Employment Protection Legislation, Capital Investment and Access to Credit: Evidence from Italy*

Federico Cingano† Marco Leonardi‡ Julián Messina§ Giovanni Pica¶

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Abstract

This paper estimates the causal impact of dismissal costs on capital deepening and productivity exploiting a reform that introduced unjust-dismissal costs in Italy for firms below 15 employees, leaving firing costs unchanged for larger firms. We show that the increase in firing costs induces an increase in the capital-labour ratio and a decline in total factor productivity in small firms relative to larger firms after the reform. Our results indicate that capital deepening is more pronounced at the low-end of the capital distribution (where the reform arguably hit harder) and among firms endowed with a larger amount of liquid resources. We also find that stricter EPL raises the share of high-tenure workers.

Keywords: Capital Deepening, Severance Payments, Regression Discontinuity Design, Financial Market Imperfections, Credit Constraints.

JEL Classification: J65, G31, D24.

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†Bank of Italy. E-mail: federico.cingano@bancaditalia.it.

‡University of Milan and IZA. E-mail: marco.leonardi@unimi.it.

§World Bank, University of Girona and CSEF. E-mail: jmessina@worldbank.org.

¶University of Salerno, CSEF, Paolo Baffi Centre and Centro Luca D'Agliano. E-mail: gpica@unisa.it.

1 Introduction

If dismissal protections cannot be undone by Coasean bargaining, theory predicts that Employment Protection Legislation (EPL) acts as a tax on both hiring and firing, reducing accessions and separations with an ambiguous final effect on the employment level. The reason is that firing costs provide incentives to retain workers whose wage exceeds their productivity during bad times and not to hire workers whose wage lies below their productivity during good times (Bentolila and Bertola, 1990). While there is a large body of evidence confirming this theoretical prediction (see the recent review by Skedinger, 2011), less is known about the impact of dismissal costs on other firm level outcomes, as capital deepening and productivity.

The theoretical predictions of the impact of EPL on capital deepening are in fact ambiguous. In competitive models with no financial and labour market frictions, an increase in EPL is expected to raise the cost of labour and induce capital-labour substitution, distorting production choices and reducing allocative efficiency (Autor, 2007); in the long-run firms can change their production techniques and adopt more capital-intensive technologies (Caballero and Hammour, 1998; Alesina and Zeira, 2006; Koeniger and Leonardi, 2007). In models with labour market frictions and wage bargaining, stricter EPL exacerbates the "hold-up" problem typical of investment decisions and reduces the stock of capital per worker (Bentolila and Dolado, 1994; Garibaldi and Violante, 2005). However, the relationship between EPL and capital intensity can turn positive if physical capital and firm-specific human capital are complements, and stricter EPL raises the employment share of senior workers with high firm-specific human capital (Janiak and Wasmer, 2013). From a theoretical standpoint, the impact of EPL on productivity is also ambiguous.[1]

The scarcity of studies on the effects of EPL on capital deepening and productivity is partly explained by the challenging identification issues faced when using aggregate country- or sector-level data, and by the lack of accurate data on capital in firm level datasets. The best prior evidence to date is the contribution by Autor, Kerr and Kugler (2007), who exploit U.S. cross-state variation in the adoption of wrongful-discharge protection norms and find evidence of a mild positive effect on capital deepening and a moderate negative impact on TFP. Cingano et al. (2010) use a large panel of

[1]On the one side, dismissal protections reduce workers' effort and induce firms to retain unproductive workers and/or to reduce the innovation rate (Ichino and Riphahn, 2005; Bartelsman and Hinloopen, 2005; Wasmer, 2006). On the other side, stricter EPL may raise aggregate productivity by driving inefficient firms out of the market and by promoting firms' and workers' willingness to engage in training activities because of increased job stability; it may also lead to a favourable compositional shift in the productivity of the employed workforce as firms may screen new hires more stringently (Bertola, 2004; Lagos, 2006; Belot et al., 2007).

European firms and find instead a negative effect of EPL on capital per worker.[2]

We contribute to this literature estimating the impact of EPL on capital intensity and productivity exploiting a change in the Italian size contingent employment legislation and detailed firm level balance sheet data for firms around the size threshold. The reform, enacted in July 1990, raised severance payment for firms with fewer than 15 employees, who were previously exempted, while leaving unchanged those for larger firms. This allows us estimating the impact of EPL contrasting changes in the outcomes of interest for firms below the threshold (treatment group) to those for firms above the threshold (control group). Our identification strategy combines a Regression Discontinuity Design with a difference-in-difference approach, accounting for unobserved time-invariant factors that influence firms' size (e.g. their choice to stay above or below the 15 employees threshold), while industry-year dummies absorb time-varying industry-specific shocks.

The availability of accurate balance-sheet data for a large sample of firms around 1990 allows us to focus on capital intensity as well as productivity and employment. Moreover, the data provide measures of firms' financial conditions which allow us studying how firms' response to changes in EPL is affected by their liquidity endowment. The theoretical literature is virtually silent on the effects of the interaction between EPL and financial market imperfections and there are very few empirical studies (limited to cross-country variation) on the effects of EPL on investment and productivity depending on the ability of the firm to access the credit market.

Our core results (largely confirmed by an extensive set of robustness checks) suggest that the 1990 Italian EPL reform increased capital intensity, reduced Total Factor Productivity (TFP) and had nearly no impact on the skill composition of the workforce and year-to-year job flows. Moreover, we also find that the substitution between capital and labour did not happen across the board: it was more pronounced in firms with lower capital-labour ratios prior to the reform and, among those, in firm with higher liquid resources. The latter findings are consistent with the idea that firms with few collateralizable assets have limited access to the credit market, and are therefore constrained by the amount of own liquidity when adjusting the capital stock.[3]

[2]The empirical literature on EPL has mostly concentrated on the effects on employment flows. See among others Autor et al. (2004) and (2006), Kugler and Saint-Paul (2004); Boeri and Jimeno (2005); Kugler and Pica (2008); Bauer et al. (2007); Marinescu (2009). A neighbouring literature provides mixed results on the impact of EPL on wages and labour costs: Martins (2009), Bird and Knopf (2009), Cervini Plá et al. (2010), Leonardi and Pica (2013) find a negative relationship; Van der Wiel (2010) finds a positive relationship while Autor et al. (2006) finds mainly insignificant results.

[3]The result is consistent with Claessens and Ueda (2008), who find that the positive effect of EPL on output growth in knowledge intensive industries is weaker in US states with more stringent bank branches regulation, and with Cingano et al. (2010) and Calcagnini et al. (2009) who find that better functioning financial markets mitigate the consequences of changes in EPL on firms' capital intensity and productivity level.

Our baseline results are in line with Autor et al. (2007) whose evidence suggests that the adoption of wrongful-discharge protection norms in U.S. states induced capital deepening and a decline in total factor productivity. Conversely, they are in contrast with studies on European countries (Calcagnini et al., 2009; Cingano et al., 2010) who tend to find a negative relationship between EPL and, respectively, investment and capital-labour ratios. These differences may be reconciled adopting the view proposed by Janiak and Wasmer (2013) of an inverse U-shaped relationship between EPL and the capital-labour ratio, positive at low levels of EPL and negative at high levels of EPL.[4]

The present study plausibly focuses on the range of EPL where the relationship between EPL and capital-labour ratios is positive (very much as Autor et al., 2007, and Claessens and Ueda, 2008, who study the low-EPL U.S. labour market). The 1990 Italian reform, in fact, mandated an increase in the cost for unfair dismissals of permanent workers employed in previously exempted small firms, plausibly located on the increasing side of the capital-EPL relationship. Differently, the cross-country studies by Calcagnini et al. (2009) and Cingano et al. (2010) exploit variation in EPL across relatively highly regulated European countries, and thus arguably capture the decreasing side of the capital-EPL relationship.

To investigate further the consistency of our results with the theory of Janiak and Wasmer (2013), we exploit Social Security worker-firm matched data to test the predicted positive relationship between EPL and firm seniority (a proxy for the amount of firm-specific human capital). The data allow computing alternative measures of firm seniority over time for a comparable sample of firms (i.e. those around the 15 employees threshold) located in two Italian provinces. We find that the reform increased the share of senior workers (namely, those with more than 2, 3 and 4 years of tenure) as well as average seniority in small relative to large firms. These results are suggestive that the increase in physical capital may be due to its complementarity with firm-specific human capital, as in Janiak and Wasmer (2013).

The rest of the paper is organized as follows. Section 2 describes how firing restrictions evolved in Italy. Section 3 describes the data set and the sample selection rules. Section 4 explains the

[4]Janiak and Wasmer (2013) obtain this result studying a matching model combining intra firm bargaining *à la* Stole and Zwiebel (1996a, 1996b), endogenous firm-specific human capital accumulation and complementarity between physical and (firm-specific) human capital. On the one side, job protection raises the expected returns in firm-specific human capital because of longer (expected) tenure; this raises workers' investment in human capital, firms' marginal productivity of capital and demand for capital. On the other side, job protection induces firms to retain relatively unproductive workers, thus reducing both the marginal productivity of capital and the demand for capital. When EPL is relatively low, a small increase in employment protection raises capital intensity, but for sufficiently high values of EPL the second effect prevails and the overall effect of employment protection on capital turns negative.

identification strategy used to evaluate the impact of EPL on capital deepening and productivity. Section 5 presents estimates of the impact of increased strictness of employment protection in small firms in Italy after 1990 and analyses the role of financial markets imperfections. Section 6 extends the analysis to the effects of EPL on workers' seniority within the firm and Section 7 concludes.

2 The institutional background

Over the years the Italian legislation ruling unfair dismissals has changed several times. Both the magnitude of the firing cost and the coverage of the firms subject to the restrictions have gone through extensive changes.

Individual dismissals were first regulated in Italy in 1966 through Law 604, which established that, in case of unfair dismissal, employers had the choice to either reinstate workers or pay severance, which depended on tenure and firm size. Severance pay for unfair dismissals ranged between 5 and 8 months for workers with less than two and a half years of tenure, between 5 and 12 months for those between two and a half and 20 years of tenure, and between 5 and 14 months for workers with more than 20 years of tenure in firms with more than 60 employees. Firms with fewer than 60 employees had to pay half the severance paid by firms with more than 60 employees, and firms with fewer than 35 workers were completely exempt.

In 1970, the *Statuto dei Lavoratori* (Law 300) established that all firms with more than 15 employees had to reinstate workers and pay their foregone wages in case of unfair dismissals. Firms with fewer than 15 employees remained exempt.[5] The law prescribes that the 15 employees threshold should refer to establishments rather than to firms. In the data we only have information at the firm level. However, this is not likely to be a concern as in the empirical analysis we focus on firms between 10 and 20 employees that are plausibly single-plant firms.

Finally, Law 108 was introduced in July 1990 restricting dismissals for permanent contracts. In particular, this law introduced severance payments of between 2.5 and 6 months pay for unfair dismissals in firms with fewer than 15 employees. Firms with more than 15 employees still had to reinstate workers and pay foregone wages in case of unfair dismissals. This means that the cost of unfair dismissals for firms with fewer than 15 employees increased relative to the cost for firms with

[5] See Boeri and Jimeno (2003) for a theoretical explanation of why these exemptions may be in place. In this paper we focus only on individual dismissals. An equivalent threshold applies in Italy for collective dismissals, i.e. dismissals of more than five employees within 120 days. Leonardi and Pica (2013) show that the reform on collective dismissals does not interfere with the results on the individual dismissal reform under consideration.

more than 15 employees after 1990.

For our purposes, this reform has two attractive features. First, it was largely unexpected: the first published news of the intention to change the EPL rules for small firms appeared in the main Italian financial newspaper *Il Sole 24 Ore* at the end of January 1990. Second, it imposed substantial costs on small firms: Kugler and Pica (2008) look at the effect of this reform on job and workers flows and find that accessions and separations decreased by about 13% and 15% in small relative to large firms after the reform.

3 Data, sample selection and descriptive evidence

Data for firms are obtained from the Company Accounts Data Service (Centrale dei Bilanci, or CB for brevity). The data provide detailed information on a large number of balance-sheet items since the early 1980s together with a full description of firm characteristics (as location, year of foundation, sector, ownership structure), plus other variables of economic interest usually not included in balance sheets, such as employment and flow of funds. Company accounts are collected for approximately 30,000 firms per year by the Service, which was established jointly by the Bank of Italy, the Italian Banking Association and a pool of leading banks to gather and share information on borrowers. Since banks rely heavily on these data when granting and pricing loans, they are subject to extensive quality controls by a pool of professionals.

Firms enter the data set when first granted a loan.[6] While accounting for a very large fraction of manufacturing employment and value added, the focus on the level of borrowing skews the sample towards larger firms. Moreover, the employment figures are not always reported accurately, as this piece of information is not a mandatory balance sheet item. To address both issues we integrated the CB data set with information recovered from the firms' file of the National Social Security (INPS) Archives. This administrative source covers the universe of private non agricultural firms, and contains accurate figures on their annual employment, an explicit requirement for firms when paying social security contributions. Merging these data with CB therefore allows us to improve on the initial information on firm-level employment; as they cover the universe of firms, the INPS data also allow

[6]More specifically, banks associated with the Centrale dei Bilanci agreed to include in the data set those clients from the Credit Register (a database of both individuals and firms who have been approved of for a loan) who have actually used the loan. Hence, CB firms are a subset of those included in the Credit Register.

computing post-stratification weights that can be used to re-balance the firm size distribution.[7] In section 5 we present results with and without weights, which do not differ significantly. This is because within the narrow size window we focus on (10-20 employees) CB representativeness is fairly homogeneous, as inspection of the weights indicates.

Standard treatment of the data lead us to our final variables and sample. We rely on CB for data on value added and investment and on INPS data for employment-related variables. Firm-level capital stocks are constructed applying the perpetual inventory method, using industry-specific deflators and depreciation rates and book capital as a proxy for the capital stock in the first year. Total Factor Productivity is obtained applying the multi-step estimation algorithm devised by Olley and Pakes (1996).[8] We delete as outliers 2,246 out of 99,391 initial firm-year observations whose capital-output ratio is two inter-quartile ranges away from the median. With regards to the sample period, we restrict the sample around the reform years (1986-1994), and remove year 1990 because the reform occurred in the month of July. To preserve comparability between treatment and control groups, we further restrict the sample to firms within the interval 10–20 employees, yielding a sample size of slightly more than 20,000 observations (6,656 firms). Tables 1 and 2 show the descriptive statistics.[9]

Figure 1 plots the size distribution of firms in our data, showing no evidence of firms lumping at 15 employees either before or after the reform. The absence of a dip in the firm size distribution right above the 15 employees threshold, either before or after the 1990 reform, suggests that firms are not reluctant to pass the threshold before the reform and that the reform itself does not change 15-employee firms' propensity to grow above 15. This visual impression is confirmed by the results from employment growth regressions (reported in Appendix A.1) indicating that the probability of expansion of firms just below 15 employees is not significantly different from that of larger firms, and this probability is not significantly affected by the reform (even controlling for firms fixed effects).

These results might seem surprising as standard models of labour demand would predict that size-contingent employment regulation hamper the expansion of firms and generate sizeable discontinuities in their size distribution (Garicano et al., 2012). However, they are in line with a substantial body of

[7]For each cell $i = 1..I$ the weights are constructed as follows:

$$weight_i = (\#firms_i/\#firms)_{INPS}/(\#firms_i/\#firms)_{CEBIL}$$

We experimented re-balancing both for size only and for multiple characteristics (size, industry and geographical location).

[8]The procedure allows for direct estimates of production coefficients, accounting for both endogeneity in the choice of inputs (by approximating unobserved productivity shocks with a non-parametric function of observable variables) and for selection in firms continuation decision (introducing a Heckman-type correction term).

[9]In appendix A.2, Table A.2 tests the robustness of our results both to different time periods and to the inclusion of 1990; Table A.3 shows that the results are invariant to different size ranges.

Table 1. Descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Standard deviation	Percentiles			N
			10	50	90	
Employment	14.962	3.104	11	15	19	20235
Log capital	6.603	0.922	5.342	6.670	7.735	20235
Log (capital/value added)	0.273	0.806	-0.853	0.340	1.279	20235
Log value added	6.330	0.526	5.702	6.327	6.974	20235
Fraction of white collars	0.299	0.222	0.083	0.235	0.600	19943
TFP	2.316	0.519	1.646	2.341	2.951	20235
Job reallocation	0.139	0.269	0.000	0.074	0.267	16145
Cash-flow / Fixed Assets	0.182	0.217	0.039	0.131	0.389	17055

Notes: Job reallocation is the Absolute Value of Labour Reallocation calculated as $\frac{2(e_{jt}-e_{jt-1})}{e_{jt}+e_{jt-1}}$; Total Factor Productivity is obtained applying the multi-step estimation algorithm devised by Olley and Pakes (1996). The ratio Cash-flow/Fixed Assets is measured in the pre-reform period.

Table 2. Descriptive statistics by treatment and control before and after the reform

	(1)	(2)	(3)	(4)
	Pre-reform		Post-reform	
	Small firms	Large firms	Small firms	Large firms
Employment	12.591	17.983	12.588	17.983
	(1.664)	(1.418)	(1.692)	(1.424)
Log capital	6.402	6.773	6.491	6.823
	(0.958)	(0.849)	(0.916)	(0.878)
Log (capital/value added)	0.298	0.364	0.225	0.228
	(0.827)	(0.747)	(0.837)	(0.784)
Log value added	6.104	6.410	6.266	6.595
	(0.510)	(0.470)	(0.512)	(0.475)
Fraction of white collars	0.299	0.269	0.320	0.297
	(0.232)	(0.198)	(0.235)	(0.212)
TFP	2.258	2.218	2.384	2.379
	(0.519)	(0.499)	(0.532)	(0.500)
Job reallocation	0.169	0.153	0.131	0.118
	(0.320)	(0.319)	(0.237)	(0.224)
Cash-flow / Fixed Assets	0.181	0.164	0.197	0.189
	(0.237)	(0.196)	(0.218)	(0.210)

Notes: Job reallocation is the Absolute Value of Labour Reallocation calculated as $\frac{2(e_{jt}-e_{jt-1})}{e_{jt}+e_{jt-1}}$; Total Factor Productivity is obtained applying the multi-step estimation algorithm devised by Olley and Pakes (1996). The ratio Cash-flow / Fixed Assets is measured in the pre-reform period. Standard deviations in parentheses.

FIGURE 1. Frequency of firm size in 1989 and 1991

empirical works focusing on the consequences of the increasing costs of dismissal at 15 employees in Italy (see e.g. Borgarello, Garibaldi and Pacelli, 2004; Boeri and Jimeno 2005; Schivardi and Torrini 2008; Leonardi and Pica, 2013). None of these papers found compelling evidence that the firm size distribution is discontinuous around the threshold, or that firms just below the threshold are less likely to expand.[10]

These findings do not imply that EPL has no consequences for Italian firms' employment decisions, however. Exploiting detailed matched employer-employee data and the same reform episode we use here, Kugler and Pica (2008) show that, while the stringency of regulation has little or no effect on *job* flows (the change in firm level employment), it has a large impact on *worker* flows, decreasing accessions and separations for workers in small relative to large firms. Similarly, Boeri and Jimeno (2005) showed that more stringent regulation lowers both hiring and firing probabilities of individual workers, but not the net job dynamics of individual firms.[11]

This discussion is informative to our analysis in at least two dimensions. On the one hand, if EPL does not affect the growth probability of firms, we might expect to find little or no significant effects

[10]Bauer et al. (2007) find similar results for the case of Germany.

[11]Other potential adjustment mechanisms may also be at work: Schivardi and Torrini (2008) and Hijzen et al. (2013) emphasize firms' adjustment through fixed-term contracts; Leonardi and Pica (2013) show that part of the adjustment takes place through lower wages.

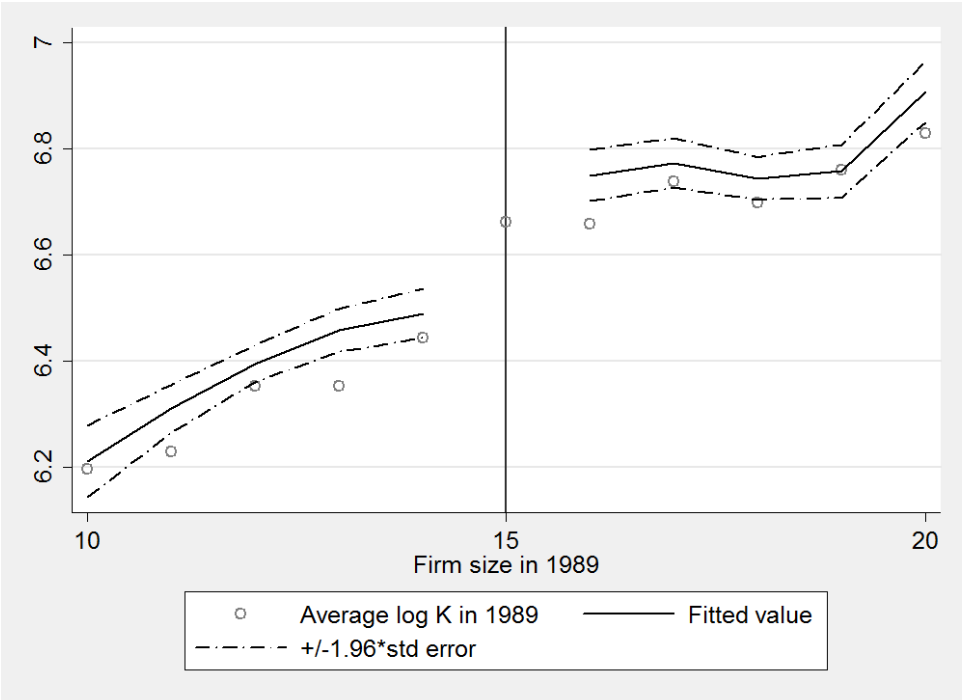

FIGURE 2. The solid line is a fitted regression of log capital on a polynomial on firm size 1989, performed separately on either side of the threshold.

of the 1990 reform on standard measures of job reallocation. On the other hand, by reducing workers' turnover stricter EPL would tend to raise the average tenure of the workforce (an issue we will explore in section 5). Under the assumption of complementarity between physical capital and firm-specific human capital, tighter job security provisions should therefore increase capital intensity (as in Janiak and Wasmer, 2013). Preliminary evidence supporting this mechanism is provided in Figure 2, which shows the distribution of the (log) capital stock by firm size just before the reform (in 1989). The presence of a visible upward jump at 15 employees indicates that firms above the threshold react to stricter EPL with higher capital intensity. The following section explains our approach to more rigorously identify this effect.

4 Identification strategy and regression model

Our estimand of interest is the average treatment effect of EPL on firms' capital intensity, their productivity as well as on their employment decisions. We exploit both the discontinuity in EPL at the 15 employees threshold and the reform of EPL which affected only small firms to build an RDD combined with a DID strategy to estimate the causal effect of EPL on various outcomes.

More specifically, we compare the change in the dependent variable – say capital – just below

15 employees before and after the 1990 reform to the change in the same variable among firms just above 15 employees. The assumption required to interpret the effect of EPL on capital as causal is that any variable that affects capital is either continuous at the threshold (as in standard RDD) or its discontinuity is constant over time (as in standard DID). In this case, the average trend of capital among firms marginally above the 15 employees threshold (16–20) represents a good counterfactual for the trend of those just below the threshold ($10-15$), which seems a reasonable assumption in such a narrow neighbourhood of the threshold. In other words, capital in firms below the threshold is expected to diverge from capital in firms just above the threshold for no other reason than the law change.

We, thus, estimate the following model:

$$
\begin{aligned}
x_{jt} &= \beta' X_{jt} + \delta_1 D_{jt}^S + \delta_2 \left(D_{jt}^S \times Post \right) + v_j + u_{jt} \qquad (1) \\
D_{jt}^S &= 1\left[\text{firm size } \leq 15 \text{ in year } t\right] \\
Post &= 1\left[\text{year} \geq 1991\right]
\end{aligned}
$$

The dependent variable x in firm j in year t is in turn the log capital stock, log capital/value added, log value added, log fraction of white collars, TFP and the Davis-Haltiwanger index of job flows.

The variable $Post$ is a dummy that takes the value of 1 after 1991 and zero otherwise (its main effect is not included because it is absorbed by the year dummies, see below); D_{jt}^S is a dummy that takes the value of 1 if the firm is small in year t and 0 if the firm is big. The interaction term $D_{jt}^S \times Post$ between the small firm dummy and the post-reform dummy is included to capture the effect of the EPL reform on the variable of interest.[12]

The matrix X_{jt} contains a polynomial of third degree in firm size. Notice that since identification comes from firm size as measured by the number of employees, we cannot use dependent variables in per-worker terms. Nevertheless, given that we control for firm size with a flexible third degree polynomial, all effects are to be read holding labour constant. Therefore, any positive impact on the capital stock implies capital deepening. We also include, in all specifications, year and industry-year

[12] Other papers have exploited the discontinuities in firing costs regimes that apply to firms of different sizes within countries. Boeri and Jimeno (2005) assess the effect of EPL on lay-off probabilities by comparing firms below and above 15 employees in Italy. Kugler and Pica (2006) examine the joint impact of EPL and product market regulation on job flows in Italy using both the firm size threshold and a law change. Using a difference-in-differences approach, Bauer et al. (2007) investigate the impact of granting employees the right to claim unfair dismissal on employment in small German firms. Leonardi and Pica (2013) look at the effects on wages.

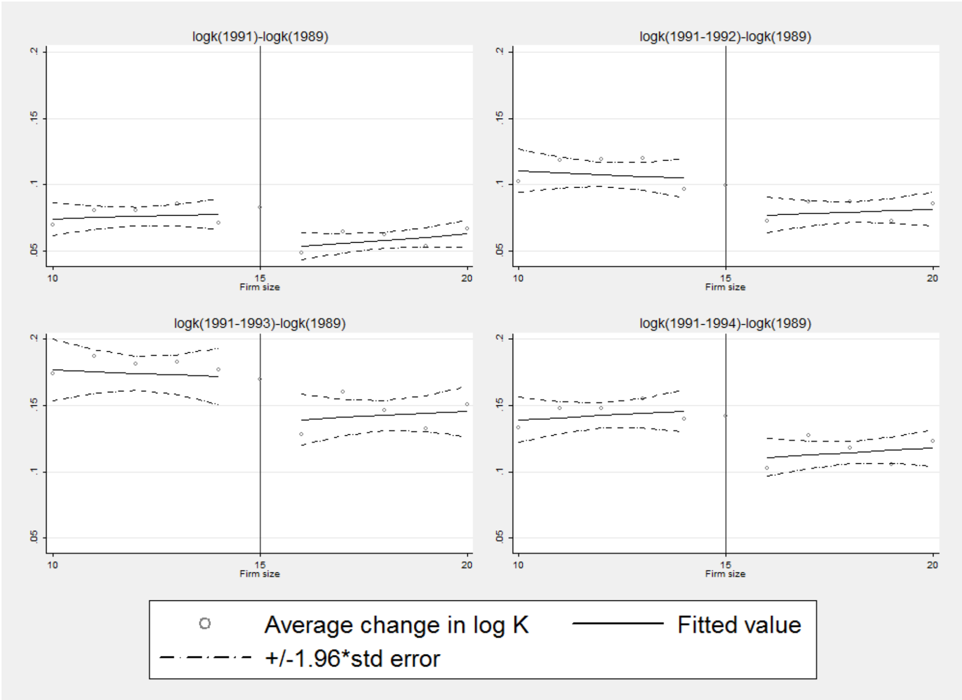

FIGURE 3. The dots are the observed differences between the post-reform log capital stock (averaged over the years indicated in parenthesis) minus the pre-reform log capital stock of year 1989. The solid line is a fitted regression of log capital differences on firm size, performed separately on either side of the threshold.

effects to reduce the sampling variability of the estimates. The inclusion of firm fixed effects accounts for all possible unobserved time-invariant factors that influence the decision of the firm to stay above or below the 15 employees threshold. The term v_j captures any unobserved firm-specific time-invariant characteristics that may affect the outcome variable and be – at the same time – correlated with the treatment status, thus biasing the results. Standard errors are clustered at the firm level to allow for time-series persistence of the shocks.

We illustrate the strategy to identify the impact of the change in dismissal costs in Figure 3 where we plot the difference in $log(K)$ against firm size. The mean of the dependent variables is estimated non parametrically separately for each side of the threshold. Firms below the 15 employees threshold show a higher capital stock than neighbouring firms, after the reform. We explore formally the effect of EPL on this dependent variables in the regressions below. At the same time Figure A.1 in Appendix A.1 plots the difference in log employment $log(e)$ against firm size confirming that there is no discontinuity at the 15 employees threshold in the growth rate of firms before and after the reform.

4.1 Quantile regression model

Theory suggests that the reform should have a larger impact on firms with lower capital-labour ratios because labour-abundant firms should be hit relatively harder by the reform. To investigate the hypothesis that firms with low capital-labour ratios react more to the reform, we run a quantile regression at different points of the distribution using as a dependent variable log capital. Let $Q_\theta(\log(k_{jt})|X_{jt})$ for $\theta \in (0,1)$ denote the θ^{th} quantile of the distribution of $\log(k_{jt})$ conditional on firm characteristics included in the matrix X_{jt} (same controls as in Equation (1)). The model of the conditional quantile is:

$$Q_\theta(\log(k_{jt})|X_{jt}) = \beta'_\theta X_{jt} + \delta_{1\theta} D^S_{jt} + \delta_{2\theta}\left(D^S_{jt} \times Post\right) + v_j \tag{2}$$

Notice that equation (2) also includes firm fixed effects. The estimation of a quantile model with fixed effects is not trivial, because its intrinsic non-linearity implies that standard demeaning techniques are not feasible. We follow the approach of Canay (2011) who introduces a simple two-step estimator, which is consistent and asymptotically normal when both the number of firms (N) and the number of period (T) approach infinity,[13] under the assumption that the firm fixed effects are pure location shifters, i.e. they affect all quantiles in the same way. Inference is based on bootstrapped standard errors obtained from individual resampling.

Finally, it is worth stressing that identification is based on the assumption that firms do not sort in or out of treatment around the time of the reform. The inclusion of firms fixed effects controls for the time-invariant unobservable characteristics that may be correlated with treatment status. However, in principle, self-selection may take place according to time-varying unobservable factors, including the reform itself. While we have no suitable instrument to properly address this issue, the employment growth regressions reported in Appendix A.1 – and discussed in Section 3 – suggest that the reform did not provide firms with incentives to select into or out of the treatment group and lend therefore support to our identification strategy.

[13]Using Monte-carlo simulations Canay (2011) shows that already with $T = 10$, the bias is fairly low irrespective of the value of N.

Table 3. Effects of the 1990 reform

VARIABLES	(1) Log(Capital)	(2) Log(Capital/ Value added)	(3) TFP	(4) Log(Value added)	(5) Fraction of white collars	(6) Job reallocation
Small firm × Post 1990	0.047***	0.059***	-0.029*	-0.013	-0.002	0.010
	(0.012)	(0.015)	(0.011)	(0.011)	(0.003)	(0.012)
Small firm	-0.045***	-0.077***	0.055***	0.032*	0.005	-0.015
	(0.013)	(0.017)	(0.014)	(0.013)	(0.003)	(0.014)
Observations	20,235	20,235	20,235	19,943	20,235	16,145
R-squared	0.164	0.049	0.070	0.241	0.044	0.030

Notes: Robust standard errors clustered by firm in parentheses. All specifications include a third degree polynomial in the size of the firm, firm fixed effects, and sector-year dummies. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%.

5 The effects of the 1990 reform

5.1 The effects of EPL on capital, productivity and employment

We estimate equation 1 focusing on the following firm-level outcomes: (the log of) Capital Stock and Capital-Value Added Ratio; a measure of TFP and (the log of) Value Added; the Fraction of White Collars and the Absolute Value of Labour Reallocation calculated as $\frac{2(e_{jt}-e_{jt-1})}{e_{jt}+e_{jt-1}}$. All specifications include a third degree polynomial in firm size, implying that the estimated effects of interest, captured by the interaction $\left(D^S_{jt} \times Post\right)$, are to be read holding labour constant. Hence, for example, the coefficients on the capital stock and value added regressions can be interpreted as capturing the consequences of EPL on, respectively, capital deepening and labour productivity. The regressions specification also accounts for local labour market dummies, year, industry and industry-year effects. The reported standard errors allow for within-firm correlation.

We find a positive and significant impact of the reform on both the log capital stock (column 1) and the log capital-value added ratio (column 2), indicating that higher EPL induced capital deepening. Based on the coefficient estimated in the first column, firms just below the threshold increased their capital stock by nearly 5% relative to larger firms as a consequence of the change in legislation. As we will discuss below, this core result is robust to a battery of checks and empirical extensions. We also find a three percent negative effect on total factor productivity (column 3), and a negative but not statistically significant effect on the log of value added (column 4). Hence, while the implications of existing theories are not clear cut, our findings provide evidence (albeit weaker than in the case of capital) that EPL has a negative impact on firms' productivity.[14]

These results are in line with the impact of the introduction of the good faith exception on investments by US firms as estimated by Autor et al. (2007) who find mixed – albeit generally positive – effects on capital-labour ratios (4.5% when controlling for plant fixed effects), and negative effects on TFP (between -2% and -1.4% with plant fixed effects). Interestingly, the estimated 5% impact of the EPL reform on capital deepening indicates that stricter EPL may induce a sizeable increase of capital investment, even in comparison with standard investment tax credit programmes explicitly targeting capital accumulation. Empirical studies in this area show mixed results and often find that firms have little or no reaction to investment tax breaks.[15] Net of the large differences between the two policies, one possible explanation is that, while changes in EPL are perceived as permanent, investment tax credits are usually temporary.

Finally, no detectable impact is found on the skill composition of the workforce (the fraction of white collars, column 5) or on job reallocation (column 6). The latter finding is consistent with Kugler and Pica (2008), who, using the same reform, found it had sizeable negative effects on *worker* flows but little or no effects on *job* flows (see Kugler and Pica, 2008, Table 4).[16] In Section 6 we will quantify the consequences of the reduction of workers' turnover for the average seniority of workers.

The positive impact of EPL on capital deepening does not seem to derive from the capital stock of small firms mechanically converging to that of large firms. Figure 4 shows that the pre-reform trends of log capital are reasonably parallel. Additionally, were this result mechanical, it should pop up also in years different from the reform year. Table A.4 in Appendix A.2 shows instead that the effect

[14] The literature (briefly reviewed in the introduction) predicts a negative effect of EPL on productivity if dismissal protections reduce workers' effort or induce firms to retain unproductive workers and/or to reduce the innovation rate. Positive effects on productivity are instead predicted if firms do more training or hire better workers because of higher EPL.

[15] Goolsbee (1998) shows that most of the benefits of an investment tax credit programme implemented in the U.S. was translated onto capital suppliers with little effects on real investment. Cohen and Cummis (2006) find that temporary partial expensing in the U.S. was largely ineffective in boosting investment, while House and Shapiro (2008), exploiting the same measure, estimate an elasticity of investment supply between 6 and 14%. Results for Italy are also mixed. Bronzini et al. (2008) examine the impact of a large investment tax credit programme aimed at lagging areas and estimate that investment by eligible firms increased by around 9% relative to non-eligible firms. The same authors found that other Italian investment subsidies programmes (e.g. Law 488, started in 1996) yield no significant impact on capital accumulation, once intertemporal substitution in investment decisions is accounted for (Bronzini and De Blasio, 2006). Notice, however, that those magnitudes are not strictly comparable with ours, as they refer to the investment effect of ITC, whereas our results refer to the impact of EPL on the stock of capital.

[16] The insignificant effect on job flows is robust to defining employment growth as $\frac{e_{jt}-e_{jt-1}}{e_{jt-1}}$ with the small firm dummy being defined based on the average pre-reform employment. In this case, the coefficient of interest equals 0.014 with a standard error of 0.008. These results might be driven by measurement issues, that is, the use of annual- as opposed to shorter-frequency data. Previous studies looking at annual rates of job reallocation also found that EPL has little effect on job flows (see Bertola and Rogerson, 1997; Blanchard and Portugal, 2001; Martins, 2009). Contrasting results obtained using quarterly and yearly rates of reallocation, Blanchard and Portugal (2001) conjecture that employment protection only impairs high-frequency flows.

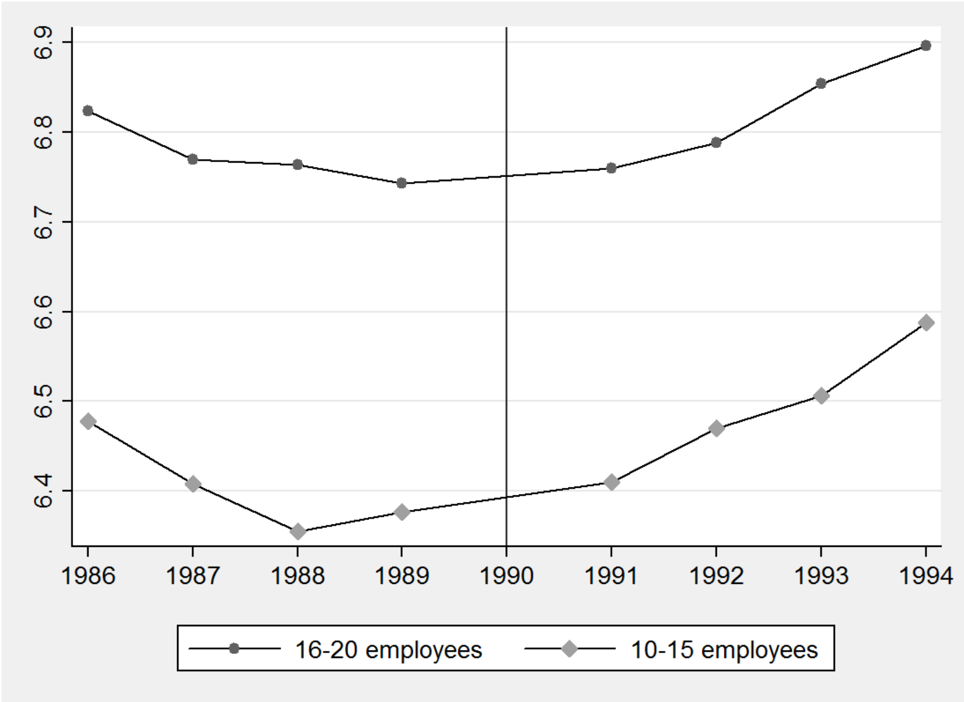

FIGURE 4. Evolution of log capital

vanishes when considering 1988 and 1992 as fake reform years. In the same Appendix we perform a wide range of robustness checks to show that our results on log capital (the main variable of interest) are robust to changes in the time period considered (Table A.2) and in the size range (Table A.3).

We next investigate the hypothesis that firms with low capital-labour ratios react more to the reform. Table 4, Panel A, shows results from a quantile regression at different points of the distribution using as a dependent variable log capital. The estimates indicate that the effect of the reform on capital is highest at the 10^{th} percentile and decreases along the capital distribution reaching non significance at the 90^{th} percentile. A similar, slightly more nuanced, decreasing pattern shows up also for the capital-output ratio in Panel B, in line with the idea that firms with a high share of labour costs were hit harder by the reform. Finally there is no clear pattern in the impact of the reform along the distribution of TFP (Panel C).

5.2 The role of financial market imperfections

In this section we further investigate the implications of stricter EPL on capital investment and we look at whether the effect of the reform on capital deepening varies with the availability of credit. The idea is that credit constrained firms belonging to the treatment group may not be able to react to the change in EPL and engage in capital deepening as much as unconstrained firms.

TABLE 4. Effect of the reform at different quantiles of the log capital distribution

	(1)	(2)	(3)	(4)	(5)
	Quantile regressions				
	10	25	50	75	90
Panel A: capital stock					
Small firm × Post 1990	0.087***	0.030***	0.037***	0.022**	0.016
	(0.015)	(0.007)	(0.004)	(0.007)	(0.013)
Observations	20,235	20,235	20,235	20,235	20,235
Panel B: capital-output ratio					
Small firm × Post 1990	0.079***	0.050***	0.059***	0.054***	0.044***
	(0.016)	(0.008)	(0.001)	(0.010)	(0.016)
Observations	20,235	20,235	20,235	20,235	20,235
Panel C: productivity					
Small firm × Post 1990	-0.040***	-0.017*	-0.029***	-0.033***	-0.030*
	(0.012)	(0.008)	(0.000)	(0.007)	(0.012)
Observations	20,235	20,235	20,235	20,235	20,235

Notes: Bootstrapped standard errors clustered by firm in parentheses (100 replications). All specifications include a third degree polynomial in the size of the firm, firm fixed effects, and sector-year dummies. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%

As mentioned in the introduction, there are not many papers that empirically investigate the joint influence of imperfect financial and labour markets on capital. Notable exceptions are Claessens and Ueda (2008), Calcagnini et al. (2009) and Cingano et al. (2010).[17]

Similarly, the theoretical impact of credit and labour market imperfections on capital investment has been analysed only in Wasmer and Weil (2004) and Rendon (2004), who shows that job creation is limited by financing constraints even in the presence of a flexible labour market.

Most empirical studies, in the tradition of Fazzari, Hubbard and Petersen (1988), focus on the impact of financing constraints on investment and regress a measure of investment on a measure of investment opportunities (Tobin's q) as well as a measure of cash flow, i.e. they estimate the sensitivity of investment to cash flow conditional on q. This empirical specification implies that, even in the absence of financing constraints, investment is subject to adjustment costs that prevent the capital stock adjusting continuously to maintain equality between the marginal revenue product and the user cost of capital. Measurement of Tobin's q requires knowledge of the market value of the firm. This piece of information is not available in our data, as the vast majority of the firms included in our sample is unlisted. For this reason, our empirical specification uses Return On Assets (ROA) as a measure of investment opportunities.

Following the literature, we measure internal funds using cash-flow normalized by fixed assets ($CF_{jpre} = \frac{cashflow_{jpre}}{FixedAssets_{jpre}}$). In order to minimize endogeneity issues we measure both cash-flow and fixed assets in the pre-reform period and consider the availability of internal resources as a firm fixed characteristic.

The triple difference specification is therefore:

$$\begin{aligned} \log(k_{jt}) \quad = \quad & \beta' X_{jt} + \gamma ROA_{jpre} + \delta_0 Post + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \delta_3 \left(CF_{jpre}\right) + \\ & + \delta_4 \left(D^S_{jt} \times CF_{jpre}\right) + \delta_5 \left(CF_{jpre} \times Post\right) + \delta_6 \left(CF_{jpre} \times D^S_{jt} \times Post\right) + v_j + u_{jt} \end{aligned} \tag{3}$$

The triple interaction term $CF_{jpre} \times D^S_{jt} \times Post$ pins down the effect of the change in EPL in firms with different levels of internal resources. We expect this interaction term to be positive because we expect capital deepening to take place more easily in firms with higher levels of internal resources.

We also run quantile regressions to check whether the impact of internal resources is different for firms at different points of the log capital distribution. Liquidity may indeed be more important for

[17] Relatedly, Caggese and Cuñat (2008) document that finance constrained Italian SMEs have more volatile employment and rely more heavily on temporary workers.

low-capital intensity firms, which are possibly subject to stricter financial constraints due to the scarce availability of collateralizable assets.

Let $Q_\theta(\log(k_{jt})|X_{jt})$ for $\theta \in (0,1)$ denote the θ^{th} quantile of the distribution of $\log(k_{jt})$ conditional on firm characteristics included in the matrix X_{jt}.[18] The model of the conditional quantile is:

$$\begin{aligned} Q_\theta(\log(k_{jt})|X_{jt}) \quad = \quad & \beta_\theta' X_{jt} + \gamma ROA_{jpre} + \delta_{1\theta} D_{jt}^S + \delta_{2\theta}\left(D_{jt}^S \times Post\right) + \delta_{3\theta} CF_{jpre} + \\ & + \delta_{4\theta}\left(D_{jt}^S \times CF_{jpre}\right) + \delta_{5\theta}\left(CF_{jpre} \times Post\right) + \delta_{6\theta}\left(CF_{jpre} \times D_{jt}^S \times Post\right) + v_j \end{aligned} \tag{4}$$

As for equation 2, we assume that the firm fixed effects are pure location shifters (i.e. they are not quantile-specific), and estimate the above quantile model using the two-step procedure suggested by Canay (2011).

Table 5 shows results from the estimation of equation (3) in the first column and of equation (4) in the remaining columns. The estimates indicate that, on average, the reform induces capital deepening in small compared to large firms (consistently with the results in Table 3), with no significant differential effects of cash-flow (column 1). The remaining columns display a pattern similar to the one in Table 4: the effect of the reform on capital deepening is highest – with a coefficient equal to 0.035 – at the 10^{th} percentile and then decreases along the distribution of log capital reaching zero at the 90^{th} percentile. In particular, a one standard deviation increase in the ratio of Cash-flow to Fixed Assets (equal to 0.22, see Table 1) raises the capital stock by 3.5% at the tenth percentile of the log capital distribution.

This result suggests that large amounts of liquidity ease the response of firms with a relatively low capital stock to the change in EPL. The reason may be that firms with little collateralizable capital may find it difficult to borrow and therefore need to rely on internal liquid resources to raise the capital stock in response to the increase in EPL. The general implication is that financial market imperfections hinder firms' reaction to the increase in firing costs and therefore amplify the allocative inefficiencies due to stricter EPL.

[18] We include the same controls as in Equation (3), plus firm ROA (measured in the pre-reform period) to account for firm profitability. Notice that despite pre-reform firm ROA being a time-invariant firm characteristic, in the non-linear quantile regression model its effect is not absorbed by the firm fixed effect.

Table 5. Differential impact of the reform on log capital stock depending on pre-reform cash-flow

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Quantile regressions				
	Regression	10	25	50	75	90
Small firm × Post 90	0.043***	0.076***	0.027***	0.030***	0.018**	0.009
	(0.012)	(0.013)	(0.007)	(0.005)	(0.007)	(0.015)
Cash-Flow / FA × Small firm × Post 90 dummy	0.017	0.035**	0.017	0.020**	0.021	-0.000
	(0.018)	(0.012)	(0.009)	(0.007)	(0.012)	(0.023)
Observations	17,055	17,055	17,055	17,055	17,055	17,055

Notes: Robust standard errors clustered by firm in parentheses in column 1. Bootstrapped standard errors clustered by firm in columns 2-6 (100 replications). All specifications include firm ROA measured in the pre-reform period, a third degree polynomial in the size of the firm, firm fixed effects, sector-year dummies, a full set of interaction terms between the ratio of cash-flow to fixed assets (both measured in the pre-reform period), the Post 1990 dummy and the small firm dummy. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%.

6 The effect of EPL on seniority

So far, the results on capital deepening and TFP are consistent with any model where dismissal protection unambiguously reduces allocative efficiency and provides firms with an incentive to substitute away from labour to other factors of production. In this section we specifically focus on the theoretical predictions of Janiak and Wasmer (2013) and provide additional evidence on the impact of the reform on the share of high-tenure workers. Janiak and Wasmer (2013) is the first theoretical paper which explicitly focusses on the link between capital and EPL. Studying a model with matching frictions and bargaining, they show that EPL should generally be expected to decrease the capital-labour ratio. A positive relationship between EPL and capital intensity can emerge, however, when there is a complementarity between physical capital and high-tenure workers (who have high firm-specific human capital): higher EPL reduces turnover and increases the share of senior workers in the firm thus generating an incentive to invest in complementary physical capital.

In light of these insights, in the following we test whether the 1990 reform also raised workers' seniority, on average. Because it requires measuring firm-specific tenure for all workers in a firm, exploring this issue necessitate a long panel of worker-firm matched data. In Italy, such data is available from Social Security (INPS) archives covering the universe of firms located in two northern provinces together with all their employees. Each record in the matched dataset describes an employ-

Table 6. Effect of the reform on the share of workers with high tenure.

Dependent variable	(1) Share of workers with Ten>2yrs	(2) Share of workers with Ten>3yrs	(3) Share of workers with Ten>4yrs	(4) Log average tenure
Small firm × Post 1990	0.021*	0.023*	0.027**	0.032*
	(0.009)	(0.010)	(0.010)	(0.014)
Small firm	-0.008	-0.005	-0.006	-0.007
	(0.010)	(0.010)	(0.010)	(0.015)
Observations	25,156	25,156	25,156	25,156
R-squared	0.662	0.717	0.758	0.879

Notes: Standard errors clustered by firm in parentheses. All specifications include a third degree polynomial in the size of the firm, firm fixed effects, sector and year dummies. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%

ment relationship, providing information on the number of weeks covered in the position, individual demographics and employer information.[19] We use the same selection rules as in the previous sample, restricting to the period around the reform years (1986-1994, excluding 1990), and to firms with 10-20 employees. The final sample size amounts to around 25000 observations (6680 firms).

We estimate the benchmark regression 1 focussing on the share of high seniority workers, defined as those with more than 2 years of tenure, taken to be the minimal amount of time needed to accumulate significant firm-specific human capital. The first column of Table 6 indicates that the reform raised this share by around two percentage points in small relative to large firms. For comparison, the large-small firms share differential before the reform was of around 4 percentage points. Similar results are obtained changing the threshold used to identify high seniority workers to 3 and 4 years (columns 2 and 3, respectively).[20] We also find that the increase in EPL raised average firm tenure of around 3% (column 4). Together with the results on the positive relationship between EPL and capital intensity, the estimated effects on the share of high-tenured workers are, overall, consistent with the predictions of Janiak and Wasmer (2013).

[19]The original data cover over 10 million employment relationships and 116,000 firms located in two northern Italian provinces during more than 20 years (1975-97). Unfortunately, however, they do not include information on firms' capital stock. For a more detailed data description see Leonardi and Pica (2013) and Cingano and Rosolia (2012).

[20]It is not surprising that the effect on the composition of the workforce appears soon after the reform, at least in firms with relatively few employees as those under scrutiny, where a reduction in workers' turnover immediately translates into a higher share of high-tenured workers.

7 Conclusion

Exploiting a law change that raised firing costs for Italian firms below 15 employees, we find a 5% positive effect of EPL on capital deepening, thus suggesting that stricter job protection induces capital-labour substitution. The estimated effect is sizeable if compared to the typically very small impact of investment tax credits programmes found in the literature. We find capital-labour substitution to be mostly concentrated among labour-intensive firms, possibly because firms with a high share of labour costs are hit harder by changes in EPL. We inspect the potential explanations of these results along two dimensions.

First, we explore the heterogeneity of the effect of EPL depending on firms' liquid financial endowments. Among the firms with low capital-labour ratios, we find that the effect is less pronounced for firms with low internal liquid resources, plausibly because these firms have little capital to pledge as collateral against lenders and no internal liquid resources to rely upon.

Second, we investigate whether these findings are consistent with Janiak and Wasmer (2013) who claim that the positive impact of EPL on capital is due to the complementarity between capital and the amount of labour endowed with firm-specific human capital. Indeed, we find that the reform positively affects the share of high-tenured workers with high firm-specific human capital who are likely to be complements with capital investment, thus lending credibility to the Janiak and Wasmer (2013) channel.

Overall, our evidence points to a mechanism whereby EPL reduces workers' turnover and increases the share of high-tenure workers. As a consequence, both the higher relative cost of labour and the complementarity of high-tenure workers with capital may contribute to induce firms to raise capital intensity. These results show that capital investment can be an important margin of adjustment in the face of EPL changes, provided that financial markets imperfections do not hinder firms' response.

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A Appendix

This appendix contains evidence on the sorting behaviour of firms around the 15-employee threshold (Section A.1) and a battery of robustness checks (Section A.2).

A.1 Firm sorting

This section investigates whether firms tend to sort above and below the 15-employee threshold, according to pre-existing observable and unobservable characteristics, before and after the 1990 reform.

To do so, we first compute for each firm the average capital stock before 1990 (the reform year) and use this time-invariant firm characteristic as one of the determinants of the firm probability of growing. We exploit the unique opportunity of observing firms' capital stock to build a variable which should capture hitherto unobserved firms' characteristics within the following linear probability model:

$$\begin{aligned} d_{jt} &= \beta^{'} X_{jt} + \delta_0 Post + \delta_1 Sizedummy_{jt-1} + \delta_2 \overline{k}_{pre,j} + \alpha_0 \left(Sizedummy_{jt-1} \times Post \right) \\ &\quad + \alpha_1 \left(\overline{k}_{pre,j} \times Post \right) + \alpha_2 \left(Sizedummy_{jt-1} \times Post \times \overline{k}_{pre,j} \right) + \eta_j + \varepsilon_{jt}, \end{aligned} \tag{5}$$

where $d_{jt} = 1$ if firm j in year t has a larger size than in $t-1$. The term $Sizedummy_{jt-1}$ denotes a set of firm size dummies while the variable $Post$ takes the value of one from 1991. The term $\overline{k}_{pre,j}$ denotes the estimated time-invariant average pre-reform capital stock. The matrix X_{jt} includes year dummies, sector dummies and a polynomial in lagged firm size. Finally, we also include firm fixed effects to account for firm-specific time-invariant factors that affect firms' propensity to grow.

The first two columns in Table A.1 show that the probability of expansion of firms just below 15 employees is not significantly different from that of other firms (col. 1), and that such transition probability is not significantly affected by the reform (col. 2). Both results are important to our analysis as they suggest that firms are not reluctant to pass the threshold before the reform, and that the reform itself does not change 15-employee firms' propensity to grow. In other words, they suggest that the reform did not provide firms with incentives to select into or out of treatment. However, what ultimately matters for our estimates is that the reform did not induce changes in the underlying composition of firms around the threshold in terms of unobserved characteristics that are correlated with the outcome of interest. In columns 3 and 4 we provide further supporting evidence that this is not the case, focusing on the case of capital intensity. Evidence that high capital intensity firms

Table A.1. Firm Sorting

	(1)	(2)	(3)	(4)
Dummy 13	-0.016	-0.033	-0.129	-0.119
	(0.016)	(0.024)	(0.118)	(0.186)
Dummy 14	-0.002	-0.012	-0.131	-0.147
	(0.016)	(0.026)	(0.128)	(0.185)
Dummy 15	-0.014	-0.054	-0.020	-0.113
	(0.017)	(0.028)	(0.132)	(0.225)
Post 1990 $\times \overline{k}_{pre,j}$				0.006
				(0.014)
$\overline{k}_{pre,j} \times$ Dummy 13			0.018	0.014
			(0.018)	(0.028)
$\overline{k}_{pre,j} \times$ Dummy 14			0.019	0.021
			(0.020)	(0.028)
$\overline{k}_{pre,j} \times$ Dummy 15			0.000	0.009
			(0.020)	(0.034)
Post 1990 $\times$ Dummy 13		0.028		-0.032
		(0.031)		(0.240)
Post 1990 $\times$ Dummy 14		0.017		0.027
		(0.032)		(0.229)
Post 1990 $\times$ Dummy 15		0.063		0.129
		(0.034)		(0.268)
$\overline{k}_{pre,j} \times$ Post 1990 $\times$ Dummy 13				0.010
				(0.036)
$\overline{k}_{pre,j} \times$ Post 1990 $\times$ Dummy 14				-0.003
				(0.035)
$\overline{k}_{pre,j} \times$ Post 1990 $\times$ Dummy 15				-0.011
				(0.040)
Observations	15,262	15,262	13,303	13,303
R-squared	0.160	0.160	0.162	0.163
Number of firms	5,272	5,272	4,198	4,198

Notes: The dependent variable is a dummy that takes the value of 1 if in firm j employment at time t is larger than employment at time $t-1$, and 0 otherwise. Firms between 10 and 20 workers are included. All specifications include a third degree polynomial in lagged firm size, sector dummies and year dummies. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%.

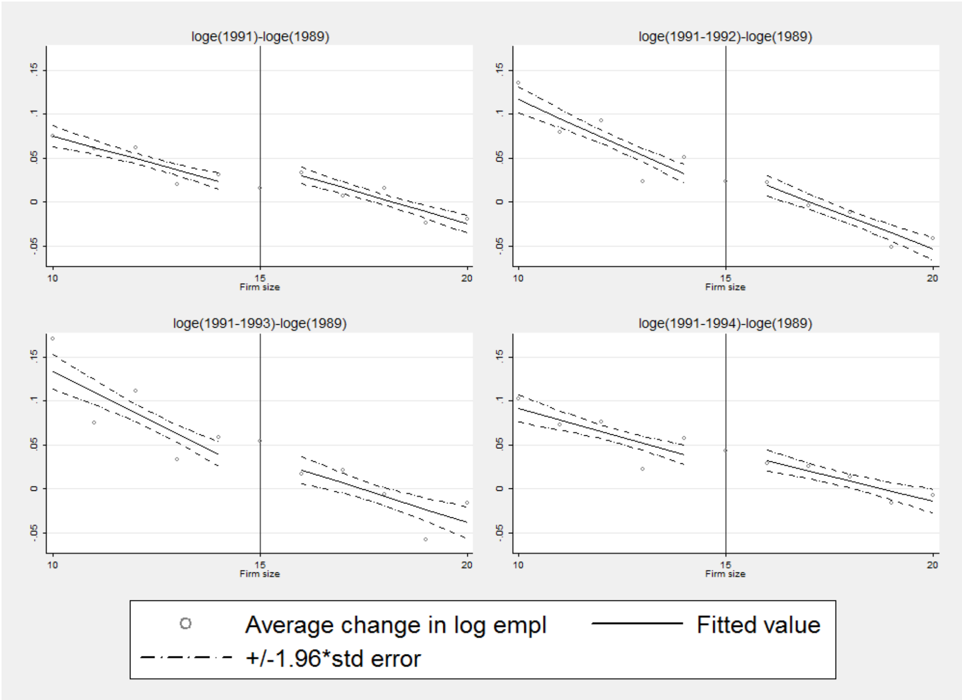

FIGURE A.1. The dots are the observed differences between the post-reform log employment (averaged over the years indicated in parenthesis) minus the pre-reform log employment of year 1989. The solid line is a fitted regression of log capital differences on firm size, performed separately on either side of the threshold.

(as measured by their pre-reform average capital stock) are disproportionately more likely to pass the threshold as a consequence of the reform would cast doubts on the reliability of our exercise. However, we do not find evidence that the growth probability depends on pre-reform capital intensity (either before or after the reform).

As a final check, Figure A.1 plots the difference in log employment $log(e)$ against firm size confirming that there is no discontinuity at the 15 employees threshold in the growth rate of firms before and after the reform.

A.2 Robustness checks

This section contains a battery of robustness checks briefly discussed in the main text. For brevity, we focus on our main variable of interest, log capital.

First, we relax the time period (Table A.2) and the size range (Table A.3) of the analysis. The results are qualitatively similar to those in Table 3, confirming the positive effect of the reform on capital intensity.

In Table A.4 we implement placebo tests by estimating the treatment effect at fake firm size

Table A.2. Robustness to different time periods: dependent variable $\log(k_{jt})$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Time period	1986-94	1987-92	1988-92	1989-93	1988-93	1987-94	1988-94
Small firm × Post 1990	0.047***	0.041***	0.041***	0.046***	0.043***	0.043***	0.047***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)	(0.013)
Small firm	-0.045***	-0.045**	-0.049**	-0.048**	-0.045**	-0.043**	-0.048**
	(0.013)	(0.015)	(0.017)	(0.018)	(0.016)	(0.014)	(0.016)
Observations	20,235	12,855	10,608	10,952	13,243	18,069	15,822
R-squared	0.164	0.136	0.134	0.120	0.137	0.157	0.155

Notes: Robust standard errors clustered by firm in parentheses. The first column includes 1990. All specifications include a third degree polynomial in the size of the firm, firm fixed effects, and sector-year dummies. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%.

thresholds and fake reform years, where there should be no effect. We estimate the treatment effect below and above the fake 12 and 18 employees thresholds. In Columns 1 and 2 we estimate the treatment effect before and after the fake reform years 1988 and 1992 (excluding in turn the fake year of the reform as we did with 1990 in Table 3). The interaction between the small firm and the post-reform dummy is not significant. This implies that the effect on capital is not a mechanical a convergence effect, due to firms with less capital accumulating it faster. Columns 3 and 4 show that the fake firm size threshold is still positive and slightly significant when considering the 12-employee threshold, but it is no longer significant at 18 employees.

Finally, Table A.5 shows results from weighted regressions to account for the possibility that the Company Accounts Data Service undersamples small firms, which are more likely to be financially constrained and less likely to show up in the data set. Regression weights by firm size are given by the ratio between the total number of firms in the economy (from Social Security Records) and the number of firms in the Company Accounts Data Service. Results are qualitatively similar to those shown in Table 3 suggesting that the undersampling of smaller firms is not a major issue within our narrow 10-20 firm-size window.

Table A.3. Robustness to different size ranges: dependent variable $\log(k_{jt})$

	(1)	(2)	(3)	(4)	(5)
Size range	5-20	5-30	5-25	10-25	10-35
Small firm × Post 1990	0.049***	0.052***	0.054***	0.051***	0.043***
	(0.012)	(0.010)	(0.011)	(0.012)	(0.011)
Small firm	-0.040***	-0.040***	-0.043***	-0.040***	-0.038***
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Observations	26,477	42,541	34,663	28,421	44,403
R-squared	0.190	0.200	0.194	0.173	0.199

Notes: Robust standard errors clustered by firm in parentheses. All specifications include a third degree polynomial in the size of the firm, firm fixed effects, and sector-year dummies. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table A.4. Falsification: fake firm size threshold and fake reform years. Dependent variable $\log(k_{jt})$

	(1)	(2)	(3)	(4)
	Fake reform year		Fake firm size threshold	
	1988	1992	12 employees	18 employees
Small firm × post 1988	-0.012			
	(0.011)			
Small firm × post 1992		0.025		
		0.014)		
Small firm 12 × post 1990			0.034*	
			(0.017)	
Small firm 18 × post 1990				0.016
				(0.014)
Observations	20,764	20,291	20,235	20,235
R-squared	0.155	0.163	0.154	0.162

Notes: Robust standard errors clustered by firm in parentheses. All specifications include a third degree polynomial in the size of the firm, firm fixed effects, and sector-year dummies. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%.

Table A.5. Weighted Regressions

VARIABLES	(1) Log(Capital)	(2) Log(Capital Value added)	(3) Log(Value added)	(4) Fraction of white collars	(5) TFP	(6) Job reallocation
Small firm × Post 1990	0.042**	0.060**	-0.018	-0.003	-0.035*	0.007
	(0.016)	(0.019)	(0.014)	(0.004)	(0.015)	(0.015)
Small firm	0.030	-0.024	0.054*	0.014*	0.032	-0.046
	(0.029)	(0.034)	(0.027)	(0.006)	(0.027)	(0.030)
Observations	20,235	20,235	20,235	19,943	20,235	15,742

Notes: Robust standard errors clustered by firm in parentheses. All specifications include a third degree polynomial in the size of the firm, firm fixed effects, and sector-year dummies. Regression weights by firm size are given by the ratio between the total number of firms in the economy (from Social Security Records) and the number of firms in the Company Accounts Data Service. One asterisk denotes significance at 5%; two asterisks denote significance at 1%; three asterisks denotes significance at 0.1%.