Does the Impact of Plant Closure on Labour Market Attachment Differ between Immigrants and Native Workers Across the Business Cycle?

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Abstract

This paper investigates the effect of plant closure on the labour market attachment of immigrants and how these effects vary with business cycles. The research covers two periods, one of economic upturn and one of economic downturn, and uses a rich employer-employee data set. Results show that experiencing a plant closure in a recession has more severe individual repercussions than experiencing a plant closure in an expanding economy, particularly for immigrants. In good times the long-term effects of plant closure are very small for both immigrants and natives, while in a recession effects are more severe for immigrants, possibly leading to lasting scars for this group. These results appear robust using both matching techniques and fixed-effect models.

Index terms: business cycles, immigrants, plant closure

JEL codes: J15, J63, J65

1. Introduction

In this paper we analyse the impact of plant closure on the labour market attachment of immigrants and natives, focusing on how these effects vary over the business cycle. Concretely, we exploit information on plant closures to investigate the impact of a randomly determined job loss on the labour market outcomes of immigrants and natives in two different states of the economy: i) a period with increasing labour demand and falling unemployment, and ii) a period with decreasing labour demand and rising unemployment.

In Norway, the immigrant population has increased considerably during the last decades, mainly due to the inflow of immigrants from so-called non-Western countries.[1] Furthermore, these groups of immigrants are known to have difficulties in the labour market: they are overrepresented among the less educated and the low paid, and they have low labour force participation rates relative to ethnic Norwegians. In this paper we analyse the extent to which displacement through plant closure is especially harmful for this group of workers, and whether the effect is amplified in times when the labour market is shrinking.

There are several reasons why experiencing a plant closure may be associated with especially high costs for immigrants. Many immigrants come from their country of birth with less-than-perfect transferable skills, many have insufficient language skills; they may have few, if any, productive networks and imperfect information on available jobs. Furthermore, the deficit of such skills may be especially detrimental in a declining economy when reduced demand for labour gives displaced workers fewer jobs to seek and fewer job opportunities. Investigating such mechanisms is the main objective of this paper.

Our paper relates to two main topics in the empirical literature: the literature of labour market assimilation for immigrants over business cycles, and the literature on the impact of plant closures. The literature on the consequences of plant closure and multiple outcomes has grown rapidly in recent years. Studies analysing employment prospects and earnings are abundant (see e.g., Ruhm 1991, Jacobson et al. 1993; Huttunen et al. 2010; Eliason and Storrie 2006; Couch and Placzek 2010). A consensus seems to be established postulating that plant closure and downsizing may have severe and long-lasting employment effects for those affected by them. Studies from the United States seem to agree that plant closures have long-lasting effects on both earnings and labour supply (e.g., Ruhm 1991; Jacobson et al. 1993). European studies also find long-lasting effects on employment but mostly small effects on earnings (Huttunen et al. 2010; Eliason and Storrie 2006). Huttunen et al. (2006) analyse

[1] Non-Western immigrants are defined as those born in Africa, Asia, Latin America, and Central-Eastern Europe of two foreign-born parents.

short- and long-term effects for prime-age male workers displaced from Norwegian manufacturing plants. They find that displacement increases the probability of leaving the labour force by about five percentage points. Eliason and Storrie (2006) used Swedish register data to analyse the effects of all plant closures in 1987. They found that displaced workers suffer worsened labour market attachment not only during a transitory period but also in the long run. None of these studies discuss the employment prospects of immigrant workers in particular.

The literature on immigrant assimilation over the business cycle is more modest. Barth et al. (2004) find that macroeconomic conditions affect the labour market outcomes of immigrants and natives in Norway differently. In particular they show that the earnings of immigrants from outside the OECD area are more sensitive to local labour market conditions than are the earnings of natives. Chiswick et al. (1997) find that, in the U.S., employment of immigrants is more adversely affected by macroeconomic downturns than that of natives. Similarly, for the Canadian labour market, McDonald and Worswick (1997) find that immigrant men are more often unemployed during economic downturns than natives.[2]

In this paper we extend the above literature in several ways. First, to our knowledge no one has focussed on the impact of plant closure on the employment opportunities of immigrants. With increasing international migration from less developed countries to highly industrialised economies, we consider knowledge on this subject of increasing importance. Second, we emphasise the importance of business cycles, and investigate whether immigrants are more adversely affected by plant closures in periods of economic downturns. Third, although European studies on plant closure are on the rise, the majority of studies focus on the United States. Kuhn (2002) suggests that this may be partly due to institutional differences between the USA and Europe (e.g., differences in wage-bargaining regimes and employment protection). Therefore, a study from Norway, a country characterised by a relatively centralised wage-bargaining system with strong employment protection and that is heavily unionised, offers comparative interest. Fourth, the majority of studies that have analysed the impact of plant closures have used fixed-effects approaches (e.g., Jacobson et al. 1993). In recent years we have also seen the use of matching methods (see e.g., Eliason and Storrie 2006). In this paper we use both methods. To check the robustness of the matching results

[2] Our paper can also be motivated by the recent debate and discussion in the public economics literature about whether unemployment benefits should be tied to unemployment rates (see e.g., Michaillat 2010; Landais et al. 2010). Landais et al. (2010) analyse optimal unemployment insurance over the business cycle in a search model in which unemployment stems from matching frictions (in booms) and job rationing (in recessions). Their model implies that the generosity of unemployment insurance should be countercyclical.

with respect to unobserved characteristics, we present results from the fixed-effect approach. To our knowledge we are the first to combine these two methods in the study of plant closure.

2. The Norwegian context

2.1 Immigration history

Historically, Norway always had considerable restrictions on labour immigration from non-Western countries. The only historic exception is a period of liberalisation between 1957 and 1975, during which there was a considerable influx of low-skilled labour immigrants mainly from Pakistan, Turkey, and Morocco, particularly in the early 1970s. Since 1975, Norway has had very strong restrictions on labour immigration from non-Western countries, with exceptions given for those with specialised skills needed in the Norwegian labour market. In Norway, immigration policy has, to a very slight extent, been motivated by labour market considerations.

In spite of the restrictions on immigration, the immigrants' share of the Norwegian population has increased considerably since 1975. From 1975 to 1994 the increase was mainly due to the influx of refugees, asylum seekers, and family reunifications. The composition of the immigrant population changed dramatically during this period, from being dominated by Nordic and Western immigrants to, at present, being dominated by immigrants from non-Western countries. Male and female non-Western immigrants came to Norway for different reasons. Men most often seek asylum, while family reunification is the most important reason for women to settle in Norway (Statistics Norway 2006). In the period from 1990 to 2005, 65% of non-Nordic immigrants who came for reasons of family reunification were non-Western immigrant spouses.

International statistics show that the overall labour market outcomes of the foreign-born in Norway are quite favourable, compared to other OECD countries (OECD 2009). Yet, for employed immigrants, median wages are below those of the native-born, for both men and women and in the aggregate; the differences are of a similar order as those observed in other OECD countries (OECD 2009).

2.2 Labour market institutions

Compared to most other OECD-countries, the Norwegian labour market is characterised by having a very compressed wage structure, which is especially compressed at the bottom of the

wage distribution, creating high wage floors (OECD 2011). The compressed wage structure is mainly due to a centrally-established wage setting system, with a high degree of unionisation.

Dismissal for individual reasons is seldom lawful and mainly restricted to serious offences such as disloyalty. The law says that the general rule for laying off a worker for economic reasons is when the job has become 'redundant' and the worker cannot be employed in another capacity. Hence, it is very difficult to replace a worker in a specific job with another worker. The Norwegian rules for collective dismissals basically follow the EU minimum standards. There is no legal rule on the selection of individuals to be dismissed, but seniority is a strong norm.

The unemployment benefit system in Norway is universal. In Norway, the compensation ratio of unemployment benefits is approximately 62% of previous labour income, a benefit which is not especially generous compared to other Nordic and Northern European countries (OECD 2006). Benefit entitlement is for a maximum of 156 weeks, depending on previous earnings. Norway is known for its active labour market policy, focussing on enhancing training and skill rather than passive payment of unemployment benefits (OECD 2006). A prerequisite to receiving unemployment benefits is that the individual is available for work and actively seeking work.

As mentioned earlier, unemployment is much higher among non-Western immigrants than among natives: as much as four to five times higher compared to the unemployment level for the population at large during the period under study. Immigrants' take up rate of sickness benefits is also higher than among natives. Dahl et al. (2010) find no differences in sickness absence between natives and immigrants from the other Nordic countries, but a much higher level of sickness absence among immigrants from Asia and Africa in the period 1992–2003. The take-up rate of social assistance is also higher among non-Western immigrants than among natives. [3]

2.3 Business cycles

After a period of recession in the late 1980s and the beginning of the 1990s, the Norwegian economy started to grow rapidly. From 1994 and onwards, unemployment fell persistently. The upturn lasted until the end of the century, when the unemployment rate first stabilised, and then started to increase from 2001 and onwards. Our base years are 1996 and 2001.

[3] Figures from Statistics Norway (http://www.ssb.no/ssp/utg/201004/11/) show that while the share of the population as a whole on social assistance in 2008 was 3%, the corresponding share among immigrants was 8%, with immigrants from Africa having the highest share.

Noteworthy is that 1996 represents the beginning of a period with a large fall in unemployment, whereas 2001 signifies the beginning of a period where the unemployment rate is on the rise.[4]

Based on Norwegian register data, Gaure and Røed (2008) find that individual monthly exit rates from unemployment tend to double from a cyclical trough to a cyclical peak, ceteris paribus. With this in mind, we make use of business cycle fluctuations to identify potential different effects of displacement caused by an exogenous shock (plant closure). The first year from which we draw plant closures is 1996. This year and the subsequent four years (1997, 1998, 1999, 2000) characterise the period of a growing economy. Similarly, 2001 and the following years (2002, 2003, 2004, 2005) characterise the period of a shrinking economy. Together, these two different states of the economy are used to analyse how immigrants are affected by changes in economic conditions and also to compare non-Western immigrants with natives.

3. Data, sample, and variables

3.1 Data

The employer-employee data used are based on very rich individual information from several public registers collected and administered by Statistics Norway. The starting point is individual register data covering all registered jobs in Norway in the period 1995–2005. Each job is registered with a start date, a stop date (if it has stopped), wage, and working hours, as well as region, economic activity (NACE), and size of the workforce at the workplace. For all individuals holding a job we have demographic information on gender, age, number and age of children, years of completed education, type of education, marital status, union membership, years since migration, and ethnic background. A unique ID-number for each individual and each plant enables us to identify the combination of worker, workplace, and individual mobility to and from plants. We have also detailed information on individual transitions in and out of the labour market.

3.2 Definition of plant closure

We define *plant closure* as a plant with more than five employees that existed by the end of December in Year t, and which is no longer registered with any employees by the end of Year t+1. In addition, we require that the plant not reappear in Year t+2. Furthermore, to avoid the potential impact of very short-lived plants, we require that the plant also existed in Year t-1.

[4] Actually, the decline in overall unemployment rate begins in 1994 rather than 1996. Due to limitations in the number of years with available register data, we must choose 1996 as our base year.

All full-time workers employed in Year t and separated from a plant that closed down in the course of Year t+1 are defined as *involuntary leavers.* This is our treatment group. Following the literature, workers displaced by a plant closure are assumed to be workers involuntarily separated from their jobs by an exogenous shock. The control group includes all full-time workers who were employed at a plant Year t and remained in the same plant the Year t+1. In line with some other studies in this field, we choose one year as our time window, i.e., the time from when the plant was operative with at least five employees until it is no longer registered with any employees.

3.3 Sample and variables

The sample in the main analysis includes all workers in the age range 25–58 who were registered in full-time employment at the end of our two base years, 1996 and 2001. The upper age limit is set to avoid problems related to early retirement. Furthermore, contrary to much of the previous research in this field, which has focussed on male workers in the manufacturing industry, we include both men and women and all industries in the economy. Moreover, as mentioned above, we distinguish between natives and non-Western immigrants.[5]

For the first period, *plant closure* is defined as plants that existed at the end of 1996, but not at the end of 1997. In the latter period, *plant closure* is defined as plants that existed at the end of 2001 but not at the end of 2002. The observation period is 1995–2000 for the 1996 base year, and 2000–2005 for the 2001 base year. This means that the analyses cover employment development from one year prior to the plant closure to four consecutive years after plant closure, five years in all.

We investigate the effect of plant closure on employment in two different ways. First, we employ a binary variable, measuring whether the person is employed or not. This variable takes the value 1 if the person is a wage earner at the end of each year during the observation period. Second, we use the number of full-time equivalent working days as an outcome variable. This variable varies from 0 to 365 days per year in each year of the observation period. Periods outside the labour market and periods as part-time workers are subtracted.[6] Since it is mandatory that employers report hires and separations of employees to the

[5] The reason for not including Western immigrants is that their labour market attachment is very similar to that of natives. Moreover, emigration following a displacement is more common among Western immigrants' rising concern about selectivity and how to interpret the results.

[6] In the register data, working hours are measured by three categorical variables: full time (30 hours and more), long part time (15–29 hours), and short part time (4–19 hours). In construction of the full-time equivalent working days, we give full-time weight 1, long part-time weight 2/3, and short part-time weight 1/3.

authorities, we have precise information on employment spells. Those who are not in employment include those outside the labour market, in categories such as unemployment, rehabilitation, disability pension, education, as well as individuals who withdraw from the labour force and are not registered in any of the above activities.

As explanatory variables, we include a battery of individual- and firm-specific variables. Individual variables include age, number of children in total, and number of children younger than seven years old. In addition, union membership is measured by a dummy variable taking the value 1 if the worker is a union member and 0 otherwise. Hourly wage is calculated based on information about total wage, duration of the job, and working time. Education is measured by three dummy variables: compulsory school, secondary school, and university or college. Marital status is measured by a dichotomous variable taking the value 1 if the individual is married and 0 otherwise. Place of residence is measured by a dummy variable taking the value 1 if he or she lives in the capital area (Oslo) and 0 otherwise.

We also include a set of labour market-related variables measured in the last year the plant was in operation, referred to as the base year. Seniority is measured in number of years at the end of the base years, i.e., 1996 and 2001. Information as to whether the spouse is employed or not is measured by a dummy variable taking the value 1 if the spouse is registered as a wage earner at the end of the base year, and 0 otherwise. Unemployment, social assistance, and sickness benefits are measured in the year proceeding the base year, i.e., in 1995 and 2000. Unemployment is measured by a dummy variable taking the value 1 if the individual is registered as unemployed during the 12-month period. Recipient of sickness benefit is measured by a dummy variable taking the value 1 if the individual is registered as a recipient of sickness benefit during the 12-month period. Recipient of social assistance is measured by a dummy variable taking the value 1 if the individual is registered as a recipient of social assistance during the 12-month period.

Plant-specific variables include information on industry and number of employees. Industry is measured by eight dummy variables, based on the first digit in the NACE-code. Number of employees is measured by four dummy variables, varying from 10 employees or less, to more than 100 employees. Of the total sample of all full-time working men, 2.6% experienced a plant closure (measured over both years). The share is almost identical for natives and non-Western immigrants and, as expected, it varies by industry. Individuals working in the manufacturing industry are overrepresented among those who go through a plant closure.

4. Empirical approach

4.1 Method of matching

Assume that there are two potential labour market outcomes for individual i, denoted (Y_{i0}, Y_{i1}), that represent the states of being displaced (D_i=0) and not displaced (D_i=1). In practice we observe either Y_{i0} or Y_{i1}, i.e. we observe Y_{i0} if the individual is not displaced and Y_{i1} if the individual is displaced. Let Y_i be the observed labour market outcome. If we compare means by treatment status of observed treated and non-treated we get

$$(1) \quad E(Y_i \mid D_i = 1) - E(Y_i \mid D_i = 0) = E(Y_{i1} \mid D_i = 1) - E(Y_{i0} \mid D_i = 0)$$
$$= \underbrace{E(Y_{i1} \mid D_i = 1) - E(Y_{i0} \mid D_i = 1)}_{ATET} + \underbrace{E(Y_{i0} \mid D_i = 1) - E(Y_{i0} \mid D_i = 0)}_{BIAS}$$

The first term is the parameter of interest, the average treatment effect on the treated (ATET) where the first term on the left is the expected potential displacement outcome given displacement and the second term is the expected non-displacement outcome given displacement (for simplicity time indexation of potential labour market outcomes is disregarded for the time being). The second term (BIAS) is different from zero when the expected counterfactual labour market outcome for a displaced worker had he not been displaced does not coincide with the expected labour market outcome for a non-displaced worker. Either selection bias or evaluation bias arises because i) there are differences in observed characteristics, ii) differences in the distribution of the observed characteristics, and iii) differences in unobserved characteristics (for more details see for example Heckman et al. 1996, 1998b).

Matching estimators typically assume that there exist a set of observed characteristics Z such that labour market outcomes are independent of displacement conditional on Z. That is, it is assumed that the labour market outcomes (Y_0, Y_1) are independent of displacement status D conditional on Z. As mentioned above matching does not control for unobservables. As a result matching only balances unobserved characteristics that are correlated with Z.

To minimise the potential of selection bias, rich data on the individuals and the plants is needed. Employment experience, seniority, education, past sick-leave history, and demographic features are considered such individual characteristics. At the plant level, available characteristics such as the age of the plant, geographical location of the plant, number of employees, and industry (nine categories) are important in determining both the likelihood of plant closure and the individual probability of finding a job. Moreover, we expect some of these variables to be correlated with unobserved characteristics. Take seniority, for instance. It is reasonable to assume that workers with longer seniority work in

better employer-employee matches. Hence, controlling for seniority between workers in closing and non-closing plants should be of great relevance.

Matching has several advantages compared to other non-experimental methods. Matching on Z or a function of Z makes units more comparable; the distribution of observed covariates Z is more balanced between those displaced and those that did not receive the shock making direct comparison meaningful. As opposed to parametric methods, matching does not require assumptions regarding the functional form or the distribution of estimated mean effects, nor does it require exclusion restrictions (i.e., variables that affect treatment but not labour market outcome).

In the empirical analysis, we use propensity score matching. Single nearest-neighbour matching with replacement, five nearest-neighbour matching with replacement, and kernel matching are used, but only the estimates using five closest neighbour are presented.[7] We also apply the least restrictive common support condition, namely the region where at least one potential non-displaced worker is available. Assuming that the matching procedure is successful, the effect on employment for those experiencing a plant closure is calculated non-parametrically by simply taking the difference in the share that is employed the year after the plant has ceased to exist, and the four consecutive years and the equivalent share for individuals working in plants that did not lay off their workers.

4.2 OLS and person fixed-effect models

The standard methodological approach for analysing the impact of displacement was introduced by Jacobson et al. (1993). One obvious advantage of this approach is that it controls for unobserved time-invariant individual effects, which is a shortcoming of the matching approach. The three approaches (including matching) weight observations differently and, as a result, may recover different estimates. In the case of regression, the estimand is a vector of population regression coefficients, while the matching estimand is typically a weighted average of comparisons across cells defines by covariates (Angrist and Pischke 2009).

A drawback of fixed-effect models is that they are parametric, and thus rely on functional form assumptions and exclusion restrictions. Moreover, between-person variation is ignored; this can yield standard errors that are larger than those produced by methods that

[7] The comparison group is relatively very large, which means that overuse of the same observation should not be of concern. Nevertheless, we test for the overuse of observations. Treating the matched comparison sample as given will understate the standard error (Smith 2000). To account for this, we present bootstrapped standard errors with 100 repetitions (standard errors using 500 repetitions changed marginally).

utilise both within- and between-person variation. The main specification estimated is given by:

$$(3) \quad Y_{it} = \alpha_1 X_{it} + \alpha_2 Z_{it} + \sum_{j=-2}^{4} D_{it-j}\beta_j + \tau_t + \delta_i + \varepsilon_{it}$$

Y measures labour supply (binary or duration measure) for individual *i* at time *t*. X is a vector of individual characteristics, and Z is a vector of plant and regional variables. Time dummy variables are included in τ_t. The parameter δ_i represents the individual fixed effect. The displacement variables are given by $D_{it\text{-}j}$, which measure whether or not displacement took place at time *t-j*, where t is the observation year and *j* runs from -2 and 4. Hence, the parameters β_j capture the effect of displacement before, during, and after displacement and our main parameter of interest. We permit displacement to affect labour supply up to four years after displacement. Finally, ε_{it} is a stochastic error term. It should be noted that the fixed effect estimators impose strict exogeneity assumption, i..e., that the error term in (3) ε_{it} is assumed to have mean zero conditional on past, current, and future values of the regressors (including the unobserved individual effect δ_i). Equation (2) and equation (3) do also make clear that in the fixed effect procedure we are linearizing and accounting for unobserved heterogeneity while in the matching procedure we are not.

In the empirical analyses, we estimate both OLS and fixed-effect versions of equation (3). OLS estimations ignore the individual heterogeneity term, but we include them to contrast them to the fixed effect estimates. In the OLS estimation, all variables, except age and local unemployment, which are measured the year displacement occurs, are measured in the pre-displacement year. In the fixed-effect estimation, only age, local unemployment rate, and time dummies are included as controls (time invariant variables cancel out). Neither of these approaches impose common support.

4.3 Empirical concerns

One potential problematic issue is that countercyclical measures may affect the observed 'smooth' pattern over the period under study, in which case differences in employment patterns between natives and immigrants would not be solely an effect of business cycles. The unemployment rate fluctuates between 2–4% over the period under analysis, which is a fairly narrow variation compared to many other countries. Furthermore, Norway has throughout the period of analysis emphasised the use of active labour market measures, rather than passive payment of unemployment benefits, and has deliberately used active policies in a

countercyclical manner to activate the economy and smooth out fluctuations (OECD 2006). If immigrant workers are 'marginal workers' to a larger extent than natives (e.g., due to language deficits, short work experience, or badly matched education), their employment level is likely to fluctuate more over the business cycle. To analyse whether such a pattern is strengthened by exogenous demand shocks such as a plant closure is an important aim of this paper. Of primary importance for our study is that there are no immigrant-specific policy changes arising during the period of analysis that could explain the cyclical pattern. To our knowledge, no such policy changes occurred.

A second concern is that the composition of individuals changes over the business cycle, making comparisons across different periods difficult. In economic upturns, the increased demand for labour may increase the share of 'marginal' workers with a lower attachment to the labour market. Conversely, in economic downturns it might be the case that those who remain in the labour market are the more positively selected. In the empirical analyses, we are not able to fully control for such fluctuations in the labour force. We do, however, present some descriptive statistics of some core variables included in the analyses and also conduct a robustness check by pooling the data from the two periods, thereby controlling for potential compositional changes.

A third concern is related to the selection of immigrants and natives in the labour market. Given the higher unemployment rate among immigrants, immigrants who succeed in obtaining full-time employment might be more positively selected compared to natives who obtain full-time employment. This may result in an employer selection effect for immigrants, where those immigrants who succeed in obtaining employment may be more likely to succeed in getting a new job soon after the plant closure. If such a pattern is present, the difference between natives and immigrants with respect to the impact of plant closure could be understated.

Last but not least, if rules, rights, and regulations are different for native Norwegians and foreigners living in Norway, it is likely that their incentives and behaviours are also different. The Norwegian public welfare system is characterised by universal rights accessible to all citizens without means testing, provided one has been resident of Norway for a minimum period of time. Exceptions apply for some benefits (the two most important are entitlement to disability pension and old age pensions), where the residency requirement is set, which for most purposes is three years. Hence, newly arrived immigrants may be affected by such conditions. However, given the chosen upper age cut-off in this study, and regulations stating that both medical rehabilitation and vocational training must be tried before a

disability pension is granted, we believe this not to be a problem. Another welfare arrangement that might in principle affect immigrants' behaviour is accessibility to unemployment benefits. Eligibility for unemployment benefits is conditional on annual labour income the previous year. However, since our sample is confined to full-time workers, and the eligibility threshold is very low (approximately 6500€ in 2001), such conditions are unlikely to affect our target group. Moreover, a descriptive analysis shows that a very low share of workers in our sample (both immigrants and natives) is ineligible for unemployment benefits.

5. Results

5.1 Matching results

In this section we present results when applying the method of matching, using five nearest neighbour matching with replacement and applying a common support condition.[8] We use a restrictive definition of displacement; that is, we include only workers who have been displaced due to plant closure in the course of a 12-month window. We carry out several tests and investigate several aspects of the matching procedure to ensure the reliability of the results. We have tested the model specification using the specification test against omitted variables, non-normality, heteroscedasticity, and general misspecification (information matrix test).[9] Very few observations are removed from the participant group when the common support condition is imposed (here defined as the region where at least one non-participant observation is available), accounting for less than 1% of the participant group at the most.[10]

The upper part of Table 5.1 presents the matching estimates for the effect of plant closure on subsequent employment duration for natives and non-Western immigrants. The lower part of Table 5.1 shows results based on the binary measure of employment. On the left of Table 5.1 are results for the 1996 cohort, while on the right are results for the 2001 cohort. Year 0 in each column is 1996 and 2001, respectively.

[8] Results from nearest neighbour matching and kernel matching are available on request. Five nearest neighbour matching lies, in all cases, between the other two matching procedures.

[9] The Gauss codes used for the specification tests were provided by Michael Lechner. The choice of variables used in each of the separate estimations of the likelihood of plant closure is guided by a score test against omitted variables. If important variables are omitted, the error term may become non-normal and heteroscedastic, while inclusion of superfluous variables may lead to loss of efficiency (see, e.g., Lechner 1995). All variables are dummy variables so as to ensure that we applied as few variable specification restrictions as possible.

[10] No sign of 'over-use' of any one observation occurs when applying nearest neighbour matching. Gerfin and Lechner (2002) suggest monitoring the occurrences by calculating the share of the largest 10% of the weights relative to the sum of the weights in the respective non-participant group. They consider a value of between 17–55% as acceptable.

Table 5.1. The impact of plant closure in 1996 and 2001. Norwegian natives and non-western immigrants compared. Dependent variables: i) employment duration and ii) holds a job. Propensity score matching estimates.

	Duration of employment			
	1996		2001	
Year	Non-Western immigrants	Natives	Non-Western immigrants	Natives
0	-2.515	2.283**	-5.449**	0.745
	(4.480)	(0.463)	(2.547)	(0.495)
1	-85.520***	-31.404**	-88.631***	-58.178**
	(8.502)	(1.074)	(5.116)	(1.390)
2	-59.856***	-16.733**	-69.576***	-32.968***
	(11.030)	(1.525)	(6.907)	(1.564)
3	-50.486***	-16.443**	-50.695***	-25.090***
	(11.919)	(1.614)	(7.815)	(1.622)
4	-25.024**	-10.953***	-37.735***	-21.838***
	(12.468)	(1.493)	(7.433)	(1.393)
	Binary job measure			
1	- 0.258***	- 0.105***	-0.271***	-0.183***
	(0.035)	(0.004)	(0.023)	(0.004)
2	- 0.139***	- 0.053***	-0.155***	-0.072***
	(0.034)	(0.004)	(0.023)	(0.005)
3	-0.121***	-0.039***	-0.122***	-0.058***
	(0.035)	(0.004)	(0.021	(0.004
4	-0.016	- 0.030***	-0.061**	-0.048**
	(0.073)	(0.005)	(0.019)	(0.004)
N	13977	748406[1]	26827	880646[1]

Note: Level of significance, *** p<.01, ** p<.05, * p<0.1. Bootstrapped standard errors with 100 replications. For computational reasons, only 50% of the control group is used in the matching procedure. The reduced control groups are approximately 10 times the size of the treatment groups.

Let us focus on the left side of the upper part first. In the base year, it is reasonable to expect no impact of plant closure, while the impact is expected to be largest in the first year after the plant closes down and to diminish thereafter. This is the pattern for immigrants. The 1996 cohort of displaced immigrants experience a large dip in the first post-closure year, approximately 85 working days on average. The employment deficit is thereafter reduced, and five years later (in 2000) it is down to 25 days. At face value, a reduction of 25 working days represents a reduction of approximately 9% compared to the mean value of working days for non-displaced immigrant workers in 2000 (276 working days).

A comparison of the 1996 cohort with the 2001 cohort shows several findings worth noting. The immediate dip is deeper for the 2001 cohort. The development and size of the employment deficit for immigrants is very much the same in the two cohorts, but the recovery is much slower for the 2001 cohort. Five years later, immigrants who experienced a plant closure still have, on average, 38 fewer working days compared to immigrants not affected by a plant closure, which is a reduction of approximately 14% compared to the mean value of working days for non-displaced workers. In summary, in both cohorts displaced immigrants

experience a dip in employment, but the dip is deeper and the recovery rate is much slower for the 2001 cohort compared to the 1996 cohort. This suggests that going through a plant closure during a recession has more long-lasting effects. Moreover, it is noteworthy that immigrants who experienced a plant closure in 2001 have a noticeably recovery in the last year of the observation period (2005).which coincides with a shift in business cycles. This suggests that the significant recovery estimated in 2005 for displaced immigrants is partly due to more favourable economic conditions.

Looking at natives and number of working days, we see that displaced native workers in 1996 also experience a dip in the first post-closure year. The fall is estimated to be 31 days. The employment deficit is reduced in the following years and down to 11 days the final year, which represents a reduction of approximately 4% compared to the mean value of working days for non-displaced native workers in 2000. For the 2001 cohort of natives, the dip for displaced workers the year following plant closure equals 58 days, which is much larger compared to the immediate dip for the 1996 cohort. The employment deficit is reduced in the following years, and by 2005 it is equal to 21 days. In relative terms, an employment deficit of 21 days represents a reduction of approximately 6% compared to the mean value of working days for non-displaced native workers in 2005.

In short, we find that a plant closure has more long-lasting effects for immigrants than natives during economic downturns. The relative employment gap between displaced and non-displaced workers is smaller for natives compared to that of immigrants. These findings suggest that, in a growing economy with increasing demand for labour, immigrants and natives alike recover rapidly, while a declining economy, with fewer available jobs, more adversely affects immigrants, indicating that losing a job is more severe for immigrants than for natives when the competition for jobs is greater.

As mentioned above, the period 2001–2005 is not a ‘pure’ downturn, but ends in the beginning of a new period of economic growth, and results suggest that displaced immigrants seem to have benefited from the economic upturn in 2005. Results also indicate that the upturn seems to have a stronger impact for displaced immigrants than for displaced natives, as shown by the fact that the negative effect of displacement for immigrants is larger if we confine our examination to the period 2001–2004. This seems to indicate that, while immigrant displaced workers recover more slowly during economic downturns, they also recover more quickly in economic upturns.

The lower part of Table 5.1 presents results when using the *binary measure* of employment. For the 1996 cohort, we see that displaced immigrants experience a large

immediate fall in the employment rate, equal to 26 percentage points. The employment deficit is reduced considerably in the following years, and by 2000 it is down to one percentage point and not statistically significant. For the 2001 cohort of immigrants, the first-year reduction in employment is about the same as for the 1996 cohort. The recovery, however, is different, and in the last year we observe a significant deficit of six percentage points. As was the case with the duration measure, we also find a considerable recovery in the last year of observation (2005) for the displaced immigrants with the binary measure. Again, this suggests that the recovery was reinforced by the more favourable macroeconomic conditions starting in 2005. In summary, the overall picture for the binary measure resembles that of the duration measure. A plant closure sets immigrants back, but it seems only to have long-lasting effects when the labour market is shrinking.

Finally, looking and the binary measure for natives, we see that displaced workers experience an immediate fall in employment of approximately 10 percentage points in 1996, which is down to approximately three percentage points by the end of a five-year period. For the 2001 cohort of natives, the immediate reduction is on the order of 18 percentage points. By 2005 it is down to five percentage points. Therefore, for natives as for immigrants, we find that, during a recession, an employment gap remains even after four years. However, the employment deficit between displaced and non-displaced workers is smaller for natives (five percentage points) than for immigrants (six percentage points), a difference which would be larger if we were to measure the deficit in percent relative to average employment level for natives and immigrants. Again, it is evident that the year 2005 is the start of a new recovery and it benefits immigrants: the employment deficit decreases considerably between 2004 and 2005, and the relative change between these two years is larger for immigrants than for natives.[11]

In summary, in so-called good times, both immigrants and natives seem to recover fairly well after a plant closure within the post four-year window we use. A high recovery rate within 4–5 years is in accordance with results from Eliason and Storrie (2006) for the Swedish labour market. Ruhm (1991) also finds recovery in employment within the first four years. Moreover, Ruhm finds that in so-called bad times the relative recovery rate between displaced

[11] To shed light on the potential problem related to selection on unobservables, we used the bounding approach introduced by Rosenbaum (2002). It addresses how strongly an unmeasured variable must influence the selection process in order to weaken the results from the matching analyses. Results (available upon request) suggest that the matching estimates are not much affected by hidden bias. One exemption is the results for natives in 1996, which appear to be least robust to the possible presence of unobserved effects. For immigrants, our previous results appear to be quite robust.

and non-displaced workers is worse for both immigrants and natives, and the recovery is slower for displaced immigrants.

5.1.1 Control for differences between cohorts

It is well known that various cohorts can differ along several dimensions that are relevant for explaining labour market performance. In the matching setup, it is not possible to control for potential cohort differences. However, this can be done by pooling the data for 1996 and 2001, and thereafter estimating labour supply models by OLS. The estimated equation is presented in equation (4). The analyses are carried out for immigrants only. Cohorts are measured by year of arrival in Norway. We use five dummy variables, measuring five years of arrival intervals: 1960–1980, 1981–1985, 1986–1990, 1991–1995, and 1996 or later. We estimate the following equation:

$$(4) \quad Y_{it} = \alpha_1 X_{it} + \alpha_2 Year2001 + \alpha_3 Cohort_i + \alpha_4 Closure_i + \alpha_5 Year2001 x Closure_i$$

where Y is a measure of employment (duration and binary measure) for individual i at time t, X is a vector of control variables including the same explanatory variables used earlier, *Cohort* is a vector of arrival cohort dummy variables, *Year2001* is a dummy variable taking the value of 1 if the year is 2001, and 0 if it is 1996, *Closure* is a dummy variable taking the value of 1 if the worker experienced a plant closure, and 0 otherwise. Finally, *Year2001xClosure* captures how plant closure affects employment over the business cycle.

To check for the importance of differences between cohorts, we estimate equation (4) *including and excluding* control for the cohort dummy variables measuring year of arrival in Norway. Table 5.2 presents results, both for the binary measure and duration measure of employment. For both labour market outcomes measures, the first model does not control for cohort, while the second model controls for cohort effects. The model is estimated separately for each pre- and post-closure year. Only results for the coefficients α_5 are reported, but the full set of control variables is included in the estimations. All regressions also include control for years since migration (YSM). Furthermore, only results for the first and the last post-plant closure years are reported. If α_5 varies considerably depending on whether we control for cohorts or not, we suspect that differences between cohorts may bias the matching estimators of Table 5.1.

Table 5.2. *The impact of plant closure in 1996 and 200. Non-western immigrants. Dependent variables: i) employment duration and ii) holds a job. OLS estimates.*

	Binary measure		Duration of employment	
	Without cohort control	With cohort control	Without cohort control	With cohort control
Year2001xClosure t1	-0.021	-0.019	-7.563	-7.271
	(0.022)	(0.022)	(6.225)	(6.224)
Year2001xClosure t4	-0.073***	-0.071***	-22.904**	-21.355**
	(0.028)	(0.022)	(9.447)	(9.445)

Note: All models include the full set of controls. We only report estimates for the α_4 coefficient; Level of significance, *** p<.01, ** p<.05, * p<0.1.

Results indicate that the first year after plant closure (1997 and 2002, respectively) there is a negative impact, but the difference between 1996 and 2001 is not significant. In the last year of observation, however, there is a significantly negative difference between the two years, suggesting that being laid off during a recession has more detrimental long-lasting effects compared to being laid off in an economic upturn. This applies for both the binary and duration measure. These results are in accordance with results using matching techniques. More important in this respect is that controlling for cohort difference in the second columns did not alter the results in any significant way (-0.071 vs. -0.073 and -21.355 vs. -22.904). The results from this simple control procedure suggest that previous findings are not driven by cohort differences.

5.2 OLS and fixed-effect estimates

We now turn to estimating OLS and fixed-effect models. As mentioned above, even though the fixed-effect approach in equation (3) does not enable us to control for unobserved time varying selection, it effectively sweeps away any time invariant unobserved effects that may affect the matching results. Table 5.3 presents estimates of the effect of displacement on both measures of labour supply using the fixed-effect approach presented in equation (3). The upper half presents the OLS estimates. The lower half exploits the panel dimension and presents the fixed-effect estimates. For the OLS estimates, we include the same battery of explanatory variables as in the matching analyses. Except for measures of age and regional unemployment rate, all variables are measured prior to displacement.

Table 5.3. The impact of plant closure in 1996 and 2001. Norwegian natives and non-western immigrants compared. Dependent variables: i) employment duration and ii) holds a job. OLS and fixed-effect estimates.

OLS				
Duration of employment				
	1996		2001	
Year	Natives	Immigrants	Natives	Immigrants
-1	8.417***	6.668	3.693***	6.690*
	(0.756)	(5.716)	(0.825)	(3.867)
0	5.069***	17.305***	9.239***	14.258***
	(0.756)	(5.716)	(0.825)	(3.867)
1	-30.061***	-69.403***	-51.988***	-72.131***
	(0.756)	(5.716)	(0.825)	(3.867)
2	-15.233***	-44.733***	-30.568***	-65.365***
	(0.756)	(5.716)	(0.824)	(3.867)
3	-14.511***	-33.698***	-25.531***	-50.630***
	(0.756)	(5.715)	(0.825)	(3.867)
4	-8.011***	-12.956***	-21.998***	-36.573**
	(0.756)	(5.716)	(0.825)	(3.867)
Binary job measure				
-1	0.030***	0.055***	0.013***	0.054***
	(0.002)	(0.017)	(0.002)	(0.012)
0	0.013***	0.039**	0.024***	0.056***
	(0.002)	(0.017)	(0.002)	(0.012)
1	- 0.095***	- 0.218***	-0.172***	-0.241***
	(0.002)	(0.018)	(0.002)	(0.012)
2	- 0.043***	- 0.099***	-0.071***	-0.156***
	(0.002)	(0.018)	(0.002)	(0.012)
3	-0.031***	-0.078***	-0.063***	-0.127***
	(0.002)	(0.018)	(0.002)	(0.012)
4	- 0.016***	0.013	-0.048***	-0.063***
	(0.002)	(0.078)	(0.002)	(0.012)
N	449177	83862	528222	160962
Fixed-effect estimates				
Duration of employment				
	1996		2001	
Year	Natives	Immigrants	Natives	Immigrants
0	-3.722***	9.643	5.535***	6.697
	(0.944)	(7.239)	(1.013)	(4.720)
1	-39.031***	-76.310***	-55.520***	-79.303***
	(0.944)	(7.239)	(1.013)	(4.720)
2	-23.951***	-50.854***	-33.621***	-71.940***
	(0.944)	(7.239)	(1.013)	(4.721)
3	-23.018***	-38.999***	-28.565***	-57.320***
	(0.944)	(7.238)	(1.013)	(4.723)
4	-16.535***	-17.931***	-24.848***	-42.850***
	(0.944)	(7.239)	(1.013)	(4.724)
Binary job measure				
0	-0.018***	-0.019	0.010***	0.000
	(0.003)	(0.022)	(0.003)	(0.014)
1	- 0.128***	- 0.273***	-0.183***	-0.294***
	(0.003)	(0.023)	(0.003)	(0.014)
2	- 0.074***	- 0.151***	-0.081***	-0.206***
	(0.003)	(0.022)	(0.003)	(0.014)
3	-0.062***	-0.127***	-0.072***	-0.176***
	(0.003)	(0.022)	(0.003)	(0.014)
4	- 0.046***	-0.033	-0.056***	-0.109***
	(0.003)	(0.022)	(0.003)	(0.014)
N	449177	83862	528222	160962

Note: In the OLS we control for the same battery of explanatory variables as in the matching analyses. The variables are all measured pre-displacement. In the fixed-effect analyses, we only control for year dummies, age and age squared, and regional unemployment. Level of significance, *** p<.01, ** p<.05, * p<0.1.

The OLS estimates in Table 5.3 present a very similar pattern compared to the matching estimates in Table 5.1. In good times, both immigrants and natives recover fairly well after a plant closure within the four-year window we use. In bad times, the recovery rate of displaced workers relative to workers that were not displaced is worse for both immigrants and natives, and the recovery rate is lower among displaced immigrant workers.

The fixed-effect specification measures effects on labour supply relative to employment two years before the displacement. Hence, this dummy variable is removed to avoid perfect collinearity. For instance, for natives in 1996, using the binary measure, the OLS estimate says that there is an early employment surplus among the displaced workers of three percentage points (0.03 in Year -1). This corresponds approximately to the difference between the OLS and fixed-effect estimates. In general, the fixed-effect estimates suggest approximately the same pattern as presented earlier. Experiencing a plant closure in a declining economy has more severe repercussions than experiencing a plant closure in an expanding economy. This is especially true for immigrants. In good times we do not find a larger impact of a plant closure for immigrants. However, in a shrinking economy, the impact of a plant closure seems to have more long-lasting effects for immigrants, possibly scarring effects.[12]

Table 5.4. The impact of plant closure in 1996 and 2001. Non-Western immigrants. Dependent variables: i) employment duration and ii) holds a job. OLS estimates.

	OLS			
	1996		2001	
Year	Binary measure	Duration of employment	Binary measure	Duration of employment
-1xNon_western immigrant	0.025*	-2.655	0.034***	0.719
	(0.014)	(4.655)	(0.010)	(3.163)
0xNon_western immigrant	0.024*	10.978**	0.024**	2.286
	(0.014)	(4.636)	(0.010)	(3.163)
1xNon_western immigrant	-0.123***	-40.425***	-0.076***	-22.702***
	(0.014)	(4.636)	(0.010)	(3.163)
2xNon_western immigrant	-0.055***	-30.367***	-0.092***	-37.102***
	(0.014)	(4.636)	(0.010)	(3.163)
3xNon_western immigrant	-0.046***	-19.797***	-0.070***	-27.524***
	(0.014)	(4.636)	(0.010)	(3.163)
4xNon_western immigrant	0.030	-5.545	-0.022**	-17.008***
	(0.014)	(4.636)	(0.010)	(3.163)
N	2329080	2329080	2802936	2802936

Note: In the OLS we control for the same battery of explanatory variables as in the matching analyses. The variables are all measured pre-displacement. Level of significance, *** p<.01, ** p<.05, * p<0.1.

[12] To see whether the ethnic composition at the plant affects the results, we have also regressed models where we include a pre-displacement variable for the share of non-Western immigrants at the plant. Since we add it as a pre-displacement variable, we only estimate it by OLS. The results (not presented) are almost unaltered compared to the OLS duration measure in Table 5.3. This means that our results are not driven by ethnical compositional differences between plants that close down and plants that do not.

Table 5.4 presents OLS estimates of the impact of plant closure on the employment of natives relative to immigrants.[13] This is achieved by using the interaction terms of the immigrant dummy and the dummy variables capturing pre- and post-displacement periods.[14] For both measures of labour supply we see that the negative relative employment effect between natives and immigrants is larger in periods with declining labour demand. The last row shows that, in the period with declining labour demand, the employment gap is still 17 days four years after displacement, compared to five days in the period with increasing demand. The estimates of the binary measure present similar results: In bad times we find an employment deficit equal to 2.2 percentage points in the last observation year, compared to no significant impact in good times.[15]

5.3 Separate analyses for men and women

Due to different labour markets for men and women, especially for immigrants, separate analyses by gender are warranted. Separate analyses by gender, in Table 5.5, present results that largely resemble the results for all workers. The upper half, presenting results for men, shows results fairly similar to those for all workers: Immigrant men recover very well in good times, but in declining labour markets their recovery rate is slower, and it is worse compared to natives towards the end of the observation period. Also, for women, shown in the lower half of Table 5.5, we find that the recovery rate for immigrant women is good when labour markets are favourable, and much less so in a declining economy.

[13] The fixed-effect estimates presented similar results to the OLS results and are not presented.
[14] The immigrant dummy is also interacted with the year dummies. These are not shown in the table.
[15] As an extra analysis, we estimate fixed-effect models for two separate groups: Low educated workers and young workers. In the first analysis we compare low educated natives with low educated immigrants to see whether the effect is different than when we compare more similar groups. If one believes that the effect of plant closure is basically a problem for the low skilled, one should expect to find a strong effect for low skilled natives as well. Similarly, if part of the effect we have picked is explained by young workers with short experience, we should get a picture of the magnitude of this by comparing young natives with young immigrants. The results (not shown, but available upon request) suggest that there are no indications that the estimated effects are a result of differences in characteristics between natives and immigrants.

Table 5.5. The impact of plant closure in 1996 and 2001. Norwegian natives and non-western immigrants compared. Dependent variables: holds a job. OLs estimates.

	OLS-Binary job measure			
	Men			
	1996		2001	
Year	Natives	Immigrants	Natives	Immigrants
-1	0.025***	0.040**	0.014***	0.039***
	(0.002)	(0.020)	(0.002)	(0.014)
0	0.012***	0.034*	0.024***	0.043***
	(0.002)	(0.020)	(0.002)	(0.014)
1	- 0.089***	- 0.191***	-0.173***	-0.224***
	(0.002)	(0.020)	(0.002)	(0.013)
2	- 0.036***	- 0.054***	-0.075***	-0.159***
	(0.002)	(0.020)	(0.003)	(0.014)
3	-0.034***	-0.074***	-0.067***	-0.123***
	(0.002)	(0.020)	(0.003)	(0.013)
4	- 0.016***	0.013	-0.050***	-0.066***
	(0.002)	(0.020)	(0.003)	(0.012)
		Women		
-1	0.042***	0.056**	0.012***	0.058***
	(0.004)	(0.036)	(0.004)	(0.020)
0	0.021***	0.054	0.027***	0.081***
	(0.005)	(0.035)	(0.004)	(0.020)
1	- 0.113***	- 0.299***	-0.168***	-0.279***
	(0.005)	(0.035)	(0.004)	(0.020)
2	- 0.0063***	- 0.237***	-0.065***	-0.151***
	(0.004)	(0.035)	(0.004)	(0.020)
3	-0.030***	-0.091***	-0.054***	-0.137***
	(0.005)	(0.035)	(0.004)	(0.021)
4	- 0.017***	0.001	-0.046***	-0.063***
	(0.002)	(0.035)	(0.004)	(0.020)
N	449177	83862	528222	160962

Note: In the OLS we control for the same battery of explanatory variables as in the matching analyses. The variables are all measured pre-displacement. Level of significance, *** p<.01, ** p<.05, * p<0.1.

5.4 Mechanisms

We have documented that, during downturns in the economy, immigrants fare less favourably compared to natives in the aftermath of a displacement. One likely explanation is that immigrants face difficulties in the competition for jobs when there are fewer openings. Lack of language skills, fewer productive networks, and less than perfect transferable skills are three likely candidates. Lack of skills may be especially severe in declining labour markets. Even though we lack information about all of these specific variables, the variables included in the analyses can indirectly shed some light on these issues.

Concretely, we use interaction terms to capture mechanisms that may be causing the observed pattern. We start with the empirical approach in Table 5.4. To achieve a measure of the difference between the two periods, we include a new interaction term, i.e., a year dummy taking the value 1 if the year is 2001 and 0 if it is 1996. Table 5.6 shows results for four models that include different variables expected to capture the above-mentioned mechanisms. Model 1 is a stripped model, including family variables and industry variables only. Model 2 adds human capital variables (education and seniority). Model 3 adds a variable intended to

capture individual attachment to the labour market prior to displacement, measured by whether or not the person has been a recipient of social benefits and/or sickness benefits. Finally, Model 4 adds information on wages as a proxy for individual productivity.

Our aim is to test whether the consequences of lack of human capital, weak labour market attachment, and generally low productivity are especially severe in economic downturns (2001 and onwards), and if this can partly explain the increasing employment gap between natives and immigrants between the two periods. Equation (5) presents the set up for the key variables:

$$(5) \quad Y_{it} = \alpha_1 X_{it} + \alpha_2 Z_{it} + \sum_{j=-2}^{4} D_{it-j}\beta_{1j} + Nw^* \sum_{j=-2}^{4} D_{it-j}\beta_{2j} + Nwx2001^* \sum_{j=-2}^{4} D_{it-j}\beta_{3j} + \tau_t + \varepsilon_{it}$$

Where Nw is a dummy variable for non-western immigrants, and 2001 is a dummy variable for the year 2001. Estimates of the above equation are presented in Table 5.6. Since the analyses show only small differences between the OLS and fixed-effect estimates, we limit the presentation to the OLS results. Furthermore, we limit the presentation to the binary measure of labour supply, and we only present results from the triple interaction estimate β_{3j}.

Table 5.6. The impact of plant closure in 1996 and 2001. Norwegian natives and non-western immigrants compared. Dependent variables: holds a job. OLS estimates.

	Binary measure			
Year	Model 1	Model 2	Model 3	Model 4
-1xNon_western immigrantx2001	-0.003	-0.002	0.004	0.006
	(0.017)	(0.017)	(0.017)	(0.017)
0xNon_western immigrantx2001	-0.013	-0.011	-0.005	-0.003
	(0.017)	(0.017)	(0.017)	(0.017)
1xNon_western immigrantx2001	0.034*	0.036*	0.041**	0.044**
	(0.018)	(0.018)	(0.018)	(0.018)
2xNon_western immigrantx2001	-0.048***	-0.046***	-0.041**	-0.038**
	(0.018)	(0.018)	(0.017)	(0.017)
3xNon_western immigrantx2001	-0.037**	-0.035**	-0.030*	-0.028*
	(0.018)	(0.018)	(0.017)	(0.017)
4xNon_western immigrantx2001	-0.065***	-0.063***	-0.058***	-0.055***
	(0.018)	(0.018)	(0.017)	(0.017)
N	2329080	2329080	2802936	2802936

Note: Level of significance, *** p<.01, ** p<.05, * p<0.1.

Model 1 shows that the labour supply deficit for immigrants (except for the first post period) is greater in the bad times than in good times. In Model 2, adding human capital variables reduces the interaction coefficient somewhat, as would be expected if the lack of human capital is especially severe in declining labour markets. However, the reduction is only minor, suggesting that this is not of critical importance. It is a bit surprising that the human capital

variables explain relatively little. This might be explained by the fact that information on completed formal education is poor for a large percentage of the immigrants. There is a large percentage with missing information, making this variable a less precise variable for formal skills for immigrants. Next we add the variables for labour market attachment. The results from Model 3 indicate that these variables appear to play a more important role, suggesting that having a history of weak attachment to the labour market is especially severe when job opportunities are few. Finally, we include a measure of wages, meant to proxy individual productivity. This reduces the coefficients further, suggesting that being a low productive worker is especially severe during economic downturns.

This exercise suggests that being a worker with a weak attachment to the labour market, and having generally low productivity is especially severe when the competition for jobs is fierce. Immigrant workers are overrepresented in these positions, and this contributes to explain the pattern.[16]

The discussion and analysis above focus on the demand-side aspects of the labour market. However, a generous welfare state like the Norwegian one may also affect the supply side through reduced work incentives, in the sense that it may give immigrants less economic gain from work compared to natives. The most important reason is that, because wages or potential wages for immigrants tend to be lower than those of natives, immigrants will typically face higher social security replacement ratios (Røed and Fevang 2006) in a welfare system with relatively high minimum benefits. Hence we cannot rule out that, due to low skills and wages, immigrants in the aftermath of a plant closure may be economically more tempted to seek welfare assistance. If potential downward pressure on wages in periods with a declining labour market is more pronounced in occupations and industries where immigrants are overrepresented, the economic disincentives may fall more for immigrants than for natives in the aftermath of a displacement. Still, when we pool the data and compare natives and immigrants (as in Table 5.4), and controlling for industry, we still find significant recovery gaps.

[16] Another explanation, which is not touched upon, is discrimination. Several audit studies have suggested that immigrants are exposed to discrimination in the labour market (see, e.g., Carlsson and Rooth 2007). We cannot rule out that part of the native-immigrant gap in post-plant closure employment may be due to discrimination. If this should explain the larger gap in declining labour markets, discrimination must be more present in declining labour markets. Unfortunately, to test this is beyond what our data material can credibly address.

6. Summary and conclusions

In this paper we analyse the effect of plant closure on the labour market attachment of immigrants and natives, and investigate if the estimated effects vary with business cycles. The data used are drawn from very rich individual information from several public registers. The sample in the main analyses includes all workers in the age range 25–58 registered in full-time employment at the end of our two base years, 1996 and 2001. The year 1996 is the beginning of a period with a large fall in unemployment, whereas 2001 is the beginning of a period when the unemployment rate is rising. To estimate the effect of plant closure on labour market attachment we use both matching and fixed-effect estimators.

With both estimators we find that the recovery rate for displaced immigrants is quite good when the state of the economy is good. Only modest long-term effects are found. However, being displaced in a period of rising unemployment seems to have long-lasting effects on the immigrants' employment opportunities: the immediate fall in employment is deeper and the recovery rate is slower compared to the fate of those being displaced in 'good' times. For natives, we also find a stronger negative effect of plant closure in an economic downturn than in an economic upturn. However, results suggest that the long-term employment effects are less severe for natives than for immigrants.

Our results have important policy implications. They suggest that on average immigrants have lower productivity compared to natives, and this productivity gap is especially severe in a declining economy. Increased focus on skill-enhancing training programmes may increase immigrants' productive capacity, making them less vulnerable in economic downturns. Even though Norway spends considerable resources on active labour market policies, and especially training policies when it comes to unemployed adults, immigrants are not a special target group. Avoiding earmarking may have some advantages, in that it cannot be considered discriminatory with respect to unemployed ethnic Norwegians. Yet the needs, capabilities, and earning capacity of immigrants differ in many respects from those of ethnic Norwegians, something which may be detrimental for all parties when such skill differences are not taken into account when providing training.

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