portable across countries (Friedberg, 2000; Bratsberg and Terrell, 2002), but we do not make any adjustments to

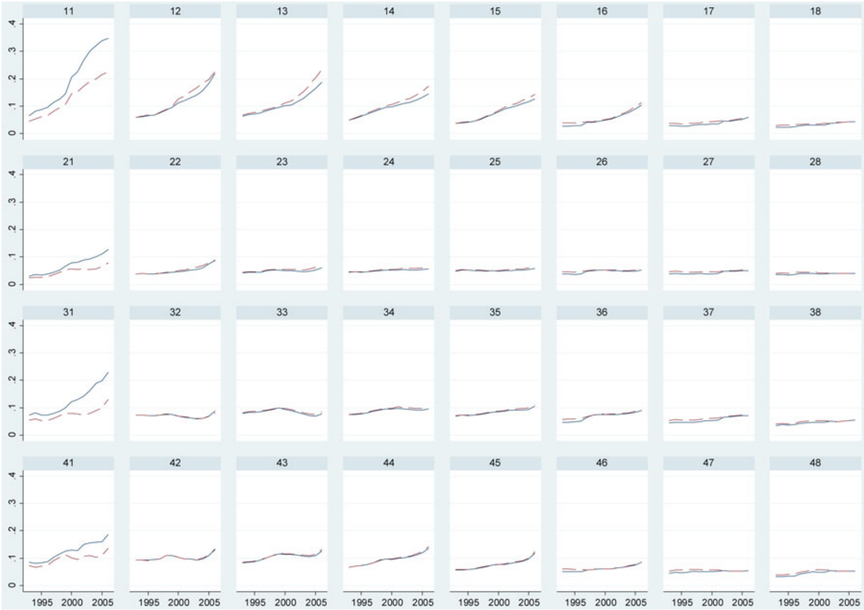


*Fig. 2.* Immigrant shares by education (first digit) and experience (second digit), 1993–2006

*Notes*: Dashed lines depict immigrant shares of the male labor force without making adjustments for effective experience while solid lines adjust immigrant counts from developing countries for effective labor market experience in Norway. The four education levels correspond to (1) less than high school, (2) high schoolgraduate, (3) some college, and (4) university graduate. The eight experience levels reflect 5-year intervals 1-5, 6-10, 11-15, and so on.

of the male labor force without making adjustments for effective experience, while the solid lines adjust immigrant counts from developing countries for effective labor-market experience in Norway. The four education levels correspond to the following: (1) less than high school; (2) high-school graduate; (3) some college; (4) university graduate. The eight experience levels reflect five-year intervals 1–5, 6–10, 11–15, etc. As the figure shows, immigrants are concentrated in skill groups with short experience and low education. Because the adjustment for effective work experience reallocates immigrants from developing countries into cells with less experience, the labor supply shocks from immigration are even more heavily concentrated in low-experience groups, according to the adjusted series.

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immigrant educational attainment. If foreign education is downgraded in the Norwegian labor market, our estimates are likely to understate the true immigration effects on wages (Dustmann and Preston, 2012).

Table 1. *Descriptive statistics for native and immigrant wage samples*

| | Native men (1) | Immigrant men: All (2) | Nordic (3) | Other high-income countries (4) | Developing countries (5) |
|---|---|---|---|---|---|
| Log daily wage | 6.71 | 6.61 | 6.73 | 6.81 | 6.47 |
| Years of schooling | 13.1 | 12.9 | 13.2 | 14.6 | 12.1 |
| Experience | 20.6 | 20.3 | 21.9 | 22.5 | 18.7 |
| Skill-cell characteristics | | | | | |
| Immigrant share | 0.065 | 0.075 | 0.068 | 0.070 | 0.080 |
| Nordic countries | 0.013 | 0.015 | 0.014 | 0.015 | 0.015 |
| Other high-income countries | 0.016 | 0.018 | 0.018 | 0.023 | 0.017 |
| Developing countries | 0.035 | 0.042 | 0.036 | 0.032 | 0.049 |
| Unemployment frequency | 0.127 | 0.130 | 0.116 | 0.091 | 0.151 |
| Log native labor force | 10.5 | 10.4 | 10.4 | 10.1 | 10.5 |
| Observations | 976,479 | 488,191 | 103,555 | 113,227 | 271,409 |

*Notes*: Sample means pertain to full-time workers; native sample is limited to a 10-percent random extract of all male wage earners. Immigrant labor force shares are based on adjusted counts using effective experience for immigrants from developing countries. "Unemployment frequency" measures the fraction of natives in the education−experience cell who received unemployment benefits during the year.

## *Native and Immigrant Wages*

Our wage data are collected from administrative payroll records submitted by employers to tax authorities. These records cover all jobs and each record contains a personal identifier for the worker. We focus on the pay record for the main job of the individual in a given year, defined by working hours (full-time versus part-time), contract period, and total pay. Hours worked are reported in three broad brackets only (two part-time brackets and one full-time bracket). Even if we cannot calculate the hourly wage, we come close when we consider full-time employees and their daily wage computed as total pay divided by the number of days of the employment contract. Our primary empirical focus is the daily wage of full-time workers, but we also report results for weekly wages of all workers including those on part-time contracts. Finally, we also examine annual labor earnings, summing wage and salary income across all jobs and any income from self-employment.

Table 1 presents the sample means. The sample underlying the analysis of native wages is based on a 10 percent random extract of all native (i.e., Norwegian born with two Norwegian-born parents) workers who appeared in the population register during the sample period. (Note, however, that the computations of immigrant shares are based on the complete labor force.) We construct a wage sample of full-time immigrant employees in a similar fashion to the native sample (but retain all immigrant wage earners in the analyses). As the table shows, the average (nominal) daily wage of the immigrant sample is about 10 percent below that of natives, and immigrant wage earners are somewhat more clustered in cells with high immigrant labor force shares, but other characteristics are broadly similar across the two samples. When we disaggregate the sample of full-time immigrant wage earners by origin, immigrants from the Nordic and other high-income countries have slightly more favorable human capital characteristics and higher wages than natives, while characteristics of developing-country immigrants are less favorable. Because we restrict the analysis of wages to full-time employees and require non-missing values for all characteristics (in particular, education), the immigrant sample will be dominated by those well established in the Norwegian labor market, explaining the similarity of average educational attainment and experience in the native and immigrant wage samples.

## V. Results

### *Baseline Results*

We start the empirical analysis with a replication of Borjas (2003), using the same model specification and variable definitions as in the original study. Our basic estimates for native, male wage earners are presented in Table 2, where Panel A lists estimated immigration effects on the daily wage of full-time workers, Panel B lists immigration effect estimates on weekly wages for a broader sample including part-time workers (which is more in line with the standard approach in the literature), and, finally, Panel C reports immigration effect estimates on annual earnings. The basic wage effect ($\theta$) on the daily wage of full-time native workers is estimated to be −0.278 (with a standard error clustered within experience−education groups of 0.175; see Panel A, Column 1), suggesting only a moderate reduction in native wages from a within-skill group immigration-induced increase of the labor force. As seen from Panels B and C, the estimated wage effect becomes more negative (and gains statistical significance) when we consider weekly wages and include pay from part-time work, and it is tripled when we estimate the effect on annual labor earnings. The particularly large effect on annual earnings indicates that hours (i.e., days) worked might be even more adversely affected by immigrant supply shocks than the daily wage.

As discussed above, prior evidence shows that immigrants from developing countries earn low returns to experience from their source country. Thus, immigrants from developing countries are likely to be misallocated when grouped with natives holding the same potential experience (i.e., years since completed schooling). In Column 2, therefore, we report the estimated immigration wage effect based on the alternative measure of immigrant labor supply with immigrants from developing countries

Table 2. *Effect of immigrant share on native log wage*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A. Log daily wage of full-time workers** | | | | |
| Immigrant share | −0.278 | −0.312* | −0.327** | −0.405*** |
| | (0.175) | (0.178) | (0.155) | (0.142) |
| Unemployment frequency | | | −0.657*** | −0.640*** |
| | | | (0.189) | (0.187) |
| Log native labor force | | | | −0.040* |
| | | | | (0.020) |
| **B. Log weekly wage** | | | | |
| Immigrant share | −0.380** | −0.439** | −0.454*** | −0.507*** |
| | (0.183) | (0.186) | (0.165) | (0.151) |
| Unemployment frequency | | | −0.538** | −0.526** |
| | | | (0.215) | (0.215) |
| Log native labor force | | | | −0.032 |
| | | | | (0.021) |
| **C. Log annual earnings** | | | | |
| Immigrant share | −0.860*** | −0.922*** | −0.943*** | −0.978*** |
| | (0.278) | (0.246) | (0.213) | (0.201) |
| Unemployment frequency | | | −0.790** | −0.781** |
| | | | (0.358) | (0.358) |
| Log native labor force | | | | −0.026 |
| | | | | (0.024) |
| Immigrant share adjusted for effective experience? | No | Yes | Yes | Yes |

*Notes*: Standard errors are reported in parentheses and are clustered by 32 education−experience cells. Regression samples consist of 976,479 (Panel A), 1,030,608 (Panel B), and 1,152,884 (Panel C) observations. The regression model also includes fixed effects for year, education group, experience cell, and interactions year∗education, year∗experience, and education∗experience (a total of 174 control variables). The immigrant share variable used in Column 1 is based on potential experience, while that used in Columns 2−4 adjusts counts of immigrants from developing countries for their effective work experience in Norway.
***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

allocated across experience cells according to their effective work experience in Norway rather than years since leaving school. For all three earnings measures, the estimated wage effect is somewhat larger in absolute terms than when skill group allocation is based on potential experience. For native full-time workers, the effect on the daily wage increases in size by 10 percent, from −0.278 to −0.312, consistent with the adjustment being effective in reallocating immigrants into experience cells where they compete with native workers. For this reason, we proceed with the adjusted series.

## *Accounting for Within-Cell Labor Demand and Supply Shocks*

In spite of the elaborate controls from the pairwise interactions between education, experience, and year of observation in the empirical model, there remains a concern that residual skill-group-specific labor demand shocks might bias the estimate of the immigration wage effect in a positive direction if immigrant inflows are larger under favorable labor-market conditions. To control for variation in labor demand within skill groups over time, we construct a business cycle indicator measuring the proportion of native workers within each cell who were registered unemployed or participated in an active labor-market program during the year. As shown in Column 3, estimates become slightly more negative when we control for the unemployment measure. Furthermore, consistent with a broad body of literature studying unemployment and wages (e.g., Blanchflower and Oswald, 1994), higher unemployment is associated with lower wages and the coefficient estimate indicates that an increased unemployment frequency of one percentage point reduces average wages by about 0.7 percent, which is equivalent to a wage curve elasticity of $-0.09$ ($= -0.7 * 0.13$, where 0.13 is the mean unemployment frequency in the sample).

Our immigrant supply shock measure ($P_{ejt}$) will be influenced by the change in the number of native workers in the skill group, because adjustments in the number of native workers mechanically will alter the fraction of immigrants in the cell. Because of shifts in educational attainment and fluctuations in birth cohort size, changes in the group-specific native workforce will be negatively correlated with changes in the immigrant share. If a positive native supply shock reduces the competitive wage, a concern is that our estimate of immigrant effect will be biased towards zero. Consistent with this argument, Column 4 reveals that the estimated immigration wage effect becomes even stronger when we condition on the log size of the native labor force in each education−experience−year cell. When we account for cell-level variation in demand conditions and native labor supply, the estimated immigrant effect coefficient on the log daily wage of full-time workers becomes $-0.405$ and is statistically significant even when we cluster standard errors within education−experience cells.[10] Panels B and C reveal similar sensitivity of immigration effect estimates with respect to accounting for demand and supply conditions when the dependent variable is the weekly wage or annual earnings. Of the three pay measures, the daily wage of full-time workers is the closest representation of the theoretical wage concept because the other two will also capture effects on hours worked. To avoid inflated estimates, we focus on immigration effects on the daily full-time wage in the remainder of the empirical analysis.

[10] The chief reason for clustering standard errors within the 32 education*experience cells, is to account for any within-skill-group serial correlation in the log wage residual. If we were instead to cluster standard errors at the unit of observation of the immigrant share variable in order to account for the grouped nature of the data (i.e., within 448 education*experience*year cells), then standard errors would be considerably smaller than those reported in Table 2. To illustrate, the standard error of the impact coefficient estimate in Panel A, Column 4, would be 0.078, about one-half of the reported standard error that also accounts for serial correlation.

Table 3. *Immigration wage effect accounting for native sample selectivity*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Immigrant share | −0.463** | −0.129 | −0.484** | −0.338 |
| | (0.225) | (0.267) | (0.215) | (0.262) |
| Observations | 868,876 | 976,479 | 319,000 | 319,000 |
| Individuals | | 113,220 | | 82,387 |
| Individual fixed effects? | No | Yes | No | Yes |
| Sample | Restricted sample: exclude low-attachment workers | Full sample | Restricted sample: individual observations three years apart | Restricted sample: individual observations three years apart |

*Notes*: Standard errors are reported in parentheses and are clustered by 32 education−experience cells. The dependent variable is the log daily wage of full-time workers. The regression model controls for cell unemployment frequency and log native labor force, as well as fixed effects for year, education group, experience cell, and interactions year∗education, year∗experience, and education∗experience.
***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

## *Selective Native Attrition*

Average wages within skill groups are potentially influenced by any presence of workers with low attachment to the labor market, who move in and out of employment (Borjas *et al*., 2008). Unless participation is random, the estimate of the wage effect on repeated cross-sectional data will be biased if native movements in and out of the wage sample are related (in time) to immigrant inflows (Card, 2001). For example, if low-wage natives are more likely than high-wage natives to leave employment concurrent with a positive immigrant supply shock, the average native wage will increase because of the change in the composition of the employment pool (Bratsberg and Raaum, 2012). This mechanism is analogous to that which induces bias in impact estimates from spatial approach studies; see the discussions by Card (2001) and Borjas (2006).

In Table 3, we report results from alternative strategies to check the implications of selective attrition in our wage sample. Rather than specifying an arbitrary selection equation based on questionable instruments, we take advantage of the individual panel structure of our data. In the first approach, we exclude individuals with low attachment to the labor market from the sample by dropping those who participated in fewer than half of their maximum possible years (i.e., fewer than 7 out of 14 years for the majority of the birth cohorts in our data). The elimination of low-attachment workers causes the estimated wage effect to increase (in absolute value) from −0.405 (Table 2, Column 4) to −0.463 (Table 3, Column 1), which is consistent with the argument that inclusion in the sample of low-paid workers whose employment status is sensitive to immigrant supply shocks renders a positive bias in impact estimates. A closer look at mobility patterns in the data reveals that low-paid employees are indeed more likely to move in and out of employment and that their employment correlates with change in the immigrant share in their education−experience cell. There are several push-and-pull factors that might explain this association, but we do not make any attempt to disentangle them in this paper. From the (limited) perspective of identifying wage effects, the inclusion in the sample of the marginal native workforce imparts a positive bias in the coefficient estimate of the immigrant share.

An alternative strategy to account for selective participation (frequently used in empirical labor economics) is to estimate the wage equation with individual fixed effects. As shown in Column 2 of Table 3, our individual fixed-effects estimate is close to zero and statistically insignificant. At face value, this result suggests that our baseline finding is driven by a negative compositional correlation between unobserved wage components and the immigrant share in the data. This conclusion directly contradicts the conclusion based on the first strategy of excluding native workers with low labor force attachment from the sample. However, we argue that little weight should be placed on the individual fixed-effects estimate because of bias towards zero from the measurement error. It is well known that when an explanatory variable is inflicted by measurement error, the fixed-effects estimator will not necessarily improve identification because attenuation bias can be severely amplified. As pointed out in Section III, the difficulty of allocating immigrants with diverse backgrounds in terms of labor-market experience and educational training into their appropriate skill cell, exacerbated by missing educational data for many immigrants, will impart measurement error in the immigrant labor supply variable. Thus, attenuation bias is a concern even with group fixed effects only, simply because the remaining variation in the immigrant share controlling for permanent factors is very limited and measurement error will represent a non-negligible proportion of overall variation (Aydemir and Borjas, 2011).[11] Moreover, because the (true) immigrant share is autocorrelated, the signal-to-noise ratio in the observed share is reduced even more (Griliches and Hausman, 1986). Note that the individual fixed-effects estimator identifies wage effects via variation in the change in the immigrant share within individuals. Because the immigrant share is correlated across years and because shares in neighboring skill cells are highly correlated, the variation in the explanatory variable will be reduced substantially when individual fixed effects are included in the empirical model. Because individuals typically alter experience interval two or three times during the data window, within-individual variation is substantially lower than total variation in the explanatory variable.

[11] In an auxiliary regression that relates the observed immigrant share to the group fixed effects and interaction terms of equation (9), the adjusted $R^2$ is above 0.9.

This will aggravate any attenuation bias, and might explain why the individual fixed-effects estimate is close to zero.

Following Griliches and Hausman (1986), we reduce the attenuation bias from measurement error by dropping (autocorrelated) observations that are close in time. In Column 4 of Table 3, we restrict the sample further and exclude individual observations less than three years apart. We find that the estimate without individual fixed effects becomes slightly more negative than the estimate without the sample restriction (−0.484 versus −0.463). When we now introduce individual fixed effects, the fixed-effects estimate does move somewhat towards zero, from −0.484 to −0.338 (see Column 4). However, compared to the consequence of introducing individual fixed effects in the full sample, the drop in (the absolute value of) the estimate is much smaller when we reduce autocorrelation in the explanatory variable. We attribute the decline in the coefficient estimate in the reduced sample largely to the (remaining) measurement error. Thus, it seems highly unlikely that the zero-impact estimate of the fixed-effects estimator is correct, in that the estimator appropriately adjusts for an underlying negative correlation between within-skill-cell wage shocks and the immigrant share. We conclude from this exercise that the concern that individual fixed-effects models can make things worse is highly relevant in the present context.

### *Immigrant−Native Substitution*

A major topic of discussion in the recent empirical body of literature is whether immigrant and native labor of the same skill are close substitutes in production. An approach to assessing immigrant−native substitutability is to study the wage effects of immigration on immigrant wages and to contrast estimates with those uncovered for native workers. As shown in the theoretical framework in Section III, the wages of immigrants and natives should be equally affected by an immigration-induced increase in labor supply if they are perfect substitutes (see equations (6) and (8)). Conversely, if immigrant and native workers are imperfect substitutes within skill cells, the immigration effect should be larger on immigrant wages than on native wages. Thus, we next estimate wage effects in a sample of full-time immigrant wage earners and we use the expressions in equations (12) and (13) to compute elasticities of substitution between immigrant and native workers within education−experience cell ($\sigma_M$) as well as the implied substitution between experience groups ($\sigma_J$). The results are listed in Table 4.

In Column 1, we first return to the basic measure of immigrant shares (without making adjustments for effective experience of immigrants from developing countries) and we ignore controls for labor demand and native supply in the regression in order to mimic specifications used in the recent

Table 4. *Immigration effect on immigrant wages and implied elasticities of substitution*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Parameter | | | | |
| $\theta^M$ | $-0.800^{***}$ | $-0.966^{***}$ | $-0.966^{***}$ | $-0.966^{***}$ |
| | (0.186) | (0.195) | (0.195) | (0.195) |
| $\theta^M - \theta^N$ | $-0.522^{*}$ | $-0.561^{**}$ | $-0.503$ | $-0.482$ |
| | (0.258) | (0.267) | (0.377) | (0.368) |
| $-1/\sigma_M$ | $-0.033^{*}$ | $-0.034^{**}$ | $-0.030$ | $-0.029$ |
| | (0.016) | (0.016) | (0.022) | (0.022) |
| $\sigma_M$ | 30.7 | 29.3 | 33.6 | 34.5 |
| $1/\sigma_J$ | $0.319^{*}$ | $0.452^{***}$ | $0.508^{**}$ | $0.529^{**}$ |
| | (0.170) | (0.138) | (0.219) | (0.210) |
| $\sigma_J$ | 3.13 | 2.21 | 1.97 | 1.89 |
| Immigrant share adjusted for effective experience? | No | Yes | Yes | Yes |
| Native reference | Full sample | Full sample | Restricted sample: exclude low-attachment workers | Restricted sample: individual observations three years apart |

*Notes*: Standard errors are reported in parentheses and are clustered by 32 education−experience cells. Immigrant wage sample consists of 488,191 observations. All columns control for immigrant origin and fixed effects for year, education group, experience cell, and interactions year∗education, year∗experience, and education∗experience. Columns 2−4 further control for cell-level native unemployment frequency and log native labor force. The immigrant share variable used in Column 1 is based on potential experience while that used in Columns 2−4 adjusts counts of immigrants from developing countries for their effective work experience in Norway.
$^{***}$, $^{**}$, and $^{*}$ denote significance at the 1, 5, and 10 percent levels, respectively.

body of literature. As the column shows, the estimate of the immigration impact coefficient on immigrant wages is −0.800 and is highly significant. In Column 2, we apply the adjusted immigrant labor force shares and add controls for demand and supply conditions to the empirical model. This brings the coefficient estimate to −0.966, which is about twice the estimated effect on native wages. Indeed, when we stack the native and immigrant wage samples together (with complete interactions on all explanatory variables) in order to formally test whether immigrant wages are more responsive than native wages to immigrant supply shocks, the difference in coefficient estimates is statistically significant at the 5 percent level. In other words, the estimate of the term $-1/\sigma_M$ is significantly different from zero, which leads to rejection of the null hypothesis that $\sigma_M$ is infinite and that immigrants and native workers are perfect substitutes within skill cells.[12] Yet, the implied value of $\sigma_M$ of 29.3 does suggest a

[12] The computation of $-1/\sigma_M$ draws on equation (12) and uses the weighted average of $p$ from the native and immigrant wage samples (0.067).

high degree of substitutability between immigrants and natives, and is much larger than the estimate of 7.8 for the UK reported by Manacorda *et al.* (2012) and somewhat larger than the estimate of 20 reported by Ottaviano and Peri (2012) for the US (see also the cross-study comparisons discussed in the empirical section).

In Columns 3 and 4, we repeat the estimation procedure with alternative native reference samples in order to account for selectivity bias in estimates of the impact coefficient on native wages. (We uncover no indication of sample selection bias in the coefficient estimate on immigrant wages and retain the full immigrant wage sample in all of the comparisons.) The alternative native reference samples yield somewhat smaller differences between impact coefficients on immigrant and native wages, and the estimates of $\theta^M - \theta^N$ are no longer significantly different from zero. Although the implied values of the immigrant−native substitution elasticity, 33.6 and 34.5, are close to the value listed in Column 2, we can no longer reject the hypothesis that the substitution elasticity is infinite and that immigrants and natives with similar skills are perfect substitutes in production.[13]

Table 4 also reports estimates of the elasticity of substitution between experience groups, based on equation (13).[14] Our estimates vary across specifications, but fall in the range 1.9−3.1, with the lower figure stemming from the preferred specification where we account for sample selectivity bias in the estimate of the immigration effect on native wages. As such, our estimates fall below those of Mancorda *et al.* (2012) and Ottaviano and Peri (2012), as well as those of Card and Lemieux (2001), who report a point estimate of the elasticity of substitution across experience groups of around 5. These comparisons suggest less substitution between experience groups in the Norwegian labor market than in the UK and the US.[15]

[13] As pointed out by Manacorda et al. (2012, p. 133), an alternative source of identification of $\sigma_M$, suggested by equation (5), is the coefficient of the log native labor force. Our estimates of this coefficient range between −0.040 and −0.035 across the specifications of Table 4, implying a value for the elasticity of substitution of between 25.0 and 28.6.

[14] To proxy for the parameter $\eta$, we adjust the sample mean immigrant labor force share to account for the 10 percent immigrant−native wage differential among full-time workers revealed in Table 1.

[15] Our estimates are, however, in line with Katz and Murphy (1992) whose findings imply an elasticity of substitution of 2.9 between young and old workers in the US. Lindquist and Skjerpen (2003) use Norwegian data to estimate labor demand wage elasticities by education (high and low), conditional on production and capital. Their estimates with respect to the wages paid to highly educated workers are −1.72 (direct) and 0.64 (cross). The implied elasticity of substitution (i.e., the percent change in relative factor intensity of skilled and unskilled labor, given a 1 percent change in the relative wage, holding output and capital constant) is given by the absolute value of the difference between the two estimates, 2.36, which is similar to our estimate of the elasticity of substitution across experience groups.

## *Immigration Wage Effects by Origin*

The composition of the immigrant labor supply shock might have implications for how native wages are affected. From the factor demand theory discussed in Section III, we would expect the wages of native males to be more strongly influenced by immigrant inflows from the neighboring Nordic countries because they are more similar and are closer substitutes to the native labor force than other immigrant groups. At first glance, the empirical evidence does not confirm this prediction because, according to the basic specification in Column 1 of Table 5, the Nordic immigrant share has no effect on the wages of native men while wages are negatively affected by immigration from developing countries. However, when we add controls for within-skill-group variation in demand and supply factors over time, as in Columns 2 and 3, we find that the estimate of the basic model for Nordic immigration in Column 1 is biased towards zero. As the table demonstrates, the inclusion of demand (and supply) controls is particularly important when it comes to the estimated wage effects of Nordic immigration. Presumably, inflows of workers from the neighboring countries, who have free access to the Norwegian labor market and low migration costs, are more responsive to changes in labor-market conditions than other immigrant flows. Unless we account for fluctuations in labor demand (and supply), the negative wage effect of immigration from close countries is likely to be masked. However, the inclusion of the labor demand control does not affect the estimate of the immigration effect from developing countries. These immigrant groups face higher migration costs and meet more restrictions on movements across countries, breaking the correlation between inflows and confounding demand factors.

The estimated wage effects of immigration by origin are sensitive to sample inclusion of native workers with low attachment to the labor market. When we exclude low-attachment workers, the negative effect of Nordic immigration becomes substantially larger and statistically significant (see Column 4). Even immigrants from other high-income countries seem to contribute to a downward pressure on native wages, while the estimated effect of immigration from developing countries becomes small and is no longer statistically significant. A pattern that emerges from the table is that the coefficient estimate for immigration from nearby countries is highly sensitive to the inclusion in the sample of native workers with low attachment and to the inclusion in the model of cell-specific labor demand and supply controls. The indication is that Nordic immigration, in particular, is positively correlated with confounding determinants of native wages, and that employment of natives with low attachment to the labor market is affected by immigration from nearby countries. The table also lists $p$-values from tests of the null hypothesis that immigrant inflows from the

Table 5. *Origin-specific immigration effects on native log wage*

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Immigrant share by origin** | | | | | |
| Nordic countries | −0.031 | −0.645 | −1.338 | −2.148** | −2.748*** |
| | (1.037) | (1.001) | (0.874) | (0.804) | (0.936) |
| Other high-income countries | −0.009 | 0.299 | 0.051 | −0.645 | −0.568 |
| | (0.558) | (0.530) | (0.559) | (0.610) | (0.582) |
| Developing countries | −0.427** | −0.437** | −0.392* | −0.167 | −0.139 |
| | (0.248) | (0.184) | (0.230) | (0.313) | (0.289) |
| Unemployment frequency | | −0.684*** | −0.677*** | −0.595*** | −0.636*** |
| | | (0.204) | (0.200) | (0.207) | (0.211) |
| Log native labor force | | | −0.042* | −0.042* | −0.046* |
| | | | (0.021) | (0.022) | (0.022) |
| Observations | 976,479 | 976,479 | 976,479 | 868,876 | 319,000 |
| ***p*-value, tests of equality of coefficients** | | | | | |
| $H_0$: $\theta^{NRDC} = \theta^{OHIC} = \theta^{DC}$ | 0.771 | 0.453 | 0.475 | 0.133 | 0.065 |
| $H_0$: $\theta^{NRDC} = \theta^{OHIC}$ | 0.986 | 0.460 | 0.227 | 0.157 | 0.043 |
| $H_0$: $\theta^{OHIC} = \theta^{DC}$ | 0.525 | 0.212 | 0.507 | 0.544 | 0.558 |
| $H_0$: $\theta^{NRDC} = \theta^{DC}$ | 0.728 | 0.846 | 0.349 | 0.046 | 0.022 |
| Sample | Full | Full | Full | Restricted: exclude low-attachment workers | Restricted: individual observations three years apart |

*Notes*: Standard errors are reported in parentheses and are clustered by 32 education−experience cells. The dependent variable is the log daily wage of full-time native male workers. The regression model also includes fixed effects for year, education group, experience cell, and interactions year∗education, year∗experience, and education∗experience. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

various regions of origin have the same effect on native wages. When we account for endogenous sample composition change, the test statistics reveal significant differences in the coefficients by origin (see Column 5). In particular, the evidence shows that immigration from the Nordic countries has a stronger negative effect on native wages than immigration from the other two source regions.

The pattern that Nordic immigration affects native wages more than immigration from developing countries is consistent with the explanation that Nordic immigrants are highly substitutable with native workers while natives and immigrants from low-income countries are imperfect substitutes in production. Further insight on such heterogeneity in substitution can be gained by examining whether there are differential immigration wage effects in the immigrant wage sample. If the coefficients on native wages reflect differences in substitutability, we might expect the effects of immigrant inflows (which are dominated by the low-income origin over our sample period, as shown in Figure 1) to be larger for the wages of immigrants from developing countries than for wages of Nordic immigrants; in other words, the reverse pattern of the impact structure uncovered for native wages. By the same token, we might expect the wages of immigrants to be more sensitive to immigrant supply shocks from their own region than to inflows from other regions.

In Table 6, we explore the patterns of differential wage effects across groups of immigrant workers according to their region of origin. Panel A lists estimates from a regression where the coefficient of the overall immigrant labor force share is allowed to differ for wage earners from the Nordic countries, other high-income countries, and developing countries. The point estimates are generally in line with the above scenario; immigration has a larger effect on the wages of immigrants from poor countries than on the wages of immigrants from the Nordic countries.[16] However, with large standard errors, we are unable to reject the hypothesis that coefficient estimates are equal across immigrant groups. In Panel B, the immigrant labor force shares are split in order to separate between immigration from own and other regions and the regression allows for different impact coefficients across samples (see Columns 1−3). For two of the three immigrant groups, estimates point to a greater sensitivity of wages to inflows from own than other regions. However, again, with large standard errors, we fail to reject the null hypothesis that impact coefficients are equal across immigrant samples and for inflows by origin. In summary, although the patterns of

[16] Applying equation (12), the coefficient estimates listed in Table 6, Panel A, combined with the impact estimate of −0.484 for native wages (Table 3, Column 3), suggest that the immigrant−native substitution elasticity ranges from 28 for immigrants from developing countries to 58 for Nordic immigrants. These computations are, however, based on a theoretical framework that only considers two labor inputs (natives and immigrants).

Table 6. *Immigration wage effects across the immigrant population*

| | Immigrant wage sample | | | $p$-value $F$-test of $H_0$: equality of coefficients | With restriction that coefficients are equal across groups |
|---|---|---|---|---|---|
| | Nordic countries (1) | Other high-income countries (2) | Developing countries (3) | (4) | (5) |
| **A. Overall immigrant share** | −0.807* | −0.997** | −1.064*** | 0.713 | −1.038*** |
| | (0.454) | (0.408) | (0.162) | | (0.177) |
| **B. Separate own and cross group wage effects** | | | | | |
| Own share | −0.944 | −1.418* | −0.736*** | 0.645 | −1.051*** |
| | (2.156) | (0.811) | (0.241) | | (0.164) |
| Immigrant share, other groups | −0.749 | −0.888** | −2.090** | 0.354 | −1.012** |
| | (0.509) | (0.429) | (0.998) | | (0.432) |

*Notes*: Standard errors are reported in parentheses and are clustered by 32 education−experience cells. The regression sample consists of 488,191 observations. The regression model also includes indicators for immigrant origin, native cell unemployment frequency, log native labor force, interactions between immigrant origin and the unemployment and labor force variables, as well as fixed effects for year, education group, experience cell, and interactions year∗education, year∗experience, and education∗experience.
***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

coefficients are broadly consistent with the explanation of heterogeneity of substitution elasticities across immigrant groups, the statistical evidence on the issue remains weak.

### *Cross-Study Comparisons*

All in all, compared to the −0.278 estimate from our baseline specification, by taking into account effective experience, native supply effects, demand shocks, measurement error, and selective native attrition, we obtain a preferred estimate of the direct effect of immigration on the wages of Norwegian workers that lies between −0.4 and −0.5, which in turn is very similar to the preferred US estimate of Borjas (2003). In light of the more compressed wage structure and stronger collective labor institutions in Norway compared to the US, this similarity is slightly surprising.[17]

For cross-country comparisons, however, the direct partial wage elasticity with respect to the size of the immigrant labor force might be a more attractive metric because of vast differences in immigration levels and because this measure is invariant with respect to proportional undercounting of the immigrant labor force. In Table 7, therefore, we compare our own estimates with those of a selected number of other studies.

For each study, we calculate the comparison metric from reported parameter estimates, such as estimates of substitution elasticities. The direct partial wage elasticities are evaluated at the relevant sample mean immigrant share. As Table 7 shows, our elasticity estimate of −0.029 is close to one-half of the same metric of −0.051 based on Borjas (2003), reflecting that the immigrant labor force is substantially smaller in Norway than in the US. For Canada, Aydemir and Borjas (2007) have found an even stronger effect on relative wages from immigration. Borjas *et al*. (2010) have reported estimates that imply wage elasticities of −0.031 for black workers and −0.047 for white workers in the US. While Card (2009), based on relative wages of high-school and college-equivalent workers across large US cities, has reported estimates with an implied direct partial wage elasticity in the same range as Borjas (i.e., −0.042), the elasticity implied by estimates based on occupational groups by Card (2001) is about −0.010, in line with the estimate of Ottaviano and Peri (2012). Manacorda *et al*. (2012) have reported negligible effects of immigration on the wages of native workers in the UK. Their evidence suggests that wages of immigrants who arrived earlier were affected by later immigrants. According to their

[17] Angrist and Kugler (2003) have studied interaction effects between immigration and labor-market rigidity and have found that the immigration effect on native employment is more severe under reduced flexibility. Brücker *et al*. (2012) have further considered interaction effects on native wages and have concluded that wage effects are larger and unemployment effects smaller in countries with flexible wage-setting institutions.

Table 7. *Cross-study comparisons of the effect of immigration on native wages*

| | Parameter | Reported estimate (1) | $\bar{p}$ or $\eta$ (2) | Direct partial elasticity: $\frac{\partial \ln W^N_{ejt}}{\partial \ln M_{ejt}}$ | Direct partial elasticity: Estimate (3) |
|---|---|---|---|---|---|
| Present study, Table 3, Col. 3 | $\theta$ | −0.484 | 0.063 | $\bar{p}(1-\bar{p})\theta$ | −0.029 |
| By origin | | | | | |
| Nordic countries | $\theta$ | −2.748 | 0.013 | $\bar{p}(1-\bar{p})\theta$ | −0.035 |
| Other high-income countries | $\theta$ | −0.568 | 0.016 | $\bar{p}(1-\bar{p})\theta$ | −0.009 |
| Developing countries | $\theta$ | −0.139 | 0.034 | $\bar{p}(1-\bar{p})\theta$ | −0.005 |
| Borjas (2003), Table 3 | $\theta$ | −0.572 | 0.100 | $\bar{p}(1-\bar{p})\theta$ | −0.051 |
| Card (2001), Table 7, men, row D | $1/(\varepsilon+\sigma_{OCC})$ | 0.099 | 0.139 | $-\bar{p}(1/\varepsilon+\sigma_{OCC})$ | −0.014 |
| Card (2009), IV, Table 5 | $1/\sigma_{ED}$ | 0.26 | 0.210 | $\bar{p}(\frac{1}{\sigma_M}-\frac{1}{\sigma_{ED}})$ | −0.042 |
| log relative supply of college vs. high school (ED) | $1/\sigma_M$ | 0.06 | | | |
| Aydemir and Borjas (2007) | | | | | |
| US | $\theta$ | −0.489 | 0.1 | $\bar{p}(1-\bar{p})\theta$ | −0.044 |
| Canada | $\theta$ | −0.507 | 0.17 | $\bar{p}(1-\bar{p})\theta$ | −0.072 |
| Borjas *et al.* (2010) | | | | | |
| Black workers | $\theta$ | −0.346 | 0.1 | $\bar{p}(1-\bar{p})\theta$ | −0.031 |
| White workers | $\theta$ | −0.522 | 0.1 | $\bar{p}(1-\bar{p})\theta$ | −0.047 |
| Manacorda *et al.* (2012), | $1/\sigma_{AGE}$ | 0.193 | 0.1 | $\eta(\frac{1}{\sigma_M}-\frac{1}{\sigma_{AGE}})$ | −0.005 |
| Table 7, col. 3 (used by authors in simulations) | $1/\sigma_M$ | 0.142 | | | |
| Ottaviano and Peri | $1/\sigma_{EXP}$ | 0.07 to 0.16 | 0.1 | $\eta(\frac{1}{\sigma_M}-\frac{1}{\sigma_{EXP}})$ | −0.002 to |
| (2012) | $1/\sigma_M$ | 0.05 | | | −0.011 |
| D'Amuri *et al.* (2010) | $1/\sigma_{EXP}$ | 0.31 | 0.11 | $\eta(\frac{1}{\sigma_M}-\frac{1}{\sigma_{EXP}})$ | −0.029 |
| | $1/\sigma_M$ | 0.046 | | | |
| Bratsberg and Raaum (2012) | $\theta^*$ | −0.724 | 0.085 | $\bar{p}\theta^*$ | −0.062 |

*Notes*: Where relevant, $\bar{p}$ is the reported mean immigrant share and $\eta$ is the immigrant share of the wage bill. For the Borjas (2003), Aydemir and Borjas (2007), and Borjas *et al*. (2010) studies, mean immigrant shares are inferred from US Census Bureau (2009) and Statistics Canada (2010). For the Manacorda *et al*. (2012) and Ottaviano and Peri (2012) studies, we use their estimates of the immigrant wage share. In Card (2001), $\varepsilon$ denotes the labor supply elasticity with respect to the wage. In the Bratsberg and Raaum (2012) study, the parameter estimate is the coefficient of the term, ln*(1+M/N)*.

reported elasticities of substitution, using data from Germany, D'Amuri *et al*. (2010) have found a direct partial elasticity of −0.029 but have concluded, none the less, that immigration has limited effects on native wages, partly because labor supply effects are mitigated by the fact that immigrants who arrived previously have reduced opportunities for employment. In summary, the recent empirical body of literature reports a range of coefficients related to effects of immigration on native wages. When we convert reported estimates to a common metric, the implied effect of a labor supply shift resulting from a 10 percent increase of the immigrant labor force is a reduction in native wages of between 0.02 and 0.7 percent, with our estimate of the overall immigration effect in the middle of this range.

## VI. Conclusions

Data on Norwegian wages are used to the study wage effects from immigration, following the national skill cell approach of Borjas (2003). The estimated direct partial wage effect resulting from an immigrant-induced labor supply shock, keeping total capital and labor constant, is found to be negative for both natives and immigrants.

Although the national skill cell method is, by design, robust to bias from endogenous locational choice, estimates remain subject to bias if native labor force participation decisions respond to immigrant supply shocks. To examine whether estimates are also affected by selective participation of native workers, we take advantage of the individual longitudinal structure of the data and exclude workers with low attachment to the employment pool. Our results show that estimates of the wage effect are easily biased because immigration and participation of low-wage native men are negatively correlated. Furthermore, we provide evidence showing that the individual fixed-effects estimator is inadequate in this setting because attenuation bias arising from measurement error in the immigrant share is severely exacerbated. Our empirical analysis also points to bias from misallocation of immigrant workers from developing countries to labor-market skill cells when such allocation is based on potential labor-market experience. When we account for the various sources of bias, such as confounding demand and supply factors, selective participation, measurement error, and misallocation of immigrants across skill cells, the point estimate of the effect of an increase in the immigrant share on the native male log daily wage increases in magnitude from −0.278 to −0.484. An important empirical finding is that each of these factors gives rise to positive bias and estimates that understate the immigration effect on native wages. For immigrants, our preferred estimate of the direct partial wage effect is −0.966, which is larger than the effect on native wages, suggesting that natives and immigrants are imperfect substitutes. When we use the difference in wage effects to estimate the elasticity of substitution between immigrant and native labor within skill cells, our estimate falls to between 29 and 35.

To compare estimated wage effects of immigration across countries, we convert the adjustment coefficient to the elasticity of native wages with respect to the size of the immigrant labor force; evaluated at the mean immigrant share in our data, this is equal to −0.029. A 10 percent increase in the immigrant labor force is predicted to reduce native wages by slightly less than one-third of a percent. Compared to prior studies from other countries relying on a national approach, our estimate of the direct effect of immigration on native wages lies in the middle of the range of elasticities that we compute from reported parameter estimates.

We also study differential wage effects by immigrant origin, and find a substantial negative native wage effect of immigration from the Nordic region, while inflows from countries that are geographically, economically, and culturally distant seem to have a modest effect on native wages, if any at all. This pattern is consistent with factor demand theory when immigrant workers from similar and neighboring countries are close substitutes to native workers. The growth in the immigrant population in Norway and other European countries until the mid-2000s was driven in the main by immigration from developing countries with low substitutability with the native workforce and, therefore, having small effects on native wages. While Card (2009) has concluded that immigration has had a limited effect on the overall native wage structure in the US, because the composition of high-school and college equivalent immigrants is similar to that of natives, our evidence suggests that wage effects in Norway have been modest over the sample period because of the composition of the origin of immigrant inflows.

To reach our conclusions, it turns out to be particularly important to take into account demand factors and selective attrition because cross-border mobility within the Nordic countries is highly sensitive to labor-market conditions (Lundborg, 2006; Pedersen and Røed, 2008). Indeed, our estimate of the direct partial native wage elasticity with respect to immigration from the Nordic countries changes from 0 to $-0.035$ when we account for these sources of bias. These lessons extend beyond the Nordic experience. To avoid the bias towards zero that is often present in spatial approaches and to uncover the true effect of immigration on wages, even national approach studies need to address endogenous immigration and selective native participation when movements between neighboring countries are liberalized, as in Europe. An important corollary is that common labor markets and free labor mobility moderate wage fluctuations, with migrant labor flows between close countries operating as automatic stabilizers over the business cycle, reducing upward pressures on wages, prices, and interest rates in times of economic expansion. Although immigration of highly substitutable labor from nearby countries limits native wage growth during good times, Norwegian workers benefit from multilateral policies with common labor markets and expanded employment opportunities in periods of low domestic labor demand.

## Appendix: Allocating Immigrants with Missing Education Data to Skill Cells

We use educational attainment collected from the National Education Database (Vangen, 2007). The education database is built up from records obtained directly from Norwegian educational institutions and the Norwegian State Educational Loan Fund, as well as self-reported attainment taken from census records and two surveys (from 1989 and 1999) that were administered to all foreign residents with missing educational attainment. Despite such sources of educational information, missing education records remains a problem in the immigrant labor force data. To illustrate, the fraction of resident immigrants in our data with missing records of educational attainment is 0.138 in 1993, 0.121 in 1999, and 0.387 in 2006. In order to compute immigrant shares by education and experience levels, it is therefore necessary to allocate immigrants with missing data across skill groups.

Our allocation procedure starts with the assumption that for each observation year, birth cohort, gender, and country of origin (broadly defined in four major regions), the distribution of attainment is the same for immigrants with missing and non-missing data. To illustrate the procedure, consider the 427 resident male immigrants born in 1959 in one of the neighboring Nordic countries and counted in the Norwegian labor force in 2006. Of these 47-year-old men, 129 have missing records for educational attainment. Among the 298 men who do have records for educational attainment, the frequency distributions across the four attainment levels used in the analysis are 40, 27, 25, and 8 percent. Accordingly, we estimate that, in 2006, the count of Nordic male high-school dropouts with 30 ( $= 47 - 17$) years of experience is 52 persons ($0.40^{*}129$) higher than the observed count (120); that of high-school graduates with 28 years of experience is 35 persons higher; that of men with some college and 24 years of experience is 32 persons higher; and that of college graduates with 21 years of experience is 10 persons higher than the observed count. When we follow the same procedure for other birth years, we estimate that the 2006 count of Nordic male dropouts with 26–30 years of experience is 202 persons higher than the observed count of 596 persons. A feature of the allocation procedure is that it tends to increase counts in low-education/low-experience cells and to leave counts in high-attainment/high-experience cells unchanged. The reason for the latter is that very few immigrants in the oldest birth cohorts (i.e., born before 1946) have missing data. Conversely, for the majority of, say, 20-year-old Nordic males, we lack education data, and these individuals must by definition belong to a low-attainment/low-experience cell.

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