EPL and Job Contract Conversion Rate: The Italian CFL Case

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Abstract

This paper analyzes the effect of EPL on the conversion rate of temporary contracts into permanent ones in the same firm. Once EPL is enforced, two effects might arise: employers could tend to replace their permanent workforce with short-term employment because of the lower expected value of a filled job, but firms might also prefer to stabilize part of their temporary workforce. In fact, firms already know about workers' skills and attitudes and workers have acquired information about wages, career prospects and employers' expectations. This in turn implies a lower risk of job-breakup. Which of these two effects is dominant is ultimately an empirical question. I exploit a natural experiment set up yielded by the Italian 1990 reform which introduced unjust dismissal costs for small businesses to identify the effect of EPL on the conversion rate of working and training contracts (*Contratti di formazione e lavoro - CFL*) into permanent ones in the same firm.

1 Introduction

It is well known that firms often need to carry out screening procedures to evaluate workers' skill levels before stabilizing the firm-worker job relationship, and it might also be costly for them to dismiss workers once they are hired. As long as firms cannot make use of any specific tool to evaluate some of the applicants' characteristics - i.e. practical skills, cooperative attitudes - their choice could be inefficient because after a worker is hired and her unsuitability for the job is understood, a firm has an incentive to dismiss the worker. But since Employment Protection Legislation (EPL) usually poses some costs on this choice, firms might be obliged to keep a worker even though her/his unsuitability[1]. This in turn implies the presence of inefficiencies and frictions in firms' hiring procedures.

Cahuc and Postel-Vinay (2002) state that because of higher firing costs, the conversion rate of temporary contracts into long-term contracts should simply drop. Following this reasoning, in the long run we should observe that firms subject to EPL should progressively convert all the workforce into a temporary one. But in reality, firms tend to keep a stable workforce even if EPL is enforced. This behavior can be compatible with economic incentives, in fact if workers need to be trained for specific tasks, firms might not be willing to iterate training activities each time they dismiss their temporary workforce. It might also be the case that firms want to retain certain workers because of their skills. Moreover, high turnover rates could be an incentive for temporary workers to exert less effort, and this in turn implies detrimental effects in terms of firm-level productivity. Finally, it could be too costly for firms to keep unfilled jobs each time a contract comes to its expiration. So, given that firms could also be interested in retaining some workers, higher EPL could also push firms to be more aware of the "screening side" of temporary contracts. Thus, EPL could result in a higher conversion

[1] See Blanchard and Portugal (2001) and Autor, Kerr and Kugler (2007).

rate of fixed-term contracts into open-ended ones.

The research question which underlies this paper is thus to shed some light on the causal effect of dismissal costs on the conversion of temporary contracts into permanent ones in the same firm. To find empirical evidence of this relationship, I study the effect of the Italian 1990 reform - which introduced unjust dismissal costs for small firms - on the conversion rate of the *Contratto di Formazione e Lavoro* (working and training contract - CFL) into permanent contracts.

The rest of the paper is organized as follows. Section 2 considers the branch of the literature which this study is nested to. Section 3 describes the CFL program. Section 4 is devoted to the empirical analysis. Section 5 concludes.

2 EPL: some theoretical and empirical advances

Employment Protection Legislation (EPL) is a widely investigated institutional feature of labour markets and consists of a set of rules according to which the firms' hiring and firing processes are regulated. In particular, EPL defines conditions to be complied with by employers in case of fair and unfair layoffs[2]. In case of individual dismissal, EPL prescribes advance notice periods, third parties' roles (for example prior negotiation with trade unions or administrative authorizations), procedures to challenge the layoff decision, and possible severance payments[3]. Despite they are intended to promote employment stability, their actual effects are at the centre of an intense debate, and, in the last two decades, the work of many economists has been of great help in exploring several dimensions of the impact of EPL reforms on labour

[2]Fair layoffs are justified by disciplinary or economic reasons, while unfair layoffs can be brought about by several reasons, such as discriminatory practices.

[3]See Bassanini, Nunziata and Venn (2008) and Cahuc and Zylberberg (2004).

market outcomes.

From a theoretical point of view, EPL is thought to be a source of distortion of labour market outcomes as long as it affects firms' employment choices and workers' behavior. Matching models with endogenous job creation and destruction in the spirit of Mortensen and Pissarides (1994) predict an ambiguous effect on the (un)employment rate. Garibaldi (1998) comes to similar conclusions by extending search models to analyze the cyclical behavior of job reallocation. Asymmetric responses of job creation and job destruction are closely related to the nature of firing permissions. Continuously available firing permissions makes job reallocation countercyclical, but when firing permissions are time consuming, the asymmetry disappears. Garibaldi and Pacelli (2008) focus on the effect of severance payments on job separation. They look at the Italian labour market and use a deferred wage scheme - the *Trattamento di Fine Rapporto* - to give empirical content to the theoretical prediction that an increase in severance payments increases labour hoarding. Indeed, they find that a 60% advance withdrawals of accumulated wages increase the probability of separation by roughly 20%. Lazear (1990) shows that in a perfect labour market, EPL has no real effects on employment, while transfers from workers to firms (formalized through properly designed labour contracts) alter the workers' wage-tenure profile. Leonardi and Pica (2007) give empirical content to this proposition. Indeed, they find a decrease of the returns to tenure by 20% in the first year and by 8% over the first two years.

Boeri and Jimeno (2005) enrich standard models of employment protection legislation to give economic explanations to the common practice of excluding small businesses from EPL coverage. Kugler and Pica (2008) use administrative data from the Italian Social Security Institute to assess the impact of the increase of dismissal costs for small firms on worker and job flows, and on firms' market entry and exit decisions. They take advantage of the same reform used in this study and observe a closing gap of worker flows after the reform as well as a closing gap of firms flows after the reform. Their findings suggest that heavier EPL reduces flows into and out of employment, but with negligible effects on net employment. They also find that after the reform small firms were less likely to enter the market. Ichino and Riphahn (2005) explore the effect of EPL on workers' behavior in terms of absenteeism as a measure of worker effort. They use data on white-collar workers from a large Italian bank, and exploit the presence of the institution of probation to check whether more employment protection alters the average number of days of absence from work. They find that after twelve weeks of probation (a period etablished by law), new hired workers tend to be more absent. More recently, Bassanini et al. (2008) empirically investigate the impact of EPL on productivity in the OECD. The authors provide some evidence of the negative impact of dismissal regulations on TFP growth, and identify the channel through which it operates. In particular they find that changes in labour composition due to stricter EPL do not play any specific role, while layoff restrictions alter the efficiency improvements and the technological change, thus the productivity.

3 The Italian *Contratto di Formazione e Lavoro*

Before the introduction of the working and training contract, Italian firms could hire either on a permanent basis or through apprenticeship contracts[4]. Employment agencies (*uffici di collocamento*) played a substantial monopolistic role. Thus, CFLs can be viewed as one of the first attempts at introducing flexibility in the labour market. Despite the success of the program over the subsequent years, only few studies have been specifically devoted to

[4]The apprenticeship contract has been introduced in 1959 to provide young people (being less than 19 years old) with a period of specific training aimed to obtain a professional degree certificate.

the analysis of this program[5].

The *Contratto di Formazione e Lavoro* (working and training contract) has been introduced in 1985 in order to tackle with the high unemployment rate among young workers. Initially, the program was targeted to people aged 15 to 29 and it was expected to increase the chance to get a job and to improve human capital accumulation among young workers. Indeed, the program established compulsory training activities (off-the-job training) beside working tasks (on-the-job training and learning by doing). Firms were encouraged to make use of this contract and to provide some forms of training through a structure of incentives, such as the reduction of Social Security fees. The CFL was thought as a fixed term contract, in fact it could not last more than two years and was not renewable. Moreover it could not be converted into an open ended contract before 18 months. After the 18th month and before the expiration of the contract, the firm had the option either to hire the worker on a permanent basis or to dismiss her/him without incurring in any separation cost. As already pointed out, to increase workers' future employability, the CFL included a compulsory training period, but it seems that this feature has remained mostly unheard[6]. Moreover, the program has been implemented *at the margin* introducing some forms of flexibility, in fact it was not targeted to existing workers.

Firms' profitability at using working and training contracts was twofold. First, firms could adjust the labor force at a lower cost in response to specific production needs. In this case, firms did not have any incentive to provide any form of stable training, because it was not convenient to share the cost of training if they knew that they were not able to exploit the potential benefits coming from the higher level of skills acquired by the worker. Thus, if this was the main goal that firms pursued when recruiting young workers, they

[5] Contini, Cornaglia, Malpede and Rettore (2002) and Tattara and Valentini (2005) explore the implications of the programme on the short- and long-term chances to get a job; Contini and Revelli (2004) perform a welfare analysis of the program.

[6] See Contini et al. (2002).

were frustrating the incentive scheme of the CFL program. Second, firms could use these contracts as a screening device through which they could select high ability young workers according to their skill requirement needs. In this case, when firms' skill requirements were met, part of the working and training contracts could be converted into permanent ones.

The effects of higher dismissal costs on small firms can be summarized as follow. An increase in dismissal costs might influence the job contract conversion rate at least in two ways. First, consider a potential direct effect it may exert on permanent job accessions. From the firms' point of view, the fall in the net present value of labour services makes hiring new workers less worth. This means that more employment protection should reduce the overall number of new hirings and thus the number of CFL which are converted in open ended contracts. Moreover, higher EPL increases labour costs, and this could be an incentive for firms to substitute long-term employment with short-term workers. Second, consider a potential indirect effect: as long as the CFL is an alternative recruitment procedure with respect to standard recruitment procedures (direct hiring by means of open ended contracts), an increase in dismissal costs should boost firms to be more aware of the hiring decision. Although the rise of firing costs for small businesses might lower the use of working and training contracts, the profitability of these contracts after the reform should rely more on the firms' screening needs. Since under working and training contracts, firms get to know workers' characteristics and abilities, they should prefer this recruitment procedure, and so if a worker has to be hired, it might be the case that a working and training contract is a good way to reduce the uncertainty about both workers' future performances and the risk of separation. According to this claim, the raise in dismissal costs might have increased the number of new hirings preceded by a CFL, so the job contract conversion rate should be rising as well. Which of these two effects is dominant is ultimately an empirical question and this paper wants to shed some light on this by following the small firms' behavior during the period 1988 to 1994 and compare it with the behavior of large firms.

My research question is relevant at least for two reasons. First it shows that a proper evaluation of Active Labour Market Policies must take into account institutional features, such as EPL. Second, after the diffusion of fixed term contracts in European countries, a key problem is to establish to what extent these contracts represents a stepping stone or a dead end to permanent employment. As shown by Gagliarducci (2005), repeated temporary jobs can be detrimental to workers future performances in terms of labour market outcomes, thus it is interesting to look at the conversion rate of job contracts within the same firm as an important aspect of policy effectiveness.

4 Dismissal costs and job contract conversion rates

4.1 Identification strategy.

In order to identify the causal effect of dismissal costs on the job contract conversion rate, I employ a difference-in-differences strategy (DID). The DID design takes full advantage of the natural experiment set up yielded by the 1990 reform. As a result, individuals belong either to small businesses or large ones and are observed either before or after the reform. In particular, individuals are indexed with the i subscript and belong to one of the mutually excluded groups, $\delta_i \in \{0, 1\}$, where $\delta_i = 0$ refers to being employed in a large firm and $\delta_i = 1$ refers to being employed in a small firm. The first group is the control group (or untreated group), while the second is the treatment group. I consider two independent cross sections, thus each individual is observed only once, either before $(t = b)$ or after $(t = a)$ the treatment. Define $\tau_i \equiv 1[t = a]$, where $1[\cdot]$ is the indicator function. Thus $\tau_i = 0$ if the $i - th$ individual is observed before the treatment and $\tau_i = 1$ if the $i - th$ individual is observed after the treatment. The outcome of interest is the conversion rate of CFL into permanent contracts (in the same firm) and can take values equal to 0 or 1. Let y_i^0 be the potential outcome for the $i-th$ individual when she does not receive the treatment, and y_i^1 be the potential outcome for an individual when she does receive the treatment. Furthermore, let $d_i = \delta_i \cdot \tau_i$ be the indicator of the treatment status, so $d_i = 1$ means that an individual is employed in a small firm after the treatment. What is observed is the triple (y_i, δ_i, τ_i), where $\delta_i = (1-\tau_i)\delta_{ib} + \tau_i\delta_{ia}$, and the actual outcome for individual i is equal to[7]

$$y_i = (1-\tau_i)y_{ib}^0 + \tau_i[(1-\delta_i)y_{ia}^0 + \delta_i y_{ia}^1] \tag{1}$$

The standard difference-in-differences formula is:

$$\begin{aligned} ATT = &\left\{E\left[y_a^1|\delta_a = 1, \tau = 1\right] - E\left[y_b^0|\delta_b = 1, \tau = 0\right]\right\} \\ &- \left\{E\left[y_a^0|\delta = 0, \tau = 1\right] - E\left[y_b^0|\delta = 0, \tau = 0\right]\right\} = DID \end{aligned} \tag{2}$$

For the identification of the treatment effect on the treated $ATT \equiv E(y_a^1 - y_a^0|\delta_a = 1)$, we need the following three conditions to hold[8]:

Condition 1 $E(y_a^0 - y_b^0|\delta_a = 1) = E(y_a^0 - y_b^0|\delta_a = 0)$

Condition 2 $E(y_b^0|\delta_b = 0) = E(y_b^0|\delta_a = 0)$ *and* $E(y_b^0|\delta_b = 1) = E(y_b^0|\delta_a = 1)$

Condition 3 *τ is mean independent of y_t^j given δ_t for all $j = 0, 1$ and $t = b, a$.*

The first condition states that if a difference in the outcome between groups exists, it must be constant over time. If several cross sections were available, the assumption would become a testable one, but in this study this

[7] See Lee (2005).
[8] See, among others, Lee (2005), and Lee and Kang (2006).

is not possible for at least two reasons: first the time dimension has been reduced in order to avoid the overlap of different policies; second, since CFLs last no more than two years, it would be problematic to built subsequent waves, because in each year there could be workers belongig from different waves. To indirectly check the validity of the assumption, I rely on four different subsamples aimed to make the treatment and control groups as similar as possible. In a first subsample, I restrict the control group to individuals belonging to firms with no more than 50 employees. This should reduce considerably any unobserved differences between treated and control units in terms of their time-varying responses to business cycle fluctuations. The remaining three subsamples are built by following a *propensity score overlap* criteria. I exclude from the original sample those observations whose propensity score lies in the tails of the distributions according to three thresholds, 5%, 10% and 15%[9].

Differently from the first identifying condition, conditions 2 and 3 are directly testable. These conditions state that the groups composition must be constant over time, otherwise there would be four different subpopulations which would not be informative to extrapolate any causal effect from the data. In practice, if the two conditions are not violated, any systematic move between groups is ruled out, and this makes the groups comparable. Section 4.3 is devoted to this analysis.

The baseline specification used to estimate the effect of EPL on job contract conversion rates is:

$$E\left[y_i^j|\delta_i, d_i\right] = \beta_0 + \beta_\delta \delta_i + \beta_\tau \tau_t + \beta_d d_i \tag{3}$$

where the dependent variable y_i^j is a binary variable which is equal to 1 every time a working and training contract is converted into a permanent one in the same firm and 0 otherwise. δ_i is a dummy which takes the value of

[9]The Appendix provides a detailed description about the procedure used to estimate the propensity score.

1 whenever a worker is employed in a firm with less than 15 employees; τ_t is the dummy variable which takes the value of 1 for those individuals observed after 1990; d_i is a dummy which takes the value of 1 if an individual is observed after the reform in a small business and is intended to capture the effect of the policy change. To control for the possibility that the change in the outcome is driven by the change in workers' and firms' characteristics, I include a set of covariates which are aimed at relieving this potentially source of bias. The estimated model is thus:

$$E\left[y_i^j|\delta_i, d_i, x_i\right] = \beta_0 + \beta_\delta \delta_i + \beta_\tau \tau_t + \beta_d d_i + \beta_x' x_i \tag{4}$$

where x_i is a vector of workers and firms characteristics, including gender, age, occupation, (log) daily wage, economic sectors, firm's age and average firm size. To control for spacial differences, I also include regional dummies in separate regressions, as well as interaction terms.

Since the Linear Probability Model has many potentially drawbacks, i.e. the predicted probabilities might not lie in the 0-1 interval, it is convenient to rely on an explicit Cumulative Distribution Function. In particular, I conduct probit estimates for all the specifications already outlined and for all the subsamples.

4.2 The dataset and preliminary analysis

To empirically test my research question, I use the Work Histories Italian Panel (WHIP)[10]. The dataset is a 1:90 random sample drawn from the Italian Social Security Administration (INPS) collecting information on employees in private firms on an annual basis. From the original data, I build two independent cross sections, one referring to the pre-reform period and the

[10]WHIP–Work Histories Italian Panel–Full Edition, work histories on Social Security Records compiled by Laboratorio R. Revelli–Centre for Employment Studies/Collegio Carlo Alberto, see http://www.laboratoriorevelli.it/whip.

other referring to the post-reform period[11]. In both cases I select workers hired under working and training contracts and follow them up to their first transition in a different labour status. I end up with two waves, the first includes 3328 observations (with 48.32% employed in small firms, and 51.68% in large firms), while the second is made up of 2997 observations (with 49.85% employed in small firms, and 50.15% in large firms). Since the maximum contract length could never exceed two years, I start following workers since 1987 and 1991. In this way, those who begun working in 1987 have been followed up to 1989, and, similarly, those who begun working in 1991 have been followed up to 1993[12]. For each worker, I observe the first transition out of the CFL status. I exploit the fact that the WHIP is a linked employer-employee dataset, so it is possible to know whether or not the subsequent job was in the same firm and under a permanent position. Every time a CFL is converted into a permanent contract in the same firm, the dependent variable takes the value of 1. For each worker, the dataset provides information on individual characteristics - gender, age, daily wage and worker/employee status- and on firm's characteristics - firm's age, economic sector, average firm size and localization on a regional basis[13].

Table 1 reports the number and percentages of observations for each time period and for control and treatment groups. Note that 1494 individuals were exposed to the treatment. The table also shows the number of transitions into permanent contracts in the same firm by firms' type and for both periods. With respect to the pre-reform period, the total number of CFL signed in 1991 declines both for large firms (-12.62%) and small firms (-7.09%). While

[11]This choice is driven by the fact that I need to cover a period as homogenous as possible in terms of the underlying legislation.

[12]This choice allows me to get a pre-reform wave which is totally unaffected by the 1990 reform because the last job contract conversion happens to be in 1989. Furthermore, by selecting the 1991 wave, I avoid taking into account intermediate waves (i.e. the 1989 wave) because the reform might not fully have exerted its effects on them.

[13]One limit of this study is that few information is available about individuals characteristics such as education.

for small businesses the proportion of conversions ($y = 1$) is almost the same in the two periods, for large businesses there is an evident decline in the conversion rate of CFL into permanent contracts.

Table 1: Transition matrix

	PRE-REFORM		POST-REFORM		
	Y=0	Y=1	Y=0	Y=1	SUM
LARGE FIRMS	695 (40.41%)	1025 (59.59%)	687 (45.71%)	816 (54.29%)	3223
SMALL FIRMS	970 (60.32%)	638 (39.68%)	918 (61.45%)	576 (38.55%)	3102
SUM	1665	1663	1605	1382	6325

Table 2 shows descriptive statistics by firm size, before and after the 1990 reform. Large firms are systematically older than small businesses, employ less women and pay slightly higher wages. Small firms are absent from the energy sector and are less present in the manufacturing sector, while they are more present as retailers and wholesalers.

4.3 Assessing the balance of covariate distributions

Before running any regression, it is important to check whether there is enough balance in the covariate distributions among treated and control groups. This step is particularly relevant in my analysis because the data come from an observational study. While in experimental studies, the researcher has the opportunity to design the experiment to obtain exact balance of covariates (and the bias related to the differences in the covariate distributions is mostly ruled out), in non-experimental data it might be the case that this source of bias is present, and must be appropriately taken into account. The most evident case of imbalance is when the support of a given covariate is different among treated and control groups, so there are ranges of covariate values that we do not observe in all the groups. In the WHIP

Table 2: Individual descriptive statistics (full sample)

	Small firms				Large firms			
	(1) Pre-90		(2) Post-90		(3) Pre-90		(4) Post-90	
Variables	Mean	S.D	Mean	S.D.	Mean	S.D.	Mean	S.D.
Gender	0.524	0.500	0.583	0.493	0.697	0.460	0.675	0.469
Age	22.169	2.818	22.950	3.352	22.35	3.163	22.936	3.184
Worker	0.574	0.495	0.645	0.479	0.655	0.475	0.643	0.479
Log daily wage	3.282	0.264	3.555	0.237	3.391	0.232	3.649	0.260
Firm's age	6.241	5.981	7.847	6.721	12.628	8.126	14.073	9.150
Extraction	0.001	0.035	0.003	0.052	0.001	0.024	0.001	0.036
Manufacturing	0.378	0.485	0.373	0.484	0.638	0.481	0.532	0.499
Energy	-	-	-	-	0.006	0.076	0.007	0.081
Building	0.095	0.293	0.131	0.337	0.070	0.255	0.094	0.293
Retail/wholesale	0.262	0.440	0.266	0.442	0.131	0.338	0.162	0.368
Hotels/restaurants	0.054	0.226	0.042	0.201	0.022	0.147	0.033	0.178
Transports	0.014	0.116	0.014	0.117	0.023	0.150	0.029	0.167
Finance	0.017	0.129	0.012	0.109	0.021	0.143	0.041	0.197
Real estate	0.150	0.357	0.128	0.334	0.074	0.262	0.088	0.284
Social services	0.030	0.170	0.031	0.173	0.014	0.117	0.014	0.117
Piemonte	0.142	0.330	0.072	0.259	0.148	0.355	0.102	0.303
Val d'Aosta	0.002	0.050	0.004	0.063	0.002	0.048	0.004	0.063
Liguria	0.027	0.163	0.027	0.161	0.014	0.117	0.017	0.130
Lombardia	0.247	0.431	0.198	0.399	0.266	0.442	0.238	0.426
Trentino A.A.	0.040	0.196	0.031	0.173	0.027	0.161	0.034	0.181
Veneto	0.142	0.349	0.116	0.321	0.124	0.330	0.116	0.321
Friuli V.G.	0.052	0.221	0.037	0.188	0.042	0.202	0.034	0.181
Emilia Romagna	0.129	0.335	0.103	0.304	0.141	0.348	0.096	0.294
Marche	0.034	0.182	0.019	0.136	0.027	0.161	0.027	0.163
Toscana	0.065	0.246	0.067	0.250	0.056	0.231	0.054	0.226
Umbria	0.024	0.152	0.024	0.153	0.018	0.133	0.013	0.115
Lazio	0.066	0.248	0.116	0.320	0.056	0.230	0.086	0.280
Campania	0.010	0.099	0.042	0.201	0.019	0.135	0.051	0.221
Abruzzo	0.008	0.090	0.023	0.149	0.016	0.127	0.026	0.159
Molise	0.003	0.056	0.008	0.089	0.003	0.059	0.006	0.077
Puglia	0.012	0.111	0.045	0.206	0.018	0.133	0.045	0.206
Basilicata	0.002	0.043	0.017	0.128	0.004	0.064	0.007	0.081
Calabria	0.001	0.035	0.012	0.109	0.004	0.064	0.007	0.085
Sicilia	0.004	0.061	0.018	0.133	0.010	0.099	0.018	0.133
Sardegna	0.008	0.090	0.021	0.145	0.003	0.059	0.017	0.130

data, we run into this situation in just one case, namely the energy sector[14]. Moreover, even if there is enough overlap in covariate supports, the distributions might differ in their shape. Here I try to mimic the output of the "research design" phase by assessing the balance of covariate distributions. If sufficient balance is there, then the control groups are more likely to have similar responsiveness to the underlying economic environment[15]. Moreover, the estimates become more credible and inference is more robust because it is less likely that systematic differences among groups' characteristics bias the results.

The assessment of the balance in covariate distributions is carried out on a univariate basis through mean-comparison tests[16], normalized differences in averages and differences in log-standard deviations for each covariate. I also conduct a graphical analysis of the covariates' distributions to capture any difference which is not detected by the above mentioned analysis. In particular, I construct histograms for those variables presenting symptoms of imbalance[17].

With a known univariate distribution, let call its first and second order moments, respectively, $\mu_\delta = E[X|\Delta = \delta]$ and $\sigma_\delta^2 = V[X|\Delta = \delta]$, with $\delta = t, c_j$ - where t is for treated and c_j is for the $j - th$ control group. The univariate analysis will look at the following measures:

i. $\Delta_1 = \left(\mu_t - \mu_{c_j}\right)$

ii. $\Delta_2 = \dfrac{\left(\mu_t - \mu_{c_j}\right)}{\sqrt{\sigma_t^2 + \sigma_{c_j}^2}}$

iii. $\Gamma = \ln(\sigma_t) - \ln\left(\sigma_{c_j}\right)$

[14] Note that by excluding the energy sector from the analysis, I only loose five observations.

[15] See Eissa and Liebman (1996).

[16] Every time the covariate under analysis is a dummy variable, a test of proportion is carried out, while when the covariate is not binary, a standard t-test is performed.

[17] Histograms are available upon request.

To estimate these measures, a natural way is to use sample means and variances[18]. Let $\overline{X}_t$ and $\overline{X}_c$ the sample averages for treated and control groups, with $\overline{X}_t = \frac{1}{N_t} \sum_{i;W_i=t} X_i$ and $\overline{X}_{c_j} = \frac{1}{N_{c_j}} \sum_{i;W_i=c_j} X_i$; where N_t is the number of treated units and N_{c_j} is the number of control units in group j. Moreover, let s_t^2 and $s_{c_j}^2$ the sample covariate variances, with $s_t^2 = \frac{1}{N_t - 1} \sum_{i;W_i=t} \left(X_i - \overline{X}_t\right)^2$ and $s_{c_j}^2 = \frac{1}{N_{c_j} - 1} \sum_{i;W_i=c_j} \left(X_i - \overline{X}_{c_j}\right)^2$. Thus, the sample counterparts of (i)-(iii) are:

i'. $\hat{\Delta}_1 = \left(\overline{X_t} - \overline{X}_{c_j}\right)$

ii'. $\hat{\Delta}_2 = \dfrac{\left(\overline{X_t} - \overline{X}_{c_j}\right)}{\sqrt{s_t^2 + s_{c_j}^2}}$

iii'. $\hat{\Gamma} = \ln\left(s_t\right) - \ln\left(s_{c_j}\right)$.

Formula (i') has been used to compute mean comparison tests, and table 3 reports the p-values. Under the null hypothesis the group averages are equal, so we do not wish to reject the null hypothesis, since our hope is to find similar averages among treated and controls. Note that in the difference-in-differences set up, we have four groups, only one of them is the treated group (small firms after the 1990 reform), while the others can be all thought of as control groups. Thus, I compare sample covariate averages for treated and control groups. In column (A), the control group is "large firms after the reform"; in column (B) the control group is "small firms before the reform"; in column (C) the control group is "large firms before the reform". In each cell, I report the difference in averages and the p-values in parentheses.

At a 10% level of significance, we do not reject the null hypothesis of equal averages in 40 out of 102 cases; at a 5% level of significance there are 46 cases in which I do not reject the null; while at a 1% level of significance the cases are 53. Even though the percentage of rejections is always below

[18] See Imbens and Rubin (2008).

Table 3: Group average tests

Variable	(A)		(B)		(C)	
Gender	-0.092	(0.0000)	0.059	(0.0010)	-0.114	(0.0000)
Age	0.014	(0.9089)	0.781	(0.0000)	0.600	(0.0000)
Worker	0.003	(0.8849)	0.071	(0.0000)	0.010	(0.5537)
Log daily wage	-0.094	(0.0000)	0.273	(0.0000)	0.164	(0.0000)
Firm's age	-6.226	(0.0000)	1.606	(0.0000)	-4.781	(0.0000)
Extraction	0.001	(0.4096)	0.001	(0.3639)	0.002	(0.1326)
Manufacturing	-0.158	(0.0000)	-0.005	(0.7909)	-0.265	(0.0000)
Building	0.036	(0.0018)	0.036	(0.0015)	0.060	(0.0000)
Retail/wholesale	0.105	(0.0000)	0.005	(0.7723)	0.135	(0.0000)
Hotels/restaurants	0.010	(0.1674)	-0.012	(0.1215)	0.020	(0.0011)
Transports	-0.015	(0.0059)	0.000	(0.9289)	-0.009	(0.0566)
Finance	-0.029	(0.0000)	-0.005	(0.2692)	-0.009	(0.0507)
Real estate	0.039	(0.0005)	-0.022	(0.0766)	0.054	(0.0000)
Social services	0.017	(0.0018)	0.001	(0.8788)	0.017	(0.0011)
Piemonte	-0.030	(0.0035)	-0.052	(0.0000)	-0.076	(0.0000)
Val d'Aosta	0.0000	(0.9917)	0.002	(0.4530)	0.002	(0.3907)
Liguria	0.009	(0.0772)	-0.000	(0.9195)	0.013	(0.0095)
Lombardia	-0.040	(0.0079)	-0.049	(0.0011)	-0.068	(0.0000)
Trentino A.A.	-0.003	(0.6269)	-0.009	(0.1751)	0.004	(0.4927)
Veneto	0.0000	(0.9978)	-0.025	(0.0359)	-0.008	(0.4901)
Friuli V.G.	0.003	(0.6693)	-0.015	(0.0457)	-0.006	(0.4158)
Emilia Romagna	0.007	(0.5060)	-0.026	(0.0260)	-0.038	(0.0012)
Marche	-0.009	(0.1192)	-0.015	(0.0077)	-0.008	(0.1314)
Toscana	0.013	(0.1340)	0.002	(0.7999)	0.011	(0.2141)
Umbria	0.011	(0.0292)	0.000	(0.9325)	0.006	(0.2294)
Lazio	0.030	(0.0064)	0.050	(0.0000)	0.060	(0.0000)
Campania	-0.009	(0.2398)	0.032	(0.0000)	0.024	(0.0001)
Abruzzo	-0.003	(0.5711)	0.015	(0.0008)	0.006	(0.1829)
Molise	0.002	(0.5024)	0.005	(0.0635)	0.005	(0.0851)
Puglia	0.000	(0.9716)	0.032	(0.0000)	0.027	(0.0000)
Basilicata	0.010	(0.0102)	0.015	(0.0000)	0.013	(0.0003)
Calabria	0.005	(0.1860)	0.011	(0.0002)	0.008	(0.0102)
Sicilia	0.000	(0.9822)	0.014	(0.0001)	0.008	(0.0463)
Sardegna	0.005	(0.4130)	0.013	(0.0019)	0.018	(0.0000)

52%, and many times the tests suggest unequal averages, the differences do not seem to be drastically away from each other. In fact, even if the mean comparison test rejects the null hypothesis, the difference in averages is often less than a standard deviation[19]. This suggests that it is important to use a measure able to take into account the dispersion in covariate distributions. This check is shown in table 4 which reports the normalized differences in averages[20] (columns 2 to 4) and the differences in log-standard deviations (columns 5 to 7)[21]. From the inspection of columns 2 to 6, we can see that there is overall balance among groups exept for two variables, namely the log daily wage and firms age. This suggests that the full sample can be conveniently used as a starting point for the analysis, while more accurate estimates can be conducted on the subsamples already mentioned.

4.4 The effects of the 1990 reform on CFL conversion rates

Tables 5 and 6 show the DID results from OLS and probit estimates. Both tables report marginal effects estimated on the full sample of CFL workers. Table 7 reports the results from probit estimates carried out on four different subsamples. Labels (S1)-(S4) refer to different specifications of the model: (S1) is the baseline specification, (S2) adds workers' and firms' characteris-

[19] Here I refer to the standard deviations computed for the covariates of the treated group.

[20] Note that the normalized difference is a useful tool because it is a pure measure of localization corrected by the square root of the sum of variances. An example might clarify this point. Suppose we have two cases both with a small difference in means (inducing the reader to think that the situation is positive), but in the first case the variances are very low, while in the second are very large. If we do not correct for the variance, we are not able to detect the lack of overlap around the averages. In fact, when the two distributions are very concentrated, even a small difference in means must be looked as a potential source of bias.

[21] The indexes refer to the comparison between treated (t) and one of the control groups: c_1 is "small firms before the reform", c_2 is "large firms before the reform" and c_3 is "large firms after the reform".

Table 4: Normalized difference in averages and differences in the log-standard deviations

Variables	$\hat{\Delta}_{tc_1}$	$\hat{\Delta}_{tc_2}$	$\hat{\Delta}_{tc_3}$	$\hat{\Gamma}_{tc_1}$	$\hat{\Gamma}_{tc_2}$	$\hat{\Gamma}_{tc_3}$
Gender	0.08	0.17	0.13	-0.01	0.07	0.05
Age	0.18	0.13	0.00	0.17	0.06	0.05
Worker	0.10	-0.01	0.00	-0.03	0.01	0.00
Log daily wage	0.77	0.49	0.27	-0.10	0.02	-0.09
Firm's age	0.18	0.45	0.55	0.12	-0.19	-0.31
Extraction	-	-	-	-	-	-
Manufacturing	0.00	0.39	0.23	0.00	0.01	-0.03
Energy	-	-	-	-	-	-
Building	0.08	0.14	0.08	0.14	0.28	0.14
Retail/wholesale	0.00	0.24	0.18	0.01	0.27	0.18
Hotels/Restaurants	-0.04	0.08	0.04	-0.12	0.31	0.12
Transports	0.00	-0.05	-0.07	0.01	-0.25	-0.35
Finance	-0.03	0.05	0.13	-0.16	-0.27	-0.59
Real estate	-0.05	0.13	0.09	-0.07	0.24	0.16
Social services	0.00	0.08	0.08	0.02	0.39	0.39
Piemonte	0.12	0.17	-0.08	-0.24	-0.32	-0.16
Val d'Aosta	0.02	0.02	0.00	0.24	0.27	0.00
Liguria	0.00	0.06	0.05	-0.01	0.32	0.21
Lombardia	-0.08	0.11	-0.07	-0.08	-0.10	-0.07
Trentino Alto Adige	-0.03	0.02	-0.01	-0.12	0.07	-0.05
Veneto	-0.05	-0.02	0.00	-0.08	-0.03	0.00
Friuli Venezia Giulia	-0.05	-0.02	0.01	-0.16	-0.07	0.04
Emilia Romagna	-0.06	-0.08	0.02	-0.10	-0.13	0.03
Marche	-0.07	-0.04	-0.04	-0.29	-0.17	-0.18
Toscana	0.00	0.03	0.04	0.02	0.08	0.10
Umbria	0.00	0.03	0.05	0.01	0.14	0.29
Lazio	0.12	0.15	0.07	0.25	0.33	0.13
Campania	0.18	0.10	-0.03	0.71	0.40	-0.09
Abruzzo	0.08	0.03	-0.01	0.51	0.16	-0.06
Molise	0.05	0.04	0.02	0.47	0.41	0.15
Puglia	0.14	0.11	0.00	0.62	0.44	0.00
Basilicata	0.11	0.09	0.07	1.09	0.70	0.46
Calabria	0.09	0.06	0.03	1.13	0.54	0.25
Sicilia	0.10	0.05	0.00	0.78	0.30	0.00
Sardegna	0.08	0.11	0.02	0.48	0.90	0.10

tics, (S3) controls for regional dummies, (S4) includes interaction terms. The coefficient of interest is the interaction term between the small firm dummy and the post treatment dummy. The results are clustered around 4 and 8% and are statistically significant in all but one case. More interestingly the sign is positive in all the specifications and for all the subsamples used for the estimations. This is somewhat reassuring, because it shows that there is a clear cut dominance of the enhancing effect of EPL on CFL conversion into permanent jobs. The coefficients reported in table 7 confirm the results and can be interpreted as a robustness check. The estimates conducted on the subsample which limit the size of control firms to the 50 employees threshold is of particular interest. Indeed, the increase in the comparability of treatment and control groups tends to emphasize the positive effect of EPL on the job contract conversion rate. Moreover, even though a large number of observations are dropped, the magnitude of the effect of the reform is very similar to the one found in other specifications. In the baseline model the coefficient is 7%, and in all the other specifications is around 8% with a standard deviation of 0.03. This can be interpreted as evidence of a switching behavior of small firms towards a more parsimonious use of working and training contracts as a way to select workers. The threat of dismissal costs makes firms more aware of the risk of separation, and CFL contracts represents a sort of insurance against this risk because firms can acquire information about workers, and - maybe more important - workers can figure out how their working life will be if they decide to sign an open-ended contract in that firm. Thus, firms are more willing to select workers for their stable workforce among those already trained under CFLs and that are less likely to start a separation process. It should also be noted that since I adopt a regression control strategy, an important check is to look at the sensitivity of the estimates to the progressive inclusion of control variables[22]. From the tables, it is straightforward to notice that the coefficients are substantially

[22] See Angrist and Krueger (1999).

stable after regional and sector dummies are included, as well as interaction terms are added to the regressions. For example, the first column of results in table 7 shows that the average treatment effect ranges between 0.071 to 0.081. In particular, the effect is equal to 0.071 in the baseline specification, and once I progressively add workers' and firms' characteristics plus economic sector dummies, regional dummies and interaction terms, the estimates are, respectively, 0.079, 0.081 and 0.080.

The results of this study have aslo policy implications. In the absence of EPL, a minor fraction of temporary workers is retained by firms. This implies that firms, anticipating this outcome, are less likely to improve training activities for fixed-term workers, reducing the overall degree of future workers employability. This channel acts through the slow productivity growth implied by less training activities.

5 Conclusions

In this paper I study the impact of stricter employment protection legislation (in the form of higher dismissal costs) on job contract conversion rates. Exploiting the Italian1990 reform which increased unfair dismissal costs for businesses below the 15 employees threshold, and looking at the pre- and post-reform CFLs conversion rates, I find that a small, but not negligible effect was actually there, meaning that dismissal costs made firms more parsimonious in their hiring procedures. The conversion of CFLs signed in small firms is 5-8% higher relative to that of large firms. Given that firms could also be interested in retaining some workers, higher EPL pushes firms to be more aware of the "screening side" of temporary contracts.

Table 5: The effect of the Italian 1990 reform on the job contract conversion rate

Regressors	Linear probability model							
	(S1)		(S2)		(S3)		(S4)	
Small firms	-0.199***	(0.017)	-0.157***	(0.018)	-0.156***	(0.018)	-0.155***	(0.018)
Post treatment	-0.053***	(0.017)	-0.066***	(0.018)	-0.066***	(0.018)	-0.066***	(0.018)
Small × Post	0.042*	(0.025)	0.050**	(0.025)	0.051**	(0.025)	0.053**	(0.025)
Gender	-		-0.021	(0.014)	-0.023	(0.014)	-0.013	(0.024)
Age	-		0.000	(0.002)	0.000	(0.002)	0.000	(0.002)
Worker	-		-0.093***	(0.015)	-0.095***	(0.015)	-0.098***	(0.029)
Log daily wage	-		0.034	(0.025)	0.028	(0.025)	0.029	(0.025)
Firm's age	-		0.005***	(0.001)	0.004***	(0.001)	0.005***	(0.001)
Economic sectors	NO		YES		YES		YES	
Regional dummies	NO		NO		YES		YES	
Interaction terms	NO		NO		NO		YES	
Constant	0.596***	(0.012)	0.370**	(0.171)	0.348*	(0.180)	0.327*	(0.181)
F-statistics	74.18		33.56		17.16		15.27	

Notes: N=6325. *** denotes significance at 1% level, ** denotes significance at 5% level, * denotes significance at 10% level. Robust standard errors in parentheses.

Table 6: Results from Probit estimations (full sample)

Regressors	Probit estimates							
	(S1)		(S2)		(S3)		(S4)	
Small firms	-0.199***	(0.017)	-0.161***	(0.019)	-0.161***	(0.019)	-0.160***	(0.019)
Post treatment	-0.054***	(0.018)	-0.069***	(0.019)	-0.070***	(0.019)	-0.070***	(0.019)
Small $\times$ Post	0.042*	(0.025)	0.052**	(0.026)	0.053**	(0.026)	0.056**	(0.026)
Gender	-		-0.022	(0.015)	-0.025*	(0.015)	0.013	(0.025)
Age	-		0.000	(0.002)	0.000	(0.002)	0.000	(0.002)
Worker	-		-0.096***	(0.016)	-0.098***	(0.016)	-0.102***	(0.030)
Log daily wage	-		0.038	(0.026)	0.031	(0.026)	0.032	(0.026)
Firm's age	-		0.005***	(0.001)	0.005***	(0.001)	0.005***	(0.001)
Economic sectors	NO		YES		YES		YES	
Regional dummies	NO		NO		YES		YES	
Interaction terms	NO		NO		NO		YES	
Wald statistics	213.65		403.43		437.58		443.20	

Notes: N=6325. *** denotes significance at 1% level, ** denotes significance at 5% level, * denotes significance at 10% level. Robust standard errors in parentheses.

Table 7: Estimation results from different subsamples

		Subsamples							
Model	Description	<50		PS1		PS2		PS3	
(S1)	Small firm	-0.142***	(0.022)	-0.196***	(0.017)	-0.195***	(0.017)	-0.183***	(0.018)
	Post treatment	-0.081***	(0.026)	-0.056***	(0.018)	-0.072***	(0.018)	-0.075***	(0.019)
	Small × Post	0.071**	(0.032)	0.047*	(0.026)	0.059**	(0.026)	0.057**	(0.027)
(S2)	Small firm	-0.142***	(0.023)	-0.161***	(0.019)	-0.162***	(0.019)	-0.159***	(0.019)
	Post treatment	-0.072**	(0.028)	-0.066***	(0.020)	-0.076***	(0.020)	-0.074***	(0.021)
	Small × Post	0.079**	(0.032)	0.053**	(0.026)	0.063**	(0.026)	0.059**	(0.027)
(S3)	Small firm	-0.143***	(0.023)	-0.162***	(0.019)	-0.163***	(0.019)	-0.159***	(0.020)
	Post treatment	-0.071**	(0.028)	-0.066***	(0.020)	-0.076***	(0.020)	-0.074***	(0.021)
	Small × Post	0.081**	(0.032)	0.055**	(0.026)	0.064**	(0.026)	0.060**	(0.028)
(S4)	Small firm	-0.140***	(0.023)	-0.160***	(0.019)	-0.161***	(0.019)	-0.158***	(0.020)
	Post treatment	-0.071**	(0.028)	-0.067***	(0.020)	-0.076***	(0.020)	-0.074***	(0.021)
	Small × Post	0.080**	(0.033)	0.057**	(0.026)	0.066**	(0.027)	0.063	(0.028)
	N. obs	4493		6238		6035		5587	

Notes:

* denotes

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Appendix

Subsamples

Since CFL workers can be observed in just one period, either before or after the 1990 reform, the data do not have a longitudinal component, but I am still able to build two waves. A crucial point is that these two waves must be comparable in order to proceed with the analysis. If the composition of small and large firms varies substantially between waves, the empirical analysis becomes unfeasible. I check the plausibility of the strategy adopted to build the data by comparing the propensity score distributions for small and large firms in both waves. Since the work of Rosenbaum and Rubin (1983), the propensity score has been widely used to reduce the dimension of the conditioning problem in matching methods. Since in this study, the set of covariates includes 35 variables, for whom I can only make inference on the marginal distributions, a practical solution is to look at the propensity score distributions.

In order to find a specification for the propensity score, I apply an iterative procedure as suggested by Imbens and Rubin (2008). Among the set of all K covariates, I first set up a model in which the propensity score is a linear function of the following set of K_B variables: gender, worker, log-daily wage and firm's age. Then, I run $K - K_B$ logistic regressions including each time a different covariate and I perform a likelihood ratio test for the additional covariate. I use the LR-test statistics to rank the $K - K_B$ covariates, and among them I choose the one with the highest test statistic to enter the propensity score specification. I repeat the procedure on the remaining covariates until none of the LR-test statistics is greater than the 2.71 cutoff value, which corresponds at a 10% level of significance[23]. According to this iterative procedure, I select a subset K_S made up of eleven covariates. Using the $(K_B + K_S)$ set of covariates, I generate interaction terms[24] and select those who perform well in terms of likelihood ratio test, as before[25]. I then estimate the propensity score according to the following logistic equation:

$$\Pr(\text{small firm} = 1) = \left(\frac{1}{1 + e^{-X\beta}}\right) \tag{5}$$

where X contains all the selected covariates and the interaction terms. Figure 2 shows the estimated propensity score by firm size, before and after the reform. The solid lines are kernel plots of the propensity score distributions. From the inspection of the histograms, we can see that there are no drastic changes between the two waves of the cross-sections.

[23]The table with the LR-test statistics is available on request.

[24]Note that some of the $N(N-1)/2$ possible interactions (where N is the number of the $K_B + K_S$ covariates) are meaningless (interactions among regions and interactions among economic sectors), while other interaction terms have not been computed because of the small number of observations.

[25]I end up with five interactions, in particular the interaction of the worker variable with, respectively, gender, manufacturing and building, and the interaction of the variable gender with, respectively, manufacturing and lombardia.

Figure 1: Propensity score distributions

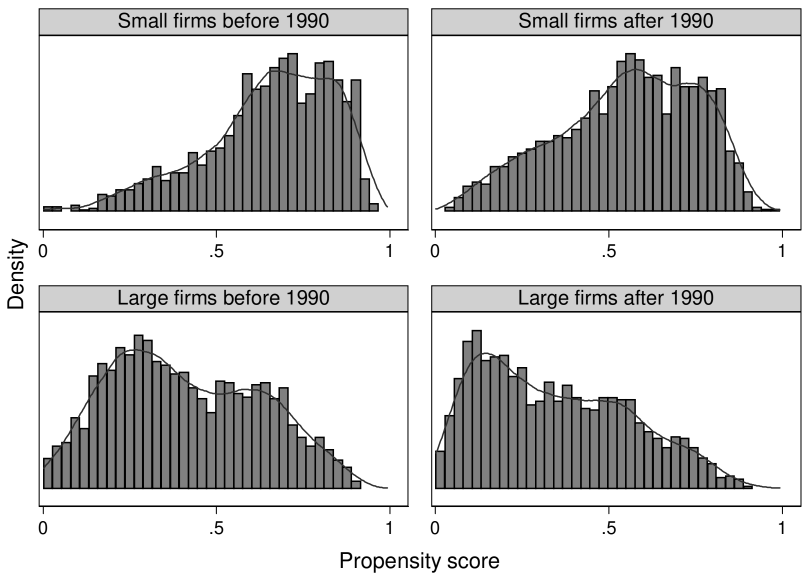

Figure 2: