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Institutions and Labour Market Performance in Italy

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Abstract

My research focuses on the interactions between institutions and the functioning of the labour market. In this respect Italy represents an interesting "case study". It is commonly considered a highly regulated market; however, there is evidence of flexibility higher than expected. Hence it must be investigated whether this postulated rigidity is real at the firm level and whether existing institutions and regulations are actually binding on human resource management at the firm level.

My contribution relies on the attempt to measure the effect of a selected set of regulations on the behaviour of the firms. I focus on different pieces of legislation, all of them recently under scrutiny to be reformed, all of them relevant to shape the functioning of the Italian labour market. Employment Protection Legislation is made of several provisions: restrictions on firings, severance payments, notice periods are the most common. I focus on the first two: a provision akin to a *severance payment* and the regulation of *individual layoffs*. *Temporary contracts* are a way to avoid firing costs altogether, and are analysed next. Preliminary to all this is the analysis of the *wage setting process*, as the effects of EPL depend on how much wages are flexible. My approach is mainly empirical, and relies on the use of a very rich data archive.

After assessing that Italian wages are quite rigid, and that only about 10% of the wage is not set outside the employer-individual employee relationship, I draw two general conclusions. (i) The estimated effect of the norms I analyse is always statistically significant, but it is always small; (ii) the effect is increasing with firm size. This is coherent with a flexibility higher than expected in a deeply regulated market. Some hints pointing to a segmented market and to a non universal enforcement of the norms emerge as well.

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Foreword

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Introduction

This thesis focuses on the interactions between institutions and the functioning of the labour market. In this respect Italy represents an interesting “case study”. It is commonly considered a highly regulated market; both employment and wages are often thought to be rigid downward and this rigidity restrains firms from increasing employment when the business cycle is positive¹. Contrary to this widely accepted idea, there is evidence of higher than expected flexibility. Since the early '90s, the OECD² has highlighted that job turnover in Italy is higher than in Germany and comparable to job turnover in the US. Workers' mobility is substantial, yearly separations involve nearly one third of jobs and one fourth of employees³. Small firms hire and fire workers quite freely; larger firms seem to use collective temporary or permanent layoff tools easily and without administrative impediments⁴. Hence it must be investigated whether this postulated rigidity is real at the firm level and whether existing institutions and regulations are actually binding on human resource management at the firm level.

One answer lies on the possible segmentation of the Italian labour market into a protected, or rigid, primary market and a less protected (more flexible) secondary market. Flexibility is concentrated among small firms, young workers, temporary contracts. However, firm size seems to drive the segmentation; in fact young workers employed in large firms enjoy a more stable working condition. The secondary market may be large. About 30% of the employees and more than 80% of firms operate below the 15 employees threshold in the private sector. By contrast, in France, Germany and United Kingdom 30% of the employees works in firms below the 100 employees threshold⁵. In addition, self employment involves one worker out of four (in the above quoted countries one worker out of ten is self employed⁶) and the hidden economy might be as large as 10-15% of total employment: both are flexible environments by definition.

A partially different answer points to the distance between the "law in the books" and the "law in action", i.e. to the enforcement of the norms; distance that might be substantial

¹Bentolila S. and G. Bertola (1990). 'Firing costs and labour demand: how bad is eurosclerosis?' Review of Economic Studies vol. 57, pp. 381-402.

²OECD Employment Outlook, 1994.

³CONTINI B. (2002), Labour Mobility and Wage Dynamics in Italy (ed. by), Rosenberg & Sellier Publishing.

⁴Del Boca A. and P. Rota (1998). 'How much does hiring and firing cost? Survey evidence from a sample of Italian firms', Labour vol. 12, pp. 427-449.

⁵TRAÛ F. (1999): "*La Questione Dimensionale Nell'Industria Italiana*" (ed. by) Bologna, Il Mulino.

⁶Villosio C. (2000), "Analysis of Self-Employment in the European Union", Study for the Commission of the European Communities.

in Italy. In addition there are norms targeted to undo the rigidity generated by other norms. E.g., in large firms firing a worker is costly, time consuming and not always possible; however, collective temporary or permanent layoff tools are easily available, provided that their use is agreed with unions and with local Public Labour Offices. Unfortunately, measuring the distance between the "law in the books" and the "law in action" is extremely difficult.

The two answers are clearly correlated; law enforcement is likely to decrease with firm size and to be lower in the secondary market in general (needless to say, small firms and secondary market do not coincide necessarily). And, both answers are likely to be relevant. My contribution relies on the attempt to measure the effect of a selected set of regulations on the behaviour of firms.

I focus on four different pieces of legislation, all of them recently under scrutiny and relevant to shape the functioning of the Italian labour market. Three of them relate to Employment Protection Legislation. As some effects of EPL depend on how much wages are flexible, I first analyse the wage setting process and assess whether wages are rigid. Employment Protection Legislation contains several provisions related to restrictions on firings, severance payments, notice. I focus on the first two: a provision akin to a severance payment and the regulation of individual layoffs. Temporary contracts allow to avoid firing costs altogether, and are analysed next. I describe briefly these provisions, summarize the results of the analyses and finally draw some general conclusions. My approach is mainly empirical, and it relies on the use of a very rich data base. I exploit the timing of reforms, and I take into account firm size in every piece of analysis.

Collective agreements fix minimum wages by industry and occupation in a rather detailed fashion. Until the early nineties, wages were set within an industry-wide, centralized wage setting process accompanied by an automatic price-indexation system. The July 1993 Income Policy Agreement changed this setting. It aimed at curbing inflation and increasing wage flexibility and responsiveness to local conditions. It abolished the automatic price-indexation clause and introduced a new two-level bargaining system. At the national level, wages are now set according to the inflation rate targeted by the Government for the following 24 months. At the regional or firm level, additional wage components are introduced and are to be geared by profit sharing considerations. Top-up wage components with respect to the nationally set wage cover about 20% of total wages; they refer to both locally bargained wage components (i.e. second level contracts and productivity premia; they account for about half top-ups) and individual premia.

Hence, on average, only about 10% of the wage is not set outside the employer-employee individual relationship. I analyse the effects of the 1993 reform on the responsiveness of wages to local labour market conditions. To do so I estimate a standard wage curve and test the existence of a structural break after 1993. I find that the elasticity of wages to local labour market conditions increases in absolute value and becomes statistically significant. Still, the estimated elasticity is low, below 3%. The sources of wage flexibility in an environment where collective contracts were and still are very influential are the top-up wage components, which display a much higher elasticity to local unemployment (about 7.5%). However, quantile regressions prove that wages are also responsive to local unemployment when they are low (in the first two deciles of the wage distribution). Results by firm size confirm that top-up components are (very) responsive to local unemployment only in large firms; otherwise (low) total wages are themselves sensitive to local labour market conditions, in small firms. These results point to the existence of a dual labour market, with rigid wages unless they are very low, and top-up components that are more flexible but small. These results also suggest a difference between the law in the books and the law in action: low wages decrease when local unemployment increases, but the elasticity to unemployment of their two components (collective national contract and top-up components) is zero; do they decrease below the national contract minimum?

I next analyse the effect of severance payments on the individual probability of (firm-initiated) separation. In Italy statutory severance payments do not exist; a mandatory deferred wage scheme is akin to a severance payment under specific conditions, which are fulfilled in Italy. The *Trattamento di Fine Rapporto* (compensation upon separation, *TFR* hereafter) is a mandatory deferred wage scheme in place since 1924. During the entire working relationship, the worker accumulates a credit vis-à-vis the firm, which is in turn obliged to recapitalize its debt to the worker at policy determined interest rates. The deferred wage is paid to the worker at the end of the employment relationship, regardless of the reasons behind the separation. *TFR* is akin to a severance payment as long as wages are rigidly set outside the employer-employee relationship, and deferred wages are accumulated below market rates (both conditions hold in Italy). Hence, a scheme like *TFR* increases the firm propensity to hoard labour (chapter 2 provides the formal proof of these statements). In 2004 the Italian Parliament approved a social security reform that envisages a two pillar system. The first pillar will be a national pension. The second pillar will be made by private pension schemes. The law considers

the *TFR* as a base payment to fill up the private pension funds. This reform has clear implications in the social security area. But it will have an indirect effect on the labour market as well, as a shift of *TFR* funds into pension funds will increase labour turnover because it will decrease the firm propensity to hoard labour. I test the impact of *TFR*-severance payment on the probability of firm-initiated separation between t and $t + 1$ conditional on having a tenure of at least t , using a variety of survival models. To be able to identify the labour hoarding effect I exploit a feature of the law that grants the worker the right to withdraw part of the *TFR* in advance, so that it generates variability in the *TFR* stock at given tenure and wage. Overall, I find that withdrawing the *TFR* fund significantly increases the hazard rate of firm-initiated separation. The results are also quantitatively non negligible, given that *TFR* was not supposed to be related to labour market behaviour. As an example, an individual with at least ten years of tenure who withdraws 60% of the *TFR* fund increases his/her hazard rate from 10% to about 12%. When the 2004 reform will be implemented, the same individual will divert from the *TFR* to the pension fund the annual *TFR* quota, equivalent to withdrawing less than 10% of the fund; his hazard would increase from 10% to about 10.4% in the first year. A significant but small effect, that will obviously increase as time goes by and *TFR* is "withdrawn" every year. The effect is significantly larger in firms above 25 employees (due to the fact that in smaller firms the withdrawal is usually consensual, i.e. agreed between the firm and the worker), confirming the regularity that small firms are *de iure* or *de facto* exempted from complying with several norms.

In the third chapter, I turn to firing restrictions. As in most European countries, the Italian legislation on individual layoffs varies across firms of different size. Firms with more than 15 employees are obliged to rehire the dismissed employee when a judge rules the dismissal unfair. Small firms, by contrast, are only obliged to compensate the unfairly dismissed worker with a monetary transfer. The different treatment of small and large firms is regulated by Article 18 of the Labour Code. In 2001 the Government proposed to cancel Article 18, claiming that it prevented small firms to grow above the 15 employees threshold and larger firms to manage efficiently their labour force (the reform was not implemented due to the fierce opposition of unions and workers). I focus on the first claim and I find a significant, albeit quantitatively small, threshold effect. Specifically, I estimate a set of transition matrices for employment size, which I then use to test whether employment dynamics around the 15 employees threshold shows any turbulence. While the probability of inaction decreases markedly with firm size, it

experiences a significant spike in the region below the threshold. Firms employing 14 and 15 employees have a probability of inaction 1.5% higher than what different non linear statistical models would predict. Similarly, the difference between the probability of moving down and moving up by one position falls slightly with firm size, but it features a 1.6% spike around the 15 employees threshold. I also estimate the effect of a 1990 reform which tightened EPL on small firms. Although this is not a natural experiment, because also the legislation referred to part of the control group changed, I find that the persistence of small firms relatively to large firms increased significantly after 1990. This chapter contains the most direct assessment of the relationship between firm size and binding legal provisions. The effect is significant but small.

In the fourth and final chapter, I focus on the use of temporary contracts. “On the job training contracts” are temporary contracts (the only available temporary contract during my period of analysis) that can be used to hire young workers. Their use is supported by the Government, that provides a significant rebate on social security contributions. In 1991 the rebate on temporary workers’ social security contributions decreased significantly for a subset of firms; furthermore, firms that had not hired on a permanent base at least 50% of their temporary workers during the previous 24 months were excluded from the use of this contract (no such constraint existed before 1991). Hence the cost of using this contract increased both in monetary terms and in terms of rigidity. I focus on the 1991 reform to infer the effects of a more costly flexible labour force on firms’ performance, using a difference in differences approach. I show that the burden of labour adjustment shifts on temporary workers if they are present in the firm. When this buffer became less available, labour productivity decreased in the short run and job destruction increased in the medium run at the firm level. All these effects are increasing with firm size, but are quite small. Total employment decreased by 0.3 workers among firms whose average size was about 20 workers; average yearly labour productivity decreased by 8,000 euro among firms whose average productivity was about 130,000 euro. This may provide indications on the potential effects of more recent reforms aimed at generating a less costly flexible labour force. In fact, after 1997, a flexible labour force became more available: other temporary contracts and leased work became legal, and quasi-subordinate work started to be widely used. Even though the effects do not need to be symmetric, my results may provide some indications. This is important because of the lack of formal evaluation of the post 1997 reforms from the point of view of the firm, i.e. evaluations focussing on the direct effect on labour demand.

Some general conclusions emerge from the four pieces of analysis. Italian wages are quite rigid; only about 10% of the wage is not set outside the employer-employee individual relationship. A measurable effect on firms' behaviour of *TFR*-severance payments, firing restrictions, temporary contracts is detectable; I estimate statistically significant effects in all cases. However, these effects are always small. This is coherent with the observation that there is evidence of flexibility higher than expected in such a deeply regulated market. Some hints pointing to a segmented market and to a non universal enforcement of the norms emerge as well: very low wages seem to be flexible despite the existence of national contracts; the effect of EPL on firm behaviour is always increasing in firm size.

Chapter 1

The Resurrection of the Italian Wage Curve*

Abstract

We show that the Italian wage curve, inexistent in the eighties and early nineties, has re-emerged after the 1993 income policy agreements as result of the greater role granted to flexible and locally bargained top-up wage components.

*This is a joint work with Francesco Devicienti and Agata Maida.

1 Introduction

The existence of an inverse relation between wage levels and local unemployment (the so-called "wage curve") has been confirmed empirically in many countries. In their meta-analysis, Nijkamp and Poot (2005) report an average unemployment elasticity of -0.07 based on over 1000 estimates for almost every OECD country. A decade after their seminal work, Blanchflower and Oswald (2005) still find an elasticity close to -0.10 for the US. More recently, Sanz-de-Galdeano and Turunen (2006) have estimated an elasticity of -0.14 for the euro area as a whole. At the same time, country specific wage curves have exhibited more variability than initially indicated by Blanchflower and Oswald (1994), owing to institutional differences but also to the methods and data used (e.g. Montuenga et al., 2003). To the extent that European labour markets continue to be regulated at the national level, it remains crucial to study country specific wage curves and to understand how they are shaped by national reforms.

For Italy previous estimates have cast doubts on the existence of a wage curve during the '80s and early '90s (Lucifora and Origo, 1999). The role of national bargaining, unable to fully incorporate local labour market conditions, has been stressed among the main determinants of such a result. In fact, until the early nineties, wages were set within an industry-wide, centralized wage setting process¹, accompanied by an automatic price-indexation system. In 1993 the Italian wage bargaining system went through important reforms, aimed at curbing inflation and at increasing wage flexibility and responsiveness to local conditions. The July 1993 Income Policy Agreement abolished the automatic price-indexation clause and depicted a new two-level bargaining system. At the national level, wages are now set according to the inflation rate targeted by the Government for the following 24 months. At the regional or firm level, additional wage components are introduced and are to be geared by profit sharing considerations, so changing also the nature of the additional (top-up) wage components. Therefore, one may expect the wage curve to resurrect after 1993.

The original contribution of this work to the existing literature relies on two features. First, we assess whether national institutional reforms have been effective in modifying the wage curve elasticity. We test the existence of a structural break in the Italian wage curve after the 1993 reforms, and we find that the unemployment elasticity increases in

¹To be more specific, a three-level bargaining process was in place: nation-wide, industry-wide and firm-specific; the three levels were not formally coordinated and often covered the same items (e.g. wages, working conditions, employment levels), chasing each other achievement with an egalitarian spirit and little consideration for local labour and product market conditions (Cella and Treu, 1989).

absolute value and becomes statistically significant. Second, and more important, we investigate the sources of wage flexibility in an environment where collective contracts were and still are very influential. The answer is relevant for Italy as well as for all continental Europe countries, where these features are widespread. We reconstruct the fraction of wage that is bargained at the national level and, residually, the individual top-up wage component bargained locally or individually. To the best of our knowledge this is new in the wage curve literature. We thus prove that top-up wage components display a much higher elasticity to local unemployment with respect to total wages, as expected. This is not, however, the whole story. We also learn that even top-up wage components were not responsive to local labour market conditions before the 1993 reform, and that wages are *per se* responsive to local unemployment when they are sufficiently low.

2 Data and first evidence

We have access to administrative data drawn from the archives of the Italian Institute for Social Security and processed in a public-use file known as the Worker History Italian Panel (WHIP) by LABORatorio Revelli². From WHIP we select a unbalanced panel of employees of private firms working in May of at least two years between 1985 and 1999, obtaining about 150,000 workers and 1,300,000 observations. Total individual wages are computed as real weekly wages, the ratio of total compensation and number of weeks worked over year t or over the job spell within t if shorter. They might include some variability in the number of days and hours worked; to control for this, the sample has been restricted to full-time workers and dummies for periods of sick leave, maternity leave and temporary layoff (*Cassa Integrazione Guadagni*) have been included in the specification³.

Individual top-up wage components have been reconstructed as the difference between employees' total wage and their base wage. The base wage, as stipulated by the national contract the worker belongs to, is a minimum wage specific to each occupation within the contract⁴. Hence, top-up components refer to both locally bargained wage components (second level contracts and productivity premia; they account for about half top-ups) and individual premia. Contractual wages are available and matched to WHIP for the whole 1985-1999 period and for about 30 main national contracts, covering about 70%

²WHIP is a 1:90 matched employer-employee database.

³Re-estimation of the model after excluding workers experiencing spells of sick or maternity leave or temporary layoffs did not alter the results and is not reported.

⁴It also includes cost-of-living allowances, and "third elements".

of the original sample.

Figure 1.A shows that the average share of top-up components over total wage is about 20% and pro-cyclical; it is higher in the North-Western Regions, fast growing in the North-Eastern ones, low and declining in the Southern ones. This can be easily contrasted to the unemployment rate by the same geographical areas (figure 2.A). Increasing unemployment matches decreasing average top-up components. Figures 1.B and 2.B illustrate the effect of the 1993 reform at the aggregate level. Before 1993, while unemployment dynamic was quite heterogeneous by area, top-up components moved uniformly all over the country. After the 1993 Income Policy Agreement, top-up components' dynamic diverged markedly by area, clearly mirroring local unemployment's dynamics. This confirms the change in their features introduced by the 1993 reform.

3 Econometric strategy

The economic theory behind the wage curve has been widely debated. Collective bargaining and efficiency wage models are the most commonly cited. Card (1995) and Bell et al. (2003) provide a complete discussion of their explanatory power and limits. Independently of the theoretical model, the specification of the wage curve has become quite standard. It aims at controlling as much as possible for observed as well as unobserved heterogeneity both at the individual and at the regional level. In this respect, the use of individual longitudinal data - instead of aggregate ones - allows us to control for the changing composition of the workforce over the business cycle, and hence to minimize the downward bias on wage procyclicality that affects aggregate data⁵.

Our contribution is twofold. First, we test the existence of a structural break after the 1993 reform, allowing the unemployment elasticity to be different after 1993 in our specification. We expect the wage curve to become steeper. Second, we estimate the wage curve using both total wages and top-up components as dependent variables. Our prior is that top-up components display a higher responsiveness to local unemployment than the total wage. We estimate:

$$\ln w_{ijt} = \theta_i + \theta_j + \theta_t + \sum_j \left(\gamma_j' D_j \right) t + \beta \ln u_{jt} + \beta_{break} \ln u_{jt} D_{1993} + \sum_k \beta_k x_{ijt}^{(k)} + v_{ijt} \quad (1)$$

⁵This composition bias has been emphasized in the literature on real wage cyclicity; see Solon et al. (1994).

where w_{ijt} is the wage (total wage or the top-up components) of individual i in region j and year t . u_{jt} is the local unemployment rate⁶, also interacted with a dummy variable signalling the period after 1993 (D_{1993}) to test $\beta_{break} < 0$. The θ s are individual, region and time fixed effects, allowed to be correlated with one another and with the local unemployment rate (θ_j are dummies on 20 administrative regions and θ_t on 15 years). $\sum_j (\gamma'_j D_j) t$ are region specific linear time trends, included to capture region specific wage pressure (Bell et al., 2002). $x_{ijt}^{(k)}$ includes a set of time varying controls⁷.

Equation (1) is estimated through the efficient fixed-effect transformation to remove the individual fixed effect θ_i . Estimated standard errors are robust to heteroschedasticity. Furthermore, as equation (1) contains region level variables, standard errors are corrected for clustering on region and, therefore, for lack of independence of errors within regions (Moulton, 1986).

Some studies using regional data on unemployment and wages also control for the possible endogeneity of unemployment (Baltagi and Blien, 1998). As argued also by Nijkamp and Poot (2005) and Sanz-de-Galdeano and Turunen (2006), it is unlikely that endogeneity is an important issue in our context because we use individual data on wages and our individual wage outcome is not expected to have an effect on the aggregate regional unemployment rate. Nevertheless, we have conducted the C-test of exogeneity estimating an IV-fixed effects model (Baum et al., 2003) on aggregate data at the regional level and using lagged average $x_{jt}^{(k)}$ as excluded instruments, as in Baltagi and Blien (2000). We cannot reject the null hypothesis of exogeneity of u_{jt} both for the total wage and for the top-up wage component specification⁸.

4 Empirical results

Our main results are reported in table 1. We obtain no significant elasticity before the reform, negative and significant after 1993 (the table reports $\beta + \beta_{break}$). The 1993 break is always strongly statistically significant. Our first result is therefore in favour of a structural break after the 1993 reform. Hence we are confident in stating the following remark.

⁶Regional unemployment, defined according to the standard ILO classification, has been added to the WHIP data by year and region. This is the most disaggregate unemployment series that is consistent over such a long period of time available for Italy (e.g. unemployment series at the province level were discontinued in 1992).

⁷Age (quadratic), dummies for 4 occupations, 5 firm size classes, 8 industries, as well as for spells of health or maternity leave or temporary layoffs.

⁸Total wages: $X^2 = 1.366$, p-value=.505. Top up components: $X^2 = 2.675$, p-value=.263.

Remark 1 *The Italian wage curve resurrects after the 1993 Income Policy Agreement.*

Although statistically significant, an elasticity of -0.029 (s.e. 0.009) is low. In fact it is well below Blanchflower and Oswald's -0.1⁹. However, it is comparable to the -0.025 (s.e. 0.007) elasticity estimated for the U.K. by Bell et al. (2002) with administrative data and controlling for fixed effects, as we do. To properly compare our results to Bell's, we replicate their dynamic specification, including the lagged wage and restricting the sample to males appearing in the sample every year ($T = 15$) to minimize the small T bias on the fixed effects estimator. In this case we expect a lower elasticity, as a balanced sample excludes frequent movers, entrants and less protected workers in general. In fact, the short run elasticity after 1993 decreases to -0.014 (s.e. 0.004)¹⁰; however, it is still barely statistically different from that estimated for the most deregulated labour market in Europe¹¹.

Our second and most novel result relates to the source of recovered wage flexibility. The top-up components are providing room for flexibility in the wage structure after 1993. In fact, top-up wage components display a much higher elasticity to unemployment with respect to total wages: -0.076 (s.e. 0.018) versus -0.029 (s.e. 0.009). To investigate this point further, we move to quantile regressions. Estimated $\beta + \beta_{break}$ are plotted in figure 3 against 19 percentiles; the dotted line refers to total wages, right hand side axis; the solid line refers to top-up components, left hand side axis¹². Wages of top earners (above the sixth decile) are systematically more responsive to unemployment than lower wages. This because the elasticity of their top-up components is increasingly higher (in absolute value) the higher the percentile¹³. From the quantile regressions we also learn that wages are per se responsive to local unemployment when they are sufficiently low. Low wages, in the first quartile, display larger elasticity than wages around the median,

⁹They attribute to Italy an elasticity as high as 11%, but the result is not robust to the inclusion of regional fixed effects. Excluding regional fixed effects, they measure the between region elasticity, that in Italy is structurally high (constantly over time southern regions are characterized by high unemployment and low wages; the reverse holds for northern regions). On the contrary, the within region elasticity, i.e. the short run responsiveness of wages to local unemployment changes, is computed when regional fixed effects are included. This is the kind of responsiveness we are interested in.

¹⁰Unemployment elasticity in the long run is -0.053 in the U.K. and -0.033 (s.e. 0.008) in Italy.

¹¹The other existing estimate for Italy after 1993 is in Montuenga et al. (2003). Their much higher elasticity is not directly comparable to ours, as they refer to survey data for the years 1994-96 and to 11 macro-regions only.

¹²Equation (1). Percentiles: 5,10 ...95. Total wage elasticity always significant at 95% confidence level. Top-up wage components elasticity significant at 95% confidence level from percentile 25 on.

¹³Correlation coefficient between percentiles of the wage distribution and percentiles of the top-up component distribution is as high as 94%, allowing us to safely compare the two series of quantile regressions.

even if the elasticity of their top-up components is not significantly different from zero. It might be the case that compliance to the national contract is not strict among these workers.

Remark 2 *The top-up components are providing room for flexibility in the wage structure after 1993. But wages are per se responsive to local unemployment when they are very low.*

The 1993 break is found in many different groups of workers, and no particular group drives the result (Table 1). However, as expected, elasticity is higher in industries facing international competition, and for women. Higher skill level entails lower wage elasticity, as implied in the literature; however, top-up components of white collars are more sensitive to local labour market conditions compared to blue collars' top-up components. This is an interesting pattern that can be uncovered only disentangling top-up components and it is consistent with our quantile analysis (white collars earn higher wages and larger top-up components). Results by firm size and by geographical area confirm the pattern emerging from the quantile regression: top-up components are (very) responsive to local unemployment only when they are large (in large firms and in the northern regions); otherwise (low) total wages are themselves sensitive to local labour market conditions.

The elasticity of top-up components is never significant before 1993, confirming the change in their features induced by the reform.

Table 1: Unemployment Elasticity.

		Before 1993			After 1993		
		β	s.e.		$\beta + \beta_{\text{break}}$	s.e.	
1	All sample						
	Total wage	-0.005	0.006		-0.029	0.009	***
	Top up components	-0.002	0.018		-0.076	0.018	***
2	Males						
	Total wage	-0.008	0.007		-0.029	0.008	***
	Top up components	-0.007	0.019		-0.073	0.020	***
3	Females						
	Total wage	-0.002	0.007		-0.041	0.015	**
	Top up components	-0.001	0.032		-0.101	0.052	*
4	International competition: yes (+)						
	Total wage	-0.005	0.009		-0.040	0.013	***
	Top up components	0.003	0.035		-0.107	0.035	***
5	International competition: no (++)						
	Total wage	-0.002	0.005		-0.016	0.006	**
	Top up components	-0.024	0.020		-0.042	0.021	*
6	Blue collars						
	Total wage	-0.005	0.007		-0.031	0.009	***
	Top up components	-0.005	0.032		-0.076	0.028	**
7	White collars						
	Total wage	-0.010	0.003	***	-0.024	0.004	***
	Top up components	-0.014	0.015		-0.085	0.029	***
8	North and center						
	Total wage	-0.011	0.004	**	-0.033	0.006	***
	Top up components	-0.012	0.021		-0.090	0.023	***
9	South						
	Total wage	-0.015	0.011		-0.035	0.012	**
	Top up components	0.008	0.031		-0.063	0.036	
10	Large firms (-)						
	Total wage	-0.005	0.007		-0.025	0.008	***
	Top up components	-0.050	0.031		-0.162	0.031	***
11	Small and medium size firms (-)						
	Total wage	-0.004	0.006		-0.031	0.011	**
	Top up components	0.014	0.023		-0.034	0.021	
Regional unemployment, ILO definition. Top up components if available and positive.							
Within Group estimator, cluster(region).							
*, ** and *** significant at the 90pct, 95pct and 99pct level, respectively.							
(+) Sectors more exposed to international competition: ATECO81 branches 2-3-4 (manufacturing)							
(++) Sectors less exposed to international competition. ATECO81 branches 1-5-6-7-8 (utilities, constructions, services)							
(-) above 200 employees.							
(-) below 200 employees.							
Coefficients of controls not reported							

5 Conclusions

Our results have shown the existence of a significant structural break after 1993, which made wages more responsive to local unemployment. The top-up components of wages (which are bargained locally, through unions or individually) display a -0.076 elasticity to unemployment, compared to -0.029 of total wages. At the same time, the fact that top-up components only take up 20% of total wages explains why the wage curve - which managed to resurrect after the 1993 reforms - still looks a bit anaemic.

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Figures

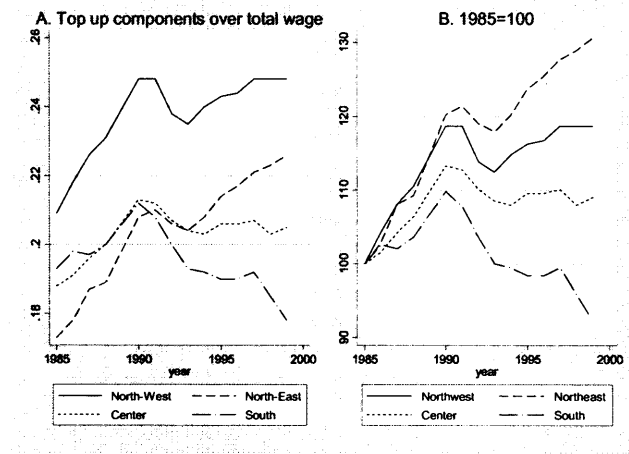


Figure 1: Top up components by area

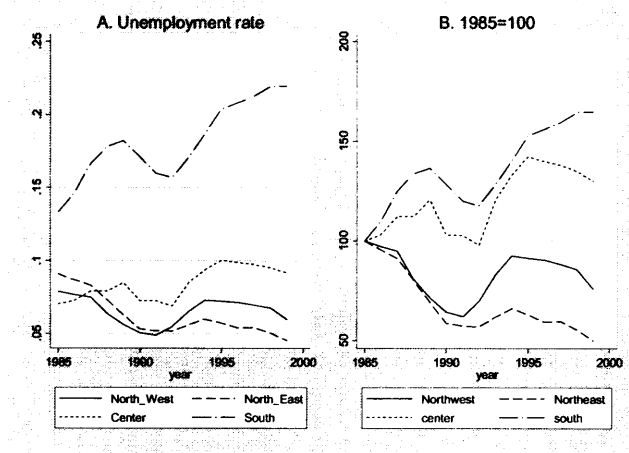


Figure 2: Unemployment rate by area

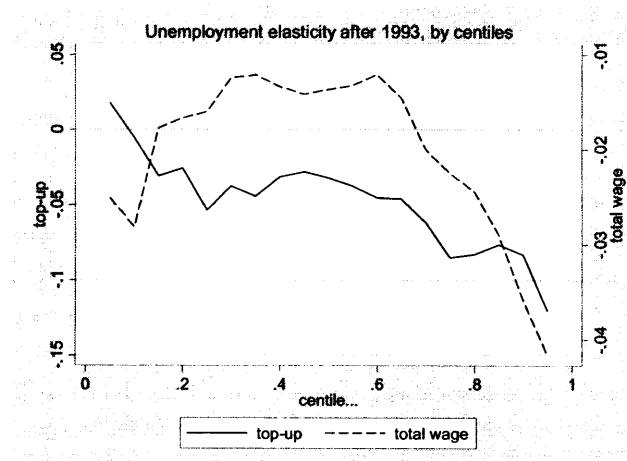


Figure 3: Quantile regressions

Chapter 2

Do Larger Severance Payments Increase Individual Job Duration? *

Abstract

This work analyzes the effect of severance payments on the probability of separation at given tenure, wages and other individual and firm characteristics. It studies a mandatory deferred wage scheme of the Italian labour market (*Trattamento di Fine Rapporto*, TFR). Deferred wages increase job duration if two conditions hold: wages are rigidly set outside the employer-employee relationship, and past provisions are accumulated at interest rates that are below market rates. Under such circumstances, workers who withdraw from their accumulated stock of unpaid wages should experience, at given tenure, a subsequent increase in the probability of separation. This prediction appears empirically robust and quantitatively sizeable. A withdrawal of 60% of the TFR stock (the median observed withdrawal) increases the instantaneous hazard rate by almost 20%. In other words, an individual with at least ten years of tenure that experiences an early withdrawal increases his/her hazard rate from 10% to about 12%. The empirical result takes into account the existence of unobserved heterogeneity and a variety of further robustness tests.

*This is a joint work with Pietro Garibaldi.

1 Introduction

More stringent Employment Protection Legislation (EPL) should induce labour hoarding from the firm stand-point: other things equal, employer initiated separations should be lower for individuals with stricter EPL¹. While such theoretical prediction is unambiguous, little is known on the empirical links between EPL and separation rates at the job level². Indeed, two empirical regularities prevent straightforward identification of the theoretical prediction. On the one hand, it is well established that the probability of job termination declines markedly with tenure (Farber 1999), independently of the presence of severance payments. On the other hand, severance payments increase with job tenure. As a result, it is very difficult to identify the effect of severance payments on labour hoarding and employer initiated separation. This work exploits an institutional feature of the Italian labour market that makes it possible to identify this effect at the micro level.

Traditional severance payments, defined as statutory firm worker transfers in case of firm initiated separation, do not exist in Italy (Brandolini and Torrini, 2002). Nevertheless, Italy features a mandatory deferred wage (*Trattamento Fine Rapporto*, *TFR* hereafter) that can be akin to a severance payment. It is paid to the worker at the end of the employment relationship, regardless of the reasons behind the separation. During the entire working relationship, the worker accumulates a credit vis-à-vis the firm, which is in turn obliged to recapitalize its debt to the worker at policy determined interest rates. Over the last twenty years, such policy determined interest rates have been traditionally very low, so that *TFR* resulted in subsidized financing of firm operation from the part of workers. We solve a simple partial equilibrium model of job destruction with deferred wages, and show that deferred wages are akin to a severance payment. As long as wages are rigidly set outside the employer-employee relationship, and deferred wages are accumulated below market rates (both conditions hold in Italy during our observation period), a scheme like *TFR* increases the firm propensity to hoard labour. As a way to keep the subsidized financing on the part of workers, firms have incentives to delay job separation and to increase the average duration of jobs. The latter result suggests that *TFR* has the same effects of a severance payment with fixed wages, and appears equivalent to a piece of employment protection legislation.

Within the existing institutional setting workers are allowed to proceed to an advance

¹See for example, Nickell, 1986; Bertola, 1990 and 1999; Hopenhayn-Rogerson, 1993; Ljungqvist, 2001.

²A possible example is Kugler (1999), who studies the effect of changes in firing costs in Colombia.

withdrawal of their accumulated credit. Our theoretical model clearly predicts that following a random shock, workers who withdraw from their accumulated credit of unpaid wages increase the probability of a firm initiated separation. This suggests that we can empirically identify the effect of severance payments on the probability of separation, at given tenure, wages and other individual and firm characteristics. Our empirical analysis suggests that such effect is both sizeable and robust.

We rely on the Work Histories Italian Panel (WHIP), a longitudinal micro dataset drawn from the social security administration (INPS) archives and processed in a public-use file by LABORatorio Revelli³. Using information on about 20,000 employment spells initiated between 1985 and 1988 and followed up to 1999, we test the impact of advance withdrawals on the hazard rate, i.e. on the probability of firm initiated separation between t and $t + 1$ conditional on having a tenure of at least t , using a variety of survival models. Overall, we find that withdrawing from the *TFR* fund significantly increases the hazard rate, even when we control for wages, industry and occupational effects. Our results are also quantitatively non negligible. A withdrawal of 60% of the *TFR* stock (the median observed withdrawal) increases the instantaneous hazard rate by almost 20%. In other words, an individual with at least ten years of tenure that experiences an advance withdrawal increases his/her hazard rate from 10% to about 12%. We perform a variety of robustness checks, and we find our results robust.

Our results are also relevant in the policy debate. The social security reform approved by the Italian Parliament in 2004 envisages a two pillar system. The first pillar will be a national pension. The second pillar will be made by private pension schemes. The law considers the *TFR* as a base payment to fill up the private pension funds. Our results suggest that a shift of *TFR* funds into pension funds will increase labour turnover, so that implementing such reform will have a direct effect in the social security area, and an indirect effect on the labour market.

The chapter proceeds as follows. Section 2 presents the Italian institutional setting, with particular emphasis on the existence of deferred wages. Section 3 presents a model of stochastic and dynamic job destruction with deferred wages, while section 4 characterizes a simple version of the model, and derives a key empirical implication. Section 5 presents the data and sample design. Section 6 describes our empirical methodology and econometric issues. Section 7 presents the empirical results while section 8 concludes.

³www.laboratoriorevelli.it/whip

2 Deferred Wages in the Italian Institutional Setting

In the literature on employment protection legislation, statutory severance payments are defined as mandatory payments (monetary transfers) to which a worker is entitled in case he or she is dismissed without fault of his or her own. In practice, beyond statutory payments, collective bargaining can and do set additional severance payments to which workers are entitled in case of redundancy.

As was pointed out by Brandolini and Torrini (2002), Italy does not feature any mandatory severance payment⁴. In the Italian institutional setting, employment restrictions take the form of an in-kind protection that forces firms to rehire unlawfully dismissed workers (reinstatement clause). Individual dismissal for economic reasons is contemplated by the law and it can be carried out at no cost. Yet, dismissed individuals have the right to appeal employer initiated separation. Ultimately, a court ruling decides whether a specific dismissal is admissible. Previous research has focused on such restriction. Ichino et al. (2003) argue that judges are biased by labour market conditions in their court ruling. In chapter 3 we study the effect of a size threshold in the reinstatement clause, and find that there is some evidence of an increase in employment persistence around the threshold, but the size of the effect is quantitatively small.

This work studies a deferred wage scheme, called *Trattamento di Fine Rapporto* (Remuneration upon Separation, *TFR* in the rest of the work). *TFR* is an amount of money to which the worker is entitled at the end of the employment relationship, regardless of the cause beyond the job separation (quit, layoff, retirement, even firing for cause). The *TFR* is technically defined as a fraction of the yearly wage that is paid to all employees (including those under probation) with a time delay. It is a sum of yearly provisions that are explicitly included in the firm's balance sheet (and so they are part of the labor costs for accounting purposes) and it is periodically re-valuated. For each year of service, a provision of two twenty-seventh (or $1/13.5$) of the yearly gross salary is included in the individual *TFR* account, and is re-valued yearly according to the following coefficients: 1.5% fixed plus 75% of the CPI inflation of the previous year. In legal terms, the *TFR* is a credit from the employee to the firm, and it is guaranteed by the social security administration in case the firm goes bankrupt.

Under normal circumstances, the *TFR* is paid to the worker at the time of job

⁴While there are no statutory severance payments, collective agreements do set severance payments when the parties bargain over layoffs. No official survey is available on the size of these transfers or on the frequency of these events. In this work we focus on a different, universal, institution that is akin to a severance payment and whose effects may be added to bargained severance payments when they exist.

separation. Nevertheless, workers have the right to withdraw up to 70% of the *TFR* in advance if several conditions hold. First and foremost, they must have at least 8 years of tenure; second, they have to use the advance payment only for health related expenses, for buying a house, or for specific periods of unpaid leave (e.g. training). Finally, the advance withdrawal is a right on the part of the worker as long as less than 10% of the eligible workers in the firm apply, and as long as it corresponds to less than 4% of the total workforce; this implies that firms below 25 employees may be exempted from this obligation⁵. Failure to meet these conditions requires that the advance withdrawal is approved by the employer. In fact, workers do withdraw their individual accounts through employer approval. We observe about 2% of employees withdrawing before the 8th year of tenure, while this share increases to 5-6% afterward.

From the firm standpoint, the policy determined interest rate implies a subsidized financing of firm operation. Between 1988 and 1999, the years on which we will base our empirical analysis, the best rate available in the banking system was approximately ten percentage points higher than the financial cost of the *TFR*. This is clearly visible in Table 1, where we report the implicit ex-post interest rate of the *TFR* as well as the prime rate available in the banking system. From the worker standpoint, the outside riskless option is a financial investment in long term government bonds. As shown in Table 1, between 1988 and 1999 the average long term rate on government bonds is some 8 percent higher than the rate of the *TFR*. This suggests that workers face a financial opportunity cost induced by the institutional setting.

3 Job Destruction with Deferred Wages

This section develops a simple model of deferred wages. It highlights the links between job duration and an institution like *TFR*. In particular, it helps understanding why we can identify the effect of severance payments at given tenure, wages and other characteristics. We make several simplifying assumptions, whose bearings on the empirical analysis will be discussed in section 6.

⁵The legislation says that early withdrawal is a worker's right as long as it is requested by "less than 10 percent that does not exceed 4% of the workforce". Indeed, it is not entirely clear whether early withdrawal after 8 years of tenure is a workers' right in firms with less than 25 employees. On the one hand, there is a high court ruling that interprets the 4% threshold literally, and argues that firms with less than 25 employees are not obliged to obey to the early withdrawal legislation. On the other hand, various judges argue that the early withdrawal request apply as soon as "the cumulative yearly value of eligible workers does not reach 1" This implies that a firm of 5 employees should agree to one advance withdrawal every 5 years.

Table 1: Prime Rates and Policy Determined Interest Rate for Recapitalizing TFR stocks

year	Policy \tilde{r}^a	Market r^b	T-Bill r_{tb}^c
1988	5.596	13.00	10.6
1989	6.387	14.00	10.9
1990	6.28	13.00	12.8
1991	6.032	12.50	13.5
1992	5.068	16.25	13.3
1993	4.491	10.38	11.2
1994	4.542	9.38	10.4
1995	5.851	11.50	12.2
1996	3.422	10.75	9.4
1997	2.643	9	6.9
1998	2.626	7.875	4.9
^a Policy Determined Yearly Capitalization rate for <i>TFR</i>			
^b Market Determined Annual Prime Rate			
^c Long Term Annual Rate on Government Bonds			
Source: Authors' calculation, and Datastream.			

We consider a firm-worker pair that is engaged in a multi-period relationship that lasts T periods and produces a value of p_t at time t . In each period p_t is drawn from a continuous distribution $F(x) = \text{prob}(p \leq x)$ with finite upper support p^{\max} . Firms and workers are risk neutral and discount the future at the constant interest rate r . We do not model job creation, and we normalize the outside option of the firm and the worker to zero. The key decision we are modeling is job destruction conditional on the state of demand p_t , and we give full authority to the firm in such dimension. There are two possible ways in which a relationship ends. First, the firm decides to stop production at time t . Second, the job survives up to time T . The most natural interpretation of T is that of the retirement age, but it is also consistent with a temporary job. In this parsimonious setup the worker is fairly passive, no quits are considered. We assume that the per period wage is exogenously fixed at w throughout the employment relationship; however, we discuss how the results would change if wages were endogenous and flexible (Appendix 9.1).

Our focus is on the deferred wage. We assume that in each period a fraction δ of the wage is postponed and paid at the end of the employment relationship, regardless of the reasons behind the job termination. Firms are obliged to accumulate the unpaid wages at rate \tilde{r} where \tilde{r} is exogenously set. For most of our purposes, we consider the case in which $r > \tilde{r}$, even though the model is perfectly viable for a different assumption. This is fully consistent with the data reported in Table 1.

3.1 Value Functions

In what follows we indicate with Π_t the expected present discounted value of a job at time t and with R_t the value of accumulated unpaid past wages. The timing of the decision is as follows. At the beginning of each period the firm decides whether continuing production is optimal. The firm decision to continue production is based on the realization of the productivity shock. If production takes place, part of the wages are paid and the relationship moves to the next period. If production is interrupted, the entire stock of TFR is paid at the end of the period. For example, the sequence of events for a job that lasts 2 periods is as follows. In the first period, production takes place, and the relationship moves to the second period where job destruction can take place. If it does, R_2 is paid to the worker; if it does not, production takes place again and the job terminates when it reaches its natural end at $T = 2$. In this case R_3 , the deferred wage, is paid to the worker.

We focus on $(\Pi_t|p_t)$ which is the present discounted value of a firm who has *just* decided to continue production at time t , and its expression reads

$$(\Pi_t|p_t) = \begin{cases} p_t - (1 - \delta)w_t + \frac{1}{1+r} \left\{ \int \text{Max}[\Pi_{t+1}(z); -R_{t+1}] dF(z) \right\} & t = 1 \dots (T - 1) \\ p_t - (1 - \delta)w_t - \frac{1}{1+r} R_{t+1} & t = T \end{cases} \quad (1)$$

where the firm maximization decision over the job continuation is done in every period but the very last one. In equation (1), the per period operational profits are $p_t - (1 - \delta)w_t$ while the continuation value depends on whether job destruction is optimal. Note that if the firm does destroy the job TFR is still to be paid. Note further that the deferred wage R_{t+1} is paid to the worker regardless of the reason behind the job termination, so that the payment R_{t+1} is not a traditional severance payment. The value of R_t evolves according to rule

$$R_{t+1} = (1 + \tilde{r})[R_t + \delta w_t] \quad t = 1 \dots T,$$

with $R_1 = 0$.

Since the function Π_t is monotonic in p_t , the firm continuation decision is described by a reservation strategy p_t^* such that

$$(\Pi_t|p_t^*) = -R_t \quad (2)$$

whose value will be determined below. While the problem is dynamic and non-stationary,

since it depends on the actual tenure of the worker, it can be easily solved by backward induction.

Definition 1 *The solution to the firm problem specified by equation 1 is a sequence of reservation productivity $\{p_t^*\}_{t=1}^T$ that solve equation 2.*

Note that at time $t = 1$, in light of the definition of deferred wages, the reservation productivity is akin to a job creation condition, and specifies that a firm is willing to open a job as long as its present discounted value is positive, or at least as large as the outside option of zero.

We also specify the worker's value function, even though his role is fairly passive. This is nevertheless useful for clearly specifying our assumptions. The present discounted value at time t of a job to a worker that is currently employed is

$$W_t = \begin{cases} (1 - \delta)w_t + \frac{1}{1+r} \{ [1 - F(p_{t+1}^*)] W_{t+1} + F(p_{t+1}^*) R_{t+1} \} & t = 1 \dots (T - 1) \\ (1 - \delta)w_t + \frac{1}{1+r} R_{t+1} & t = T \end{cases}$$

where it is clear that the worker takes as given the firm decision over the continuation policy, which is described by the probability of separation $F(p_t^*)$.

4 Characterizing The Model

4.1 One Period Model

To establish some very basic results we can start from a 1 period model ($T = 1$), where the only decision is a static reservation productivity, which is akin to a job creation decision. The firm profit can be written as

$$\Pi|p = p - (1 - \delta)w - \frac{\delta w(1 + \tilde{r})}{1 + r}$$

Note that if wages are fully flexible and proportional to the realization of productivity, so that $w = \gamma p$ with $\gamma < 1$, firm profits are strictly proportional to productivity and can be written as

$$\begin{aligned} \Pi|p &= p\Delta; & w &= \gamma p \\ \Delta &= (1 - \gamma) + \delta\gamma \left[1 - \frac{1 + \tilde{r}}{1 + r} \right] > 0 \text{ if } \tilde{r} < r \end{aligned}$$

In the one period model with flexible wages the reservation job solves $(\Pi|p^*) = 0$, i.e. $p^* = 0$, and the firm job creation decision is $p > 0$ independent of deferred wages. This implies that mandatory deferred wages have no allocative impact. This feature should not be surprising, since it is just an application of the Lazear (1990) neutrality results: any mandatory transfer from the worker to the firm can be neutralized by wage flexibility, i.e. deferred wages are irrelevant.⁶

Remark 2 *With flexible wages, deferred wages are irrelevant*

To make the problem interesting, wages need not be strictly proportional to productivity. An extreme assumption is assuming that wages are fixed and determined outside the relationship. As chapter 1 discusses, in the case of Italy such assumption turns out to be not so extreme. With constant wages, the reservation productivity reads

$$p^* - w = -\delta w \left[\frac{r - \tilde{r}}{1 + r} \right] \quad (3)$$

Equation 3 highlights an important result. When wages are fixed and $\tilde{r} < r$, the firm has an incentive to run a job with a marginal productivity that is lower than the wage. This mechanism represents the basic channel for the labour hoarding effect of deferred wages, a result that can be much more appreciated with a proper dynamic setting.

4.2 Dynamic Setting

In this section we analytically solve the model with $T = 2$, so that the solution of the model is described by the productivity p_1^* and p_2^* . It turns out that most of the properties that we want to emphasize apply to a two periods model.

We solve the model backward and obtain, sequentially, the reservation productivity at time $T = 2$ and $T = 1$. The equations for the two productivities solve $(\Pi_2|p_2^*) = -R_2$ where R_2 is the stock of TFR at the beginning of period 2 and $(\Pi_1|p_1^*) = -R_1$, where R_1 is the stock of TFR at the beginning of the relationship at $T = 1$. Their respective value solves

⁶The neutrality result can also be seen in terms of total surplus of the job. If one writes the worker value function in the one period model one has

$$W|p = (1 - \delta)w + \frac{\delta w(1 + \tilde{r})}{1 + r}$$

and the total surplus from the job $S = W + \Pi = p$ which is positive as long as $p > 0$

$$p_2^* - w = -\Gamma_2(r, \tilde{r}, \delta w) \quad (4)$$

$$p_1^* - w = -\int_{p_2^*}^{p^{\max}} (z - w) dF(z) - \Gamma_1(r, \tilde{r}, \delta w, p_2^*) \quad (5)$$

where

$$\Gamma_2(\cdot) = \delta w \left[\frac{r - \tilde{r}}{1 + r} \right] [1 + (1 + \tilde{r})] > 0$$

$$\Gamma_1(\cdot) = \delta w \left[F(p_2^*) \frac{r - \tilde{r}}{1 + r} + (1 - F(p_2^*)) \frac{r^2 + 3(r - \tilde{r}) - \tilde{r}^2}{(1 + r)^2} \right] > 0$$

The sign of the two Γ functions is positive as long as $r > \tilde{r}$, an assumption that we maintain throughout the work. The structure of the model, which is based on the assumption that the firm has an outside opportunity of 0, implies that the firm has an option value associated to hiring labour, even when deferred wages do not exist. To see this, one can solve the model when $\delta = 0$, and obtain

$$\begin{aligned} p_{2(\delta=0)}^* - w &= 0 & (\delta = 0) \\ p_{1(\delta=0)}^* - w &= -\int_{p_{2(\delta=0)}^*}^{p^{\max}} (z - w) dF(z) & (\delta = 0) \end{aligned}$$

where it is clear that $p_1^*(\delta = 0) < w$ so that at time $t = 1$ the firm is willing to run a current loss in exchange of future profit gains. In the very last period, conversely, the problem is static and the firm hires only if labour productivity is as large as the wage, exactly as in a static textbook model of labor demand. As the next remark shows, we say that the firm hoards labour

Remark 3 *With an outside option of zero and constant wages, the firm hoards labour in every period but the very last one*

Over and beyond the fixed wage assumption, the firm propensity to hoard labour depends also on the structure of the productivity shock. In our current setting shocks are i.i.d. and the distribution faced by the firm is time invariant. If shocks were persistent and autoregressive, the firm propensity to hoard would fall, since the probability of a fast and large turnaround would fall. For analytical simplicity, we work only with i.i.d. shocks.

One of the main questions of this work is whether deferred wages increase the firm

propensity to hoard labour. To see this one needs to study the marginal impact of the differed share δ on the reservation productivity. After simple algebra, the result reads

$$\frac{\partial p_2^*}{\partial \delta} = -w \left[\frac{r - \tilde{r}}{1 + r} \right] [1 + (1 + \tilde{r})] < 0. \quad (6)$$

$$\frac{\partial p_1^*}{\partial \delta} = -(p_2^* - w)F(p_2^*) \frac{\partial p_2^*}{\partial \delta} - \frac{\Gamma_1(\cdot)}{\delta} + \delta w f(p_2^*) \left(\frac{(2 + \tilde{r})(r - \tilde{r})}{(1 + r)^2} \right) \frac{\partial p_2^*}{\partial \delta} < 0 \quad (7)$$

where the sign of the latter equation depends on the fact that $(p_2^* - w) < 0$. Equations (6) and (7) are key equations, and they show that the existence of *TFR* increases the firm propensity to hoard labour. Note also that the result requires not only $\delta > 0$, but also $r > \tilde{r}$. We can now state one of our key results.

Remark 4 *TFR increases the firm propensity to hoard labour. If $\tilde{r} < r$ and $\delta > 0$, the reservation productivity at time t falls with the size of *TFR*.*

An intuition of this result is as follows. *TFR* creates on the part of the firm an incentive to delay the time of separation, since the longer the average tenure, the lower the average labour cost. Thus, following a negative temporary shocks, the firm optimally holds on to current losses just to increase tenure and postpone the payment of the *TFR*.

While the presence of *TFR* unambiguously reduces the reservation productivity at time t , the dynamic evolution of the reservation productivity for given *TFR* is more complicated, since there are two labour hoarding effects that influence the value of p_t^* and p_{t+1}^* . On the one hand, the larger is t the larger is the accumulated stock of *TFR*, and the larger the firm incentive to hold on to the worker and reduce the reservation productivity. On the other hand, the larger is t the lower is the future value of the firm rent. The net effect of these two forces is thus ambiguous, and one can not establish ex-ante the dynamic evolution of the productivity.

One can nevertheless establish that for a given forward looking time span, the labour hoarding effect of *TFR* increases with tenure. To see this we just consider two workers in the very last period of the relationship ($T = 2$ in the context of our model) but with different elapsed tenure. Equation 4 describes a firm employing a worker with elapsed tenure $\tau = 1$. Suppose the firm employs also a worker with elapsed tenure $\tau = 2$. Simple algebra shows that in this case⁷ $p_2^* - w = -[\Gamma_2(r, \tilde{r}, \delta w) + \hat{\Gamma}_2(r, \tilde{r}, \delta w)]$, where $\hat{\Gamma}_2 = \delta w \frac{r - \tilde{r}}{1 + r} (1 + \tilde{r})^2 > 0$ if $\tilde{r} < r$ and $\delta > 0$, i.e. the wedge between productivity

⁷The reservation rule becomes $-\delta w(1 + \tilde{r}) - \delta w(1 + \tilde{r})^2 = \hat{p} - (1 - \delta)w - \frac{1}{1 + r}[\delta w(1 + \tilde{r}) + \delta w(1 + \tilde{r})^2 + \delta w(1 + \tilde{r})^3]$

and wages increases with elapsed tenure. It is straightforward to generalize the result for a worker with elapsed tenure $\tau = K$. In this case $\bar{p}_2 - w = -\bar{\Gamma}(r, \tilde{r}, \delta w, K)$ where $\bar{\Gamma}(r, \tilde{r}, \delta w, K) = \delta w \frac{r - \tilde{r}}{1 + r} \sum_{\tau=1}^{K+1} (1 + \tilde{r})^{\tau-1} > 0$ if $\tilde{r} < r$ and $\delta > 0$, increasing in K . As an example, $\bar{\Gamma}(r, \tilde{r}, \delta w, K)$ relative to w can be computed setting $\delta = 0.074$ (as stated by law), $r = 0.12$, $\tilde{r} = 0.05$, based on average values from table 1. $\bar{\Gamma}(r, \tilde{r}, \delta w, K)$ increases from 1% of w for $K = 1$ to 3.1% of w for $K = 5$ to 6.6% of w for $K = 10$; all but a negligible size.

Having characterized the firm reservation productivity, we can turn to the effects of *TFR* on firm profits. To study such effects, we can differentiate total expected profits at the beginning of the employment relationship with respect to δ . We can prove the following statement (see proof in Appendix 9.2):

Remark 5 *As long as $\delta > 0$ and $\tilde{r} < r$ TFR increases firms' present discounted profits*

The previous remark suggests that *TFR*, through its effect on profits, is likely to have an effect on job creation. Such effect would arise in an equilibrium model of the labour market, which is not the scope of the current research. Models of this type have been extensively solved in the literature. See notably for a survey Bertola (1999), Ljungqvist (2001) for the effects of EPL in a variety of models and Garibaldi and Violante (2005) for a paper that distinguishes between various forms of EPL.

Finally, we turn to workers' behaviour, and we explicit the key assumption on workers' behaviour from our theoretical perspective:

We assume that for a given wage, workers enjoy the job security provisions determined by the TFR.

Technically, this assumption is linked to the effect of a larger *TFR* on workers' welfare. As we show in more details in the appendix 9.3, the impact of larger *TFR* on workers welfare is made of two components, which have opposite effects, and we label them "income effect" and "labour hoarding effect". The *income effect* of TFR decreases workers' welfare. A larger *TFR* (i.e. an increase in the share δ of wages that are paid at the end of the relationship) induces a fall in workers' utility, since the worker is financing the firm at the interest rate \tilde{r} and reduces the present discounted value of its wage stream. The *labour hoarding effect of TFR* increases workers' welfare, since it grants more stability to the worker. Our assumption implies that for a given wage and firm profit maximization behaviour, workers are better off with *TFR*. This is consistent with the evidence that advance withdrawals are a rare event.

4.3 The Effects of Advance Withdrawals

In this section, as a way to obtain a key empirical implication, we look into another institutional dimension of the *TFR* legislation, namely the possibility that *TFR* is paid in advance to the worker. Our model assumes that, for given wages, workers enjoy the job security determined by the *TFR*. This assumption implies that in the baseline model the individual is better off with *TFR*, and if s/he had the option to withdraw the accumulated stock of *TFR* s/he would not exploit such option. Hence, since *TFR* is welfare improving at the given wage, an advance withdrawal has to be the result of an exogenous and random shock, that takes place at an exogenous rate μ . The interpretation we give to the shock μ is an i.i.d. liquidity shock at the individual level.

The advance withdrawal has an impact on firm behaviour. The existence of the withdrawing shock modifies the sequence of events within the model. At the beginning of the period a withdrawing shock is realized. Then the firm observes the realization of the productivity shock. If the worker withdrew from its stock of *TFR* (at rate μ), the firm continues operation as long as $p_2 > \tilde{p}_2^*$. In case there is a firm initiated separation (i.e. p is below the threshold \tilde{p}_2^*), no *TFR* is due⁸. If the worker did not withdraw from its stock of *TFR* (at rate $1 - \mu$), the firm continues operation as long as $p > p_2^*$. In case there is a firm initiated separation (i.e. p is below the threshold p^*), the full *TFR* is due.

Clearly, the value of the continuation profits depends on whether an advance withdrawal shock hits the relationship. As the analysis in the appendix 9.4 makes clear, the value function for $\Pi_t(p_t)$ specifies a different reservation productivity conditional on the fact that a withdrawal will or will not take place. Characterizing the two reservation productivities p_2^* and \tilde{p}_2^* in the context of our two periods model, it is immediate to see that:

$$\tilde{p}_2^* > p_2^*$$

from which it follows that when a worker withdraws its *TFR*, the firm reduces its incentive to hold to marginal losses. The following empirical implication follows.

- **EMPIRICAL IMPLICATION.** *Other things equal, workers who withdraw their TFR, have a larger probability of experiencing a firm initiated separation.*

The intuition in this respect is straightforward. Conditional on an advance withdrawal, the firm incentive to hold on to marginal losses disappears, and the labour

⁸For the sake of simplicity, we impose that 100% of the stock of *TFR* is withdrawn. The law imposes a maximum withdrawal of 70%; in the actual data the median share is about 60%.

hoarding dimension of *TFR* is no longer relevant. As a consequence, for given tenure, the firm will prefer to hold on to those individuals that did not experience an advance withdrawal.

The empirical implication highlighted above is the key prediction that we take to the data in the rest of the work. Before introducing the empirical analysis we present the dataset available to us, and some descriptive statistics

5 Data and descriptive statistics

We have access to a single-spell flow-sample of Italian employment spells. The data source is the Work Histories Italian Panel (WHIP) originated from the Italian Social Security Administration (INPS) archives and processed in a public-use file by Laboratorio Revelli. We have a 1:90 random sample of the entire archive of employees of private firms for the period 1985 to 1999. This is a longitudinal archive, as the same worker can be followed over time through possibly different employers. From such sample we select all employees that start a new job between February 1985 and December 1988⁹ and we follow those particular employment spells till they end or until December 1999. Ongoing spells at December 1999 are right censored¹⁰. Notice that, in principle we can observe more than one employment spell for the same worker. However, as we need "long" spells to observe advance withdrawals (8 years of tenure is the legal minimum) and as the observation period lasts between 11 and 14 years this possibility is actually unavailable. Finally, we select spells that last a minimum of four years, to be able to condition on lagged covariates (about 20,000 spells). This unavoidable selection excludes from the analysis very short employment spells, scarcely relevant for the purpose of the work¹¹.

We observe the workers once a year, even though we know the exact month in which the hire and the (possible) separation took place. Starting time and censoring are clearly exogenous.

An important empirical remark concerns the cause of firm-worker separation. There are three possible causes of separation: natural turnover (reaching the retirement age T^{12}), worker initiated separation (quits), firm initiated separation. We are interested in

⁹For those working in January 1985 we cannot distinguish between new hirings and left censored ongoing employment spells.

¹⁰For those working in December 1999 we cannot distinguish between separations occurring in December and right censored ongoing employment spells. This generates an - unavoidable - underestimation of the separation rate in 1999.

¹¹The point is discussed further in section 6.3 on reverse causality.

¹²There are no temporary contracts in our sample. In the years 1985-1989 temporary contracts could

firm initiated separations only. However, our dataset reports separations but not their cause. Empirically, we distinguish between different causes of separation in the following way. First, we are very conservative vis-à-vis the quit for retirement, and we define "retirements" all separations that occur when the worker is 55 or over. More subtle and problematic is how to disentangle quits and layoffs. There is a well known grey area between worker initiated and firm initiated separations, both from a theoretical and from an empirical point of view. We address the issue using the observed duration of the subsequent non-employment spell; the idea being that on average a quit is "more likely" to lead to a new job "sooner" than a layoff¹³. Hence, we label "firm initiated separation" those separations followed by a spell of non-employment of at least two months. Of course what we obtain is to increase the share of firm initiated separations, not to exclude quits altogether. In Appendix 9.6 we discuss the robustness of the econometric results to this definition of firm initiated separation.

We now turn to some descriptive statistics on *TFR* and advance withdrawals. The *WHIP* archive records the *TFR* stock at December of year t . It is then possible to compute the rate of accumulation of *TFR* with respect to the total annual gross wage. Figure 1 shows its distribution. There is some variability around the $1/13.5=0.074$ coefficient stated by the law. This is likely to reflect a number of unobservable events that may be the outcome of union - firms agreements with respect to the "relevant part of the wage" mentioned by the law on which the *TFR* yearly increase is computed. However, there is also some variability that cannot easily be explained (e.g. small positive and negative values, clearly visible in Figure 1), and we label it "measurement error". The existence of measurement error imposes a more precise definition of "withdrawal", since "negative changes in *TFR* stock" would overestimate the event of interest. In the rest of the work, we define a withdrawal as a negative change in the stock of *TFR* between $t - 1$ and t that (i) does not occur in the separation year (it would be the compulsory payment, not a withdrawal); (ii) involves at least 20% of the existing *TFR* stock; (iii) amounts at least at 500 euro in real terms. Sensitivity analysis confirms that the - necessarily arbitrary - choice of the above mentioned thresholds is non influential on the econometric estimates.

As we mention in Section 2, withdrawing from the stock of *TFR* is a legal right of

last a maximum of 2 years (they are analysed in chapter 4); hence imposing a minimum tenure of 4 years selects either permanent contract workers or workers hired with a temporary contract subsequently transformed into a permanent one.

¹³Evidence supporting this choice is in Pacelli (2005). Displaced workers are significantly less likely to find a new job within two months after separation, with respect to quitters.

the worker only under specific circumstances: eight years of tenure are elapsed, a few causes are met (health, housing, leave for training), firms are larger than 25 employees (even though among law scholars there is no agreement over whether such constraint binds) and less than 10 percent of the workforce applies for an advance withdrawal. As a results, it should not be surprising that the total number of advance withdrawals observed is not large, and it concerns less than 2 percent of the entire firm-worker pairs we observe (table 2). The share of advance withdrawals from the stock of *TFR* increases with tenure, and reaches some 6% of the workforce having 12 years of tenure. This very low share is consistent with our hypothesis that workers value the labour hoarding effect of TFR and normally do not withdraw, even if they could. Information on the mean and median shares of advance withdrawals suggests that workers go for a large share of the *TFR* (about 60%). Such amount is quantitatively relevant. Since the *TFR* stock increases approximately by one month of salary for every year of tenure, withdrawing 60% of the stock at the 10th year of tenure is equivalent to receiving about half of the yearly salary.

Withdrawals before tenure 8 are by definition consensual. Since an early withdrawal tends to reduce profits, we believe that firms authorize such advance withdrawals only if business conditions are good (a sort of informal profit sharing agreement on this specific aspect only), while advance withdrawals are denied if business conditions are less favorable. While we do not observe firms' profit, Table 2 shows that up to tenure 8 advance withdrawals are constantly more likely in growing firms than in shrinking firms. The opposite becomes true after the 8th year of tenure, when withdrawals may be non consensual. Although not a formal test, this is consistent with our interpretation.

Table 3 compares the sample composition at tenure 10 for the entire sample of those who survived in the job for at least 10 years and for those individuals among them that experienced an advance withdrawal at tenure 9. Individuals that experience an advance withdrawal are more likely to be males, in the mid thirties, non-manual workers, employed in larger firms (median firm size is 100 workers instead of about 50). We will discuss the link between firm size and withdrawals presenting the results.

6 Empirical Analysis and Econometric Strategy

In this section we first present the basic empirical model; then we turn to other important empirical issues, namely unobserved heterogeneity and reverse causality.

Table 2: Characteristics of advance withdrawals.

elapsed tenure (years)	total number (a)	percentage of employees (b)	percentage in growing firms (c)	percentage in shrinking firms (d)	mean withdrawal (e)	median withdrawal (f)
1	39	0.11	0.13	0.11	0.82	0.98
2	136	0.39	0.51	0.44	0.82	0.97
3	213	0.81	0.85	0.96	0.71	0.71
4	268	1.31	1.42	1.21	0.64	0.60
5	243	1.46	1.67	1.28	0.63	0.62
6	264	1.90	1.83	1.74	0.63	0.58
7	319	2.64	2.97	2.3	0.62	0.59
8	439	4.14	5.12	4.05	0.59	0.57
9	546	5.83	5.94	7.14	0.57	0.57
10	454	5.46	5.99	6.23	0.58	0.58
11	397	5.38	5.26	6.89	0.58	0.59
12	283	6.19	5.99	8.13	0.58	0.60
13	129	5.14	6.9	4.91	0.61	0.61
14	50	4.96			0.57	0.60
Total	3,780	1.59				
(a) Absolute number of withdrawals observed in the sample						
(b) Pct. of withdrawers over total no. of employees, for given tenure						
(c) Pct. of withdrawers over total no. of employees in growing firms, for given tenure						
(d) Pct. of withdrawers over total no. of employees in shrinking firms, for given tenure						
(e) Mean computed on positive withdrawals only						
(f) Median of positive withdrawals only						

6.1 The Basic Empirical Model

We highlight once more the key empirical implication that stemmed from our theoretical model: other things equal, workers that withdraw increase the probability of firm initiated separation. Empirically, we exploit the variability the withdrawal generates in the *TFR* stocks for given wage and tenure; such variability can be used to test our hypothesis. In words, we set a test to compare the probability of observing a firm initiated separation, at a given tenure and wage, for workers that did not withdraw their *TFR* stock and for workers who did. The natural setting for testing this empirical implication is the use of survivals models (Lancaster, 1990, Wooldridge, 2002). We estimate

$$h(t; D, X) = f(t|D, X)/[1 - F(t|D, X)] \quad (8)$$

where $h(t; D, X)$ is the hazard rate, i.e. the probability of ending the employment spell between t and $t + 1$ conditional on having "survived" on the job up to t , or the ratio between the density $f()$ and its cumulative function $1 - F()$, or survival function. It is expressed as a function of elapsed tenure t , of an eventual withdrawal D , and of a set of characteristics X including the wage.

Table 3: Sample composition at tenure=10 years.

		all workers at tenure=10 (a)	withdrawers at tenure=9 only (b)
daily real wage (c)	median	67	74
firm size	median	48	103
age at entry	median	25	24
manual	%	0.569	0.533
female	%	0.333	0.267
part time	%	0.063	0.038
geografic mover	%	0.289	0.300
industry= utilities	%	0.023	0.026
industry= chemical etc.	%	0.073	0.097
industry= mechanical	%	0.239	0.239
industry= textile, food etc.	%	0.236	0.200
industry= construction	%	0.064	0.072
industry= wholesale, retail	%	0.180	0.127
industry= transports	%	0.061	0.139
industry= banking, insurances	%	0.124	0.101
Observations	no.	8320	546
(a) Characteristics of all workers at 10 years of tenure			
(a) Characteristics of workers at 10 years of tenure that withdrew at tenure=9 years			
(c) Euro. (.000 lire in the estimates)			

Remark 6 *The goal of our empirical exercise is to test whether the impact of D on h is positive, i.e. whether the probability of a firm initiated separation between t and $t + 1$, conditional on an elapsed tenure t , is increased by withdrawing from TFR.*

Before discussing how to bring the theoretical model - and its simplifying assumptions - to the data, we present the econometric setup.

The firm decision of job termination is clearly a continuous time process, even though we describe it in discrete time in our theoretical model for analytical simplicity. The empirical approach to the problem should take into account such property, even though we work with discrete time data. Our specification of the hazard does take care of this issue. Assuming that the continuous time process can be specified as a proportional hazard model, one has:

$$h(t; D, X) = \kappa(D, X)h_0(t) \quad (9)$$

where κ is a non-negative function of the covariates and $h_0(t)$ is the baseline hazard. The important assumption here is that the process is separable in X, D and t , i.e. that the baseline hazard is the same for all individuals and it shifts due to the effect of the covariates. This is a very convenient assumption because odds ratios are then constant for every X . If we assume - as it is standard in the literature - that $\kappa(D, X)$ is an

exponential function, the hazard reads

$$h(t; D, X) = \exp(X\alpha + \beta D)h_0(t)$$

The specification of $h_0(t)$ can be parametric or non parametric. The most flexible option is to use a set of dummies on each t ; we choose it to mitigate as much as possible the effect of unobserved heterogeneity on the estimates¹⁴. Notice that the coefficients α, β are semi-elasticities of the hazard with respect to the covariates.

Prentice and Gloeckler (1978) and Jenkins (1995) show that the discrete time counterpart of an underlying continuous time proportional hazard model is a complementary log-log function. This means that under the hypothesis that the true process in continuous time is the above one, and under the hypothesis that the econometrician observes the process only at discrete points in time, the resulting hazard is the following:

$$h(t; D, X) = 1 - \exp\{-\exp[h_0(t) + X\alpha + \beta D]\}$$

so that taking logs

$$\log[-\log(1 - h(t; D, X))] = h_0(t) + X\alpha + \beta D \quad (10)$$

In light of the proportional hazard specification of the underlying continuous process, obtaining instantaneous odds ratios is straightforward:

$$OR = \frac{h(t; D = d, X)}{h(t; D = 0, X)} = \exp[\beta(d - 0)]$$

where we compute the relative increase in the instantaneous hazard due to D being equal to d with respect to D being equal to the baseline case 0 (no withdrawal).

6.2 Unobserved Heterogeneity

In the model individuals are ex-ante identical. In the empirical analysis we include a set controls X for observable worker and firm specific characteristics. However, equation (10) does not allow for unobserved heterogeneity. If unobserved heterogeneity is important, omitting it implies (i) a downward biased estimated duration dependence, (ii) a downward bias on the (absolute value of the) estimated parameters α and β , (iii) that the downward bias on the (absolute value of the) estimated parameters α and β increases

¹⁴More on this point below.

with t ¹⁵. It must be stated upfront that the effects of unobserved heterogeneity are mitigated by the use of a flexible specification of the baseline hazard (see Jenkins, 2005). This is the reason why we choose a non parametric and totally flexible specification for $h_0(t)$, i.e. a set of dummies on each t . Furthermore, the effects of non including unobserved heterogeneity work against the empirical implication we aim at testing, so that if we obtain a significant and positive β we may have a lower bound of the true and larger effect of a withdrawal on the hazard rate¹⁶. Nevertheless, we estimate equation (10) also including unobserved heterogeneity, as a robustness check. We assume that unobserved heterogeneity is uncorrelated to observables and we estimate a random intercept model. Data limitation forbids to model general correlation between unobserved heterogeneity and the (time invariant as well as time varying) covariates¹⁷. Defining v as a positive random variable with unit mean and finite variance σ^2 , distributed independently of t, X, D , equation (9) becomes

$$h(t; D, X, v) = \kappa(D, X)h_0(t)v \quad (11)$$

and equation (10) becomes

$$\log[-\log(1 - h(t; D, X, v))] = h_0(t) + X\alpha + \beta D + v \quad (12)$$

To estimate equation (12) we must specify the distribution of v . Two options are available: a parametric and a non parametric one. In the first case the most common choice is the Gamma distribution, that can be easily integrated out of equation (12) providing a closed form of the unconditional hazard. The non parametric approach applies Heckman and Singer (1984) seminal work, fitting an arbitrary discrete distribution whose parameters are its mass points and the probabilities that individuals belong to one of them. To be more specific: suppose we have two mass points, i.e. two kinds of individuals; equation (12) becomes

¹⁵It can be shown that $[1 - F(t, D, X|v)] = S(t, D, X|v) = [S(t, D, X)]^v$, i.e. unobserved heterogeneity v , or frailty, scales the no-frailty component survival function (see Jenkins, 2005). It means that high- v individuals leave the job faster than low- v individuals, changing the sample composition over time and generating the effects mentioned in text when omitted from the model.

¹⁶An intuition could be the following. Workers with low (unobserved) propensity to change jobs will stay longer and therefore be more likely to withdraw, making it harder to obtain a positive effect of withdrawal on separation.

¹⁷Horowitz and Lee (2004) provide a consistent estimator for survival models with unobserved heterogeneity possibly correlated to observables. Such estimator requires data with repeated spells for each individual. As already anticipated, such dataset is not currently available for the specific purpose of this analysis.

$$\log[-\log(1 - h_1(t; D, X, v_1))] = h_0(t) + X\alpha + \beta D + v_1 \quad (13)$$

$$\log[-\log(1 - h_2(t; D, X, v_2))] = h_0(t) + X\alpha + \beta D + v_2 \quad (14)$$

and the contribution of each individual to the estimate will be the probability-weighted average of the two above hazards.

In terms of our model, a parametric Gamma distribution for v implies that every individual is different from the others depending on a random draw from that distribution. A non parametric discrete distribution implies that we have n groups of individuals, that every individual is identical to the other members of his group and different from members of other groups; it also implies that each person is allocated randomly to a group. We apply both assumptions; however the non parametric one is more flexible and it could have a more straightforward interpretation in terms of our model. If $n = 2$ we could label the groups "good health" and "poor health" people, or "shirkers" and "non shirkers", or "movers" and "stayers", and so on¹⁸.

6.3 Reverse Causality and Other Issues

The theoretical model excludes quits and imposes exogenous withdrawals. Our empirical strategy encompasses these two assumptions. In the real world, with endogenous quits, it may well be possible that individuals first decide to leave their current job, and subsequently withdraw from their accumulated stock of TFR . In other words, there may be a reverse causality effect, in the sense that an anticipated separation is followed by an advance withdrawal of the TFR stock and then by the actual separation itself. Clearly, individuals that are certain to leave their current job are only interested in the income effect of TFR , the job security effect vanishes. However, the possibility of reverse causality can be fairly easily ruled out by introducing lags between the withdrawal and the observed separation. We introduce up to three lags, regarding as totally unrealistic the hypothesis that workers can plan to quit 24 to 36 months in advance. This makes unavoidable the selection of employment spells that last a minimum of four years. While this is important to avoid the reverse causality problem, the drawback is limited. In fact, we exclude short spells that provide little information for the purpose of the analy-

¹⁸The obvious limitation of both approaches is the randomness of v . As already stated, the only way of relaxing this assumption is to have access to a repeated spells sample.

sis: accumulated TFR stock is very low, advance withdrawals are extremely rare; hence, variability in TFR stock at given wage and tenure is very limited. There is a clear trade off between higher order lags and sample selection. Three lags proved to be the best compromise.

Finally, we assume that the per period wage is exogenously fixed at w throughout the employment relationship. This is certainly a reasonable assumption at the individual level in Italy, since most of the wages are negotiated at the industry or national level¹⁹; individual wage cuts below the collectively set wage are hardly possible²⁰.

7 Results

7.1 Unconditional Hazard

We begin by providing simple statistics on the hazard rate conditional on a withdrawal having or not having occurred at $t - 3$. We choose the longest lag to avoid any reverse causality problem. These hazard rates are estimated non parametrically, using the Kaplan-Meier estimator of the probability of separation between t and $t + 1$ conditional on having been employed for t periods:

$$h(t) = \frac{m(t)}{n(t)}$$

where m is the number of spells terminated between t and $t + 1$ and n is the number of ongoing spells at t . We compute $h(t)$ separately for individuals that have withdrawn at $t - 3$ and for individuals that have not. Figure 2 plots $h(t)$ for $t = 5$ to 14. Remarkably, the hazard rate conditional on advance withdrawals having occurred lies clearly above the hazard rate for individuals that did not withdraw three years before. Such difference is very large up to tenure 11, while the gap closes for longer tenures. While this pattern may look puzzling, it is actually linked to firm size. Figure 3 shows that up to 7 years of tenure withdrawals take place in firms with size that is very similar to the the median firm size for the corresponding tenure. Thereafter, the median firm size of workers that withdraw their stock of *TFR* is twice as large the average firm size at that particular tenure. In other words, the additional withdrawals (that increase the share of withdrawers from 2% to 5%) after tenure 7 take place in large firms. Not surprisingly, if we replicate Figure

¹⁹Only about 10% of the total wage is bargained at the individual level, on average (chapter 1).

²⁰Notice that this does not deny the firm the possibility to consent to an advance withdrawal if the worker is hit by an adverse liquidity shock when s/he has no right to access the *TFR* fund. This consent makes the wage neither endogenous nor downward flexible.

2 selecting only firms in the first quintile of the size distribution (by elapsed tenure) the puzzle disappears (Figure 4). This discussion suggests that firm size is an important determinant of the individual probability of withdrawing, as we further discuss in the multivariate analysis that follows.

7.2 Empirical Specification

The regressor of interest can be specified in different ways. The straightforward one is to use the dummy D to signal that an advance withdrawal has taken place. However, there are better alternatives. First, the (absolute value of the) withdrawal rate if drawing occurred, zero otherwise, so that

$$TFRdrawR(t) = \begin{cases} \left| \frac{TFR_t - TFR_{t-1}}{TFR_{t-1}} \right| & \text{if } D(t) = 1 \\ 0 & \text{if } D(t) = 0 \end{cases} \quad (15)$$

Second, the log of the (absolute value of the) amount withdrawn if it occurred, zero otherwise, so that

$$TFRdrawA(t) = \begin{cases} \ln |TFR_t - TFR_{t-1}| & \text{if } D(t) = 1 \\ 0 & \text{if } D(t) = 0 \end{cases}$$

The advantage of using $TFRdrawR$ or $TFRdrawA$ instead of the indicator D is clear: we allow for larger withdrawals to have a more sizeable effect on the hazard. The advantage of using $TFRdrawR$ with respect to $TFRdrawA$ is in the ease to interpret its estimated coefficient. However, as we will see, all definitions provide coherent estimated results, as a further proof of robustness.

As anticipated, the advance withdrawal regressor always enters the analysis as a lagged value. This is coherent with our theoretical setting, where we show that the realization of a withdrawal leads to a subsequent increase in the probability of separation. The risk of reverse causality in the relationship between advance withdrawal and subsequent job separation is present when the time lag between the two events is sufficiently short. Such risk, conversely, is not present when the time lag between the two events is large. So we include three lags in the regressor of interest, acknowledging that lag 1 might be affected by reverse causality bias, but not higher order lags. As a robustness check we also estimate the model excluding lag 1 and lag 2 from the specification, providing further evidence on the direct effect of an advance withdrawal on the probability of separation.

Our specification includes time varying as well as time invariant controls, detailed in Table 4. $h_0(t)$ is specified as a set of dummy variables on t . Available time invariant characteristics are individual's gender, age at entry, occupation, type of contract (full or part time), whether s/he works in a province different from where s/he was born, and firm's industry. Among time varying covariates, observed once a year, we have daily average real wage (at 2003 prices, 000 lire) and firm size (the number of employees); both enter the specification in logs and lagged one period. Two covariates deserve a special remark. We have a dummy for growing and a dummy for shrinking firms, defined as positive (negative) yearly changes in the stock of employees (lagged one period). They aim at controlling for firms' profitability in general terms, as separation rates should naturally be different in expanding and contracting firms.

7.3 Bottom-line Results

The bottom-line results of our multivariate analysis (equation 10) are specified in table 4. Estimated standard errors are robust to heteroschedasticity. Duration dependence is negative, as expected, but not smooth, supporting the choice of a flexible specification. Lagged wages and firm size have both the expected negative impact on the hazard, while the manual occupation dummy, as well as the part time and female ones feature the expected positive sign. Age at entry has a positive impact on the hazard of separation up to 27 years, then negative. Having reduced the workforce at the firm level in the past has a positive impact on the hazard, as expected, while having increased it does not impact the hazard differently from having had a constant employment level.

The main empirical result concerns the sign and significance of the advance withdrawals, that are indicated in the figure as "withdrawal at $t-j$ ". Remarkably, *withdrawal at $t-1$* and *withdrawal at $t-3$* are positive and significant, while it does not appear significant at lag 2²¹. This is so whatever the definition of withdrawal (share, log of absolute value, dummy on the event), as reported in table 5, specification A²². Quantitatively, the result is also sizeable. Taking at face value the coefficient on lag 3 in the first model (share of withdrawal), in figure 5 we draw the instantaneous odds ratio of drawing a given share of *TFR* relative to not drawing, along with the 95% confidence interval. The implied increase in the instantaneous hazard is not negligible. An indi-

²¹We must remember that we are dealing with a few cases of advance withdrawal, so that statistical significance cannot always be achieved.

²²Only the coefficients of interest are reported. Coefficients on controls are unchanged with respect to table 4 and are not reported.

Table 4: Hazard rate estimate. Baseline specification.

	β	s.e.	p value
withdrawal at t-1	0.332	0.101	0.001
withdrawal at t-2	-0.010	0.130	0.937
withdrawal at t-3	0.274	0.131	0.036
elapsed tenure=4			
elapsed tenure=5	-0.043	0.036	0.230
elapsed tenure=6	-0.231	0.040	0
elapsed tenure=7	-0.266	0.043	0
elapsed tenure=8	-0.306	0.046	0
elapsed tenure=9	-0.270	0.049	0
elapsed tenure=10	-0.185	0.050	0
elapsed tenure=11	-0.338	0.056	0
elapsed tenure=12	-0.237	0.067	0
elapsed tenure=13	-0.362	0.093	0
elapsed tenure=14	-0.830	0.179	0
log real daily wage at t-1	-0.423	0.042	0
log firm size at t-1	-0.129	0.008	0
dummy growing firm at t-1	0.008	0.035	0.828
dummy shrinking firm at t-1	0.346	0.031	0
dummy part time	0.347	0.049	0
dummy female	0.095	0.029	0.001
age at entry	-0.219	0.010	0
age at entry squared/100	0.388	0.016	0
dummy manual occupation	0.106	0.030	0.001
dummy geographic mover	0.021	0.027	0.441
industry= utilities	-1.538	0.226	0
industry= chemical etc.	-0.355	0.059	0
industry= mechanical	-0.428	0.044	0
industry= textile, food etc.	-0.323	0.043	0
industry= construction			
industry= wholesale, retail	-0.254	0.044	0
industry= transports	-0.588	0.075	0
industry= banking, insurances	-0.498	0.057	0
constant	3.061	0.241	0
Wald chi2	3113.09		
no. observations	99765		
no. workers	19554		
no. Separations	7624		
no. Sep. and withdraw at t-1	226		
no. Sep. and withdraw at t-2	141		
no. Sep. and withdraw at t-3	130		
Probability of firm initiated separation.			
Complementary log log model. Robust s.e.			
Withdrawal is defined as withdrawal rate.			

Table 5: Hazard rate estimate. Robustness to definition of withdrawal and to reverse causality.

	Specification A			Specification B		
	β	s.e.	p value	β	s.e.	p value
1. withdrawal rate						
withdrawal at t-1	0.332	0.101	0.001			
withdrawal at t-2	-0.010	0.130	0.937			
withdrawal at t-3	0.274	0.131	0.036	0.276	0.131	0.035
2. log absolute withdrawal						
withdrawal at t-1	0.015	0.008	0.052			
withdrawal at t-2	-0.008	0.010	0.454			
withdrawal at t-3	0.021	0.011	0.046	0.021	0.011	0.044
3. withdrawal dummy						
withdrawal at t-1	0.128	0.069	0.063			
withdrawal at t-2	-0.067	0.086	0.438			
withdrawal at t-3	0.176	0.090	0.051	0.179	0.090	0.048
Probability of firm initiated separation.						
Complementary log log model. Robust s.e.						
Coefficients of interest only. Controls as in baseline specification, not reported						
Specification A - baseline specification						
Specification B - robustness to reverse causality (lag 3 only)						

vidual that withdraws 60% of the fund increases the instantaneous hazard rate by some 17-18% three years later. In other words, an individual with at least 10 years of tenure that withdraws the *TFR* fund increases his hazard rate from 10% to about 12% three years later.

7.4 Extensions and Robustness

The first key robustness test refers to unobserved heterogeneity. Working on our bottomline specification, we introduce unobserved heterogeneity both Gamma distributed and non parametric (with 2 mass points). Table 6 shows that, although significant, unobserved heterogeneity has a negligible effect on the parameters of interest²³. Hence we are confident in presenting estimates that impose no unobserved heterogeneity.

Table 5 above, specification B presents the second key robustness test. In order to fully avoid the risk of reverse causality, we disregard all advance withdrawals that take place in the 24 months before the actual separation date, since such advance withdrawals may capture the reverse causality mechanism. An advance withdrawal that takes place 3 years before the separation date features a positive and significant coefficient.

We now turn to the effect of firm size on the hazard of job termination. As we pointed out in section 2, there is a discontinuity over the firm size distribution in the application

²³It has a negligible effect also on the other controls, not reported.

Table 6: Hazard rate estimate. Robustness to unobserved heterogeneity.

	β	s.e.	p value
1. unobserved heterogeneity: no			
withdrawal at t-1	0.3324	0.101	0.001
withdrawal at t-2	-0.0104	0.130	0.937
withdrawal at t-3	0.2741	0.131	0.036
2. unobserved heterogeneity: gamma			
withdrawal at t-1	0.3317	0.100	0.001
withdrawal at t-2	-0.0111	0.128	0.931
withdrawal at t-3	0.2744	0.133	0.039
<i>Gamma var.</i>	<i>0.207</i>	<i>0.113</i>	<i>0.068</i>
<i>LR test of gamma var = 0: chibar2(01)=</i>	<i>3.589</i>		<i>0.029</i>
3. unobserved heterogeneity: discrete, 2 m.p.			
withdrawal at t-1	0.3342	0.102	0.001
withdrawal at t-2	-0.0057	0.130	0.965
withdrawal at t-3	0.2753	0.135	0.041
<i>v1</i>	<i>0.000</i>		
<i>v2</i>	<i>2.066</i>	<i>0.221</i>	<i>0.000</i>
<i>prob. Type 1</i>	<i>0.911</i>	<i>0.027</i>	<i>0.000</i>
<i>prob. Type 2</i>	<i>0.089</i>	<i>0.027</i>	<i>0.001</i>
Probability of firm initiated separation.			
Complementary log log model. Robust s.e.			
Coefficients of interest only. Controls as in baseline specification, not reported			
Withdrawal is defined as withdrawal rate.			

of the legislation on *TFR*, namely firms below 25 employees seems to be exempted from the obligation to allow withdrawals from the fund. This implies that in these firms withdrawals should be consensual, whatever the elapsed tenure, and hence they should be less harmful for the career of those who withdraw. As Table 2 confirms, the small number of observed withdrawals does not allow us to perform a proper Regression Discontinuity Design; however, we can interact the rate of *withdrawal at t* – 3²⁴ with firm size at *t* – 3 (above or below 25 employees). Notice that this does not mean that we have only consensual withdrawals in small firms and only non consensual withdrawals in larger firms. In the first case, the jurisprudence is not unanimous in interpreting the law with respect to firms below 25 employees²⁵; in the second case, consensual withdrawals can take place before the 8th year of tenure and even afterward (when parameters set by the law are exceeded). What we can achieve is to increase the share of non consensual withdrawals when focussing on firms above 25 employees. Table 7 reports the coefficients of interest²⁶. Although less precisely estimated, we learn that the average estimated coefficient of .27 increases to .34 among large firms and decreases to .22, not significant, among small firms. This is what we expected. This also sheds some light on the pattern

²⁴ Changing the definition of the regressor does not change the results.

²⁵ See footnote 4.

²⁶ Again, coefficients on controls are unchanged and not reported.

Table 7: Hazard rate estimate. Interaction with firm size.

	β	s.e.	p value
withdrawal at t-1	0.332	0.101	0.001
withdrawal at t-2	-0.010	0.130	0.936
withdrawal at t-3 in firms below 25 employees	0.222	0.174	0.201
withdrawal at t-3 in firms above 25 employees	0.341	0.196	0.082
Probability of firm initiated separation.			
Complementary log log model. Robust s.e.			
Coefficients of interest only. Controls as in baseline specification, not reported			
Withdrawal is defined as withdrawal rate.			

by size of the Kaplan-Meyer estimator of the unconditional hazard we discussed at the beginning of the section. Excluding composition effects confirms that a non consensual withdrawal is more likely to increase the hazard of firm initiated separation.

The legislation introduces also a discontinuity over the elapsed tenure dimension (before or after the 8th year). Again, a Regression Discontinuity Design is beyond the possibility of the available data. Related to this is the estimated pattern of the duration dependence, that seems to show a slowdown in the hazard at year 8 or 9 (Table 4). Does this point to a possible interaction between D and t in the data? Abbring and Van Der Berg (2003) propose an estimator for this case; however, that estimator is identified only if the "no anticipation" assumption is acceptable. If reverse causality is an issue in our model, and we so believe, than the estimator is not identified. Even if this was not an issue, the discussion on unobserved heterogeneity applies here as well; unobserved heterogeneity, misspecification and heteroschedasticity are indistinguishable, from a pure econometric point of view. Our very flexible baseline hazard captures not only the effect of unobserved heterogeneity but also of eventual missing interactions between t and the covariates. Net of it, in our baseline specification we estimate the residual average effect of a withdrawal on the hazard, getting possibly a lower bound estimate of the true effect.

Finally, in Appendix 9.5 we check the effect of our definition of firm initiated separation, as well as the effect of sample selection with respect to the inclusion or exclusion of workers separating because of retirement or quit.

Overall all our results point consistently in favour of the Empirical Implication of our theoretical model: other things equal, workers that withdraw from their stock of TFR increase the probability of a firm initiated separation.

8 Conclusions

This work has studied the severance payment dimension of a mandatory deferred wage payment (*TFR*). Theoretically, we have shown that deferred wages increase labor hoarding from the firm standpoint if two conditions hold: wages are rigid, and deferred wages are accumulated at interest rates that are below market rates. Indeed, if wages were fully flexible, the problem would not even arise, since deferred wages would be completely irrelevant from the allocative standpoint. The second condition ensures that the longer the tenure, the lower the average wage cost. As long as both conditions are satisfied, as it certainly seems to be the case for Italy, firms hoard labour in bad times, holding on to marginal loss as a way to increase the average duration of jobs and hence decrease average labour cost.

Empirically, we have shown that individuals with larger severance payments (in the form of larger *TFR* credit) at given tenure and wage, have lower hazard rates of job termination. Specifically, we have used information on about 20,000 employment spells to test a key empirical implication: workers who withdraw their accumulated stock of unpaid wages in advance, have a subsequent increase in the probability of being fired. This prediction seems to be empirically robust and quantitatively sizeable. A withdrawal of 60% of the *TFR* stock increases the instantaneous hazard rate of job termination by some 20%.

Our results are also relevant in the policy debate, which has always analyzed the impact of deferred wage payments in terms of the social security system. To the best of our knowledge, the effect of *TFR* on labour market and labour mobility is new. Further, the social security reform approved by the Italian Parliament in 2004 aims at using the *TFR* to boost the second pillar of the social security system. Our results suggest that a shift of *TFR* funds into pension funds will increase labour turnover, so that implementing such reform would have a direct effects in the social security area, and an indirect effect on the labour market. For example, when the 2004 reform will be implemented, an individual with at least ten years of tenure will divert from the *TFR* to the pension fund the annual *TFR* quota, equivalent to withdrawing less than 10% of the fund; his hazard would increase from 10% to about 10.4% in the first year (figure 5). A significant but small effect, that will obviously increase as time goes by and *TFR* is "withdrawn" every year.

9 Appendix

9.1 The Solution with Flexible Wages

If wages were fully flexible, and the firm and the worker shared the total surplus from the job, mandatory deferred wages would not have any allocative impact. To see this simply define the surplus from the job as

$$S_t(p_t) = \Pi_t(p_t) + W_t$$

and assume that the worker gets a fraction β of the total surplus in each and every period. The total surplus at time t reads

$$S_t(p_t) = \begin{cases} p_t + \frac{1}{1+r} [\int_{p_{t+1}^*}^{p^{\max}} S_{t+1}(z) dF(z)] & t = 1, \dots, (T-1) \\ p_t & t = T \end{cases} \quad (16)$$

which is an expression that is independent of wages and deferred wages. This is not surprising, since the total surplus is independent of a transfer between the two parts. The optimal separation policy in this case would be $S_t(\hat{p}_t) = 0$. Proceeding backward it is clear that the reservation productivity in this case would be $\hat{p}_t = 0$ and that the wage would simply be

$$w(p_t) = (1 - \delta)\beta p_t$$

so that the marginal wage would be zero ($w(\hat{p}_t) = 0$). In words, deferred wages are irrelevant.

9.2 TFR increases firms' profits

To prove it, as long as $\delta > 0$ and $r > \tilde{r}$, we just need to take the derivative with respect to δ of the firm present discounted value that in our two period specification reads

$$\begin{aligned} \Pi_1(p_1) &= p_1 - (1 - \delta)w + \frac{1}{1+r} \left\{ \int_{p_2^*}^{p^{\max}} \Pi_2(z) dF(z) - F(p_2^*) \delta w (1 + \tilde{r}) \right\} \\ \Pi_1(p_1) &= p_1 - (1 - \delta)w + \frac{1}{1+r} \left\{ \int_{p_2^*}^{p^{\max}} \left[z - (1 - \delta)w - \frac{\delta w (1 + \tilde{r})}{1+r} \right] dF(z) - F(p_2^*) \delta w (1 + \tilde{r}) \right\} \end{aligned}$$

$$\begin{aligned}\frac{\partial \Pi_1}{\partial \delta} &= w + \frac{1}{1+r} \left\{ \int_{p_2^*}^{p^{\max}} \frac{\partial \Pi_2(z)}{\partial \delta} dF(z) - F(p_2^*) \delta w(1+\tilde{r}) \right\} - \frac{f(p_2^*)}{1+r} \Pi_2(p_2^*) \frac{\partial p_2^*}{\partial \delta} - \frac{f(p_2^*)}{1+r} \delta w(1+\tilde{r}) \\ \frac{\partial \Pi_1}{\partial \delta} &= w + \frac{w}{1+r} \left[1 + \frac{(1+\tilde{r})^2}{1+r} \right] (1 - F(p_2^*)) - \frac{F(p_2^*) w(1+\tilde{r})}{1+r} - \frac{f(p_2^*)}{1+r} \frac{\partial p_2^*}{\partial \delta} [\Pi_2(p_2^*) + \delta w(1+\tilde{r})]\end{aligned}$$

Since the last expression in the brackets is zero by virtue of the reservation productivity

$\Pi_2(p_2^*) = -\delta w(1+\tilde{r})$ the previous expression reads

$$\frac{\partial \Pi_1}{\partial \delta} = w \left[1 - \frac{F(p_2^*)(1+\tilde{r})}{1+r} \right] + \frac{w}{1+r} \left[1 + \frac{(1+\tilde{r})^2}{1+r} \right] (1 - F(p_2^*)) > 0$$

9.3 The Impact of TFR on Workers' Welfare

In the two periods version of the model, the welfare of the worker depends stochastically on the probability of being employed at time $t = 2$. The welfare of the worker at the beginning of the relationship is

$$W_1 = (1-\delta)w + \frac{1}{1+r} \left\{ [1 - F(p_2^*)] \left\{ (1-\delta)w + \frac{1}{1+r} [(1+\tilde{r})\delta w + (1+\tilde{r})^2 \delta w] \right\} + F(p_2^*)(1+\tilde{r})\delta w \right\}$$

where the worker enjoys the current wage for certainty, while the value of the relationship at time $t = 2$ depends on the probability that the worker is not fired, which happens with probability $1 - F(p_2^*)$ where p_2^* is determined by the firm continuation policy. To study the impact of *TFR* we need to study $\frac{\partial W_1}{\partial \delta}$ which can be written as

$$\frac{\partial W_1}{\partial \delta} = \frac{\partial W_1}{\partial \delta} \Big|_{p_2^* = \bar{p}_2^*} + \frac{\partial W_1}{\partial p_2^*} \frac{\partial p_2^*}{\partial \delta}$$

The formal value of the two derivatives is

$$\begin{aligned}\frac{\partial W_1}{\partial \delta} \Big|_{p_2^* = \bar{p}_2^*} &= -\frac{w(1 - F(p_2^*))}{(1+r)} \left[\frac{r - \tilde{r}}{1+r} \right] - w \left\{ 1 - \frac{(1+\tilde{r})}{(1+r)} \left[(1 - F(p_2^*)) \frac{(1+\tilde{r})}{(1+r)} + F(p_2^*) \right] \right\} < 0 \\ \frac{\partial W_1}{\partial p_2^*} \frac{\partial p_2^*}{\partial \delta} &= -\frac{f(p_2^*) p_2^*}{1+r} \frac{\partial p_2^*}{\partial \delta} > 0\end{aligned}$$

Our assumption on the welfare effect of *TFR* implies assuming

$$\frac{\partial W_1}{\partial \delta} > 0 \quad \Rightarrow \quad \frac{\partial W_1}{\partial p_2^*} \frac{\partial p_2^*}{\partial \delta} > \left| \frac{\partial W_1}{\partial \delta} \Big|_{p_2^* = \bar{p}_2^*} \right|$$

9.4 Withdrawing Shocks

Formally, the existence of a withdrawing shock modifies the firm problem. Assuming that withdrawing takes place at rate μ , the firm problem reads

$$\Pi_t(p_t) = \begin{cases} p_t - (1 - \delta)w + \frac{1}{1+r} \left\{ \begin{aligned} & + \mu[-R_{t+1} + \int_{\tilde{p}_{t+1}^*}^{p^{\max}} \Pi_{t+1}(z) dF(z)] \\ & + (1 - \mu) \left[\int_{p_{t+1}^*}^{p^{\max}} \Pi_{t+1}(z) dF(z) - F(p^*) R_{t+1} \right] \end{aligned} \right\} & t = 1 \dots (T - 1) \\ p_t - (1 - \delta)w - \frac{1}{1+r} R_{t+1} & t = T \end{cases} \quad (17)$$

while the value of R_t evolves according to rule

$$R_{t+1} = \begin{cases} (1 + \tilde{r})\delta w & \text{if worker withdraws at } t \\ (1 + \tilde{r})[R_t + \delta w] & \text{if worker does not withdraw at } t \end{cases}$$

with $R_1 = 0$. Let us we consider a firm at the beginning of the second period that is employing a worker who has just withdrawn the stock of TFR. For such firm, the continuation policy is described by the following reservation productivity \tilde{p}_2^*

$$0 = \tilde{p}_2^* - (1 - \delta)w - \frac{1}{1+r}[\delta w(1 + \tilde{r})]$$

Conversely, when a firm is hiring a worker who has not withdrawn in the previous period, the continuation policy would be

$$0 = p_2^* - (1 - \delta)w - \frac{1}{1+r}[\delta w(1 + \tilde{r})^2 + \delta w(1 + \tilde{r})]$$

so that clearly

$$\tilde{p}_2^* > p_2^*$$

from which it follows that when a worker withdraws the *TFR*, the firm reduces its incentive to hold to marginal losses.

9.5 Firm initiated separations and sample selection: robustness check

Our separation indicator is defined as follows. $S = 0$ if the spell is right censored; $S = 1$ means firm initiated separation, i.e. subsequent non employment spell length $U \geq n$ and age at separation < 55 ; $S = 2$ means quit, i.e. subsequent non employment spell length $U < n$ and age at separation < 55 ; $S = 3$ means retirement, i.e. age at separation ≥ 55 . The cloglog model always tests $S = 0$ vs $S = 1$.

The first robustness check concerns n , the subsequent non employment spell length that defines quits. We modify it from 0 months (all separations are firm initiated,

provided that the worker is younger than 55) to 4 months. We do not claim to disentangle quits from layoffs perfectly; what we obtain, as we increase n , is to increase the share of pure firm initiated separations included in $S = 1$. This should let emerge more clearly the effect of the withdrawal. Table 8, sample 2, shows that while the coefficient of *withdrawal at $t - 1$* is almost unaffected by n , the coefficient of *withdrawal at $t - 3$* increases with n ²⁷. As expected, the labour hoarding effect emerges stronger in this case. $n = 2$ is the benchmark case, used in the text.

The second robustness check concerns the sample definition. Provided that we test $S = 0$ vs $S = 1$ we can either include or exclude from the sample individuals for which $S = 2$ or $S = 3$. Under the assumption of independence in competing risks, the destination-specific hazards can be estimated separately. In this case we can keep the whole sample, and estimate the parameters specific to the exit $S = 1$ directly (sample 1 in Table 8). This is exactly true in continuous time models, where in every instant only one exit can be taken. In our case of interval-censored data this is approximately true, accuracy depending on how small destination specific hazards are (Jenkins, 2005). The alternative is selecting the sample to exclude individuals for which $S = 2$ and/or $S = 3$ to be able to estimate the parameters specific to the exit $S = 1$ (sample 2 and 3 in Table 8). The drawback in the first case is the mentioned approximation; in the second case it is the possibly endogenous sample selection introduced. Table 8 shows that all this is non influential on our results. The coefficients of interest are modified only marginally by the sample used, their significance never changes.

We choose sample 2 as the benchmark case (excluding only retirements from the sample). The reason for this choice becomes clear in Table 9. To be able to estimate all destination-specific hazards simultaneously we pretend just for this exercise to have a proper intrinsically discrete time data, and we assume a convenient multinomial logistic specification of the hazard (abandoning the assumption of proportional hazard). The results for $S = 1$ are coherent with those presented in section 7, as a further robustness. The results for $S = 2$ confirm that lag 1 withdrawal suffers from reverse causality, while lag 3 does not. The results for $S = 3$ point to the fact that retirement time is known well in advance, so that also lag 3 could be influenced by reverse causality. Hence, to be very conservative we exclude retirements from the sample and in text we always use sample 2.

²⁷Share of withdrawal. Log absolute withdrawal and dummy on withdrawal show exactly the same behaviour. Results not reported.

Table 8: Hazard rate estimate. Robustness over quit definition and sample selection.

	sample 1			sample 2 (base)			sample 3		
	β	s.e.	p value	β	s.e.	p value	β	s.e.	p value
U length ≥ 0 months									
withdrawal at t-1	0.2898	0.073	0	0.3285	0.075	0			
withdrawal at t-2	0.0312	0.090	0.728	0.0841	0.093	0.366			
withdrawal at t-3	0.1437	0.098	0.141	0.2093	0.101	0.039			
U length ≥ 1 month									
withdrawal at t-1	0.3418	0.082	0	0.3354	0.083	0	0.3637	0.083	
withdrawal at t-2	0.0107	0.105	0.919	0.0075	0.106	0.944	0.0323	0.105	0.
withdrawal at t-3	0.1648	0.113	0.145	0.1806	0.113	0.109	0.1807	0.113	0.
U length ≥ 2 months (base)									
withdrawal at t-1	0.3424	0.101	0.001	0.3324	0.101	0.001	0.3684	0.101	
withdrawal at t-2	-0.0069	0.130	0.958	-0.0104	0.130	0.937	0.0121	0.130	0.
withdrawal at t-3	0.2618	0.131	0.046	0.2741	0.131	0.036	0.2762	0.131	0.
U length ≥ 3 months									
withdrawal at t-1	0.3548	0.103	0.001	0.3433	0.104	0.001	0.3954	0.104	
withdrawal at t-2	-0.0878	0.137	0.522	-0.0924	0.138	0.503	-0.0620	0.137	0.
withdrawal at t-3	0.2876	0.133	0.031	0.3002	0.133	0.024	0.3030	0.133	0.
U length ≥ 4 months									
withdrawal at t-1	0.3226	0.106	0.002	0.3115	0.107	0.004	0.3655	0.107	0.
withdrawal at t-2	-0.0867	0.140	0.535	-0.0911	0.140	0.516	-0.0641	0.140	0.
withdrawal at t-3	0.3277	0.134	0.015	0.3408	0.134	0.011	0.3364	0.134	0.
<i>Definition of separation (S) and sample:</i>									
<i>S=0 right censored spell</i>				<i>sample 1</i>	<i>sample 2</i>	<i>sample 3</i>			
				<i>in</i>	<i>in</i>	<i>in</i>			
<i>S=1 firing i.e. U length $\geq n$ months and age at sep < 55</i>				<i>in</i>	<i>in</i>	<i>in</i>			
<i>S=2 quit i.e. U length $< n$ months and age at sep < 55</i>				<i>in</i>	<i>in</i>	<i>out</i>			
<i>S=3 retirement i.e. age at separation ≥ 55</i>				<i>in</i>	<i>out</i>	<i>out</i>			
<i>cloglog always tests separation=0 versus separation=1</i>									
Probability of firm initiated separation. Complementary log log model. Robust s.e.									
Coefficients of interest only. Controls as in baseline specification, not reported									
Withdrawal is defined as withdrawal rate.									

Table 9: Multinomial logit estimate.

	β	s.e.	p value
S=1			
withdrawal at t-1	0.384	0.107	0
withdrawal at t-2	0.014	0.136	0.918
withdrawal at t-3	0.305	0.139	0.028
S=2			
withdrawal at t-1	0.316	0.117	0.007
withdrawal at t-2	0.187	0.136	0.171
withdrawal at t-3	0.122	0.165	0.459
S=3			
withdrawal at t-1	-0.051	0.240	0.833
withdrawal at t-2	0.209	0.246	0.395
withdrawal at t-3	0.458	0.246	0.063
Definition of separation (S):			
S=0 right censored spell: benchmark case			
S=1 firing			
S=2 quit			
S=3 retirement			
Multinomial logit model			
Coefficients of interest only. Controls as in baseline specification, not reported			
Withdrawal is defined as withdrawal rate.			

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Figures

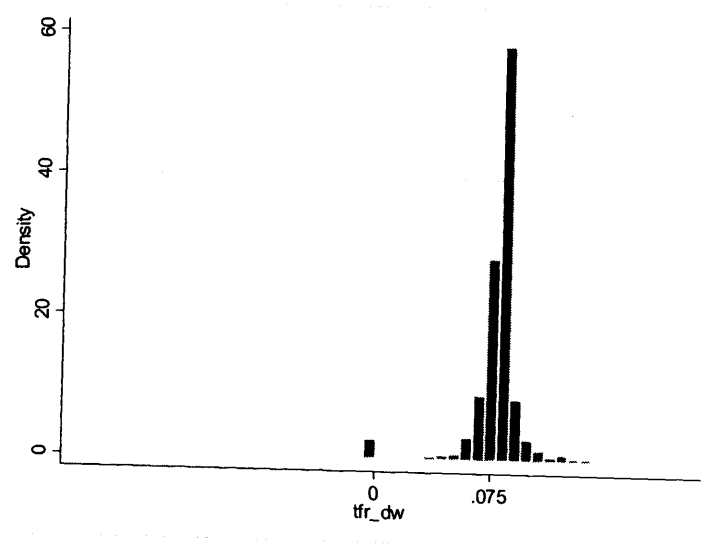


Figure 1: Distribution of TFR changes over annual wage, 1993

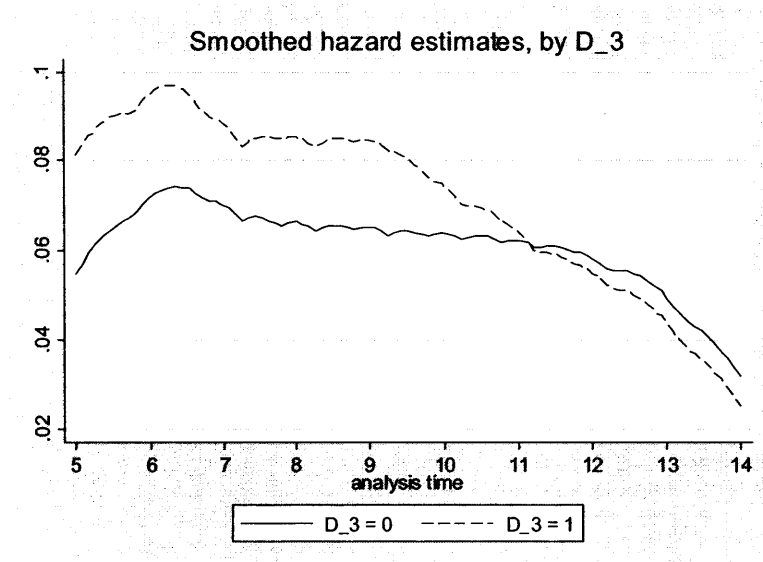


Figure 2: Kaplan-Meier hazard, withdrawers and non withdrawers at t-3

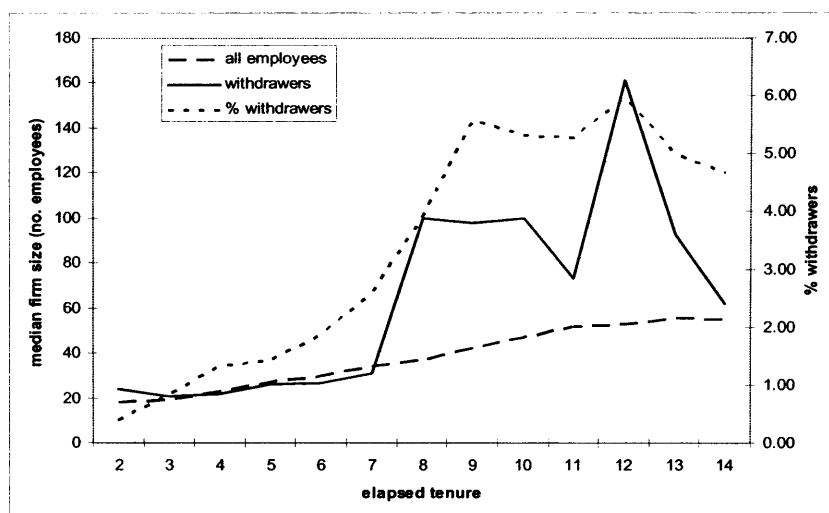


Figure 3: Median firm size and % withdrawers, by tenure

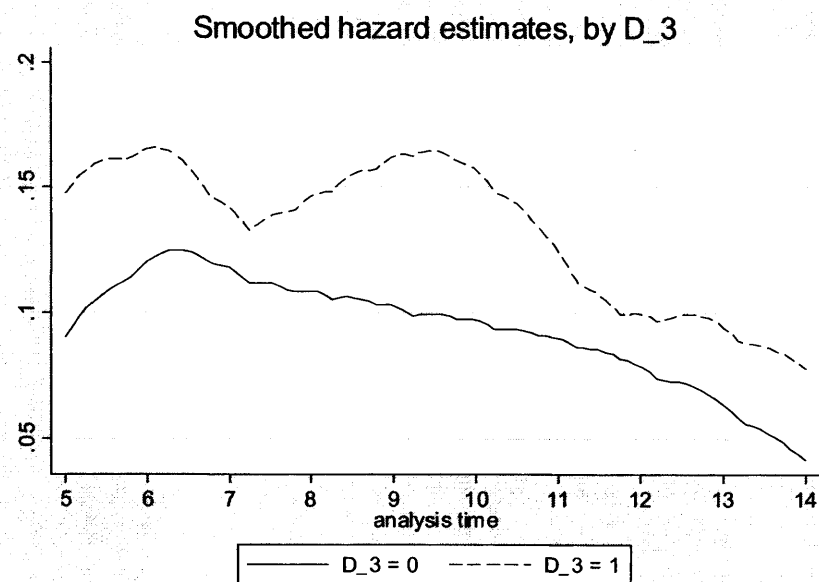


Figure 4: Kaplan-Meyer hazard, withdrawers and non withdrawers at t-3. First quintile of firm size distribution only

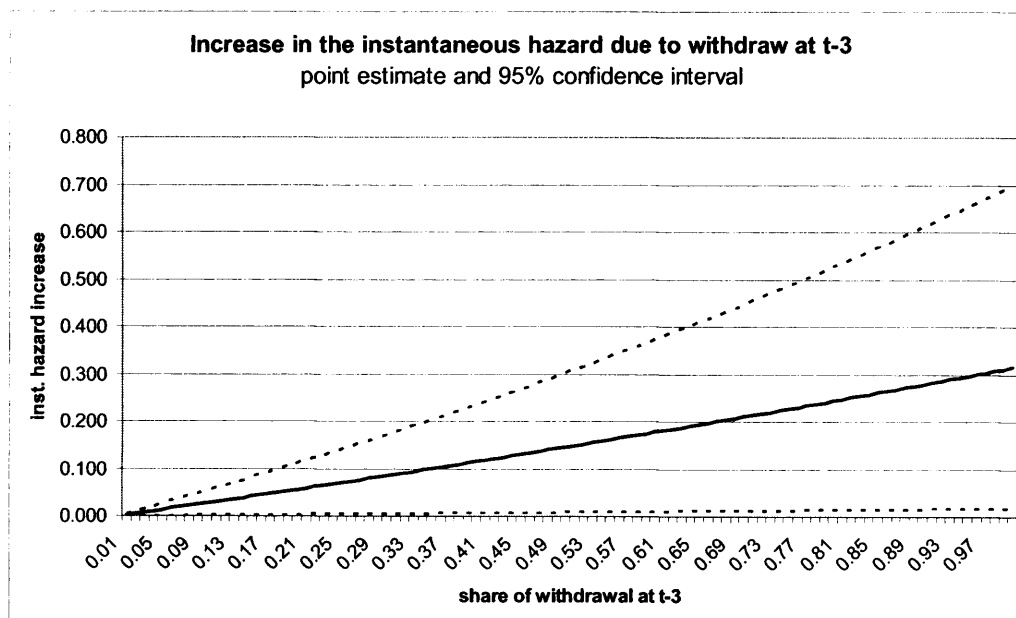


Figure 5: Odds ratios, bottom line specification

Chapter 3

Employment Protection Legislation and the Size of Firms*

Abstract

The existing literature ignores the fact that in most European countries the strictness of Employment Protection Legislation (EPL) varies across the firm size distribution. In Italy firms are obliged to rehire an unfairly dismissed worker only if they employ more than 15 employees. Theoretically, the work solves a baseline model of EPL with threshold effects, and shows that firms close to the threshold are characterized by an increase in inaction and by a reluctance to grow. Empirically, the work estimates transition probability matrices on firm level employment using a longitudinal data set based on Italian Social Security (INPS) records, and finds two results. First, firms close to the 15 employees threshold experience an increase in persistence of 1.5 percent with respect to a baseline statistical model. Second, firms with 15 employees are more likely to move backward than upward. Finally, we test the effect of a 1990 reform which tightened the regulation on individual dismissal only for small firms. The persistence of small firms relative to large firms increased significantly. Overall, these threshold effects are significant and robust, but quantitatively small.

*This is a joint work with Pietro Garibaldi and Andrea Borgarello. It has been published in *Il Giornale degli Economisti*, 1, 2004.

1 Introduction

Over the last decade, Employment Protection Legislation (EPL) has attracted a large interest among labour economists and policy makers. The accumulated empirical evidence and theoretical analysis has greatly improved the overall understanding of the effects of EPL on the aggregate labour markets. The main empirical regularities are as follows. EPL reduces unemployment inflows and outflows, but it has ambiguous effects on aggregate employment stocks. In addition, EPL has important effects on the composition of employment, since countries with stricter EPL are associated with higher youth unemployment and larger self employment. These empirical regularities, surveyed by the OECD (1999), are broadly in line with the existing theoretical models, who treat EPL as a tax on labour shedding (Bentolila - Bertola, 1990, and Bertola, 1999).

Most of the traditional empirical literature works with macroeconomic data, and analyses the effects of EPL on aggregate labour markets flows and stocks. More recently, different scholars have used individual and firm level data for analyzing the effects of various EPL reforms. Acemoglu - Angrist (2001) have studied the effects of the Employment Disability Act, while Kugler (1999) has studied the effect of the EPL reform in Colombia. Further, Blanchard - Portugal (2001) have compared the labour market dynamics of U.S. and Portugal, and have found that despite their similar unemployment level, Portugal experiences much lower worker flows in and out of unemployment.

Despite the growing number of studies on EPL based on individual and firm level data, little or no attention has been devoted to the effect of EPL on the size distribution of firms, and on the behaviour of firms of different size. This is surprising, since in most European countries the existing legislation varies across firms of different size. The case of Italy stands out in this respect. In the existing legislation firms with more than 15 employees are obliged to rehire the dismissed employee when a judge rules the dismissal unfair. Small firms, by contrast, are only obliged to compensate the dismissed worker with a monetary transfer. In the Italian context, the 15 employees threshold is certainly relevant, since it leaves below about 30% of the employees and more than 80% of private firms. Indeed, small firms are quantitatively more important in Italy relatively to other OECD economies, as was shown by Bartelsman et al. (2003).

This chapter studies theoretically and empirically the effects of such EPL thresholds on employment dynamics. Theoretically, we solve a baseline model of EPL with threshold effects, and show that firms close to the threshold are characterized by an increase in inaction and by a reluctance to grow. This implies that average employment is affected

by size contingent EPL under conditions that would make it independent of uniform EPL provisions. This result is new, and it is due to the flexibility induced by size contingent EPL. Specifically, profit maximizing firms below the threshold face a trade-off between dynamic efficiency (the possibility of adjusting their size in response to future shocks) and average long-run size (the possibility of growing beyond the threshold). Our model shows that there exists a well defined mass of firms that solves such trade-off in favour of dynamic efficiency, and optimally chooses not to cross the threshold. Such trade-off does not arise in a world with uniform EPL provision, when firms are forced to solve the same trade-off in terms of a larger average long-run size. As a result, average employment is larger under uniform EPL than under size contingent EPL, even though profits and allocative efficiency are larger in the latter scenario.

Empirically, the work uses firm level data drawn from the Italian Social Security (INPS) Archives, and studies whether the existence of the 15 employees threshold modifies employment dynamics, and whether firms' inaction vis-à-vis employment increases close to the 15 employees threshold, as our theoretical model predicts. We find a significant, albeit quantitatively small, threshold effect. Specifically, we estimate transition probability matrices for a sample of some 30,000 Italian firms between 1987 and 1996. While the probability of inaction decreases markedly with firm size, it experiences a significant spike in the region below the threshold. Indeed, we find that firms employing 14 and 15 employees have a probability of inaction that is 1.5 percent higher than what different non linear statistical models would predict. Similarly, the difference between the probability of moving down and moving up by one position falls slightly with firm size, but it features a 1.6 percent spike around the 15 employees threshold. Finally, we estimate the effect of a 1990 reform which tightened EPL on small firms. Although this is not a natural experiment, because also the legislation referred to part of the control group changed, we find that the persistence of small firms relatively to large firms increased significantly after 1990.

The chapter proceeds as follows. Section 2 looks at the existing legislation, and describes the labour market institutions that apply only to firms of certain size, with particular emphasis on the EPL differential. Section 3 presents a toy model of EPL with threshold effects, and derives two empirical predictions. Section 4 describes the dataset used in the empirical analysis, and presents the methodological approach pursued. Section 5 presents the results in terms of firm level persistence and firm level probability of increasing and decreasing employment. Section 6 looks at the 1990 EPL reform which

increased firing costs for small firms relatively to large firms. Section 7 summarizes and concludes.

2 Employment Protection Legislation and Firm Size: The Italian Institutional Setting.

Within the Italian Institutional settings, there are five types of regulations that depend on firm's size¹. The institutional areas involved are the following: employment protection legislation, mandatory quotas on hiring, firm level rights to organize union related institutions, firms safety standards and collective dismissals rules. The existence of rules and constraints to be applied only to larger firms can potentially affect firms' size, can push firms to enter in markets in which the optimal long-run position is below the thresholds, and can affect the geographical location of firms and establishments.

The most important institutional constraint is linked to the individual dismissal procedures, as legislated in the Article 18 of the labour code. Within the Italian institutional setting, individual dismissals must be justified by a just cause rule, and workers have the right to appeal firm initiated dismissals². Whenever a judge rules the dismissal unfair, workers are entitled to a compensation that depends crucially on firm size. Firms employing less than 15 employees must compensate the unlawfully dismissed workers and pay a severance payment that varies between a minimum of 2.5 and a maximum of 6 months (*tutela obbligatoria*). Conversely, firms employing more than 15 workers must compensate the worker for the foregone wages from the date of the dismissal, and are also obliged to rehire the worker (Article 18, *tutela reale*)³. If the worker does not exercise the option to be reinstated, he or she can receive a severance payment of 15 months. Even though the large majority of dismissals does not go to court and is settled through pre-trial agreements, the threat of reinstatement is always present, and it is the relevant constraint whose effects we analyze.

It is also important to stress how the labour code computes the 15 employees threshold relevant for Article 18, *tutela reale*. First of all, the 15 employees refer to establishments rather than to firms, and to different establishments as long as they are located within

¹See also Baffi – Baffi (1999).

²Here we refer to objective just cause (economic reasons). Subjective just causes (as misconduct) are linked to worker behaviour, and are not the object of our theoretical analysis. In practice, however, it is only a judge ruling that defines a specific situation as subjective or objective.

³Notice that this case is different from discriminatory dismissals (based on race, gender, political opinions for example), where reinstatement is automatic and independent of firm size.

the same city. In addition, the 15 employees refer to the date in which the firing was intimated, which can be ahead of the actual separation date. Further, apprentices and temporary workers below nine months should not be computed. Conversely, part-time workers should be included in proportion on to their actual time, and all other temporary contracts should be counted. Finally, any form of employment which does not classify as dependent employment (interim workers, full-time and part-time consultants) should not be included in the labour code based definition of employment. These measurement issues are relevant in the empirical strategy discussed in Section 4.

Notice that there are other relevant constraints that apply above a given threshold⁴. Firms employing more than 10 workers, are obliged to hire disadvantaged workers, which refer to officially registered long term unemployed. Further, as of 1999, firms employing more than 15 workers must employ disabled workers.⁵ Further, rules of the labour code linked to union related activity applies only to firms employing more than 15 employees. Such norms entitle workers to establish a firm level institution (*Rappresentanze Sindacali Aziendali*) that has the right to call general meetings, establish referendum, and post union related posters within the establishments. Also, firms with more than 15 employees have the right to vote for a worker representative for safety related issues. Finally, since 1991 collective dismissals procedures are in place above the 15 employees threshold. These procedures require a credible risk of bankruptcy and require the dismissal of at least 5 employees⁶; they imply (long) negotiations with the union but do not generate further firing costs (or reinstatement risks) when implemented.

3 A Toy Model of EPL with Threshold Effects

EPL is traditionally modelled as a firing tax on labour shedding, and the original theoretical framework is the dynamic labour demand under uncertainty. Bentolila - Bertola (1990) characterize the optimal employment strategy of a monopolistic firm subject to idiosyncratic shocks and firing costs, holding wages fixed. Most of this literature takes EPL as given, and looks at the employment effect of different degrees of job security provisions. A very simple exposition of the Bertola-Bentolila model is the one of Schivardi

⁴These thresholds are computed according to rules that are somehow different from those relevant for Article 18 detailed above.

⁵As we discuss in section 4, our dataset refers to the period 1987-1996, so those mandatory rules on disabled workers are not binding in our time period.

⁶Firms undergoing temporary crisis may access supplementation schemes instead of firing part of their workforce; wages are temporarily paid by supplementation funds and the employment spell is not broken.

(2000). To the best of our knowledge, there are no explicit models that derive employment predictions when EPL is binding only for firms larger than a given size. In this section, we build on the work of Schivardi (2000) and Bertola (1999), and introduce threshold effects in a toy model of labour demand. This section proceeds as follows. First, we solve for the efficient allocation, next we show the properties of the model with an extreme form of EPL. Finally, we introduce threshold effects, and derive the main empirical predictions on firm level dynamics.

3.1 The Set-Up of the Model

We assume that there is a continuum of firms of mass 1, and that wages are exogenously fixed and equal to w . Each firm hires only labour and produces and sells a homogenous output with a convex production function $y = f(\alpha, \epsilon, l)$, where α is a stochastic shifter of labor demand, l is the quantity of labour employed, and ϵ is a fixed-firm-specific parameter heterogeneous across firms. The shifter parameter α is an index of business conditions at each firm. It can take two different values, $\alpha = \alpha_b$ in bad business conditions and $\alpha = \epsilon$ (with $\epsilon > \alpha_b$) in good business conditions. Firms are subject to an i.i.d. idiosyncratic shock and in each period there is a probability p that business conditions are good and a probability $(1 - p)$ that business conditions are bad. The parameter ϵ differs across firms, and is distributed according to the distribution function $F(x) = \text{Prob}(\epsilon \leq x)$, where F is continuous with no point mass and defined over the support $\Omega \in [\alpha_b, \alpha_g^{max}]$. This implies that firms are identical when business conditions are idiosyncratically bad, while differ in their profit schedule when business conditions are good. Since firms differ only for their idiosyncratic parameter ϵ , in what follow we index firms simply by ϵ . Firms are dislocated in islands, there is no entry or exit and profits exist in good and bad times as long as $\alpha_b > w$. In this respect, the analysis is left at the partial equilibrium level. The model is stationary and we do not need to explicitly keep track of the time index t , even when we introduce EPL. If the production function is quadratic in labour, firm's profit for a type- ϵ firm can be written as

$$\Pi(\alpha, \epsilon, l) = \alpha l - \frac{1}{2}l^2 - wl$$

where $\alpha = \alpha_b$ with probability p ; $\alpha = \epsilon$ with probability $(1 - p)$.

3.2 The Efficient Allocation

Assume now that each type- ϵ firm can choose the optimal employment level after observing the realization of the shock α , and assume that hiring and firing can take place at no cost. Firm optimal employment behaviour is obtained simply by maximizing profits in each period, so that the firm continuously sets the marginal product equal to the wage, or $l^*(\epsilon) = l_b = a_b - w$ if $\alpha = a_b$; $l^*(\epsilon) = l_g(\epsilon) = \epsilon - w$ if $\alpha = \epsilon$

This implies that a type- ϵ firm, in steady state, spends a fraction p of its time in bad business conditions with $l^* = l_b$ and a fraction $(1 - p)$ in good business conditions with $l^* = l_g(\epsilon)$, where the star symbol refers to the efficient allocation. In this situation firms shed all labour in excess of l_b when business conditions turn bad and hire up to $l_g(\epsilon) = \epsilon - w$ when business conditions turn good. Expected profits for a type- ϵ firm are

$$E\Pi^*(\epsilon) = \frac{p}{2}[a_b - w]^2 + \frac{(1-p)}{2}[\epsilon - w]^2 .$$

Profits are obviously increasing in ϵ .

3.3 The Rigid System

Assume now that EPL is so strict that firing is impossible. A type- ϵ firm will then choose a level of employment that maximizes average profits, and will keep its employment constant at all time. In other words, a type- ϵ firm will choose a level of employment to maximize average expected profits

$$\Pi^R(\epsilon, l) = p(a_b l - \frac{1}{2}l^2 - wl) + (1-p)(\epsilon l - \frac{1}{2}l^2 - wl),$$

where $\Pi^R(\epsilon, l)$ are the profits for a type- ϵ firm in the rigid system. If we indicate with $l^R(\epsilon)$ the result of the maximization, its expression reads

$$l^R(\epsilon) = pa_b + (1-p)\epsilon - w .$$

Confronting the rigid and the efficient allocation, an important implication immediately follows.

Result 1. *Average employment for a type- ϵ firm in the efficient and in the rigid allocation is identical.*

The result is obtained by simple inspection of $l^R(\epsilon)$, which can be written as $l^R(\epsilon) = p(a_b - w) + (1-p)(\epsilon - w)$. But then $l^R(\epsilon)$ is the average level of employment of a type- ϵ firm in the efficient allocation. Further, profits are larger in the efficient allocation, as

long as p is different from 0 and 1. To obtain the latter result simply observe that profits in the rigid system are

$$\Pi^R(\epsilon) = \frac{1}{2}[pa_b + (1-p)\epsilon - w]^2$$

which is an expression that is always lower than $E\Pi^*(\epsilon)$ as long as p is strictly positive and less than one. In addition, one can also observe that firm employment in the rigid system is less volatile than in the efficient allocation, since firms never hire and fire. These results are the standard implications of the EPL literature with fixed wages, and are just reported for introducing threshold effects, on which we turn next⁷.

3.4 The Role of Threshold Effects

Assume now that the rigid regime is enforced only for employment level larger than l^{thr} , where l^{thr} is an exogenous threshold specified by the legislation. The only restriction we impose is that $l^{thr} > a_b - w$, otherwise the problem is not even interesting. In this setting, once a firm grows beyond the employment level l^{thr} firing becomes impossible, while it can take place at no cost for employment levels less or equal than l^{thr} . With threshold effects, some type- ϵ firms have the option to permanently fluctuate in the flexible fringe of the firm size distribution, or in the interval $l \in [l_b, l^{thr}]$, where l_b is the efficient level of employment when business conditions are bad. We label these type of firms as “constrained firms”, and their formal definition follows.

Definition: CONSTRAINED FIRM: A type- ϵ firm with efficient employment allocation in good business conditions larger than the threshold ($l_g(\epsilon) > l^{thr}$) is constrained when it employs $l = l_b$ in bad business conditions and $l = l^{thr}$ in good business conditions.

Thus, a constrained firm never passes the threshold, sheds labour up to l_b when business conditions turn bad, and hire up to l^{thr} when business conditions are good, and features average employment level $l^{SC} = pl_b + (1-p)l^{thr}$. A constrained firm follows a stay-small-policy, since in good times is reluctant to grow beyond the threshold. Expected profits of a constrained firm are

$$\Pi^{SC}(\epsilon, l) = p(a_b l - \frac{1}{2}l^2 - wl) + (1-p)(\epsilon l^{thr} - \frac{1}{2}l^{thr2} - wl^{thr})$$

while its employment behaviour is $l^{SC}(\epsilon) = l_b = a_b - w$ if $\alpha = a_b$; $l^{SC}(\epsilon) = l_g = l^{thr}$ if

⁷While the model holds wage fixed, the results of modelling EPL as a tax on labour shedding do not change in models with endogenous wage as long as EPL is modelled as a tax. See Garibaldi - Violante (2002).

$\alpha = \epsilon$; so that the average level of profits is

$$E\Pi^{SC}(\epsilon) = \frac{1}{2}p(a_b - w)^2 + (1 - p)(\epsilon l^{thr} - \frac{1}{2}l^{thr2} - w l^{thr})$$

With threshold effects, some firms have to choose between a rigid allocation and a stay small policy. In the former case they have an employment base larger than the threshold, they permanently employ $l^R(\epsilon)$, and never fire. In the latter case, they permanently fluctuate inside the flexible fringe of the size distribution.

To complete our description, we need to characterize the conditions that insure that constrained firms exist in equilibrium. In general, a type- ϵ firm will be constrained and will follow a stay small policy as long as its average profits are higher than the average profits from the rigid system, or when $E\Pi^{SC}(\epsilon) > E\Pi^R(\epsilon)$. Among other things, this condition clearly depends on the specific value of the idiosyncratic parameter ϵ , as we show in our next result.

Result 2. *Firms in the interval $\epsilon \in [\epsilon^*, \epsilon^{**}]$ are constrained, where $\epsilon^* = l^{thr} + w$ and ϵ^{**} is a positive number larger than ϵ^* .*

In light of this result, the firm size distribution is partitioned in three intervals. Firms with idiosyncratic component ϵ lower than ϵ^* are totally efficient and do not interact in any way with the threshold (their employment level in good times is lower than the threshold). Firms with idiosyncratic component in the interval $[\epsilon^*, \epsilon^{**}]$ are constrained, and in good times bunch with employment $l^{thr} = \epsilon^* - w$. Finally for idiosyncratic values of ϵ larger than ϵ^{**} , firms are rigid and hire $l^R(\epsilon)$.

To prove this result one needs simply to introduce the function $z(\epsilon) = \Pi^{SC}(\epsilon) - \Pi^R(\epsilon)$ whose expression reads

$$z(\epsilon) = \frac{p}{2}(a_b - w)^2 + (1 - p)(\epsilon l^{thr} - \frac{1}{2}l^{thr2} - w l^{thr}) - \frac{1}{2}[p a_b + (1 - p)\epsilon - w]^2$$

First note that the threshold is irrelevant for those firms for which $l_g(\epsilon) < l^{thr}$, which is a condition that is satisfied as long as $\epsilon < \epsilon^*$, with $\epsilon^* = l^{thr} + w$. Type- ϵ firms with idiosyncratic component below ϵ^* are totally efficient and do not interact in any way with the threshold. Second, note that $z(\epsilon^*) = \frac{1}{2}p(1 - p)[a_b - w - l^{thr}]^2 > 0$ and that $z'(\epsilon^*) > 0$ so that firms with $\epsilon > \epsilon^*$ are certainly constrained. To find the upper support of the interval $[\epsilon^*, \epsilon^{**}]$ one needs to solve the quadratic equation $z(\epsilon) = 0$ whose largest root reads⁸

⁸Note that in the quadratic equation $z(\epsilon) = 0$ there are two positive roots, but the smallest one is

$$\epsilon^{**} = \frac{\epsilon^* - \sqrt{pa_b}}{1 - \sqrt{p}}$$

It is immediate to see that $\epsilon^{**} > \epsilon^*$ strictly, so that all firms in the interval $[\epsilon^*, \epsilon^{**}]$ are constrained. Conversely, for $\epsilon > \epsilon^{**}$ $z(\epsilon) < 0$ and firms choose the rigid system.

An important result easily follows.

Result 3. *A type- ϵ firm that is constrained has average employment level that is lower than the average employment level in the efficient allocation and in the fully rigid system.*

Proof. From Result 2 and from the definition of constrained firms it follows immediately that $l_g(\epsilon) > l^{thr}$ for all $\epsilon \in (\epsilon^*, \epsilon^{**}]$ where $l_g(\epsilon) = \epsilon - w$. Since $l^R(\epsilon) - l^{SC}(\epsilon) = (1 - p)[\epsilon - \epsilon^*]$, it is obvious that all constrained firms with $\epsilon \in (\epsilon^*, \epsilon^{**}]$ feature average employment that is lower than the average employment in the rigid system.

This result is important, since it shows that one of the standard predictions of traditional EPL models, namely result 1, does no longer hold when constrained firms exist. Result 3 is further summarized by looking at Figure 1, where we report the optimal employment level for a type- ϵ firm under three regimes: the efficient allocation, the rigid system, and a stay small policy. Points A and B in the figure refer to the employment level under the efficient allocation, when the firm switches its employment level between l_b and $l_g(\epsilon)$. Point C refers to the employment level under the rigid system, and $l^R(\epsilon)$ is the amount of labour that the firm permanently employs, independently of business conditions. When a firm is constrained, its employment level shifts between point A in bad times and point D in good times. Clearly, the average between A and D is lower than the employment level associated to point C.

3.5 Threshold Effects and Reality: Two Empirical Predictions

The main results of our theoretical model, namely that average employment is affected by size contingent EPL, under conditions that would make it independent of uniform EPL provisions is new in the literature, and would hold under more general models. The general intuition is as follows. A size contingent EPL introduces more flexibility in the firms' dynamic optimization problem, and such flexibility is going to be exploited in equilibrium by profit-maximizing firms. Specifically, firms close to the threshold face a trade-off between dynamic efficiency (the possibility of adjusting their size in response

lower than ϵ^* and is economically meaningless, since for values of $\epsilon < \epsilon^*$ firms are totally efficient and can not be constrained.

to future shocks) and average long-run size, which in our model is proxied by the idiosyncratic parameter ϵ . Our main result shows that there exists a well defined mass of firms that solve such trade-off in favour of dynamic efficiency, at the expense of a smaller average long run size. Obviously, such trade-off does not arise in the case of a uniform EPL provision, and all firms are obliged to solve the trade-off in favour of the larger average long-run size option.

Real life firms are obviously much more complicated than the firms described in the toy model, since they differ in many dimensions beyond the single parameter ϵ . Moreover, it would be quite difficult to find an empirical counterpart to the idiosyncratic parameter ϵ , which in reality may represent technological, managerial as well as demand factors. One may certainly try to write down a model that keeps track of all such dimensions, and properly calibrate the size distribution of firms. But that is neither the purpose of our model, nor the purpose of the work, which is mainly empirical.

In what follows, we restrict the attention to the employment level around the threshold, where our model suggests that three type of firms coexist: i) firms whose long run position in good times is below the threshold, and have no interest in growing beyond the threshold; ii) firms that are growing beyond the threshold toward the no flexible regime and iii) constrained firms that are reluctant to hire. Beyond the threshold, flexible and constrained firms disappear. Since constrained firms are likely to be stuck before the threshold, a key prediction of our analysis is the following persistence prediction, where the threshold refers to the 15 employees threshold of the Italian labour code.

Persistence Prediction: *firm level persistence in employment dynamics increases right below the threshold.*

While the persistence prediction is our key prediction, at least another prediction can be derived. At the threshold, constrained firms face a probability p of reducing their employment base, while they have no chance of increasing their size. There is a key asymmetric behaviour. To derive a clear prediction in this respect, assume that each firm is characterized by more than two levels of the shifter parameter ϵ , so that firms have more than two possible employment states. Then, it is clear that constrained firms at the threshold will not react to small shocks that would increase their employment level, while they certainly react to negative shocks. This leads to our second prediction.

Asymmetric Prediction: *Firms at the threshold should respond asymmetrically to positive and negative employment shocks, and react more markedly to negative than positive shocks.*

While our focus is mainly on the effects of the EPL threshold, we should recall that in reality there are other institutions that may affect employment dynamics around the 15 employees. The requirement to hire specific categories clearly imply an increase in average labour costs. The same effect should be played by the presence of union related institutions, since beyond the threshold a subset of the workers can spend paid time in off production activities. In both cases, the increase in labour costs should reduce labour demand, and slow down employment growth.

Finally, we should recall that our model and our predictions are only relevant when EPL takes the form of a firing tax. Lazear (1990) has shown that a pure severance payment with flexible wages has no allocative impact on the labour market. Nevertheless, the reinstatement clause of the article 18 of the labour code is more akin to a tax than to a transfer, so that the predictions spelled out above appear appropriate.

4 Empirical Analysis

4.1 Data and Empirical Strategy

The empirical goal of this work is to study employment dynamics of firms close to the 15 employees threshold, and to check whether such behaviour is consistent with the theoretical predictions outlined above. This empirical exercise can be done successfully only using longitudinal microdata on employment.

The existing literature, albeit scant, does not find any significant evidence of threshold effects. Anastasia (1999) studies the firm size distribution in the Italian economy and in Veneto (a large Italian region), and does not find any significant bunching of firms close to the threshold. Tattara (1999) focuses on two provinces of the Veneto region, and does not find any significant threshold effect on accession and separation rates of workers, as well as on the probability of growing/shrinking of firms when this implies crossing the 15 employees threshold. Istat (2002), in its annual report, looks at the firm size distribution, and finds a very small bunching of firms at 15 employees. With respect to those studies, the present work emphasizes the effects of EPL on firm inaction and asymmetric behaviour around the threshold, two dimensions that have not been analysed yet.

4.1.1 Data Description

Our data are drawn from the Italian Social Security Administration (INPS) archive of firms⁹. The archive includes the population of private Italian firms that have at least one employee. It is a rolling panel and, for each firm, it records the monthly total number of employees over 6 years. From this archive we extract a series of cross-sections of firms for the period 1987-1996 as follows; in a given year t , we select all firms that in May t employ a worker born on the 10th of March, June, September or December. For sampled firms at year t , we keep information only on the employment stock in December of year t and $t - 1$ ¹⁰. This generates in each year a random sample of firms, representative of the population; the sampling probability being $1/90$ ¹¹ times the size of the firm in May t . Note that a firm sampled in year t will be sampled in year $t + 1$ if and only if it still employs a workers born in the specified dates.

We drop firms above 30 employees, so that the 15 employees threshold lies perfectly in the middle of our size interval. Every year, the sample includes some 900 firms of 14, 15 or 16 employees. Controlling for size classes (1-5, 6-9, 10-19 employees), our sample matches very closely the distribution of firms in the population by other dimensions, as published by the INPS Observatory¹². For example, the distribution of firms by branch in our sample and in the population is almost exactly identical. We obtain the same result by geographical area (north-west, north-east, centre, south). This is an indication in line with a random draw from the relevant population.

As we mentioned above, the sampling probability is not constant, but it is proportional to the size of the firm. This can be seen by comparing the distribution of firms in our sample to that of the INPS Observatory. While our sample consists of some 5 percent of the total firms in the Italian economy, it clearly under-samples very small firms. In table 1 we report the ratio between our firms and the number of firms in the INPS Observatory. Such ratio increases from some 2% for the 1-5 employees category to 12% for firms in the 10-19 employees categories. The over weight in the sample given by

⁹We exclude from the records agricultural and public sector firms whose social security records are administered by INPS.

¹⁰In terms of age, our sample requires firms to have at least one year of life, while it is perfectly possible that firms currently active leave the market in the following year.

¹¹ $1/90$ because we select 4 days of birth out of 365.

¹²The yearly report published by the Social Security Administration; it is the official source of statistics on the population of firms covered by INPS. Size classes in the INPS Observatory are those reported, followed by 20-49, 50-99 employees, etc. Unfortunately neither 15 nor 30 employees define a class.

large firms can be seen also in figure 2, where we report the proportional number of firms between 10 and 20 employees in our sample and in the ASIA archive, the ISTAT register of all active Italian firms. Notice that the sampling ratio increases smoothly over size.

Table 1: Number of Firms by Size in the sample and sampling ratio with respect to INPS Observatory

year	1987	1988	1989	1990	1991	1992	1993	1994	1995
Ratio Sample-Observatory	5.04	5.05	5.04	5.02	5.06	5.06	6.01	5.09	6.00
By Firm Size:									
Up to 5 employees									
Ratio Sample -Observatory	1.09	1.09	1.09	1.09	1.03	1.07	2.03	2.02	2.02
Number of Firms	12720	12978	12655	8793	11982	14561	14424	14280	12091
From 6 to 9 employees									
Ratio Sample -Observatory	6.05	6.05	6.05	6.04	4.06	5.08	7.04	7.02	7.02
Number of Firms	6578	6613	6565	4659	5932	7392	7268	7206	6105
From 10 to 19 employees									
Ratio Sample -Observatory	11.08	11.09	11.06	11.03	8.08	10.07	13.03	12.09	12.09
Number of Firms	9743	10090	9927	7361	9004	10914	10405	10745	8995

4.1.2 Empirical Methodology

We proceed in two steps. The first step uses data at the firm level to estimate transition probability matrices, while the second step uses the estimated transition probabilities. In the first step we use the cross sections of firms to estimate a set of transition matrices for employment size. As we said above, we concentrate on firms with employment in the interval $[i = 1, I = 30]$. We let $n_{i,t}$ be the number of firms that employ i workers at time t and $n_{ij,t+1}$ the number of firms of size i at t that employ j workers in $t + 1$. We then estimate the following set of transition probabilities

$$p_{ij,t} = \frac{n_{ij,t+1}}{n_{i,t}} \quad i = 1, \dots, I \quad (1)$$

where $p_{ij,t}$ represents the maximum likelihood estimator of the underlying transition probability. When $i = j$, the transition probability refers to the persistence probability, or the probability of employment inaction. Since there are $T + 1$ years in the sample (i.e. 10 years from 1987 to 1996), and I size categories, the previous estimate generates a dataset of $(I * I * T)$ observations¹³, in which the representative observation is just $p_{ij,t}$ or the estimated probability that a firm of size i switches to size j in $t + 1$ ¹⁴.

¹³I.e. $30 * 30 * 9 = 8100$ observations.

¹⁴Notice again that our transition probabilities refer only to continuing firms. In section 4.3, we look at entry and exit behaviour, and we show that there is no evidence of irregular exit patterns around the

The second step uses transition matrices to test whether employment dynamics around the 15 employees threshold is consistent with the theoretical predictions. We perform tests on employment persistence and on asymmetric behaviour. The spirit of the tests we perform is as follow. First, we fit a parametric relationship linking the transition probabilities to firm size. Second, we test whether the transition probability around the threshold is an outlier vis-à-vis the estimated parametric relationship.

Before specifying the details of our methodology, two preambles are needed. First, an alternative empirical strategy would be to estimate an employment growth equation at the firm level. In the literature, several Gibrat law type of models have been proposed, although to the best of our knowledge none of them addresses the threshold effect we are focussing on.¹⁵ We choose the transition matrix approach for three reasons: its interpretation is in line with our theoretical predictions; its econometric approach is relatively simple; it fits well with the structure of our dataset (repeated cross sections). Second, our relatively short-time dimension does not allow us to study the cyclical behaviour of firms. Further, our data set does not allow us to disaggregate along other dimensions the transition matrices we estimate, since such disaggregation would significantly decrease the number of firms in each cell of the transition matrices. The only breakdown we can do is to split the sample in manufacturing and service sector. We next turn to the specification of the test on the persistence and on the asymmetric behaviour.

4.1.3 Persistence Test

If we indicate with $p_{ii,t}$ the estimate of the persistence probability for firms of size i between time t and $t + 1$, our first step requires estimating the following baseline regression

$$p_{ii,t} = \gamma_t + \alpha f(\text{size}_i) + u_{it} \quad (2)$$

where $f(\text{size}_i)$ is a (possibly non-linear) function of size_i , and γ_t is a time effect. Let δ_j be a dummy of window m , or a dummy variable that takes the value 1 if size belongs to the interval $[j - m, j + m]$, $m = 0, 1$. We then estimate the following regression

$$p_{ii,t} = \gamma_t + \alpha f(\text{size}_i) + \delta_j + u_{it} \quad (3)$$

for various values of δ_j . The persistence test is equivalent to the following test

threshold.

¹⁵See Sutton (1997) and references therein for a survey of Gibrat's law literature.

$$H_0 : \delta_j = 0 \quad H_1 : \delta_j > 0; j = 13, 14, 15 \quad (4)$$

which is simply a test that looks for an increase in $p_{ii,t}$ around the 15 employees threshold. This type of test needs to be carried out with different baseline statistical models, and several robustness tests in terms of the size of the dummy.

To estimate this model we use a subset of the $I * I * T$ matrix; we select only the elements in the main diagonal, and obtain an $I * T$ matrix. Notice also that we include only one size dummy in each estimated model, i.e. we estimate the same model several times including size dummies in turn:

$$p_{ii,t} = \gamma_t + \alpha f(\text{size}_i) + \delta_{14} + u_{it} \quad (5)$$

$$p_{ii,t} = \gamma_t + \alpha f(\text{size}_i) + \delta_{15} + u_{it} \quad (6)$$

$$p_{ii,t} = \gamma_t + \alpha f(\text{size}_i) + \delta_{16} + u_{it} \quad (7)$$

and so on.

While the previous test can highlight the existence of outliers close to the threshold, it ultimately relies on the parametric estimate of a statistical model which is not directly derived from the underlying economic analysis. In a more non parametric fashion, one can perform a bootstrapping exercise for estimating the standard deviations of the probabilities in the original transition matrices. More specifically, we bootstrap x samples of size n , where n is the number of firms used to estimate the transition probability $p_{ii,t}$. With the x samples in hand, we estimate the standard deviations, and construct confidence intervals of our persistence probability. We perform this exercise separately for each year.¹⁶

4.1.4 Asymmetric Behaviour Test

Let pdi_{ikt} be the difference between the probability of increasing and decreasing firm size by k positions, so that pdi_{ikt} reads $pdi_{ikt} = p_{[i,i+k],t} - p_{[i,i-k],t}$ where $p_{[i,i+k],t}$ and $p_{[i,i-k],t}$ are the probabilities of moving from size i to size $i+k$ and to $i-k$ respectively. In other words, k is the size of the off-diagonal jump that we consider. If $k = 1$, pdi_{ikt} is just the difference between the probabilities alongside the main diagonal. We focus on pdi_{i1t}

¹⁶Pooling transition matrices over time might generate correlation over time of $p_{ii,t}$ and bias the results of the bootstrap procedure.

and pdi_{i2t} only, as off diagonal observations decline sharply as k increases, worsening the precision of the maximum likelihood estimate of p_{ijt} . We then estimate a new baseline regression of the form

$$pdi_{ikt} = \gamma_t + \alpha_1 f(size_i) + u_{it} \quad (8)$$

If δ_j is the dummy defined in the previous paragraph, we run a set of regressions for different size dummies δ_j

$$pdi_{ikt} = \gamma_t + \alpha_1 f(size_i) + \delta_j + u_{it} \quad (9)$$

and we test the following assumption

$$H_0 : \delta_j = 0 \quad H_1 : \delta_j < 0; j = 15 \quad (10)$$

so that at the 15 employees threshold, the probability of moving down by one or two positions is larger than the probability of moving up by one or two positions. The same approach followed in estimating the inaction probability is applied here: we select $I * T$ differences in transition probabilities and we include size dummies in turn.

4.2 Strategic Behaviour and Measurement Issues

Before proceeding to discussing the results of our analysis, we should realize that firms may take strategic behaviours aimed at avoiding the implementation of the constraint imposed by the law. The dataset we exploit, although rich and detailed in many respects, does not allow us to control for various possibilities, which we discuss next. Specifically, we face three different problems.

First, suppose that a firm reaches 16 employees and wants to avoid the institutional constraint; it may divide the firm in two new distinct legal entities of less than 15 employees that do not satisfy the requirements imposed by the labour code. All we can do in this respect is using data on entry and exit of firms to check whether firm exit rate before the threshold is particularly high, suggesting that firms exit before the threshold, just for re-enter the labour market with two different entities below the threshold. A close check on this point shows that average entry and exit rates by firm size decline sharply

with firm size, but do not experience any clear pattern around the threshold¹⁷. Yet, our methodology and our results do not exclude the existence of other, more sophisticated, strategic actions aimed at eluding the EPL threshold. For example, a firm may split its activity in two different plants located in different cities when it reaches the 15 employees threshold. This is relevant since in the labour code the 15 employees must be hired within the same cities. However, this action is likely to be more costly than facing the EPL provisions.

Second, our dataset refers to firms, while the threshold rules specified in the labour code refer to establishments. Statistics on the distribution of multiplant firms by size are not easily available; in general small firms are likely to be single establishment firms¹⁸. Hence, these discrepancies should not affect our results.

Third, firms close to the threshold may start hiring categories of workers that are not counted in the labour code definition of employment (consultants, apprentices, interim workers). Further, firms close to the threshold may increase the incidence of irregular employment and hire the marginal workers as shadow employees. In these cases, the existence of an EPL threshold has modified firm's employment dynamics, but only a longitudinal dataset that follows the firm across all these dimensions would be able to identify these effects. Our main dataset records only total dependent employment (without distinguishing for apprentices and temporary workers). Nevertheless, we can use a different INPS dataset recording firms located in Turin only (an industrialized province in the North West) from 1990 to 1992 to perform a more detailed analysis. Such dataset distinguishes between apprentices, trainees, and part timers, and allows us to measure the 15 employees relevant for the labour code. We will present the results using this richer dataset (albeit geographically not representative) as a robustness check to the empirical analysis for the entire Italian economy.

4.3 Econometric Issues

Since our sampling strategy is proportional to firm size, the precision of the maximum likelihood estimates of the transition probabilities increases with firm size. One may argue that since in our regressions size (or a function of it) is a regressor, this may lead to biased OLS estimates. While this may be partially true, what we want to find is the threshold effect around the 15 employees, and our sample coverage increase smoothly

¹⁷In addition, the observation on entry and exit rates suggests that our focus to continuing firms should not give us obvious problems vis-à-vis the tests that we provide.

¹⁸See also Contini (2002).

with firm size, as clearly reported in Figure 2. Since we are mainly interested in the specific effect of moving from 15 to 16 employees, we do not expect any systematic error on this part of the estimates. In other words, we do not see why these problems should be correlated to the 15-employees threshold.

As it is clear from the discussion above, we estimate a transition matrix for each year in our sample, so that our panel data is obtained by pooling over time these different transition matrices. In light of our sampling construction, a similar pool of firms may contribute to the estimate of the cell (i, j) of the transition matrix in different years. Since this effect may introduce time correlation in our estimated probabilities, all the results we report refer to robust standard errors, that allow for correlation over time of probabilities referred to the same size class.

Finally, we use OLS when our dependent variable lies between zero and one. This forbids to use predicted values, as they may lie outside the acceptable range, but it does not bias our estimates.

5 Results

Table 2 reports the estimate of the time average transition matrix for our sample of firms. In the table we report the estimate of the average diagonal element, $p_{i,i}$ as well as the average value of the two off diagonal terms close to the main diagonal, namely $p_{i,i+1}$, $p_{i,i+2}$ and $p_{i,i-1}$, $p_{i,i-2}$. Few comments are in order. First, the persistence probability declines smoothly with firm size. Second, for the smallest firm size categories the probability density is concentrated around the main diagonal. Larger firms, conversely, have also a sizable probability of changing their employment size by several employees. Third, the probability of increasing and decreasing by one position declines also as a function of size. All this should not be surprising, since the relative employment weight of an extra employee (hired or fired) declines dramatically with firm size.

Figure 3 reports the implied long-run distribution obtained from the average transition matrix in table 2. Clearly, the long-run distribution features a smooth monotonic shape, in a way similar to the empirical distribution observed in the actual data. In particular, the empirical distribution does not feature a dramatic bunching of firms at size 15. Nevertheless, the figure suggests a “small turbulence” in the neighbourhood of the threshold, since the long-run number of firms at size 15 is as large as the number of firms at 14 employees. Remarkably, Istat (2002) in its annual report, observed a similar small “turbulence” relatively to the 1999 firm size distribution. This is particularly reassuring

Table 2: Average Transition Probability by Firm Size

Size	$p(i,i-2)$ (1)	$p(i,i-1)$ (2)	$p(i,i)$ (3)	$p(i,i+1)$ (4)	$p(i,i+2)$ (5)
1			72.2	17.3	5.1
2		14.8	57.3	17.5	5.3
3	4.4	17.3	48.4	17.3	6.1
4	5.5	18.1	42.7	17.7	6.9
5	6.2	18.3	37.8	17.9	7.8
6	6.6	17.9	34.6	17.9	8.0
7	7.5	17.0	31.2	18.0	8.6
8	7.8	16.9	29.0	17.4	9.2
9	8.3	17.0	27.9	16.5	9.3
10	8.2	16.7	26.5	16.6	8.8
11	8.0	16.5	24.3	16.8	9.7
12	8.5	16.3	23.2	16.3	9.7
13	8.7	15.9	22.8	15.7	10.3
14	9.3	15.3	22.1	16.9	9.0
15	9.3	15.7	21.9	14.1	9.0
16	9.3	15.3	19.1	14.4	9.6
17	9.3	14.8	18.4	14.5	9.4
18	9.5	14.0	18.2	13.8	9.2
19	9.1	14.0	17.8	12.9	9.3
20	8.3	13.1	18.7	12.8	9.4
21	9.6	13.6	17.0	12.2	8.8
22	9.4	12.6	16.0	13.1	8.8
23	8.3	12.8	16.0	12.0	8.6
24	8.2	13.6	15.0	11.6	9.0
25	8.4	12.2	15.0	12.0	8.7
26	9.0	11.6	14.3	11.5	8.2
27	9.6	11.6	13.7	11.3	8.9
28	9.2	11.7	13.0	12.1	34.2
29	7.9	12.7	12.8	45.7	
30 or more	0.7	0.9	95.4		

All Economy. Average 1987-1995. Percentages.
(1) Probability of Reducing Size by 2 Employees
(2) Probability of Reducing Size by 1 Employee
(3) Probability of Inaction
(4) Probability of Increasing Size by 1 Employee
(5) Probability of Increasing Size by 2 Employees

for our analysis, since we use a dataset of only 5 percent of firms, and our implied long run distribution ignores the role of entry and exit. We now move to our tests.

5.1 Persistence Effects

Figure 4 reports the estimates of the persistence probability by firm size over the period 1987-1995. The Figure reports the estimates for different years, and shows that the dispersion around the median (solid line) is relatively small for the spike at the 15 employees. This small deviation is quite important, since it increases the probability of rejecting the null of the persistence test. The Figure highlights two spikes in the persistence probability, one at 15 employees and one at 20 employees. The first one is the spike we are looking for, since it appears exactly at the EPL threshold imposed by

the Italian legislation. We have no obvious explanation for the 20 employees spike, as heaping at round numbers is not a likely event in administrative archives¹⁹.

After looking at Figure 4, an obvious model candidate for our test is a linear regression in which the persistence probability is regressed on the inverse of size so that

$$pp_{it} = \gamma_t + \frac{\alpha}{size_i} + u_{it} \quad (11)$$

where the regressor $1/size$ captures the underlying smooth relationship between the persistence probability and firm size. Figure 5 reports the solid line of Figure 4 and the benchmark statistic relation estimated according to the above equation: the persistence probability increases close to the threshold, and declines thereafter. Table 3 (in the column labelled Model 1) reports the estimated coefficients obtained by adding size-one dummies in turn to the model outlined above. The dummy is positive and strongly significant already at size 13, and reaches a peak at size 15, with a quantitative value that is about 2 percent. When the size of the dummy is 3 (column labelled Model 2), the results do not change.

Figure 6 reports the residuals from the baseline model, and shows that the residuals below the threshold are consistently positive. On the one hand, one can argue that such pattern is exactly what the theory would predict, since firms below the threshold anticipate the effect. On the other hand, one can argue that the baseline model is not properly estimated, and it is necessary to reduce the bunching of positive residuals below the threshold. In this respect, we also run a baseline regression in which the size variable enters both in linear and non linear terms, so that

$$p_{iit} = \gamma_t + \frac{\alpha}{size_i} + \beta size_i + u_{it} \quad (12)$$

Figure 7 shows that the residuals are now more randomly distributed around zero in the baseline model. The threshold effect is still present, as highlighted by the results of Table 3 (columns labelled Model 3 and 4). This holds with dummies both of size 1 and 3, and the results are quite similar if the analysis is restricted simply to the manufacturing or the service sector (results not reported). Model 3 highlights also a negative and strongly significant dummy at size 16 and 17 (lower persistence probability with respect

¹⁹However, while the spike at 15 employees is quite robust to different estimation methods, the one at 20 is not.

Table 3: Persistence Probability, all economy, 1987-1995

RHS Variables	Model 1	Model 2	Model 3	Model 4
Dummies: window size	Inverse of Size	Inverse of Size	Inverse of Size; Size	Inverse of Size; Size
Size Dummy	1	3	1	3
	Coefficient	Coefficient	Coefficient	Coefficient
6	-2.28**		0,4	
7	-1,29		-0.44***	
8	-0,42		-0.38**	
9	0.93**	0,77	0.49***	0,29
10	1.24***	-0,29	0.53**	0,08
11	0.52*	0.88**	-0.38*	-0,11
12	0.58**	0.86**	-0.4*	-0,23
13	1.22***	1.18***	0,25	0,14
14	1.40***	1.71***	0.47*	0.83**
15	2.02***	1.14*	1.19***	0,21
16	-0,31	0,44	-1.14***	-0,47
17	-0.51**	-0,28	-1.19***	-1.09***
18	0,07	-0,11	-0.41**	-0.69**
19	0,13	0,62	-0,17	0,28
20	1.42***	-0,06	1.31***	0.32*
21	0,19		0.29*	
22	-0.57*		-0.24*	
23	-0,33		0.24*	
24	-1.09***		-0.28**	
Constant (1)	10.3***	10.3***	16.6***	16.6***
Inverse of Size	160.36***	160.36***	106.05***	106.05***
Size		-	-0.32***	-0.32***
Time Dummies	Yes	Yes	Yes	Yes
Num. Observations	198	198	198	198
Rsq2 without dummy	0,92	0,92	0,94	0,94

***, ** and * refer to 90,95 and 99 percent significance
(1) below line: estimated coefficients when size dummies are excluded

to the baseline model) consistent with the theoretical predictions. Overall, the evidence provided suggests that, on average, firm persistence below the threshold increases by some 1.5 percent relatively to a baseline specification.

From the discussion above it comes out a clear change of regimes at the 15 employees threshold. We can infer from this that the threshold is quite well measured, or that measurement errors discussed above do not bias the econometric analysis of the estimated transition probabilities. In fact, if measurement errors were important, we would observe a grey area around the 15 employees threshold. The next section addresses this point further.

5.2 Persistence Effects: Further Robustness Tests

We perform two different robustness checks on the persistence effect. We try a non parametric approach and a correction of the measurement error in the number of employees that is relevant for the labour code.

First, it is possible to estimate non parametrically the standard deviations of the transition probabilities. This is done in Table 4, where we report the results of the bootstrapping of 500 samples from our original transition matrix 1994-1995, following the procedure described in the previous section. Notice that results are unchanged increasing the number of draws or selecting different years. The Table reports the standard deviation and the lower and upper tail of a 95 percent confidence interval around the mean, where the confidence intervals are obtained either by assuming a normal distribution of the mean of the various samples or using bootstrapped 5th and 95th percentiles corrected for the eventual bias in the bootstrapped mean. Table 4 shows that the 1994-1995 persistence probabilities are decreasing as firm size increases, with the exception of $p_{15,15}$ that is higher and of $p_{16,16}$ that is lower than the neighbouring $p_{i,i}$. Considering the bootstrapped 95% confidence interval we obtain that $p_{i,i}$ is non significantly different from $p_{i-1,i-1}$ and from $p_{i+1,i+1}$ for all firm sizes with the exception of the couple $p_{15,15}$ and $p_{16,16}$. These two are significantly different. We interpret the result as non parametric evidence of the spike at 15 employees of the persistence probability.

Table 4: Bootstrapped Confidence Intervals for Persistence Probability. Year 1994-1995

Size	Value	Std Dev.	(a)		(b)	
			Lower	Higher	Lower	Higher
12	23.91	1.21	21.53	26.29	21.66	26.48
13	22.39	1.04	20.13	24.65	20.43	24.85
14	21.9	1.12	19.54	24.26	19.66	24.26
15	23.44	1.29	20.92	25.96	20.99	25.81
16	18.41	1.28	15.89	20.93	16.02	21.24
17	21.79	1.39	19.06	24.53	19.32	24.6
18	17.83	1.36	15.16	20.49	15.31	20.44

(a) 95pct Confidence Interval assuming normality
(b) 95pct Confidence interval, bootstrapped bias corrected percentiles

Second, the definition of employment relevant for the labour code is slightly different from the total dependent employment observed in our main sample. The labour code computes 15 employees based on specific definitions, which exclude some employment categories such as apprentices and temporary workers below 9 months. This implies that our total employment variable overestimates the effective threshold, since it includes employment categories that are excluded from a labour code standpoint. While for the overall Italian sample we can not distinguish among different employment categories, and we have to rely on the total dependent employment variable, fortunately, we are able to reconstruct the definition of employment relevant for the labour code for the Turin province, a highly industrialized area in the North of the Country, for the period 1990-

1992. This is an important robustness test, since it gives us the possibility of estimating the difference between the two employment measures.

The results based on the Turin province are reported in Figure 8 and Figure 9. An immediate flavour of the importance of this problem can be given by the share of firms in which no difference between the two measures of size is recorded: 56%. Figure 8 plots the mean difference between total employment and labour code based employment by firm size (based on total employment) in 1992, conditional on the difference being positive. It also plots the share of firms for which the difference is null, i.e. the share of firms that do not use any of the contracts non included in the threshold definition. Firms up to 15 employees hired an average measure two of labour non computed for the labour code. Such measure increases to 2.5 as firms cross the 15 employees threshold. Furthermore, beyond the 15 employees threshold there is a clear drop in the share of firms that do not make use of contracts excluded from the labour code definition of employment. Summing up, there is some evidence in favour of a strategic use of contracts to stay below the threshold.

The next question to address is how the use of non-standard employment contracts affects the test based on the transition probabilities. Figure 9 plots the average persistence probability of employment using the labour code based definition of employment and the total employment. Two observations are relevant. First, the two estimates of the persistence probability are very much correlated, with both variables experiencing a sizable threshold effect. Second, the size of the threshold effect appears more pronounced for the labour code based employment variable. This is exactly what one would expect. Furthermore, p_{ii} drops at 16 employees using both the aggregate definition of size and the labour code based definition. The largest difference is observed at 12 and 13 employees, not right around the threshold. This is consistent with the fact that the analysis of the full dataset (discussed in the previous paragraph) shows a clear change of regimes at 15 employees. Hence, the characteristics we can highlight in Turin may be more general. Overall, these calculations suggests that the results obtained using total employment are similar to those obtained with a labour code based definition of employment, and that the behaviour of the two variables is highly correlated around the threshold.

5.3 Asymmetric Behaviour

To test the asymmetric prediction we need to work with the off diagonal terms of the transition matrix. Once we combine the probability of increasing and decreasing employ-

ment by one (two) positions, we obtain the variable pdi_{i1t} (pdi_{i2t}), which is simply the net probability of growing by one (two) positions. This allows us to test the existence of asymmetric effects around the 15 employees threshold.

Figure 10 reports the actual and estimated relationship with size of the net probability of growing by one position (pdi_{i1t}), and shows that there are several spikes along the size distribution. Among such spikes, two of them appear particularly large. The first is a positive spike at 14 employees, while the second is a negative spike at 15 employees. The latter spike is consistent with an asymmetric effect driven by the EPL threshold, since it suggests that firms employing 15 employees are more likely to move downward than upward. To further check on the asymmetric behaviour, Figure 11 reports the actual and estimated relationship with size of the net probability of growing by two positions (pdi_{i2t}). Indeed, the spikes observed in Figure 10 at 14 and 15 employees should be observed here respectively at 13 (positive), 14 and 15 employees (negative). As Figure 11 shows, this appears to be case, providing further evidence of the asymmetric effect around the threshold.

Table 5 presents the detailed analysis of the asymmetry test, focusing on the net probability of growing by one position. It reports the value of the size dummies for a simple linear model estimated for the whole economy, as well as for the manufacturing and service industries separately:

$$pdi_{i1t} = \gamma_t + \alpha size_i + u_{it} \quad (13)$$

Table 5 suggests that among the various outliers and spikes observed in Figure 10, the most important ones are the positive and negative spikes close to the threshold.²⁰ When the transition probability refers to the whole economy, a firm with 15 employees features a decrease in the net probability of moving up by 1.5 percentage points relatively to the simple linear model. This effect increases to 2 percent if the analysis is restricted to the manufacturing sector, while is unchanged when referred to the services. The effect is smaller but significantly negative at 16 employees as well. Overall, firms employing 15 employees are significantly more likely to move downward than upward, in a way consistent with asymmetric behaviour around the threshold.

²⁰In table 5 several negative and significant spikes can be noticed, reflecting the larger variability of pdi_{i1t} with respect to the persistence probability p_{iit} . We do not attach a meaning to them, a part from statistical variability. The only threshold we are aware of is at 15 employees, and in fact only around it we notice the meaningful pattern of a large positive spike followed by two large negative ones.

Table 5: Net Probability of Increasing One Position, 1987-1995

Sector	All Economy	Manufacturing	Service
RHS Variables	Size	Size	Size
Dummies: window size	1	1	1
Size Dummy	Coefficient	Coefficient	Coefficient
6	-0.2	0.26	0.33
7	0.86***	0.24	1.48***
8	0.52*	0.52*	0.18
9	-0.74***	-0.1	-1.29***
10	-0.22	0.05	0.22*
11	0.26	0.6**	-1.14***
12	0.07	-0.03	0.93***
13	-0.04	-0.05	0
14	1.83***	2.18***	0.71***
15	-1.58***	-2.10***	-1.58***
16	-0.69***	-0.77***	-1.77***
17	0.02	-0.41*	0.63**
18	0.129	-0.86***	1.59***
19	-0.615***	-0.12	-1.23***
20	0.04	0.1	0.07
21	-0.85***	-1.92***	-1.5***
22	1.22***	0.6*	2.92***
23	-0.18	0.01	-0.02
24	-1.71***	-1.59***	-1*
Constant (1)	10.35***	12.20***	8.36***
Size	-0.23***	-0.21***	-0.26***
Time Dummies	Yes	Yes	Yes
Num. Observations	198	198	198
Rsquared without dummy	0.32	0.38	0.14

*, **, and *** refer to 90, 95 and 99 percent significance
 (1) below line: estimated coefficients when size dummies are excluded

6 The 1990 Reform of Individual Dismissal

In 1990 the Italian legislation on dismissal rules applied to small firms changed drastically. Before 1990, workers dismissed from small firms could not appeal the employer initiated dismissal, i.e. before 1990 firms employing less than 15 employees were not obliged to obey to “just-cause” rule for their individual dismissals. Since 1990, small firms are required to justify their dismissal in accordance to the labour code, and whenever the dismissal is ruled unfair, they are obliged to compensate the worker with a severance payment. The legislated severance payment varies between 2.5 and 6 monthly wages, with the actual payment linked to the seniority of the dismissed worker. As far as individual dismissals are concerned, after 1990 the difference in the EPL between small and large firms was reduced to the article 18 of the labour code: while large firms are obliged to rehire unlawfully dismissed workers, small firms can compensate workers through a severance payment. This policy change is akin to a tightening in EPL on small firms relative to large firms.

Unfortunately, it is not possible to treat the 1990 reform as a natural experiment, since in 1991 a collective dismissal procedure was introduced for firms employing more than 15 employees. As of 1991, whenever a large firm faces a credible risk of bankruptcy, and needs to dismiss at least 5 employees, it has the option to undergo (long) negotiations with the unions, and in case the dismissal is authorized no further liabilities are imposed on the firm. The collective dismissal procedure reduces EPL on the part of large firms, and may also reduce the cost of crossing the threshold for small firms²¹.

In what follows we consider the behaviour of “large” firms with 20-25 employees and the behaviour of small firms with 6-10 workers. While we could obviously expand the range of small firms considered, we prefer to focus on the very small ones, since firms close to threshold are likely to be affected by the introduction of the collective dismissal procedure. We could also opt for “large” firms with 16-20 employees; however, to avoid spillover effects that might be present right above the threshold we prefer to be conservative.

Our focus is on the persistence behaviour. A stricter EPL on the part of small firms relative to large firms should translate in an increase in relative persistence and employment inaction. In Table 6 we look for a significant discontinuity in the persistence of small firms relative to large firms. The dependent variable in our regressions is p_{itk} or the probability of inaction for firms of size i between time t and $t + 1$ in sector k (manufacturing, services). With this definition of the dependent variable, the panel data we consider reaches 288 observations. The baseline regression model is the following

$$p_{itk} = \gamma_t + \alpha * small_{itk} + \beta * small91_{itk} + u_{ikt}$$

where γ_t is a time dummy, *small* is a dummy variable that takes the value of 1 if firm size is between 6 and 10, *small91* is a dummy variable identical to *small* but with positive values only if the year t is larger than 1990. The coefficient of interest is β , whose estimates are reported in Table 6 (model I), which refers to both the manufacturing and the service sector. The Table suggests that after 1990 the relative persistence of small firms increased significantly by 2.76 percentage points. As predicted, the 1990 reform increased relative inaction on the part of small firms. The models labelled column II and column III restrict the analysis to only one sector, reducing the size of the dataset from

²¹This is strictly true for manufacturing firms. Services were involved in the reform later and above a larger threshold with respect to manufacturing firms.

288 to 144 observations. The Table suggests that the effect in the service sector is larger than the aggregate one, reaching 3.45 percentage points. Conversely the coefficient in the manufacturing sector is smaller and not significant. Results by sector are consistent with the fact that services were involved in the collective dismissal reform later and above a larger threshold with respect to manufacturing firms. Overall, the results in Table 6 suggest that after 1990 the persistence of small firms relatively to large firms increased significantly by more than 2 percentage points.

Table 6: Persistence Before and After 1990. Regression

Model	(I)	(II)	(III)
Sector	All Economy	Service	Manufacturing
small firms	16.1***	18.36**	13.75***
small firms after 1991	2.76**	3.45**	2.07
time dummies	Yes	Yes	Yes
sector dummies	Yes	No	No
Rsquared	0.75	0.79	0.74
Observations	288	144	144

Dependent Variable: Persistence Probability
Small refers to size 6-10
Large refers to size 20-25

7 Conclusions

This work fits into the empirical EPL literature, and provides fresh evidence on the links between employment dynamics and asymmetric EPL across the firm size distribution. We focused on the Italian institutional setting, where firms with more than 15 employees are obliged to rehire the dismissed employee when a judge rules the dismissal unfair. Small firms, by contrast, are only obliged to compensate the dismissed worker with a monetary transfer.

We found a significant, albeit quantitatively small, threshold effect. Specifically, we found that firms employing 14 and 15 employees have a probability of inaction that is 1.5 percent higher than what different non linear statistical models would predict. Similarly, the difference between the probability of moving down and moving up by one position falls slightly with firm size, but it features a 1.6 percent spike around the 15 employees threshold. Both these results are consistent with the predictions of our toy model of EPL with threshold effects. Finally, we test the effect of the 1990 EPL reform which increased firing costs for small firms relatively to large firms. We find that relative inaction on the

part of small firms increased significantly.

Our general conclusion - significant but small threshold effects- is also consistent with an existing study based on a different country and a different piece of legislation. Specifically, Wagner et al. (2001) studied the threshold effects of the disability law in Germany, a piece of legislation that forces firms larger than 15 employees to hire disable workers. Using a technique similar to the one presented here, Wagner et al. find that the German disability law induces quantitatively small effects on firm level employment dynamics.

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Figures

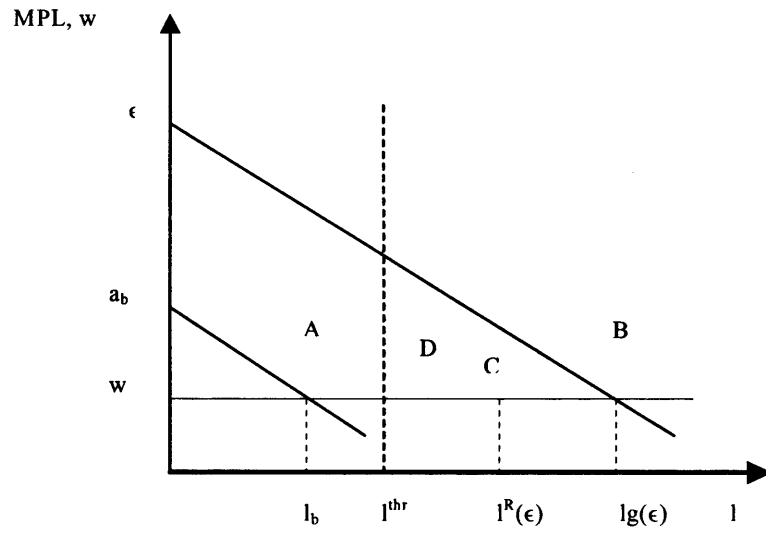


Figure 1: Employment Level in Efficient, Rigid and Stay Small Regime of a type- ϵ firm

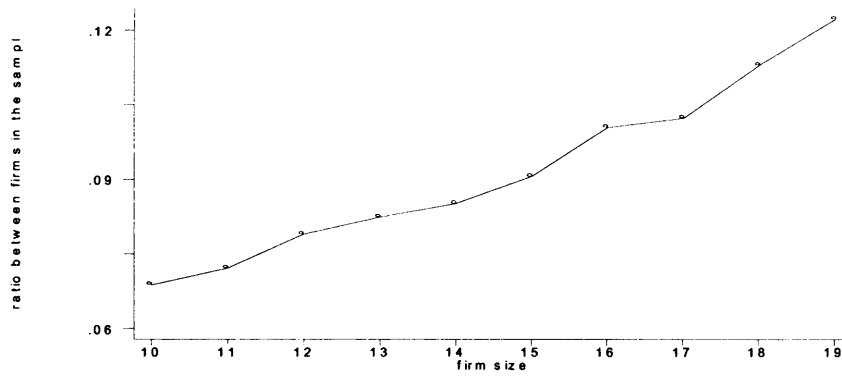


Figure 2: Relative Number of Firms by Size in our sample and in Istat-ASIA Data

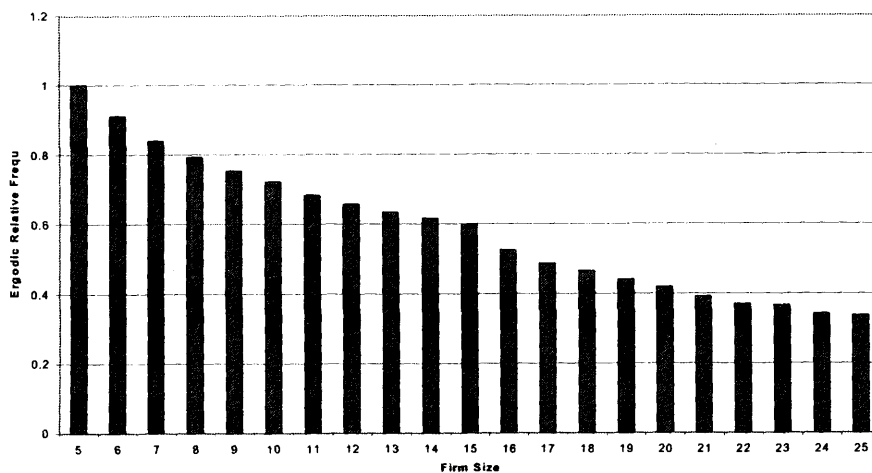


Figure 3: Implied Ergodic Firm Size Distribution (Relative to No. of Firms in Size 5)

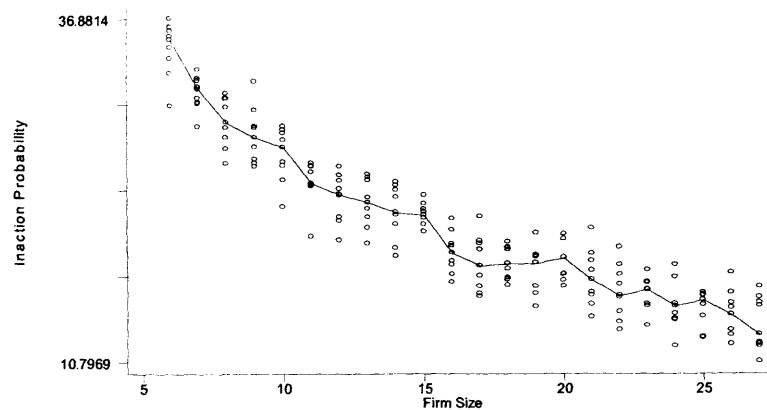


Figure 4: Inaction % Probability by Firm Size in Different Years: 1987-1995 (line connecting median values).

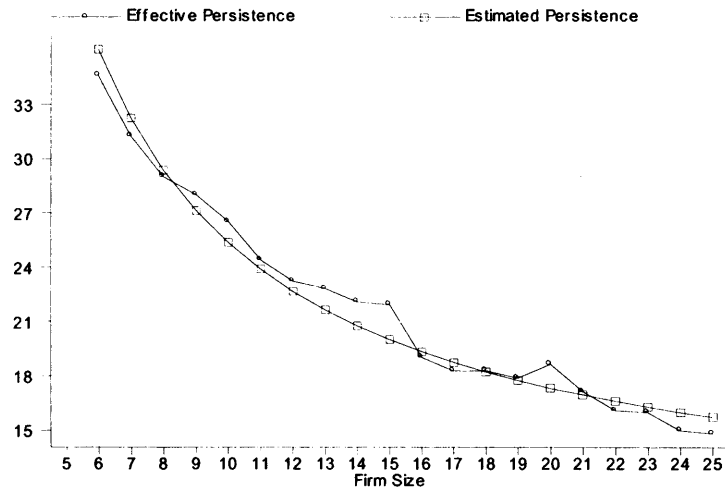


Figure 5: Median and Estimated Inaction Probability: 1987-1995

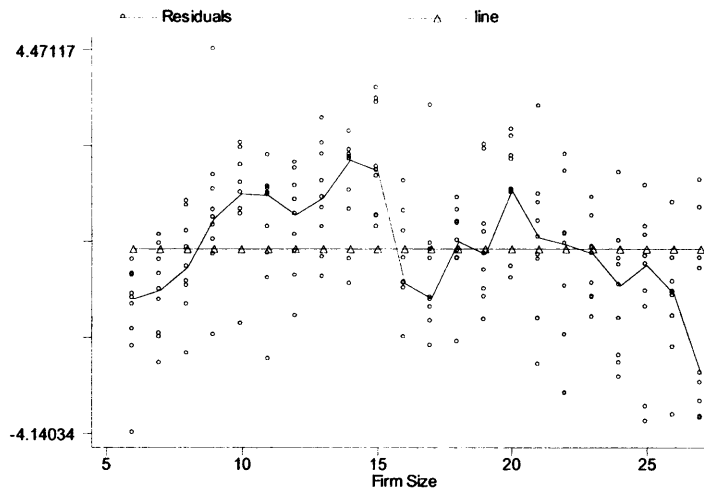


Figure 6: Residuals from Baseline Persistence Probability Model by Firm Size (line connecting median values, line at zero).

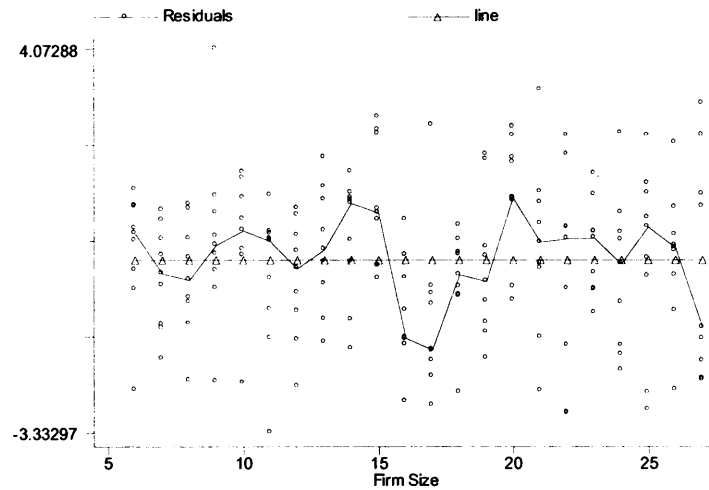


Figure 7: Residuals of Persistence Probability with Size and inverse of Size (line connecting median values, line at zero).

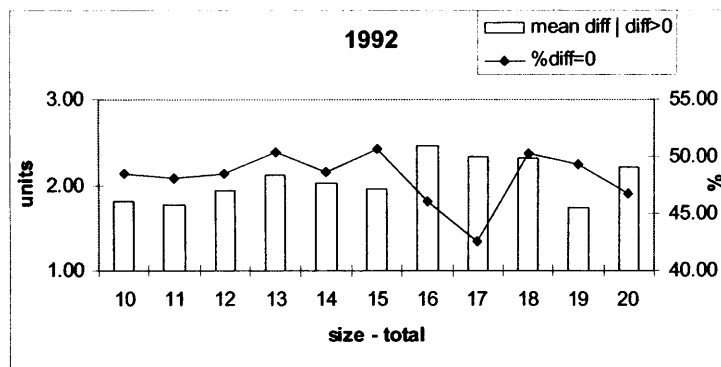


Figure 8: Labour code definition of employment, Turin, 1992. (Left axis: Absolute difference total employment-labour code employment if difference is positive. Right axis: share of firms where total employment equals labour code definition of employment. Firm size is total employment)



Figure 9: Persistence by firm Size in Total Employment and Labour Code Based Employment, Turin, 1990-1992

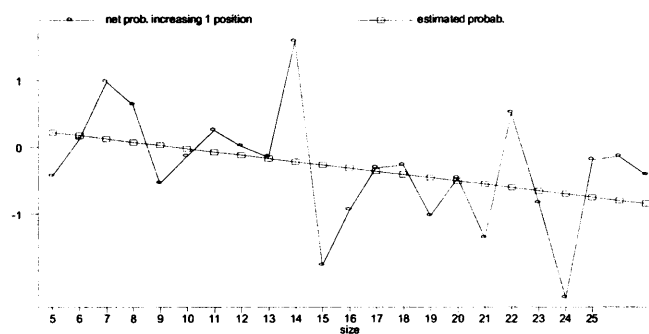


Figure 10: Net Probability of Increasing employment by One Position by Size: 1987-1995

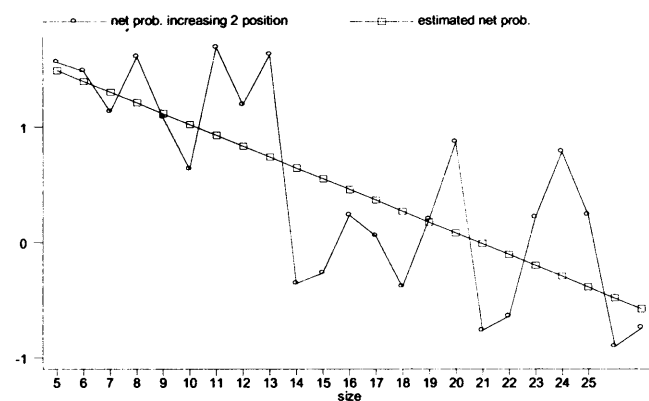


Figure 11: Net Probability of Increasing employment by Two Positions by Size: 1987-1995

Chapter 4

Temporary Contracts and Firms' Labour Demand

Abstract

The availability of a flexible labour force might influence adjustment decisions regarding the rigid part of the labour force. To test this idea, we contrast the use of temporary contracts and permanent ones when a reform made it more costly to use temporary workers. The results of our Difference In Differences analysis indicate that the burden of adjustment shifts on temporary workers if they are present in the firm. If this buffer becomes less available, firms employing temporary workers see their average labour productivity decrease in the short run and their job destruction rate increase in the medium run: when the shock hits, productivity reacts (and substitution between temporary and permanent workers takes place); then as time goes by, the stock of total employment is adjusted and productivity goes back to its previous level. In other words, firms react to shocks adjusting organisation and output on intensive margins in the short run, and more rigid stocks on extensive margins later on. These results may provide indications on the potential effects on firms' labour demand of more recent reforms aimed at generating a less costly flexible labour force.

1 Introduction

This work aims at contributing to the analysis of labour demand determinants and constraints in the Italian labour market. It focuses on firing costs imposed by employment protection legislation and on the role they play in the adjustment process of the labour force at the firm level.

To estimate the impact of dismissal regulations on labour demand, we focus on the use of “on the job training” contracts. They are temporary contracts (to be precise they are the only available temporary contract during our observation period) that can be used to hire young workers; their use is supported by the Government, that provides a significant rebate on social security contributions. Being temporary contracts, they do not generate firing costs when they expire; on the contrary, permanent contracts are expected to generate substantial firing costs when terminated. The availability of a flexible part of the labour force (we use “flexible” in the sense that it does not generate firing costs) might influence adjustment decisions regarding the rigid part of the labour force (that does generate firing costs) and it might influence overall firm organisation. In particular, we address two points: (i) whether temporary contracts act as a buffer to adjust employment, and (ii) the effects of a more costly buffer on firm performance. The answers are relevant per se, but also in a more general sense. In fact, Italian labour market is known to be highly regulated; nevertheless, there is widespread evidence of high flexibility. Hence it becomes important to understand whether institutional constraints are binding on human resources management at the firm level in Italy. This work aims at adding new elements to answer this more general question. While there is a widespread literature on the effects of temporary contracts on the individuals’ working career¹, very little has been done - for the lack of suitable data - from the point of view of the firm. In this respect, contributions of this work to the existing literature rely on the approach from the firm point of view and on the use of firm level data.

To motivate the assumption that temporary contracts do not generate firing costs when they expire and that permanent contracts do, we can refer to several results existing in the literature on adjustment costs. In fact, there is empirical evidence of small, if any, adjustment costs involving temporary contracts, and of substantial costs in adjusting the level of permanent employees in continental Europe. For example, Rota (2001) estimates a labour demand model with fixed and linear adjustment costs using data on permanent contracts in medium-sized manufacturing firms located in the north of Italy (close to an

¹See Booth et al. (2002) as an example.

upper bound of labour rigidity in Italy). She finds that fixed firing costs are substantial, around 40% of annual wage costs, and that linear firing costs are less important (3.6% of annual wage costs)². Goux et al. (2001) have access to a very rich French dataset where they can actually separate costly and non costly employment changes, i.e. they observe quits, layoffs and retirements as separate events. They estimate - in a convex adjustment cost framework - substantial costs to adjust the level of permanent workers; on the contrary they cannot find significant costs involved in adjusting the number of temporary workers. Aguirregabiria and Alonso-Borrego (1999) - in a linear adjustment cost framework - estimate that firing costs are between one third and a half of the gross annual wage of permanent workers in Spain.

The law regulating “on the job training” contracts changed significantly during our observation period, decreasing the rebate on social security contributions and imposing eligibility rules for firms, thus restricting the availability of what we call “the flexible part” of the labour force. This provides a sort of natural experiment on the impact this event had on human resource management at the firm level. After assessing that the reform actually had a significant impact on the demand for temporary workers in our sample³, we address the first point (whether temporary contracts act as a buffer to adjust employment) in two different ways. First, testing whether temporary and permanent workers are substitute; second, testing whether permanent workers are isolated from fluctuations by the presence of temporary workers in the same firm. According to our estimates, temporary workers actually behave as a buffer to adjust employment. Hence, we can proceed and estimate the effects of a more costly buffer on firm performance. We have two priors: (i) we expect average productivity of labour to decrease, because a less flexible labour force is often relatively more distant from its desired level⁴; (ii) we expect total employment to decrease, as a consequence of a decreased marginal product of labour and fixed (increased) wages. Notice that this is not the same as asking whether an increase in flexibility enhances productivity and job creation (the usual claims in the media debate); the two effects may not be symmetric. However, our results may provide indications on the potential effects of more recent reforms (since 1997) aimed at generating a less costly flexible labour force. This is important because of the lack of

²This is consistent with the importance of collective dismissals and the absence of individual severance payments in the Italian labour market.

³We also discuss in which sense we can consider “flexible”, or “disposable”, a group of temporary workers on which – by definition – the firm is supposed to invest in training.

⁴If we expect that firms have to overcome non convex adjustment costs to change the level of employment, inaction will emerge in their behaviour, and actual and desired level of employment will possibly be different.

formal evaluation of the effects of these more recent reforms from the point of view of the firm, i.e. evaluations focussing on their direct effect on labour demand⁵.

A brief foreword about data is necessary, because we use two different dataset in our analysis (different coverage, different unit of observation, different time span)⁶. We use the first - WHIP - to provide descriptive statistics on the phenomenon as a whole in Italy over a quite long period of time; we use the other - the "firm dataset" - for the econometric analysis as well as for descriptive purposes in the four provinces we study. From WHIP we select employees of private firms only, obtaining a 1:90 sample of *individuals* from 1985 to 2002⁷. The "firm dataset" has been drawn from the INPS Archive of private firms with employees, and it has been linked to some balance sheet summary statistics⁸. The original dataset includes the population of firms in Torino, Parma, Caserta, Taranto. These towns represent different productive areas in Italy⁹. It covers the period January 1990 - December 1992: from the peak of a long positive period toward a sharp economic recession. From the original dataset, we selected a balanced panel of firms employing less than 200 employees.

The chapter is organised as follows. Section 2 discusses the characteristics of temporary contracts and of their use; section 3 presents the empirical model; section 4 outlines the econometric method; section 5 details the results; section 6 concludes. Full details about the two dataset are provided in the Appendix.

2 On the Job Training Contracts

It is worth describing very briefly the Italian institutional framework related to firing costs. Since the early '70s very strong regulations against individual dismissals have been in place (discussed in chapter 3). On the other hand, collective and negotiated dismissals (either temporary or permanent) are supported by public funds to ease the transition of both the firm and the workers to a better condition. Temporary contracts have not been available till 1984 and part time jobs were not easily accessible. Over time and up to

⁵Very recent surveys conducted among selected samples of firms produced some descriptive, and not very conclusive, evidence. See ISAE (2006), Confindustria (2006).

⁶Full details are provided in the appendix.

⁷Although, for a neater presentation, we use it up to 1996, i.e. before the implementation of the most relevant reforms regarding temporary contracts.

⁸It is courtesy of R&P srl that provided the matched and anonymous dataset.

⁹Torino is a large industrial city in the north west of the Country, with rising unemployment rate due to declining manufacturing activities. Parma is a small town in the north east, fast growing area with low unemployment rate. Caserta and Taranto are in the south of Italy, where unemployment rate is high and the economy is weak.

these days we went through a slow process of reorganisation of the labour market, toward a more flexible setting. Among other reforms, in 1984 “on the job training” contracts were introduced; over the ‘90s more collective dismissal tools were brought up; in 1997 temporary contracts and leased work became legal, quasi-subordinate work started to be widely used. Before 1997 in Italy a very small share of the stock of employees worked with a temporary contract; starting from zero in 1984 it reached a maximum of about 8% in 1990; this percentage was 5% in 1996¹⁰. “On the job training” contracts have been the only available temporary contract till 1993, and in 1996 they still represented about 90% of existing temporary contracts. What matters here is that during our period of analysis (1990-1992) “on the job training” contracts and temporary contracts were synonyms.

Finally, it must be stressed that medium and large firms are more constrained by the institutional setting. As discussed in the previous chapters, very small firms have always been – *de facto* - more able to adjust their labour force through informal channels, and employment protection legislation usually does not apply below the 15 employees threshold. Our data cover both very small and medium sized firms, so we will be able to analyse this aspect from an empirical point of view. Furthermore, if we find that institutional constraints are binding in our dataset of small-medium size firms, we can infer that they are very likely to be binding in larger firms as well.

2.1 The Law

“On the job training” contracts are temporary contracts that can be used to hire young workers; their use is supported by the Government, that provides a significant rebate on social security contributions.

After 1984 Italian employers can hire young workers (15-29 years old) using this contract. They should provide them with a formal training program; however, the actual investment in training is neither quantified by the law nor checked by the authorities, i.e. there is a positive probability of cheating on this point. It is a temporary contract that can last up to 24 months. It can be transformed into a permanent one when it expires or when it is still valid; in the second case the firm retains the rebate provided by the law till the end of the original temporary period. Otherwise, the temporary worker is dismissed when the contract expires.

¹⁰In 1996 inflows (accessions) using “on the job training” contracts represent 10% of total accessions and 15% of accessions of young workers (under 30 years of age); the same figures reached a maximum of 16% and 23% respectively in 1989.

The law reduces significantly the social security contributions due on a given wage (in Italy on average social security contributions for employees amount to 33% of the gross wage). Table 1 presents the characteristics of this contract and the evolution over time of the rebate during the period covered by this study.

Table 1: Percentage rebate provided by the law over time

	year	(a)	(b)	(c)
On the job	1990	lump sum (98%)	50%	50%
training contract	1991 - 1992	lump sum (98%)	40%	25%
Permanent contract	1990 - 1992	(aa) 100% (aaa) 0%	0%	0%
(a) Firms located in the south or defined artisans				
(aa) Firms located in the south				
(aaa) Artisans not in southern regions				
(b) Firms in the trade or tourism industries employing less than 15 workers				
(c) All other firms				

Southern firms and artisans¹¹ enjoy a full rebate all-over the observation period; the other firms enjoy a lower rebate, decreased further by the 1991 reform. This should be contrasted with the characteristics of a permanent contract: an open-end contract that provides no rebate on social security contributions outside the southern regions¹². The saving generated by the use of temporary contracts is a substantial amount: social security rebate goes from 8% to almost 33% of yearly wage¹³, plus the absence of firing costs (up to about 40% of the yearly wage¹⁴); i.e. they may reach a maximum of 73% of the yearly wage. In the north of Italy the incentive to use these contracts is twofold: social security contributions rebate and no firing costs; in the south the incentive is lower: no firing costs. This is likely to generate a different selection of firms using “on the job training” contracts in the two areas; we will come back to this point when presenting evidence on the use of temporary workers and again presenting the econometric results.

On 1/1/1991 the rebate on temporary workers’ social security contributions decreased significantly for a subset of firms, as table 1 shows. Furthermore, firms that had not hired on a permanent base at least 50% of their temporary workers during the previous 24 months were excluded from the use of this contract (no such constraint existed before 1991).

On aggregate, the effect of the 1991 reform has been massive. Focusing on the

¹¹Legal definition indicating small manufacturing firms where the owner works in the firm.

¹²At that time in the south of Italy other laws provided full rebate on social security contributions for every employee (incumbent or newly hired, regardless the age). These laws changed after 1992.

¹³From 25% to 98% of SSC, when SSC represent 33% of yearly gross wage.

¹⁴Rota (2001). Of course a collective dismissal would generate lower firing costs per capita.

dynamics of the aggregate employment level of young workers by contract (temporary vs. permanent) over the 1987-1996 period we notice what follows (figure 1). In 1988, when the first change in the law regulating “on the job training” contracts took place (they became a bit more expensive) we observe some substitution between permanent and temporary contracts. In 1991, when temporary contracts became much more expensive than before (but still cheaper than permanent ones), we observe a massive substitution from temporary to permanent contracts that lasts a couple of years (temporary contracts can last up to 24 months). Afterwards both groups follow the business cycle.

Focussing on the four provinces we analyse we see that the stock of temporary workers decreased in the areas affected by the reform (Torino, figure 2, and Parma, figure 3) when the reform came into effect, while it was basically constant where the reform did not change their price (South, figure 4). In fact in the first two figures there is a clear break in the series of temporary employees at the end of 1990; their number starts decreasing smoothly as firms wait for the temporary contracts to expire.

The second constraint¹⁵ had an obvious but limited impact on the transformation rate (Table 2). Interestingly, the equilibrium transformation rate, without any constraint, was already about 50%; the constraint made it increase by 10 percentage points, but it went back to the previous value in a few years. It must be noticed that this eligibility constraint applies to all firms, so that the differential effect between the “treated” and the “control” groups relies only on the decreased rebate.

Table 2: Transformation rate from temporary to permanent contract in the same firm

Year	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
rate	45.5	45.4	49.4	52.7	51	61.3	60	63.1	61.8	55.7	45.9
Pct. relative to the stock of temporary workers											
Source: WHIP											

On aggregate the effect of the reform on the use of temporary contracts is clearly to reduce their use. We also want to test whether the reform had a statistically significant effect on the behaviour of the firms observed in our dataset. This because they are small and medium size firms, and as we have already argued, very small firms have always been *de facto* more flexible, also despite legal constraints. We formally test this point after presenting the econometric method in the next section. Before doing so we present a descriptive analysis of the characteristics of the firms using these contracts.

¹⁵Firms that had not hired on a permanent base at least 50% of their temporary workers during the previous 24 months were excluded from the use of this contract.

2.2 The Use of On the Job Training Contracts at the Firm Level

In general, during our observation period (1990-1992), the use of this contract is not widespread; and it decreases over time. In the South (Caserta and Taranto), 80% of firms never uses this contract, with respect to 60% in North East (Parma) and 70% in North West (Torino). This points to the wider range of supplemented contracts available only in Southern regions. About 10% of firms used these contracts both in 1990 and 1991 in the South; the percentage decreased to 6.4 between 1990 and 1992. The same figures are about 20% and 12% in North East; 11.4% and 8.1% in North West.

A simple multivariate analysis checks which firm characteristics are positively or negatively correlated to the number of temporary workers present in the firm. We do not intend to estimate any structural model; we also assume different error structures, as a robustness check¹⁶. As expected, the number of temporary workers is increasing with the potential rebate for which the firm is eligible if it decides to use temporary contracts; it is decreasing with the potential wage¹⁷ the firm has to pay to each worker. The number of temporary workers is positively correlated to the size of the firm, measured by real sales; this is consistent with the observation that very small firms are less influenced by employment protection legislation, hence they need to cut firing costs less than larger firms. It is positively correlated to the use of self-employees and of overtime (i.e. other flexibility enhancing tools). It is lower in services than in manufacturing (artisans can only be manufacturing firms). It increases with firm's age and the fact that the firm exports or imports some positive amount (proxies for better quality firms, given size). And it is positively correlated with the percentage of white collars¹⁸. It is worth noticing that unconditional statistics on the use of temporary contracts show that their use increases as the provided rebate decreases. In fact, the law provides the highest rebate to the "worst" firms, as it is targeted to help weaker firms to hire young workers. However, firms in the Northern regions are likely to use temporary contracts more extensively on average, despite the lower rebate, because they are "good" firms, i.e. more innovative in their internal organisation, more ready and able to use new instruments, more adaptable to a new environment.

¹⁶First, we estimate a Tobit model on each year in the sample, assuming white noise errors. Then we pool the observations and estimate a random effect Tobit with the balanced panel obtained. Finally we estimate a logit model with fixed effects, where the dependent variable is just a dummy on the use of the contract in the firm. The drawback of this last method is that only firms that change status (from users to non-users or viceversa) can be used to estimate it.

¹⁷The real wage in the cell the firm belongs to, defined by 13 industries, 3 geographical areas, 3 size classes.

¹⁸These results are robust to a partition of the dataset into small and medium size firms.

3 The empirical model

We use standard labour demand schedules to answer the questions we are interested in. The original contribution is in the way we specify the price of the "temporary work" input, exploiting the provisions of the 1991 reform. In particular we use a dummy D_{it} signalling the fact that firm i was affected by the reform, after the reform took place and we perform a difference in differences analysis (DID). This is discussed at length in the next section.

A few preliminary remarks are needed. First, to discuss the labour adjustment process we focus on a law whose use is restricted to young workers. This choice is not limiting. In fact young people (under 30) bear almost alone the burden of positive and negative changes in total employment, as figure 5 confirms. Second, given the length of our panel of firms only a short run analysis is possible. I.e. we cannot identify a long run movement toward a different equilibrium; we can only identify a short term reaction to different prices set by the reform. Third, we need to assess that the 1991 reform had a significant effect on the use of temporary workers in our dataset of small and medium size firms, as in previous sections we have only showed its aggregate effect. We estimate a textbook demand schedule for temporary workers at the firm level:

$$l_{it} = \gamma^{(l)} D_{it} + X_{it}^{(l)} \beta^{(l)} + u_{it}^{(l)} \quad (1)$$

where l_{it} is the number of temporary workers in firm i at time t , D_{it} measures the labour cost of temporary workers, X_{it} includes a measure of size (real sales) and the share of white collars as a proxy for the level of complexity of technology used by the firm¹⁹. We test $\gamma^{(l)} < 0$.

In what follows, we loosely refer to a standard profit maximizing firm, producing with two inputs (temporary and permanent workers) and facing possibly non-convex turnover costs to adjust the level of permanent workers, i.e. the level of permanent contract workers is adjusted when the actual number is "too far" from the desired level.

¹⁹The share of non-production work (white collars / workforce) can proxy the level of complexity of technology and/or of organisation in the firm. Pacelli et al. (1998) show the link between composition of the workforce by occupation and technology in private Italian firms. For example, if we split firms by the intensity of innovation observed in the industry they belong to, we have 43% of white collars in high intensity of innovation sectors and 8% in low intensity of innovation sectors, in the class 1-20 employees; in the class 1000 employees or more these percentages become respectively 51% and 26%.

Capital is kept fixed, consistently with the short run approach. The desired level of each input is set equal to its profit maximising level, computed under the hypothesis of no adjustment costs²⁰.

3.1 Temporary contracts as a buffer to adjust employment

A more basic question needs to be answered first: can temporary workers be used as a buffer by the firm when the firm is supposed to invest in their training? The answer is yes if the investment in training is significantly lower than the saving provided both by social security rebate and by the absence of firing costs. Given the high amount of saving provided by this contract, we would not be surprised if firms invested significantly less than that in training²¹. An indication in this sense can be provided by transformation rates. Splitting the average transformation rate by occupations or by industries defined by their intensity of innovation we observe a higher transformation rate - as expected - in high tech industries with respect to traditional ones, and among white collars with respect to blue collars. This seems an indication of positive correlation between the degree of skill required by the job and the transformation of the temporary contract into a permanent one; the reason can be a higher investment in specific human capital in these cases. However, the spread in transformation rates is about or below 10 percentage points in both cases (50% vs. 60% both by occupation and by industry in 1990), and both high tech firms and white collars represent a minority in the stock of temporary contracts (about 5% and 30% respectively). It does not seem likely that the large majority of firms invests heavily in training their temporary workers.

We infer the answer to the main question from two different points of view. First, we test the existence of the characteristics of a buffer. To be a buffer, temporary workers must be more flexible than permanent workers; they must be hired and dismissed more frequently than permanent contract workers. And, temporary workers must be substitute of permanent workers. Second, we test the existence of the expected effect of a buffer: temporary workers should be able to isolate permanent workers from fluctuations of demand, i.e. their presence in the firm should influence not only the number of permanent workers but also their variability over time.

Temporary contracts are more flexible than permanent ones. The point seems on one side obvious, on the other hand difficult to measure in a convincing way without a

²⁰We can refer to Attanasio (2000) as an example of the same choice.

²¹Furthermore, anecdotic evidence points to a large amount of cheating by firms on the training provided to workers hired with this contract.

structural model that estimates adjustment costs attached to the two kinds of contracts. In section 1 we mentioned some empirical evidence of small, if any, adjustment costs involving temporary contracts, and of substantial costs in adjusting the level of permanent employees in continental Europe. Descriptive statistics on the relative flexibility of temporary and permanent workers in Italy can be computed and are presented in section 5.

We expect temporary workers and permanent workers to be substitutes, but not perfect. This would be consistent with Aguirregabiria and Alonso-Borrego (1999), who find that in Spain 15 years after they became legal, the equilibrium share of temporary contracts is far from 100%. This indicates non perfect substitutability²², although the level (and turnover) of permanent-contract employees decreases if there are temporary-contract workers in the firm.

We test whether temporary workers and permanent workers are substitutes estimating a permanent-contract labour demand equation to single out the effect of the price of temporary workers on the demand of permanent ones:

$$L_{it} = \gamma^{(L)} D_{it} + X_{it}^{(L)} \beta^{(L)} + u_{it}^{(L)} \quad (2)$$

where L_{it} is the number of permanent workers in firm i at time t , D_{it} measures the labour cost of temporary workers, $X_{it}^{(L)}$ includes permanent workers' real wage, a measure of size (real sales), the share of white collars as a proxy for technology. We test $\gamma^{(L)} > 0$.

Another way of deciding whether temporary workers are a buffer to adjust employment is to test whether the fact that they are employed by a firm decreases the variability of the number of permanent workers employed by the same firm. We estimate a reduced form model where gross worker turnover of firm's permanent workers²³ depends on the size and quality of the firm as well as on the availability (i.e. price) of temporary workers:

$$GWT_{it}^L = \gamma^{(GWTL)} D_{it} + X_{it}^{(GWTL)} \beta^{(GWTL)} + u_{it}^{(GWTL)} \quad (3)$$

where GWT_{it}^L is the gross worker turnover of permanent workers in the firm, D_{it}

²²They explain this non perfect substitutability by the existence of hiring costs also for temporary contracts.

²³Sum of monthly accessions and separations from January to December relative to the stock of permanent workers. It excludes December to January employment changes to avoid measurement problems generated by legal transformations of firms. In fact, legal transformations usually take place at the end of the Italian fiscal year (31 December).

measures the labour cost of temporary workers, $X^{(GWTL)}$ includes a measure of size (real sales) and the share of white collars as a proxy for technology. We expect $\gamma^{(GWTL)}$ to be positive, i.e. when the buffer becomes more costly its demand decreases and GWT of permanent contract workers increases.

3.2 The effects of a more costly buffer on firm performance

If temporary workers are a buffer to adjust employment, we can then estimate the effect of a more costly buffer on firm performance. We may suppose that productivity of labour decreases, because a less flexible labour force is relatively more distant from its desired level. And we may suppose that total employment decreases at the firm level, due to fixed (or higher) wages and lower productivity.

We estimate the correlation between the availability of temporary workers and, in turn, average productivity of labour and total labour demand. Our reduced form productivity equation regresses average productivity of labour in the firm on the availability of temporary workers (D_{it}), controlling for average wages, investment and the level of technology ($X_{it}^{(1)}$)

$$\frac{S_{it}}{E_{it}} = \gamma^{(1)} D_{it} + X_{it}^{(1)} \beta^{(1)} + u_{it}^{(1)} \quad (4)$$

S_{it} are real sales and $E_{it} = L_{it} + l_{it}$. Our reduced form total employment equation regresses E_{it} on the availability of temporary workers (D_{it}), controlling for average wages, sales and the level of technology ($X_{it}^{(2)}$)

$$E_{it} = \gamma^{(2)} D_{it} + X_{it}^{(2)} \beta^{(2)} + u_{it}^{(2)} \quad (5)$$

We test $\gamma^{(1)} < 0$ and $\gamma^{(2)} < 0$.

4 Econometric Method

We perform a difference in differences analysis to estimate the coefficients of interest (γ) in equations (1) to (5). The reform involved only a subset of firms; hence it provides a natural control group made of firms that – although they were using temporary contracts – were not affected by the reform. As we will explain in details below, we can also estimate

the common macroeconomic trend, using the same model with data on firms that do not use temporary contracts.

The following remark is important. The 1991 reform imposed a lower rebate for some firms and eligibility rules for *all* firms. Hence, our DID approach estimates the effects of the change in trainees labour cost only, not the effect of the change in eligibility rules, that is universal. In this sense we intend "affected" and "non affected" firms.

Notice finally that there is an endogenous selection of firms that decide to use or not to use temporary contracts; however on 1/1/1991 there was an exogenous²⁴ selection of users that were involved in the reform and users that were not. We will exploit this exogenous variability in the price of temporary workers in the difference in differences analysis. We define four groups of firms:

1. those affected by the reform (firms located in the north of Italy not defined artisans) that were employing temporary workers in 1990 (treatment group $g = T$)
2. those non affected by the reform (firms located in the south and artisans) that were employing temporary workers in 1990 (control group $g = C$)
3. those who would have been affected by the reform but were not employing temporary workers in 1990 (counterfactual treatment group $g = T_0$)
4. those who would not have been affected by the reform and were not employing temporary workers in 1990 (counterfactual control group $g = C_0$)

We use two periods: 1990, i.e. before the reform ($t = 1$); 1991 i.e. immediately after the reform (short run effect, $t = 2$). Alternatively we use 1992 to estimate the medium run effect (in this case $t = 2$ is 1992).

Write a generic model

$$y_{it} = \gamma D_{it} + X_{it}\beta + u_{it} \quad (6)$$

where y is a generic dependent variable, X is a matrix of controls and D is the dummy on the reform: D is equal to 1 iff $t=2$ (after the reform) and $g = T$. γ can be estimated using a difference in differences estimator and exploiting the variability in prices generated by the 1991 reform.

²⁴Firms may be endogenously selected into industries or locations or size classes, but this is not due to causes related to the use of temporary contracts. I.e. no firm switched to a different area, for example, between 1990 and 1991 to escape the effect of the reform.

$$\begin{aligned}\gamma^{DID} &= (\tilde{y}_2^T - \tilde{y}_1^T) - (\tilde{y}_2^C - \tilde{y}_1^C) \\ \tilde{y}_s^G &= E \left(y_{is} - X_{is} \hat{\beta} | t = s, g = G \right)\end{aligned}\tag{7}$$

A proper control for observable and unobservable quality of the firm is crucial to single out the price effect on the quantity of temporary workers used by the firms. The law provides the highest rebate to the “worst” firms, as it is targeted to help weaker firms to hire young workers. However, “stronger” firms are likely to use temporary contracts more extensively on average, because they are “good” firms, as already argued. Furthermore these firms are more likely to be able to hire workers, whatever the contract. Between 1990 and 1991 labour cost of temporary workers increased only for those firms that were already facing the lowest rebate. So if we take evidence at its face value we might see that both cost of labour and employment grew in “better” firms. Of course the quality of the firm drives the result.

The error term

$$u_{it} = \psi_i + k_i m_t + \epsilon_{it}\tag{8}$$

includes an individual fixed effect ψ_i , a macro trend m_t that affects individual firms differently according to k_i , and a white noise ϵ_{it} . We can make two different assumptions about the expected value of the error term, conditional on time, X and the group the firm belongs to. It is standard in this kind of literature (see Blundell et al., 1998) to impose:

$$E[u_{it} | t = s, i \in g, X_{is}] = \psi_g + k m_s\tag{9}$$

the conditional expectation of the individual fixed effect is constant within a group and the macroeconomic environment affects everybody in the same way. The first assumption should not be problematic, even if groups are quite heterogeneous and only a few observables are available to condition on, because the DID estimator is robust to everything that is constant over time or that grows at a constant rate. On the contrary, the second hypothesis may be problematic because the affected and non-affected firms are chosen by the policy maker according to the fact that a group is weaker than the other. It might be that groups defined in this way respond differently to the macro-

economic environment. For this reason we relax the second assumption. A looser and maybe more realistic assumption is to impose that the macro-factor can affect groups differently:

$$E[u_{it}|t = s, i \in g, X_{is}] = \psi_g + k_g m_s \quad (10)$$

Under the more stringent hypothesis on the errors (equation 9), to estimate γ we apply equation (7); in fact

$$E[\tilde{y}_s^G | t = s, g = G] = y_{is} - X_{is}\hat{\beta} = \gamma D_{it} + E[u_{it} | t = s, i \in g, X_{is}] = \gamma D_{it} + \psi_g + k_g m_s \quad (11)$$

$$E[\gamma^{DID}] = [(\gamma + \psi_T + k m_2) - (\psi_T + k m_1)] - [(\psi_C + k m_2) - (\psi_C + k m_1)] = \gamma \quad (12)$$

Under the looser hypothesis on the error structure (equation 10) this does not hold. The consistent way of estimating γ in this case is the following. We estimate the same model (equation 6) using the groups T_0 and C_0 . They do not use temporary workers, hence they will not show any sign of reaction to the reform. Although users and non-users are self selected, it is enough to assume $(k_T - k_{T_0}) = (k_C - k_{C_0})$ to obtain a consistent estimate of γ ²⁵. In fact, under equation (10):

$$E[\tilde{y}_s^G | t = s, g = G] = y_{is} - X_{is}\hat{\beta} = \gamma D_{it} + E[u_{it} | t = s, i \in g, X_{is}] = \gamma D_{it} + \psi_g + k_g m_s \quad (13)$$

Using T and C :

$$\begin{aligned} E[\gamma^{DID}] &= [(\gamma + \psi_T + k_T m_2) - (\psi_T + k_T m_1)] - [(\psi_C + k_C m_2) - (\psi_C + k_C m_1)] \\ &= \gamma + (k_T - k_C)(m_2 - m_1) \end{aligned} \quad (15)$$

Using T_0 and C_0 we estimate the macro trend:

²⁵Notice that this is weaker than requiring $k_T = k_{T_0}$ and $k_C = k_{C_0}$

$$\begin{aligned}
E[\gamma_0^{DID}] &= [(0 + \psi_{T_0} + k_{T_0}m_2) - (\psi_{T_0} + k_{T_0}m_1)] - [(\psi_{C_0} + k_{C_0}m_2) - (\psi_{C_0} + k_{C_0}m_1)] \\
&= (k_{T_0} - k_{C_0})(m_2 - m_1)
\end{aligned} \tag{17}$$

and assuming

$$(k_T - k_C) = (k_{T_0} - k_{C_0}) \tag{18}$$

we have

$$E[\gamma^{DID}] - E[\gamma_0^{DID}] = \gamma \tag{19}$$

Finally, we need a consistent estimate of β . To estimate β we pool the two groups and the two periods and we estimate $y_{it} = X_{it}\beta + u'_{it}$ by first differences (as we are including individual fixed effects), using IV for the wage if it is included among the controls. Then we compute

$$\tilde{y}_s^G = \sum_{i=1}^n \left(y_{is} - X_{is}\beta \right) / n \text{ for } i \in G, t \in s \tag{20}$$

and

$$\gamma^{DID} = \left(\tilde{y}_2^T - \tilde{y}_1^T \right) - \left(\tilde{y}_2^C - \tilde{y}_1^C \right) \tag{21}$$

As \tilde{y}_s^G is a random variable, we use its sample variance to estimate the standard error of γ^{DID} .

5 Results

In what follows we discuss in turns the estimated values of γ in equations (1) to (5) above²⁶. Statistics on the sample used are in the Appendix.

²⁶Regression results not reported.

5.1 Had the 1991 reform an effect on the demand of temporary workers?

The estimate of γ from equation (1) is reported in table 3. γ is negative and significant, confirming that the reform had an effect not only on aggregate in Italy but also on the firms included in our dataset. As a robustness check we selected very small firms (total employment below 5 employees) and small firms (total employment below 16 employees): γ , although smaller in absolute value, is still negative and significant. This shows that also very small firms reacted, although to a more limited extent, to the increase in the price of temporary workers decreasing their use. This is true both including and excluding controls (the level of sales and the percentage of white collars in the firm).

Notice that in this particular case we must assume that the structure of errors is the one described in equation (9), i.e. the more stringent one. In fact to be able to relax this hypothesis and estimate the model assuming that equation (10) holds, we should use firms that do not use temporary workers to estimate the macro trend, where the dependent variable is zero by definition.

Table 3: Demand of temporary workers. Difference in differences estimates of gamma.

	all		E<5		E<16	
	1990-1991	1990-1992	1990-1991	1990-1992	1990-1991	1990-1992
no controls						
γ	-0.3035	-0.4686	-0.1233	0.0096	-0.114	-0.094
s.e.	0.0296	0.0351	0.0237	0.0261	0.018	0.0212
$\gamma/s.e.$	-10.2682	-13.3481	-5.195	0.3692	-6.3208	-4.4326
with controls						
γ	-0.3023	-0.4726	-0.1217	0.0173	-0.1127	-0.0961
s.e.	0.0294	0.0352	0.0233	0.0261	0.0179	0.0213
$\gamma/s.e.$	-10.2718	-13.439	-5.2207	0.6629	-6.2895	-4.5105

5.2 Are temporary contracts a buffer to adjust employment?

5.2.1 Are temporary contracts more flexible than permanent ones?

Here we present some descriptive statistics on the relative flexibility of temporary and permanent workers in Italy. They are not on adjustment costs directly but on observed flows in and out of the firms; these are strictly correlated to the costs involved, even if they are noisy measures. As temporary contracts are temporary obligations, it seems plainly obvious to observe that separation rates are higher for them, even allowing for an average 50% transformation rate at the end of the training period. It may be more

convincing to look at accession rates²⁷. Furthermore, as temporary contracts are for young workers only while permanent contracts are for everybody, we focus on accession rates of young workers only. This because job shopping activity at early stages of the working career generates - *ceteris paribus* - a higher number of accessions among young workers. In table 4 we see that accession rates of temporary workers are twice that of young permanent workers. We may consider this as evidence consistent with lower adjustment costs attached to temporary contracts in Italy.

Table 4: Accession rates, Italy, 1986-1996. Workers under 30 years of age.

year	temporary contract	permanent contract
1986	1.53	0.53
1987	1.12	0.45
1988	0.98	0.46
1989	0.90	0.47
1990	0.81	0.48
1991	0.70	0.49
1992	0.64	0.39
1993	0.64	0.37
1994	0.84	0.35
1995	0.84	0.40
1996	0.74	0.39
Source: WHIP		

5.2.2 Are temporary workers and permanent workers substitutes?

The results of the DID estimates of the demand of permanent workers as a function of the price of temporary workers (equation 2²⁸) confirm out priors. Table 5 prints the estimated values of γ . The column labelled “ γ ” shows the relevant point estimate if we believe in the more stringent hypothesis on the errors (equation 9), the column labelled “ $\gamma - \mu$ ” is the one if we believe in the looser hypothesis (equation 10).

Table 5: Demand of permanent workers. Difference in differences estimates of gamma.

	1990-1991		1990-1992	
	γ	$\gamma - \mu$	γ	$\gamma - \mu$
γ	0.526	0.502	0.194	0.123
s.e.	0.06	0.062	0.09	0.093
$\gamma/s.e.$	8.784	8.084	2.16	1.323

²⁷Total number of hirings in year t over the stock of employees at the end of the year. Notice that using WHIP, transformations of temporary contracts into permanent ones are not defined as accessions (this is not true in the firm dataset). Notice also that accessions are not equal to separations if employment is not in steady state, as it is the case here.

²⁸IV for wages: 13 industry, 4 size and 3 area dummies.

In the short run γ is significant and positive, under both assumptions on the error structure. This provides evidence that temporary workers and permanent workers are substitute, at least in the short run. The medium run picture is less clear cut; only under the most stringent hypothesis on the error structure γ is positive and significant. In two years firms may be able to adjust in a more complex way; in fact even if temporary contracts can last up to 24 months very few firms have 1990 contracts still in existence in 1992 (about 100 firms out of 4000).

The size of γ is interesting. The reform increased labour demand for permanent workers on average by one half of a worker. The user affected group, in 1990, employs an average of 17.7 permanent workers. The effect, although statistically significant, does not seem large. If we compute the implied cross wage elasticity we get about 0.3²⁹.

Several robustness checks confirm the above results. We cut the sample by firm size and by industry, we tried different estimation methods for equation 2 and we excluded the south of Italy from the sample. Results are unchanged. Particularly reassuring is the cut that excludes firms located in the south of Italy and uses only artisans in the control group, because of the different selection of firms in the use of this contract. In this case $\gamma - \mu$ increases to .678 (s.e..060). Cutting the sample by industry we confirm that temporary workers and permanent workers are substitute both in services and in manufacturing firms; this is relevant because artisan firms can only be manufacturing activities. Estimates by firm size are as follows. Splitting the sample by size (below and above 5 employees) and re-estimating the model, in the short run³⁰ the estimated $\gamma - \mu$ is significantly lower among small firms (.149, s.e..043) than among larger firms (.599, s.e..096), positive and significant in both cases. We also tried a different threshold, 15 employees, because most of employment protection legislation applies only above that threshold. Below 15 employees $\gamma - \mu$ decreases to .301 (s.e..060). Unfortunately, no significant result can be obtained above that threshold (although $\gamma - \mu$ is higher: 0.582, s.e. .348); the sample size above that threshold may be too small to provide precise estimates. An indication that the effect of the reform is increasing with firm size proceeds from these results together.

²⁹From Table 8, defining w_l as average wage plus social security contributions minus the rebate on SSC:

$$\epsilon_{L, w_l} = \frac{\Delta L/L}{\Delta w_l/w_l} = \frac{.526/17.77}{2358.5 [(1 + .33 - .33 * .25) - (1 + .33 - .33 * .5)] / 2358.5 (1 + .33 - .33 * .5)} = .308$$

³⁰In the medium run no significant effect of $\gamma - \mu$ can be obtained.

5.2.3 Are temporary workers able to isolate permanent workers from fluctuations of demand?

The effect of the reform on permanent contract worker turnover is more difficult to assess. The number of permanent workers and the level of their turnover desired by the firm as an effect of the 1991 reform may be different from the observed level and turnover. The distance between desired and observed values is a positive function of adjustment costs. If the firm wants to increase the number of permanent workers – as a reaction to the 1991 reform – it has to face hiring costs; if it wants to increase permanent contract worker turnover it has to face both hiring and firing costs. It is clear that the reform is more easily effective on the level of employment than on its variability. However, in our sample of firms employing less than 200 workers, average turnover of permanent-contract workers is as high as 50%, leaving scope for an effect to arise.

Table 6 presents the results. No controls are included in equation (3), as they are never significant. $\gamma - \mu$ is positive and significant in the medium run under the looser hypothesis on the error structure (equation (10)). We can read this as an indication that supports – although not strongly – what obtained above; it might take more time to adjust both hirings and firings with respect to adjusting just hirings.

Table 6: Gross worker turnover of permanent workers. Difference in differences estimates of gamma.

	1990-1991		1990-1992	
	γ	$\gamma - \mu$	γ	$\gamma - \mu$
γ	-0.025	0.021	-0.022	0.069
s.e.	0.02	0.031	0.027	0.037
$\gamma/\text{s.e.}$	-1.263	0.677	-0.811	1.864

5.3 What are the effects of a more costly buffer on firm performance?

Having shown that temporary workers act as a buffer to adjust employment, we estimate the effect of a more costly buffer on firm performance. In section 3 we said that if it is an adverse condition to be less able to adjust employment easily because the buffer part of the labour force is more costly, than we should be able to measure this effect on productivity and on total employment in the firm. Table 7 shows the results³¹.

³¹In the productivity equation instrument for the wage is the average wage in the cell defined by size, industry, province. In the employment equation instruments for the wage are 13 industry 4 size and 3 area dummies.

Table 7: Productivity and employment equations. Difference in differences estimates of gamma.

	1990-1991		1990-1992	
	γ	$\gamma - \mu$	γ	$\gamma - \mu$
S/E				
γ	-0.0174	-0.015	0.0049	0.004
s.e.	0.0028	0.004	0.0033	0.004
$\gamma/s.e.$	-6.1396	-3.398	1.4989	0.857
E				
γ	0.04	0.019	-0.281	-0.3
s.e.	0.056	0.059	0.092	0.095
$\gamma/s.e.$	0.72	0.325	-3.058	-3.148

In the short run productivity is decreased, but not in the medium-long run. It decreases by 0.0174 billions lire per worker, i.e. about 8,700 euro. The average productivity per worker was 262 million lire in the user affected group in 1990, i.e. about 131,000 euro. The effect, although statistically significant, is not large. On the contrary, no significant effect can be estimated on the level of employment in the short run; however in the medium run, when there is no effect on productivity any more, there is a negative and significant effect on the level of employment. About a third of a worker more would have been employed after the reform in the treated group had the reform not taken place. Again, the effect is significant but small (average total employment in this group of firms is about 20 workers). We can rationalise these results as follows. When the shock hits then productivity reacts (and substitution between temporary and permanent workers takes place); then as time goes by, the stock of total employment is adjusted and productivity goes back to its previous level. I.e., it seems that firms react to shocks adjusting organisation and output on intensive margins in the short run, and more rigid stocks on extensive margins later on.

We performed several robustness checks, to verify to which extent this conclusion can be sustained. We cut the sample by geographical area, by firm size and we used different estimation methods. In addition, for the productivity equation we used different definitions of total employment, that may be relevant in very small firms (they are a large part of our sample). Our results are confirmed. In particular excluding firms employing up to 4 employees, γ decreases from -.017 (s.e. .004) to -.023 (s.e. .005); it decreases further to -.040 (s.e..008) excluding firms employing less than 15 employees. This is true under the more stringent hypothesis on the error structure (equation 9)³². This is consistent with the results obtained on equation 2: the effect of the reform seems to be increasing

³²Nothing is significant under the looser hypothesis (equation 10).

with firm size. The same can be said about the employment equation in the medium run. Excluding firms employing up to 4 employees γ decreases from $-.281$ (s.e. $.092$) to $-.386$ (s.e. $.132$); it decreases further to $-.804$ (s.e. $.389$) excluding firms employing less than 15 employees. Again, this is true under the more stringent hypothesis on the error structure only (equation 9).

6 Conclusions

The interest in the effects of firing costs imposed by employment protection legislation on firm organisation and firm performance motivated this work. We focused on the use of “on the job training” contracts and contrasted temporary and permanent workers. The availability of a more flexible part of the labour force might influence adjustment decisions regarding the more rigid part of the labour force, and this in turn might influence overall firm organisation and performance.

We exploited the effect that the 1991 reform of this contract had on temporary workers’ use: in 1991 the rebate decreased for a subset of firms and the use of the contract became more difficult. The aggregate effect of the reform was a massive substitution of young permanent workers to temporary workers. The number of firms using this contract and the total number of temporary workers decreased. A multivariate analysis with firm data confirms that the use of temporary workers is positively correlated to the potential rebate provided by the law. Finally, a DID analysis estimated a significant and negative effect of the reform on the demand of temporary workers among the firms included in our dataset.

We wanted to answer two questions: “*Are temporary contracts a buffer to adjust employment?*” and “*What are the effects of a more costly buffer on firm performance?*” There is empirical evidence of small, if any, adjustment costs involving temporary contracts, and of substantial costs in adjusting the level of permanent employees in continental Europe. Some descriptive statistics on the relative flexibility of temporary and permanent workers confirm the result in Italy. We then estimated a labour demand model of permanent workers, as a function also of the price of temporary workers. The cross price effect is positive, statistically significant but quite small. The effect is increasing with firm size. Several robustness checks confirm the validity of these conclusions. Finally we tested whether the presence of temporary workers isolates permanent workers from fluctuations of demand. The answer we get from the data is positive, although less ro-

bust. We answered the second question (*What are the effects of a more costly buffer on firm performance?*) testing whether average productivity of labour is decreased by the reform, and whether total employment level is lower in firms that employ a less flexible labour force. A DID analysis showed that in the short run productivity is decreased, but not in the medium-long run. The effect is statistically significant, but not large. On the contrary, no significant effect can be estimated on the level of employment in the short run; however in the medium run, when there is no effect on productivity any more, there is a negative and significant (but quite small) effect on the level of total employment. Again these effects are increasing (in absolute value) with firm size. It seems that firms react to shocks adjusting organisation and output on intensive margins in the short run, and more rigid stocks on extensive margins later on. This is consistent with the prior that employment – regulated by permanent contracts – is a rigid factor of production.

Some caveats are necessary in order to draw policy implications from this study on more recent reforms. We are studying a case in which the flexible part of the labour force became less available and we obtain some negative and significant effects on firms' performance. More recently in Italy the flexible part of the labour force became more available, due to a wider availability of temporary contracts. We do not know whether the effects are symmetric, hence we can not infer directly that – now that we have more temporary contracts – productivity has grown in the short run and total employment has grown in the medium run. However, our results may provide indications in this sense. This is important because of the lack of formal evaluation of the effects of post 1997 reforms from the point of view of the firm, i.e. evaluations focussing on the direct effect on labour demand.

7 APPENDIX: The data

7.1 WHIP

WHIP is a random sample from all INPS archives (employees, self-employed, pensioners). People born on the 10th of March, of June, of September and of December have been selected and observed during the period 1985-2002. The resulting 1:90 sample has been reorganised into a longitudinal dataset, where the unit of observation is the work-history of the selected individuals.

We use the section of WHIP containing the employees of private firms. There is one record for each employment spell in the year. Each worker may be connected at any

point in time with his/her firm; it is therefore possible to assign employer's attributes to the employee.

7.2 Firm data

Our sample has been drawn from the INPS Archive of private firms with employees, and it has been linked to some balance sheet summary statistics³³.

The unit of observation is the firm. Each record contains (among other things) employer code, location, industry, monthly number of workers and their wage bill by occupation and contract. It has been linked to some balance sheet summary statistics (e.g. sales, import, export, number of self-employees).

The original dataset includes the population of firms in Torino, Parma, Caserta, Taranto. It covers the period January 1990 - December 1992.

From the original dataset, we have selected a balanced panel of existing firms (i.e. no entries or exits³⁴). This because entry and job creation (or exit and job destruction) are different decisions that in our opinion cannot be described by the same model (see for example Hamermesh, 1993, on this point); furthermore, the difference in differences analysis we perform requires data both "before" and "after" the event we are studying, i.e. a balanced panel.

Also, we have to exclude firms employing more than 200 employees in January 1990³⁵ because these are usually multi-plant firms. Each plant is likely to administrate its own workforce; hence pooling plants together - as INPS data do - could hide exactly what we want to study. Furthermore, these firms are usually located in more than one province; if this is the case they are not observed in our dataset based on four provinces only, hence reducing significantly its coverage in this size class. We are aware of the limitations imposed to our analysis by this (necessary) exclusion; notice, however, that on average only

³³It is courtesy of R&P srl that provided the matched and anonymous dataset.

³⁴Entries and exits are frequent events and involve a substantial part of the labour force, mainly among small firms. About 22% of firms employing less than 6 workers enters or exits the market every year. This figure drops to about 10% among firms employing 6-100 employees. Entries and exits of larger firms are often only apparent and mainly due to mergers, splits, legal transformations. Notice that even if our observation period is not long, the selection of existing firms might cause an over-sampling of "good" firms, because they could survive during a negative phase of the business cycle. However, this seems not to be the case in our sample. One way of checking this is to see if selected firms are larger, given their size class, compared to the population. Mean employment by size class is only marginally larger in the selected size classes in 1990 (our first year) and the difference between average size in the selected sample and in the population is decreasing over time, often becoming negative. This indicating that the selection is not strong and, more important, it is not worsening over time when the recession hits.

³⁵All included firms are kept for the following three years, even if they do not match the size criteria any more. This to avoid endogenous selection.

about 30% of employees of private firms works in firms above the 200 workers threshold. Finally we select manufacturing and private service sectors only. We exclude the construction industry because it includes mainly very small and short-lived firms (every new construction site may appear as a firm in an administrative archive).

After the described selections we have a balanced panel of 25,457 firms; 10,902 of them classified as manufacturing. 1,732 are located in Caserta, 1904 in Taranto, (i.e. 3,636 in the south of Italy), 4,227 in Parma and 17,594 in Torino. They are very small, small and medium size firms, in fact about 60% of our firms employs less than five workers, 10% of them employs more than 20 people. Table 8 illustrates the sample averages of some of the variables used in the estimates. There is a noticeable difference in size (measured by sales or employment) between firms that use temporary workers and have been affected by the reform with respect to all other firms. The difference is made of permanent workers, not of temporary ones. They also pay higher average wages to their employees, as usual in larger firms. Real wage of temporary workers is much lower than real wage of permanent workers in all groups; temporary workers are younger and have no tenure in the firm, hence they start from the bottom step of the wage scale. Both average sales per employee and the percentage of white collars in the firm are higher among firms that would have been affected by the reform, had they employed temporary workers.

Table 8: Sample description.

	year	no. firms	E	L	l	w(E)	w(L)	w(l)	S	S/E	white
users affected (T)	1990	4120	20.44	17.77	2.67	3379	3587	2439	5.16	0.262	0.53
	1991	4120	20.43	18.82	1.61	3550	3640	2617	5.11	0.264	0.53
	1992	4120	20.13	19.09	1.04	3558	3618	2546	5.78	0.305	0.54
users non affected (C)	1990	2002	9.04	7.09	1.95	2721	2875	2263	1.46	0.170	0.19
	1991	2002	9.01	7.80	1.21	2922	2989	2479	1.46	0.182	0.20
	1992	2002	8.86	8.06	0.80	2891	2941	2325	1.65	0.209	0.20
non users affected (T0)	1990	10328	5.03	5.03	0.00	2964	2964		1.68	0.365	0.57
	1991	10328	5.12	5.01	0.11	3081	3102	2236	1.71	0.356	0.58
	1992	10328	5.07	4.96	0.11	3080	3099	2341	1.86	0.385	0.58
non users non affected (C0)	1990	9007	4.08	4.08	0.00	2559	2559		0.64	0.199	0.13
	1991	9007	4.13	4.03	0.10	2665	2678	2084	0.64	0.197	0.14
	1992	9007	4.07	3.93	0.14	2647	2659	2244	0.70	0.215	0.14

w = yearly average gross wage, 0,000 lire

S = yearly average sales, billion lire

white: average pct. white collars

Source: Firm data

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Figures



Figure 1: Absolute changes in the employment level of young workers (under 30): temporary vs. permanent contract holders. Source: WHIP.

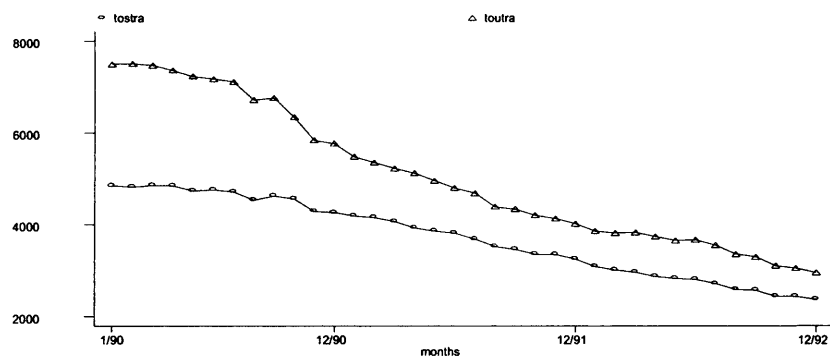


Figure 2: Total number of temporary workers over time. Torino (north west). Skilled (dot) and unskilled (triangle). Source: firm data.

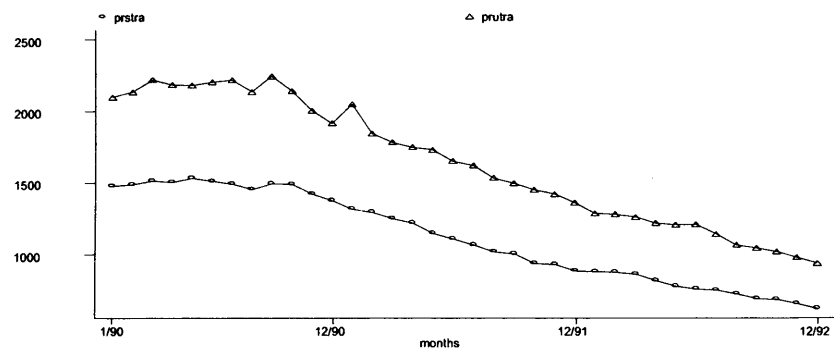


Figure 3: Total number of temporary workers over time. Parma (north east). Skilled (dot) and unskilled (triangle). Source: firm data.

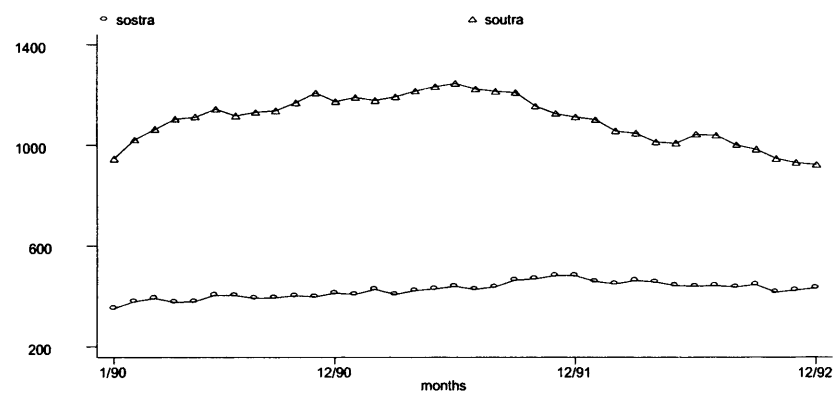


Figure 4: Total number of temporary workers over time. South (Caserta + Taranto). Skilled (dot) and unskilled (triangle). Source: firm data.

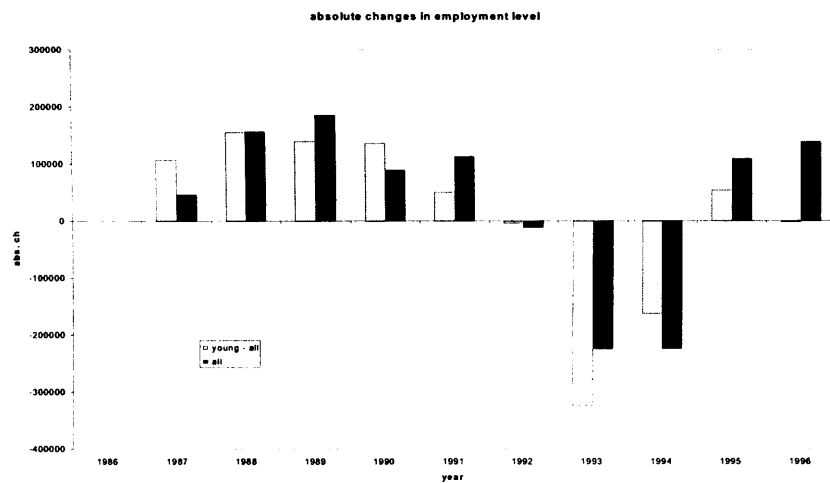


Figure 5: Absolute changes in employment level: all workers vs. young workers (under 30). Source: WHIP.