Carl Magnus Bjuggren & Per Skedinger
2018:7

Does Job Security Hamper Employment Prospects?

Linnæus University Centre
Discrimination and Integration Studies

Does Job Security Hamper Employment Prospects?*

Carl Magnus Bjuggren†
Per Skedinger‡

November 29, 2018

Abstract

We use a reform in the Swedish employment protection legislation (EPL) that decreased dismissal costs for small firms only, to investigate the effect of EPL on the propensity to hire workers who were unemployed or in active labor market programs (ALMPs). The results indicate that less stringent EPL increased the share of workers hired from unemployment. In addition, our results suggest that transitions from some ALMPs to employment increased. Taken together, our results suggest that there was less screening of new hires after the reform, and that liberalization of EPL mitigates the stigma associated with unemployment and participation in ALMPs.

JEL *Codes:* J60, J64, K31, H80

Keywords: Employment protection, Active labor market programs, Unemployment

---

* We are grateful to participants at the 2018 AEA meeting in Philadelphia, the 2018 EALE meeting in Lyon, the 2018 MVEA meeting in Memphis, the 2017 Public Choice Society meeting in New Orleans, and seminar participants at Linnaeus University for their valuable comments and suggestions. Olga Pugatšova and Charlotta Olofsson provided excellent research assistance. We gratefully acknowledge financial support from the Swedish Research Council for Health, Working Life and Welfare (Forte), grant number 2014-2740 (first author), the Johan and Jakob Söderberg Foundation (second author) and the Marianne and Marcus Wallenberg Foundation (both authors).

† Research Institute of Industrial Economics (IFN), Stockholm. E-mail: carl.magnus.bjuggren@ifn.se.

‡ Research Institute of Industrial Economics (IFN), Stockholm, and Centre for Discrimination and Integration Studies, Linnaeus University, Växjö. E-mail: per.skedinger@ifn.se.

1. Introduction

The effect of employment protection legislation (EPL) on employment is theoretically ambiguous. Increased firing costs make employers both less prone to dismiss workers and less inclined to hire them, as employers anticipate these potential costs already in their hiring decisions (Bertola, 1999). In accordance with the theoretical prediction, empirical evidence on the overall employment effect is mixed (see, e.g., OECD, 2013, and Skedinger, 2010, for surveys).

There is a theoretical rationale why the employment effect of EPL may be *heterogeneous* across groups. Reduced hiring could disproportionally affect new entrants to the labor market, with reduced firing mainly benefiting incumbent workers. If more stringent EPL makes employers more cautious when recruiting workers, vulnerable groups may be put to a disadvantage. Kugler and Saint-Paul (2004) argue that more stringent EPL causes employers to increasingly prefer hiring employed workers rather than the unemployed, due to adverse selection. Unemployment serves as a signal of low productivity, and incentives to avoid hiring low-ability workers (lemons) strengthen with stricter EPL.[4] In addition, EPL may strengthen the bargaining power of key groups of employees, at the expense of outsiders (Lindbeck and Snower, 2001).

There is ample evidence, based on various types of data and identification strategies, suggesting that stricter EPL indeed hurts the employment prospects of vulnerable groups, like women, youth, immigrants, the low-skilled, and the disabled.[5] A reduction in employment is not the only effect, as some of these studies show that the incidence of fixed-term employment increases, rendering available jobs more insecure (Fervers and Schwander, 2015; Kahn, 2007). With the exception of Kugler and Saint-Paul (2004), these studies do not examine effects on the hiring of unemployed workers.

---

[4] One could also conceive of factors making unemployed workers more attractive than employed workers, e.g., to the extent that the former can start working sooner or are willing to accept a lower wage (Eriksson and Lagerström, 2006). However, collective bargaining and the presence of regulated wage floors may prevent such wage adjustments.

[5] For studies using aggregate cross-country data, see, e.g., Bertola et al. (2007), Botero et al. (2004), Feldmann (2009), Heckman and Pagés-Serra (2000), Kawaguchi and Tetsushi (2012), and Skedinger (1995). Examples of cross-country studies, using data disaggregated by establishment or individual-level data, are Daniel and Siebert (2005), Fervers and Schwander (2015), and Kahn (2007).Within-country studies, relying mostly on individual-level data and on partial reforms affecting particular regions or subgroups of workers and firms, are, e.g., Acemoglu and Angrist (2001), Autor et al. (2006), Kugler (2004), Kugler and Pica (2006), Kugler and Saint-Paul (2004), MacLeod and Nakavachara (2007), and Montenegro and Pagés (2004).

Unlike previous work in this field, we focus on the employment prospects of the unemployed and participants in active labor market programs (ALMPs). We examine a reform of seniority rules in Sweden in 2001 that affected small firms only.[6] Seniority rules – or last-in first-out – imply that dismissals should occur in reverse order of seniority. Firms with up to ten employees were allowed to exempt two workers from the seniority rules when dismissing workers for economic reasons. The design of the reform enables us to use slightly larger firms as controls in a difference-in-difference framework. We investigate whether the liberalization of EPL made employers in small firms more willing to hire "wild cards", with little previous experience. Seniority rules are also likely to increase the average productivity of dismissed workers, thus making unemployment less of a stigma and increasing the hiring rate of unemployed workers, as argued by, e.g., Baumann (2010) and Kugler and Saint-Paul (2004).

To the best of our knowledge, this is the first study to examine the link between the stringency of EPL and employers' recruitment of previous participants in ALMPs. Such participation could, like unemployment, be associated with stigmatization and this indirect effect is more likely to occur the more the programs are targeted toward disadvantaged groups.[7]

Standard evaluations of ALMPs are typically plagued by sample selection bias, as participants usually are not assigned randomly to the programs. Since we rely on an arbitrary firm size threshold for identification, we are able to examine stigmatization effects in ALMPs in a setting in which such sample selection should not affect our results. This line of reasoning also applies to the non-random selection into unemployment.

Our results indicate that the reform increased the share of workers hired from unemployment by 5-10 percent, depending on specification. We obtain mixed results concerning transitions from

---

[6] The 2001 reform has been exploited as a natural experiment in several previous studies, investigating effects on sickness absence (Olsson 2009), parental childcare (Olsson, 2017), job flows (von Below and Skogman Thoursie, 2010), labor productivity (Bjuggren, 2018), and firm growth (Bornhäll et al., 2017). A different approach is used by Böckerman et al. (2018), who analyze the impact of seniority rules on worker mobility and wages by comparing firms in Sweden with the same firms operating in Finland, where there are no such rules in legislation.

[7] Several effects of ALMPs have been identified in the literature (Calmfors, 1994). Besides direct effects on employment, unemployment and earnings, there are indirect and potential deadweight and substitution effects, locking-in effects, general equilibrium wage effects, etc.

ALMPs to employment following the reform. The findings suggest an increase in transitions for programs focusing on preparatory training by 6-11 percent. The reform appears to have decreased the share of workers hired from subsidized employment, albeit only for certain firm size categories. No effect is found on transitions from longer unemployment spells to employment. Finally, we note that the increase in the share of workers hired from unemployment and ALMPs seems to be driven primarily by those with some college education.

The paper is organized as follows. In the next section, we provide a brief overview of EPL in Sweden and discuss the design of the seniority rules and the implementation of the 2001 reform of these rules. Section 3 presents our data and the econometric results. Our findings are summarized in Section 4.

2. Seniority Rules and the 2001 Reform

The Swedish Employment Protection Act (EPA) was introduced in 1974. According to the law, a firm cannot fire a worker on an open-ended contract without just cause, which includes economic reasons or gross misconduct on part of the worker. The stringency of regulations of permanent employment is above the OECD average, whereas the rules for temporary employment are among the most lenient (Skedinger, 2010). In case of layoffs for economic reasons, seniority rules apply to workers with open-ended contracts.[8] These workers should be laid off in reverse order of seniority, according to a list made by the employer. The rules apply to the establishment level, so that workers in the same firm but in different establishments appear on different lists. If two employees have accumulated the same tenure, priority is given to the older worker. The seniority rules exclude members of the employer's family, persons hired to work in the employer's household, and workers participating in employment subsidy programs.

The seniority regulation is perhaps the most controversial part of EPA. Employers argue that seniority rules make it more difficult to retain the most competent workers in the firm, while unions are of the opinion that the rules are necessary to prevent arbitrary dismissals at the individual level. An important feature of EPA is the possibility to depart from seniority rules (and many other regulations) in local agreements between employers and unions, as long as the agreements are not

[8] For more details on Swedish seniority rules, see, e.g., Cahuc (2010).

discriminatory or otherwise improper.[9] Obviously, such agreements make it easier for employers to retain the most competent workers. However, local agreements are selectively approved and require sufficient bargaining power against the union. Some unions of blue-collar workers have adopted a policy of approving local agreements only in exceptional cases.

As discussed in Böckerman et al. (2018), seniority rules are part of a wider concept in EPL, namely right-to-priority rules, which define criteria according to which employees should be prioritized in case of dismissals for economic reasons. A number of countries incorporate such rules in some form in their legislations, but very few explicitly refer to seniority as the exclusive or main criterion. Besides Sweden, these countries include India and the Netherlands. However, in many countries, including Denmark, Finland, Norway and the United States, seniority rules are included in collective agreements, but to varying degrees depending on the agreement and its coverage.

Swedish reforms of EPL have mainly concerned terms for temporary employment, with successive liberalizations since the early 1990's. Legislation regarding permanent employment has largely been left intact, with the exception of the 2001 reform of seniority rules. This reform allowed firms with up to ten employees to exempt two workers from the priority list when dismissing workers for economic reasons. Since the exemption is in absolute number, the impact of reform is not uniform across firm size. The change (in percentage points) in the number of workers that are protected and not protected by the seniority rules is portrayed in Figure 1. The 2001 reform increased the share of protected and unprotected workers disproportionately. If, for example, a firm with five employees intended to dismiss a worker before the reform, it had to lay off the last one hired. Hence four workers (80 percent) were protected by the seniority rules, and one worker (20 percent) was unprotected. After the 2001 reform, a firm with five employees has the possibility to exempt the two last persons hired, leaving two workers (40 percent) protected and three workers (60 percent) unprotected. The change in percentage points for protected and unprotected workers is largest for firms with three employees, and then decreases with firm size.

[9] Heyman and Skedinger (2016) provide an analysis of the effects of collectively agreed periods of notice on worker flows.

The reform was the outcome of an unusual collaboration between the Center-Right alliance and the Green Party, neither of whom were in power at the time. Furthermore, the process leading up to the implementation was fast. The reform was not discussed in public until 2000, approved by Parliament in October 2000, and implemented in January 2001.[10] The fast and rather unexpected implementation of the reform in combination with its partial nature makes it suitable for a difference-in-difference analysis, with firms up to ten employees as the treatment group and slightly larger firms constituting the control.

The 2001 reform size cut-off at ten employees applies to the firm level, and not establishments, in order to prevent firms from exploiting the reform by having multiple establishments. Surveys by the Confederation of Swedish Enterprise show that the exemption from the seniority rules are being used extensively and that employers view them as vital for their firm's future (Confederation of Swedish Enterprise, 2009, 2014).

3. Data and Empirical Estimation

We use matched employer-employee register data from Statistics Sweden and the *Integrated Database for Labour Market Research* (LISA). The data set covers all individuals and all firms with at least one employee on an annual basis, 1996-2004. For individuals, the data contain information on labor market status and education, and for firms there is information on region and industry.

In the data, ALMPs are divided into two groups: Preparatory programs (*åtgärdsstudier*), which consist of labor market training, job-search assistance and work practice, and subsidized employment (*åtgärdssysselsättning*). While the former group of programs involves compensation from the Public Employment Service to the participant, the latter basically entails a subsidy payable to the firm employing the worker.[11] Around 89,000 individuals participated in various preparatory programs in March 2000, with labor market training engaging most participants, 33,000 (Statistics Sweden, 2009). Various forms of subsidized employment involved 77,000

[10] See, e.g., Bjuggren (2018) for more details about the reform.
[11] A relatively small number of individuals in subsidized employment are enrolled in the program Start-up Grant, which involves income support to self-employed workers.

participants. The largest program was the Wage Subsidy (*Lönebidrag*), in which 48,000 individuals participated in March 2000. This program is targeted toward the work disabled and up to 80 percent of the wage is subsidized, depending on the work capacity of the individual, for a maximum period of four years (Runeson and Bergeskog, 2003). The number of persons who at any time during 2000 participated in preparatory programs is 277,000 and the corresponding figure for subsidized employment is 123,000 (Statistics Sweden, 2009).

In our baseline setting, we capture individuals that transitioned from unemployment to employment or transitioned from ALMPs to employment.[12] Employment is defined by the individual's employment status in November each year, and each individual is linked to only one firm depending on their main source of income. In addition, to be classified as employed, we do not allow the individual to have been in an ALMP or unemployed at any point during the year. An individual is classified as unemployed if she has been registered as unemployed for at least one day during a year. An individual is defined as in an ALMP if she is in a preparatory program or in a subsidized employment for at least one day during a year. Based on these definitions we create transitions to and from employment according to Table 1.[13]

All individual data are aggregated to the firm level. It is important that we measure the number of employees in the firm accurately. Since there is no information on workers' positions within the firm, we assume that at least one of the workers holds a managerial position. The variable for firm size, in terms of employees, is therefore reduced by one for all firms. Our outcome variables measure the share of workers in each firm that were hired from unemployment and ALMPs, respectively. For example, if a firm in year t has a total of five employees, one of whom was unemployed the previous year, then the share of workers hired from unemployment in t is 0.2. If there are no new hires from unemployment, the share is 0. These shares are calculated for each firm and outcome variable. Because we use information in $t-1$, 1997 is the first year for which we have data on the share of workers hired from unemployment and ALMPs.

[12] Unemployment refers to openly unemployed individuals. Participants in ALMPs are typically classified by national statistics offices as out of the labor force (preparatory programs) or employed (subsidized employment).
[13] Note that employment and firm size is defined in November each year, but for ALMPs we can only locate the number of days per year that the individual participated in and we do not know the ALMP status in November. We address the potential problems with measuring firm size in section 3.1.1 below.

Figure 2 relates the three outcome variables to the number of employees in the firm. In small firms, values are low or irregular for several of the outcome variables. In addition, very small firms are likely to differ in unobservable ways from larger firms. Firms with four or less employees are therefore dropped from all estimations.[14] Figure 2 shows that the share of workers hired from unemployment, as well as from ALMPs, has decreased over time.

3.1 Empirical Estimation

We use a difference-in-differences (DiD) framework to estimate the effect of the 2001 reform on the share of workers hired from unemployment, preparatory programs, and subsidized employment. In our DiD framework, the outcome for workers in firms with 5-10 employees (treatment group) is compared to that of workers in firms with 11-15 employees (control group).

Firms and workers may select themselves into or out of the treatment category. In order to mitigate a potential problem with selection, we use firm size in 1999 as the treatment indicator, that is, one year before the reform was discussed in public and two years before the reform took place. Using firm size in 1999 as the treatment indicator allows us to estimate both the intention-to-treat (ITT) effect and the local average treatment effect (LATE).[15] A positive effect on the share of workers hired from unemployment or ALMPs would indicate that the reform affected the smaller firms' screening of new hires, increasing firms' propensity to employ "wild cards" as opposed to individuals that were previously employed at another firm.

Summary statistics for the two groups are presented in Table 2.[16] On average, the share of workers that transitioned from the various states in the treatment group are similar to those in the control group. The difference in the share of workers leaving unemployment before and after the reform is -0.0011 for the treated firms and -0.0032 for the control group. The difference in differences is

[14] Excluding firms with less than four employees will also remove the majority of self-employed individuals. In addition, fishing and forestry sectors are dropped from all estimations to improve consistency over time. The fishing and forestry sectors were included in the official statistics from 2001.
[15] A similar strategy is used by Bjuggren (2018) and Olsson (2017).
[16] Yearly averages for all three outcome variables, not separated by treatment assignment, are presented in the Appendix, Table A1. Yearly averages of the number of individuals who transitioned from unemployment and ALMPs are presented in the Appendix, Table A2.

thus 0.0021, and indicates a relative increase in the share of workers that were hired from unemployment. The increase corresponds to 5 percent of the pre-reform share of workers hired from unemployment in the treatment group. This can be seen as preliminary evidence on the effect of the reform.

To investigate whether the 2001 reform affected the share of new hires leaving unemployment, as well as new hires from ALMPs, we use OLS to estimate the ITT with the following equation:

$$Y_{it} = \propto + \tau_t + \delta d_{i99} + \beta Z_{it} + \mathrm{X}_{i99}\gamma + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the share of workers corresponding to the outcome variables, for each firm *i* and time *t*. τ_t is a full set of year dummies, $Z_{it} = Post_t \times d_{i99}$, where d_{i99} is a dummy variable taking the value 1 if a firm had 5-10 employees in 1999 and 0 if it had 11-15 employees in that same year. $Post_t$ is a dummy variable that indicates the post reform time period. The coefficient β captures the treatment effect of the reform. The vector X_{i99} includes fixed effects for industry, fixed effects for region, and an interaction between industry and year. The industry and region fixed effects are defined by the situation in 1999 in order to make them exogenous to the reform.

To capture the LATE, we will use Z_{it} as an instrument in a two stage least squares regression. We have the following equation:

$$Y_{it} = \propto + \tau_t + \delta d_{i99} + \beta \widehat{D}_{it} + \mathrm{X}_{i99}\gamma + \varepsilon_{it}, \quad (2)$$

where $\widehat{D}_{it}$ is the predicted value from the first stage equation:

$$D_{it} = \phi_0 + \tau_t + \phi_1 d_{i99} + \phi_2 Z_{it} + \mathrm{X}_{i99}\phi_3 + \upsilon_{it}, \quad (3)$$

where $D_{it} = Post_t \times d_{it}$ is a dummy variable taking the value 1 if the firm is in the treatment group at time *t*, and Z_{it} is defined as above. Given the assumptions of independence, exclusion, the existence of a first stage, and monotonicity of the instrument Z_{it}, equation (2) will capture the LATE, which is the effect of treatment on compliers (Imbens and Angrist, 1994). In this scenario, compliers are firms that stayed within the treatment group of small firms. Hence, in the presence of endogeneity caused by firms selecting themselves into treatment, the IV setting above will still give us consistent estimates.

The coefficient β from equation (2) is scaling the ITT parameter with the probability of treatment, similar to Havnes and Mogstad (2011), Olsson (2017), and Bjuggren (2018). We follow Bjuggren (2018) in our argument for LATE. First-stage equations exist and are presented in Table A3 in the Appendix. Z_{it} is assumed to be independent of potential treatment assignment as well as potential outcome. The reform could not have been anticipated due to its fast implementation and unusual cooperation of parties across the political spectrum. In addition, there appears to be no difference in trends between the treatment and control group prior to the reform (see Figure 3). Based on Figure 3, we also assume that Z_{it} has no effect on the outcomes in the absence of the reform (exclusion restriction). Finally, we assume monotonicity, which implies that having less than 11 employees in 1999 does not make it less likely to have less than 11 employees after the reform. A potential threat to identification is attrition. As shown by both von Below and Thoursie (2010) and Bjuggren (2018), firm exits do not seem to have been affected by the reform.

To investigate year-specific effects, as an assessment of the validity of the parallel trends assumption, we estimate the following model:

$$\mathrm{Y_{it}} = \propto + \tau_t + \delta \mathrm{d_{i99}} + \sum_{t=1997}^{2004} \beta_t (\tau_t \times \mathrm{d_{i99}}) + X_{i99}\gamma + \varepsilon_{it}, \tag{4}$$

where each separate year dummy, τ_t, is interacted with the treatment indicator d_{i99}. The year 1997 is used as a benchmark. This will serve as an indication of the validity of the parallel trends assumption, since we also consider placebo periods that occurred prior to the reform. The results are presented in Figure 3. The DiD estimates before the reform – the placebo tests – are not significant, meaning that there is no statistically significant difference between the treatment and the control group before the reform, which supports the parallel trends assumption. The annual estimates for the share of workers hired from unemployment, Figure 3a, indicate a clear effect after the timing of the reform. Similarly, in Figure 3b, there is an increase in the share of workers hired from preparatory programs in 2002 and onward. Although showing a slight negative shift in the point estimates, there are no statistically significant effects of the reform on the share of workers hired from subsidized employment in Figure 3c.

3.1.1 Basic Results

The results from equations (1) and (2) are presented in Table 3, where covariates are introduced stepwise. Our findings clearly indicate that the reform mitigated the stigma associated with unemployment. The estimated coefficients for the share of workers hired from unemployment range between 0.00236-0.00259 for the ITT and between 0.00444-0.00487 for the LATE. That corresponds to an increase of the pre-reform share of 5-6 percent for the ITT and 10 percent for the LATE (see Table 2 for pre-reform shares). All specifications for both the ITT and LATE yield statistically significant coefficients; however, including all covariates as in column (3) will likely improve the accuracy of the estimation.[17]

The estimated coefficients for the share of workers hired from preparatory programs are statistically significant and indicate an increase of 5-6 percent for the ITT and 10-11 percent for the LATE. The year-specific effects presented in Figure 3b, indicates a positive effect starting in 2002. The underlying coefficients for Figure 3b are presented in Table A6 in the Appendix, and the estimated coefficient for 2002 (0.0021) indicates a 14 percent increase of the pre-reform share. In Table 3, the estimated coefficients for workers hired from subsidized employment are only statistically significant at the 10 percent level, with the exception of the LATE effect, suggesting a negative effect of 14 percent using the full model (3). However, none of the year-specific estimates are statistically significant in Figure 3c and Table A6, and we therefore conclude that the estimates should be interpreted with some caution.

The accuracy of the size cut-off at 10 or 11 employees may be affected by measurement errors. Firm size is measured in November each year and there is no differentiation between workers on open-ended and fixed-term contracts. In addition, the 2001 reform excludes members of the employer's family, persons hired to work in the employer's household, and workers participating

[17] In order to prevent overlapping between the different treatment categories, in our baseline setting we do not count individuals that were in two or more categories the year before they transitioned to employment. In Tables A4-5 and Figure A1 in Appendix, we show that the results still hold when we allow for overlapping between the different treatment categories. The effect on transitions from unemployment is similar whereas the effect on transitions from preparatory programs is only present in 2002 (Figure A1).

in employment subsidy programs, when determining the firm size threshold. In order to allow for potential measurement errors around the reform threshold we expand the gap between the treatment and control group. In Table 4, we show that the positive effect on transfers from unemployment still persist when we exclude firms of size 10-11 and firms of size 9-12. The estimated effect is lightly larger for hires from preparatory programs (7-13 percent) and for hires from subsidized employment (8-20 percent), and somewhat lower for hires from unemployment (4-7 percent).

In the baseline setting above we look at transitions from potentially short spells of unemployment since we define unemployed individuals as those that were registered as unemployed for at least one day during a year. As a sensitivity analysis, we look at transitions from longer unemployment spells defined as those that at those that were registered as unemployed for at least 60 days during a year. The results are presented in Table 5 and indicate that there is no effect of the 2001 reform on hires from longer spells of unemployment. It should also be noted that the share of workers hired from longer spells of unemployment is at a lower level initially (Table A8 in the Appendix).

The standard errors are clustered at the firm level in our baseline setting. Since we have a single reform and a single threshold that defines treatment and control group, we may have group error structures that could lead to underestimation of our standard errors, as described by Moulton (1986). To address this potential problem, we collapse the data to yearly means for each year and treatment assignment. Using the remaining 16 data points, we estimate the DiD for the three different outcomes using only year dummies as additional covariates. The results are presented in Table A9. The estimated coefficients and the standard errors are similar in size to those in our baseline setting (see model 1, Table 3).

3.1.2 Heterogeneity According to Firm Size and Educational Level

In order to investigate the effect of the 2001 reform on firms of different sizes within the treatment group, we estimate the following equation:

$$\mathrm{Y_{it}} = \propto + \tau_{\mathrm{t}} + \sum_{s=5}^{10} \chi_s \, \mathrm{Size}_{is99} + \sum_{s=5}^{10} \beta_s (\mathrm{Size}_{is99} \times \mathrm{Post}_t) + \mathrm{X}_{i99}\gamma + \varepsilon_{\mathrm{it}}, \quad (5)$$

where Size_{is99} is a dummy variable for a firm of size s in 1999, β_s is the coefficient for the DiD estimate that interacts Size_{is99} with the post-reform dummy. The firms in the control group are used as benchmark for each of the six size categories in the treatment group. The estimated β_s are shown in Figure 4 and presented in detail in Table A10 in the Appendix. The estimated coefficients for hires from unemployment are somewhat larger for the smaller firms (Figure 4a). For hires from preparatory programs, the estimated coefficient is statistically significant for the smallest firms of sizes 5–6. The estimate for firms of size 5 corresponds to a 12 percent increase of the pre-reform share. For hires from subsidized programs, firms in size categories 10 and 7, display the largest effect. However, when we exclude firms of size 10–11, just around the threshold, we see that also for hires from subsidized programs, the estimates for the smaller firms of sizes 6–7 are greater in magnitude and statistically significant (Figure A2).

We proceed by describing the educational level of workers that were hired from unemployment and ALMPs.[18] To be able to trace educational levels across firms and individuals we use individual-level data that are not aggregated to the firm level. Figure A3, in the Appendix, shows the distribution of the highest obtained educational level for workers that were hired from unemployment and ALMPs, respectively. The distribution for workers hired from unemployment has thicker tails, indicating a larger share of workers with both low and high levels of education compared to workers that were hired from the two ALMPs. All distributions are slightly left skewed, which indicates that there are more workers with low levels of education than there are workers with high levels of education.

Figure A3 reveals slight differences in the educational levels before and after the reform as well as between the treatment groups and control group. In order to capture these differences, we estimate the DiD on the probability of being in a specific educational category, given that the worker was hired from unemployment or an ALMP. More specifically, the outcome variable is a dummy variable taking the value one if the worker is in one of the educational categories, and the regressions are then run separately for each educational category and employment status (hired

[18] We focus only on workers hired from unemployment and ALMPs. Bjuggren (2018), focuses on all workers and finds no effects of the 2001 reform on the educational level of workers.

from unemployment, hired from preparatory programs, and hired from subsidized employment).[19] Table 6 reveals that there is a positive effect on the probability of having a tertiary education (≤3 years) for workers hired from unemployment and preparatory programs. Hence, the treated firms hired a larger share of workers with some college education after the reform, compared to the control group.[20] There is no effect on any of the other educational categories. In conclusion, the increase in the share of workers hired from unemployment and ALMPs, that we found in the previous section, seems to originate mainly from individuals with some college education.

4. Conclusions

Using a reform in Sweden in 2001, we show that a less stringent EPL made it easier for employers to allow for more uncertainty regarding the productivity of workers in their hiring decisions. Our results indicate that the reform increased the share of workers hired from unemployment by 5-10 percent. We regard this as a quite substantial effect, given that the reform was rather small by international standards. In countries that differentiate EPL by firm size, the differences in stringency are typically much larger than in Sweden (see Venn, 2009). However, we did not detect any effect on the share of workers who transitioned from longer spells of unemployment.

To the best of our knowledge, this is the first study to examine the link between the stringency of EPL and employers' recruitment of previous participants in ALMPs. We find an increase in the share of workers hired from preparatory programs by 5-11 percent. The reform decreased the share of workers hired from subsidized employment, but only for certain firm size categories. These programs are more targeted toward marginal groups than preparatory programs.

For all outcome variables, the magnitudes of the estimated coefficients are greater for smaller firms. This is in line with our expectations. By exempting two workers from the seniority rules, the 2001 reform decreased the share of workers protected from dismissal more in the smaller firms.

---

[19] Educational categories are determined in 1999, as before. We have limited the estimations to educational categories that have at least 500 individuals in the treatment group and in the control group within each employment status. No estimations on workers with a PhD education satisfied this criterion.

[20] Year-specific effects on the probability of having a tertiary education (≤3 years) for workers hired from unemployment and preparatory programs are presented in Figure A4 in the Appendix.

A possible interpretation of our results is that the reform was helpful for alleviating the employment consequences of *moderate* forms of stigma associated with short-term unemployment and participation in preparatory labor market programs. But the reform may not have been far-reaching enough to have any favorable effect on the employment of the long-term unemployed and those in subsidized jobs, for whom the stigma is arguably more severe. The increase in workers hired from short-term unemployment and preparatory labor market programs may have adversely affected the remainder of the labor market, crowding out the employment of some of the most marginal groups. This could explain the observed decrease in hires from subsidized employment. In line with this reasoning, the increase in the share of workers hired from unemployment and ALMPs seems to be driven primarily by those with higher levels of education.

References

Acemoglu, D. and Angrist, J.D. (2001), Consequences of employment protection? The case of the Americans with Disabilities Act, Journal of Political Economy 109, 915-957.

Autor, D.H., Donohue III, J.J., and Schwab, S.J. (2006), The costs of wrongful-discharge laws, Review of Economics and Statistics 88, 211-231.

Baumann, F. (2010), On unobserved worker heterogeneity and employment protection, European Journal of Law and Economics 29, 155-175.

von Below, D. and Skogman Thoursie, P. (2010), Last-in first-out? Estimating the effect of seniority rules in Sweden, Labour Economics 17, 987-997.

Bertola, G. (1999), Microeconomic perspectives on aggregate labor markets, in Ashenfelter, O. and Card, D. (eds.), Handbook of labor economics, vol. 3, Elsevier Science, North-Holland, Amsterdam.

Bertola, G., Blau, F.D. and Kahn, L.M. (2007), Labor market institutions and demographic employment patterns, Journal of Population Economics 20, 833-867.

Bjuggren, C. M. (2018), Employment protection on labor productivity, Journal of Public Economics, 157, 138-157.

Böckerman, P., Skedinger, P. and Uusitalo, R. (2018), Seniority rules, worker mobility and wages: Evidence from multi-country linked employer-employee data, Labour Economics, 51, 48-62.

Bornhäll, A., Daunfeldt, S.-O. and Rudholm, N. (2017), Employment protection legislation and firm growth: evidence from a natural experiment, Industrial and Corporate Change 26, 169-185.

Botero, J.C., Djankov, S., La Porta, R., Lopez-de-Silanes, F. and Shleifer, A. (2004), The regulation of labor, Quarterly Journal of Economics 119, 1339-1382.

Cahuc, P. (2010), Employment protection legislation in Sweden, in Att skapa arbeten – löner, anställningsskydd och konkurrens, SOU 2010:93, Ministry of Finance, Stockholm.

Calmfors, L. (1994), Active labour market policy and unemployment: A framework for the analysis of crucial design features, OECD Economic Studies, no. 22, 7-47.

Confederation of Swedish Enterprise (2009), Tvåundantaget. https://www.svensktnaringsliv.se/migration_catalog/Nyhetsbrev/tvaundantaget_530368.html/BINARY/Tv%C3%A5undantaget?forceDownloadOnId=530368 (accessed June 21, 2018).

Confederation of Swedish Enterprise (2014), Företagarpanelen Q3 Västra Götalands län. https://www.svensktnaringsliv.se/regioner/vastra-gotaland/foretagarpanelen-q3-vastra-gotalands-lan_602366.html/BINARY/F%C3%B6retagarpanelen%20Q3%20V%C3%A4stra%20G%C3%B6talands%20l%C3%A4n?forceDownloadOnId=602366 (accessed June 21, 2018).

Daniel, K. and Siebert, W.S. (2005), Does employment protection reduce the demand for unskilled labour?, International Economic Journal 19, 197-222.

Eriksson, S. and Lagerström, J.(2006), Competition between employed and unemployed job applicants: Swedish evidence, Scandinavian Journal of Economics 108, 373-396.

Feldmann, H. (2009), The unemployment effects of labor regulation around the world, Journal of Comparative Economics 37, 76-90.

Fervers, L. and Schwander, H. (2015), Are outsiders equally out everywhere? The economic disadvantage of outsiders in cross-national perspective, European Journal of Industrial Relations 21, 369-387.

Havnes, T. and Mogstad, M. (2011), No child left behind: Subsidized child care and children's long-run outcomes, American Economic Journal: Economic Policy 3, 97-129.
Heckman, J.J. and Pagés-Serra, C. (2000), The cost of job security regulation: Evidence from Latin American labor markets, Economía 1, 109-144.
Heyman, F. and Skedinger, P. (2016), Employment protection reform, enforcement in collective agreements and worker flows, Industrial Relations 155, 315-337.
Imbens, G.W. and Angrist, J.D. (1994), Identification and estimation of local average treatment effects, Econometrica 62, 467-75.
Kahn, L.M. (2007), The impact of employment protection mandates on demographic temporary employment patterns: International microeconomic evidence, Economic Journal 117, F333-356.
Kawaguchi, D. and Tetsushi, M. (2012), Who bears the cost of the business cycle? Labor-market institutions and volatility of the youth unemployment rate, IZA Journal of Labor Policy 1.
Kugler, A. D. (2004), The effect of job security regulations on labor market flexibility. Evidence from the Colombian labor market reform, in Heckman, J.J. and Pagés, C. (eds.), Law and employment: Lessons from Latin America and the Caribbean, University of Chicago Press.
Kugler, A.D. and Pica, G. (2006), The effects of employment protection and product market regulations on the Italian labour market, in Messina, J., Michelacci, C., Turunen, J. and Zoega, G. (eds.), Labour market adjustments in Europe, Edward Elgar, Cheltenham and Northampton, MA.
Kugler, A.D. and Saint-Paul, G. (2004), How do firing costs affect worker flows in a world with adverse selection?, Journal of Labor Economics 22, 553-584.
Lindbeck, A. and Snower, D.J. (2001), Insiders versus outsiders, Journal of Economic Perspectives 15, 165-188.
MacLeod, B. and Nakavachara, V. (2007), Can wrongful discharge law enhance employment?, Economic Journal 117, F218-F278.
Montenegro, C. E. and Pagés, C. (2004), Who benefits from labor market regulations? Chile, 1960–1998, in Heckman, J.J. and Pagés, C. (eds.), Law and employment: Lessons from Latin America and the Caribbean, University of Chicago Press.
Moulton, B.R. (1986), Random group effects and the precision of regression estimates, Journal of Econometrics 32, 385-397.
OECD (2013), Employment Outlook, Paris.
Olsson, M. (2009), Employment protection and sickness absence, Labour Economics 16, 208-214.
Olsson, M. (2017), Direct and cross effects of employment protection - the case of parental childcare, Scandinavian Journal of Economics 119, 1105-1128.
Runeson, C. and Bergeskog, A. (2003), Arbetsmarknadspolitisk översikt 2000, Rapport 2003:2, Institute for Labour Market Policy Evaluation, Uppsala.
Skedinger, P. (1995), Employment policies and displacement in the youth labour market, Swedish Economic Policy Review 2, 137-171.
Skedinger, P. (2010), Employment protection legislation: Evolution, effects, winners and losers, Edward Elgar, Cheltenham and Northampton, MA.
Statistics Sweden (2009), Longitudinell integrationsdatabas för sjukförsäkrings- och arbetsmarknadsstudier (LISA) 1990–2007. Arbetsmarknads- och utbildningsstatistik, 2009:1, Stockholm.

Venn, D. (2009), Legislation, collective bargaining and enforcement: updating the OECD employment protection indicators", Working Paper No. 89, Directorate for Employment, Labour and Social Affairs, OECD, Paris.

Tables

Table 1. Transitions from different states

Transition	Definition
Hired from unemployment	Employed at t, and unemployed at $t-1$.
Hired from ALMP–preparatory programs	Employed at t, and in a preparatory program at $t-1$.
Hired from ALMP–subsidized employment	Employed at t, and in subsidized employment at $t-1$.

Table 2. Summary statistics for treatment and control, before and after the reform (means)

	Treatment group		Control group		DiD
	Before	After	Before	After	
Hired from	0.0464	0.0453	0.0455	0.0423	0.0021
unemployment	(0.0861)	(0.0797)	(0.0735)	(0.0668)	
Hired from ALMP –	0.0148	0.0074	0.0150	0.0069	0.0007
preparatory programs	(0.0448)	(0.0296)	(0.0366)	(0.0230)	
Hired from ALMP –	0.0084	0.0046	0.0076	0.0040	-0.0003
subsidized employment	(0.0369)	(0.0243)	(0.0293)	(0.0193)	
Observations (1997-2004)	87,848	70,115	31,708	26,192	
Underlying variable					
Firm size	7.646	9.337	12.90	15.21	-0.619
	(9.600)	(11.69)	(17.52)	(20.12)	
Observations (1996-2004)	102,507	70,115	38,116	26,192	

Notes: Standard deviation in parentheses. All variables except firm size refer to the share of workers within each firm. DiD is the change in the treatment group minus the change in the control group.

Table 3. Effect of the reform on the share of workers hired from unemployment and ALMPs

	Hired from unemployment			*Hired from preparatory programs*		
	(1)	(2)	(3)	(1)	(2)	(3)
ITT	0.00249***	0.00259***	0.00236***	0.000816***	0.000780**	0.000889***
	(0.000671)	(0.000653)	(0.000649)	(0.000314)	(0.000314)	(0.000313)
% of pre-reform share	5%	6%	5%	6%	5%	6%
LATE	0.00469***	0.00487***	0.00444***	0.00154***	0.00147**	0.00167***
	(0.00126)	(0.00123)	(0.00122)	(0.000592)	(0.000591)	(0.000589)
% of pre-reform share	10%	10%	10%	10%	10%	11%
Observations	215,863	215,863	215,863	215,863	215,863	215,863
	Hired from subsidized employment					
	(1)	(2)	(3)			
ITT	-0.000439*	-0.000432*	-0.000438*			
	(0.000255)	(0.000255)	(0.000255)			
% of pre-reform share	-5%	-5%	-5%			
LATE	-0.00110**	-0.00108*	-0.00115**			
	(0.000555)	(0.000554)	(0.000553)			
% of pre-reform share	-13%	-13%	-14%			
Observations	215,863	215,863	215,863			
Covariates	Year FE	Year FE Industry FE	Year FE Industry FE Region FE Industry × Year FE	Year FE	Year FE Industry FE	Year FE Industry FE Region FE Industry × Year FE

Notes: Difference-in-difference estimates, corresponding to the coefficient β in equation (1) for ITT and (2) for LATE. Robust standard errors within parentheses, clustered on firms. Each column corresponds to separate estimations for ITT and LATE. % of pre-reform share corresponds to the ratio of the estimated coefficient to the pre-reform share for the treatment group. Pre-reform shares are reported in Table 2.

*** p<0.01, ** p<0.05, * p<0.1

Table 4. Effect of the reform excluding firms around the threshold

	Hired from unemployment		Hired from preparatory programs		Hired from subsidized employment	
	Excluding firms of size		*Excluding firms of size*		*Excluding firms of size*	
	10-11	9-12	10-11	9-12	10-11	9-12
ITT	0.00186**	0.00188**	0.00109***	0.00127***	-0.000693**	-0.00114***
	(0.000729)	(0.000833)	(0.000350)	(0.000398)	(0.000281)	(0.000326)
% of pre-reform share	4%	4%	7%	9%	-8%	-14%
LATE	0.00303**	0.00277**	0.00177***	0.00187***	-0.00113**	-0.00168***
	(0.00119)	(0.00123)	(0.000571)	(0.000587)	(0.000458)	(0.000480)
% of pre-reform share	7%	6%	12%	13%	-13%	-20%
Observations	183937	150480	183937	150480	183937	150480
Covariates	Year FE	Year FE	Year FE	Year FE	Year FE	Year FE
	Industry FE	Industry FE	Industry FE	Industry FE	Industry FE	Industry FE
	Region FE	Region FE	Region FE	Region FE	Region FE	Region FE
	Industry × Year FE	Industry × Year FE	Industry × Year FE	Industry × Year FE	Industry × Year FE	Industry × Year FE

Notes: Difference-in-difference estimates, corresponding to the coefficient β in equation (1) for ITT and (2) for LATE. Robust standard errors within parentheses, clustered on firms. Each column corresponds to separate estimations for ITT and LATE. % of pre-reform share corresponds to the ratio of the estimated coefficient to the pre-reform share for the treatment group. Pre-reform shares are reported in Table A7 in the Appendix.
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Effect of the reform on the share of workers hired from unemployment using longer unemployment spells

	Hired from unemployment		
	(1)	(2)	(3)
ITT	0.000187	0.000185	0.000185
	(0.000182)	(0.000182)	(0.000181)
LATE	0.000353	0.000347	0.000348
	(0.000343)	(0.000342)	(0.000341)
Observations	215,863	215,863	215,863
Covariates	Year FE	Year FE Industry FE	Year FE Industry FE Region FE Industry × Year FE

Notes: Difference-in-difference estimates, corresponding to the coefficient β in equation (1) for ITT and (2) for LATE. Robust standard errors within parentheses, clustered on firms. Each column corresponds to separate estimations for ITT and LATE.
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Effect of the reform by educational level

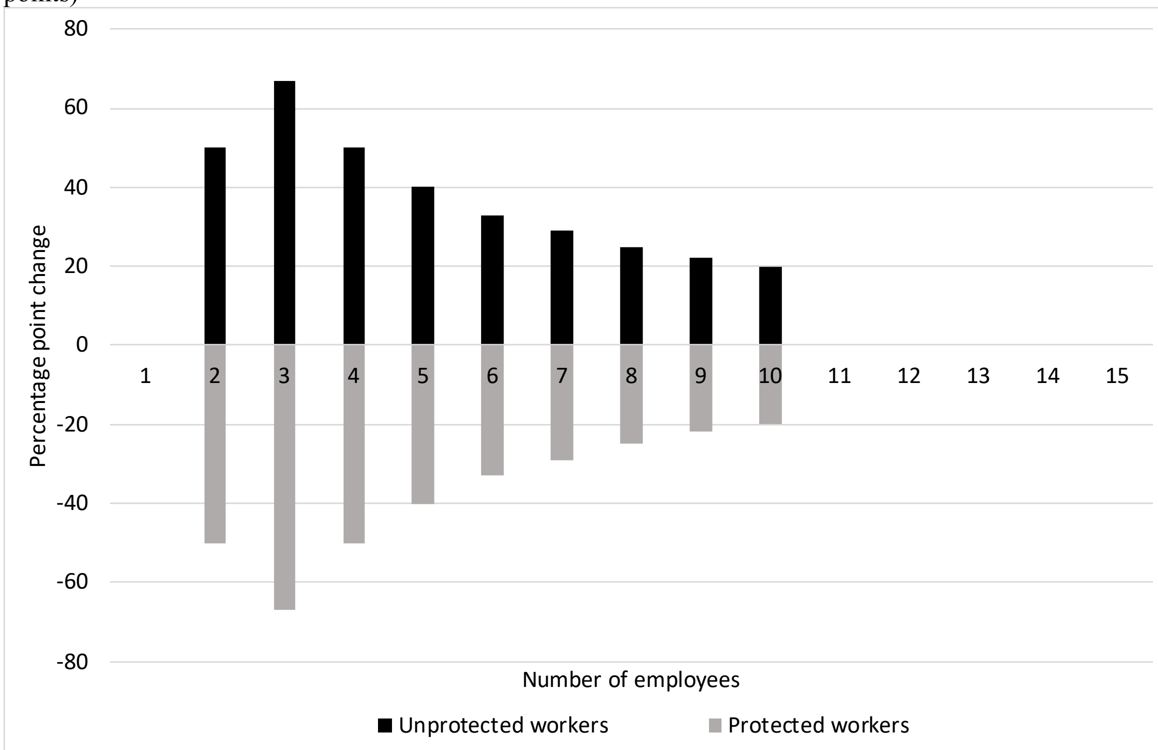
	Educational category					
	Compulsory school (< 9 years)	Compulsory school (9 or 10 years)	Senior high school (≤2 years)	Senior high school (>2 years)	Tertiary education (≤3 years)	Tertiary education (>3 years)
Hired from unemployment						
ITT	-0.00274	-0.00109	-0.0166	0.00191	0.0148**	0.00364
	(0.00465)	(0.00747)	(0.0125)	(0.0102)	(0.00657)	(0.00449)
LATE	-0.00652	-0.00258	-0.0395	0.00455	0.0353**	0.00865
	(0.0110)	(0.0177)	(0.0295)	(0.0243)	(0.0155)	(0.0106)
Observations	118,901	118,901	118,901	118,901	118,901	118,901
Hired from preparatory programs						
ITT	0.0127	0.00160	-0.0245	-0.00479	0.0161**	-
	(0.00789)	(0.0103)	(0.0156)	(0.0128)	(0.00756)	
LATE	0.0298	0.00375	-0.0575	-0.0112	0.0377**	-
	(0.0184)	(0.0240)	(0.0365)	(0.0299)	(0.0176)	
Observations	37,526	37,526	37,526	37,526	37,526	
Hired from subsidized employment						
ITT	-	-0.00345	-0.0159	0.00802	-	-
		(0.0129)	(0.0185)	(0.0141)		
LATE	-	-0.00708	-0.0326	0.0165	-	-
		(0.0263)	(0.0377)	(0.0287)		
Observations		18,791	18,791	18,791		

Notes: Difference-in-difference estimates on the probability of being in a specific educational category. Robust standard errors within parentheses, clustered on firms. Each column corresponds to separate estimations for ITT and LATE. Estimations are limited to educational categories that have at least 500 individuals in the treatment group and in the control group within each employment status. The most saturated model with all covariates is used for all estimations.

*** p<0.01, ** p<0.05, * p<0.1

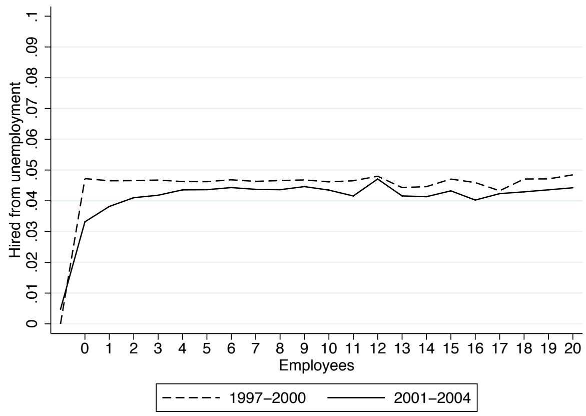
Figures

Figure 1. Change in the number of protected and unprotected workers after the 2001 reform (percentage points)

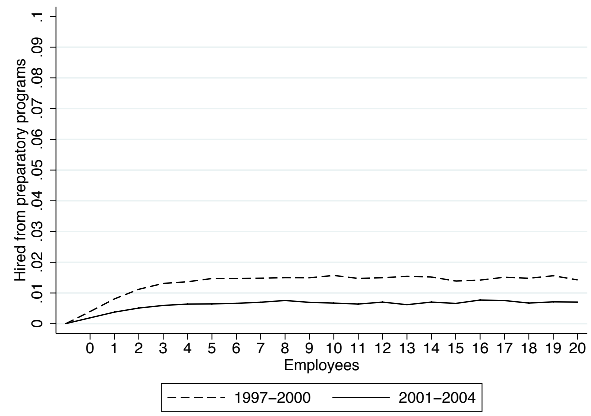


Notes: The calculations are based on the assumptions that only one worker is laid off and that all workers are on open-ended contracts.

Figure 2. Outcome variables and number of employees



(a) Share of workers hired from unemployment

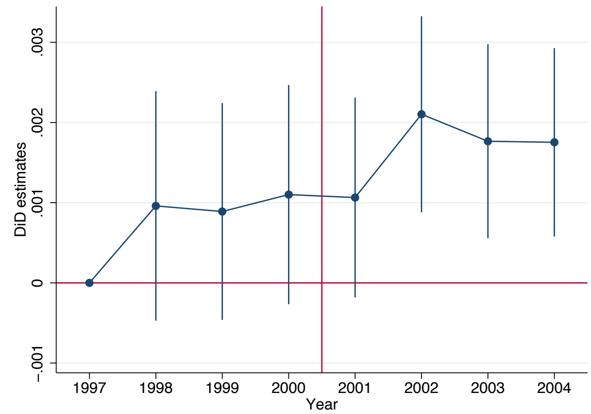


(b) Share of workers hired from preparatory programs

(c) Share of workers hired from subsidized employment

Figure 3. Annual DiD effects of the 2001 reform

(a) Share of workers hired from unemployment

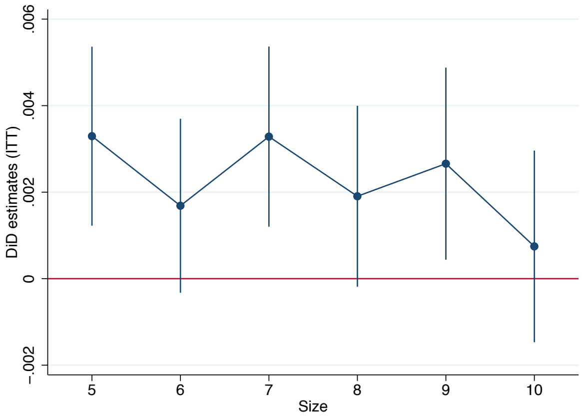


(b) Share of workers hired from preparatory programs

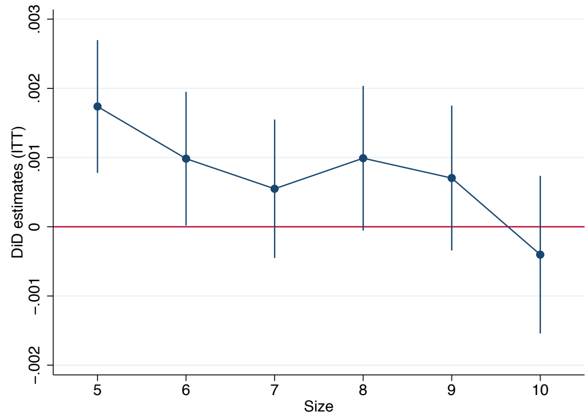
(c) Share of workers hired from subsidized employment

Notes: The DiD estimates on the y-axis are the estimated coefficients β_t in equation (4). The year 1997 is used as baseline. The vertical lines are 95 percent confidence intervals.

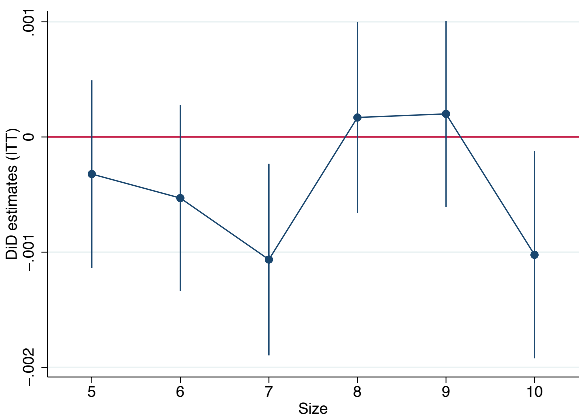
Figure 4. Effects of the 2001 reform, by firm size



(a) Share of workers hired from unemployment



(b) Share of workers hired from preparatory programs



(c) Share of workers hired from subsidized employment

Notes: The DiD estimates on the y-axis are the estimated coefficients β_S in equation (5). The control group, firms of size 11-15, is used as baseline. The vertical lines are 95 percent confidence intervals.

Appendix

Table A1. Yearly averages – share of workers

Year	Hired from unemployment	Hired from preparatory programs	Hired from subsidized employment
1997	0.037	0.012	0.012
1998	0.046	0.016	0.007
1999	0.048	0.016	0.007
2000	0.051	0.014	0.008
2001	0.047	0.009	0.006
2002	0.046	0.008	0.003
2003	0.041	0.007	0.004
2004	0.043	0.005	0.004

Notes: The variables refer to the share of workers within each firm. The averages include both treatment and control groups.

Table A2. Yearly averages – individuals

Year	Hired from unemployment	Hired from preparatory programs	Hired from subsidized employment
1997	9,208	2,997	2,705
1998	13,278	4,478	1,916
1999	16,112	5,514	2,254
2000	17,774	4,630	2,451
2001	14,997	2,810	1,815
2002	14,525	2,377	992
2003	12,250	2,151	1,091
2004	12,765	1,566	1,000

Notes: The variables refer to the number of individuals who transitioned from unemployment or ALMPs. The averages include both treatment and control groups.

Table A3. First stage equation on the DiD estimator D_{it}

	Model (1)	Model (2)	Model (3)
Z_{it}	0.53118***	0.53162***	0. 53151***
	(0.004826)	(0.004818)	(0.004825)
F-statistics (robust)	12115.7	12174	12135.1
Adjusted R^2	0.6001	0.6030	0.6038
Partial R^2	0.1461	0.1473	0.1473
Shea's adj. partial R^2	0.1461	0.1464	0.1460
Observations	215,863	215,863	215,863
Covariates	Year FE	Year FE Industry FE	Year FE Industry FE Region FE Industry × Year FE

Notes: First stage estimations on D_{it} from equation (3). Robust standard errors within parentheses, clustered on firms.
*** p<0.01, ** p<0.05, * p<0.1

Table A4. Summary statistics for treatment and control, before and after the reform (means) – allowing for overlap between the different treatment categories

	Treatment group		Control group		DiD
	Before	After	Before	After	
Hired from unemployment	0.0512	0.0473	0.0502	0.0441	0.0022
	(0.0905)	(0.0814)	(0.0771)	(0.0683)	
Hired from ALMP – preparatory programs	0.0218	0.00986	0.0216	0.00915	0.00051
	(0.0549)	(0.0342)	(0.0452)	(0.0269)	
Hired from ALMP – subsidized employment	0.0115	0.00549	0.0105	0.00480	-0.0003
	(0.0433)	(0.0267)	(0.0347)	(0.0211)	
Observations (1997-2004)	87,848	70,115	31,708	26,192	
Underlying variable					
Firm size	7.646	9.337	12.90	15.21	-0.619
	(9.600)	(11.69)	(17.52)	(20.12)	
Observations (1996-2004)	102,507	70,115	38,116	26,192	

Notes: Standard deviation in parentheses. All variables except firm size refer to the share of workers within each firm. DiD is the change in the treatment group minus the change in the control group.

Table A5. Effect of the reform on the share of workers hired from unemployment and ALMPs – allowing for overlap between the different treatment categories

	Hired from unemployment			*Hired from preparatory programs*		
	(1)	(2)	(3)	(1)	(2)	(3)
ITT	0.00245***	0.00254***	0.00236***	0.000627*	0.000585	0.000694*
	(0.000695)	(0.000677)	(0.000673)	(0.000379)	(0.000379)	(0.000377)
% of pre-reform share	5%	5%	5%	3%	3%	3%
LATE	0.00462***	0.00478***	0.00443***	0.00118*	0.0011	0.00131*
	(0.00131)	(0.00128)	(0.00127)	(0.000714)	(0.000713)	(0.000709)
% of pre-reform share	9%	9%	9%	5%	5%	6%
Observations	215,863	215,863	215,863	215,863	215,863	215,863
	Hired from subsidized employment					
	(1)	(2)	(3)			
ITT	-0.000584**	-0.000574*	-0.000613**			
	(0.000295)	(0.000295)	(0.000294)			
% of pre-reform share	-5%	-5%	-5%			
LATE	-0.00110**	-0.00108*	-0.00115**			
	(0.000555)	(0.000554)	(0.000553)			
% of pre-reform share	-10%	-9%	-10%			
Observations	215,863	215,863	215,863			
Covariates	Year FE	Year FE Industry FE	Year FE Industry FE Region FE Industry × Year FE	Year FE	Year FE Industry FE	Year FE Industry FE Region FE Industry × Year FE

Notes: Difference-in-difference estimates, corresponding to the coefficient β in equation (1) for ITT and (2) for LATE. Robust standard errors within parentheses, clustered on firms. Each column corresponds to separate estimations for ITT and LATE. % of pre-reform share corresponds to the ratio of the estimated coefficient to the pre-reform share for the treatment group. Pre-reform shares are reported in Table A1.
*** p<0.01, ** p<0.05, * p<0.1

Table A6. Year-specific effects

Variable	Hired from unemployment	Hired from preparatory programs	Hired from subsidized employment
$\tau_{1998} \times d_{i99}$	0.00199	0.00096	-0.00049
	(0.00126)	(0.00073)	(0.00064)
$\tau_{1999} \times d_{i99}$	0.00182	0.00089	-0.00022
	(0.00122)	(0.00069)	(0.00062)
$\tau_{2000} \times d_{i99}$	0.00073	0.00110	-0.00021
	(0.00126)	(0.00070)	(0.00066)
$\tau_{2001} \times d_{i99}$	0.00258**	0.00106*	-0.00047
	(0.00125)	(0.00064)	(0.00064)
$\tau_{2002} \times d_{i99}$	0.00339***	0.00210***	-0.00079
	(0.00128)	(0.00062)	(0.00058)
$\tau_{2003} \times d_{i99}$	0.00493***	0.00177***	-0.00056
	(0.00126)	(0.00062)	(0.00059)
$\tau_{2004} \times d_{i99}$	0.00349***	0.00175***	-0.00092
	(0.00129)	(0.00060)	(0.00059)
Observations	215,863	215,863	215,863
Covariates	Year FE	Year FE	Year FE
	Industry FE	Industry FE	Industry FE
	Region FE	Region FE	Region FE

Notes: Year-specific effects from equation (4) corresponding to the coefficients β_t. Robust standard errors within parentheses, clustered on firms.
*** p<0.01, ** p<0.05, * p<0.1

Table A7. Excluding firms around the threshold – summary statistics for treatment and control, before and after the reform (means)

	Excluding firms of size 10-11				Excluding firms of size 9-12			
	Treatment group		Control group		Treatment group		Control group	
	Before	After	Before	After	Before	After	Before	After
Hired from unemployment	0.0465	0.0456	0.0454	0.0429	0.0466	0.0457	0.0445	0.0418
	(0.0872)	(0.0809)	(0.0725)	(0.0664)	(0.0882)	(0.0816)	(0.0709)	(0.0639)
Hired from ALMP – preparatory programs	0.0147	0.0074	0.0151	0.0070	0.0146	0.0074	0.0151	0.0068
	(0.0452)	(0.0300)	(0.0361)	(0.0227)	(0.0458)	(0.0303)	(0.0352)	(0.0221)
Hired from ALMP – subsidized employment	0.0084	0.0046	0.0073	0.0041	0.0086	0.0048	0.0070	0.0042
	(0.0372)	(0.0248)	(0.0278)	(0.0196)	(0.0383)	(0.0254)	(0.0274)	(0.0197)
Observations (1997-2004)	78,424	62,274	23,663	19,576	67,177	52,994	16,530	13,779

Table A8. Summary statistics for treatment and control, using longer unemployment spells (means)

	Treatment group		Control group		DiD
	Before	After	Before	After	
Hired from longer unemployment spells	0.00467	0.00313	0.00467	0.00295	0.00018
	(0.0248)	(0.0186)	(0.0207)	(0.0148)	
Observations (1997-2004)	87,848	70,115	31,708	26,192	

Notes: Standard deviation in parentheses. DiD is the change in the treatment group minus the change in the control group.

Table A9. Effect of the reform using yearly means

	Hired from unemployment	Hired from preparatory programs	Hired from subsidized employment
ITT	0.00261**	0.000863**	-0.000452**
	(0.000721)	(0.000336)	(0.000721)
Observations	16	16	16
Covariates	Year FE	Year FE	Year FE

Notes: Robust standard errors within parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A10. Size-specific effects

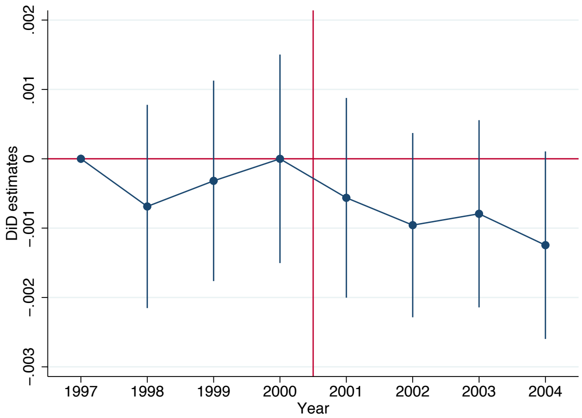
	Hired from unemployment	Hired from preparatory programs	Hired from subsidized employment
Int_Size_5	0.00329***	0.00174***	-0.00032
	(0.00106)	(0.00049)	(0.00042)
Int_Size_6	0.00169	0.00098**	-0.00053
	(0.00103)	(0.00049)	(0.00041)
Int_Size_7	0.00328***	0.00055	-0.00106**
	(0.00106)	(0.00051)	(0.00042)
Int_Size_8	0.00191*	0.00099*	0.00017
	(0.00107)	(0.00053)	(0.00042)
Int_Size_9	0.00266**	0.00070	0.00020
	(0.00113)	(0.00053)	(0.00041)
Int_Size_{10}	0.00075	-0.00040	-0.00102**
	(0.00113)	(0.00058)	(0.00046)
Observations	215,863	215,863	215,863
Covariates	Year FE	Year FE	Year FE
	Industry FE	Industry FE	Industry FE
	Region FE	Region FE	Region FE
	Industry × Year FE	Industry × Year FE	Industry × Year FE

Notes: Int_Size_s denotes the interaction $(\text{Size}_{is99} \times \text{Post}_t)$ in equation (5) and the estimated coefficients corresponds to β_s. Robust standard errors within parentheses, clustered on firms.
*** p<0.01, ** p<0.05, * p<0.1

Figure A1. Annual DiD effects of the 2001 reform – allowing for overlap between the different treatment categories

(a) Share of workers hired from unemployment

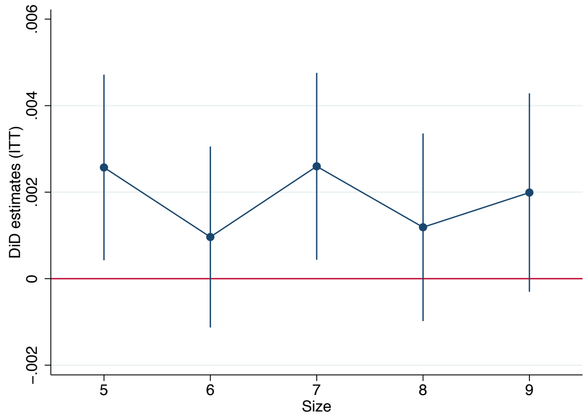
(b) Share of workers hired from preparatory programs



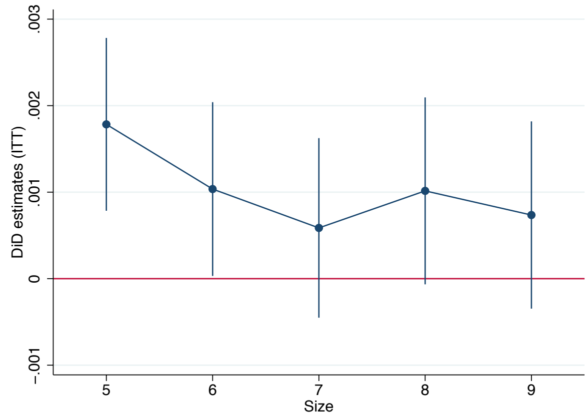
(c) Share of workers hired from subsidized employment

Notes: The DiD estimates on the y-axis correspond to the coefficients β_t in equation (4). The year 1997 is used as baseline. The vertical lines are 95 percent confidence intervals.

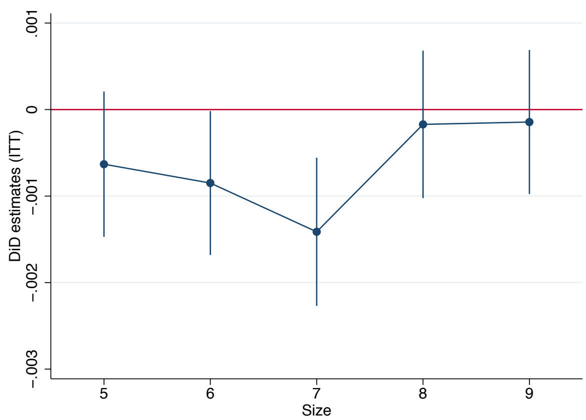
Figure A2. Effects of the 2001 reform, by firm size - excluding firms of size 10-11



(a) Share of workers hired from unemployment



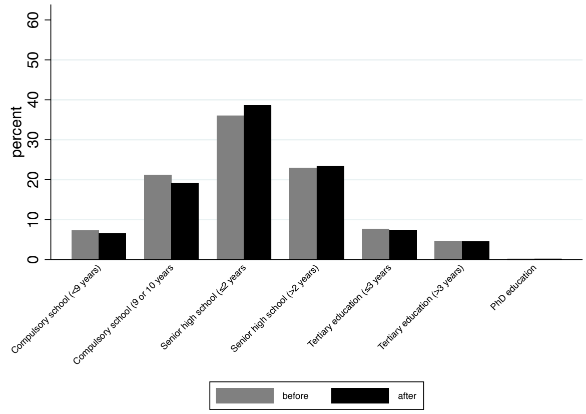
(b) Share of workers hired from preparatory programs



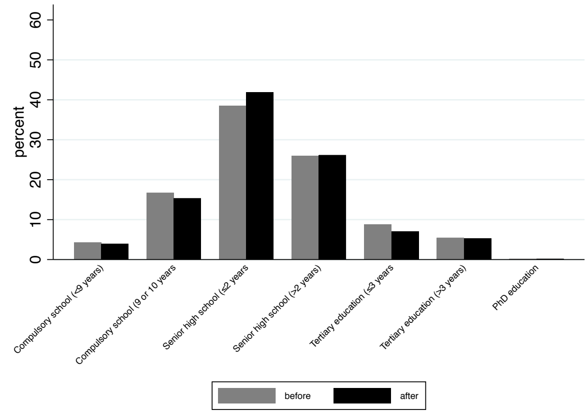
(c) Share of workers hired from subsidized employment

Notes: The DiD estimates on the y-axis are the estimated coefficients β_t in equation (5). The control group, firms of size 12-15, is used as baseline. The vertical lines are 95 percent confidence intervals.

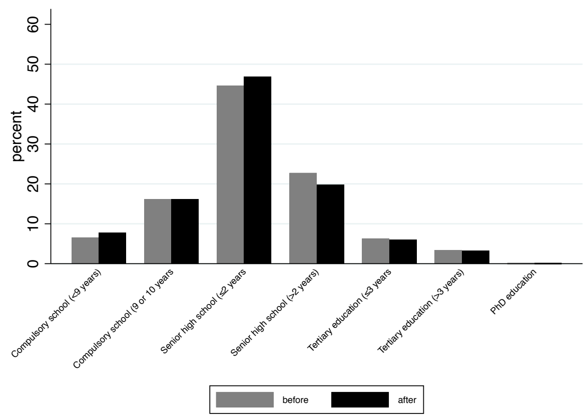
Figure A3. Distributions of educational level before and after the 2001 reform



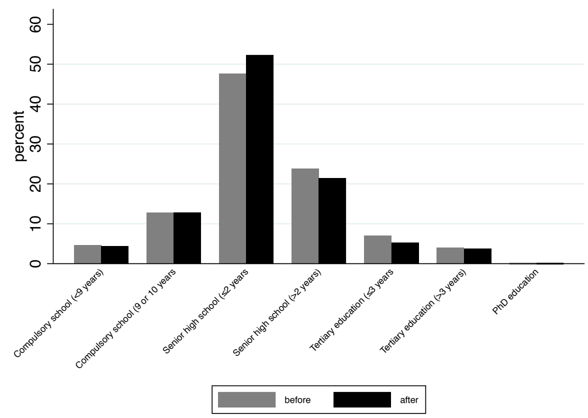
(a) Workers hired from unemployment – treatment group



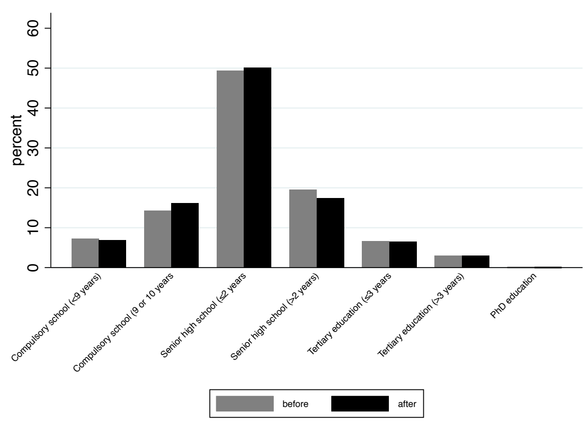
(b) Workers hired from unemployment – control group



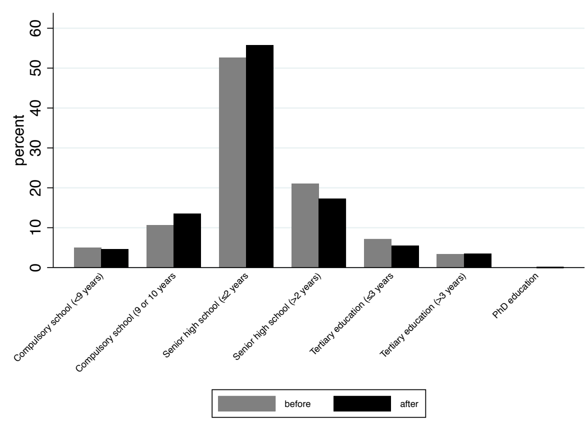
(c) Workers hired from preparatory programs – treatment group



(d) Workers hired from preparatory programs – control group



(e) Workers hired from subsidized employment – treatment group



(f) Workers hired from subsidized employment – control group

Figure A4. Effects of the 2001 reform, on the probability of having some college education (tertiary education, ≤3 years)

(a) Workers hired from unemployment

(b) Workers hired from preparatory programs

Notes: The DiD estimates on the y-axis correspond to the coefficients β_t in equation (4). The year 1997 is used as baseline. The vertical lines are 95 percent confidence intervals.