Employment Protection Legislation and the Labor Market Position of Immigrants - A Natural Experiment

Anders Bornhäll
Sven-Olov Daunfeldt
Niklas Elert

HUI Working Paper Series, number: 125
August, 2017

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Employment Protection Legislation and the Labor Market Position of Immigrants - A Natural Experiment

Anders Bornhäll I, II, Sven-Olov Daunfeldt I, Niklas Elert III

Abstract. How does employment protection legislation affect the labor market position of immigrants? We investigate this question, which has scarcely been studied in a robust way previously, even though it is highly relevant from a policy perspective. A natural experiment enables us to identify a causal relationship between the reform of an employment protection law and the employment status of immigrants in Sweden. The experiment took the form of a partial reform of the Swedish employment protection law in 2001, which made it possible for firms with ten employees or less to exclude two workers from the last-in-first-out rule when dismissing personnel. Our results show that the reform positively affected the decision to hire groups that were relatively close to insider status but that the reform had no effect on decisions to hire non-western immigrants. We also find that the reform mainly increased net-hires of natives that were unemployed or outside the workforce, showing the importance of considering the labor market status of individuals when evaluating the effects of changes in employment protection legislation.

Keywords: Employment protection; immigration; integration; hires; separations

JEL-codes: D22, J15, J21, J23, J61, J82, K31, L25

I HUI Research, SE-103 29 Stockholm, Sweden; Department of Economics, Dalarna University, SE-781 88 Borlänge, Sweden.
II School of Business, Örebro University, SE-701 82 Örebro, Sweden.
1. Introduction

The Swedish employment rate is currently approximately 76.8 percent, far above the OECD average of 67.4 percent (OECD, 2017), but the picture looks more problematic when outcomes for marginalized groups are considered. Most notably, unemployment among the foreign-born is more than three times as high as among native-born Swedes (SCB, 2017). This situation is not unique to Sweden, as foreign-born individuals have lower labor market participation rates than natives across Europe (Eurostat, 2015).

The large discrepancy in employment between natives and immigrants is deeply problematic. Exclusion from the labor market is an important cause of social exclusion (Lindbeck and Snower, 2001), and the resulting detachment from social relations can prevent people from fully participating “in the normal, normatively prescribed activities of the society in which they live” (Silver, 2007). Furthermore, the position of immigrants in the labor market is decisive for a country in realizing the potential benefits of immigration. For example, simulations by Ekberg (2009) show that, while the actual employment rate of immigrants in Sweden in 2006 was 56 percent, at an unemployment rate of 72 percent or higher, immigrants would have become a net gain for Swedish public finances (c.f. Ruist, 2013; Bergh, 2014).

Many explanations of the native-immigrant labor market gap have merit – for example, discrimination (Carlsson and Rooth, 2007), compressed wage structures (Edin and Topel, 1997; Davis and Henrekson, 1997), and high reservation wages for low-productivity workers (Meyer, 1990; Roed and Zhang, 2004). In this paper, we consider another potential explanation: employment protection legislation. While such legislation can be a source of safety and protect employees from unfair dismissals, strict employment protection legislation also benefits people with an already strong position in the labor market at the expense of marginal groups. The legislation thus works as a regressive distribution mechanism in the labor market (Skedinger, 2010).

Because labor market institutions are heavily socially embedded, empirical approaches to understanding their effects must be characterized by good identification. While many studies have exploited natural experiments in the guise of partial labor market reforms to study a host

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1 Share of working age population, Q1 2017.
2 Unemployment among the foreign-born population aged 15-74 was 15.7 percent in May 2017, compared with 5.0 percent among the Swedish-born population.
of labor market outcomes, we know of no such study that examines in detail how the labor market position of immigrants is affected by changes in employment protection laws. The purpose of this paper is to remedy this gap in the literature by focusing on a reform of the Swedish seniority rules: the so-called last-in-first-out (LIFO) rule, stipulating that seniority should apply when there are layoffs due to a labor shortage.

Insider-outsider theory (Lindbeck and Snower, 1988, 2001) provides a framework for understanding how labor market legislation can cause problems for immigrants entering the labor market. It stipulates that when firms replace insiders (incumbent workers in the labor market) by outsiders, they incur labor turnover costs. These may be costs of firing, hiring and training workers, but they can also arise from attempts by insiders to protect themselves against competition from outsiders through union organization, collective agreements, legislation and other measures. Because turnover costs are partly borne by the employer, such costs provide insiders with market power. This enables insiders to bargain for wages that are above the market-clearing rate, resulting in involuntary unemployment for outsiders.

Insider-outsider theory leads to the expectation “that the frequency and duration of unemployment spells for typical outsider groups such as young workers, women and some minorities will be comparatively high in countries where insiders enjoy relatively high job security and strong market power” (Lindbeck and Snower, 2001:182). The LIFO rule’s emphasis on seniority leads us to believe that this rule is an example of the type of labor market legislation that increases the market power of insiders and prevents immigrants from entering the labor market.

As mentioned, LIFO stipulates that the most recently employed are to be let go before more senior employees when layoffs occur due to a labor shortage. In 2001, LIFO was reformed to allow firms with ten employees or less to exclude two employees from the priority list. The reform was quite unexpected, as it was the outcome of an unusual collaboration between the center-right alliance and the Green party, neither of which were in power at the time. Furthermore, the partial nature of the reform makes it possible to construct reasonable

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3 Researchers using such natural experiments in other countries have studied the effect on job flows (Kugler, 2004; Autor et al., 2007; Bauer et al., 2007; Martins, 2007), employment probabilities of unemployed individuals (Kugler and Saint-Paul, 2004; Nicholson and North, 2004), the overall employment level (Miles 2000; Kugler and Pica 2003; Autor et al 2004, 2006; Verick 2004; Schivardi and Torrini, 2008), wages (Friesen, 1996; Leonardi and Pica, 2007; Schivardi and Torrini, 2008), firm productivity (Autor et al., 2007; Martins, 2007), and work absence (Riphahn, 2004; Engelandt and Riphahn, 2005; Ichino and Riphahn, 2005).
treatment and control groups. For these reasons, the reform has previously been exploited as a natural experiment in relation to work absence (Lindbeck et al., 2006; Olsson, 2009), job flows (von Below and Skogman Thoursie, 2010), labor productivity (Bjuggren, 2015), and firm growth (Bornhäll et al., 2017). We believe that the reform is also suitable as a natural experiment for our line of inquiry and use it in conjunction with a matched employer-employee database provided by Statistics Sweden (SCB).

More specifically, we examine the effects of the 2001 LIFO reform on hires and separations of immigrants, using a difference-in-differences framework, taking care to carefully specify our underlying identifying assumptions as well as the generalizability of our results. We limit noise in the data by limiting the sample to cover only one year before and one year after the reform.

Von Below and Skogman Thoursie (2010) previously considered the effect of the LIFO-reform on hires and separations of immigrants. Defining immigrants as those individuals who were born outside the Nordic countries, they found a modest positive effect of the reform on hires but not on separations. Immigrants are, however, a very heterogenous group, with different labor market outcomes. The vast majority of immigrants from non-western countries are, for example, refugees or relatives of refugees (Calleman & Herzfeld Olsson, 2015), and they have in general the greatest difficulties establishing themselves in the Swedish labor market (Lundborg, 2013). We therefore distinguish between western and non-western immigrants when estimating the effects of the LIFO-reform on hires and separations.

A liberalization of the LIFO rule furthermore lowers the incentives of workers to obtain seniority in the workplace. This could lead to an increase in voluntary job changers, as the cost of moving to another firm is reduced. We therefore distinguish between the effects of the reform on job flows to and from unemployment, on flows between firms, and on flows into and out of the workforce, whereas von Below and Skogman Thoursie (2010) only investigate the effect of the reform on total hires and separations.

Our results show that hires from unemployment and from outside the workforce increased because of the reform. We find no effects on separations, which means that the possibility of excluding two workers from the LIFO rule reduced unemployment and expanded the workforce. The positive effects of the reforms were, however, limited to native workers, while

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4 We use the term “separations”, as we cannot distinguish between voluntary job changers and involuntary firings.
we find no effects on the labor market position of immigrants. The effects of the 2001 LIFO reform thus appear to depend on the relative insider-status of employees, so that groups of employees that are closer to being insiders benefit more from less strict employment protection legislation than groups that are further from being insiders.

2. Employment protection legislation

2.1. Theoretical rationale and consequences

Employment protection legislation can be theoretically rationalized on the grounds of efficiency, e.g., as insurance against dismissal because of market imperfections (Pissarides, 2001). In incomplete insurance markets, the employer can have an incentive to renege on an agreement of compensation in periods when the employee is less productive. In that case, protection legislation can be less costly for employees than lawsuits. A possible objection to this interpretation is that legislation then satisfies an insurance need that it may help create in that fewer hires and longer unemployment spans may result in adjustment difficulties for fired personnel (Saint-Paul, 2007). Furthermore, collective dismissals can have negative social consequences and costs for parties other than those who are fired, e.g., in small cities with weak labor markets. According to this argument, taxpayers will carry such costs unless there is legislation (Skedinger, 2010).

Other explanations emphasize political and institutional factors rather than efficiency. For example, Botero et al. (2004) provide evidence that legal traditions are the most important explanatory factor in country differences regarding employment protection, the regulation of collective agreements, and the generosity of welfare systems. In political economy models, such as the aforementioned insider-outsider model, different interest groups attempt to affect the political system. Employees with a relatively strong position in the labor market – insiders – may push for stricter employment protection legislation and are typically a larger group than marginalized groups – outsiders – and capital owners who may oppose said legislation due to decreased recruitment probabilities and profits (Saint Paul, 2002).

To summarize, if employment protection is an effective response to market imperfections, then employment protection is efficiency enhancing. If, by contrast, legislation is a result of group interests or legal traditions, efficiency is less likely to increase and may even decrease. That said, these explanations are not mutually exclusive; both may help explain the emergence of employment protection legislation (Skedinger, 2010).
From a theoretical perspective, regulated employment protection increases the employer’s cost of adjusting the size and composition of the workforce. Increased costs of firing also decrease the employer’s willingness to recruit. Thus, labor turnover decreases, and as a consequence, average employment and unemployment spans should increase. The net effect on employment and unemployment is, however, theoretically indeterminable, as it depends on whether the hiring or separation flow dominates (see e.g., Bertola, 1999). Furthermore, employment protection is likely to affect the composition of the employed and the unemployed (Bertola et al., 2008). For example, benefits such as terms of notice and severance pay usually increase with employment time. Such benefits indirectly increase the layoff risk for people with shorter employment time. The same effect can be expected from seniority rules based on employment time.

Employers may, furthermore, be less likely to hire new employees, who are considered riskier when employment protection laws are strict (Kugler and Saint-Paul, 2004). They should prefer to employ people who already have jobs, as such workers are potentially less risky compared with individuals with little work experience or a foreign education whose value is difficult to verify. These factors suggest that marginal groups in the labor market, such as young people, immigrants, and the long-term unemployed, will be disadvantaged relative to other groups when employment protection is strong. In this respect, it has frequently been argued that the Swedish LIFO rule makes it costlier to revoke a bad recruitment decision (Skedinger, 2010).

However, the fact that theory predicts that a specific labor market regulation affects groups of individuals differently also implies that a reform of the regulation should affect groups differently. What happens when reform occurs is in fact that the border between insiders and outsiders expands outwards, so that people who were previously relatively close to being insiders now become insiders. The effect of the reform on labor market outcomes should be more modest, the further away from insider status a group/individual is at the outset.

2.2 Previous empirical literature

Skedinger’s (2010) overview of the empirical literature suggests that employment protection works as intended in that the risk of being fired decreases with stricter legislation. More generally, it appears that employment turnover decreases with more stringent employment
protection (Kugler and Pica, 2008).\(^5\) Furthermore, as hypothesized, employment protection appears to create higher thresholds in the labor market for people who already have a weak connection to it (Skedinger, 2010).

Researchers, using aggregated cross-country data, usually find that stricter employment protection decreases employment (or increases unemployment) among young people and to some extent women (Bassanini and Duval, 2006; Bertola et al., 2008; Botero et al., 2004; Feldmann, 2003; Heckman and Pagés-Serra, 2000; Allard and Lindert, 2006; OECD 2004; Scarpetta, 1996; Skedinger, 1995).\(^6\) There do not appear to be any studies of the effects on other marginal groups, e.g., immigrants (Skedinger, 2010).

Cross-country studies, using disaggregated data, usually rely on better identification and generally find that stricter employment protection legislation leads employers to make more careful hiring evaluations\(^7\) (Daniel and Siebert, 2005; Pierre and Scarpetta, 2004). Notably, Kahn (2007) finds that stricter rules decrease employment among young people and immigrants relative to other groups. In addition, given that one is employed, the probability of having a temporary job is particularly high among women and immigrants (Skedinger, 2010). It should be added, however, that cross-country studies usually suffer from omitted variable problems, as unobservables that are correlated with the independent variables may be the true causal factors. Furthermore, it is difficult to compare legislations across countries (OECD, 2004).

Country-specific studies are usually better placed in terms of identification and often rely on some form of natural experiment, exploiting labor market reforms that treat groups of individuals or groups of firms differently to compare outcomes between “treated” and “control” groups. Additionally, the ability to control for country-specific conditions is greater. The results of such studies suggest that stricter employment legislation negatively affects employment of the previously unemployed (Kugler and Saint-Paul, 2004; Nicholson and North, 2004), the low

\(^5\) At the same time, stricter legislation appears to increase the costs associated with adjusting to structural changes, which impacts productivity and growth (Autor et al., 2007; Hopenhayn and Rogerson, 1993; Saint-Paul, 1997, 2002).

\(^6\) There are, however, exceptions where the effect on young people is either nonexistent (OECD, 1999) or even beneficial (Amable et al., 2007).

\(^7\) Daniel and Siebert (2005) find that the average education level of new employees increases in countries with stricter legislation. Pierre and Scarpetta (2004) obtain results suggesting that firms in countries with stricter legislation regarding permanent employment use more workplace education and more temporary employment contracts.
educated (Montenegro and Pages, 2004; MacLeod and Nakavachara, 2007), women relative to men (Kugler and Pica, 2006), and the young (Montenegro and Pages, 2004).

Studies evaluating the effects of the Swedish LIFO reform point in a similar direction. Lindbeck et al. (2006) and Olsson (2009) find that small firms became more prone to employ people with a history of absence due to illness as a result of the reform. This may be because the reform made them more willing to take risks when recruiting, as the costs of firing have decreased (Skedinger, 2010). Bornhäll et al. (2017) find that employment growth increased among firms with 5-9 employees. They also show that firms with 10 employees, i.e., just below the threshold, became less likely to grow after the reform, indicating that the new threshold created by the reform acted as a firm-growth barrier.

The study most similar to ours is von Below and Skogman Thoursie (2010). They find that both hires and separations increased by approximately 5 percent in small firms in response to the Swedish reform, while net employment was unaffected. They also restrict their sample to immigrants, defined as individuals born outside the Nordic countries. They find a modest effect of the reform on hires of immigrants but not on separations. As mentioned, however, there are reasons to expect that the effects of the reform differed substantially across the heterogeneous group of immigrants to Sweden. There is therefore a need for a more detailed examination of how the reform affected the labor market position of immigrants, taking care to distinguish between different immigrant groups and their labor market status before being hired and after being separated from the firm.

3. The natural experiment

3.1. Sweden’s EPL and LIFO

Employment protection in Sweden is relatively encompassing compared with other countries. Sweden ranks 11th among the 42 countries covered by the OECDs employment protection index (OECD 2013). Regulations are stricter than average in regard to regular employment and collective dismissal but more liberal than average as regards temporary employment (Skedinger, 2008: 34).

The current Swedish employment protection act (EPA) was adopted in 1982 (SFS 1982:80). While the default contract, according to the act, is a permanent one, employers are allowed a trial period of up to 6 months before offering a permanent contract. Temporary contracts may
also be allowed if they are justified by the nature of the work. In 2007, the length of such contracts was extended to up to two years.\textsuperscript{8}

According to Swedish law, a firm cannot fire a permanent employee without just cause, which exists when there is a labor shortage or in the case of personal reasons, such as worker misconduct. Labor shortage is the most common reason for the LIFO rule to come into play. The rule is a rather uncommon feature in labor market legislation; seniority rules are seldom legislated in other OECD countries, while rules of precedence are more common. In France, Italy, Mexico and the Netherlands, the law requires that some form of seniority rules can or should be used in relation to collective dismissals, but this is often combined with other criteria (OECD, 1999; Skedinger, 2008:36).

According to LIFO, firms are required to dismiss the most recent employee first in cases of redundancies (Skedinge, 2010). In practice, this means that the employer must comply with an established list that ranks individuals based on their accumulated tenure within the firm. If two workers have accumulated the same tenure, priority is given to the elder. The rules are analyzed in detail by Calleman (2000) and summarized by von Below and Skogman Thoursie (2010) and Skogman Thoursie (2009).

3.2 The LIFO reform

The environmentalist party and the non-socialist opposition made the first proposal for what was to become the 2001 reform on April 28, 1999. Sweden was ruled by a Social Democratic minority government at the time, and collaboration between the environmentalists and the non-socialists was uncommon. The improbability of such cooperation makes it reasonable to assume that the reform was not anticipated until at least late in 2000 (Lindbeck et al., 2006). This provides us with a form of exogenous variation in the job security experienced by workers.

The reform that passed the parliamentary vote on October 11, 2000 was, moreover, partial in nature, as it softened LIFO only for small firms in that firms with ten or fewer employees were allowed to retain two employees, regardless of their seniority. It has been recognized that such a partial reform creates, in a natural way, appropriate control groups that can be assumed to be unaffected by the reforms and thus can be used to estimate their causal effects (Skedinger, 2008:14).

\textsuperscript{8} As in most other European countries, Swedish reforms of employment protection legislation have been rather modest and have mainly concerned conditions of temporary employment. In addition to the 2001 reform, rules pertaining to permanent employment have in large part been left intact (Skedinger, 2008: 14).
The reform applies to the firm level, so the exemption is independent of the number of establishments that a firm has. At the same time, temporary and permanent employees have equal weight when the size of the firm is determined.

The fact that the 2001 reform has remained in place has also made it possible for researchers to evaluate it (Skedinger, 2008: 43, 89). In particular, the reform has been exploited as a natural experiment, e.g., in relation to job flows (von Below and Skogman Thoursie, 2010), labor productivity (Bjuggren, 2015), absence from work (Lindbeck et al., 2006; Olsson 2009), and firm growth (Bornhäll et al., 2017). We believe that the reform can be used as a natural experiment in our line of inquiry as well.

3.3. Identification

Natural experiments are ideal for identifying causal effects (Angrist and Pischke, 2009): the idea is that they mimic a randomized trial by changing the variable of interest while keeping all control variables of interest constant (Angrist and Lavy, 1999). Researchers who take this approach assume that firing costs are a function, e.g., of passing a size threshold at which the firm becomes subject to different legislation (Skedinger, 2008: 73-74).

We use a difference-in-difference approach and utilize the variation in the employment protection legislation across firm size and time to identify the effect of LIFO on the labor market position of immigrants. We classify firms that are just below the size threshold of 10 employees as in the treatment group. Our main identifying assumption is that firms just above that size threshold resemble firms in the treatment group enough to represent a valid control group.

We must take care to assess the validity of this assumption. For example, ample empirical evidence suggests that wages are consistently higher in larger firms, even after controlling for observable worker characteristics and other job attributes (e.g., Oi and Idson, 1999). Garen

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9 A first attempt at reform occurred in 1994 when the then center-right government made it possible for employers to exempt two people from LIFO in cases of work shortages. However, the succeeding Social Democratic governments eliminated this possibility in the following year (Skedinger, 2008:41).

10 Researchers have analyzed the removal of an exemption from employment protection legislation for firms with less than 15 employees in Italy in 1990 (see Kugler and Pica, 2003, 2008; Garibaldi et al., 2004; Schivardi an Torrini, 2004; Cingano et al., 2010), relaxation of dismissal protection for small firms in Germany (Bauernschuster, 2009), and less restrictive employment protection legislation for firms with 20 employees or less (Boeri and Jimeno, 2005; Martins, 2007).
(1985) and Kremer (1993) develop theoretical models that explain the systematic sorting of more productive workers to larger employers as an efficiency-enhancing outcome in economies with heterogeneous, imperfectly substitutable labor. Small firms, meanwhile, may favor less specialized labor if recruitment and training costs are lower than the costs of recruiting specialized labor. Conversely, individuals with a weak labor market position may see small firms as a springboard to enhance their position and learn specific skills (Coad et al., 2014).

There are also several ways in which firms can circumvent the LIFO rule. While most other parts of EPA are compulsory (Edström, 2006), collective agreements between unions and employers can be used to deviate from the rules regarding temporary employment and the LIFO rule (Rönnmar, 2006). Such deviations appear to be common: according to an article in Dagens Arbete (2010), unions agree to exempt workers from LIFO in 75 percent of cases (c.f. Calleman, 2000). Deviations can go in a more or less strict direction compared with existing legislation (c.f. Ahlberg et al., 2006; Storrie, 1995), but the limited evidence that exists suggests that deviations in most cases have been in a less strict direction (Skedinger, 2008: 109). Nevertheless, it is unclear how dispositivity with respect to LIFO affects marginalized groups (Skedinger, 2008: 23), although deviations from LIFO probably benefit people with shorter employment time than other employees. Apart from dispositivity and the possibility of negotiating with unions, firms can also circumvent LIFO by using fixed- or short term-contracts, to which LIFO does not apply. In addition, firms can hire individuals through temporary work agencies.

We have no reliable way of controlling for whether control and treatment groups differ systematically with respect to their ability to circumvent LIFO rules. It has, however, been argued that it is easier for larger firms to take advantage of dispositivity, as they have greater bargaining power with respect to unions (Henrekson and Johansson, 1999). It is thus likely that the effects of the 2001 reform of the Swedish employment protection legislation are different for small and large firms.

The aforementioned issues highlight the importance of carefully selecting treatment and control groups, so that they are comparable. We therefore choose to exclude from our analysis firms with less than 7 employees and firms with more than 14 employees. Furthermore, as shown by Bornhäll et al. (2017), firms with 10 employees have an incentive to remain beneath the threshold by not hiring additional personnel, whereas firms with 11 employees face the

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11 See also the discussion on how immigrants are affected by dispositivity in Calleman (2003).
opposite problem in terms of separations. We therefore exclude firms that had 10-11 employees in 2000 from the analysis.\textsuperscript{12} As a result, our final treatment group consists of firms that had 7-9 employees in the period before the reform, whereas the control group consists of firms with 12-14 employees.

Over time, firms can expand or reduce their staffs and thereby move in and out of treatment. We use data for only one year before and one year after the reform to counter this problem. Furthermore, by using only the year 2001 as the post-reform period, noise is reduced to a minimum (more noise gets introduced in the data for each year after the reform; see Mian and Sufi, 2012). This means that we estimate the short-term reform effect with high accuracy.

An additional concern is possible endogeneity of the treatment status, i.e., that firms selected themselves into the treatment group prior to the implementation of the reform. The problem with non-random selection into or out of treatment is handled in many studies by restricting the analysis to firms that remain in the same size class for a number of years before and after reform (Skedinger, 2008: 97). However, as noted by Lindbeck et al., (2006), these concerns are mitigated by the fact that the reform was unexpected and that there were uncertainties concerning where the size threshold would be placed before the reform passed the parliamentary vote.

Nonetheless, it is possible that some of the effects of the reform had already materialized in 2000. In anticipation of the reform, some firms may have become willing to take greater risks when hiring, and some workers may have decided to leave their jobs (for obvious reasons, the effects on firms’ propensity to fire could not have occurred until 2001). As an additional robustness check, we therefore perform a placebo tests by estimating the treatment effects of a hypothetical reforms before the true reform occurred. If “false” difference-in-difference-coefficients in years prior to the reform are not statistically significant, this mitigates the endogeneity concerns while also strengthening the assumption of parallel trends.

4. Data

Our analysis is based on a matched employer-employee dataset provided by Statistics Sweden (SCB). We thus use both individual- and firm-level data in our estimations. We use firm-level data because the exemption from the LIFO rule was applied at the firm level and not at the

\textsuperscript{12} Alternative model specifications show that the estimates are not sensitive to including firms with 10 or 11 employees. The estimated effects become somewhat smaller but with no qualitative differences.
establishment level. Only active firms with 7-9 or 12-14 employees in 2000 were included in the sample. The firm-specific data are matched with individual data from LISA\textsuperscript{13}, a longitudinal database that provides yearly information on all inhabitants in Sweden who are at least 16 years old.

When determining the number of employees in order to exclude two workers from the LIFO rule, employees with managerial positions should be excluded. Because we are unable to identify managers in our data, we follow von Below and Skogman Thoursie (2010) and assume that each firm has one manager. The total number of employees is therefore reduced by one for each firm.

A sample description and summary statistics are provided in Table 1. As seen, the final sample contains 16,734 firms, 11,725 in the treatment group and 5,009 in the control group, in the year 2000. The number of firms is reduced to 15,726 in 2001, as some firms exited the dataset at that time. In 2000, 92,202 workers were employed by firms with 7-9 employees and 64,671 by firms with 12-14 employees (Panel A in Table 1).

Worker flows to and from firms in the control group are higher than for firms in the treatment group in 2000 (Panel B in Table 1). This is expected because the average firm in the control group is larger (12.91 employees per firm) than the average firm in the treatment group (7.86 employees per firm) in 2000.

\textsuperscript{13} Longitudinell integrationsdatabas för sjukförsäkrings- och arbetsmarknadsstudier.
Table 1. Sample description and summary statistics

<table>
<thead>
<tr>
<th></th>
<th>Treatment group</th>
<th></th>
<th>Control group</th>
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<tbody>
<tr>
<td></td>
<td>2000</td>
<td>2001</td>
<td>2000</td>
<td>2001</td>
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<tr>
<td><strong>Panel A: Sample description.</strong></td>
<td></td>
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<td></td>
<td></td>
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<tr>
<td>No. firms</td>
<td>11,725</td>
<td>11,014</td>
<td>5,009</td>
<td>4,712</td>
</tr>
<tr>
<td>No. workers</td>
<td>92,202</td>
<td>86,961</td>
<td>64,671</td>
<td>61,503</td>
</tr>
<tr>
<td>Swedish</td>
<td>78,937</td>
<td>74,815</td>
<td>55,639</td>
<td>52,940</td>
</tr>
<tr>
<td>2nd gen immigrants</td>
<td>853</td>
<td>745</td>
<td>818</td>
<td>725</td>
</tr>
<tr>
<td>Western</td>
<td>2,705</td>
<td>2,416</td>
<td>2,128</td>
<td>2,081</td>
</tr>
<tr>
<td>Non-western</td>
<td>798</td>
<td>747</td>
<td>468</td>
<td>521</td>
</tr>
<tr>
<td><strong>Panel B: Mean values of dependent variables. Standard deviations in parentheses.</strong></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Total hires</td>
<td>2.50 (1.90)</td>
<td>2.18 (3.49)</td>
<td>4.05 (2.85)</td>
<td>3.57 (4.22)</td>
</tr>
<tr>
<td>Hires from firm</td>
<td>1.48 (1.48)</td>
<td>1.39 (2.76)</td>
<td>2.47 (2.19)</td>
<td>2.32 (3.30)</td>
</tr>
<tr>
<td>Hires from unemployment</td>
<td>0.28 (0.60)</td>
<td>0.18 (0.48)</td>
<td>0.42 (0.78)</td>
<td>0.30 (0.77)</td>
</tr>
<tr>
<td>Hires from outside workforce</td>
<td>0.73 (1.00)</td>
<td>0.60 (1.11)</td>
<td>1.16 (1.42)</td>
<td>0.94 (1.41)</td>
</tr>
<tr>
<td>Total separations</td>
<td>2.01 (5.25)</td>
<td>2.16 (1.78)</td>
<td>3.35 (9.62)</td>
<td>3.44 (2.64)</td>
</tr>
<tr>
<td>Separations to firm</td>
<td>1.43 (4.59)</td>
<td>1.42 (1.42)</td>
<td>2.41 (7.36)</td>
<td>2.32 (2.16)</td>
</tr>
<tr>
<td>Separations to unemployment</td>
<td>0.12 (0.41)</td>
<td>0.15 (0.43)</td>
<td>0.20 (1.27)</td>
<td>0.24 (0.57)</td>
</tr>
<tr>
<td>Separations outside of workforce</td>
<td>0.46 (0.98)</td>
<td>0.59 (0.85)</td>
<td>0.73 (1.81)</td>
<td>0.88 (1.09)</td>
</tr>
</tbody>
</table>

Data for the employment status of individuals are obtained from the RAMS\textsuperscript{14}-database, provided by Statistics Sweden. According to the International Labour Organization (ILO), an individual is defined as employed if he/she works at least one hour per week in November. Because hours worked are not included in the RAMS database, Statistics Sweden instead uses information about paid gross wages to identify employed individuals.

Data on unemployment are gathered from the Swedish public employment office’s (Arbetsförmedlingen) database, HÄNDEL. In our data, an individual is classified as unemployed if that person is registered with the employment office on the last day of November of a given year. The fact that both employment and unemployment are measured in November gives us a snapshot of the labor market at that time. Individuals who are neither employed nor

\textsuperscript{14} SCB:s (Statistics Sweden) registerbaserade arbetsmarknadsstatistik
unemployed are classified as not in the workforce. The use of two different registers means that approximately 1.3 percent of individuals are classified as both employed and unemployed by Statistics Sweden. In this study, we choose to classify these individuals as employed.

Each employed individual in our sample is linked with one’s primary employer in November. This allows us to follow individuals and firms over time, thereby making it possible to analyze worker flows between firms, between firms and unemployment, and into and out of the workforce. One limitation of yearly data is that we have no information about an individual’s occupation during the year between consecutive Novembers. Individuals who are classified as job changers can therefore have an unemployment spell between two periods of employment. This means that we cannot distinguish between voluntary and involuntary separation in the data.

The distinction between outcomes for different immigrant groups is a central part of our analysis, and we have information on regions of birth of individuals in our dataset. Immigrants are a highly heterogeneous group, with different probabilities of entering the labor market. For example, Fleischmann and Donkers (2010) find that immigrants from more politically stable, freer, more highly developed and wealthier societies are less often unemployed in European labor markets. Immigrants from Islamic countries have higher rates of unemployment, while those from Western European countries are less likely to be unemployed.

Data on the regions of origin of all immigrants are used to distinguish between different types of immigrants in the empirical analysis. More specifically, we divide people in the sample into the following four groups, according to origin: (i) Native Swedes; (ii) Second generation immigrants, i.e., people with two foreign-born parents who were themselves born in Sweden; (iii) Western immigrants from Europe, North America, or Oceania; (iv) Non-western immigrants from Africa, Asia, and South America. The largest group in our sample is natives Swedes, at 85.6 percent, followed by western immigrants, at 3.0 percent, second generation immigrants, at 1.0 percent, and non-western immigrants, at 0.9 percent. The latter group of immigrants is of special interest, as this group consists mostly of refugees or relatives of refugees who, in general, have the greatest difficulty securing employment (Calleman & Herzfeld Olsson, 2015; Lundborg, 2013).

5. The model

5.1. Parallel trends
A difference-in-difference approach is built on the assumption of parallel trends in the hypothetical case of no treatment. Because it is impossible to compare two counterfactual trends, our best option is to examine the trends before treatment. Figure 1 shows the trends in hires, measured as the average number of added employees, in the treatment group and control group over the time period 1994-2004. The figure lends support to the idea of parallel trends in the pre-reform period, even if the trend during the final years, 1999-2000, is somewhat steeper for the control group.

**Figure 1.** Average trends in hires in control and treatment groups, 1994-2000. Figure from Bornhäll (2017).
The treatment and control groups also exhibit similar trends in terms of separations, measured as the average number of workers separated from the workplace (Figure 2). As in the case of hires, the treatment and control groups both exhibit negative trends in the post-reform period.

5.2. Hiring and separations

We proceed by performing a difference-in-difference analysis. This will give us an estimate of the treatment effect on the treated (TT), as we evaluate the effect on small firms of being exempt from LIFO. Following previous work (e.g., Bjuggren, 2015; Bornhäll et al., 2017), we formulate a linear difference-in-difference model of the type,

\[ Y_{it} = \alpha + \beta_1 D_t + \beta_2 D^g + \beta_3 (D_t \ast D^g) + \epsilon_{it} \]  

where \( Y_{it} \) is the number of hires or separations for firm \( i \) at time \( t \). \( D_t \) is a time dummy equal to one after the reform. It acts like a time-invariant variable in a fixed-effects model by filtering out any time-specific trends in the data. \( D^g \) is a dummy equal to one for firms in the treatment group, i.e., those with 7-9 employees. It controls for all differences that are fixed between the
treatment and the control group. The variable of interest is the interaction between the treatment group dummy $D^g$ and the post-reform dummy $D_t$. $\beta_3$ is thus our difference-in-difference-estimator, capturing the causal effect of the reform under the assumption that small and large firms exhibit parallel trends in the outcome variable in the counterfactual case of no treatment.

All coefficients should be interpreted as the result of the treatment group compared with the control group and the pre-reform period. By using a difference-in-difference model in a natural experiment setting, we can exclude any explanatory variable that is not correlated with both the group identification and the timing of the reform.

We estimate Equation (1) several times by letting the dependent variable vary from one estimation to another. In the first estimation (henceforth, A1), $Y_{it}$ equals the number of individuals hired, whether previously employed or not. The estimated coefficient $\beta_3$ in this specification can be seen as the total effect of the reform on a firm’s hiring decision. We then perform separate estimations in which $Y_{it}$ equals the number of hires of previously employed individuals (A2), the number of hires of previously unemployed individuals (A3), and the number of hires of individuals not previously in the workforce (A4). This means that we can investigate whether the effect of the reform of the Swedish LIFO-rule on hires depends on the labor market status of individuals.

Corresponding estimations are then performed for separations. First, we estimate the effect on total separations (B1), followed by separations to other firms (B2), separations to unemployment (B3), and separations of workers leaving the workforce (B4). This is of particular interest because it allows us to test whether former employees became unemployed as a result of separations and by extension because of the reform or because they found work elsewhere. Finally, we estimate the net effect of the reform by using the difference between hires and separations as the dependent variable, divided into the same categories as above (C1-C4).

In addition, for each of the twelve estimations, we restrict the sample to only: (i) Natives (born in Sweden); (ii) second-generation immigrants; (iii) western immigrants; and (iv) non-western immigrants. In total, we perform 48 estimations to determine whether the effects of the LIFO reform on hires and separations depend on individuals’ region of birth and labor market status.
Due to the large number of estimations, the estimated $\beta_3$-coefficients are presented graphically, together with 95 percent confidence intervals.\textsuperscript{15}

The reason for changing the dependent variable in this manner is to understand how the LIFO reform affected the employment prospects of different groups. Following insider-outsider theory, we expect that the effect should depend on the relative insider-outsider status of each group, with the effect positive and significant for individuals who are relative insiders. Employed natives have higher insider status than unemployed natives, who in turn have higher insider status than unemployed non-western immigrants. When the reform occurs, the boundary between insiders and outsiders expands outwards. Groups closer to being insiders at the outset should see a greater benefit from the reform than groups further from being insiders. We therefore expect firms’ decisions to hire, for example, unemployed Swedes to be more positively affected by the reform than their decisions to hire unemployed immigrants from outside Europe.

5.3. Results

First, we analyze how the LIFO reform affected hires among treated firms (7-9 employees). The estimated effect for each group is presented in Figure 3, together with its associated confidence interval (95 percent). We observe that the reform increased hires from other firms and hires from outside the workforce.

The reform effect is largest for Swedish-born individuals and smaller, or insignificant, for foreign-born workers. In total, the reform led to an increase in hires of Swedish-born workers of 0.130 workers per firm, so that, on average, every eighth firm added a new employee. We also find an increase of 0.024 hires of second generation immigrants, which implies that only one in approximately forty firms added a second-generation immigrant due to the reform. We find no increase in hires of individuals from the other groups.

\textsuperscript{15} Tables with full regression results are available from the authors upon request.
Panel A2 shows worker flows between firms. We find that the 2001 reform had no effect on hires from other firms. Estimates for all groups are very small, with confidence intervals overlapping zero. However, the reform increases the recruitment of Swedes who were previously unemployed (A3). On average, firms with 7-9 employees hired 0.025 more unemployed Swedes than they would have in the absence of reform.

Finally, panel A4 shows that the reform mostly affected hires of individuals not previously in the workforce. Firms in the treatment group hired, on average, 0.076 workers, or one worker per thirteen firms, more individuals than in the control group and the pre-reform period. Once again, there is no significant effect on hires of second generation immigrants or individuals born outside Sweden.

Next, we analyze worker separations. As before, the regression results for the difference-in-difference model are presented in four graphs (Figure 4). The results in panel B1 show that the reform had no effect on total separations among firms with 7-9 employees. The same is true for worker flows to other firms and to unemployment. The estimates for separations to
unemployment and to outside the workforce show a slight decrease. These effects are, however, small and not statistically significant.

**Figure 4.** Difference-in-difference estimates of the reform’s effect on separations, point estimates and 95 percent confidence intervals.

By combining the results for hires (Figure 3) and separations (Figure 4), we calculate the net effect of the reform (Figure 5). In the upper-left panel (C1), we observe that the reform did not have any effect on overall net-hires. The magnitudes of the point estimates are close to those for hires but with higher variances. Because the reform affected neither hires nor separations from/to other firms, the net effect is also insignificant (C2). However, we find a positive outflow from unemployment into employment for individuals born in Sweden (C3), significant at the 90 percent level. The estimated net effect is 0.037, so that recruitment by treated firms reduced unemployment by 408 individuals. However, the net flows of foreign-born workers to and from unemployment were unaffected by the reform.

Finally, we examine the net effects on net-hires of individuals from outside the workforce, where the reform had its largest effect (C4). The reform led treated firms to hire, on average, 0.105 more Swedish workers and 0.018 more second generation immigrants than would be
expected in the absence of the reform. In total, 1,360 more workers entered the workforce for employment at a treated firm, due to the reform.

**Figure 5.** Difference-in-difference estimates of reform effect on net-hires, point estimates and 95 percent confidence intervals.

To check the robustness of the results, we also perform placebo-estimations of several hypothetical reforms. The treatment and control groups are defined in the same way, but we use data from 1997-2004. Only two years are used for each estimation, so that the first placebo-estimation is conducted using 1997 and 1998, the second estimation is conducted using 1998 and 1999, and so on. Note that this means that the estimations for hypothetical reforms after 2001 are based on two actual treatment years. These estimates can therefore be used to interpret how any reform effect changes over time. Due to limitations in the data, it is not possible to analyze movements into and out of unemployment before 2000. The placebo estimations are therefore based on the total reform effect on hires and separations.

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16 All estimations have also been performed with regional and industry fixed effects included, with no significant changes to the estimates.
The results for hires show no significant effects prior to the true reform year. In comparing 2001 with 2002, we find an effect similar in magnitude to that of the initial reform effect (Figure 6, Panel D1), implying that hires continued to increase at the same rate for the first two years after the reform. For 2003 and 2004, the effect stops increasing and levels off at the higher level of hires. Note that the results for the true reform year (2000 versus 2001) strengthen when we compare the combined effect for all groups with the effect for each group separately.

**Figure 6.** Difference-in-difference estimates of the reform effect on hypothetical reform years, 1997-2004, point estimates and 95 percent confidence intervals. Figure from Bornhäll (2017).

For separations, the estimates show a negative effect for a hypothetical reform in 1999 (Panel D2). It is not clear whether this is a spurious result due to noise in the data or whether the model picks up some change in firms’ behavior. All other years show insignificant results.

**6. Discussion and conclusions**

Low labor market participation among immigrants should be cause for concern for policymakers in Europe, notably because it could result in social exclusion. Insider-outsider theory suggests that the difficulties for immigrants in establishing themselves in the labor
market are related to their outsider status, with insiders taking advantage of labor market turnover costs and bargaining for wages above the market-clearing rate. Labor market turnover costs include costs of firing, hiring and training but also arise from attempts of insiders to protect themselves against competition from outsiders, for example, through unions, collective agreements, and legislation.

Employment protection legislation is a possible source of market power for insiders, as it may prevent many immigrants from entering the labor market. However, we still lack knowledge of how employment protection legislation affects outsider groups, such as immigrants from non-western countries, and whether employment protection legislation reforms can help unemployed immigrants establish themselves in the labor market.

The Swedish reform of the LIFO rules in 2001 created conditions for a natural experiment, making it possible to draw causal inferences regarding how less strict employment protection legislation influences different outcome variables. Using matched employer-employee data from Statistics Sweden, we investigated whether this reform affected job flows among native Swedes and foreign-born individuals differently. Our analysis was based on a difference-in-difference regression framework that enabled us to assess how the reform affected job flows for treated firms (7-9 employees) compared with a control group of firms (12-14 employees) that were unaffected by the reform.

The results show that the reform increased hires among treated firms (7-9 employees), significantly reduced unemployment levels, and increased worker flows into the workforce. On the other hand, no significant effects of the reform on the labor market position of non-western immigrants were found. The positive employment effects of the reform were mostly limited to Swedish-born individuals. Our results thus suggest that the reform effects depend on the relative insider-status of employees, with groups of employees closer to being insiders seeing greater benefits of less strict employment protection than groups that are further from being insiders.

However, we should emphasize that no labor market group appears to be a clear loser from the reform. This is an important finding for policy makers pondering future liberalization of employment protection laws in Europe; after all, there is an increasing awareness that EPL may be too stringent, and firm-size thresholds below which labor regulations are less onerous (such as the exceptions to the LIFO rule) have been instituted mainly to mitigate the negative effects of the legislation. Arguably, however, such exceptions are of a second-best nature. In fact, they
can be seen as the equivalent of a tax on firm growth, incentivizing firms to remain small. This, however, is not an argument to remove the exceptions but rather to (gradually or immediately) apply them so broadly that they cease to be exceptions. The fact that we find no clear reform losers provides some hope that such liberalizations can be undertaken without too much grief.

Previous studies have indicated that the LIFO reform produced several benefits, such as decreased absence due to illness, increased labor productivity, increased job dynamics and increased employment growth. The positive effects on job flows and employment are supported by our study, but we also show that the reform had no discernable effect on the employment of immigrants. Note, however, that our study is limited to the effects of the reform of 2001. Furthermore, the reform was rather marginal in scope. An important objective of future research would therefore be to determine what a more extensive employment protection legislation reform would mean for different groups in the labor market.

Another limitation of our study is that we are unable to distinguish between voluntary and involuntary separations in the data – we can only observe whether workers had a new job in November of the following year. It is nevertheless possible that workers had longer unemployment spells between their job positions, which may come at a considerable cost to the individual. This is an important question that merits further research.

References


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