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Immigration Wage Impacts by Origin

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Non-Technical Abstract

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1. Introduction

During the past three decades the immigrant share of the Norwegian population increased from two to ten percent. And, in line with trends in other high-income countries, the composition of immigrant inflows changed radically with regard to country of origin (Bauer et al, 2000). Prior to the 1980s, the majority of 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. An important question is whether such demographic changes have implications for the labor market. Both for the evaluation of immigration policy and for insight into drivers of economic development more generally, solid evidence on how immigration from different origins affects the labor market is needed.

Our interest is wage effects of immigration. The standard theory of equilibrium wages based on a 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, with the size of the wage reduction determined by the degree of substitution between skill groups as well as between immigrant and native workers with similar skills. In this paper, we seek to identify this direct partial wage effect of immigration (see the discussion of partial and total wage effects in Ottaviano and Peri, 2008). We apply the “national approach” introduced by 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 wages of individual native workers are modeled as a function of the immigrant share in their skill group.

The paper contributes to the European national approach literature on the wage effect of immigration. In addition, we contribute to the general literature in several ways. First, 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. To our knowledge, no prior study has addressed the composition of immigrant supply shocks *within* skill group. Immigrants from diverse source countries and cultures are likely to differ in their substitutability with native workers. While migrants from 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.

Another reason to disaggregate immigrant inflows by origin is that admission categories vary dramatically 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.

Second, allocating immigrants to the appropriate education-experience cell is a fundamental challenge to this methodology, accentuated by the high rates of non-employment among immigrants from developing countries, particularly during the first period after arrival. As 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 employment rather than years since arrival.

A third contribution is that, in contrast to past studies, wage effects are estimated using a large register-based data set with individual panel information. If immigrant supply shocks affect wages and employment opportunities of native workers, a major concern for wage impact studies is that native attrition might be non-random. For example, 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. 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 studies, we include fixed effects for education, experience, and year of observation, as well as 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, as in Welch (1979), 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 understatement of the effect of immigration on native wages.

2. Background

Wage and employment effects of immigration have typically been studied empirically by the "spatial approach," in which labor market clusters are delimited by geographical borders within the receiving country. 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 with national labor market clusters. However, as regional boundaries are easier to cross than national borders, endogenous location presents a challenge to identification in studies of local labor market effects. Immigrants may seek out geographical areas with relatively favorable labor demand conditions. Moreover, if native workers respond to high immigrant inflows by moving out – 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 the simultaneity problem researchers have applied instrumental variable techniques and explored natural experiment situations (Card, 1990; Hunt, 1992; Friedberg, 2001). Reviews of a vast research literature – of which the majority 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; and Okkerse, 2008).

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 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 may confound immigration with other skill-group specific labor supply or demand shocks that affect relative wages. One candidate is skill-biased technological change that may have improved the labor market opportunities of relatively young and highly skilled natives over time. Another problem is selective participation within skill cells, causing the within-cell composition of individual unobserved characteristics to change over time.

Empirical evidence on the relationship between immigration and native outcomes remains dominated by studies using the spatial approach. Two recent examples are Dustmann et al (2008) and Card (2009). Dustmann et al use the spatial approach to analyze the impact of immigration along the wage distribution of native UK workers. The study concludes that immigration depresses wages below the 20th percentile but generates slight wage increases in the upper part of the distribution. The authors conclude that the overall wage effect of immigration is slightly positive. Card presents several analyses of the relationship between immigration and wage inequality in the United States. Using across city comparisons, he reaches three main conclusions: i) workers with below high school education are perfect substitutes for those with a high school education, ii) high school equivalent and college equivalent workers are imperfect substitutes, and iii) within education groups, immigrants and natives are imperfect substitutes. Together, these results imply that the impacts of recent immigration on native relative wages are small.

Compared to the bulk of studies using geographical variation in immigration, the numbers based on national variation are fewer, albeit fast growing. Analyzing US data from 1960 to 2000, Borjas (2003) concludes that an immigrant inflow that leads to a ten percent labor supply shock reduces the weekly earnings of native workers by about four percent. Aydemir and Borjas (2007) analyze wage effects from changes in labor supply in three countries: the United States, Canada, and Mexico. They find numerically comparable and statistically significant wage effects of immigration in each of the three countries and in the same range as the original Borjas study. The three countries have experienced very different patterns of immigration-induced supply shifts over time. The similarity in estimated wage effects across countries can therefore hardly be the result of the same underlying process of skill-biased technical change, a possible confounding factor in impact studies. Bohn and Sanders (2007) study the impact of immigration on wages in the Canadian labor market. In contrast to Aydemir and Borjas (2007), Bohn and Sanders find very small wage effects. Aydemir and Borjas (2007: 664) argue that the main reason for the discrepancy between the two studies is that Bohn and Sanders use a smaller data set with too few immigrants.

In recent work based on US census data, Ottaviano and Peri (2008) extends the structural modeling approach of Borjas (2003) to assess the overall impact of immigration on wages while allowing for imperfect substitutability between native and immigrant workers. Their empirical estimates point to a negative, but small, direct partial effect: an immigration shock that increases the labor force in a particular skill cell by ten percent reduces wages of natives of the same group by approximately one percent.

Peri and Sparber (2009) focus on comparative advantages and task specialization. If less-educated foreign and native-born workers specialize in performing different tasks, their model predicts that immigration will cause natives to reallocate their task supply, thereby reducing downward wage pressures. Using occupational task-intensity data across US states from 1960 to 2000, the study finds that foreign-born workers specialize in occupations that require manual and physical labor skills while natives specialize in jobs more intensive in communication and language tasks. Peri and Sparber argue that increased specialization might explain why many empirical analyses of the impact of immigration on wages and employment for less-educated native born find small effects.

Prior European studies that use the national approach include Bonin (2005), Steinhardt (2010), D'Amuri et al (2010), Carrasco et al (2008), and Manacorda et al (2010). Using German data for the period 1975-1997, Bonin (2005) concludes that the direct impact of immigration on native wages is small as a ten percent increase in labor supply stemming from immigration is predicted to reduce wages by less than one percent, with a stronger negative impact for low-skilled natives. Steinhardt (2010) replicates the Bonin study and argues that the low impact estimate of the prior study is caused by non-applicability of the skill-cell approach in German data. When he instead defines labor-market cells by occupation and experience, he finds much larger effects of immigration on wages of German natives. Examining the effects of immigrant flows to Germany during the 1990s, D'Amuri et al (2010) conclude that immigration had limited effect on native wages, but sizable effects on employment and small adverse effects on wages of previous immigrants. While previous and recent immigrants seem to be perfect substitutes within education-age cells in the German data, immigrants and natives are not. Carrasco et al (2008) estimate the partial impact of immigration on wages of native workers in Spain with an approach based on gender-education-experience cells and find no significant effects. Manacorda et al (2010) analyze the impact of immigration on the wages of male UK workers using micro data from the mid-1970s to the mid-2000s. The study fails to uncover discernable effects of increased immigration on the wages of native workers, partly because of imperfect substitutability within education-age cells. The only sizeable effect of increased immigration is on the wages of immigrants who arrived in the UK at an earlier date.

To our knowledge, no prior study has addressed the origin composition of immigrant supply shocks *within* skill group as we do in this paper. Moreover, prior national approach studies rely on repeated cross-sectional data and are unable to address consequences of compositional change within skills groups, which we do drawing on our longitudinal data.

3. 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 literature assessing wage impacts of immigration to interpret reduced form regression coefficients within a structural framework of one-output, nested, constant elasticity of substitution (CES) production technology. Ignoring capital, total product (Q_t) depends on labor (L_t) and a technology parameter (B_t),

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

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, Manacorda et al, 2010; Ottaviano and Peri, 2006; 2008). At the highest level, labor is the aggregate of E levels of education (L_{et}),

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

where a_{et} reflects the relative efficiency of education level e in year t. L_{et} is the number of workers with education e in year t. The substitution parameter, $\rho = 1 - \sigma_E^{-1}$, where σ_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

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

where b_{ejt} reflects the relative efficiency of different experience groups for each education group in year t. L_{ejt} is the number of workers with education e and experience j in year t, and $\tau = 1 - \sigma_J^{-1}$ where σ_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,

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

where c_{ejt} reflects the relative efficiency of immigrants within skill group. The parameter $\lambda = 1 - \sigma_M^{-1}$, where σ_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,

(5) $$W_{ejt}^{N} = \alpha Q_t L_t^{-\rho} a_{et} L_{et}^{\rho-\tau} b_{ejt} L_{ejt}^{\tau-\lambda} N_{ejt}^{\lambda-1}.$$

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 effect* (Ottaviano and Peri, 2008) resulting from an immigrant-induced increase in supply, holding native labor supply and capital constant (Borjas, 2009). Within the present theoretical framework, the direct partial wage effect of immigration may be expressed by the elasticity

(6) $$\frac{d \ln W_{ejt}^{N}}{d \ln M_{ejt}} = (\tau - \lambda) \frac{d \ln L_{ejt}}{d \ln M_{ejt}} = (\sigma_M^{-1} - \sigma_J^{-1}) \eta_{ejt}$$

where η_{ejt} is the immigrant wage share. In (6) we ignore the shared wage impact of changes in total labor supply of group *e*.

When $\sigma_M^{-1} = 0$ there is perfect substitutability between immigrants and natives within skill group and the partial derivative in equation (6) may 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. Some recent studies (Ottaviano and Peri, 2008, using US data, and Manacorda et al, 2010, using UK data) indicate imperfect substitutability within skill group, based on the finding that the relative wage of (previously arrived) immigrants 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 start out with the approach of Borjas (2003). Educational attainment and work experience are used to classify individuals into (4 levels of education * 8 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

$$P_{ejt} = \frac{M_{ejt}}{M_{ejt} + N_{ejt}}, \tag{7}$$

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 setup is the wage regression model (Borjas, 2003),

$$\ln W_{iejt} = \theta P_{ejt} + s_e + x_j + \pi_t + (s_e \cdot x_j) + (s_e \cdot \pi_t) + (x_j \cdot \pi_t) + \gamma Z_{ejt} + u_{iejt} \tag{8}$$

where W_{iejt} is the wage of 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 π_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. Thus, the (group) fixed effects absorb the influence on wages of changes in total labor supply and in the aggregate supply of workers with different levels of educational attainment, as well as the change in aggregate supply within experience groups.

Unlike previous studies, we also split the immigrant labor force share by origin (P_{rejt}), where

$$P_{rejt} = \frac{M_{rejt}}{M_{ejt} + N_{ejt}}, \quad \sum M_{rejt} = M_{ejt}. \tag{9}$$

The regions 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 differential 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 immigrant wage effects by origin, we simply replace the term θP_{ejt} in equation (8) with three separate immigrant shares by origin and free coefficients.

Note that θ is estimated conditionally on a rich set of time and skill group fixed effects that will capture any effects of increases in total and education specific labor supply and where interactions with differential time patterns will account for demand shocks that are shared within education and experience levels. The coefficient θ will be consistently estimated as long as the residual unobserved component of equation (8) is 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 may be outside factors that influence both native wages and immigrant inflows. For example, as business cycle movements and labor demand shocks can be expected to affect migration flows of workers with low mobility costs and easy access to the Norwegian labor market, one might worry that the immigrant share increases in years with favorable employment and wage conditions. In an extended 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 group over time by means of an indicator measuring 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 impact. 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. One, we estimate equation (8) with individual fixed effects; i.e., where $u_{iejt} = \alpha_i + v_{iejt}$, and two, we exclude from the wage sample marginal workers who move in and out employment, i.e., workers with low attachment who will be the source of bias from any selective attrition.

Since 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 may create substantial attenuation bias. Although sampling error, as in Aydemir and Borjas (2005), is not directly relevant due to our administrative full coverage register data, there are other potential sources of measurement imprecision. 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, 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 due to differences in schooling structure, quality, and curriculum. Allocation of immigrants with missing information on educational attainment (see details in the appendix) is also another contributor to measurement error in P_{ejt}. While estimation with individual fixed effects will account for selective attrition, a drawback of the fixed-effects estimator is that any attenuation bias from measurement error in P_{ejt} can be greatly exacerbated. Drawing on the approach of Griliches and Hausman (1986), we will examine the importance of attenuation by eliminating individual observations close in time and where regression residuals are likely to be highly auto-correlated.

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}). [1] Incomplete registration suggests that immigrants may be systematically undercounted. Unlike attenuation bias from classical measurement error, incomplete registration could lead to inflated estimates of the effect of immigration ("scaling bias"). 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 2-4 million illegal immigrants living in the United States 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. Below we therefore also report the elasticity of native wages with respect to the size of the immigrant labor force, as this metric is unaffected by any (proportional) undercounting of immigrants. In the context of the CES framework and the empirical specification in equation (8), the elasticity in equation (6) becomes

$$\text{(6')} \qquad \frac{d \ln W_{ejt}^N}{d \ln M_{ejt}} = (\sigma_M^{-1} - \sigma_J^{-1})\eta_{ejt} = \theta P_{ejt}(1 - P_{ejt}) .$$

Because of large differences in immigrant shares across origin groups, the elasticity in equation (6') may be a more appealing metric for cross-group comparisons than are direct estimates of the parameter θ. For similar reasons, the wage elasticity with respect to the size of the immigrant labor force emerges as a more meaningful metric for cross-study comparisons of the effect of immigration on wages.[2]

[1] In a study of the Norwegian construction sector, Bratsberg and Raaum (2010) report that about one half of the immigrants employed in that sector are not registered permanent residents of Norway.

[2] Note that the alternative metric, $\partial \ln W / \partial (M/N)$, which is commonly used in the literature and forms the basis for evaluation of wage effects of a ten percent immigration-induced labor supply shock cited in Section 2, will also be sensitive to scaling bias.

4. Data

Our data are extracts of information from several administrative registers that cover all residents in Norway during the 14-year period 1993-2006. The core variables are residency, labor force participation, educational attainment, work experience, and wage earnings. This section provides details.

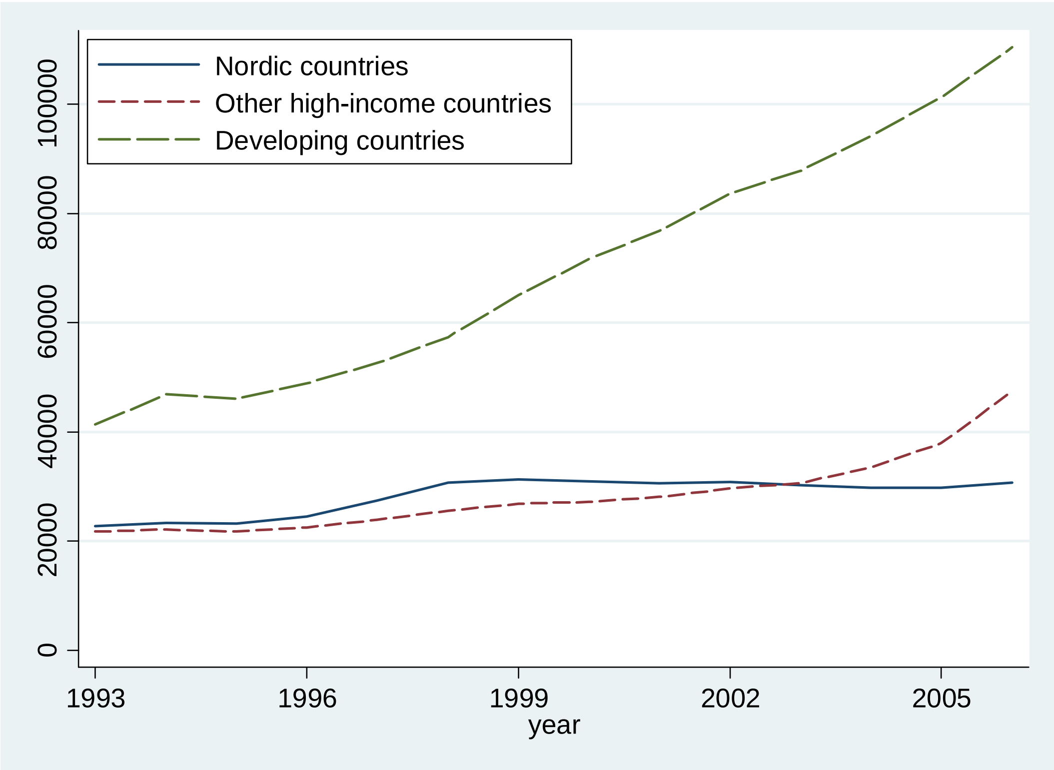
4.1. The immigrant labor force

The trends in the resident immigrant labor force over the sample period are shown in Figure 1, where immigrants are defined as foreign-born residents with two foreign-born parents. Due to high inactivity rates among many groups of immigrants from developing countries (OECD, 2001), we estimate effects of supply shocks from those actively participating in the labor market rather than the stock of foreign-born residents. Counted as labor force participants are individuals who are employed, registered unemployed, or labor market program participants. The immigrant labor force has increased sharply over time, mainly because of large inflows of immigrants from developing countries. While Nordic immigrants remained dominant until the late 1970s, individuals born in poorer and culturally more distant countries outside Western Europe constitute the vast majority of the immigrant population today.

Since 1954, the Nordic countries have constituted a common labor region. As Nordic citizens need no 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 due to 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 Nordic immigrants in Norway earn a little less than natives with comparable human capital characteristics just after arrival, they catch up within a few years (Barth et al, 2004).

In spite of restrictions on immigration from countries outside the Nordic region, most workers, independent of country of origin, would receive a work permit if s/he had 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 protection (political asylum) rather than work. After the

Figure 1. Resident immigrant labor force by origin, 1993-2006



Note: Resident 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.

"immigration stop," non-Nordic citizens were granted work permits only if accepted as "specialist workers."[3] In 1994, most West Europeans gained access to the Norwegian labor market through the establishment of the common EU labor market, and in 2004 citizens of the new EU member countries in Eastern and Central Europe gained access on similar terms (with some temporary restrictions). After 2005 the inflow of labor immigrants from this region has increased considerably.

Between 1990 and 2007, over 50 percent of immigrants from high-income countries were admitted as labor immigrants, while nearly 35 percent entered due to family relations with these or other people living in Norway (Statistics Norway, 2010). Among immigrants from developing countries, only 4 percent arrived as labor immigrants while 57 percent were admitted as refugees and about 30 percent on family reunification. Thus, immigrant flows

[3] 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 five thousand per year, a limit that has not been filled to date.

from outside the Nordic and other high-income countries were less likely directly related to business cycle movements compared to other inflows.

4.2. Immigrant supply shocks by skill

Following Borjas (2003), 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 one to 40 years of potential experience are allocated into four education levels (less than high school, high school, short college/university, long college/university) and eight five-year Mincer experience intervals. Our data contain information on educational attainment for (practically) all natives and we measure Mincer experience as years since leaving school, with school-leaving age computed as six plus statutory years of the individual attainment.

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. We collect data on attainment from the education register, 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 equal 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 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 a very different labor market experience due to conflicts and high rates of unemployment. 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 back to 1967 of all residents enabling us to observe post-arrival labor force participation among 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.

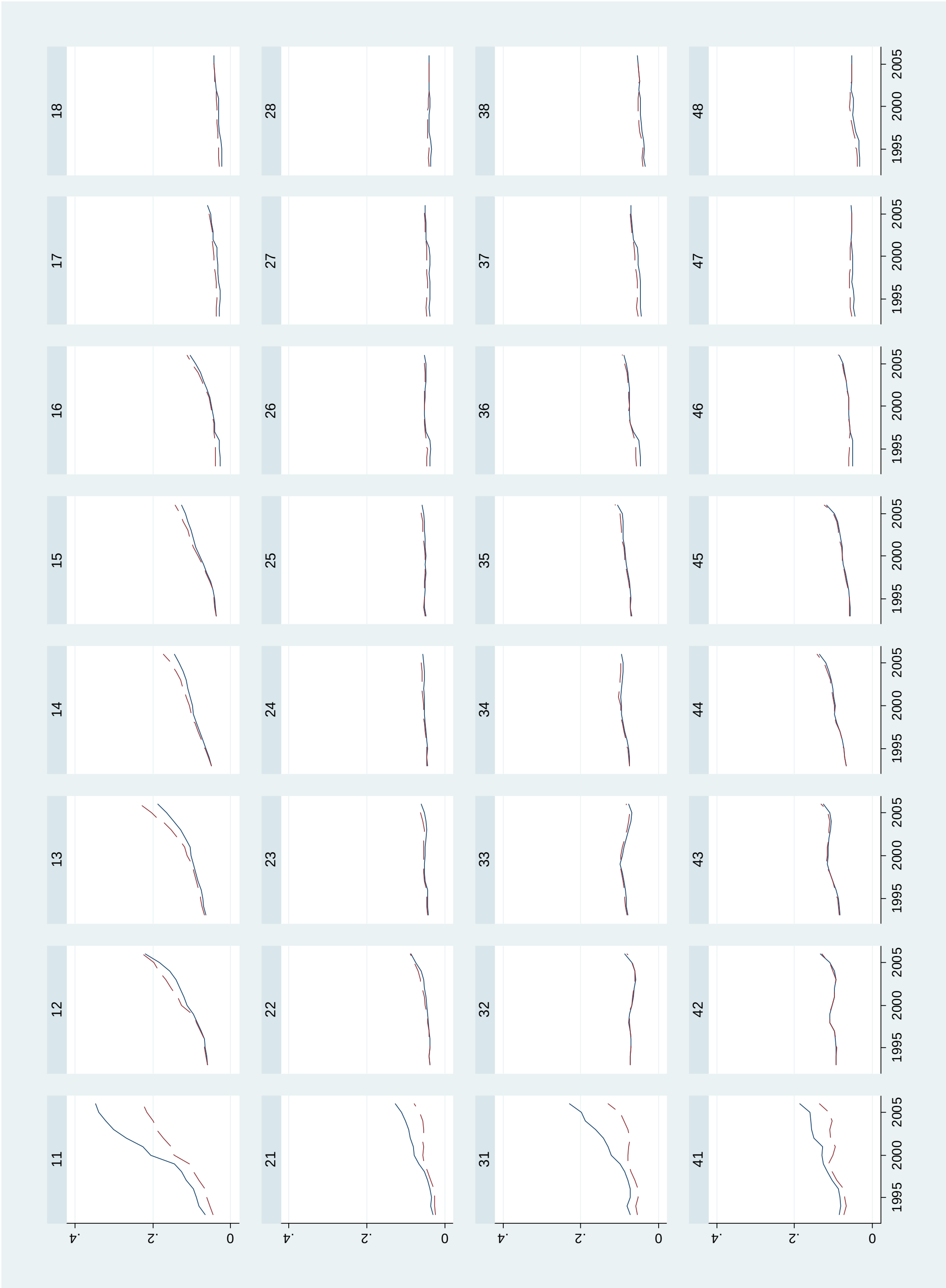
Figure 2 displays how the total male immigrant shares (P_{ejt}) evolve over the sample period. The dashed lines show immigrant shares based on potential (Mincer) experience, while the solid lines are based on effective experience. As the figure shows, immigrants are concentrated in skill groups with short experience and low education. Since the adjusted measure 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.

4.3. Native wages

The wage data are taken 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 vs. part time), contract period, and total pay. Hours worked are reported in three broad brackets only (two part time and a full time bracket). Even if we cannot calculate the hourly wage, we come close by the constructed daily wage computed as total pay divided on the number of days of the employment contract. Our primary empirical focus is the daily wage for full time workers, but we also report results for all workers including those on part time contracts. Finally, we also have annual labor earnings which sums wage income across all jobs as well as income from self-employment.

Sample means are shown in Table 1. The samples underlying wage regressions are based on a ten percent random extract of native workers (i.e., Norwegian born with two Norwegian-born parents) who appear in the population register during the sample period, 1993-2006. (Note however that computations of immigrant shares are based on the complete labor force.) Wages are increasing in educational attainment, but differentials are not large by international standards reflecting the low returns to schooling in Norway. In our samples, workers with low education have longer work experience than other groups because they left school early and because they on average represent older birth cohorts. The immigrant share is highest among university graduates reflecting high shares of European immigrants in particular. Immigrants from developing countries are overrepresented among the least educated, although developing country immigrant shares are high even among those with

Figure 2. Male labor force immigrant shares
by education (1st digit) and experience (2nd digit), 1993-2006



(Legend: Effective experience = solid lines; potential experience = dashed lines)

tertiary education. For men, the unemployment frequency is sharply and monotonically declining in educational attainment. For women, unemployment is more evenly distributed although with a sharp distinction between workers with or without higher education.

Table 1. Sample means

	(1) Full sample	(2) Less than high-school	(3) High-school	(4) Some college	(5) University graduate
A. Men					
Log daily wage	6.71	6.55	6.69	6.87	7.06
Experience	20.6	24.3	18.2	19.7	18.9
Unadj. imm share (male)	.066	.072	.051	.077	.085
Adj. imm share (male)	.065	.068	.050	.077	.086
Nordic	.013	.013	.011	.017	.017
Other high-income	.016	.010	.011	.025	.040
Dev. Countries	.035	.045	.029	.036	.029
Unemployment frequency	.127	.181	.129	.071	.038
Log native labor force	10.5	10.8	10.7	9.9	9.3
Observations	976,479	324,710	374,980	189,171	87,618
B. Women					
Log daily wage	6.48	6.32	6.42	6.64	6.88
Experience	20.2	25.9	16.9	17.2	14.4
Unadj. imm share (fem)	.065	.054	.062	.069	.135
Adj. imm share (female)	.064	.051	.061	.070	.137
Nordic	.017	.011	.016	.024	.031
Other high-income	.016	.007	.013	.020	.065
Dev. Countries	.031	.032	.032	.026	.041
Unemployment frequency	.129	.165	.162	.072	.071
Log native labor force	10.4	11.0	10.2	10.2	8.5
Observations	599,529	221,702	154,508	188,107	35,212

Note: Sample means pertain to full-time workers; samples consist of 10-percent random extracts of all native full-time workers.

5. Results

5.1. 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 male wage earners are presented in Table 2. In row A, the immigrant share is defined for the male labor force. The estimated wage impact (θ) for the daily wage of full-time native workers is -0.278 with a standard error of 0.073, suggesting that an immigration-induced increase in workers within a skill group lowers the wage of native male workers in that group. The estimated wage impact becomes even more negative when we include earnings from part-time work, and is tripled when we estimate the effect on *annual* labor earnings. The

Table 2. Impact of immigrant share on male native log wage

	Dependent variable		
	Daily full-time wage	Daily wage, incl. part-time work	Annual labor earnings
A. Male labor force immigrant share	-0.278 (0.073)	-0.384 (0.079)	-0.860 (0.142)
B. Include women in the labor force	-0.188 (0.065)	-0.286 (0.071)	-0.730 (0.131)
C. Effective experience (male labor force)	-0.312 (0.077)	-0.450 (0.088)	-0.922 (0.149)
Observations	976,479	1,031,233	1,152,884

Note: Standard errors in parentheses are clustered within 448 education-experience-year cells. Fixed effects for year, education group, experience cell and interactions year*education, year*experience as well as education*experience (a total of 174 control variables) are included in the regression model.

particularly large effect on annual earnings indicates that hours (i.e., days) worked may be even more adversely affected by immigrant supply shocks than the daily wage. When we include women in the labor force, the effect estimate is generally smaller as shown in row B. One interpretation is that native men and immigrant men are closer substitutes than are native men and immigrant women.

As discussed in section 4, prior evidence both from Europe and North America 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 Table 2, row C, we report the estimated wage impact from immigration when immigrants from developing countries are allocated across experience cells using their effective work experience in Norway rather than years since leaving school. For all three wage 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 ten 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.

Table 3. Impact estimates with additional controls for demand and supply shocks

	Dependent variable		
	Daily full-time wage	Daily wage, incl. part-time work	Annual labor earnings
A. With demand control			
Male labor force immigrant share	-0.327	-0.465	-0.943
	(0.077)	(0.086)	(0.147)
Unemployment frequency	-0.657	-0.526	-0.790
	(0.094)	(0.108)	(0.180)
B. With supply and demand controls			
Male labor force immigrant share	-0.405	-0.517	-0.978
	(0.078)	(0.088)	(0.152)
Unemployment frequency	-0.640	-0.514	-0.781
	(0.093)	(0.107)	(0.179)
log native labor force	-0.040	-0.031	-0.026
	(0.010)	(0.010)	(0.015)
Observations	976,479	1,031,233	1,152,884

Note: Immigrant shares are computed from male labor force using effective experience for immigrants. Standard errors (clustered within education-experience-year cells) are reported in parentheses.

5.2. Accounting for within-skill cell labor demand and supply shocks

In spite of the elaborate controls included in the model, there remains a concern that residual skill-group specific labor demand shocks may bias the estimate of the immigration wage effect in a positive direction if immigrants tend to enter the Norwegian labor market under favorable conditions. To control for variation in labor demand within skill group 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 Table 3, Panel A, estimates become slightly more negative when we control for the unemployment frequency. Further, consistent with a broad 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 change in the number of native worker in the skill group, as adjustments in the number of native workers mechanically will alter the fraction of immigrants in the cell. Due to shifts in educational

Table 4. Wage impacts of immigration by origin

	(1) Basic	(2) With demand control	(3) With demand and supply controls
Immigrant share by origin			
Nordic countries	-0.031	-0.645	-1.338
	(0.553)	(0.516)	(0.488)
Other high-income countries	-0.009	0.299	0.051
	(0.359)	(0.316)	(0.321)
Developing countries	-0.427	-0.437	-0.392
	(0.127)	(0.118)	(0.129)
Unemployment frequency		-0.684	-0.677
		(0.099)	(0.096)
log native labor force			-0.042
			(0.010)
F-test of H_0: Equality of origin-specific coefficients, p-value	0.444	0.130	0.102

Note: Dependent variable is the daily wage of full-time workers. Sample contains 976,479 observations. Standard errors (clustered within education-experience-year cells) are reported in parentheses. Immigrant shares are computed from male labor force using effective experience for immigrants from developing countries.

attainment and fluctuations in birth cohort size, change in the group-specific native workforce will be negatively correlated with change in the immigrant share. If a positive native supply shock reduces the competitive wage, a concern is that our immigrant impact estimate will be biased towards zero. Consistent with this argument, Panel B of Table 3 reveals that the estimated wage effect becomes even stronger when we condition on the log size of the native labor force in each education-experience-year cell.[4]

The composition of the immigrant labor supply shock may have implications for how native wages are affected. From the factor demand theory discussed in section 3, we would expect wages of native males to be more strongly affected by immigrants from the neighboring Nordic countries because they represent closer substitutes to the native labor force than other immigrant groups. At first glance, the empirical evidence does not confirm this prediction as the Nordic immigrant share has no effect in the basic specification while wages of native men are negatively affected by immigration from developing countries; see Table 4, column (1). 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

[4] Borjas (2003, p. 1350) fails to uncover a similar bias. The implication is that the Norwegian data contain variation in skill cell size (correlated with wages and not captured by the two-way interactions of the model) that is not present in the US data.

the basic model for Nordic immigration in column (1) is biased towards zero. As the table demonstrates, including demand (and supply) controls are particularly important when it comes to 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 are other immigrant flows. Unless we account for fluctuations in labor demand (and supply), the negative wage impact of immigration from close countries is likely to be masked. On the other hand, inclusion of the labor demand control does not affect the impact estimate of immigration from developing countries. These immigrant groups face higher migration costs and meet more restrictions on movements across countries. Yet, even though the point estimates from the extended specification in column (3) suggest that immigration from the Nordic countries limits native wage growth more than immigrant flows from developing countries, the hypothesis of equal effects by origin cannot be rejected by a standard Wald test.

5.3. Selective native attrition

Average wages within skill groups are potentially influenced by any presence of workers with low labor market attachment who move in and out of employment (Borjas et al, 2008). Unless participation is random (i.e., unrelated to job opportunities), the wage impact estimate based 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 due to change in the composition of the employment pool (Bratsberg and Raaum, 2010). In Table 5, 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. First, in column (2) we exclude individuals with low labor market attachment from the sample by dropping those who participated 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). With a total impact factor including all immigrant groups (row A), the estimated wage effect increases (in absolute value) to -0.465, which is consistent with the argument that sample inclusion of marginal workers renders a positive bias in impact estimate reported in prior tables.

Table 5. Wage impacts accounting for selective sample attrition

	(1) Full sample (from Tables 3 and 4)	(2) Restricted sample: Exclude low-attachment workers	(3) Full sample: Individual fixed effects	(4) Restricted sample: Individual obs 3 years apart	(5) Restricted sample: Individual fixed effects, obs 3 years apart
A. Common immigrant impact coefficient	-0.405	-0.465	-0.129	-0.484	-0.338
	(0.078)	(0.096)	(0.121)	(0.108)	(0.166)
B. Immigrant share by origin					
Nordic countries	-1.338	-2.148	-0.887	-2.748	-0.581
	(0.488)	(0.465)	(0.453)	(0.649)	(0.596)
Other high-income countries	0.051	-0.645	-0.132	-0.568	-0.337
	(0.321)	(0.320)	(0.254)	(0.403)	(0.301)
Dev. countries	-0.392	-0.167	0.001	-0.139	-0.297
	(0.129)	(0.129)	(0.184)	(0.149)	(0.275)
Observations	976,479	868,876	976,479	319,000	319,000
Individuals			113,220		82,387
F-test of H_0: Equality of coeffs, p-value	0.102	0.001	0.269	0.001	0.926

Note: Dependent variable is the daily wage of full-time workers. Regressions control for cell unemployment rate and log native labor force. Standard errors (clustered within education-experience-year cells) are reported in parentheses.

A closer look at mobility patterns in the data reveals that low-pay employees indeed are 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.[5] There are several pull and push factors that may explain this association, but we do not make any attempt to disentangle them in this paper. From the (limited) perspective of identifying wage effects, sample inclusion of the marginal native workforce imparts a positive bias in the coefficient estimate of the immigrant share.

The wage effects of immigration by origin change dramatically when we exclude low-attachment workers from the wage sample, see Table 5, panel B, column (2). The negative effect of Nordic immigration now becomes substantially larger and statistically significant. 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. Importantly, the null

[5] Results are available on request.

hypothesis that the immigration wage effect is independent of immigrant origin is strongly rejected by a Wald test. A persuasive pattern to emerge in Tables 4 and 5 is that the coefficient estimate for immigration from nearby countries is highly sensitive to sample inclusion of native workers with low attachment and to model inclusion 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 labor market attachment is affected by immigration from the Nordic countries. Once the sample and model specification accounts for such factors, estimates show that immigrant inflows from neighboring countries have strong effects on the native wage structure and that wage impacts depend on immigrant origin.

An alternative strategy to account for selective participation (frequently used in empirical labor economics) is to estimate the wage equation with individual fixed effects. Ignoring origin composition, our individual fixed effects estimate (Table 5, col. 3, row A) 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. This conclusion directly contradicts that based on the first strategy of excluding native workers with low labor force attachment from the sample. We will however argue that little weight should be placed on the full-sample individual fixed effects estimates in column (3) because they are severely biased towards zero due to 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 as attenuation bias can be severely amplified. As discussed in section 3, in our application there are several reasons why supply shocks from immigrant labor are hard to measure correctly at the skill-cell level. 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.[6] Moreover, since the (true) immigrant share is auto-correlated, 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 impacts via variation in the change in the immigrant share *within* individuals. Becasue the immigrant share is correlated across years and shares in neighboring skill cells are highly correlated, the variation in the explanatory variable will be reduced substantially when individual fixed effects are included

[6] In an auxiliary regression that relates the observed immigrant share to the group fixed effects and interaction terms of equation (8), the adjusted R^2 is above 0.9.

in the empirical model. As individuals typically alter experience interval two or three times during our data window, within-individual variation is substantially lower than total variation in the explanatory variable. This will exacerbate 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 (auto-correlated) observations that are close in time. In column (4) we restrict the sample further and exclude individual observations less than three years apart and find that the estimate without individual fixed grows slightly more negative than without the restriction (-0.484 vs. -0.465). When we now introduce individual fixed effects, the fixed-effects estimate does move somewhat towards zero (from -0.484 to -0.338), but remains significantly negative (see col. 5). Compared to the consequence of including 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 (remaining) measurement error. Thus, it seems highly *unlikely* that the zero–impact estimate of the fixed-effects estimator in the full sample (Table 5, col. 3) is correct, in that the estimator adjusts for an underlying negative correlation between within-skill cell wage shocks and the immigrant share. If this were actually true, then the fixed-effects estimate based on the restricted sample with observations three years apart should also drop to zero. We conclude from this exercise that the concern that individual fixed effects models can make things worse is highly relevant in the present context.

Our main conclusions build on measures of immigrant labor force shares that are based on effective experience relevant for the Norwegian labor market. It turns out, however, that the main structure of results remains similar if we instead base immigrant shares on potential (Mincer) experience; see Table 6. The full sample model with demand and supply controls has a common immigrant wage impact coefficient of -0.397 (Table 6, col. 2), compared to -0.405 using effective experience. Again, when we exclude natives with low labor force attachment from the sample, the overall estimate is close to -0.5 and the coefficient for Nordic immigration is significantly more negative than that for immigration from developing countries (col. 4). As in Table 5, the individual fixed effect estimates are generally small in absolute value and not statistically significant, indicating severe attenuation bias in the individual fixed-effects estimates.

Table 6. Wage impacts of immigration, potential experience, native men

	(1)	(2)	(3)	(4)	(5)	(6)
	Basic (from Table 2)	Full sample (as in Tables 3 and 4)	Full sample with individual fixed effects	Restricted sample: Exclude low attachment	Restricted sample: Within-individual obs 3 years apart	Restricted sample with ind. fixed effects, obs 3 years apart
A. Common immigrant impact coefficient	-0.278	-0.397	-0.227	-0.405	-0.493	-0.263
	(0.073)	(0.075)	(0.122)	(0.074)	(0.110)	(0.149)
B. Immigrant share by origin						
Nordic countries	-0.201	-1.490	-0.523	-1.976	-2.797	-0.819
	(0.578)	(0.500)	(0.397)	(0.474)	(0.659)	(0.552)
Other high-income countries	-0.120	-0.032	0.132	-0.650	-0.768	-0.294
	(0.461)	(0.399)	(0.258)	(0.358)	(0.454)	(0.343)
Dev. countries	-0.339	-0.330	-0.319	-0.208	-0.045	-0.154
	(0.166)	(0.159)	(0.207)	(0.155)	(0.215)	(0.270)
Observations	976,479	976,479	976,479	868,876	319,000	319,000
Individuals			113,220			82,387
F-test of H_0: Equality of coeffs, p-value	0.924	0.066	0.316	0.007	0.002	0.601

Note: Dependent variable is the daily wage of full-time workers. Columns (2)-(6) control for cell unemployment rate and log native labor force. See also note to Table 5.

5.4. Cross-study comparisons

All in all, compared to the -0.278 estimate from our baseline specification, accounting for effective experience, native supply effects, demand shocks, measurement error, and selective native attrition, we end up with a preferred estimate (Table 5, column 4) of the direct effect of immigration the wage of Norwegian workers that is close to -0.5, which in turn is very similar to the preferred U.S. estimate of Borjas (2003). In light of the more compressed wage structure and stronger collective labor institutions in Norway compared to the United States, this similarity is a bit surprising.

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

Table 7. Cross-study comparisons

	Parameter	(1) Reported estimate	(2) $\bar{p}$ or η	(3) Direct partial elasticity $\frac{\partial \ln W^N_{ejt}}{\partial \ln M_{ejt}}$	Estimate
Present study, Table 5, col. 4	θ	-0.484	.062	$\bar{p}(1-\bar{p})\theta$	-0.028
By origin:					
Nordic countries	θ	-2.748	.013	$\bar{p}(1-\bar{p})\theta$	-0.035
Other high-income cntrs	θ	-0.568	.016	$\bar{p}(1-\bar{p})\theta$	-0.009
Dev. countries	θ	-0.139	.034	$\bar{p}(1-\bar{p})\theta$	-0.005
Borjas (2003), Table 3	θ	-0.572	.1	$\bar{p}(1-\bar{p})\theta$	-0.051
Card (2001), Table 7, men, row D	$1/(\varepsilon+\sigma_{OCC})$	0.099	.139	$-\bar{p}(1/\varepsilon+\sigma_{OCC})$	-0.014
Card (2009), IV, Table 5 log relative supply of college vs. high school (ED)	$1/\sigma_{ED}$	0.26	.21	$\bar{p}(\frac{1}{\sigma_M}-\frac{1}{\sigma_{ED}})$	-0.042
	$1/\sigma_M$	0.06			
Aydemir and Borjas (2007)					
USA	θ	-0.489	0.1	$\bar{p}(1-\bar{p})\theta$	-0.044
Canada	θ	-0.507	0.17	$\bar{p}(1-\bar{p})\theta$	-0.072
Borjas et al (2010) Blacks	θ	-0.346	0.1	$\bar{p}(1-\bar{p})\theta$	-0.031
Whites	θ	-0.522	0.1	$\bar{p}(1-\bar{p})\theta$	-0.047
Manacorda et al (2010), Table 7, col. 3 (used by authors in simulations)	$1/\sigma_{AGE}$	0.193	.1	$\eta(\frac{1}{\sigma_M}-\frac{1}{\sigma_{AGE}})$	-0.005
	$1/\sigma_M$	0.142			
Ottaviano and Peri (2008)	$1/\sigma_{EXP}$	0.07 to 0.16	.1	$\eta(\frac{1}{\sigma_M}-\frac{1}{\sigma_{EXP}})$	-0.002 to -0.011
	$1/\sigma_M$	0.05			
D'Amuri et al (2010)	$1/\sigma_{EXP}$	0.31	.11	$\eta(\frac{1}{\sigma_M}-\frac{1}{\sigma_{EXP}})$	-0.029
	$1/\sigma_M$	0.046			
Bratsberg and Raaum (2010)	θ*	-0.724	0.085	$\bar{p}\theta^*$	-0.062

Note: For the Borjas, Aydemir and Borjas, and Borjas et al studies, mean immigrant shares are inferred from US Census Bureau (2009) and Statistics Canada (2010). For the Manacorda et al and Ottaviano and Peri studies, we use their estimates of the immigrant wage share. In Card (2001), ε denotes the labor supply elasticity wrt the wage. In the Bratsberg and Raaum study, the parameter estimate is the coefficient of the term, *ln(1+M/N)*.

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 -0.028 is close to one half of the same metric based on Borjas (2003), -0.051, reflecting that the immigrant labor force is substantially smaller in Norway than in the United States. For Canada, Aydemir and Borjas (2007) find an even stronger impact on relative wages from immigration. Borjas et al (2010) report estimates that imply wage elasticities of -0.031 for black workers and -0.047 for white workers in the United States. While Card (2009), based on relative wages of high-school and college-equivalent workers across large US cities, reports estimates with an implied direct partial wage elasticity in the same range as Borjas (-0.042), the elasticity implied by estimates based on occupational groups in Card (2001) is about -0.010 and in line with the national time series estimate reported by Ottaviano and Peri (2008). Manacorda et al (2010) report negligible effects of immigration on the wages of native workers in the UK and their evidence suggests that immigrants who arrived earlier took the hit from new immigration and especially those with a university education. According to their reported elasticities of substitution, D'Amuri et al (2010) find a direct partial elasticity of -0.029 but conclude nonetheless that immigration has limited effects on native wages, partly because labor supply effects are mitigated by crowding out of previously arrived immigrants from employment. In sum, the recent empirical 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 impact of a labor supply shift resulting from doubling the immigrant labor force is a reduction in native wages between one half and seven percent.

5.5. Female wages

So far, all results have been for wages and earnings of native men. In Table 8, we report estimates of immigration wage impacts for native women, starting with the baseline Borjas (2003) specification comparable to Table 2 above (see Table 8, Panel A).

With the total immigrant share including both women and men in the labor force, we find a negative wage effect for native full-time women, -0.386, which is somewhat stronger than the equivalent estimate for men (see Table 2). As for men, immigrant inflows seem to reduce labor supply of native women and weekly hours in particular; the estimated wage effect is nearly tripled when we include part-time daily wages (col. 2). Unlike for men, the gender-specific immigrant share has a slightly lower effect indicating that male immigrants have a considerable effect on native female wages, see Table 8, Panel A, row 2. Adjusting

Table 8. Impact of immigrant share on female native wage

	Daily full-time wage	*Dependent variable* Daily wage, incl part-time work	Annual labor earnings
A. Potential experience			
1. Total labor force immigrant share	-0.386 (0.075)	-0.989 (0.091)	-1.097 (0.102)
2. Female labor force immigrant share	-0.356 (0.068)	-0.922 (0.081)	-1.048 (0.087)
B. Effective experience			
1. Total labor force immigrant share	-0.360 (0.083)	-0.968 (0.065)	-0.893 (0.108)
2. Total labor force immigrant share	-0.361 (0.090)	-0.926 (0.066)	-0.894 (0.095)
Unemployment freq	-0.467 (0.086)	-0.073 (0.103)	-0.423 (0.152)
log native labor force	0.021 (0.008)	0.030 (0.007)	0.030 (0.012)
Observations	599,529	918,708	1,032,402

Note: Standard errors (clustered within education-experience-year cells) are reported in parentheses.

the labor market experience of immigrants from developing countries turns out less important than for men, as allocation into groups based on effective experience provides results very similar to those based on Mincer experience; see Table 8, Panel B. As for men, the estimated wage effect is larger when we control for demands shocks (not reported in the Table 8). But since the association between the female labor force and wages is positive (presumably reflecting a labor supply effect), the estimated wage impact actually falls when we also control for native labor force in addition to the labor demand shocks.

Excluding low-attachment labor market participants from the wage sample has the opposite effect on the impact estimate for women as for men, as the estimate declines in absolute value (changes from -0.361 to -0.289; see Table 9, col. 3). While low labor market attachment and mobility in and out of employment are associated with low pay for native men, many women with high market wages spend periods out of employment (e.g., during child-bearing/caring). When we split wage effects of immigration by origin, immigrant inflows from the Nordic countries have the strongest effect on native wages, which is in line with results for men as well as theoretical predictions. Unlike for men, even immigration

Table 9. Wage impacts of immigration by origin, native women

	(1)	(2)	(3)
	Basic	With demand and supply controls	Restricted sample: Exclude low-attachment workers
A. Total labor force immigrant share	-0.360	-0.361	-0.289
	(0.083)	(0.090)	(0.074)
B. Immigrant share by origin			
Nordic countries	-1.880	-2.048	-1.467
	(0.612)	(0.650)	(0.606)
Other high-income countries	0.639	1.545	0.772
	(0.396)	(0.392)	(0.418)
Developing countries	-0.312	-0.495	-0.350
	(0.189)	(0.185)	(0.127)
Observations	599,529	599,529	442,571
Demand and supply controls	No	Yes	Yes
F-test of H_0: Equality of origin-specific coefficients, p-value	0.002	0.000	0.011

Note: Dependent variable is the daily wage of full-time workers. Cells based on effective experience. Panel A, Column (1) and (2) repeated from Table 8. Standard errors (clustered within education-experience-year cells) are reported in parentheses.

from developing countries seems to have a negative effect on the wages of Norwegian-born women. Apparently, immigrants from developing countries who participate in the labor market are closer substitutes with Norwegian women than is the case for men.

6. Conclusions

Norwegian wage data are used to the study native wage effects from immigration, following the national approach of Borjas (2003). The estimated wage impact is a *direct partial effect* resulting from an immigrant-induced increase in supply, holding native labor supply and capital constant. We find an overall negative wage impact for both men and women. For men, the wage impact is partly masked by demand and supply factors that are correlated with changes in the immigrant share.

To examine whether estimates are also affected by selective attrition of native workers, we take advantage of the individual longitudinal structure of the data and exclude workers with low attachment to the employment pool. Results show that wage impact estimates are easily biased as immigration and participation of low-wage native men are negatively correlated. We further provide evidence showing that the individual fixed effects estimator is inadequate in this setting as 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 experience. When we account for the various sources of bias—confounding demand and supply factors; selective attrition; measurement error; and misallocation to skill cell—the point estimate of the effect of an increase in the immigrant share on the male native log 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 wage impact.

To evaluate the wage impact of immigration, we convert the adjustment coefficient to the elasticity of native wages with respect to the size of the immigrant labor force, which evaluated at the mean immigrant share in our data is equal to -0.028. A ten percent increase in the immigrant labor force is predicted to reduce native wages by slightly less than one third of a percent. We argue that this elasticity is an appealing metric for cross-study comparisons of wage impacts of immigration.

Unlike other impact studies, we focus on differential wage effects by immigrant origin. We find a substantial negative native wage impact of immigration from the Nordic region, while inflows from countries that are geographically, economically, and culturally distant seem to have modest effects 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. To reach this conclusion, accounting for demand factors and selective attrition turns out to be particularly important 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 zero to -0.035 when we account for these sources of bias.

This insight extends beyond the Nordic experience. To avoid the bias towards zero that is often present in spatial approaches, 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 clearly reduce wage fluctuations, with migrant labor flows between close countries operating as automatic stabilizers over the business cycle.

Compared to previous studies from the United States, our Norwegian estimates of the impact of immigration on native wages are more in line with estimates of Borjas (2003) than those of Card (2001) and other studies that find small direct partial wage elasticities. However, because the increase in the immigrant population in Norway over the past decades has been driven in main by immigration from distant, developing countries with small wage effects, as in Card (2009) we conclude that immigration has had a very limited impact on the overall native wage structure during the period under study.

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Appendix: Allocating immigrants with missing education data to skill cells

We use educational attainment collected from the National Education Register. The education register is built up from records obtained directly from Norwegian educational institutions; the Norwegian State Educational Loan Fund ("lånekassen"); the Norwegian Agency for Quality Assurance in Education ("NOKUT," the agency that certifies education from abroad); 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 in the register. Despite the many sources of educational information, missing education remains a problem in the immigrant labor force data. To illustrate, the fraction of resident immigrants in our data with missing education 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 for educational attainment. Among the 298 men with non-missing data, the frequency distribution across the four attainment levels used in the analysis is 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 32 persons higher; and that of college graduates with 21 years of experience 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.

Figure A-1 illustrates the observed and estimated counts of Nordic males in the Norwegian labor force by skill group and year. We recognize the above example in the panel labeled "16" (educational attainment "1" and 5-year experience group "6," denoting attainment less than high-school and 26-30 years of experience). As the figure shows, in 2006 the observed cell count is almost 600 while the estimated cell count is approximately 800 (actually, 596 + 202). A pattern to emerge from the figure is that the allocation procedure "blows up" the counts in low education-low experience cells, but does not affect the counts in high attainment-high experience cells. The reason for the latter is that, among resident Nordic males, no one in the oldest birth cohorts (i.e., born before 1946) has missing education 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.

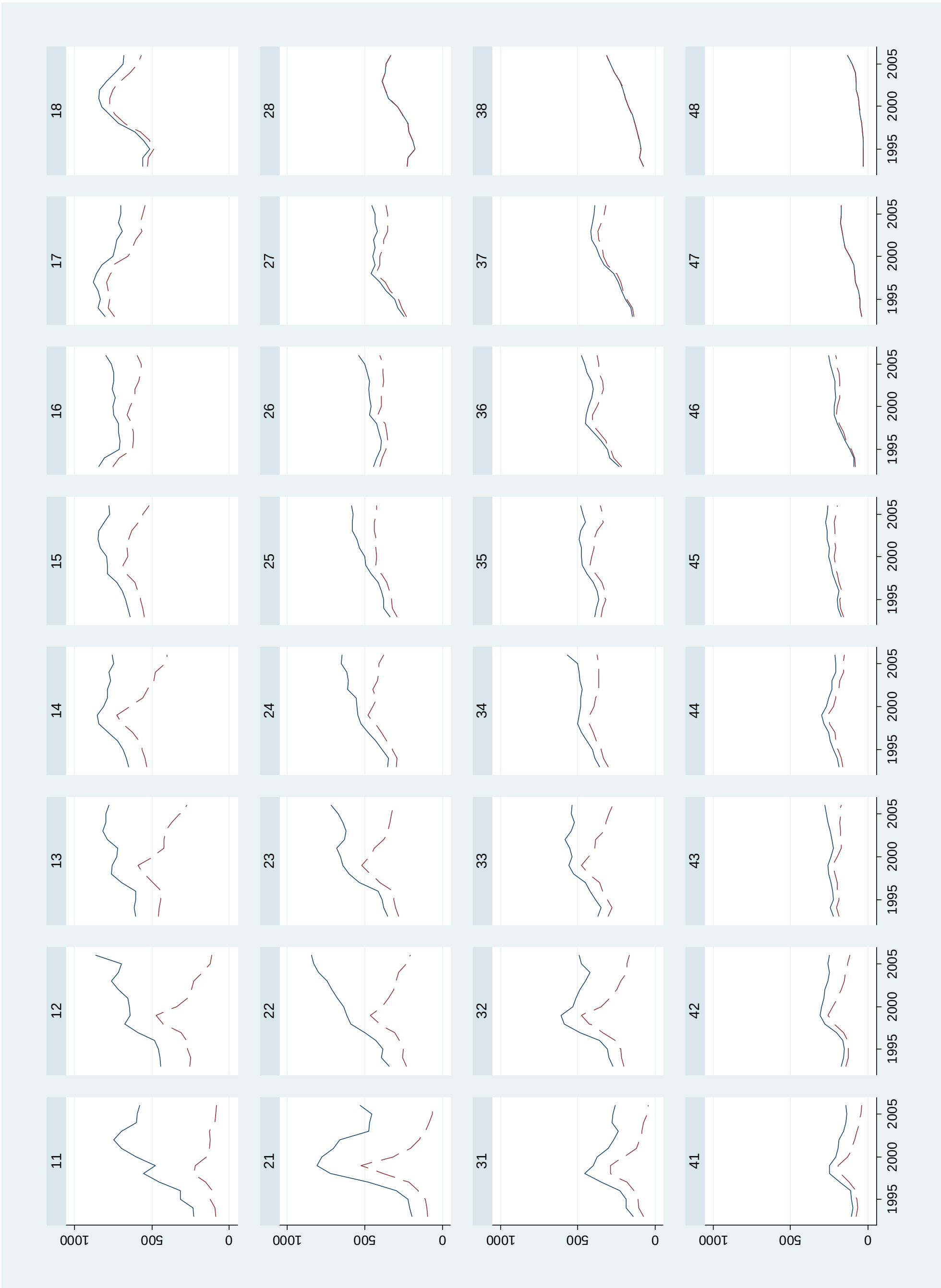

Figure A-1: Estimated (solid lines) and observed (dashes) counts of resident Nordic male immigrants in the Norwegian labor force 1993-2006, by educational attainment (1st digit) and 5-year experience cell (2nd digit).