

Job Loss and Immigrant Labour Market Performance

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Final version received 4 April 2017.

While integration policies typically focus on labour market entry, we present evidence showing that immigrants from low-income countries tend to have more precarious jobs, and face more severe consequences of job loss, than natives. For immigrant workers in the Norwegian private sector, the probability of job loss in the near future is more than twice that of native workers. Using corporate bankruptcy filings for identification, we find that the adverse effects of job loss on future employment and earnings are twice as large for immigrant employees from low-income source countries.

INTRODUCTION

The recent waves of asylum seekers to Europe have placed economic integration at the top of the policy agenda. Successful labour market integration of refugees and family immigrants is crucial for the migrants themselves as well as for the social and economic consequences of the influx. Evidence across European destinations shows, however, that in most countries, employment rates of refugee and family immigrants from developing countries fall considerably below those of comparable natives (OECD 2015; Dumont *et al.* 2016; Dustmann *et al.* 2016). In northern Europe and the Nordic welfare states, differentials are particularly large (Åslund *et al.* 2017; Bratsberg *et al.* 2017; Frattini *et al.* 2017; Sarvimäki 2017; Schultz-Nielsen 2017). Several studies also suggest that the low employment rates of immigrants from developing countries are not only due to a slow and halting integration processes after arrival, but also reflect a disproportional risk of exiting the labour market after they appear to be successfully integrated (Husted *et al.* 2001; Bratsberg *et al.* 2010, 2014; Kirdar 2012). The implication is that *job loss*, and its consequences for future employment opportunities, plays an important role in explaining differences in long-term economic outcomes between immigrants and natives.

This paper addresses the sources and consequences of job loss that is not voluntary or caused by misconduct, and hence exogenous from the individual worker's point of view. We explore two main reasons why involuntary job loss may have particularly severe impacts on the employment and earnings patterns of immigrants. First, immigrants can be more exposed to job loss, either because they happen to work in firms, industries and occupations that are prone to closure and downsizing, or because they are more likely than their native co-workers to be selected for layoff during downsizing processes, for example, because they hold marginal jobs or have short tenure. Second, job displacement may have particularly severe consequences for immigrants, as they typically possess less general human capital directly applicable in the host-country labour market and have inferior majority language skills, social capital and networks when compared to native workers (Dustmann *et al.* 2015).

Methodologically, we follow a large literature examining the individual consequences of job loss by means of comparing employment and earnings paths for separated and non-separated workers (Hamermesh 1987; Ruhm 1991; Jacobson *et al.* 1993; Neal 1995; Kletzer 1998; Hallock 2009; Huttunen *et al.* 2011). The major challenge is to identify the *causal effects* of involuntary job loss on future earnings and employment, with a need to

disentangle effects of job loss from systematic differences in outcomes between displaced and non-displaced workers that are causally unrelated to the displacement event. While displacement studies typically compare stayers and displaced workers originating from the same firm, we define ‘treatment’ at the firm level and include future outcomes of all workers, in line with two recent studies based on Norwegian register data (Rege *et al.* 2009; Bratsberg *et al.* 2013). Our identification strategy relies heavily on heterogeneity across firms in the degree to which their employees are exposed to the risk of layoff. While the role of firm heterogeneity has been emphasized in recent studies of wage inequality (e.g. Card *et al.* 2013) and immigrant–native wage differences (Aydemir and Skuterud 2008; Barth *et al.* 2012), there is little empirical evidence on the importance of immigrant job allocation across firms when it comes to employment shocks. Yet the very same mechanisms that lie behind the observed underrepresentation of immigrants in high-pay firms are likely to generate a similar immigrant overexposure to workplace downsizing and bankruptcy.

The empirical analyses build on administrative registers from Norway covering all private sector employees and firms from 1994 to 2010. By combining data from employer–employee registers with records from bankruptcy court proceedings, we identify all incidences of mass layoffs and firm closures in this period. We consider two types of firm events—bankruptcy and major downsizing—and use workers in stable (non-treated) firms to measure counterfactual outcomes. The motivation is twofold. First, distinguishing involuntary from voluntary separations is not possible from administrative register data alone. The reason for the observed separation is not filed, many workers leave because they receive a better offer elsewhere, and a large fraction of workers who actually lose their job will find a new job without ever registering as unemployed and thereby disclosing the involuntary nature of the separation. Second, by focusing on all workers in closing firms, or those exposed to mass layoffs, we avoid any selective processes that may take place within the firm. Although infrequent, bankruptcies have, from a research point of view, the great advantage of causing almost indisputably involuntary job loss. When we consider workers in downsizing firms, this approach has an intention-to-treat flavour, as only a fraction of the ‘treated’ workers are actually laid off. However, when we consider mass layoffs in bankruptcy firms, we know with certainty that all workers are displaced. Thus we can circumvent some otherwise tricky selection issues and obtain consistent estimates of the individual effects of job loss for immigrants and natives, respectively.

Our study consists of three parts. First, we examine the extent to which immigrants more than natives tend to work in firms exposed to major downsizing and closure events. Compared to natives, migrants from less developed countries (LDCs) are more likely to work in firms that are going to scale down or close down over the next few years. We find that their observed ‘overexposure’ to a full closure event is 55%. Second, we analyse the causal impacts of such events for immigrants and native workers, with a focus on subsequent employment and earnings outcomes. The adverse consequences of being exposed to such events—in the form of lower subsequent employment and earnings—are significantly larger for LDC migrants than for natives. In contrast, migrants from western European countries (the European economic area, EEA) exhibit very similar patterns to those of natives, in terms of both exposure and effects. Third, we extrapolate our findings from these rare events to explain why immigrants are more likely to leave employment due to job loss in general. The implied relationship between exogenous job loss and the probability of becoming unemployed provides information that we use to estimate the total exposure to job loss. Further, the causal effects identified by

bankruptcies are used to predict the overall impact on subsequent employment and earnings growth, for immigrants and natives, respectively. Over a two-year period, LDC immigrants face a 130% higher probability than natives of involuntary dismissal. Combining this finding with the estimated effects of job loss, we conclude that the combination of higher job loss rates and more severe effects of job loss accounts for 50–60% of the higher three-year transition rate out of employment and lower earnings growth of LDC immigrants.

I. EXPOSURE TO FIRM BANKRUPTCY AND DOWNSIZING

The employer–employee data that we use in our empirical analyses are collected from administrative registers and contain longitudinal information on individual employment spells with firm identifiers, earnings and unemployment insurance programme participation. Firm closures are identified from bankruptcy court proceedings and mass layoffs. We examine individual outcomes for workers aged 25 to 55 who in a ‘base year’ between 1994 and 2010 are full-time employed in a private sector firm. The data are organized on a person–year basis, such that each person contributes one observation each year for which the condition of full-time private sector employment is satisfied. To the observation, we next attach vectors of worker characteristics (such as age, gender, immigrant status and human capital measures), firm characteristics (such as industry and future downsizing/closure events) and outcomes (such as future employment, unemployment and earnings).

We divide the population of workers into three groups based on country of origin. The first group consists of immigrants from less developed countries (LDCs). The LDC immigrant category comprises labour and family migrants from Pakistan and Turkey, and refugees, asylum seekers and family migrants from Vietnam, Bosnia, Sri Lanka, Iraq, Iran, Chile, Kosovo and Somalia—the ten major low-income source countries in the immigrant labour force during our study period. The second group counts immigrants from countries in Western Europe (the EEA, i.e. pre-2004 European Union and European Free Trade Association member states).¹ Labour migrants from Sweden, Denmark, the UK and Germany dominate this group. The third group consists of natives, defined as persons born in Norway to two Norwegian-born parents. While we use complete population data for the two immigrant groups, we use a 10% random sample of natives (and reweight the data to account for this sampling).

Each year, approximately 10% of all private sector jobs in Norway disappear (Salvanes 1997; Bratsberg *et al.* 2013). This section provides an overview of the extent to which native and immigrant workers are exposed to major downsizing and closure events. More specifically, in Table 1 we look at the incidence of downsizings and closures over a three-year period. The reason why we use a three-year perspective here rather than focusing on, say, downsizings and closures occurring the next year only, is worker behaviour. We expect a considerable sorting process to take place over a period prior to downsizing and closure events, as workers start to leave the presumably ‘sinking ship’. Including events that occur further into the future reduces—though does not entirely solve—this potential selection problem. This improvement comes at a cost, however, as additional measurement error is introduced by the fact that some of the workers assumed to be exposed to the adverse employment shock in reality will have left the firm well before realization of the shock (for reasons unrelated to the forthcoming downsizing or closure).

TABLE 1
EXPOSURE TO BANKRUPTCY OR MAJOR DOWNSIZING; DESCRIPTIVE STATISTICS AND
REGRESSION ANALYSIS

	Firm bankrupt years 1–3 (%)	Firm downsizes years 1–3 (%)	Regression		
			(3)	(4)	(5)
LDC immigrant	2.8	11.0	0.040*** (0.001)	0.036*** (0.001)	0.018*** (0.002)
EEA immigrant	2.6	9.0	0.017*** (0.001)	0.017*** (0.001)	0.006*** (0.001)
Native/constant	1.8	8.1	0.098*** (0.000)		
Controls			None	Year and industry	Add age, gender, experience, tenure and schooling; county, firm size, proprietor, immigrant and foreign ownership

Notes

Standard errors, clustered within individuals, are shown in parentheses. The bankruptcy variable indicates that the firm closed down with a subsequent bankruptcy filing, and the downsizing variable indicates that the firm reduced its staff by at least 60% (but did not go bankrupt) in one of the next three years. The observation period is every third year between 1995 and 2010. The dependent variable of the regression analysis takes the value 1 if the firm goes bankrupt or downsizes. Samples consist of private-sector employees as of 31 December and aged 25–55 in the observation year (full population of the two immigrant groups and 10% extract of natives). Regressions have 4,806,159 observations (112,869 LDC immigrants, 133,530 EEA immigrants and 4,559,760 natives); regressions are weighted using frequency weights to account for the 10% random extract of native workers. Specification in column (4) controls for year (15 indicators) and two-digit industry (87 indicators); and that in column (5) adds gender, age and its square, actual experience and its square, tenure and its square, 7 indicators for educational attainment, 19 counties of residence, $\ln(\text{firm size})$, and indicators for sole proprietorship, immigrant owner and foreign ownership.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

As columns (1) and (2) of Table 1 reveal, native workers in private sector firms have a 8.1% chance of being exposed to a major downsizing event, and a 1.8% chance that the firm for which they work goes bankrupt over the next three years.² LDC immigrants are considerably more exposed to displacement, with an 11.0% probability of the firm downsizing and a 2.8% probability that the firm goes bankrupt. In other words, a bankruptcy is 55% more likely for an LDC immigrant worker than for a native, whereas experiencing a major downsizing is 35% more likely. EEA immigrants also appear to be overexposed to downsizing events, although to a lesser extent than LDC immigrants.

In columns (3)–(5) of Table 1, we investigate whether differential exposure to adverse employment shocks is explained by firm characteristics (industry affiliation, size, ownership) and/or by individual human capital characteristics. The estimates are from linear probability models where the outcome of interest is an indicator for whether or not the firm of employment will undergo a major downsizing or closure event during the upcoming three calendar years.

As shown in column (3) of Table 1, without any control variables, LDC immigrants have a 4.0 percentage points (or 41%) higher probability of exposure to a downsizing or

closure event than natives, whereas EEA immigrants have a 1.7 percentage points (19%) higher probability. Controlling for industry (with 87 dummy variables) and calendar year reduces the differential for LDC immigrants somewhat, suggesting that these immigrants indeed are overrepresented in risky industries; see column (4). Yet large and statistically significant differences remain. Accounting for differences in individual human capital variables (educational attainment, age, work experience in Norway, and tenure) as well as firm characteristics reduces the immigrant–native differentials further, indicating that LDC immigrants to a certain degree are sorted into precarious firms due to their lower levels of human capital and work experience (column (5)). Regardless of the underlying sorting mechanism, the fact that immigrants are considerably overrepresented in declining and dying firms is bound to have consequences for their relative labour market performance.

II. IDENTIFICATION OF JOB LOSS EFFECTS

As job loss is anything but randomly assigned across workers, it is not trivial to identify the causal impact. A common approach used in the literature compares displaced workers to colleagues who retained their job (Jacobson *et al.* 1993; Couch and Placzek 2010) or to similar workers in other firms (Huttunen *et al.* 2011). As layoffs can be selective with respect to worker characteristics, studies typically focus on major downsizings.³ To avoid any remaining bias from non-random layoffs, we follow Rege *et al.* (2009, 2011) and Bratsberg *et al.* (2013), and classify workers according to the downsizing and closure *status of their firm* rather than according to individual worker displacement, focusing on closures due to bankruptcy and events where firms drastically reduce their staff.

Despite our efforts to exploit layoff events that are exogenous in the sense that they have not been affected by the individual worker's own behaviour, we cannot rule out non-random sorting of workers into firms that vary according to their downsizing or closure probability. Hence we evaluate the effects of involuntary layoff within the framework of a regression analysis where we control for potential confounders, that is, variables that may be correlated with both the probability of being exposed to downsizing and closure events *and* future labour market performance. This includes all observed human capital characteristics as well as observed labour market performance indicators prior to the base year.

The regression equations will have the structure

$$(1) \quad y_{ijt} = \mathbf{X}'_{it}\boldsymbol{\beta} + \mathbf{D}'_{it}\boldsymbol{\gamma} + \mathbf{1}'\mathbf{M}_i(\mathbf{X}'_{it}\boldsymbol{\beta}_I + \mathbf{D}'_{it}\boldsymbol{\gamma}_I) + \mu_j + \tau_t + \varepsilon_{ijt},$$

where y_{ijt} is some (future) labour market outcome recorded for an individual i who in the base year t is employed in industry or firm j . The vector \mathbf{M} contains indicators for the two immigrant groups (LDC or EEA), and the vector \mathbf{D} contains indicators for downsizing and closure events occurring in a given time period after year t ; μ_j is alternatively an industry or firm fixed effect, τ_t is a year fixed effect, \mathbf{X} is a vector of individual covariates, and ε_{ijt} is a residual. The individual covariates include educational attainment (eight categories), age, work experience and tenure (the latter three in quadratic polynomials), as well as log earnings measured over a three-year period ending with the base year.

We include in \mathbf{D} two different downsizing and closure events on a firm–year basis: (i) a downsizing of the workforce by at least 60%, without filing for bankruptcy; and

(ii) a downsizing by 100% in combination with a subsequent bankruptcy filing.⁴ A potential problem for the non-bankruptcy downsizing event is that register-based records of downsizings invariably include a number of ‘spurious’ events, caused by restructuring (e.g. a merger or a demerger) that cannot be disentangled from genuine mass layoffs. Focusing on bankruptcies almost eliminates this problem. Although bankruptcies are rare, they provide the best case for displacements that are uncorrelated with worker characteristics. These events also constitute the cleanest case of mass layoffs that can be identified in our data, with a minimum of measurement error. Hence by tracking outcomes among employees during years following their employer’s bankruptcy, we get as close as possible to identify the causal effect of a job loss. In our setting, it is nevertheless of interest to study the impact of downsizings that do not involve the complete workforce, as they induce layoffs that are more selective and hence potentially have different bearings on immigrant–native differentials. Note, however, that we model the impacts of these events within an intention-to-treat framework; that is, we do not exploit information regarding individual layoffs.

Our main interest lies in the coefficient vectors (γ, γ_I) , that is, the ‘main’ (native) effects of the two displacement events and the additional effects for immigrants, captured by coefficients of interaction terms. As the empirical model includes the full set of interactions between immigrant background and individual characteristics, there is no single immigrant–native differential.

In this methodological setup, where future employment and earnings of workers who in the base year work in stable firms constitute the counterfactual for those affected by an adverse firm shock, there remain a number of challenges to identification. These challenges are related both to change in the composition of employees *within firms* in the period leading up to the layoff or closure event, and to systematic differences *between firms* (and their workers) exposed or not exposed to an adverse event in the near future. To ensure internal validity, we impose a number of sample restrictions aimed at minimizing the influence of sorting within and between firms.⁵

First, job separations caused by a downsizing or closure event may occur long before the mass layoff actually takes place; hence the stock of employees at the time of the event may already be selected. To circumvent this problem, we condition the causal analysis on firm stability over a period up to the major displacement event. More specifically, in the main empirical analysis the data are constructed as follows: For each base year $t_0 = 1994, \dots, 2010$, we identify private sector full-time workers aged 25–55 in firms that have *not* undergone any major downsizings in any of the years $t_0 - 2, t_0 - 1, t_0$ or $t_0 + 1$. Then in year $t_0 + 2$, some of the firms downsize or close down, and some do not. The purpose of our analysis is to study how these events affect employment and earnings outcomes from year $t_0 + 3$ onwards. This setup involves sources of attenuation bias, as firms *not* experiencing a displacement event in $t_0 + 2$ may do so in $t_0 + 3$ or later. In particular, a firm which in a given year is subject to a forthcoming closure, will for the baseline observation in the prior year be defined as stable, even though it will close down in three years. In a set of robustness analyses, we therefore redefine the group of untreated (stable) firms as firms without any major downsizing or closure event over the full outcome period.

Second, in very small firms we cannot rule out that the employees themselves influence the displacement event in question, in which case these events may not be orthogonal to the residual ε_{ijt} . To minimize this potential source of bias, we exclude firms with fewer than ten employees in the base year.

Third, a number of workers move in and out of firms at a relatively high frequency, often on temporary contracts (e.g. as substitute workers) and with intermittent spells of unemployment. As the prevalence of such contracts may vary systematically between firms according to their future downsizing or closure status, we seek to ensure a certain degree of worker homogeneity across firm types by conditioning on stable employment up to and including the base year. More specifically, we require that the worker was employed and did not receive welfare benefits (including unemployment insurance) in each of the past three calendar years, and has more than 365 days of tenure in the firm at the end of the base year. In separate robustness analyses, we relax these worker stability conditions.

Even though we include an extensive set of control variables in equation (1), it is probable that systematic unobserved differences remain between employees in firms on the verge of a major displacement process and those in stable firms. To address this concern, we conduct a set of robustness analyses where in equation (1) we substitute initial firm fixed effects for the industry effects. To further isolate co-workers, we restrict the fixed effects analyses to the last base year for which the firm is observed in the data. With only one year of observations per firm, a drawback is that the main effects of bankruptcy and downsizing are absorbed by the fixed effects. In these analyses, our focus is therefore on the differential impacts for immigrants and natives. The major advantage of the approach is that estimates reflect differences in post-displacement outcomes comparing immigrant and native co-workers from the same firm and exposed to the same downsizing or closure process.

Finally, particularly in the immigrant population, a probable response to job displacement is to leave the country (Bijwaard *et al.* 2014). In the main part of our analyses, we condition the samples on continued residency in Norway through the respective outcome periods. However, in additional robustness analyses presented in Section IV, we also include outmigrants in the analysis populations.

III. EFFECTS OF ADVERSE EMPLOYMENT SHOCKS

Descriptive patterns

Before turning to the regression analyses, we present some key descriptive patterns for the samples of workers used to identify and estimate the causal impacts of job displacement. As Table 2 shows, the additional sample restrictions result in far lower exposure rates to adverse firm shocks than in the overall workforce (refer back to Table 1). The differences between the three groups have also become smaller, particularly for downsizing events. Yet immigrant overexposure to bankruptcies remains large—with 53% higher exposure for LDC immigrants and 29% for EEA immigrants when compared to native workers.

The three demographic groups also differ in terms of observed characteristics. LDC immigrants are younger and have less work experience, shorter tenure and lower educational attainment than EEA immigrants.⁶ As we account for only post-migration labour market experience, both immigrant groups have fewer years of experience and have shorter tenure with the firm than natives of the same age.

The impacts of downsizing and bankruptcy events on future labour market outcomes are illustrated in Figures 1–3, separately for natives and the two immigrant groups. The figures illustrate how three key outcomes—unemployment, employment and annual earnings—evolve over a ten-year window from three years before to six years after the

TABLE 2
DESCRIPTIVE STATISTICS, RESTRICTED SAMPLES

	LDC immigrant (1)	EEA immigrant (2)	Native (3)
Firm bankrupt year $t_0 + 2$ (%)	0.53	0.45	0.34
Firm downsizes $t_0 + 2$ (%)	2.35	2.15	2.17
Age year t_0	38.7	41.5	40.7
Years of schooling	12.2	13.8	12.9
Actual experience year t_0	11.2	13.7	19.9
Tenure year t_0	5.5	6.1	7.6
Female (%)	24.1	33.1	31.9
Observations	108,581	133,847	6,394,840

Notes

Samples consist of private-sector workers aged 25–55 with more than one year of tenure in a firm with at least 10 employees on 31 December of the base year, which is two years prior to any bankruptcy/downsizing event. Samples are further restricted to those with employment each year and not receiving benefits nor working in a firm that downsized during the three-year period ending with the base year. Base years cover the period 1994 to 2010. The bankruptcy variable indicates that the firm filed for bankruptcy, and the downsizing variable indicates that the firm reduced its staff by at least 60% (but did not go bankrupt) during the calendar year two years after the base year.

base year for exposed and non-exposed workers. In the figures, year 0 denotes the base year (the last of three years with conditioned employment), whereas year 2 (marked with a vertical line) is the year of the potential downsizing or closure event.

First, Figure 1 shows the patterns of registered unemployment incidence over time and across groups. Unemployment is here defined as being registered as an unemployed job-seeker with the employment agency by the end of any month during the calendar year. Not surprisingly, workers exposed to a bankruptcy have much higher unemployment incidence than workers in stable firms, and this applies to immigrants as well as natives. Employees in firms with a major downsizing are also more prone to unemployment; whereas not all employees are laid off, the incidence is much lower than for bankruptcies. The higher observed incidence among LDC immigrants may in part reflect that they have less seniority in the firm, and therefore are more likely directly affected by the downsizing event.

An important reason for discussing Figure 1 is that it provides insight into the relationship between job loss and unemployment incidence. By definition, a closure due to bankruptcy is known to imply that *all* employees lose their job. The fact that ‘only’ 40% of native workers in this category register as unemployed during the year of firm closure, shows that a slight majority of displaced native workers either are able to find new employment in time to avoid an intermittent unemployment spell, or pull out of the labour market. For LDC immigrants, the fraction registering as unemployed is somewhat higher (about 50%), indicating that these workers to a lesser extent than natives find a new job in time to circumvent a spell of unemployment. Based on these numbers, it is possible to use the observed rates of registered unemployment for workers in non-closing firms to back out the approximate fraction of job loss in these firms—a point to which we return in Section VI.

Figures 2 and 3 display patterns of employment and labour earnings relative to the base year for workers in closing, downsizing and stable firms. It is evident that for workers employed in firms that go bankrupt, there are few signs of convergence even four

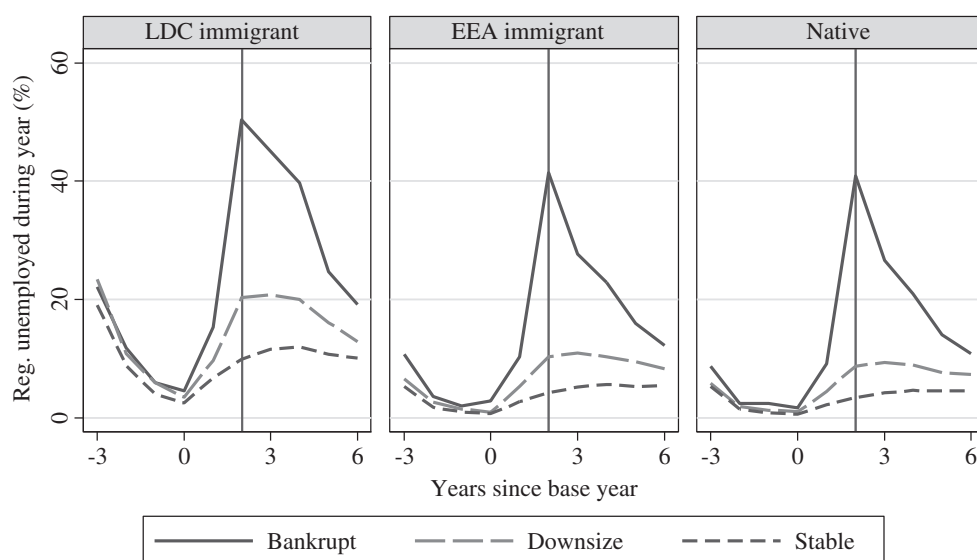


FIGURE 1. Yearly registered unemployment by immigrant background and firm shock
Notes: A vertical line marks the year of a major downsizing or bankruptcy event.

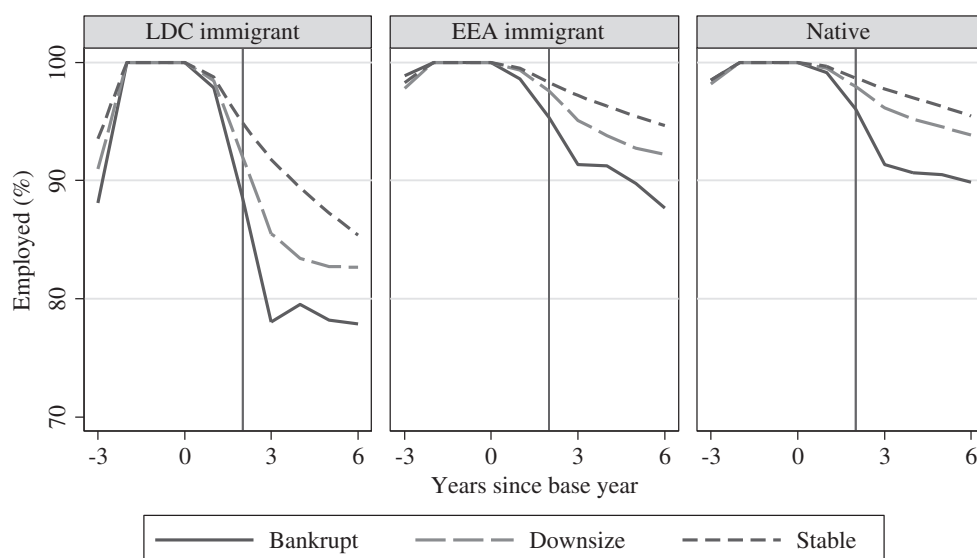


FIGURE 2. Employment by immigrant background and firm shock.

years after the closure event. A negative employment effect is indicated for all groups, regardless of immigrant background. However, the immediate negative employment shock appears to be much larger for LDC immigrants than for natives and EEA immigrants. The pattern is similar for the responses to major downsizings, although the magnitudes of the effects, as expected, are much smaller than for bankruptcies.

As was the case for employment, Figure 3 suggests considerable earnings losses in the years following adverse employment shocks. Since we have not conditioned on continued

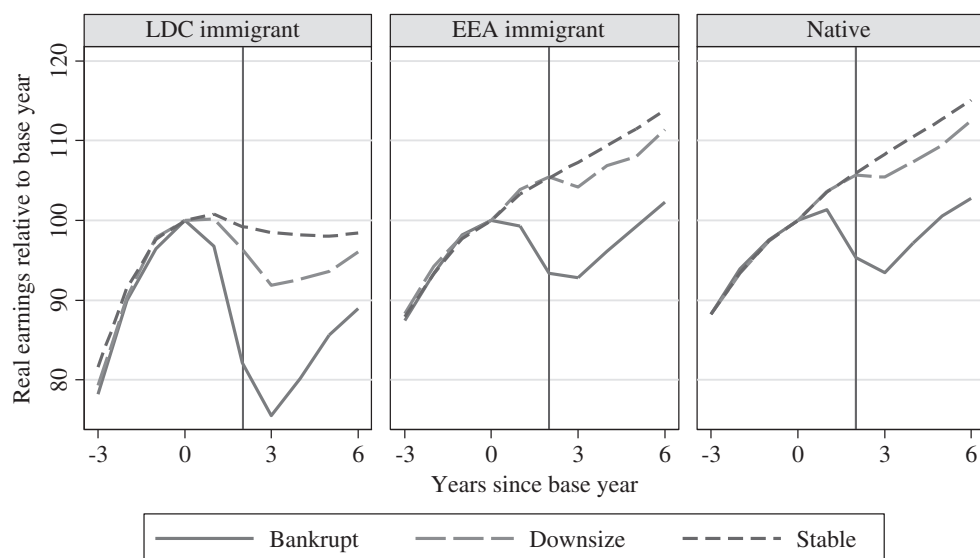


FIGURE 3. Earnings relative to base year by immigrant background and firm shock.

Notes: Earnings are normalized to 100 in the base year t_0 . In year $t_0 - 3$ and years $t_0 + 1$ to $t_0 + 6$, average earnings are not conditional on employment and will include some individuals with zero earnings.

employment in these graphs, most of the earnings losses mirror the employment decline described in Figure 2. For both EEA immigrants and natives, earnings losses seem moderate, but persistent. For workers in closing firms, there remains a non-trivial earnings loss four years after the bankruptcy. The immediate earnings drop associated with a bankruptcy appears much larger for LDC immigrants than for the other groups. Four years after the event, the earnings losses relative to employees in stable firms are similar across the three groups, however. This similarity reflects not that the earnings of LDC immigrants in bankruptcy firm are catching up, but rather that none of the LDC immigrant groups experiences any earnings growth over time, irrespective of initial employment in stable, downsizing or closing firms. Hence the convergence of earnings profiles by type of firm shock among LDC immigrants largely reflects the failure of workers in stable firms to improve their earnings over time. A point to note here is that although those in stable firms by construction did not experience any major employment shock in year 2, some will have experienced such shocks later. Based on the descriptive statistics in Table 2, we can assume that this happens more frequently for LDC immigrants. This, along with a greater exposure to moderate downsizings, may explain the steeper employment decline among LDC immigrants in ‘stable’ firms revealed by Figure 2.

Effects of displacement on employment and earnings

Table 3 presents our estimated effects of bankruptcy and downsizing on employment and earnings. The first outcome is a dichotomous variable indicating employment in the year after the potential closure/downsizing event (i.e. in year $t_0 + 3$). The second outcome measures the average employment rate over three years, extending the post-displacement period. Similarly, the log earnings outcome is annual for year $t_0 + 3$ and log average real annual earnings over the three-year period following any bankruptcy or downsizing event.

TABLE 3
EMPLOYMENT AND LOG EARNINGS FOLLOWING BANKRUPTCY OR DOWNSIZING

	Employment		Log earnings	
	Year 3 (1)	Years 3–5 (2)	Year 3 (3)	Years 3–5 (4)
<i>A. Sample means</i>				
LDC immigrants	0.916	0.895	12.678	12.638
EEA immigrants	0.971	0.963	13.048	13.053
Natives	0.977	0.970	12.986	12.995
<i>B. Regression</i>				
Bankrupt	−0.057*** (0.006)	−0.053*** (0.005)	−0.246*** (0.018)	−0.221*** (0.017)
LDC * Bankrupt	−0.067*** (0.018)	−0.045*** (0.015)	−0.184*** (0.054)	−0.107** (0.053)
EEA * Bankrupt	0.007 (0.013)	0.012 (0.011)	0.012 (0.042)	0.031 (0.038)
Downsize	−0.014*** (0.002)	−0.015*** (0.002)	−0.060*** (0.005)	−0.065*** (0.006)
LDC * Downsize	−0.041*** (0.007)	−0.035*** (0.006)	−0.098*** (0.022)	−0.082*** (0.024)
EEA * Downsize	−0.003 (0.004)	−0.005 (0.004)	0.012 (0.013)	0.009 (0.014)
Observations	6,589,558	6,563,959	6,535,663	6,533,031

Notes

Standard errors, clustered within individuals, are shown in parentheses. The dependent variable is an indicator for employment (column (1)) or log earnings from work (column (3)), or the average over the three-year period (columns (2) and (4)). Samples in columns (1) and (3) consist of those in the country on 31 December three years after the base year, and those in columns (2) and (4) five years after the base year, in addition to the sample restrictions detailed in the Notes to Table 3. Native samples are 10% random population extracts; regressions are weighted with frequency weights. All specifications control for year and two-digit industry, as well as age and its square, education and its square, actual experience and its square, tenure and its square, log base-period earnings and gender, with all individual characteristics interacted with indicators for each of the two immigrant categories.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

To facilitate interpretation of the estimated effects, panel A of Table 3 reports average outcomes for each of the three demographic groups. As the panel shows, employment rates are considerably lower for LDC immigrants than for natives and EEA immigrants. For example, three years after the base year, the LDC immigrant employment gap relative to natives is 6.1 percentage points (0.977 - 0.916, column (1)). For real earnings, the gap between natives and LDC immigrants is about 0.3 log points.⁷ Labour market outcomes of EEA immigrants are very similar to those of natives. If anything, earnings are slightly higher among EEA immigrants.

The estimated effects of a bankruptcy, or a major downsizing, are given by the regression coefficients in panel B of Table 3. We focus primarily on the impact of a bankruptcy, as this is the cleanest case of an exogenous job loss. First, bankruptcy has a significant, negative, short-run effect on labour market outcomes. For native workers, employment drops by 5.7 percentage points the year following firm closure, while annual earnings are 0.246 log points lower for workers from firms that go bankrupt. The estimated employment effect is comparable to the immediate effect of displacement on

labour force participation (a reduction of 7.3 percentage points) uncovered by Huttunen *et al.* (2011), who use a different methodology and study displaced males in Norwegian manufacturing industries.

The consequences of job loss are more severe for LDC immigrants, for whom a bankruptcy reduces employment by 12.4 percentage points ($-0.057 - 0.067 = -0.124$). The earnings loss is also more severe for LDC immigrants, and the additional effect of -0.185 log points is highly significant. For EEA immigrants, there is no indication that employment or earnings effects are different from those of natives.

Downsizing also affects short-run employment. The effects are less severe than those of bankruptcy, as expected. The impacts of downsizing events can be interpreted as ‘intention to treat’ effects as a fraction of the workforce remains with the firm. The employment effect for natives is a negative 1.4 percentage points. Again, the adverse employment effects are considerably larger for LDC immigrants. Actually, the short-term employment loss for LDCs is four times that for natives, suggesting that LDC immigrants are strongly overrepresented among workers laid off in the downsizing process. Earnings losses from working in a downsizing firm are also significant and estimated to 0.060 log points for natives. Again, earnings of LDC immigrants are more adversely affected, with an effect estimate of -0.158 log points.

Moving on to the three-year averages, we first note that the sample means reveal that the average employment differential for LDC immigrants, relative to natives, is -0.075 over the three-year period after a possible closure. The earnings gap is 0.357 log points; see column (4) of Table 3. Turning to the regression results in panel B, with all available control variables included in the specification, we find that a bankruptcy reduces the subsequent average three-year employment rate of natives by 5.3 percentage points (column (2)), indicating a persistent job loss effect that is slightly reduced over time. The more severe employment loss for LDC immigrants also persists as those exposed to bankruptcy have an average employment drop of 9.8 percentage points over the three-year period following firm closure. Earnings also fall when we extend the post-displacement period. Natives exposed to a bankruptcy experience a drop in earnings of 0.221 log points over the three-year period. Again, LDC immigrants take an extra hit (of 0.109 log points), implying an earnings drop of 0.330 log points due to firm bankruptcy. Even the impacts of a downsizing are long-lasting. For both natives and immigrants, the downsizing effects on employment and earnings in the extended outcome period are very similar to the short-run effects.

Effects of displacement on labour force exit and unemployment insurance uptake

Prior studies of labour market performance of LDC immigrants in Norway document rising rates of labour market withdrawal with years since immigration, often through enrolment in the disability insurance programme (Bratsberg *et al.* 2010, 2014). In light of the large employment effects uncovered in Table 3, one might expect that exposure to bankruptcy is part of the explanation for the increasing non-participation rates. In Table 4, we therefore address the impacts of bankruptcy and downsizing on subsequent labour force withdrawal and receipt of unemployment insurance benefits. According to panel A, LDC immigrants are three times more likely to exit the labour force, here defined as being not employed or not registered as unemployed during the calendar year. Even for unemployment insurance, the share of LDC immigrants with benefits is three times that of natives. Note that employment in Table 3 and unemployment insurance in

TABLE 4
LABOUR FORCE EXIT AND UNEMPLOYMENT INSURANCE RECEIPT FOLLOWING
BANKRUPTCY OR DOWNSIZING

	Out of labour force		Unemployment insurance	
	Year 3 (1)	Years 3–5 (2)	Year 3 (3)	Years 3–5 (4)
<i>A. Sample means</i>				
LDC immigrants	0.054	0.070	0.090	0.088
EEA immigrants	0.021	0.028	0.040	0.042
Natives	0.018	0.024	0.031	0.033
<i>B. Regression</i>				
Bankrupt	0.031*** (0.005)	0.033*** (0.004)	0.170*** (0.009)	0.125*** (0.007)
LDC * Bankrupt	−0.018 (0.012)	−0.014 (0.011)	0.110*** (0.022)	0.094*** (0.018)
EEA * Bankrupt	−0.007 (0.011)	−0.012 (0.009)	0.008 (0.020)	0.008 (0.015)
Downsize	0.007*** (0.001)	0.008*** (0.001)	0.039*** (0.002)	0.030*** (0.002)
LDC * Downsize	0.017*** (0.006)	0.016*** (0.005)	0.037*** (0.008)	0.025*** (0.006)
EEA * Downsize	0.002 (0.004)	0.004 (0.004)	0.006 (0.006)	0.007 (0.005)
Observations	6,589,558	6,563,959	6,589,558	6,563,959

Notes

Standard errors, clustered within individuals, are shown in parentheses. The dependent variable is an indicator for not employed or not registered unemployed (column (1)) or receipt of disability insurance during the calendar year (column (3)), or the average over the three-year period (columns (2) and (4)). See also the Notes to Table 3.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

Table 4 are not mutually exclusive as some employed individuals may receive unemployment insurance benefits for part of the year.

According to the estimates in Table 4, exposure to a bankruptcy significantly affects both labour force exit and unemployment insurance benefits receipt. Compared to the short-run employment effect of −0.057 (see Table 3), the labour force exit effect of 0.031 suggests that about one-half of the drop in native employment is due to withdrawal from the labour force. Unlike employment, there is no significant additional effect for LDC immigrants. When we consider unemployment benefit receipt, however, the bankruptcy effect is significantly larger for LDC immigrants. In magnitude, the unemployment insurance effect of bankruptcy is much larger, in absolute terms, than the employment effects, reflecting that workers exposed to firm closure are much more likely to experience short unemployment spells during a year (of employment). The bankruptcy estimates in Tables 3 and 4 show that effects on employment and unemployment benefit receipt are more severe for LDC immigrants, but that the impact on labour force participation is the same as for natives.

Turning to the impacts of a major downsizing, we find positive effects on labour force withdrawal as well as unemployment benefit receipt. Unlike for bankruptcies, the effect on labour force participation is stronger for LDC immigrants than for natives.

Compared to the employment effects of Table 3, however, the magnitude of the differential effect on labour force participation is limited. Overall, adverse firm shocks have modest effects on labour force exit. The vast majority of LDC immigrants directly affected by such shocks remain in the labour force, but are much more likely to claim unemployment benefits as compared to native workers.

IV. ROBUSTNESS CHECKS

Even conditional on the extensive set of control variables, there is some risk that downsizing and closure events might be correlated with unobserved worker characteristics that also affect post-displacement outcomes. In fact, there is some indication that workers exposed to bankruptcy, in particular, differ from other workers in their industry. When we re-estimate the models underlying Tables 3 and 4 after removing all individual worker characteristics from the list of control variables, estimated effects of bankruptcy become slightly larger for both natives and LDC immigrants (downsizing effect estimates remain largely unaffected); see Appendix Tables A1 and A2.

In terms of unobserved characteristics, concerns about identification relate to the composition of employees both *within firms* as well as *between firms* depending on their downsizing or closure status. In Table 5, we examine the sensitivity of results to relaxing various restrictions on what workers we include in the sample, focusing the analysis on the intermediate-run outcomes measured over the three-year period following any job displacement. The first concern is selective outmigration. Job loss arising from bankruptcy and downsizing is likely to trigger mobility, and for migrants who become unemployed, returning to their country of origin represents a relevant alternative. In our baseline analyses, we conditioned results on the populations that remain in Norway throughout the outcome period. If, for example, employer bankruptcy triggers outmigration of those with the highest earnings potential, then the remaining group will be negatively selected, and the observed effect of bankruptcy on future labour market outcomes would appear more severe than the true effect.

In the first two columns of Table 5, we include in the sample workers who left Norway (in any year after t_0), with outcomes still capturing employment and earnings in Norway only.⁸ As can be seen from the sample sizes, very few outmigrated in total, and the scope for bias from selective outmigration is therefore limited. In fact, when we compare estimates in Table 5 columns (1) and (2) to the baseline estimates in Table 3 columns (2) and (4), the results are strikingly similar. The one exception is the estimated effect of downsizing on employment of EEA immigrants, which now becomes larger and statistically significant, reflecting their much higher outmigration propensity (Bratsberg *et al.* 2007). For coefficients of the bankruptcy terms, there is no sign that failure to account for outmigration renders biased estimates.

To ensure a certain degree of worker homogeneity, our baseline sample restrictions excluded workers with short tenure and non-employment or social insurance claims during the three-year base period. A legitimate concern is that such sample selection criteria may have different implications for LDC immigrants than for other workers, as stable employment in a stable firm may itself be an endogenous outcome of their labour market integration process. In Table 5 columns (3)–(6), we relax these restrictions. Again, the exercise has little impact on key coefficient estimates; our baseline results appear robust to such sample concerns, as point estimates are similar to those in Table 3, in terms of both magnitude and precision.

TABLE 5
ROBUSTNESS ANALYSES, WORKER SAMPLE RESTRICTIONS

	Include outmigrants		Relax restrictions on employment prior to base year		Relax restrictions on employment and social insurance prior to base year	
	Employment (1)	Log earnings (2)	Employment (3)	Log earnings (4)	Employment (5)	Log earnings (6)
Bankrupt	-0.053*** (0.005)	-0.221*** (0.017)	-0.054*** (0.005)	-0.221*** (0.018)	-0.057*** (0.005)	-0.223*** (0.017)
LDC *	-0.043*** (0.015)	-0.093** (0.052)	-0.045*** (0.014)	-0.124*** (0.051)	-0.031*** (0.011)	-0.084** (0.041)
EEA *	-0.002 (0.013)	-0.009 (0.045)	0.014 (0.011)	0.022 (0.037)	0.014 (0.010)	0.002 (0.033)
Downsize	-0.015*** (0.002)	-0.065*** (0.006)	-0.015*** (0.002)	-0.063*** (0.006)	-0.019*** (0.002)	-0.066*** (0.006)
LDC *	-0.035*** (0.007)	-0.079*** (0.024)	-0.031*** (0.006)	-0.067*** (0.022)	-0.020*** (0.005)	-0.047** (0.019)
EEA *	-0.012** (0.005)	-0.006 (0.014)	-0.007* (0.004)	0.000 (0.013)	-0.007* (0.004)	-0.013 (0.013)
Observations	6,574,688	6,540,434	6,699,750	6,667,748	7,628,181	7,571,649

Notes

Standard errors, clustered within individuals, are shown in parentheses. Dependent variables are average employment or log average annual earnings over the period 3–5 years after the base year. Samples in columns (1) and (2) include all workers at the end of the base period (but subject to the restrictions described in the Notes to Table 3), regardless of residency status in the outcome year. Samples in columns (3) and (4) exclude outmigrants, but relax the sample restrictions of employment in years $t-1$ and $t-2$, as well as restrictions on immigration at least three years before the base year. Samples in columns (5) and (6) further relax the sample restrictions of no social insurance benefit receipt in years $t-1$ and $t-2$. See also the Notes to Table 3.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

Table 6 addresses robustness with respect to unobserved sorting of workers across firms. In the baseline approach, changes in employment and earnings of workers in stable firms represent the counterfactual outcomes for workers in firms that close down or downsize. One might worry that these counterfactuals are affected by sorting, whereby ‘good’ firms attract workers with above-average employment and earnings trajectories. To account for such heterogeneity across firms, in columns (1) and (3) we therefore include firm fixed effects in the specification. In this robustness check, we limit the sample to one observation year per firm, because repeated observation years would be subject to similar concerns about changes in the composition of the workforce in years before and after downsizing events. With one observation year per firm, the effect of an event common to all workers in the firm is not identified in the fixed effects regression, explaining the absence of main effect estimates in columns (1) and (3). Differential effects across groups are, however, identified intuitively by the difference in post-displacement outcomes between immigrant and native co-workers within the same firm. To further ensure that results are not driven by the sampling procedure, in columns (2) and (4) we also report estimates from the reduced sample but without initial firm fixed effects; these columns confirm that the additional effect estimates for LDC migrants are the same as in

TABLE 6
ROBUSTNESS ANALYSES, FIRM FIXED EFFECTS AND SAMPLE RESTRICTIONS

	Firm fixed effects				Drop bankrupt or downsize years $t + 3$ to $t + 5$	
	Employment		Log earnings		Employment (5)	Log earnings (6)
	With firm fixed effects (1)	As Table 3, firm fixed effects sample (2)	With firm fixed effects (3)	As Table 3, firm fixed effects sample (4)		
Bankrupt		-0.042*** (0.005)		-0.185*** (0.018)	-0.058*** (0.006)	-0.225*** (0.019)
LDC * Bankrupt	-0.046** (0.023)	-0.048*** (0.015)	-0.135* (0.073)	-0.114** (0.053)	-0.044*** (0.017)	-0.105* (0.062)
EEA * Bankrupt	0.024 (0.020)	0.010 (0.011)	0.001 (0.057)	0.037 (0.038)	0.005 (0.013)	0.001 (0.045)
Downsize		-0.007*** (0.002)		-0.038*** (0.007)	-0.015*** (0.002)	-0.065*** (0.006)
LDC * Downsize	-0.029*** (0.010)	-0.038*** (0.007)	-0.071** (0.033)	-0.087*** (0.026)	-0.029*** (0.007)	-0.062*** (0.025)
EEA * Downsize	-0.009 (0.006)	-0.008* (0.005)	-0.011 (0.020)	0.009 (0.016)	-0.007 (0.005)	0.007 (0.015)
Observations	850,151	850,151	844,859	844,859	5,306,559	5,282,639
Fixed effects	36,403		36,165			

Notes

Standard errors, clustered within individuals, are shown in parentheses. Dependent variables are average employment or log average annual earnings over the period 3–5 years after the base year. Samples in columns (1) and (4) are restricted to workers for the last year the firm is observed in the data. Samples in columns (5) and (6) exclude from the baseline sample workers in firm that do not downsize in year $t + 2$ but go bankrupt or downsize in year $t + 3$, $t + 4$ or $t + 5$; sample period is adjusted to 1994–2008. See also the Notes to Table 3.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

the full sample. Even more important, however, the initial firm fixed effects estimates confirm that the displacement effects on employment and earnings of LDC immigrants are more severe than those of native workers. Indeed, coefficient estimates of the interaction term between bankruptcy and LDC immigrant are very similar across specifications with and without firm fixed effects, and there is no indication that our baseline estimates are impaired by bias caused by differential sorting of immigrant and native workers across firms.

Some of the firms that we classify as stable according to their status in year t_0 do actually downsize or close down later during the outcome window. That is, in our baseline setup there are non-treated employees who become treated at some point during the period. In columns (5) and (6) of Table 6 we investigate how exclusion of outcome observations of workers in stable firms that close down or undergo a major downsizing during the outcome window affects our estimates. Not surprisingly, reducing this source of contamination yields estimates of main effects that are slightly more negative than those reported in Table 3. Once again, coefficient estimates of interaction terms for LDC migrants are very similar to those of the baseline sample.

The strategy of identifying job loss effects by means of firm events hinges on an (implicit) common trends assumption as workers in stable firms are used to form counterfactual estimates for workers in firms with adverse employment shocks. A standard procedure to back up the plausibility of this assumption is the pre-treatment test. In our case, this is a falsification test where we estimate the ‘effect’ of future firm bankruptcy and downsizing events on (pre-period) employment and earnings of workers. In Table 7, we follow this line of reasoning and check whether pre-displacement

TABLE 7
FALSIFICATION TESTS

	Pre-period employment		Pre-period log earnings	
	Without firm fixed effects (1)	With firm fixed effects (2)	Without firm fixed effects (3)	With firm fixed effects (4)
Bankrupt	0.002 (0.003)		0.006 (0.007)	
LDC * Bankrupt	-0.011 (0.010)	0.014 (0.013)	-0.025 (0.028)	-0.027 (0.033)
EEA * Bankrupt	0.003 (0.008)	0.004 (0.011)	0.008 (0.023)	0.047* (0.029)
Downsize	0.000 (0.001)		0.000 (0.003)	
LDC * Downsize	-0.008* (0.005)	0.008 (0.007)	-0.021 (0.014)	-0.021 (0.020)
EEA * Downsize	-0.005 (0.004)	-0.006 (0.005)	0.006 (0.010)	0.012 (0.015)
Observations	6,637,268	859,448	6,627,408	858,225
Fixed effects		36,888		36,770

Notes

Standard errors, clustered within individuals, are shown in parentheses. The dependent variable is the average employment rate (columns (1) and (2)) or log average earnings (columns (3) and (4)) over the three-year period preceding the base period (i.e. years $t_0 - 5$ to $t_0 - 3$). See also the Notes to Tables 3 and 6.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

outcomes, measured by the average employment rate (columns (1) and (2)) and log average earnings (columns (3) and (4)) over the three-year period preceding the base period (i.e. years $t_0 - 5$ to $t_0 - 3$), correlate with bankruptcy or downsizing exposure in year $t_0 + 2$. As the table shows, there is no indication that, conditional on the sample restrictions and the explanatory variables included in the regression, workers in firms that go bankrupt had lower employment and earnings than other workers seven to five years before the bankruptcy. Equally important, the table shows that in the pre-treatment period, differential outcomes between LDC immigrants and natives were similar for workers in stable and bankrupt firms. In other words, there is no indication that our key results reflect differential pre-treatment trends in outcomes of LDC immigrants and natives who worked in firms that remained stable or went bankrupt two years after the base period.

Overall, the checks in this section show that a number of potential concerns about our identification strategy turn out to be unfounded. Our estimated effects of displacement on employment and earnings of native and immigrant workers appear robust to a large set of sample and specification checks, in terms of both structure and magnitude.

V. IMMIGRANTS OR CHARACTERISTICS OF IMMIGRANTS?

If effects of job loss are heterogeneous across skills, then the fact that LDC migrants possess different skills could potentially explain why they, on average, suffer more from negative shocks to their workplace. Studies of unemployment incidence and duration show that highly educated individuals are less prone to experience unemployment, and if they do, then they have shorter-spell durations than the less educated (see, for example, Nickell 1979; Røed and Zhang 2005). Indeed, Hoynes *et al.* (2012) show that low-education workers were more severely affected by the Great Recession than workers with high educational attainment. This raises the question of whether the more adverse effects of job displacement uncovered for LDC immigrants simply reflect lower average educational attainment when compared to EEA immigrants and natives, and were we to compare workers with similar qualifications, would there be no differential effects of displacement across groups?⁹ If so, the additional impacts observed for immigrants are due to their characteristics rather than their immigrant status.

We investigate this interpretation by augmenting the empirical models so that they allow for effects of bankruptcy and major downsizing events to differ by educational attainment, actual work experience, and tenure. The extended model specification even includes three-way interactions, permitting any effect heterogeneity to vary across immigrant and native workers. As it turns out, only the interactions between the displacement events and education were significantly different for LDC immigrants and natives. Hence in Table 8, for the purpose of brevity, we focus on the education interactions, leaving out the three-way interactions between displacement and tenure and experience and the two immigrant groups. Similarly, for reasons of parsimony, we report only the main, linear effects of education, experience, and tenure for workers in stable firms.

In Table 8, the outcome is the average employment rate (columns (1) and (2)) and log average earnings (columns (3) and (4)) over the medium-term three-year period. When treatment effects are heterogeneous, the size of the immigrant–native effect differential depends on skills as well. In the table, we report effect estimates of

TABLE 8
EXTENDED REGRESSION MODEL WITH EDUCATION, EXPERIENCE AND TENURE
INTERACTIONS

	Employment		Log earnings	
	Without firm fixed effects (1)	With firm fixed effects (2)	Without firm fixed effects (3)	With firm fixed effects (4)
Bankrupt	-0.043*** (0.007)		-0.162*** (0.023)	
LDC * Bankrupt	-0.063*** (0.016)	-0.054** (0.024)	-0.193*** (0.056)	-0.177** (0.081)
EEA * Bankrupt	-0.006 (0.014)	0.019 (0.024)	-0.058 (0.047)	-0.056 (0.074)
Experience/10 * Bankrupt	-0.016** (0.007)	-0.008 (0.010)	-0.088*** (0.024)	-0.054 (0.034)
Tenure/10 * Bankrupt	0.006 (0.011)	0.005 (0.014)	0.009 (0.038)	0.003 (0.053)
Education * Bankrupt	0.011*** (0.003)	0.014*** (0.004)	0.028*** (0.009)	0.040*** (0.014)
Education * Bankrupt * LDC	-0.008* (0.005)	-0.018** (0.006)	-0.018 (0.017)	-0.044** (0.022)
Downsize	-0.014*** (0.003)		-0.044*** (0.009)	
LDC * Downsize	-0.038*** (0.007)	-0.029*** (0.010)	-0.109*** (0.025)	-0.102*** (0.034)
EEA * Downsize	-0.006 (0.005)	-0.008 (0.008)	-0.011 (0.018)	-0.034 (0.025)
Experience/10 * Downsize	-0.004 (0.002)	-0.000 (0.003)	-0.030*** (0.008)	-0.034*** (0.011)
Tenure/10 * Downsize	0.002 (0.003)	-0.004 (0.004)	0.011 (0.010)	-0.007 (0.013)
Education * Downsize	0.002*** (0.001)	0.004*** (0.001)	0.003 (0.002)	0.007** (0.003)
Education * Downsize * LDC	0.004** (0.002)	0.003 (0.003)	0.017** (0.008)	0.005 (0.010)
Education	0.006*** (0.000)	0.004*** (0.001)	0.032*** (0.001)	0.025*** (0.002)
Experience/10	0.041*** (0.002)	0.037*** (0.004)	0.056*** (0.007)	0.052*** (0.016)
Tenure/10	0.006*** (0.001)	0.005** (0.002)	-0.012*** (0.003)	-0.014** (0.007)
Observations	6,562,959	850,151	6,533,031	844,859
Fixed effects		36,403		36,165

Notes

Standard errors, clustered within individuals, are shown in parentheses. The dependent variable is the average employment rate (columns (1) and (2)) or log average earnings (columns (3) and (4)) over the three-year period following any bankruptcy or downsizing (i.e. $t_0 + 3$ to $t_0 + 5$). The coefficients of bankrupt and downsize are evaluated at means of education, experience and tenure in the LDC immigrant sample. See also the Notes to Tables 3 and 5.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

bankruptcy and downsizing evaluated at the mean values of educational attainment (12.2 years), experience (11.2) and tenure (5.5) in the LDC immigrant sample. Focusing on the specifications without initial firm fixed effects, we find that bankruptcy effects are more severe for workers with low education and long work experience. Low-skilled and older workers are less likely to find a new job, and they suffer larger earnings losses following displacement. More importantly, the additional effects of bankruptcy and downsizing for LDC immigrants prevail. Evaluated at LDC immigrant averages of the human capital measures, the estimated LDC immigrant employment effect of a bankruptcy is -0.106 compared to the baseline estimate of -0.098 reported in Table 3. For earnings, the LDC immigrant loss from a bankruptcy is estimated to be 0.355 log points compared to 0.328 in Table 3.

The three-way interaction terms show that for LDC immigrants, schooling protects less against losses from bankruptcy than is the case for native workers. Conversely, the disadvantage of low education is greater for natives than for LDC immigrants. Even so, evaluated at compulsory schooling, the additional LDC immigrant effects of bankruptcy remain negative, although statistically significant only in the specifications without initial firm fixed effects. According to the coefficient estimates in columns (1) and (3), evaluated at compulsory schooling the LDC immigrant–bankruptcy interaction effect is -0.045 ($p = 0.022$, not shown in the table) on employment and -0.153 ($p = 0.029$) on log earnings. Similarly, the adverse effects of downsizing events are less severe for highly educated workers. Here, we find that the negative interaction effect for LDC immigrants is somewhat moderated by years of schooling. However, if we compare those with a bachelor's degree, LDC immigrants still suffer more severe labour market losses from downsizing than natives.

Overall, when we allow for differential effects of displacement across the skill distribution, also permitting skill interactions to differ by immigrant background, the additional effects of bankruptcy and downsizing for LDC immigrants prevail within sample ranges of skills, with, regardless of specification, statistically significant interactions when evaluated at mean skills in the immigrant sample. The fact that LDC migrants hold different (observed) skills like education, work experience and tenure from native workers cannot explain why they suffer more from negative shocks to their employer.

VI. JOB LOSS AND IMMIGRANT–NATIVE EMPLOYMENT AND EARNINGS GAPS

In this section, we discuss how the insights from our study of bankruptcy can inform on the role of involuntary job loss in explaining the lower employment and earnings of LDC immigrants compared to natives, focusing on transitions out of employment. Figure 4 illustrates the key phenomena that we address. Starting from the base year of conditioned employment, the figure shows, separately for the three demographic groups considered, the fraction that remains employed (in any job) in subsequent years as well as earnings relative to those in the base year. The differences that materialize between the groups are striking. While natives and EEA immigrants have very similar employment profiles, with employment rates well above 90% throughout the ten-year period, the employment rates of LDC immigrants drop sharply and end up at 80%. Similarly, while real earnings of natives and EEA immigrants over the interval grow by more than 20%, LDC immigrants experience little change in real earnings. The question that we ask is to what extent job loss is a part of this process, explaining the differential developments in

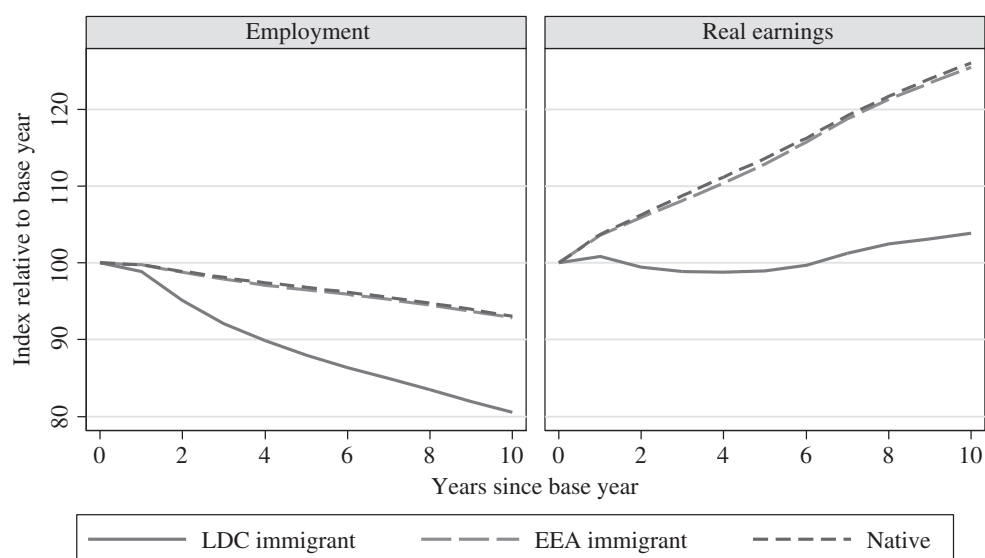


FIGURE 4. Employment continuation and earnings growth of immigrants and natives.

labour market outcomes. Further, does the major contribution come from differences in exposure to, or from differences in effects of, job loss?

To assess the role of job loss, we need to make two sets of adjustments to observed displacement rates and effect estimates. First, because we cannot distinguish between voluntary and involuntary job separations, we impute overall job loss rates from observed inflows to registered unemployment. Table 9 reports observed rates of registered unemployment and adjusted rates of job loss over the 24-month period between the base year and the event year in our samples of LDC immigrants and natives. The row labelled A first shows that among LDC immigrants in firms that go bankrupt, 52.1% registered with the employment agency as unemployed between the end of the base year and the bankruptcy. We know, however, that all these workers lost their jobs, so the adjusted job loss rate for this group is 100% (see row B). In other words, a certain fraction of LDC immigrants who lose their jobs either find a new job without registering as unemployed in the interim or withdraw from the labour market. According to column (1), this fraction is 47.9% ($100 - 52.1$). For native workers, the fraction is 56.5% (see column (1), rows C and D). The key idea behind our adjustments is that these fractions also apply to other workers who lose their jobs, regardless of whether their workplace was subject to bankruptcy or not. While we observe that 12.6 of all LDC immigrants and 4.6% of natives in our samples register as unemployed between the base year and the event year, the adjusted job loss rate is 24.1% for LDC immigrants and 10.5% for native workers (see column (3)). In other words, when we also account for job separations from stable firms, LDC immigrants face a risk of job loss that is 2.3 times as large as that of natives.

Second, we adjust the estimated effects of bankruptcy so that they reflect job loss. In order to correct estimates for ‘contamination bias’ stemming from the fact that the control group (i.e. workers in stable firms) includes some who also lose their jobs, we follow Bratsberg *et al.* (2013) and divide the bankruptcy effect estimates from Table 3 by one minus the job loss rate among workers in stable firms, as computed in Table 9,

TABLE 9
JOB LOSS RATES, LDC IMMIGRANTS AND NATIVES

	From bankruptcy (1)	From stable firms (2)	All (3)
A. LDC immigrants, registered	0.521	0.121	0.126
B. LDC immigrants, adjusted	1	0.232	0.241
C. Natives, registered	0.435	0.043	0.046
D. Natives, adjusted	1	0.099	0.105

Notes

Registered job loss rates describe the fraction of the workforce at the end the base year who registered as unemployed with the employment agency over the next 24 months.

column (2). For example, with a job loss rate of 10% from stable firms, we adjust for contamination bias by dividing the bankruptcy effect estimate by 0.9. The resulting job loss effects reported in Table 10 are slightly larger than the estimated effects of bankruptcy, for both LDC immigrants (row B) and natives (row D).

Finally, with adjusted job loss rates and effect estimates from Tables 9 and 10 in hand, we calculate the contribution of job loss to the observed differences in employment change and earnings growth between immigrants and natives, both in the short run (i.e. between the base year and year 3) and the intermediate run (between the three-year base period and the post period, years 3–5). Row C of Table 11 reveals that, depending on the time interval considered, the employment rate of LDC immigrants declines by 6.2 and 7.5 percentage points more than that of natives, and the earnings growth of the immigrant group is 0.171 and 0.212 log points below that of natives. As in any decomposition exercise, the contributions of effect versus exposure will depend on the choice of two alternative weights. In our application, do we evaluate the contribution of differences in exposure using native or immigrant effects of job loss? To circumvent this issue, we instead use the average of the two weights in evaluating the size of the two components. The resultant components are reported in rows D (difference in effects) and E (difference in exposure). In size, the two components are similar across columns, indicating that differential effects and differences in exposure contribute about the same to the overall effect of job loss on immigrant–native gaps in labour market outcomes. Row F sums the two components, and row G reports the ratio of the total contribution to the observed immigrant–native differential. As the final row shows, differential job loss explains 53% of the short-run gap in employment, and 62% of the short-run gap in earnings growth. In

TABLE 10
EFFECTS OF JOB LOSS ON EMPLOYMENT AND LOG EARNINGS, LDC IMMIGRANTS VS.
NATIVES

	Employment		Log earnings	
	Year 3 (1)	Years 3–5 (2)	Year 3 (3)	Years 3–5 (4)
A. Immigrant bankruptcy effect from Table 3	−0.124	−0.098	−0.430	−0.328
B. Adjusted for contamination bias	−0.162	−0.128	−0.559	−0.427
C. Native bankruptcy effect from Table 3	−0.057	−0.053	−0.246	−0.221
D. Adjusted for contamination bias	−0.064	−0.059	−0.273	−0.245

TABLE 11
ACCOUNTING FOR JOB LOSS IN CHANGE IN EMPLOYMENT AND LOG EARNINGS, LDC
IMMIGRANTS VS. NATIVES

	Employment		Log earnings	
	Year 0 to year 3 (1)	Base to post period (2)	Year 0 to year 3 (3)	Base to post period (4)
A. Observed change, LDC immigrants	−0.084	−0.105	−0.150	−0.145
B. Observed change, natives	−0.023	−0.030	0.022	0.068
C. Observed immigrant–native difference, A − B	−0.062	−0.075	−0.171	−0.212
D. Immigrant–native effect difference * Average job loss rate	−0.017	−0.012	−0.050	−0.032
E. Job loss difference * Average effect	−0.015	−0.013	−0.057	−0.046
F. Explained immigrant–native difference, D + E	−0.032	−0.025	−0.106	−0.077
G. Percent explained, 100 * F/C	52.7	32.8	61.9	36.4

the intermediate run, the contribution of job loss is somewhat attenuated, explaining about one-third of the differential employment and earnings change of LDC immigrants and natives.

In sum, over time immigrant workers from low-income countries experience inferior developments of employment and earnings compared to native workers. What the decomposition exercise shows, is that job loss accounts for a substantial portion of the evolution of immigrant–native gaps in labour market outcomes, with equal contributions from differences in exposure to and effects of job loss.

VII. CONCLUSION

In order to ensure lasting labour market integration of immigrants from developing countries, it may not be sufficient to facilitate successful labour market entry. The starting point of this paper was that the labour market performance of immigrants from low-income source countries falls below that of natives across many European host countries. Although there is a strong process of labour market assimilation for most immigrant groups during the first years after arrival, we have seen a disturbing tendency for this process to lose steam, and even go into reverse well before reaching employment parity with natives.

Based on administrative longitudinal data from Norway, we have shown in this paper that involuntary transitions out of employment are more frequent among immigrants from developing countries. In particular, LDC immigrants are much more exposed to adverse employment shocks hitting their employer than (similar) natives. From examinations of future outcomes among workers in firms that closed down due to bankruptcy, we also show that the consequences of such shocks—in terms of subsequent employment and earnings paths—are more severe for LDC immigrants than for native-born workers.

An important reason why immigrants from developing countries are more likely than other workers to leave employment is that they tend to work in precarious firms, with high risks of experiencing a downsizing or closure event in the near future. The overexposure of LDC immigrants to such events is 41%. Moreover, LDC immigrants

typically have short job tenure and relatively low (and inflexible) qualifications, making them more likely to be selected for layoff during downsizing and reorganization processes. Overall, including displacements from stable firms, we estimate that the job loss rate of LDC immigrants is 2.3 times that of native workers.

It is less obvious why the individual *consequences* of job loss are found to be more severe for immigrants than for natives. One probable explanation is that upon job loss, the lack of general skills, including fluency in the native language, forms a barrier in the search for new employment. This interpretation is bolstered by analyses showing that the lower levels of education, experience and tenure among LDC immigrants cannot explain why they suffer more from displacement. Even when we allow effects to vary across the skill distribution, the adverse consequences of bankruptcy and workplace downsizing are more severe for LDC immigrants than for natives and EEA immigrants. Skills may also interact with the social insurance system, as its progressive nature implies relatively high benefit replacement rates for persons with low potential labour earnings, contributing to lower return rates to employment among immigrants hit by job displacement.

Although we estimate the impacts of job loss on the basis of the relatively rare events of firm bankruptcy, we extrapolate our findings to the labour market as a whole. This extrapolation is based on the additional assumption that, within each demographic group, a given fraction of displaced workers will register as unemployed job-seekers, implying that we can back out the total number of involuntary job losses from observed rates of registered unemployment. Depending on the time horizon, we find that between 33% and 53% of the elevated probability of LDC immigrants to leave employment, and an even greater fraction of their lower earnings growth, can be attributed to the combination of higher involuntary job loss rates and more severe effects of job loss.

Our results show that lack of job stability and the more frequent transitions out of employment contribute importantly to the explanation why immigrants fail to integrate fully in the host-country labour market. For the rising rates of immigration to represent a solution to—rather than an aggravation of—the fiscal challenges facing many developed nations, integration efforts therefore need to have a long-term perspective, securing robust labour market attachment over the life course. Our findings point to the necessity of greater policy focus on integration efforts in relation to early immigrant job loss—for example, in terms of activation and language skills enhancement.

APPENDIX:

TABLE A1
ESTIMATES OF EFFECTS OF BANKRUPTCY OR DOWNSIZING ON EMPLOYMENT AND LOG EARNINGS WITHOUT CONTROLLING FOR INDIVIDUAL WORKER CHARACTERISTICS

	Employment		Log earnings	
	Year 3 (1)	Years 3–5 (2)	Year 3 (3)	Years 3–5 (4)
Bankrupt	−0.061*** (0.006)	−0.056*** (0.005)	−0.302*** (0.020)	−0.275*** (0.019)
LDC * Bankrupt	−0.075*** (0.018)	−0.053*** (0.015)	−0.209*** (0.057)	−0.130** (0.056)
EEA * Bankrupt	0.007 (0.013)	0.012 (0.011)	−0.008 (0.044)	0.011 (0.040)
Downsize	−0.014*** (0.002)	−0.015*** (0.002)	−0.071*** (0.006)	−0.074*** (0.006)
LDC * Downsize	−0.044*** (0.007)	−0.038*** (0.007)	−0.075*** (0.023)	−0.056*** (0.025)
EEA * Downsize	−0.003 (0.004)	−0.005 (0.004)	0.014 (0.015)	0.010 (0.016)
Observations	6,589,558	6,563,959	6,535,663	6,533,031

Notes

Standard errors, clustered within individuals, are shown in parentheses. Regressions omit any individual characteristics for the regression specification. For further explanation of dependent variables and samples, see the Notes to Table 3.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

TABLE A2
ESTIMATES OF EFFECTS OF BANKRUPTCY OR DOWNSIZING ON LABOUR FORCE EXIT AND UNEMPLOYMENT INSURANCE BENEFIT RECEIPT WITHOUT CONTROLLING FOR INDIVIDUAL WORKER CHARACTERISTICS

	Out of labour force		Unemployment insurance	
	Year 3 (1)	Years 3–5 (2)	Year 3 (3)	Years 3–5 (4)
Bankrupt	0.033*** (0.005)	0.036*** (0.004)	0.174*** (0.009)	0.129*** (0.007)
LDC * Bankrupt	−0.016 (0.012)	−0.011 (0.011)	0.119*** (0.022)	0.102*** (0.018)
EEA * Bankrupt	−0.007 (0.011)	−0.012 (0.009)	0.011 (0.020)	0.010 (0.015)
Downsize	0.007*** (0.001)	0.008*** (0.001)	0.039*** (0.002)	0.031*** (0.002)

TABLE A2
CONTINUED

	Out of labour force		Unemployment insurance	
	Year 3 (1)	Years 3–5 (2)	Year 3 (3)	Years 3–5 (4)
LDC * Downsize	0.017*** (0.006)	0.016*** (0.005)	0.043*** (0.008)	0.030*** (0.006)
EEA * Downsize	0.002 (0.004)	0.004 (0.004)	0.006 (0.006)	0.007 (0.005)
Observations	6,589,558	6,563,959	6,589,558	6,563,959

Notes

Standard errors, clustered within individuals, are shown in parentheses. Regressions omit any individual characteristics for the regression specification. For further explanation of dependent variables and samples, see the Notes to Tables 3 and 4.

***, **, * indicate statistically significant at the 1%, 5%, 10% level, respectively.

ACKNOWLEDGMENTS

We are grateful to Taryn Galloway, Sverre Try and three anonymous referees for valuable comments. We acknowledge funding from the Ministry of Labour (project ‘Immigrant Employment Profiles’) and the Norwegian Research Council (projects ‘European Strains’ and ‘Striving for Excellence’). The paper is part of the research activities of the centre of Equality, Social Organization, and Performance, University of Oslo. Data made available by Statistics Norway have been essential for this research.

NOTES

1. Although Norway has stayed outside the European Union, the 2004 and 2007 eastwards enlargements of the union opened the Norwegian labour market to citizens of accession countries owing to Norway’s EEA membership. Our EEA sample does not include the wave of labour migrants for Eastern Europe that followed, however, as our methodological design, where we track workers in the labour market for ten years, entails that the recent cohort would be dropped from the analyses.
2. To avoid including the same downsizing or bankruptcy event multiple times, Table 1 reports statistics for the subset of the data consisting of every third observation year between 1995 and 2010. Statistics and regression results are, however, practically identical when based on the full sample.
3. As noted by Jacobson *et al.* (1993, p. 696): ‘However, we can substantially lessen the importance of this selectivity bias by restricting the analysis to workers who separate from firms that close all or a large part of their operations. Such workers are unlikely to have left their jobs as a result of their own poor performance. Therefore, in the empirical work we give greater weight to the estimated earnings loss of workers in our mass-layoff sample.’
4. Specifically, we define a major downsizing in a year as a reduction of the firm workforce by at least 60% during that year. We override the downsizing indicator if at least 60% of the workforce work together in another firm during the same year (i.e. in a different firm, interpreted as a reorganization event) or if the workforce moved back above the 40% mark in the next year.
5. Some of the sample restrictions may appear *ad hoc*, and they come at a potential cost of less external validity. For this reason, we conduct a number of auxiliary analyses that assess the robustness of results with respect to the imposition and design of sample restrictions.
6. Experience counts years with earnings from work exceeding the base amount of the public insurance system, and tenure is computed from the start date in the job record. Educational attainment is collected from the national education database. Attainment is missing for 4.5% of the records in the LDC immigrant regression sample and 5.9% of the EEA immigrant sample. For these records, we impute years of schooling from the mode attainment of workers with the same four-digit occupation code.
7. Given the log earnings specification, observations with zero earnings are dropped. This is a minor problem in our samples, however, particularly for the three-year outcome where only 0.5% have zero earnings for all three years.

8. Specifically, both employment and earnings of outmigrants are set to zero in years after they leave the country. Results are similar if we instead set earnings equal to their earnings in the year before outmigration.
9. Recall that Table 2 showed that educational attainment is much lower among LDC immigrants than natives and EEA immigrants. Work experience and tenure are lower for immigrants compared to natives.

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