

Firing costs and job loss: The case of the Italian Jobs Act

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Abstract

A recent reform in the Italian labour market has modified the characteristics of the permanent contract by reducing firing costs. By using a discontinuity in the application of the reform, which only involved firms with more than 15 employees, we evaluate the effect of lowering firing costs on the probability of being still employed a year and a half later. We find that the job survival probability is not lower for the treated and even significantly higher in some cases. This result apparently contradicts theoretical predictions and does not support the common feeling of a higher firing probability for permanent workers hired after the reform, but it can be explained with firms recruiting riskier workers because of lower firing costs.

KEYWORDS: Deregulation, Employment Protection Legislation, Graded Security, Open-Ended Contracts

JEL CODES: J23, J30, J41

1 Introduction

The Italian labour market has been traditionally characterised by a strong protection against dismissal of permanent workers. The employment protection legislation has generated a harsh and continuous debate between unions, entrepreneurs and governments. The focal point of the debate is Article 18 of the Workers Statute,¹ which compelled the employer, in firms with 15 employees or more, to reinstate the worker or to a compensation of 15 months pay following an unfair dismissal certified by a court ruling. A modification of Article 18 in 1990 provided the possibility of reinstatement after 3 days alternatively to a lower severance payment for firms with less than 15 employees,² while firms with 15 employees or more had to reinstate the worker in case of certified unfair dismissal.

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¹Art. 18 of Law no. 300 of May 20th 1970, known as Workers Statute (Statuto dei Lavoratori).

²Law no. 108 of May 11th 1990 established severance payments between 2.5 and 6 months pay under 10 years for tenure, up to 10 for workers with 10 to 20 years of tenure, 14 for more than 20 years of tenure.

Nonetheless, in 2013 the OECD still accounted Italy as one of the countries with highest degree of protection of permanent workers against individual and collective dismissals (OECD, 2013).

In an attempt to loosen the employment protection legislation, the Law no. 183 of December 20th 2014, also known as the Jobs Act, defined a new type of contract for permanent workers hired in firms with 15 employees or more, known as *increasing protection contract* (contratto a tutele crescenti, IPC hereafter). The new open-ended contract limits reinstatement to discriminatory and very specific disciplinary dismissals, thereby excluding from unfair dismissals those occurring for economic reasons. It also introduces a compulsory severance payment in case of layoff, which is flat and equal to 4 months pay for the first two years of service and then proportional to tenure with a maximum of 24 months pay. This new type of contract reduces not only the expected amount of firing costs but also their uncertainty, because the cost faced by firms in case of dismissals is no longer subject to the arbitrariness of court decisions (Sestito and Viviano, 2018). Workers were hired under the new IPC starting March 7th 2015. As a matter of fact, the Jobs Act was a broader reform that introduced also other important changes in the Italian labour market legislation. In particular, a hiring subsidy was applied to all new contracts starting January 1st 2015.

The reduction of firing costs brought by the Jobs Act was not well received by unions and, in general, it was harshly criticised in the media. According to the Secretary of the Italian main union (CGIL) the Jobs Act “liberalises layoffs and makes the permanent contract precarious”.³ This opinion finds supporters in the Italian system of industrial relations and in the political arena. Unions and left parties demanded an abrogative referendum, that the Constitutional Court has in fact declared inadmissible.

The aim of this paper is to evaluate if the new IPC introduced by the Jobs Act made permanent contracts more precarious, by investigating whether the reform decreased the probability of still being employed in the same firm 600 days after being hired. Our identification strategy exploits the firm size threshold of 15 employees entailed by the reform, with workers hired in firms with more than 15 employees after March 7th representing the treated group. The empirical analysis is based on the administrative data LOSAI, released by the Ministry of Labour and Social Policies together with the National Social Insurance Agency (INPS). The database contains working histories on a sample of Italian workers up to December 2017.⁴

From the theoretical point of view, a stricter employment protection legislation implies that the optimal strategy for the firm is to reduce both hirings and separations

³Audition at the Chamber of Deputies, Labour Commission, January 27th 2015.

⁴The dataset is available at <https://www.cliclavoro.gov.it/Barometro-Del-Lavoro/Pagine/Microdati-per-la-ricerca.aspx>.

(Ljungqvist, 2002), or insignificantly increase firms marginal propensity to hire (Bentolila and Bertola, 1990). The overall effect on employment is however ambiguous, other than a clear reduction of job mobility (Cahuc and Postel-Vinay, 2002; Autor et al., 2007). Moreover, with higher firing costs, firms may prefer hiring employed workers, who are already screened and therefore less likely to be lemons (Kugler and Saint-Paul, 2004). Therefore it may be conjectured that lowering firing costs may increase firms propensity to hire permanent workers characterised by a lower average and a greater variance in their expected productivity.

Treated workers are therefore less costly to dismiss and are expected to be less productive, which puts them at a higher risk of contract termination. Furthermore, in the specific case of the Jobs Act, employees are aware that seeking a better job position after March 7th 2015 implies giving up the old contract (regulated by Article 18) for the new deregulated one. This could translate into a lower willingness to seek a new job and into a reduction of voluntary resignations. Workers hired after March 7th in firms with 15 employees or more should therefore face a lower employment survival probability compared to untreated workers.

Because the Italian Jobs Act is a very recent reform, the empirical literature evaluating its effects on labour demand is still limited. The only evidence of the effects of the new firing rules on the employment survival probability is provided by Boeri and Garibaldi (2019). Using firm-level data, they find a significant increase in firings, which amounts to approximately 50% more with respect to the control group.⁵ We argue that focusing only on firings may be misleading, because the Jobs Act may have changed the relative appeal of firing versus voluntary resignation in the case of dismissals for economic reasons.⁶

Further descriptive evidence on the Jobs Act based on aggregate data is provided by Cirillo et al. (2017), who show that the reduction of firing costs did not affect the dynamics of new open-ended contract. By contrast Sestito and Viviano (2018), using employer-employee data for the Veneto region, find an increase in hirings following March 7th 2015 of about 8%. They also find that the reduction of firing costs increased the propensity to offer permanent job positions to workers unknown to the firms that, under the old firing rules, might have preferred to test prospective permanent employees with a temporary position. Effects of changes in firing costs on dismissal probability in Italy have also been evaluated with respect to the modifications of Article 18 in 1990. In particular, Boeri and Jimeno (2005) and Kugler and Pica (2008) found that increasing firing costs for small firms brought a significant decrease in separations (of about 14% according to

⁵Boeri and Garibaldi (2019) also find an increase of about 60% in the hiring rate.

⁶The Jobs Act also changed the modality of resignation, which is now an on-line procedure aimed at eliminating the phenomenon of *white resignations* (dimissioni in bianco). In essence, it is deplorable practice with which some employers force just-hired workers to sign an undated letter of resignation, that the employer can later use to dismiss the employee, thus avoiding to face firing costs.

Kugler and Pica, 2008).

We find that there is no substantial difference in the probability of still being employed 600 days after the job started between the treated and the control group. In some cases, we even find that workers hired with the new IPC have a slightly but significantly higher probability of still being employed after some time. This is in contrast with the empirical results presented by Boeri and Garibaldi (2019) and earlier findings on the effects of firing costs on separations, and contradicts the common feeling of an increased vulnerability of the *new* permanent workers.

Based on the changed composition of hired permanent workers after March 7th, which we find in the data and in line with the results by Sestito and Viviano (2018), we provide a tentative theoretical explanation of this apparently puzzling result based on the different productivity level of workers hired with the two contract types in the medium run. With lower firing costs, because of the higher propensity to recruit unscreened workers, new permanent employees have a more volatile productivity distribution than those hired with the old contract. For this reason, in presence of a negative shock in the short run, firms will prefer to dismiss riskier workers, namely those hired with lower firing costs and in the left tail of the productivity distribution. However, in the medium-run, the remaining new workers may have a higher productivity level than employees under the old firing rules and, in presence of a negative shock, firms may choose to dismiss the latter, even with higher firing costs.

The rest of the paper is organised as follows: Section 2 describes the data; Section 3 describes the identification strategy; in Section 4 we report and discuss the estimation results along with falsification exercises and robustness checks; Section 5 provides a discussion of the results, a tentative theoretical explanation and partial evidence based on workers age and experience; Section 6 concludes.

2 The LoSai database

The Italian Institute of Social Security (INPS) collects administrative data on the universe of Italian dependent workers. The Italian Ministry of labour and social policies periodically extracts the LOSAI from the administrative archive.⁷ LOSAI contains information on the contracts signed, transformed, renewed and ceased referring to workers born on the 1st and 9th day of each month, that amount to 6.5% of the workers population. For each contract, information refer to the starting date, the ending date (if any), the type of contract, the type of working time arrangement, the hiring and dismissal reasons, and the worker's qualification. Firms and employees characteristics can be matched, such as firm

⁷More information at the *ClicLavoro* web page <https://www.cliclavoro.gov.it/Barometro-Del-Lavoro/Pagine/Microdati-per-la-ricerca.aspx>

size and sector and worker age, gender, and region of residence.⁸ In the empirical analysis, we will also use the worker's years of experience and years of tenure, evaluated at the end of 2014.⁹

We select the permanent contracts signed during the nine weeks before and after the change in firing costs on March 7th, that are all contracts signed between the January 3rd and May 8th 2015.¹⁰ The choice of the 18 weeks window depends on the left threshold: on January 1st, firms could start applying for the hiring subsidy that was also part of the Jobs Act.¹¹ Because this measure can be an important confounder in the analysis of the effects of firing costs, the choice of the window ensures that all the workers, whether in the treated or in the control group, are eligible for the subsidy. In order to keep some degree of homogeneity in our sample, we also focus on small and medium enterprises between 6 and 200 employees.¹² It is also worth mentioning that the information on firm size refers to the average number of employees in a given year.¹³ Our final sample consists of 14,665 full-time permanent contracts.¹⁴

Table 1 reports the frequency of permanent contracts that started in the 9 weeks before and after the implementation of the new firing rules, May 7th 2015, by firm size, as the reform affected only those employing 15 workers or more. In the table and throughout the paper, we refer to firms with less and more than 15 employees as small and large firms, respectively. Table 2 reports averages of workers characteristics before and after the reform, by firm size. After the reform, large firms seem more likely to hire younger, less experienced and unknown workers than small firms. The last column of Table 2 reports the frequency of workers still employed in the same firm 600 days after they are hired, that is our outcome variable of interest. 600 days is the furthest we are able to observe the working histories in LOSAI for those who are hired in the last day of the 18 weeks

⁸Unfortunately, workers education is not available.

⁹Even workers already employed in the same firm or even past employees were eligible to be hired with the new IPC, as long as they were not employed by the firm with an open-ended contract in the 6 months before the reform implementation. This measure was in place so as to avoid workers to be fired only to be re-hired after March 7th with lower firing costs.

¹⁰Weeks start on Saturdays, because the Jobs act entered in force on that day.

¹¹The 2015 stability law introduced a 3-year reduction of social security contributions on permanent employment contracts signed between January 1st to December 31st 2015. For more details about the whole reform package and the timing of the implementation of the different measures, we refer the reader to Sestito and Viviano (2018); Boeri and Garibaldi (2019).

¹²The available information on the class size allows us to distinguish only firms between 101 and 200 from firms with more than 200 employees, which prevents us to use the conventional threshold of 250 employees for medium enterprises.

¹³As argued by Boeri and Garibaldi (2019), the definition of the threshold is nontrivial if this is varying around the time of the reform, especially for firms with a number of employees close to 15.

¹⁴We start with 24,021 in the 18 weeks window, from which we select full-time jobs, we exclude *interim* and on-call work contracts, we leave out those signed in agriculture sector, for which the new firing costs applied in firms with more than 5 employees. We also drop new contracts and terminations resulting from a direct transfer within the same company, contracts started after a leave due to union activities, and contracts ended because of the worker's death. We also drop a few workers who started more than one contract with the same firm in the window considered. Finally, we drop observations for which we had missing workers or firm characteristics.

window. It emerges that this frequency is similar for workers employed in large firms both before and after the reform, and in small firms after May 7th.

Table 1: New contracts before and after March 7th 2015 by firm size

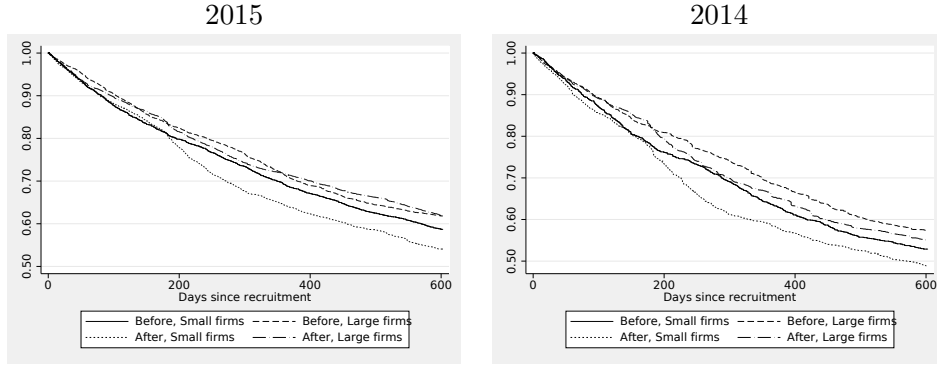
		Contracts	%
Before	Small	3,301	23
Before	Large	4,121	28
After	Small	3,105	21
After	Large	4,138	28
Total		14,665	100

Table 2: Descriptive statistics on workers characteristics before and after March 7th 2015 by firm size

		Women	Age	Experience	Tenure	Employed 600
Before	Small	0.21	40.65	6.84	5.79	0.59
Before	Large	0.20	40.60	7.31	4.77	0.62
After	Small	0.22	41.33	6.67	4.62	0.54
After	Large	0.22	39.94	6.84	4.31	0.61
Total		1.00	40.58	6.93	4.86	0.59

Figure 1 depicts the Kaplan-Meier survival probabilities by the four groups divided according to firm size and reform implementation. The figure in the left panel is based on data referring to the contracts signed between January 1st and May 8th 2015, whereas the figure in the right panel refers to the same window taken in 2014, when no labour market reforms were in place. It emerges that survival probabilities are lower in small firms and workers hired after the job act in small firms seem to have significantly lower survival probabilities. In large firms, noticeable differences between survival probabilities seem to appear around 200 days after recruitment, with treated workers showing a slightly higher survival probability than untreated ones. In 2014 instead, workers hired after March 7th exhibit a lower survival probability than those hired before that date. It can also be noticed that for small firms before after May 7th in both 2014 and 2015 there is a drop in the survival probability about 180 days after signing the contract. This is probably due to the fact that small firms may have hired seasonal workers for the summer with permanent contracts. The sectors of commerce and catering are in fact the main cause for this drop. Finally, a longer negative trend in the length of permanent contracts can represent a reason for the lower survival probability in large firms after May 7th 2014.

Figure 1: Kaplan-Meier survival probability estimates before and after May 7th



3 Identification strategy

The identification of the effect of lower firing costs on the probability of being employed 600 days after being hired is based on a Difference-in-difference approach with repeated cross-sections.

Let h_{it} be a binary variable equal to 1 if worker i hired at time t is still employed in the same firm 600 days after the job started and 0 otherwise. Let us also define D_i as a binary variable equal to 1 if worker i is hired in a firm with 15 employees or more and 0 otherwise, and R_i as a dummy variable equal to 1 if worker i is hired after the reform implementation on March 7th 2015. We set up the following linear regression model

$$h_{it} = \beta_0 + \beta_1 D_i + \beta_2 R_i + \beta_3 D_i \times R_i + \mathbf{x}'_i \boldsymbol{\beta}_4 + \mathbf{z}'_{it} \boldsymbol{\beta}_5 + \varepsilon_{it}, \quad (1)$$

where $D_i \times R_i$ is the binary treatment and β_3 is the average treatment effect on the treated. Vector \mathbf{x}_i contains worker's exogenous or pre-treatment characteristics, such as gender, age, years of experience and years of tenure at the end of 2014, region of residence, and qualification (blue collar, white collar, manager).¹⁵ In addition, we include in \mathbf{x}_i the sector of the firm the worker is employed in. Vector \mathbf{z}_{it} includes controls for the day of the recruitment, that are the day of the week and the day of the month worker i is hired.

Further to the exogeneity of the explanatory variables in (1), the identification of the treatment effect of interests relies on other two key assumptions (see Angrist and Pischke, 2009; Lechner, 2011). One is the *no anticipation* assumption, according to which in the pre-treatment period, the treatment has no effect on the outcome of the pre-treatment population. It is difficult to argue whether there may be anticipatory effects in our setting

¹⁵The available information on the worker's qualification refers to the job considered in the analysis. We consider this information exogenous because, given that the qualification is classified in only 3 categories, switching between these classes due to different firing costs is highly unlikely, especially since firing costs were not differentiated between occupations. We rather use this variable as a proxy of the worker's skill level, in absence of any information on the level of educational attainment.

and, if so, in which direction they affect the outcome of the pre-treatment group. As the policy was announced in December 2014, during the first two months of 2015 prospective employees may have been more likely to look for a job before the reform implementation, when the employment protection legislation was stricter, thereby supposedly increasing their probability of still being employed 600 days later. At the same time, though, firms may hold off hirings until firing costs are lower. In order to check the robustness of our main result to potential anticipatory effects, in Section 4.1 we report an exercise where we build the pre-treatment population by considering workers hired in the first 9 weeks of 2014.

The second identifying assumption is *common* or *parallel* trend assumption, according to which the probability of being employed 600 days after the job started should have the same trend over time for workers hired in small and large firms. This way, the discrepancy between the before-after average differences for the workers employed in small and large firms can be ascribed only to the treatment and not confounded by the different evolution over time of employment survival probability in the two groups. There is no definitive way to test for this assumption. It can be inspected by graphical analysis and supported by the results of the auxiliary regression function proposed by Autor (2003). The results of this exercise are also reported in Section 4.1.

4 Empirical Results

In the following, we first present the main estimation results and falsification exercises aimed at assessing the viability of our identification assumption. Then, we turn to some robustness checks concerning the choice of the time window around the reform implementation and the sample selection based on firm size. The full set of estimation results is available in Appendix.

4.1 Main results and falsification exercises

The estimation results based on the difference-in-difference estimates are reported in Table 3, where the columns correspond to different specifications based on the choice of controls. In contrast with the theoretical prediction, we find that being hired with lower firing costs does not reduce the probability of being employed with the same job 600 days after the contract started, with respect to being hired with a stricter employment protection legislation.

The average outcome probability for workers employed in small firms before March 7th, that is β_0 , is between 0.5 and 0.7 across the specifications considered and differences between large and small firms in the same period, β_1 , disappear when individual and time effects are accounted for. It is also worth noticing that, on average, the difference between

Table 3: Probability of being employed 600 days after recruitment

	(1)	(2)	(3)	(4)
β_0 - Before, Small	0.586*** [0.009]	0.693*** [0.031]	0.522*** [0.057]	0.629*** [0.063]
β_1 - Before, Diff Large vs Small	0.030** [0.011]	0.017 [0.011]	0.011 [0.011]	0.006 [0.011]
β_2 - Small, Diff After vs Before	-0.047*** [0.012]	0.033 [0.022]	-0.033*** [0.012]	0.022 [0.021]
β_3 - Diff-in-Diff (ATET)	0.043*** [0.016]	0.037** [0.016]	0.032** [0.010]	0.027* [0.016]
Time effects	No	Yes	No	Yes
Individual characteristics	No	No	Yes	Yes
Worker qualification	No	No	Yes	Yes
Region of residence	No	No	Yes	Yes
Firm sector	No	No	Yes	Yes
# Observations	14,665	14,665	14,665	14,665

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. Time effects in specifications (2) and (4) include intercepts for the day of the week and the day of the month. Individual characteristics in (3) and (4) include age, age squared, gender, years of experience and years of tenure at the end of 2014. Workers qualification refers to dummy variables for managers, white collars, blue collars (ref. category) and other qualifications. Specifications (3) and (4) also include 19 region fixed effects and 1 digit sector fixed effects.

after and before March 7th in the outcome probability for small firms, β_2 , disappears whenever time effects are controlled for. Finally the ATET, β_3 , is positive and its magnitude is reduced by the inclusion of covariates. According to the results in column (4), workers hired in firms with 15 employees or more have, on average, a probability of still being employed 600 days later that is 2.7 percentage points higher than those hired with a stricter employment protection legislation. This effect is not statistically significant unless we consider a 10% nominal size.

The reliability of this result rests on the identifying assumptions needed for the consistency of the difference-in-difference estimator. As discussed in Section 3, one of these requirements in the *no anticipation* assumption, by which the treatment must not have any affect on the outcome of pre-treatment population. Although the assumption cannot be tested directly, we report the result of a placebo test in Table 4, which can help assess its viability. The first column reports difference-in-difference estimates based on a sample where the pre-treatment period is taken in 2014 (from January 3rd to March 6th), when the reform was not in place nor announced. The results again suggest no effect of the treatment.

For completeness, the second column of Table 4 reports the result of a proper placebo test, where difference-in-difference estimates are based on the same 18 weeks window taken in 2014. As expected, being hired before or after May 7th 2014 makes no difference on the probability of being employed 600 days later, not even between small and large firms.

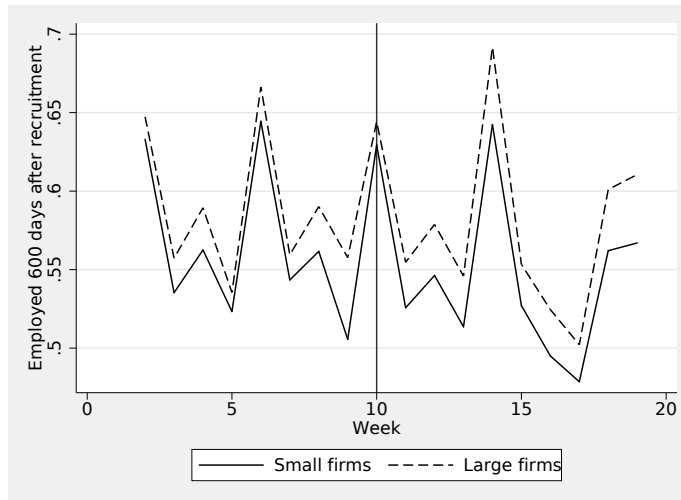
Table 4: Probability of being employed 600 days after recruitment: Placebo tests

	2014/1/3 - 2014/3/6 2015/3/7 - 2015/5/8	2014/1/3 - 2014/3/6 2014/3/7 - 2014/5/8
β_0 - Before, Small	0.550*** [0.067]	0.535*** [0.084]
β_1 - Before, Diff Large vs Small	0.011 [0.014]	0.009 [0.014]
β_2 - Small, Diff After vs Before	0.089*** [0.026]	-0.038 [0.027]
β_3 - Diff-in-Diff (ATET)	0.018 [0.018]	0.008 [0.021]
# Observations	11,955	8,702

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. Both specifications include time effects, individual characteristics, workers qualification, region and 1 digit firm fixed effects (see Table 3 for details).

The second identifying assumption is the *common trend* assumption, according to which the outcome variable in small and large firms before the treatment should share the same evolution over time. One way to check for the presence of a common trend is by performing a graphical analysis. Figure 2 reports the fitted share of workers still employed in the same job position 600 days estimated by means of two separate linear regression models, one for small and one for large firms. Model specifications include time effects, individual characteristics, worker qualification, region and sector fixed effects. From the figure, no relevant differences seem to emerge in the evolution of the share of workers still employed after 600 days between small and large firms before the treatment (nor they do afterwards).

Figure 2: Share of workers still employed 600 days after recruitment: Small and Large firms

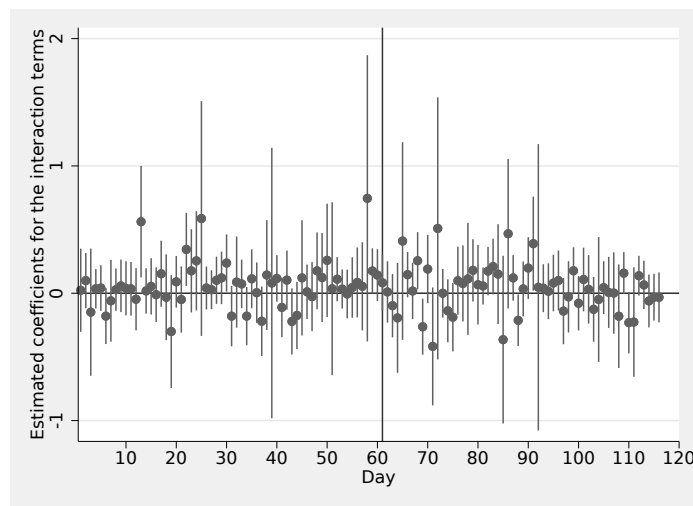


Another common practice used to check for common trends is to follow Autor (2003) and specify the following auxiliary regression

$$\begin{aligned}
 h_{it} = & \gamma_0 + \gamma_1 D_i + \sum_{s=1}^S \gamma_{2s} 1\{t = s\} + \sum_{s=1}^M \gamma_{3s} 1\{t = s\} D_i \\
 & + \sum_{s=M+1}^S \gamma_{4s} 1\{t = s\} D_i + \mathbf{x}'_i \gamma_5 + v_{it},
 \end{aligned} \tag{2}$$

where y_{it} is the outcome variable for worker i hired at time t and the set of regressors contains time dummies and interaction terms between the time dummies and firm size. With the above notation, we separate the interaction effects in the M time occasions before the treatment occurred, γ_{3s} with $s = 1, \dots, M$, and in those after the treatment, γ_{4s} with $s = M + 1, \dots, S$. If the common trend holds, then the null hypothesis $M_0 : \gamma_{31} = \dots = \gamma_{3M}$ should not be rejected. Figure 3 plots the estimated coefficients $\hat{\gamma}_{3s}$, $s = 1, \dots, M$, and γ_{4s} , $s = M + 1, \dots, S$, based on a regression where the time occasions are the days between January 3rd and may 8th 2015 with their 95% confidence intervals: almost none of the coefficients before the treatment are statistically significant. The formal F -test for the null hypothesis $M_0 : \gamma_{31} = \dots = \gamma_{3M}$ is $F(8, 14389) = 1.3$ and the associated p-value is 0.072, which leads to reject M_0 only if the fixed nominal size is 10%. We repeated the exercise by considering as time occasions weeks instead of days, obtaining $F(8, 14594) = 0.8$ with the associated p-value equal to 0.595. We therefore conclude that, in this case, there is not enough evidence to consider the common trend assumption unsatisfied.¹⁶

Figure 3: Estimated coefficients, 95% confidence intervals



¹⁶The full set of estimation results is available upon request from the authors.

Table 5: Probability of being employed 600 days after recruitment: Models by different time windows

	2 weeks	4 weeks	6 weeks	8 weeks
β_0 - Before, Small	0.704***	0.441***	0.664***	0.584***
	[0.258]	[0.122]	[0.083]	[0.066]
β_1 - Before, Diff Large vs Small	0.009	0.007	0.011	0.006
	[0.020]	[0.014]	[0.013]	[0.011]
β_2 - Small, Diff After vs Before	-0.205	-0.016	-0.004	0.017
	[0.222]	[0.027]	[0.025]	[0.022]
β_3 - Diff-in-Diff (ATET)	0.055*	0.037*	0.023	0.031**
	[0.033]	[0.022]	[0.019]	[0.016]
# Observations	3,701	7,920	10,156	13,504

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. All specifications include time effects, individual characteristics, workers qualification, region and 1 digit firm fixed effects (see Table 3 for details).

4.2 Robustness checks

In the following, we report the estimation results of two further exercises aimed at assessing the robustness of our baseline results to different choices of the time window around the reform implementation and to the criteria applied to the sample selection based on firm size.

Our results are based on a symmetric 18-week window starting January 3rd 2015. The choice was driven by another measure in the Jobs Act package, a 3-year reduction of social security contributions on all the permanent employment contracts signed between January 1st to December 31st 2015. Since this measure could be a potential confounder, we decided to consider only contracts eligible for this rebate. Hence taking January 3rd as the lower bound, we have the widest window. In Table 5 we report the estimation results based on different sample sizes, selected according to different widths of the time window. It is worth noticing that the choice of the window does not substantially affect the magnitude of the estimated ATET.

Finally, we check whether choosing a different sample based on the firm size actually affects the results. The baseline model is estimated on a sample of contracts signed in firms between 6 and 200 employees, which resembles the conventional definition of small and medium enterprises. We left out contracts signed in micro enterprises and large firms with more than 200 employees in order to keep our sample somewhat homogeneous, avoiding potentially confounding factors that could systematically affect their labour demand dynamics. Looking at the results reported in Table 6, it is worth noticing that shrinking the sample size to contracts signed in firms between 11 and 20 employees and in firms between 6 and 50 employees leaves the results unchanged. Instead, significant differences emerge if contracts signed in micro enterprises or in firms with more than 200 employees are included, thereby confirming the conjecture that there are some peculiarities which could have affected the impact of the reform.

Table 6: Probability of being employed 600 days after recruitment: Models by different firm size

	11-20	6-50	1-200	All firms
β_0 - Before, Small	0.742*** [0.112]	0.614*** [0.072]	0.606*** [0.051]	0.604*** [0.047]
β_1 - Before, Diff Large vs Small	-0.021 [0.021]	-0.002 [0.013]	0.003 [0.010]	0.042*** [0.009]
β_2 - Small, Diff After vs Before	-0.008 [0.037]	0.023 [0.023]	0.015 [0.016]	0.014 [0.015]
β_3 - Diff-in-Diff (ATET)	-0.019 [0.029]	0.014 [0.018]	0.039*** [0.013]	0.026*** [0.011]
# Observations	4,431	7,920	22,069	26,195

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. All specifications include time effects, individual characteristics, workers qualification, region and 1 digit firm fixed effects (see Table 3 for details).

5 Discussion

The results reported in Section 4 are somewhat puzzling: workers hired with the permanent IPC are no more at risk of job termination than those hired under a stricter employment protection legislation. In some cases, they even seem significantly more likely to be still employed after 600 days. This finding is in sharp contrast with the results by Boeri and Garibaldi (2019)¹⁷ on firings and with the theoretical predictions, that would see workers hired with lower firing costs more at risk of contract termination.

We argue that there may be three possible explanations driving our results, that depend on the firm recruitment strategy and workers motivation.

1. First, the reduction of firing costs may have pushed firms to hire riskier workers Kugler and Saint-Paul (2004); Sestito and Viviano (2018), that would have a more volatile probability distribution. If so, those with low productivity are rapidly screened and more at risk of job termination. However, in the medium-run, the new workers may be preferred to those hired under the old firing rules and, therefore, may be less at risk of job termination in presence of a negative shock. In the next sections, we provide some descriptive statistics and a tentative theoretical explanation that support this conjecture.
2. A second explanation stems from the fact that lower firing costs may have made permanent contracts more convenient for firms. Firms may have substituted permanent positions with temporary ones even if the working relationship was set to last a fixed amount of time τ . If this is the case, we should then observe a reduction in terminations of permanent contracts before τ , possibly followed by an increase in

¹⁷It is worth recalling, however, that the analysis of Boeri and Garibaldi (2019) is based on a sample on firms and they look at firings, and not to overall separations, over the year and a half after the reform.

separations after τ . However, the data do not allow us to check for this possibility if τ is greater than 600 days.

3. Finally, the new firing rules may have affected workers behaviour. Given the greater risk of job termination, workers hired with the IPC may have raised their effort on the job, which may in contrast have reduced the probability of termination. Again, the available data do not allow us to test this prediction.

5.1 The recruitment of risky workers

The first conjecture is based on the hypothesis that lowering firing costs may have increased firms propensity to hire riskier workers. In the opposite context, Kugler and Saint-Paul (2004) showed that a stricter employment protection legislation favours the hiring of screened workers, such as those who are already employed and are therefore less likely to be lemons.

Based on this rationale, we attempt to find some descriptive evidence in our data that firms with 15 employees or more had a higher propensity to recruit risky workers after May 7th 2015, by characterising *risky workers* as those less than 38 years old (young workers henceforth) or those who had no previous working experience. We model the probability of hiring a risky worker with the following linear model

$$r_{it} = \alpha_0 + \alpha_1 D_i + \alpha_2 R_i + \alpha_3 D_i \times R_i + \mathbf{w}_i' \boldsymbol{\alpha}_4 + u_{it},$$

where r_{it} is equal to 1 if worker i recruited at time t is young or has no experience and zero otherwise, D_i and R_i are the firm dimension and treatment dummies defined in Section 3, and \mathbf{w}_i is a vector of controls including gender, qualification, sector and region fixed effects.

The first two columns of Table 7 report the results based on the contracts signed in the 18-week window centred at May 7th 2015. It emerges that large firms had a higher propensity to recruit young and unexperienced workers after the reform than before, as opposed to small firms that were less likely to hire young workers after May 7th and had the same propensity to recruit unexperienced ones. In order to check whether these results are driven by seasonality, we repeated the exercise using the same 18-week window centred at May 7th 2014. The results of these checks are reported in the last two columns of Table 7, from which it is clear that no such trend emerges in 2014.

5.2 Unintended consequences of the Jobs Act?

The second hypothesis underpinning our conjecture is that riskier workers, hired after the reform, have a more volatile productivity distribution. For this reason, in the short

Table 7: Probability of hiring young and unexperienced workers

	Young 2015	No experience 2015	Young 2014	No experience 2014
α_0 - Before, Small	0.499***	0.066***	0.407***	0.064***
α_2 - Small, Diff After vs Before	-0.024**	0.004	0.006	-0.006
$\alpha_0 + \alpha_1$ - Before, Large	0.482***	0.045***	0.407***	0.046***
$\alpha_1 + \alpha_3$ - Large, Diff After vs Before	0.030***	0.022***	0.010	-0.003
# Observations	14,665	14,665	8,702	8,702

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. P-values are based on F tests. All specifications include workers gender, workers qualification, region and 1 digit firm fixed effects.

run less productive workers are quickly screened and dismissed if needed, whereas more productive workers may be preferred to those hired with the old firing rules in the medium run in case of an adverse shock.

In this section we present a simple theoretical framework describing this mechanism, where the recruitment of two types of workers is considered: experienced or unexperienced. We assume that the productivity of an experienced worker can be perfectly predicted by the firm and the worker is fired only in the event of a negative shock. Conversely, the productivity of an unexperienced worker can be hardly predicted and the worker can be fired even as a result of a bad quality of the match. The aim of the model is therefore to pin down the relationship between firing costs and the willingness to recruit unexperienced workers.

A match with an experienced worker ends up with a perfectly predictable productivity level, say \bar{y} . A match with an unexperienced worker can instead turn out to be Good, with a productivity equal to y^G , or Bad, with y^B , with $y^B < \bar{y} < y^G$. Let q be the expected share of good matches when hiring unexperienced workers and we assume that $\bar{y} = qy^G + (1 - q)y^B$, so that, on average, the two types of workers have the same productivity.

Assuming that the value of a vacancy is zero, the expected profit from an experienced worker is

$$\Pi^E = \bar{y} - w + \frac{(1 - \lambda)\Pi^E + \lambda(-F)}{1 + r} \quad \rightarrow \quad \Pi^E = \frac{1 + r}{r + \lambda}(\bar{y} - w) - \frac{\lambda}{r + \lambda}F,$$

where w is the wage, r the discount factor, λ is the exogenous probability of a negative shock that destroys the job position (whose expected value then becomes 0), and F are the firing costs.

The expected profit from an unexperienced worker takes into account the productivity uncertainty, which is unknown at the time of the recruitment and observable only after some time. If the match turns out bad, the unexperienced worker will be fired after a first period and a new vacancy for another unexperienced worker will be opened. Instead, if the match turns out to be a good one, the working relationship will continue unless negative shocks occur. Because a good match entails a higher productivity, we assume that good

matches with unexperienced workers give rise to a provability of job termination, due to a negative shock, equal to $\mu\lambda$, with $\mu < 1$. Therefore, the expected profit from an unexperienced worker is

$$\Pi^U = \bar{y} - w + \frac{(1-q)(\Pi^U - F) + q\Pi^{UG}}{1+r},$$

where Π^{UG} is the expected profit from good matches with unexperienced workers. Solving in Π^U , we get

$$\Pi^U = \frac{(1+r)(\bar{y} - w) + q\Pi^{UG} - (1-q)F}{r+q}, \quad (3)$$

where

$$\Pi^{UG} = y^G - w + \frac{(1-\mu\lambda)\Pi^{UG} - \mu\lambda F}{1+r}, \quad \rightarrow \quad \Pi^{UG} = \frac{(y^G - w)(1+r) - \mu\lambda F}{r + \mu\lambda}.$$

Substituting Π^{UG} in (3), we obtain

$$\Pi^U = \frac{1+r}{r+q} \left[(\bar{y} - w) + \frac{q}{r + \mu\lambda} (y^G - w) \right] - \frac{r(1-q) + \mu\lambda}{(r + \mu\lambda)(r + q)} F.$$

Therefore, a vacant job position will be filled with an unexperienced worker if the two following conditions hold:

- (a) $\Pi^E < \Pi^U|_{F=0}$
- (b) $\left| \frac{d\Pi^E}{dF} \right| > \left| \frac{d\Pi^U}{dF} \right|$,

meaning that with low firing costs firms will prefer hiring unexperienced workers, whereas with a stricter employment protection legislation they can prefer to hire experienced ones. Condition (a) is satisfied for every $y^G > \bar{y}$.¹⁸ Condition (b) is satisfied if

$$\frac{r(1-q) + \mu\lambda}{(r + \mu\lambda)(r + q)} > \frac{\lambda}{\lambda + r}.$$

¹⁸Condition (a) requires:

$$\frac{1+r}{r+\lambda}(\bar{y} - w) < \frac{1+r}{r+q} \left[(\bar{y} - w) + \frac{q}{r + \mu\lambda} (y^G - w) \right],$$

that can be written as

$$\frac{q - \lambda}{r + \lambda} \cdot \frac{r + \mu\lambda}{q} < \frac{y^G - w}{\bar{y} - w}.$$

Given $y^G > \bar{y}$, a sufficient condition is

$$\frac{q - \lambda}{r + \lambda} \cdot \frac{r + \mu\lambda}{q} < 1 \quad \rightarrow \quad q\mu - r - \lambda\mu - q < 0$$

that, given $\mu < 1$, always holds.

Solving in q , we obtain

$$q < \frac{\lambda + r(1 - \lambda)}{\lambda + r \frac{r + \lambda}{r + \mu\lambda}},$$

where the numerator is always smaller than the denominator if $r > 0$. The previous condition requires that the expected share of good matches with unexperienced workers is smaller than some critical value.

In this case, a solution in F , say \bar{F} , giving rise to $\Pi^E = \Pi^U$ exists. If $F < \bar{F}$, firms prefer hiring unexperienced workers. Now assume that, because of the treatment, F reduces from $F_0 > \bar{F}$ to $F_1 < \bar{F}$ for a given share, say a half for simplicity, of the new vacancies. Both experienced and unexperienced workers are hired *in treated firms* at time 0. Given that a negative shock hits a share of workers equal to λ , the same share λ of experienced workers is fired in the first period, whereas a share $(1 - q) + q\mu\lambda$ of unexperienced workers is fired, that is all bad matches with unexperienced workers and a share $\mu\lambda$ with the good ones. As a consequence, in the first period the termination rate is lower for experienced workers only if $\lambda < 1 - q(1 - \mu\lambda)$. By solving in q , if

$$q < \frac{1 - \lambda}{1 - \lambda\mu} \tag{4}$$

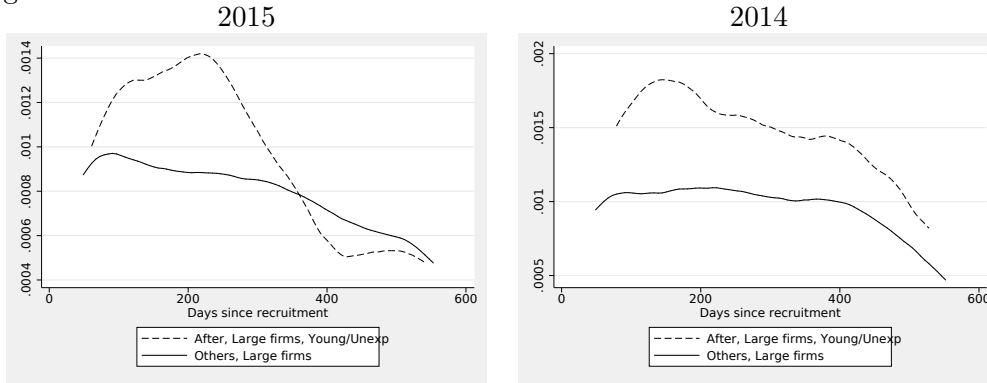
holds, then the termination rate is higher for contracts signed with unexperienced workers in the first period. Notice that this requires that the share of good matches with unexperienced workers is not very high.

In the second period, a share $(1 - \lambda)$ of the experienced and a share $1 - [q\mu\lambda + (1 - q)]$ of the unexperienced workers are still employed. In case of a negative shock, a share $(1 - \lambda)\lambda$ of experienced workers will lose their job, whereas among the unexperienced the share is $[1 - [q\mu\lambda + (1 - q)]\mu\lambda$. Therefore, in the second period, the termination rate is lower for unexperienced workers if $q < \frac{1 - \lambda}{(1 - \mu\lambda)\mu}$ that is, for $\mu < 1$, a weaker condition than (4).

If the share of good matches among unexperienced workers is below a given threshold, the model implication is twofold: a reduction in firing costs increases the likelihood of hiring unexperienced workers; the termination rate for these workers is higher right after the recruitment, with respect to that of experienced workers, and lower in the medium run. Furthermore, as time goes on, firms who screen unexperienced workers will have an increasing number of good matches and, when fully operational, the screening system implies that a larger share of matches will turn out to be good and with a low probability of job termination.¹⁹

¹⁹A corollary of the model is that the reduction in firing costs should improve the productivity of the match in the long run. The higher productivity could also have a positive effect on the level of the wage rate. However, other confounding effects can nevertheless play an important role in the wage setting. Firstly, the lower firing cost can push treated workers toward a higher effort on the job: they know that for the firm is easier to fire them than untreated workers. Secondly, lower firing costs imply that a lower efficient wage is required to push workers to non shirk on the job. We therefore end up with a situation where,

Figure 4: Hazard rates for young/unexperienced workers in treated firms vs other workers in large firms



Some descriptive evidence based on our data can be used to support these theoretical predictions. Figure 4 shows that the hazard rate for young and unexperienced workers hired with the new firing rules follows the path described in the model. In the first year after recruitment, the selection process leads to a higher hazard rate for the unexperienced workers hired after the reform. Once the initial selection has been made, this hazard rate becomes lower than that for untreated, unexperienced and older workers. No similar evidence is found in 2014.²⁰

6 Conclusions

Law no. 183, of December 20th 2014, generally known as the Jobs Act, defined the new IPC for permanent workers, that removes the possibility of reinstatement in case of dismissal without a just cause and sensibly reduced firing costs. It is common sense that this reform has made the Italian labour market more flexible, encouraged companies to increase their recruitment with open-ended contracts and generated more precarious and unstable jobs for workers hired after the reform. Extant empirical evidence seems to confirm this common feeling: the reform raised turnover, so that both hirings and separations increased.

In this paper, we attempt an evaluation of whether this higher turnover actually affected workers recruited after the reform. This could be expected on the basis of theoretical predictions, both because workers hired after the reform should be less productive and job termination, to which they should be more exposed, is less expensive to the firm. Contrary to the expected, our results show that workers hired under the firing rules show,

after the reform, more productive workers exerting a higher effort could be paid more than comparable workers hired before the job act. Future research in this direction is warranted.

²⁰By considering only unexperienced and young workers hired before and after the reform we obtain a figure very similar to 4. The hazard rate for young and unexperienced workers is higher in the first year and lower in the second year for the treated.

on average, the same probability of contract termination 600 days after the job started as untreated workers or, in some cases, the probability of still being employed is a few percentage points significantly higher.

A possible explanation for this result comes from the fact that, with lower firing costs, firms have moved towards the recruitment of riskier workers, probably younger and with no previous experience, that have a more volatile productivity distribution and are more at risk of job termination. If, right after hiring, firms have a screening process in place, less productive workers are rapidly dismissed in case of an adverse shock. In the medium run, however, the remaining treated workers may be preferred to those hired under the old firing rules and, therefore, may be less at risk of job termination in presence of a negative shock. This explanation is formalised by a simple theoretical framework and supported by some descriptive evidence.

Whether the recruitment strategy represents the driver to this unexpected result requires further research based on a longer period of observation of the two categories of workers coexisting in the Italian labour market.

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Appendix

A Full set of estimation results

Table A.1: Probability of being employed 600 days after recruitment. Full estimates for Table 3

	(1)	(2)	(3)	(4)
β_0 - Before, Small	0.586*** [0.009]	0.693*** [0.031]	0.522*** [0.057]	0.629*** [0.063]
β_1 - Before, Diff Large vs Small	0.030** [0.011]	0.017 [0.011]	0.011 [0.011]	0.006 [0.011]
β_2 - Small, Diff After vs Before	-0.047*** [0.012]	0.033 [0.022]	-0.033*** [0.012]	0.022 [0.021]
β_3 - Diff-in-Diff (ATET)	0.043*** [0.016]	0.037** [0.016]	0.032** [0.010]	0.027* [0.016]
Woman			0.031*** [0.010]	0.032*** [0.010]
Age			0.001 [0.003]	0.001 [0.003]
Age ²			-0.000 [0.000]	-0.000 [0.000]
Tenure			0.015*** [0.002]	0.015*** [0.002]
Experience			0.010*** [0.001]	0.009*** [0.001]
Qualification (ref. Manager)				
White collar			0.173*** [0.010]	0.163*** [0.010]
Blue collar			0.126*** [0.036]	0.114*** [0.036]
Other			0.151 [0.138]	0.169 [0.129]
Time effects	No	Yes	No	Yes
Region of residence	No	No	Yes	Yes
Firm sector	No	No	Yes	Yes
# Observations	14,665	14,665	14,665	14,665

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. Time effects in specifications (2) and (4) include intercepts for the day of the week and the day of the month. Specifications (3) and (4) also include 19 region fixed effects and 1 digit sector fixed effects.

Table A.2: Probability of being employed 600 days after recruitment: Placebo tests. Full estimates for Table 4

	2014/1/3 - 2014/3/6 2015/3/7 - 2015/5/8	2014/1/3 - 2014/3/6 2014/3/7 - 2014/5/8
β_0 - Before, Small	0.550*** [0.067]	0.535*** [0.084]
β_1 - Before, Diff Large vs Small	0.011 [0.014]	0.009 [0.014]
β_2 - Small, Diff After vs Before	0.089*** [0.026]	-0.038 [0.027]
β_3 - Diff-in-Diff (ATET)	0.018 [0.018]	0.008 [0.021]
Woman	0.022** [0.011]	0.025** [0.014]
Age	0.001 [0.003]	-0.000 [0.004]
Age ²	-0.000 [0.000]	-0.000 [0.000]
Tenure	0.008*** [0.002]	0.004 [0.003]
Experience	0.010*** [0.001]	0.012*** [0.001]
Qualification (ref. Manager)		
White collar	0.160*** [0.011]	0.162*** [0.013]
Blue collar	0.202*** [0.035]	0.222*** [0.013]
Other	0.250 [0.154]	0.200 [0.144]
Time effects	Yes	Yes
Region of residence	Yes	Yes
Firm sector	Yes	Yes
# Observations	11,955	8,702

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. Both specifications include time effects region and 1 digit firm fixed effects (see Table 3 for details).

Table A.3: Probability of being employed 600 days after recruitment: Models by different time windows. Full estimates for Table 5

	2 weeks	4 weeks	6 weeks	8 weeks
β_0 - Before, Small	0.704*** [0.258]	0.441*** [0.122]	0.664*** [0.083]	0.584*** [0.066]
β_1 - Before, Diff Large vs Small	0.009 [0.020]	0.007 [0.014]	0.011 [0.013]	0.006 [0.011]
β_2 - Small, Diff After vs Before	-0.205 [0.222]	-0.016 [0.027]	-0.004 [0.025]	0.017 [0.022]
β_3 - Diff-in-Diff (ATET)	0.055* [0.033]	0.037* [0.022]	0.023 [0.019]	0.031** [0.016]
Woman	0.055*** [0.020]	0.043*** [0.013]	0.039*** [0.012]	0.034*** [0.010]
Age	-0.003 [0.005]	0.001 [0.004]	0.001 [0.003]	0.002 [0.003]
Age ²	-0.000 [0.000]	-0.000 [0.000]	-0.000 [0.000]	-0.000* [0.000]
Tenure	0.019*** [0.005]	0.018*** [0.003]	0.018*** [0.003]	0.015*** [0.002]
Experience	0.009*** [0.002]	0.009*** [0.001]	0.010*** [0.001]	0.010*** [0.001]
Qualification (ref. Manager)				
White collar	0.160*** [0.020]	0.146*** [0.013]	0.154*** [0.012]	0.159*** [0.010]
Blue collar	0.100 [0.067]	0.126*** [0.036]	0.149*** [0.040]	0.111*** [0.037]
Other	0.452*** [0.047]	0.103 [0.160]	0.094 [0.162]	0.171 [0.136]
Time effects	Yes	Yes	Yes	Yes
Region of residence	Yes	Yes	Yes	Yes
Firm sector	Yes	Yes	Yes	Yes
# Observations	3,701	7,920	10,156	13,504

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. All specifications include time effects, region and 1 digit firm fixed effects (see Table 3 for details).

Table A.4: Probability of being employed 600 days after recruitment: Models by different firm size. Full estimates for Table 6

	11-20	6-50	1-200	All firms
β_0 - Before, Small	0.742*** [0.112]	0.614*** [0.072]	0.606*** [0.051]	0.604*** [0.047]
β_1 - Before, Diff Large vs Small	-0.021 [0.021]	-0.002 [0.013]	0.003 [0.010]	0.042*** [0.009]
β_2 - Small, Diff After vs Before	-0.008 [0.037]	0.023 [0.023]	0.015 [0.016]	0.014 [0.015]
β_3 - Diff-in-Diff (ATET)	-0.019 [0.029]	0.014 [0.018]	0.039*** [0.013]	0.026** [0.011]
Woman	0.038** [0.019]	0.034*** [0.012]	0.036*** [0.008]	0.032*** [0.007]
Age	-0.006 [0.005]	0.001 [0.003]	0.002 [0.008]	0.002 [0.002]
Age ²	0.000 [0.000]	-0.000 [0.000]	-0.000 [0.000]	-0.000 [0.000]
Tenure	0.010*** [0.004]	0.015*** [0.003]	0.011*** [0.002]	0.010*** [0.002]
Experience	0.011*** [0.001]	0.009*** [0.001]	0.010*** [0.001]	0.010*** [0.002]
Qualification (ref. Manager)				
White collar	0.157*** [0.019]	0.174*** [0.013]	0.170*** [0.009]	0.166*** [0.006]
Blue collar	0.176** [0.072]	0.146*** [0.036]	0.121*** [0.038]	0.095*** [0.024]
Other	0.108 [0.256]	0.137 [0.134]	0.149 [0.120]	0.264*** [0.077]
Time effects	Yes	Yes	Yes	Yes
Region of residence	Yes	Yes	Yes	Yes
Firm sector	Yes	Yes	Yes	Yes
# Observations	4,431	7,920	22,069	26,195

*: p-value<0.10; **: p-value<0.05; ***: p-value<0.01. Robust standard errors in square brackets. All specifications include time effects, individual characteristics, workers qualification, region and 1 digit firm fixed effects (see Table 3 for details).