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

Anders Bornhäll, Sven-Olov Daunfeldt, Niklas Elert

Abstract. How does employment protection legislation affect the labor market position of immigrants? We attempt to answer 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 the causal relationship between a reform of employment protection legislation and the employment status of immigrants in Sweden. The experiment took the form of a partial reform of the Swedish employment protection legislation 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 closer to insider status, but that the reform had no effect on the decision to hire non-western immigrants. In accordance with previous studies, we also find that the reform increased both hires and separations. However, the increase in separations is explained by the fact that people change jobs; the reform had no effect on separations to unemployment when these are considered separately. Among firms with 5-9 employees, the reform increased employment by 3.36 percent or 5,965 workers over the period 2000-2001.

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

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

The Swedish employment-level is currently about 76.3 percent, far above the OECD average of 66.9 percent (OECD, 2017), but the picture looks more problematic when outcomes for marginalized groups are considered. Most notably, unemployment among the foreign-born is almost four times as high as unemployment among native-born Swedes (SCB, 2016). This situation is not unique for Sweden, with foreign-born individuals having lower labor market participation rates compared to natives across Europe (Eurostat, 2015).

This large discrepancy in employment between natives and immigrants is deeply problematic. To be excluded from the labor market is an important source 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’s possibility to benefit from migration. For example, simulations by Ekberg (2009) show that while the actual employment rate of immigrants in Sweden was 56 percent in 2006, at a rate of 72 percent or higher they would become a net gain for Swedish public finances (c.f. Ruist, 2013; Bergh, 2014).

Many explanations for the native-immigrant labor market gap can be considered to have merit, such as discrimination (Carlsson and Rooth, 2007), compressed wage structures (Edin and Topel, 1997; Davis and Henrekson, 1997), and high reservation wages for low-productive workers (Meyer, 1990; Roed and Zhang, 2004). In this paper we consider another potential explanation: employment protection legislation. Empirical evidence suggests that strict employment protection legislation benefits people with an already strong position in the labor market, at the expense of marginal groups. The legislation hence works as a regressive distribution mechanism in the labor market (Skedinger, 2010).

Since labor market institutions are highly socially embedded, empirical approaches need to be characterized by good identification. While many studies have exploited

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1 Share of working age population, Q2 2016.
2 Unemployment among the foreign-born population aged 15-74 was 15.2 percent in November 2016, compared to 3.9 percent among the Swedish-born population.
natural experiments in the guise of partial labor market reforms to study a host of labor market outcomes\(^3\), we know of no such study that in detail examines how the labor market position for immigrants is affected by changes in employment protection legislation. 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 labor shortage.

The insider-outsider theory (Lindbeck and Snower, 1988, 2001) provides a framework that helps us understand how labor market legislation can cause problems for immigrants in 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 the costs of firing, hiring and training, but can also arise from attempts of insiders to protect themselves against competition from outsiders, through unions, collective agreements, legislation and other measures. Since the turnover costs are in part borne by the employer, they provide insiders with market power. This enables the insiders to bargain for wages higher than 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 it is an example of the type 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 labor shortage. In 2001, LIFO was reformed to allow firms with ten employees or less to exclude two

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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 for 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; Engellandt and Riphahn, 2005; Ichino and Riphahn, 2005).
employees from the priority list. The reform was quite unexpected, since it was the outcome of an unusual collaboration between the center-right alliance and the green party, neither of whom where in power at the time. Furthermore, the partial nature of the reform makes it possible to construct reasonable 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., 2016). We believe that the reform is also suitable as a natural experiment in regards to our line of inquiry, and use it in conjunction with a matched employer-employee database provided by Statistics Sweden (SCB).

More specifically, we examine whether the LIFO reform increased hires and separations of immigrants. This question has previously been considered by von Below and Skogman Thoursie (2010), who defined immigrants as those individuals that were born outside of the Nordic countries. They found a modest positive effect of the reform on hires, but not on separations.

We analyze the effects of the 2001 reform of the Swedish seniority rules on the labor market positions 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 any noise in the data by limiting the sample to cover only one year before and one year after the reform. In contrast to von Below and Skogman Thoursie (2010), we distinguish between western and non-western immigrants, and also the effect of the reform on job flows to and from unemployment, other firms, and outside the workforce.

Using data for 2000-2001, we find that hires from other firms, from unemployment and from outside of the workforce all increased because of the reform. Separations also increased, but this effect is mostly explained by increased job flows to other firms. The reform had no significant effect on separations to unemployment, which means that the possibility to exclude two workers from the LIFO-rule reduced

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4 We use the term separations since we cannot distinguish between voluntary job changers and involuntary firings.
unemployment. In total, we estimate that the reform increased employment by 3.36 percent or 5,965 workers among firms with 5-9 employees.

However, the reform did not affect job flows for non-western immigrants. The effects of the reform thus seem to depend on the relative insider-status of the employees, so that groups of employees that are closer to being insiders benefit more from a less strict employment protection legislation than groups that are further from being insiders.

2. Employment protection legislation

2.1. Theoretical rational and consequences

Employment protection legislation can be theoretically rationalized on reasons of efficiency, e.g. as insurance against dismissal because of market imperfections (Pissarides, 2001). In incomplete insurance markets the employer can have incentives 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 which it may contribute to create, to the extent that fewer hires and longer unemployment spans result in adjustment difficulties for fired personnel (Saint-Paul, 2007). Furthermore, the many voluntary agreements in collective agreements indicate that market failure is not general. Furthermore, collective dismissals can have negative social consequences and costs for others than the fired people, 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 suggesting that legal tradition is the most important explanatory factor behind country differences as regards both employment protection, regulation of collective agreements, and 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 can even decrease. This said, the explanations are not mutually exclusive to explain the emergence of employment protection legislation (Skedinger, 2010).

From a theoretical perspective, regulated employment protection increases the employer’s cost for adjusting the size and composition of the workforce. Increased costs of firing also decrease the employer’s willingness to recruit. Labor turnover hence decreases and as a consequence average employment and unemployment spans should increase. The net effect on employment and unemployment is however theoretically indeterminable, since 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 employees that are considered more risky when employment protection laws are strict (Kugler and Saint-Paul, 2004). They should prefer to employ people who already have jobs, since they are potentially less risky compared to people 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 Swedish LIFO rules make it more costly to revoke a bad recruitment decision (Skedinger, 2010).
Yet 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 if a reform occurs is in fact that the border for who is an insider 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 become 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 the sense 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). Furthermore, as hypothesized, it 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 also among 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). There does not seem 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 (Daniel and Siebert, 2005; Pierre and Scarpetta, 2004). Notably, Kahn (2007) finds that stricter rules decrease

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5 At the same time, stricter legislation appears to increase the costs associated with adjusting to structural changes, which has an impact on 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 inexistent (OECD, 1999), or even beneficial (Amable et al., 2007).

7 Daniel and Siebert (2005) find that the average education level of new employees increase in countries with stricter legislation. Pierre and Scarpetta (2004) find results suggesting that firms in countries with stricter legislation regarding permanent employment use more work place education and more temporary employment contracts.
employment among young people and immigrants relative to other groups. In addition, given employment, the probability of having a temporary employment 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 have treated groups of individuals or groups of firms differently, to compare outcomes between “treated” and “control” groups. Additionally, the possibilities to control for country-specific conditions are greater. Results from such studies suggest that stricter employment legislation negatively affects employment of the previously unemployed (Kugler and Saint-Paul, 2004; Nicholson and North, 2004), of the low educated (Montenegro and Pages, 2004; MacLeod and Nakavachara, 2007), of women compared to men (Kugler and Pica, 2006), and of 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 high sickness absence 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 decreased (Skedinger, 2010). von Below and Skogman Thoursie (2010) find that both hires and separations increased with about 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 of the Nordic countries. For this sample they find a modest effect of the reform on hires, 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 hence 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.

3. The natural experiment
3.1. Sweden’s EPL and LIFO

Employment protection in Sweden is relatively encompassing compared to other countries. Sweden ranks on place 11 among the 42 countries covered by OECDs employment protection index (OECD 2013). Regulations are stricter than average when it comes 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), although its origins can be found in the 1970s. 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.8

According to Swedish law, a firm cannot fire a permanent employee without just cause, which exists when there is labor shortage, or in the case of personal reasons, such as worker misconduct. Labor shortage is the most common reason, and then LIFO comes into play. This 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 says that some form of seniority rules can or should be used in relation to collective dismissals, but this is often together with other criteria (OECD, 1999; Skedinger, 2008:36).

According to LIFO, firms are required to dismiss the most recent employee first in case of redundancies (Skedinger, 2010). In practice, this means that the employer must comply with an established list of priority, ranking 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).

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8 As in most other European countries, Swedish reforms of employment protection legislation have been rather modest and mainly concerned conditions for temporary employment. Besides the 2001 reform, rules for permanent employment have in large part been left intact (Skedinger, 2008: 14).
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 collaborations between the environmentalists and the non-socialists were not common. The improbability of the 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 the workers.

The reform that passed the parliamentary vote in October 11, 2000 was furthermore partial in nature, as it softened LIFO only for small firms in the sense that firms with ten employees or less were allowed to keep two persons regardless of their seniority. It has been realized that such a partial reform in a natural way creates appropriate control groups, which are assumed to be unaffected by the reforms and thus can be used the estimate the causal effects of the reform (Skedinger, 2008). The reform applies to the firm level, so that 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). Notably, 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), work absence (Lindbeck et al., 2006; Olsson 2009), and firm growth (Bornhäll et al., 2016). We believe that the reform can be used as a natural experiment to our line of inquiry as well.

3.3. Identification

Natural experiments are ideal to identify 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,

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9 A first attempt of reform occurred in 1994, when the then center-right government introduced in 1994 a possibility for employers to exempt two people from LIFO in the case of work shortage, but the succeeding social democratic governments eliminated this possibility the following year (Skedinger, 2008:41).
Researchers who take this approach assume that firing costs are a function of e.g. passing a size threshold where the firm becomes the subject of different legislation (Skedinger, 2008: 73-74).\textsuperscript{10}

We use a difference-in-difference approach, and utilize the variation in the employment protection legislation across firm size and over 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 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 (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 may meanwhile 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 that position and to 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 concerning 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 to the existing

\textsuperscript{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).
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 (See also the discussion on how immigrants are affected by dispositivity in Calleman, 2003). Besides dispositivity and the possibility to negotiate with the union, 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 since they have a greater bargaining power against the 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 the treatment and control groups so that they are comparable. We therefore choose to exclude from our analysis firms with less than 5 employees, and firms with more than 16 employees. Furthermore, firms with 10 employees have incentives to stay beneath the threshold by not hiring additional personnel which is shown by Bornhäll et al. (2016). Firms with 11 employees face the opposite problem in terms of separations. We therefore exclude firms with 10-11 employees in 2000. Our treatment group then consists of firms that had 5-9 employees in the period before the reform, whereas our control group consists of firms with 12-16 employees. They still differ in size, however. Since this means that firms in the control group should have somewhat greater possibilities of circumventing LIFO, we expect our estimates of the effect of the LIFO reform to be downward biased.

\[11\] 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.
Over time, firms can expand or reduce their workforce 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. This means that we estimate the short-term reform effect with high accuracy. Also, more noise gets introduced in the data for each year after the reform (Mian and Sufi, 2012). By only using year 2001 as post-reform period, this noise is reduced to a minimum.

An additional concern is the risk of 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 remedied by the fact that the reform was unexpected, and that there were uncertainties concerning where the size threshold would be prior to it passing the parliamentary vote.

Nonetheless, it is possible that some of the effects of the reform may have materialized already 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 analysis, however, we perform so called placebo-estimations by estimating the treatment effects from a hypothetical reform one year before the true reform. If “false” DiD coefficients in years prior to the reform are not statistically significant, this remedies the endogeneity concerns while also strengthening the assumption of parallel trends.

4. Data

Our analysis is based on a matched employer-employee data-set provided by Statistics Sweden (SCB). We thus include both individual- and firm-level data in our estimations. We use firm-level data since the exemption rule from the LIFO-rule was applied on the firm-level, and not on the establishment level. Only active firms with 5-9 or 12-16 employees in 2000 were included in the sample. The firm-
specific data are matched with individual data from LISA\textsuperscript{12}, a longitudinal database that provides yearly information on all inhabitants in Sweden that are at least 16 years old.

Table 1. Sample description and summary statistics

<table>
<thead>
<tr>
<th></th>
<th>Treatment group</th>
<th>Control group</th>
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<tr>
<td></td>
<td>2000</td>
<td>2001</td>
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<td><strong>Panel A:</strong> Sample description.</td>
<td></td>
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<tr>
<td>No. firms</td>
<td>27,071</td>
<td>25,167</td>
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<tr>
<td>No. workers</td>
<td>177,393</td>
<td>166,609</td>
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<tr>
<td>Swedish</td>
<td>151,131</td>
<td>142,452</td>
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<tr>
<td>2nd gen immigrants</td>
<td>1,506</td>
<td>1,342</td>
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<tr>
<td>Western</td>
<td>4,924</td>
<td>4,560</td>
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<tr>
<td>Non-western</td>
<td>1,685</td>
<td>1,605</td>
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<tr>
<td><strong>Panel B:</strong> Mean values of dependent variables. Standard deviations in parentheses.</td>
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<tr>
<td>Total hires</td>
<td>2.45</td>
<td>1.90</td>
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<td></td>
<td>(2.14)</td>
<td>(2.32)</td>
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<tr>
<td>Hires from firm</td>
<td>1.55</td>
<td>1.20</td>
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<td></td>
<td>(1.80)</td>
<td>(1.81)</td>
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<tr>
<td>Hires from unemployment</td>
<td>0.25</td>
<td>0.17</td>
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<td></td>
<td>(0.56)</td>
<td>(0.46)</td>
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<tr>
<td>Hires from outside of workforce</td>
<td>0.65</td>
<td>0.54</td>
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<tr>
<td></td>
<td>(0.95)</td>
<td>(0.95)</td>
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<tr>
<td>Total separations</td>
<td>2.22</td>
<td>1.97</td>
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<tr>
<td></td>
<td>(2.11)</td>
<td>(2.18)</td>
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<tr>
<td>Separations to firm</td>
<td>1.53</td>
<td>1.36</td>
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<tr>
<td></td>
<td>(1.79)</td>
<td>(1.81)</td>
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<tr>
<td>Separations to unemployment</td>
<td>0.16</td>
<td>0.15</td>
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<tr>
<td></td>
<td>(0.44)</td>
<td>(0.43)</td>
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<tr>
<td>Separations outside of workforce</td>
<td>0.53</td>
<td>0.46</td>
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<tr>
<td></td>
<td>(0.82)</td>
<td>(0.79)</td>
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</tbody>
</table>

Sample description and summary statistics are given in Table 1. As can be seen, the final sample contains 35,004 firms in 2000, 27,071 in the treatment group and 7,933 in the control group. The number of firms is reduced to 32,431 in 2001 because we can only consider surviving firms when estimating a difference-in-difference model. In 2001, 166,609 workers are employed by firms with 5-9 employees and 99,666 by firms with 12-16 employees (Panel A in Table 1).

\textsuperscript{12} Longitudinell integrationsdatabas för sjukförsäkrings- och arbetsmarknadsstudier.
Worker flows to and from firms in the control group is around twice as high as for firms in the treatment group in 2000 (Panel B in Table 1). This is expected since the average firm in the control group is around twice as large (13.76 employees per firm) as the average firm in the treatment group (6.55 employees per firm) in 2000.

Data for individuals’ employment status is obtained from the RAMS\textsuperscript{13}-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. Since hours worked is not included in the RAMS database, Statistics Sweden instead uses information about paid gross wages to identify employed individuals since hours worked is not included in the RAMS-database.

Data for unemployment is gathered from the Swedish public employment office’s (\textit{Arbetsförmedlingen}) database HÄNDEL. In our data, an individual is classified as unemployed if that person is registered at the employment office the last of November each year. The fact that both employment and unemployment is measured in November gives us a snapshot of the labor market at that time. Individuals neither in employment nor in unemployment are classified as not being a part of the workforce. The use of two different registers means that around 1.3 percent of the individuals are classified as being both employed and unemployed by Statistics Sweden. In this study, we classify these individuals as employed.

A liberalization of the LIFO-rule lowers the incentives for workers to obtain seniority in the workplace. This could lead to an increase of voluntary job changers since the cost of moving to another firm is reduced. It thus becomes important to distinguish between separations to other firms, and separations into unemployment and outside the workforce.

Each employed individual in our sample is linked with its 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 flows into and out of the workforce. One limitation with yearly data is that we have no information regarding an individual’s occupation between November each year. Individuals that are classified as job changers can therefore

\textsuperscript{13} SCB:s (Statistics Sweden) registerbaserade arbetsmarknadsstatistik
have an unemployment spell between the two employments. 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 region of origin in our dataset. This is important, since immigrants are a highly heterogeneous group with different probabilities of entering the labor market. For example, Fleischmann and Donkers (2010) found that immigrants from more politically stable and free, more developed and wealthier societies were less often unemployed in the European labor markets. Immigrants coming from Islamic countries had higher rates of unemployment, while those originating from Western Europe were less likely to be unemployed.

Data on the region 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 their origin: (i) Native Swedes; (ii) Second generation immigrants, i.e., people with two foreign-born parents who are 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, 85.6 percent, followed by western immigrants, 3.0 percent, second generations immigrants, 1.0 percent, and non-western immigrants, 0.9 percent.

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. Since it is impossible to compare the two counterfactual trends, our best option is to look at the trends before treatment. Figure 1 shows the trends in hires for the treatment group and the control group over the time period 1994-2000. The figure lends support to the idea of parallel trends in the pre-reform period even if the trend during the last years, 1999-2000, is somewhat steeper for the control group.
The treatment and control groups also have similar trends in terms of average separations (Figure 2). Again, there is some indication of a steeper upwards trend for the control group.

The steeper trend of the control group for both hires and separations means that our estimated effects of the reform might be downwards biased. The estimated treatment effect, $\hat{\text{Treat}}$, can be written as the true effect, $\text{Treat}$, plus any difference in trends, $\text{Trend}(\text{treatment}) - \text{Trend}(\text{control})$, in the counterfactual case of no treatment (Equation 1), i.e.:

$$\hat{\text{Treat}} = \text{Treat} + (\text{Trend}(\text{treatment}) - \text{Trend}(\text{control})) \quad (1)$$

Figure 1 and 2 suggest that the pre-reform trend for the control group is larger than for the treatment group, which would imply that $\text{Trend}(\text{treatment}) - \text{Trend}(\text{control})$ is negative and that $\hat{\text{Treat}}$ is smaller than $\text{Treat}$. Our estimated effects on of the LIFO-reform on hires and separations might thus be conservative.
5.2. Hiring and separations

We proceed by making a difference-in-difference analysis (c.f. Angrist and Kreuger, 1999). This will give us an estimate of the treatment effect on the treated (TT), as it evaluates the effect on small firms of being exempt from LIFO.

Following previous work (Bjuggren, 2015; Bornhäll et al., 2016) we formulate a linear DD model of the type,

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

(2)

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, which acts like a time fixed-effects 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 5-9 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 \( D_t \). This is our DiD-estimator, with \( \beta_3 \) capturing the causal effect of the reform under the assumption that small and large firms have parallel trends in the outcome variable in the counterfactual case of no treatment. \( \beta_3 \) thus estimates the treatment effect of the reform. All coefficients should be interpreted as the result on the treatment group compared to 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 with the timing of the reform.

We estimate Equation (2) several times by letting \( Y_{it} \) vary from one estimation to another. In the first estimation (henceforth, A1), 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 make separate estimations where \( Y_{it} \) 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 depend on the labor market status of the individuals.
The corresponding estimations are then done 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 the separation, or if they found work elsewhere.

In addition, for each of the eight estimations, we restrict the sample to include only: (i) Natives (born in Sweden); (ii) second-generation immigrants; (iii) western immigrants; and (iv) non-western immigrants. In total, we thus conduct 32 estimations to capture whether the effect of the LIFO-reform on hires and separations depend on the individual’s region of birth. Due to the large amounts of estimations, the estimated $\hat{\beta}_3$-coefficients are presented graphically together with a 95 percent confidence interval.\(^{14}\)

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

5.3. Results

First, we analyze how the LIFO-reform affected hires among the treated firms (5-9 employees). The estimated effect for each group of individuals is presented in Figure 2, together with its associated confidence interval (95 percent). We see that

\(^{14}\) Tables with full regression results are available from the authors upon request.
the reform increased hires from other firms, from unemployment and from outside of the workforce. We also see that the new hires come from all three recruitment sources, other firms (A2), unemployment (A3), and from outside of the workforce (A4).

The reform effect is largest for Swedish-born individuals and drops, or turns insignificant, for the hiring of foreign-born workers. In total, the reform led to an increase in hires of Swedish-born workers by 0.56 workers per firm compared to 0.04 for second generation immigrants and for westerners. We find no increase in hires of individuals born in non-western countries.

**Figure 3.** Difference-in-difference estimate of the reform’s effect on hires, point estimates and 95 percent confidence interval.

Panel A2 shows worker flows between firms. We find that the 2001 reform increased hires of Swedish-born workers from other firms with 0.35 workers per firm. The reform also increased recruitment of western immigrants, by 0.03 per firm. Second generation immigrants and individuals born in non-western countries where not recruited from other firms to any higher extent due to the reform.
The reform also increased recruitment from unemployment (A3). Again, most of the increase is attributed to recruitment of unemployed Swedes. Firms with 5-9 employees hired 0.07 more unemployed Swedes, whereas the reform had no effect on the hires of immigrants from non-western countries. The possibility to exclude two workers from the LIFO-rule thus seem to benefit outsiders that are relatively close to the labor market, whereas it had no significant effect on unemployed non-western immigrants. Note also that the effect of the reform on the hiring of unemployed individuals is smaller than the effect on recruitments from other firms, suggesting that the reform had a stronger effect on insiders than on outsiders.

Lastly, panel A4 shows that the reform also increased hires of individuals not previously in the workforce. Firms in the treatment group hired on average 0.15 more Swedes and 0.02 more second generation immigrants than one would expect in the absence of the reform. Once again, there is, no significant effect on the hires of individuals born outside of Sweden.

Next, we do the analysis for worker separations. As before, the regression results from the difference-in-difference model is presented through four graphs (Figure 3). The total effect on separations for the four groups is shown in panel B1. As in the case of hires, we see that the reform also increased separations. On average, 0.34 more Swedish workers were separated from firms with 5-9 employees, compared to firms in the control group and to the year before the reform.

However, when we investigate the effect on the direction of the job flows, we can see that the separation effect is driven by job changers (B2) and that the reform had no effect on separations to unemployment (B3). Again, we see that the reform had its largest effect on Swedish workers. These separations increased by 0.27 workers per firm, meaning that it accounts for the larger share of the total effect of 0.34. If we combine this result with the previous results which showed that the flow out of unemployment increases, the results show that the overall effect of the reform was to lower unemployment. Swedish workers employed in firms in the treatment group also got slightly more prone to leave the workforce because of the reform (B4).
However, the fact that the reform did not affect separations to unemployment, indicates that the reform increased voluntary separations or that those that were dismissed found work in other firms within a year.

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

By combining the results on hires (Figure 3) and separations (Figure 4), we get the net-effect of the reform (Figure 5). From the upper-left panel (C1) we see that total net-employment increased among firms with 5-9 employees. In total, the results show that the reform increased employment by 0.24 individuals per firm. The treatment group in our sample consists of 25,167 firms in 2001 which means that employment increased by 5,965 workers or 3.36 percent.

Both hires and separations to other firms increased with similar magnitudes, meaning that even if the reform increased dynamics between firms, it did not have an effect on the net-flows (C2).
On the other hand, we find a positive outflow from unemployment into employment for individuals born in Sweden (C3). The combined effect for the four groups is estimated at 0.070, meaning that 1,762 individuals left unemployment in favor of employment in a treated firm. However, the net-flows of foreign-born workers to and from unemployment was unaffected by the reform.

Finally, we look at net-effects on net-hires of individuals from outside of the workforce (C4). The reform only affected Swedish workers, who entered the workforce for employment to a higher extent than one would expect in the absence of the reform. In total, firms with 5-9 employees hired 0.090 more workers per firm which corresponds to 2,265 new workers.

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

In order to check the robustness of the results we also perform placebo-estimations of a hypothetical reform. The treatment and control groups are defined in the same way, but we use 1999 as the pre-reform year and 2000 as the post-reform year, and the results are presented in Figures A1-A3 in the Appendix. These regressions
should pick up any change in a firm’s behavior due to anticipating the reform. We find no significant results on net-hires of the placebo-estimations. When looking at hires from other firms, we see a significant but much smaller positive effect compared to the results for the true reform. For separations, the placebo-estimations show a negative effect on workers leaving the workforce. The magnitude of the effect is, however, once again significantly smaller than the true reform effect. The placebo estimations thus indicate that our results are driven by the reform of the LIFO principle in 2001, but also suggest that some firms might have expected the reform and changed behavior already in 2000. Due to limitations in the data, we are unable to estimate the effect for hypothetical reforms prior to 2000.

6. Discussion and conclusions

The low labor market participation among immigrants should be a cause for concern for policymakers in Europe, notably since it risks resulting in social exclusion. The insider-outsider theory suggests that the difficulties for immigrants to establish themselves at the labor market are related to their outsider status, with insiders taking advantage of labor market turnover costs and bargaining for higher wages than the market-clearing rate. These 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, through for example unions, collective agreements, and legislation.

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

The Swedish reform of the LIFO rules in 2001 constitutes a natural experiment, which makes it possible to draw causal inference on 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 for native swedes and foreign-born individuals differently. Our analysis was based on a difference-in-difference regression model where we compared how the reform affected job flows for treated firms (5-9 employees) in comparison to a control group of firms (12-16 employees) that were unaffected by the reform.

The results showed that the reform increased employment among the treated firms (5-9 employees) with 5,965 workers in total, and significantly reduced unemployment levels. 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 limited to Swedish-born individuals, and mainly lead firms in the treatment group to increase recruitments of employees from other firms. Our results thus suggest that the reform effects are dependent on the relative insider-status of the employees, so that groups of employees that are closer to being insiders see greater benefits of a less strict employment protection legislation than groups that are further from being insiders. However, we should emphasize that no group seems to be a clear loser from the reform.

One limitation is that we are not able to distinguish between voluntary and involuntary separations in the data. We found clear evidence that separations increased due to the reform, but to other firms and not to unemployment. It is, however, possible that workers had a longer unemployment spell between their job positions. We believe that this question merits further research.

Previous studies have indicated that the LIFO reform produced a number of benefits, such as decreased sick absence, 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 immigrants. Note, however, that our study is limited to consider the effect of the reform in 2001. Furthermore, the reform was rather marginal in scope. An important venue for future research would therefore be to determine what a more extensive employment protection legislation reform would mean for different groups in the labor market.
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Appendix

Robustness tests

A key identification assumption is that of no interaction between the treatment group $D_g$ and the treatment period $D_t$ except for the reform. To investigate the exogeneity of treatment and to evaluate the parallel-trends assumption we use a “placebo test” to estimate the treatment effect on a hypothetical reform in 2000, i.e. one year before the true reform. “False” DiD coefficients, i.e. for the year prior to the reform, should not be statistically significant. If this is so it also strengthens the assumption of parallel trends.

Figure A1. Placebo-estimation on hires, hypothetical reform in 2000, point estimates and 95 percent confidence interval.
The placebo-estimations for hires reveal a positive and significant effect on hires from other firms (Panel D2). The estimated coefficient is smaller than the true effect (Panel A2). No effect is found for hires from unemployment (D3) or from outside of the workforce (D4) which means that also the total effect on hires (D1) is positive.

**Figure A2.** Placebo-estimation on separations, hypothetical reform in 2000, point estimates and 95 percent confidence interval.

The placebo-estimations for separations (Figure A2) also show a positive effect for separations to other firms (E2). Again, it is smaller than the true reform-effect (Panel B2 in Figure 3). We also see a negative effect for separations to outside of the workforce (E4) compared to the positive effect found in our main model (B4). Since the two effects have opposite signs, we see no significant effect on total separations.
Lastly, when estimating the net-effect for a hypothetical reform year, we find no significant effects (Figure A3). The false effect on hires from firms (D2) is visible as a positive point estimate in panels F1 and F2, it is however not significant.

We are not able to analyze why we find these false effects. With a large number of estimations, it is reasonable to expect some spurious results showing significance. These results could also be due to misclassification of firms into treatment and control groups or that some firms changed behavior in year 2000 in expectation of the reform.

**Figure A3.** Placebo-estimation of net-effects of hypothetical reform in 2000, point estimates and 95 percent confidence interval.