The Effect of Employment Protection Legislation on Inter-Industry Mobility of Workers

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Abstract

This study investigates the impact of Employment Protection Legislation (EPL) on inter-industry labor mobility, using Italy's 1990 reform (Law 108/1990) as a natural experiment. The reform increased severance pay requirements for small firms, creating a discontinuity that allows for a Differences-in-Differences analysis. Matched employer-employee data from the provinces of Treviso and Vicenza reveal a 1.3–2.0 percentage point reduction in the probability of inter-industry hiring, with a larger effect (1.9–3.1 percentage points) for job-to-job transitions. A survival analysis further shows that workers hired from a different industry face higher dismissal probabilities, suggesting that heightened dismissal costs reduce employer willingness to hire such candidates. The results, robust across multiple specifications and controls, highlight how stricter EPL heightens employer selectivity, influencing inter-industry skill flows and recruitment practices.

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1 Introduction

The level of employment protection legislation (EPL) is one of the key factors that influence labor market outcomes. Early studies have examined the effect of changes in EPL on flows and stocks of employment, unemployment, and labor force in general (e.g., E. P. Lazear 1990, S. Bentolila and G. Bertola 1990, Garibaldi 1998, Pissarides 2001, Blanchard and Portugal 2001, Kugler and Pica 2008, Di Tella and MacCulloch 2005), on wages (Van der Wiel 2010, Leonardi and Pica 2013).

Other works have studied how EPL impacts other labor and non-labor market outcomes. For example, Boeri and Jimeno (2005) study the effect of EPL on equilibrium firms' size, Autor, Kerr, and Kugler (2007) and Bjuggren (2018) examine the effect on productivity, Prifti and Vuri (2013) investigate the effect on fertility rates.

Another variable that is impacted by the level of employment protection legislation is the selectiveness of firms when hiring new employees. For example, Blackburn and Hart (2002)[1] present a survey of UK employers that shows how small firms managers claimed being more selective after the reform of Employment Protection in United Kingdom in 1999.

E. Lazear (1998) and Pries and Rogerson (2005) presented two different models to explain the impact of increased employment protection on recruitment practices. However, the only empirical studies that investigate this topic are Marinescu (2009) and Butschek and Sauermann (2022). The former exploits a British Reform in 1999, while the latter a Swedish reform in 2001. In the British case, the firing costs were increased and the paper shows an increase in recruitment quality, while in the Swedish case the firing costs were decreased and the paper shows a decrease in minimum hire quality.

This work contributes to this strand of literature investigating whether the same channel of increased selectiveness impacts the inter-industry flows of workers. In other words, I study whether employers are more or less prone to hire people from other industries following a change in Employment Protection Legislation.

I exploit a change in the level of EPL in Italy in 1990: Law 108/1990 increased the payment that the employer had to pay to unfairly dismissed workers in firms with up to 15 employees. The reform did not change the level of EPL for firms with more than 15 employees. This discontinuity allows the researcher to analyze the impact of such policy using a Differences in Differences approach. Depending on the specification, the treated group is the group of matches in firms with/firms with up to 15 employees. The control group is the group of matches in firms with/firms with more than 15 employees. The same policy change has been examined with similar DiD approaches by Kugler and Pica (2008), Leonardi and Pica (2013), and Prifti and Vuri (2013).

I use matched employer-employee data from the archives of the Italian National Security Agency (INPS). Data includes the universe of firms in the Italian provinces of Treviso and Vicenza. This allows the researcher to count the number of employees of the firm, essential to distinguish treated and control groups. The working history of each individual is needed to be able to identify the previous industry of employment.

First, I show that worker whose previous job was in a different industry are more likely to be dismissed. With higher firing costs, employers should be less prone to hire workers from different industries. I use a Differences-in-Differences approach to quantify this effect.

I find that the change in EPL decreases the probability of new matches being inter-industry by 1.3 to 2.0 percentage points, while the pre-reform probability is 43%. I also investigate matches that involve workers who did not experience a previous unemployment spell. I call these matches job-to-jobm matches. The effect is larger: I find a decrease of the probability of the new job-to-job matches being inter-industry by 1.9 to 3.1 percentage points.

1. This study is also quoted by Marinescu (2009)

The results are robust to the introduction of multiple controls and fixed effects, to the employment of both OLS and probit estimation of DiD, and to different definitions of treated and control groups.

This work develops as follows: Section 2 exposes the related literature, Section 3 presents the institutional background of employment protection in Italy and on the 1990 change. Section 4 presents the main data source, and some descriptives about workers and firms in the sample, Section 5 shows descriptive facts about inter-industry mobility of workers, Section 6 explains the empirical specification, Section 7 presents the empirical strategy, and Section 8 concludes.

2 Literature Review

This work contributes to the study of the effects of changes in the level of Employment Protection on labor market outcomes. This policy has been an object of interest for economists both under a theoretical and an empirical point of view. Extensive literature reviews on the topic have been collected by Addison and Teixeira (2003) and Cahuc, Carcillo, and Zylberberg (2014). This section also relies on the literature review section by Butschek and Sauermann (2022).

Early literature on EPL has studied its effects on *stock* measures in the labor market (i.e., employment, unemployment, labor force) and *flows* (e.g., from unemployment to employment, from inactivity to labor force). This literature exploits aggregate data on flows and stocks and include papers by Samuel Bentolila and Giuseppe Bertola (1990), E. P. Lazear (1990), Boeri (1999), Pissarides (2001), and Di Tella and MacCulloch (2005).

Early findings on EPL are based on cross-country regressions. For example, E. P. Lazear (1990) uses data from 22 countries and 29 years (between 1956 and 1984). He uses two indicators of EPL: *sev* and *notice*. *sev* is the number of months of salary given to workers as severance upon dismissal after at 10 years of work, and *notice* is the number of months of notice required before termination to workers with 10 years of service. He finds negative correlation of both with employment rate, activity rate, and hours of work. He finds positive correlation with unemployment rate. Depending on the specification considered, an additional month of severance pay is associated with a decrease of employment rate between 0.34% and 0.40%[2]. The effect on unemployment, despite being significant, is much smaller.

Boeri (1999) further investigates the role of EPL in determining labor market flows. He shows how lower labor market flexibility in Europe is not associated to low labor market turnover rates. This happens because of the increased amount of job-to-job shifts of people employed in short-term contracts that seek jobs together with unemployed people.

Di Tella and MacCulloch (2005) adopt a similar approach, but they employ a panel data at the country level that measures the flexibility in hiring and firing using a survey of employers. They find positive correlation between labor market flexibility and levels of employment and participation to the labor force: they claim that 14% of the employment gap between US and France is explained by higher flexibility in the US labor market. They do not find significant effects on inflow rates. They observe that more flexibility is associated with less unfilled vacancies. Lastly, they find that unemployment is less persistent in countries with more flexible labor markets.

Autor, Donohue III, and Schwab (2006) is one of the first papers that use microdata (in this case, Current Population Survey) to investigate the effects of EPL. In particular, they exploit the staggered nature of the application of EPL laws in the US and shows negative impact of stricter EPL on the employment rate, with more pronounced effects for female, younger, and less-educated workers.

2. The decrease drops to 0.14% when the variable *notice* is also included among the regressors.

Available data and the nature of the policy change of interest do not allow me to exploit a staggered implementation of the policy. Other studies have exploited an isolated policy change to study the effect of EPL on multiple outcome variables. To my knowledge, extensively investigated policy changes are the Spanish liberalization of fixed-term contracts (García-Pérez, Marinescu, and Vall Castello 2019), the UK Reform that increased EPL for short-tenured workers in 1999 (Marinescu 2009), the Swedish Reform in 2001 that relaxed the LIFO rule for collective lay-offs (Lindbeck, Palme, and Persson 2006, Olsson 2009, Olsson 2017, Bjuggren 2018, and Butschek and Sauermann 2022[3]), the Italian Jobs Act in 2015 that, among other things, introduced a contract with job security increasing in tenure (Boeri and Garibaldi 2019, De Paola, Nisticò, and Scoppa 2021).

In particular, this work contributes to the literature on the effects of the increase in EPL for small firms occured in Italy in 1990. Previous papers have studied its effects on firms' size (Schivardi and Torrini 2004, Garibaldi, Pacelli, and Borgarello 2004), hiring and separation rates of workers, entry and exit rates of firms (Kugler and Pica 2008), on wages (Leonardi and Pica 2013), on fertility rates (Prifti and Vuri 2013), and on firms' capital-labor ratio and productivity (Cingano et al. 2016).

The strand of literature about the effects of changes in EPL on recruitment practices is not extensive. Theoretical models are proposed by E. Lazear (1998), Kugler and Saint-Paul (2004), and Pries and Rogerson (2005). Empirical evidence is shown by Kugler and Saint-Paul (2004), Marinescu (2009), Bjuggren (2018) and Butschek and Sauermann (2022). To my knowledge, there is no study that investigates the effect of EPL on inter-industry mobility of workers through the lens of change in recruitment selectiveness.

E. P. Lazear (1990) frames the selection of a new employer as the choice of an option: if the employee turns out to be less productive than the level at which she would be profitable, the employer can fire her. Thus, with no to low firing costs, the employers may prefer more risky workers, while this preference may decrease if firing costs increase.

Pries and Rogerson (2005), on the other hand, use a matching model in which the quality of the firm-worker match is not known before the match. Firms and job'seekers know the same probability that a match turns out to be good or bad, but the quality of the match can be discovered only by engaging in production. They analyze how this model would respond in presence of unemployment insurance, dismissal costs, and minimum wage. They also study the interactions of these policies. With respect to dismissal costs, they show that higher dismissal costs imply higher threshold of the quality signal that both job-seekers and firms observe as this imply lower probability of paying dismissal costs.

Marinescu (2009) and Butschek and Sauermann (2022) use two different policy changes (respectively, an increase in EPL in UK in 1999 and a decrease in EPL in Sweden in 2001) to study the impact of EPL on match quality.

The policy change exploited by Marinescu (2009) is a decrease in the qualifying period to sue the firm for unfair dismissal, in case of dismissal. In particular, this threshold went from 24 to 12 months, making the workers with a tenure between 1 and 2 years covered by higher job security. She finds that the firing hazard for workers between 0 and 1 years of tenure decreased by 19% with respect to workers with 2 to 4 years of tenure. Workers with 0-1 years of tenure did not see their job security changed, and thus it is likely that this effect is due to improved match quality. She also finds a small increase in the probability that the worker gets some form of training on the job.

On the other hand, Butschek and Sauermann (2022) exploits a relaxation of the LIFO rule in case of collective dismissals for firms with less than 11 workers in Sweden in 2001. They measure

3. This list if provided by Butschek and Sauermann (2022)

the quality of workers with worker fixed effects (AKM) and scores from different tests (cognitive and psychological from military draft and GPA). They find a 5% decrease in the minimum hire quality (i.e., the quality of the hire with the minimum quality at the firm-year level). They use a simulation in which they reshuffle the quality of hires keeping the number of hires constant at the year-firm level to show that half of the effect they find is due to an increase in hiring and half to a decrease of firm selectiveness. They also show that this effect is more consistent with Pries and Rogerson (2005) than with E. Lazear (1998), as the maximum hire quality does not increase.

This work builds a bridge between the literature on EPL and, more specifically, its effects on recruitment strategies, and the literature on specificity of human capital. There are multiple studies that discuss the extent to which human capital is firm-, industry-, or occupational-specific.

One of the firs studies on industry-specificity of human capital is Neal (1995), who uses data of displaced workers in the US to show that displaced workers who find a job in pre-displacement industry experience higher returns with respect to both their pre-displacement job and the displaced workers that went into a different industry. This suggests that human capital specificity is not general, but also not firm-specific. On the other hand, Kambourov and Manovskii (2009) use PSID data and show that occupational tenure is more important than industrial tenure in determining wages. In any case, with the available data, I am not able to distinguish occupations. Thus, I cannot take take into account occupational mobility.

The heterogeneity results of this work exploit the conclusions of Sullivan (2010) who show that the industry specificity of human capital is higher for white-collar workers with respect to blue-collar workers. Similar conclusions can be drawn from Neffke, Otto, and Weyh (2017).

3 Institutional Background

3.1 Evolution of Italian Legislation

In this subsection, I present the evolution of Employment Protection Legislation in Italy, from 1966 (first law on dismissals) to the 1990s. The institutional background until 1990s is useful to frame the empirical strategy adopted in this work. The evolution that followed is useful to emphasize the policy relevance of this work and the limitations to external validity. The main references for this subsection are De Luca (2013) and Amoroso (2022).

The first law about workers' dismissal has been approved in 1966 (Law 604/1966). Article 3 states that a worker can be dismissed either due a subjective or an objective reason. The subjective reason consists of a clear failure to fulfill contractual obligations, while the objective reason is related to the economic, organizational, or productive situation of the firm. The dismissed worker can appeal a courthouse against the dismissal. This circumstance is regulated by Article 8. If the dismissal is proven to be unfair, the employer could choose between re-instating the worker and pay them compensation (between 5 and 12 monthly wages, taking into account firm's size and worker's tenure). The maximum payment cannot be higher than 8 months if the tenure of the worker is shorter than 30 months and can be increased to 14 months if the worker's tenure is longer then 20 years. Minimum and maximum compensations are halved if the employer employs up to 60 workers. Article 11 exempted employers with less than 35 employees from these provisions.

In 1970, the Italian Parliament approved Law 300/1970 (so called *Statuto dei Lavoratori*). In particular, it updated the provisions about unfair dismissals, making the re-instatement compulsory (Article 18). Moreover, the employer had to pay compensation of the damage (not less than 5 monthly wages). Article 35 limited the application of such provisions to establishments who employed more than 15 employees.

This work exploits a change in the Employment Protection level for firms with up to 15 employees occurred in Italy in 1990. Indeed, Law 300/1970 was updated by the Law 108/1990. In particular, Article 18 introduced provisions in cases of unfair dismissals of permanent workers for employers with up to 15 employees. The employer had to pay between 2.5 and 6 monthly wages to the unfairly dismissed worker (Kugler and Pica 2008). Notice that Law 108/1990 did not extend the provisions of big firms to small firms, as the re-instatement was still not compulsory for small firms (again, Kugler and Pica 2008).

The law has been approved after multiple referendum proposals (1981-1982, not accepted by the Constitutional Court[4] and 1989-1990, accepted by the Constitutional Court[5]). However, Law 108/1990 was approved by the Parliament before the referendum took place as the legislator wanted to avoid to leave such subject to the referendum outcome (Amoroso 2022). Indeed, the Central Office for Referendum (within the *Corte di Cassazione*) cancelled the referendum.

Despite the referendum proposals, Leonardi and Pica (2013) argue that the policy change was not anticipated: they notice that the first news about the possibility of such change was found on the pages of *Il Sole 24 Ore* in January 1990.

3.2 International Comparison

Compared to other countries, the level of employment protection in Italy legislation has always been very strict. Emerson (1988) collected multiple indicators that rank Italy as the strictest country in the former European Community in terms of barriers to firing. These indicators are percentage of employees holding jobs for less than 2 years, percentage annual average of new recruits and separations, percentage of unemployed who became so because of dismissal or redundancy (from Eurostat Labour Force Survey), and percentage of employers that believe there is not enough flexibility in hiring (from CEC 1986).

Grubb and Wells (1993) analyzed EPL strictness and barrier to firing in OECD countries according to three areas: procedural delays and complications, notice and severance pay, and difficulty of dismissal. Again, Italy was the strictest country in terms of notice and severance pay, the second strictest (after Portugal) in terms of difficulty of dismissal, but among the least strict in terms of regular procedural inconveniences. According to the overall ranking, Italy had the third strictest EPL, after Spain and Portugal. This is consistent with a pattern that is outlined by the authors: countries may require employers to go through complex procedures before dismissal, or may impose high costs after the dismissal (severance pay, consequence in case of unfair dismissal), but rarely both. Grubb and Wells (1993) mention Netherlands as a country which had significant procedural delay and complications (pre-dismissal), but low costs post-dismissal.

OECD 1994 introduced a variant of the Grubb and Wells (1993) index. Addison and Teixeira (2003) underlines two major differences between the two: restrictions to working hours and regulation of temporary agency work are not considered and the sample is extended by 5 countries (Australia, Finland, Norway, Sweden, and Switzerland).

The OECD index is periodically updated (OECD Employment Protection Database) and allows to track trends of employment protection legislation at the country level. Italy, Portugal, and Spain remained the 3 OECD countries with the strictest employment protection of regular employment until 2003, when Germany increased its level. Italy, Portugal, and Spain lowered the strictness of EPL after the 2008 crisis (Cahuc, Carcillo, and Zylberberg 2014).

4 Data

Veneto Workers' Histories is provided by Fondazione Rodolfo De Benedetti. Details about the dataset are provided in Occari, Tattara, and Volpe (2001), that is also the main source for this paragraph. The dataset has been developed by the Economics Department in Universita Ca' Foscari Venezia under the supervision of Giuseppe Tattara. The dataset is an employee-employer

4. Decision 27, 1982 [Constitutional Court website]
5. Decision 65, 1990 [Constitutional Court website]

matched dataset extracted from the administrative archives of the Italian National Social Insurance Agency (INPS) and it covers the universe of the firms that were active in the provinces of Treviso and Vicenza between 1975 and 2001. The worker enters the dataset as soon as she spends one day in one of the firms. After that, she is followed through her career even if she works for firms outside of the original sample. The new firms that will be reached by this worker will then be included in the firms' archive. However, I usually do not include those in the analysis to avoid incurring in selection bias problems. The dataset does not cover the firms for which the social insurance is not managed by INPS. Moreover, they do not cover agricultural firms and public administration. Occari, Tattara, and Volpe (2001) notice that the most relevant sectors that are not included are public healthcare and public railways. Lastly, firms with no employees are not included.

Data are provided to INPS by the firms. Information about the firm is collected through the DM10 form, while information about the workers is collected from O1M forms. The former is used to compute the amount of money that the firm should transfer to INPS (as firms collect workers' social insurance contributions). The O1M form is instead used to compute the accrued entitlements for pension purposes for each worker.

The dataset is organized in three different tables (`azien`, `anagr`, and `contr`). The list of variables in each 3 is provided in Table 8. `azien` collects information about the firms, `anagr` information about the workers, and `contr` information about the matches.

For each firm, the dataset provides the name, the address, the birth and termination dates, and industry code (both ATECO '81 and '91) and a description of the economic activity. The dataset provides two firm identifiers: INPS Firm Code and VAT code. The latter is particularly useful to merge information from other data sources (e.g., balance sheet information). In this work, however, I do not include additional firm-level information, because I do not have access to firm-level data before 2013. The original dataset files in `.dta` suggest the researcher not to use the variable ATECO '91, i.e., the industry code based on the 1991 classification. However, I have contacted Giuseppe Tattara, who has supervised the creation of the dataset, and he confirmed that the variable can be used. Moreover, the same variable is employed in multiple published papers (e.g., Card, Devicienti, and Maida 2014, Bartolucci, Devicienti, and Monzón 2018).

For each worker, the dataset provides information about gender, year of birth, birth place, nationality, and place of residence. As explained above, the individual is registered in the dataset once she works at least for one day in a firm in the original dataset. For example, even if a worker lives in the provinces of Treviso and Vicenza, but always works outside between 1975 and 2001, she will never be observed in the dataset.

For each match, the dataset provides information about the duration, number of weeks and months paid, wage level, collective contract type, qualification, type of relation, date of ceasing, and working weeks[6]. Notice that, since the data originate from administrative procedures, they do not come with information about education levels or occupation. Each individual is not observed when she is out of the dataset. Thus, one can not distinguish whether she is unemployed, inactive, self-employed, or retired. One can exclude that she is retired if she comes back into the dataset at a later stage. The variable about worker's qualification classifies the job in blue-collar workers, white-collar workers, and managers and part-time or full-time. The information about whether the contract is temporary, permanent, or seasonal is available starting 1998 (Cascioli 2006). However, it is common practice to use the variable `tipo_rap` to approximate the classification in open-ended and fixed term contracts[7]. In particular, the variable `tipo_rap` specifies whether the contract is subject to incentives for hiring through social security contribution relief (it is common in cases

6. The difference between working weeks and weeks paid (that are FTE weeks) allow the researcher to distinguish between full- and part-time contracts.

7. Distinguishing between the two is necessary because the policy change examined in this work impacted the permanent contracts only.

of employees hired under training contracts). I assume that all the contracts that did not receive any incentive are open-ended.

Information about workers and matches come from Report O1M, that is submitted by the firm to INPS. This report is needed to compute the benefits (e.g., retirement and unemployment) that INPS will pay to the worker. As clearly explained in Cascioli (2006), the submission of the Report is compulsory for those firms whose employees' social insurance must be provided by INPS. Moreover, she also notices that the dataset may not include, in year t, those employees that have been paid directly by INPS during the entire year t. The cases in which the employee is entirely paid by INPS, while working for a firm include 100% payroll subsidy (in Italy, *Cassa Integrazione*), some cases of sick or accident leave and maternity leave.

I arrange the data to obtain a quarterly panel data at the quarter-worker level. Thus, the dataset contains one record for each worker for each quarter, starting the month she is observed working for one of the firms in the sample for the first time and ending the month she leaves the last job for which she is recorded. Remember that this does not mean that she is retiring, as she could becoming self-employed, inactive, unemployed and then retiring, etc. To organize the dataset in this way, I need to provide criteria to attribute an employment situation for each quarter to each worker:

1. If a worker i is observed working for at least one day in quarter t, she is considered employed in that quarter
2. If a worker i works only for firm j during quarter t, she is considered working for firm j during the entire quarter
3. If a worker i is observed working for both firms j and k during quarter t, multiple cases are contemplated.
   - If the number of months during which worker i worked for at least one day during the quarter in each company is different, the worker is considered working the entire quarter in the company at which she is recorded working for at least one day for at least 2 months.
   - If the number of months during which worker i worked for at least one day during the quarter in each company is the same, the worker is considered working the entire quarter in the company where she earned the highest weekly wage

4.1 Firms

The firms included in the sample are 1,126,568. The set of firms can be divided in two subsets, the *original* and the *derived*. A firm in the sample is either part of the former or the latter, it cannot be out of both or in both. A firm is part of the *original* subset if it is resident in the provinces of Treviso or Vicenza and it has been active for at least one day between 1975 and 2001. A firm is part of the *derived* dataset if it has employed, for at least one day between 1975 and 2001, one of the workers that has previously worked in a firm of the *original* subset between 1975 and 2001. A large number of firms has always had only one employee. These firms are not considered in the causal analysis, to avoid selection bias, and Table 1 report their incidence by 1-digit industry codes.

Figure 5 shows the number of active firms by year and by subset. You can observe that the number of firms in the original dataset increases between 1975 and 2001 and, while the growth pace decreases slowly, there is no shock around 1990. This is good for the robustness of our results, that focus on a 8-year window around 1990. Figure 6 shows how firms are distributed by provinces. As expected, one can notice that they concentrate in Veneto and Northern Italy in general.

Table 1 reports descriptive statistics about the number of employees and wages at the industry level for the firms that have been active between 1986 and 1994. The number of employees is not affected by the few big firms with more hundreds of employees, because it is computed winsorizing the employment level. Observing the average firm size one can realize how relevant has been a reform that has targeted firms with less than 15 employees, as the reform that we study in this paper has done. The table also allows the reader to compare firms in the original dataset with those in the derived dataset. In particular, it is more likely that a firm in the derived dataset is a 1-firm employee. This is particularly true for Hotels and Restaurants, Constructions, Transportation, and Trade[8]. The average number of employees is lower in all industries in the *derived*, but not statistically different. The wage level is higher in almost all industries, but not statistically different. The incidence of industries is similar, except for Manufacturing (42.9% in the original dataset and 29.6% in the derived one) and Construction (8.8% in the original dataset and 16.2% in the derived one).

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		1-employee firms (%)		Number of employees		Wages		% on firms	
1-digit ATECO code		O	D	O	D	O	D	O	D
A	Agriculture, hunting	27.6	49.7	5.4	4.1	651.5	636.7	3.4	6.8
				(4.7)	(4.1)	(181.2)	(161.0)		
B	Fishing	0.0	65.5	2.8	4.1	.	283.1	0.0	0.4
				(.)	(4.7)	(.)	(103.1)		
C	Mining	11.4	60.1	7.5	3.9	730.1	723.8	0.2	0.3
				(6.0)	(4.2)	(126.3)	(152.1)		
D	Manifacturing	16.2	54.4	9.2	6.2	597.8	632.9	42.9	29.6
				(8.6)	(7.2)	(141.0)	(191.5)		
E	Water	13.6	45.6	15.0	12.4	905.9	964.4	0.0	0.1
				(23.0)	(20.1)	(155.3)	(171.7)		
F	Construction	27.1	60.7	4.5	3.4	590.8	600.6	8.8	16.2
				(3.6)	(3.1)	(98.2)	(126.6)		
G	Trade	36.8	64.3	4.2	3.7	640.1	654.8	18.1	15.6
				(3.8)	(3.5)	(122.2)	(144.4)		
H	Hotels and restaurants	39.1	62.0	3.1	2.8	497.6	538.5	5.3	9.1
				(2.1)	(2.0)	(111.1)	(111.9)		
I	Transportation	32.9	57.4	5.2	4.5	697.2	707.0	2.2	3.2
				(5.3)	(5.2)	(174.8)	(237.7)		
J	Finance	40.2	62.0	4.1	4.0	773.4	897.1	3.3	3.1
				(4.1)	(4.2)	(309.2)	(346.7)		
K	Misc1 see caption	40.6	63.2	3.7	3.6	557.1	545.7	7.3	6.4
				(3.5)	(3.5)	(177.8)	(205.6)		
L	PA	5.4	55.9	12.1	4.0	617.9	598.6	0.6	2.0
				(5.8)	(4.9)	(62.4)	(97.9)		
N	Healthcare	31.4	57.9	4.5	3.6	476.8	489.0	3.2	2.8
				(4.4)	(3.8)	(163.8)	(181.2)		
O	Misc2 see caption	42.9	54.3	5.5	4.8	545.2	667.8	4.8	4.4
				(6.6)	(5.6)	(204.8)	(343.1)		
Total								70,367	487,038

Table 1: *Descriptive Statistics for firms in the sample between 1986 and 1994. This is the time span for the baseline specification in this paper. Columns (1), (3), (5), and (7) report statistics for the original subset (firms in Treviso and Vicenza). Columns (2), (4), (6), and (8) report statistics for the derived subset. Columns (1) and (2) report the percentage of firms that have always had at most 1-employee between 1986 and 1994. The number of employees are obtained as the average of the number of employees of each firm, windsorized at the 2.5% and 97.5% levels. In turn, the number of employees for each firm is the average number of employees between 1986q1 and 1994q4. The weekly wage is obtained as the weigthed average of weekly wages paid in each firm. Weight is average number of employees. In turn, the weekly wage at the firm-level is obtained as the average wage between 1986 and 1994. 1-digit ATECO code K includes real-estate, leasing, IT, R&D, and other professional and entrepreneurial activities. 1-digit ATECO code O includes waste disposal, membership organizations, cultural and recreative activities, sport, and other services.*

8. The difference between column (2) and (1) is wider for other industries (e.g., fishing, mining, water supply, and PA). However, the number of firms in those industries that are present in the sample do not allow us to draw conclusions.

4.2 Workers

The workers included in the sample are 3,650,312. A worker enters in the dataset if it has worked at least one day in one of the firms in the *original* dataset. After that, she is followed throughout her entire working life (at least until she is covered by INPS social insurance). 59% of the workers are male, 41% are female. Figure 7a visualizes the number of active workers in the provinces of Treviso and Vicenza between 1986 and 1994 by gender. Figure 7b visualizes the number of active workers in the two provinces by age class. One can notice that no significant shock occurred and no emphasized trend is in place during the said time span. This is a good point in favor of the empirical specification that is adopted in this work.

Figure 8 visualizes the number of workers by qualification. Most of the workers are blue-collar workers (61-64%), followed by white-collar workers (24-26%), and apprentices (5-10%). One can also observe a constant increase in the number of part-time workers (from 1.8% at the beginning of 1986 to 6.8% at the end of 1994).

4.3 Limitations of the Dataset

The dataset that is used in this work is particularly rich, it is derived from administrative data, and spans a wide time window. However, there are some limitations that restrict the external validity of the results. In any case, we need the universe of workers to compute the number of employees for each firm. This is, to my knowledge, the only easily accessible data of this kind.

One limitation of this dataset is the limited geographical scope, and the fact that the labor markets in the two provinces of Treviso and Vicenza may have peculiar features that do not allow to draw general conclusions about labor markets in Italy. Figure 1 plots the activity, employment, and unemployment rate for Veneto and Italy (for the entire population, for the young people, and for women). One can notice that (i) the activity and employment rates have always been higher in Veneto, and (ii) the levels of employment and unemployment remained more stable around 1990 in Veneto compared to Italy in general. This is another point in favor of the empirical specification adopted in this work. Leonardi and Pica (2013) observe that the high concentration of small firms and the tight labor market in the Italian North-East make conclusions based on this data likely to apply to other labor markets outside Veneto (e.g., manufacturing regions of France and Germany).

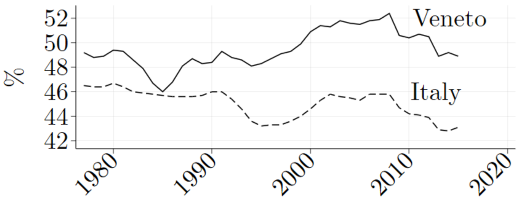
Another limitation of the dataset is that it stops in 2001. It is a significant limitation in our case, because one cannot use the dataset to study more recent changes to employment protection legislation in Italy (e.g., 2008, 2015).

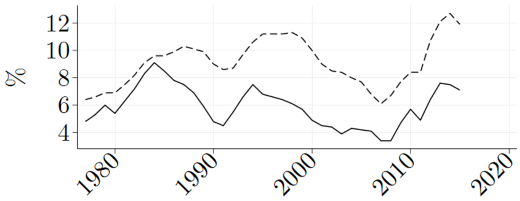
Lastly, the fact that I am not able to merge information from balance sheet does not allow me to study whether the effect of the EPL policy change is heterogeneous depending on multiple firm-level characteristics (e.g., balance sheet size and performance).

5 Descriptive Facts on Inter-Industry Mobility of Workers

In this section, I present some descriptive statistics about the inter-industry mobility of workers, that are going to be useful to interpret the main empirical results. In particular, in this section I show that high-wage workers moves across industries less frequently than low-wage workers (consistently with previous literature), the probability of going into unemployment is higher for workers who come from a different industry, but entry wages do not differ much.

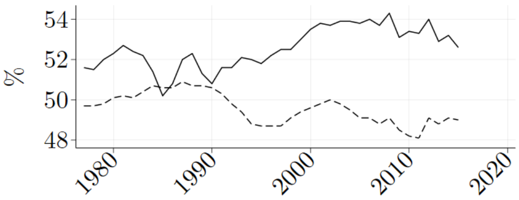
It is very frequent that a worker switches from one industry to the other. Figure 1 shows that, between 1975 and 2000, the percentage of workers who had a separation within the year has been between 20 and 25 percent. A growing share of these separations is represented by job-to-job transitions to firms in other industries (at the 2-digit ATECO 91 code). The percentage of workers

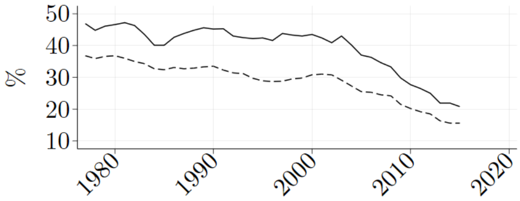




(a) Employment Rate

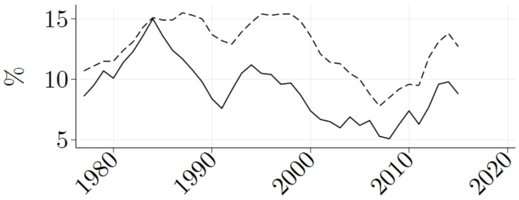
(b) Unemployment Rate

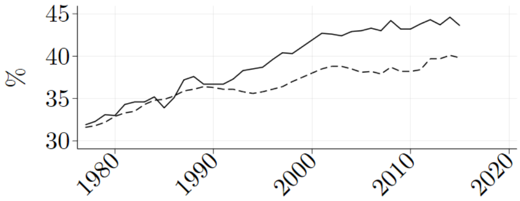




(c) Activity Rate

(d) Youth Employment Rate





(e) Female Unemployment Rate

(f) Female Activity Rate

Figure 1: *Employment, unemployment, activity, youth employment, female employment, and female unemployment rates in Veneto (solid line) and Italy (dashed line). Annual frequency, between 1977 and 2015. Youth defined as 15-24. Data from ISTAT (here)*

who experienced a job-to-job transition was around 7% in 1975 and around 16% in 2000. The percentage of workers who experiences a job-to-job transition to other industries was around 3% in 1975 and 9% in 2000.

Table 2, on the other hand, shows the number of transitions between industries in the dataset (in the pre-reform period, years 1975-1988), by wage levels. In particular, I consider all job changes that can be detected in the dataset. I assign them to a wage tertile and I observe the previous industry of the worker. I find that low wage workers switch industries much more than high wage workers. This is consistent with Neffke, Otto, and Weyh (2017) and Sullivan (2010). The inter-industry job transitions at the 2-digit ATECO 91 codes are 65% of the total for low wage earners, 50% for medium wage earners, and 46% for high wage earners.

In presence of industry-specific human capital, the expected productivity of the potential employee

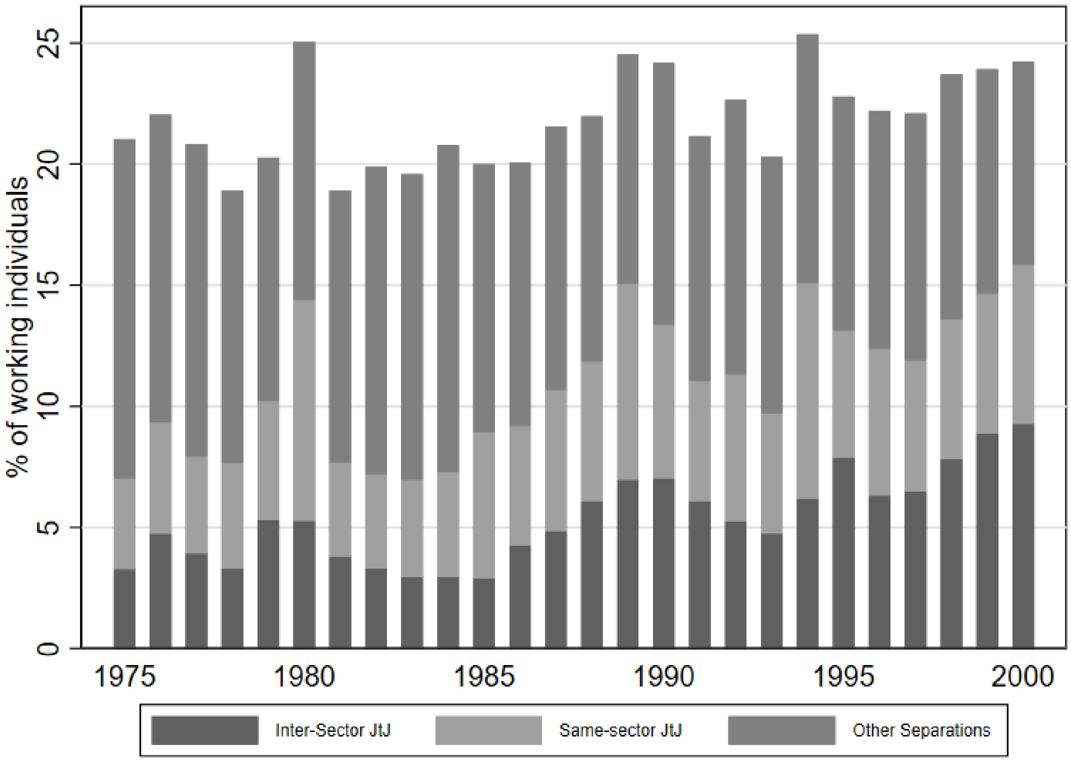


Figure 2: *Proportion of Workers that had a separation within the year. Annual data between 1975 and 2000. Job-to-job separations are the separations that became job-to-job shifts by the next quarter. Sectors are distinguished using 2-digit ATECO '91 codes. The last exit from the dataset is not considered, to avoid counting retirement as a separation.*

Transition	**All**	**Low Wage**	**Medium Wage**	**High Wage**
Same Industry	217,976	53,057	79,194	85,699
6-digit change	44,902	11,361	17,031	16,510
4-digit change	19,007	5,621	7,225	6,159
2-digit change	324,823	130,128	102,761	91,906
Total	606,708	200,167	206,211	200,274

Table 2: *Number of Job Transitions between 1975 and 1988 in the dataset. Selected transitions are new hirings in firms within the provinces of Treviso and Vicenza, the match last at least 4 quarters, are regulated by an open-ended contract, do not represent the first match of the worker, and occur in firms with less than 35 employees. Weekly wage is winsorized at the 2.5 and 97.5% level. A new match is low wage if the entry wage belongs to the first tertile of the wages on that year. A new match is medium wage if the wage belongs to the second tertile. A new match is high wage if the wage belongs to the third tertile. Industry codes used here are ATECO 91.*

is higher if she has experience in the same industry, *ceteris paribus*. Now, suppose that firm j employs N employees. The set of employees is E. Some of them (those in the set $A \in E$) have previous experience in the same sector, while some (those in the set $B \in E$). In the presence of industry-specific human capital, the productivity of workers in A is higher than the productivity of workers in B. This implies that the firing probability of group A is lower. Indeed, suppose that the productivity of the worker can be hit by a negative random shock (the distribution of which does not depend on the match being inter-industry or not). If the productivity is lower, the probability that the shock makes keeping the worker unprofitable for the firm is higher.

I check that this fact holds in the data. In other words, I am interested in observing whether it is more likely for a worker who comes from a different industry to be dismissed. There are two empirical issues.

First of all, in the dataset I do not observe the reason behind the separation: I am not able to distinguish a quit from a dismissal, or a dismissal because of objective reasons (i.e., economic, productive, or organizational issues) from a dismissal because of subjective reasons (i.e., failure to fulfill contractual obligations). In order to distinguish a quit from a firing, I isolate those matches that are followed by at least 3 quarters of unemployment. Indeed, it is more likely that a quit is followed by a job-to-job transition. I choose three quarters because, given the way the dataset is structured, it is the lowest threshold that ensures that the individual has spent at least 3 months out of work.

The second issue is that the firing rate may be explained by other variables, such as age, growth in wage throughout the match, size of the firm. As a result, one cannot only compare the survival rates. As a result, I employ a Weibull proportional hazard model. The variable of interest is *Different Industry* (1 if the worker comes from another industry, 0 otherwise), age of the worker (at the end of the match), change in wage (between the beginning and the end of the match), number of employees. I also add qualification, industry, and quarter fixed effects.

Even with these two adjustments, the estimation is not perfect, due to selection bias. Indeed, in order to select the matches that were followed by 3 quarters of out-of-work, I am only considering matches that ended before the period covered by the dataset (1975-2001). Moreover, since I am excluding the first match observed in the dataset for each worker, I am selecting short matches. This explains the steep survival function plotted in Figure 3. Moreover, the sample includes matches from periods that had different EPL strictness (e.g., pre- and post-1990). I tackle this issue estimating the same model including matches in firms with more than 15 employees (that were not affected by the reform in 1990).

Table 3 reports the results of the Weibull proportional hazard model. The point estimates are for hazard ratios. If the hazard ratio for a variable is less (more) than 1, it means that an unitary increase in that variable is associated with a reduction (increase) in the hazard of contract termination. If the hazard ratio is 1, it means that a unitary increase in the variable is associated with no effect on the hazard of contract termination.

Table 3 shows that the fact that the worker comes from a different industry is associated with a firing hazard that is from 4 to 9% higher compared to a worker coming from the same industry. The magnitude and the sign of the result is robust to a set of robustness checks (different fixed effects and covariates included, different sample of firms).

Now that I have shown that it is likely that industry of previous occupation plays a role in the productivity of the worker at the new firm, I proceed with the main empirical specification of this work, the aim of which is to understand whether an increase in job security reduces the probability of being hired from a different industry.

6 Empirical Specification

This work exploits a policy change to the workers' dismissal rules that occurred in Italy in 1990. The change regards unfair dismissals: before 1990, firms with more than 15 employees[9] had to reinstate the unfairly dismissed worker and pay them compensation. On May 10th, 1990, the Italian Parliament passed a bill that introduced payments in case of unfair dismissal for firms with up to 15 employees. The employer had to pay between 2.5 and 6 monthly wages to the unfairly dismissed worker. Moreover, the law updated the rule to count the number of workers, including employees in training. The same policy change has been studied by Schivardi and

9. The threshold is decreased to 5 employees for agricultural firms. However, they are not included in the dataset.

	All Firms			Firms with more than 15 empl.		
	(1)	(2)	(3)	(4)	(5)	(6)
Different Industry	1.04	1.05	1.08	1.08	1.09	1.09
	(1.02;1.05)	(1.02;1.07)	(1.05;1.10)	(1.06;1.11)	(1.06;1.12)	(1.05;1.13)
Age	0.96	0.96	0.97	0.97	0.97	0.97
	(0.96;0.97)	(0.96;0.97)	(0.97;0.97)	(0.96;0.97)	(0.96;0.97)	(0.97;0.97)
Change in wage		1.00	1.00		1.00	1.00
		(1.00;1.00)	(1.00;1.00)		(1.00;1.00)	(1.00;1.00)
Number of Employees		1.00	1.00	0.06	1.00	1.00
		(1.00;1.00)	(1.00;1.00)		(1.00;1.00)	(1.00;1.00)
Qualification FEs			✓			✓
Industry FEs			✓			✓
Quarter FEs			✓			✓

Table 3: *Results of Weibull survival model for duration of matches. The table reports the hazard ratio. The covariates are age (at the end of the match), difference in industry (i.e., whether the previous match of the worker was in the same 2-digit ATECO 91 industry (variable has value 0)), change in wage (i.e., difference between wage at the end of the match and wage at the beginning), and number of employees (measured as the firm's minimum number of employees observed in the dataset). Fixed Effects for qualification (blue-collar or white-collar), industry (at the 2-digit ATECO 91 level), and quarter. Columns (1)-(3) report results for all firms with more than 2 employees, columns (4)-(6) report results for firms with more than 15 employees. Matches included are all open-ended matches after the first observed in the dataset for each worker (years 1975-2001) and that are followed by a period of out-of-dataset (most likely unemployment) of at least 3 quarters. 95% confidence intervals in brackets. Bootstrapped SEs. For computational reasons, the sample is a random subsample of the dataset: I randomly select 1/3 of the firms that are active in the provinces of Treviso and Vicenza.*

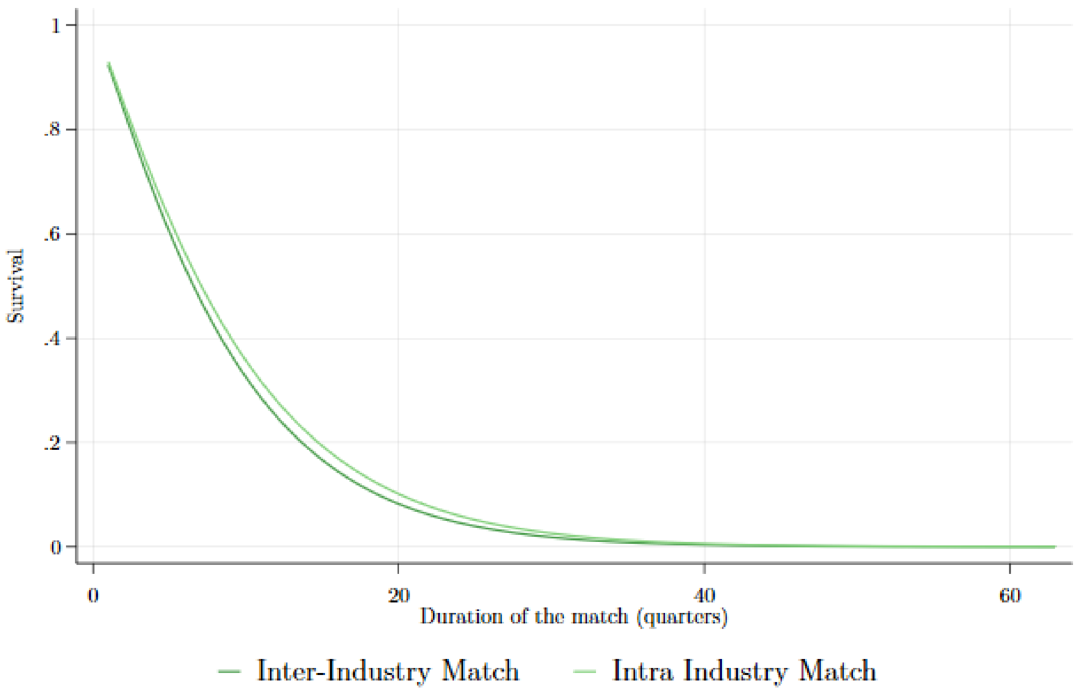


Figure 3: *Survival Function of Matches, for inter'industry and intra-industry matches, controlling for changes in wage, number of employees, qualification, and industry. The function is estimated using a sample of firms with more than 15 employees, active in the provinces of Treviso and Vicenza. A match is included if it is not the first match of the worker observed in the dataset and it is followed by a period out-of-dataset of at least 3 quarters.*

Torrini (2004), Garibaldi, Pacelli, and Borgarello (2004), Kugler and Pica (2008), Leonardi and Pica (2013), Prifti and Vuri (2013), and Cingano et al. (2016).

In this work, I try to estimate the effect of this change on inter-industry mobility of workers. Given that the change of industry can only happen when a new match is formed, I re-organize the dataset keeping only the observations that correspond to new matches. In particular, the new matches of interest (a) last for at least 4 quarters[10], (b) are regulated by an open-ended contract, (c) are not the first match of the worker, (d) occur in firms with less than 35 employees, (e) occur in firms located within the provinces of Treviso and Vicenza.

The main empirical specification is a differences-in-differences, that uses the threshold of 15 employees to discriminate firms into treatment and control group. The year of treatment is 1990. I exclude it to avoid misleading effects of possible anticipation effects.

The general form of the specification that I employ is

$$I = \tau_t + \gamma_k + \phi_r + \beta' X_{ijt} + \delta_1 D_j^S + \delta_2 (D_j^S \times Post_t) \tag{1}$$

where I is the measure of inter-industry mobility; i is the worker's subscript, j the firm's, and t the quarter's. Notice that the triple ijt univocally identifies the match, since I cannot have more than one match per quarter-worker, given the way I have organized the dataset. τ_t are time fixed-effects, γ_k the sector fixed-effects, ϕ_r the city fixed-effects. X_{ijt} is a matrix of covariates, that are discussed below, D_j^S is a dummy that takes value 1 if the firms has less than 15 employees and 0 otherwise, $Post_t$ is a dummy that takes value 1 if the match began between 1991 and 1994 and value 0 if the match began between 1986 and 1989. The coefficient δ_2 of the interaction term is the coefficient of interest, that estimates the effect of the EPL change.

I now list the covariates that are stored in the matrix X_{ijt}. Firms' controls are years of activity and average number of employees. Workers' controls are age, gender (FEs), occupation (FEs). Match-specific controls are wage, increase in wage between previous and current employment situation, job-to-job switch (dummy 1 if yes, 0 if no). Controls and FEs are introduced gradually, so that their effects can be observed.

Inter-industry mobility is measured with a dummy variable I_{dummy}. For each new match of worker i with firm j at quarter t, the dummy takes value 1 if the previous job of i was in a firm that operated in a different industry. The dummy takes value 0 otherwise. When this measure is employed, I also estimate a probit model. The empirical specification becomes:

$$\Pr\left\{\text{Sector Change}|X_{ijt}, D_j^S, Post_t\right\} = \tau_t + \gamma_k + \phi_r + \beta' X_{ijt} + \delta_1 D_j^S + \delta_2 (D_j^S \times Post_t) \tag{2}$$

Using OLS to estimate Linear Probability Models (LPMs) is a common strategy in similar studies (e.g., Kugler and Pica 2008, Prifti and Vuri 2013), but it has some issues, that are clearly explained in Wooldridge (2010). The first is that fitted values could be greater than 1 and lower than 0. The second is heteroskedasticity[11]. I adopt the strategies employed by Wooldridge (2010) to deal with both. In particular, (i) I provide the percentage of fitted values that lie within the interval $[0, 1]$, (ii) I use heteroskedasticity robust standard errors, and (iii) I provide the results for probit estimation of the same models.

To show the robustness of my results, I provide estimates for multiple similar specifications, both for OLS and probit models. Moreover, I use two different samples of firms. The main specification estimates the results using firms with 3 to 28 employees, but the same results hold when I only include firms with 10 to 20.

10. Notice that, due to the way I have organized the dataset, this criteria could be met by a match that begins in March and is terminated in October, if pre- and post-employment situation is out-of-dataset.

11. Wooldridge (2010) explains that, being the dependent variable y distributed as a Bernoulli, the variance is $\text{Var}(y|\mathbf{x}) = \mathbf{x}\beta(1 - \mathbf{x}\beta)$, where $\mathbf{x}$ is the set of independent variables and β is the set of coefficients. Omoskedasticity can be achieved only in the case $\beta_1 = ... = \beta_0$.

7 Results

I run regression 2 with multiple specifications, that vary in terms of years included, firms included, controls and fixed effects, an workers included. For robustness, I also estimate the same regression using probit and report the marginal effect. The main specification of the model includes all firms between 3 and 28 employees (see Tables 4, 5, 6, and 7). For robustness, I also provide results for a sample that only include firms between 10 and 20 employees (see Tables 9, 10, 11, and 12). I run the specification for all new matches (Tables 4, 6, 9, and 11) and only for job-to-job new matches (Tables 5, 7, 10, and 12). Figure 4 plots the probability of inter-industry new matches for treated and control groups.

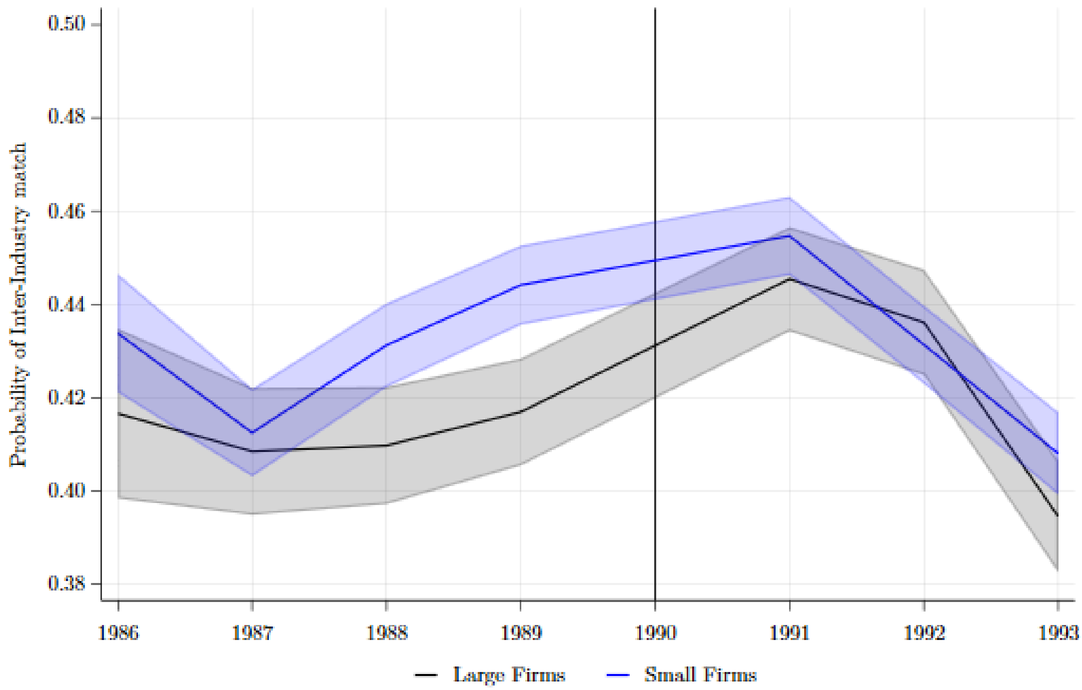


Figure 4: *Probability of Inter-Industry match for Large and Small FIrms between 1986 and 1993. Small Firms are those that have between 2 and 15 employees. Large firms are those that have between 16 and 35 employers. The probability is computed as the ratio between the inter-industry new-matches and the total new matches. Standard errors are computed as* $\frac{p(1-p)}{n}$ *where p is the point estimate of the probability and n is the number of total new matches. Confidence interval are at the 95% confidence level.*

Table 4 reports the results for the specification with all new matches included, pre-treatment years 1986-1989 and post-treatment years 1991-1994. I never include 1990 to avoid possible confounding effects of anticipatory behavior by the employer[12]. The table uses two different measures of industry mobility dummies. The first one (columns (1), (2), and (3)) is based on ATECO 1981 codes and the second one (columns (4), (5), and (6)) on ATECO 1991 codes. Columns (1) and (4) do not include controls and only year and gender fixed effects. Columns (2) and (5) also add Job to Job and Sector fixed effects. The Job to Job fixed effect is a dummy that takes value 1 if the new match is part of a job-to-job transition (i.e., the individual worked in a different firm the year before). The sector fixed effect exploits ATECO 1981 codes in columns (2) and (3), and ATECO 1991 codes in columns (5) and (6). The Job to Job dummy is included to account for the difference in how skills are evaluated when a worker is going from a job to another one and when a worker is joining the firm from unemployment. The sector fixed effect is include to rule out the

12. As discussed in the institutional setting, the reform has been approved in May 1990 and it has been object of discussion multiple months before

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.012	0.010	-0.004	0.018	0.015	0.000
	(0.004;0.020)	(0.002;0.019)	(-0.012;0.004)	(0.010;0.026)	(0.006;0.023)	(-0.007;0.008)
Post × Treat	-0.019	-0.018	-0.012	-0.014	-0.013	-0.012
	(-0.031;-0.007)	(-0.030;-0.006)	(-0.023;-0.001)	(-0.026;-0.003)	(-0.024;-0.001)	(-0.023;-0.001)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;-0.000)
Years of Firm's Activity			0.001			0.002
			(0.001;0.002)			(0.002;0.002)
Year FEs	✓	✓	✓	✓	✓	✓
Job to Job FEs		✓	✓		✓	✓
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
Adj. R^2	0.0012	0.0159	0.1324	0.0083	0.0177	0.1258
N. Obs.	122,044	122,044	122,009	122,044	122,044	122,010
% Fitted ∈ [0, 1]	100.00	100.00	99.97	100.00	100.00	99.97

Table 4: *Results for Regression 2. Estimation of Linear Probability Model. All new matches included. Samples includes firms between 3 and 28 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.020	0.010	0.002	0.026	0.016	0.010
	(0.011;0.030)	(0.001;0.020)	(-0.007;0.011)	(0.017;0.035)	(0.007;0.025)	(0.001;0.019)
Post × Treat	-0.031	-0.028	-0.017	-0.027	-0.023	-0.018
	(-0.045;-0.017)	(-0.041;-0.014)	(-0.029;-0.004)	(-0.040;-0.013)	(-0.037;-0.010)	(-0.031;-0.006)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;-0.000)
Years of Firm's Activity			0.003			0.003
			(0.003;0.003)			(0.003;0.003)
Year FEs	✓	✓	✓	✓	✓	✓
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
Adj. R^2	0.0080	0.0136	0.1710	0.0262	0.0317	0.1779
N. Obs.	88,244	88,244	88,202	88,244	88,244	88,202
% Fitted ∈ [0, 1]	100.00	100.00	99.95	100.00	100.00	99.95

Table 5: *Results for Regression 2. Estimation of Linear Probability Model. Only job-to-job new matches included. Samples includes firms between 3 and 28 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

possibility that the results come from composition effects. Indeed, without controlling for sector FEs, I would not rule out the possibility that the results come from sector-level heterogenous effect of EPL on hiring rates[13]. Column (2) includes weekly wage as control, and column (3) includes Δ wage and years of firm's activity. Δ wage is defined as the difference in weekly wage between the entry wage in the new match and the last wage the individual received (not withstanding how many years before).

The table also provides the percentage of fitted values that are in the interval $[0, 1]$. The high percentage is a robust sign that the OLS-DID specification for this Linear Probability Model is a good choice in terms of interpretability of results without affecting the precision of the estimate. In any case, Table 6 provides the result for the probit estimation. The results between OLS-DID and probit-DID are very close and the difference is not statistically significant.

I find a negative result of the increase in EPL on the probability that a new match is inter-industry. Depending on the specification the effect is a decrease of the probability by 1.3 to 2.0 percentage points. My preferred specification (in Column (6), with ATECO 91 industry codes, all controls and fixed effects) gives an effect of -1.3 percentage points. Given that the average pre-treatment probability of inter-industry matches is 43% the magnitude of the estimated effect is between -3.0% and -4.7%. The estimated effect of the preferred specification is -3.0%. The

13. For example, a negative estimate for the treat-post coefficient without the sector FEs could come from the EPL lowering the hiring rates of industry A more than the hiring rates of industry B, where industry A has a higher percentage of out-of-industry hires.

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.012	0.007	0.011	0.018	0.012	0.012
	(0.003;0.020)	(-0.002;0.015)	(0.003;0.019)	(0.009;0.026)	(0.004;0.020)	(0.004;0.020)
Post × Treat	-0.019	-0.017	-0.015	-0.014	-0.012	-0.013
	(-0.031;-0.007)	(-0.029;-0.006)	(-0.027;-0.004)	(-0.026;-0.003)	(-0.024;-0.001)	(-0.025;-0.002)
Post	0.020	0.024	0.018	0.016	0.020	0.011
	(0.011;0.030)	(0.014;0.034)	(0.008;0.027)	(0.007;0.025)	(0.011;0.030)	(0.002;0.021)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;-0.000)
Years of Firm's Activity			0.001			0.001
			(0.001;0.001)			(0.001;0.001)
Job to Job FEs		✓	✓		✓	✓
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
N. Obs.	122,044	122,044	122,044	122,044	122,044	122,044

Table 6: *Results for Regression 2. Estimation of Probit Model. All new matches included. Samples includes firms between 3 and 28 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.019	0.009	0.025	0.025	0.015	0.025
	(0.010;0.029)	(-0.000;0.019)	(0.016;0.034)	(0.016;0.035)	(0.006;0.025)	(0.016;0.035)
Post × Treat	-0.031	-0.027	-0.020	-0.026	-0.023	-0.019
	(-0.044;-0.017)	(-0.041;-0.013)	(-0.034;-0.007)	(-0.040;-0.013)	(-0.036;-0.009)	(-0.032;-0.006)
Post	0.017	0.028	0.015	0.010	0.020	0.006
	(0.007;0.028)	(0.017;0.039)	(0.004;0.026)	(-0.001;0.021)	(0.009;0.031)	(-0.005;0.016)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;-0.000)
Years of Firm's Activity			0.003			0.003
			(0.002;0.003)			(0.003;0.003)
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
N. Obs.	88,244	88,244	88,244	88,244	88,244	88,244

Table 7: *Results for Regression 2. Estimation of Probit Model. Only job-to-job new matches included. Samples includes firms between 3 and 28 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

probit estimation of the model (Table 6) leads to very close point estimates (between -1.4% and -1.9%).

Table 5 provides the results of the same specification including only new matches that are part of job-to-job transitions. A transition is job-to-job if, in the dataset, the individual goes from one job to another, without being unemployed. This means that the dataset observes them at quarter t working for firm α, and at quarter $t+1$ working for firm β[14].

The table shows that almost all the fitted values are within $[0, 1]$. The fixed effects and the controls are introduced gradually as in Table 4. The effect of the increase in EPL is a decrease of between 1.9 and 3.1%, depending on the specification and on the industry codes considered. My preferred specification (in Column (6) with ATECO 91 industry codes, all controls and fixed effects) gives a decrease of 2.0%. The pre-treatment probability for a new job-to-job match to be inter-industry was 45.6%. This means that the magnitude of the estimated effect is between -4.2% and -6.8%. The estimated effect of the preferred specification is -4.4%. The probit estimation of the model (see Table 7) leads to very close point estimates (between -2.1% and -3.1%).

For robustness, I provide the results of the same model estimated on a smaller sample that includes firms between 10 and 20 employees. Results of the Linear Probability Model for all new matches and for job-to-job matches are provided, respectively, in Tables 9 and 11. On the other hand, probit estimates for all new matches and for job-to-job new matches are provided, respectively, in Tables 11 and 12. In general, the effect is confirmed as the estimate is statistically significant, it goes in the same direction as the main specification, and the magnitude is comparable. In general, these robustness checks lead to larger point estimates than the main specification (even though the difference is not statistically significant).

8 Conclusions

This work explores the effects of an increase in Employment Protection (EPL) on inter-industry worker mobility. I exploit the increase in job security for employees in small firms in Italy in 1990 (Law 108/1990).

My findings reveal that stricter EPL leads to a significant reduction in the likelihood of inter-industry hires. The estimated decrease is of 1.3–2.0 percentage points for all new matches, and 1.9–3.1 percentage points for job-to-job transition. These results suggest that higher firing costs make employers more willing to hire candidates from the same industry, to minimize the risks associated with mismatches and potential dismissals.

The survival analysis supports this interpretation, showing that workers hired from different industries face a higher likelihood of dismissal. This underscores the role of industry-specific human capital in shaping employment outcomes under strict EPL regimes.

These findings contribute to the broader literature on EPL by emphasizing its influence not only on aggregate labor market outcomes but also on more granular aspects like recruitment practices and skill mobility. While previous studies have documented the effects of EPL on hiring rates, productivity, and workforce composition, this work sheds light on how EPL can indirectly affect the allocation of human capital across industries.

From a policy perspective, the results suggest that while EPL can provide job security for incumbent workers, it may create unintended barriers to labor mobility and skill reallocation.

14. One should notice that, by defining job-to-job transition in this way, I am actually putting in the same basket two very different phenomena. On one side, transitions due to the worker's choice. On the other side, transitions due to the individual being fired by the previous employer, and being hired by another employer within the following quarter.

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A Graphs

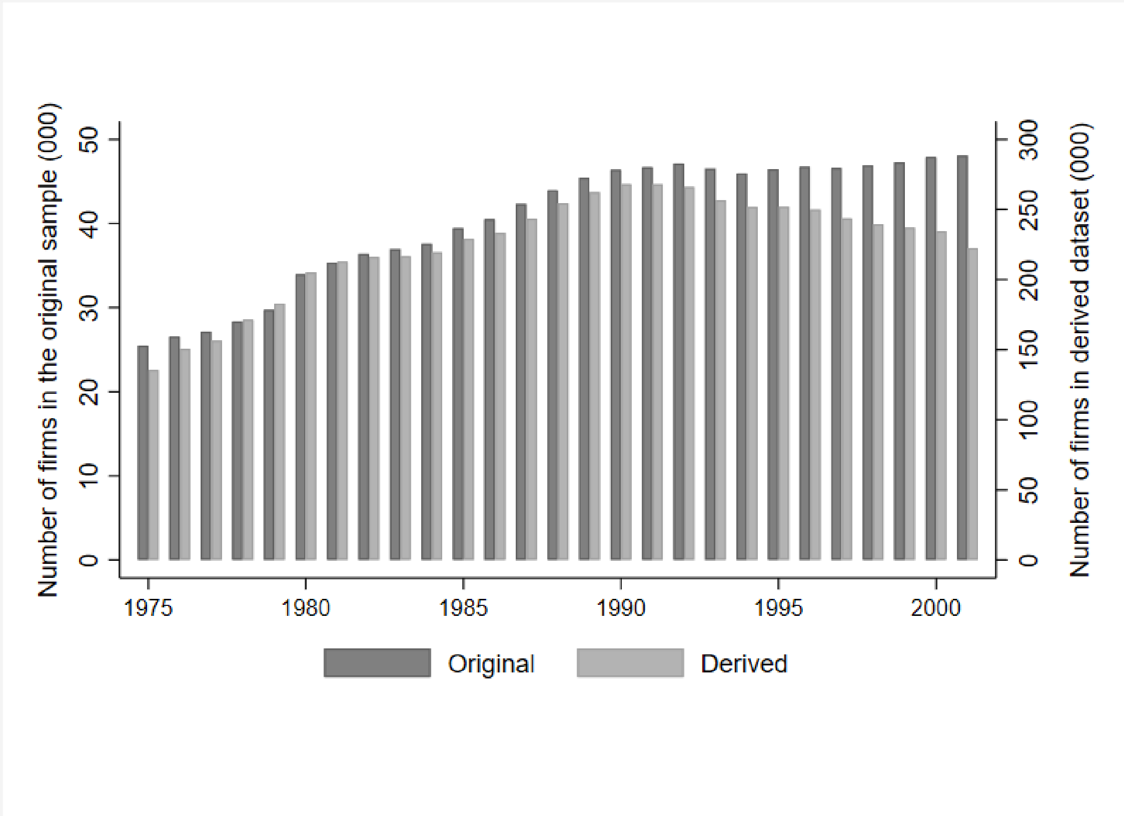


Figure 5: *Number of active firms by year and by dataset subset. Firms in the original subset are all firms that are resident in the provinces of Treviso or Vicenza. The firms in the subset are the remaining. The original subset bars refer to the left y-axis, the derived subset bars refer to the right y-axis. In a given year t, a firm is considered active if it has initiated activities during year t or previous years and it has terminated activities during year t or following years.*

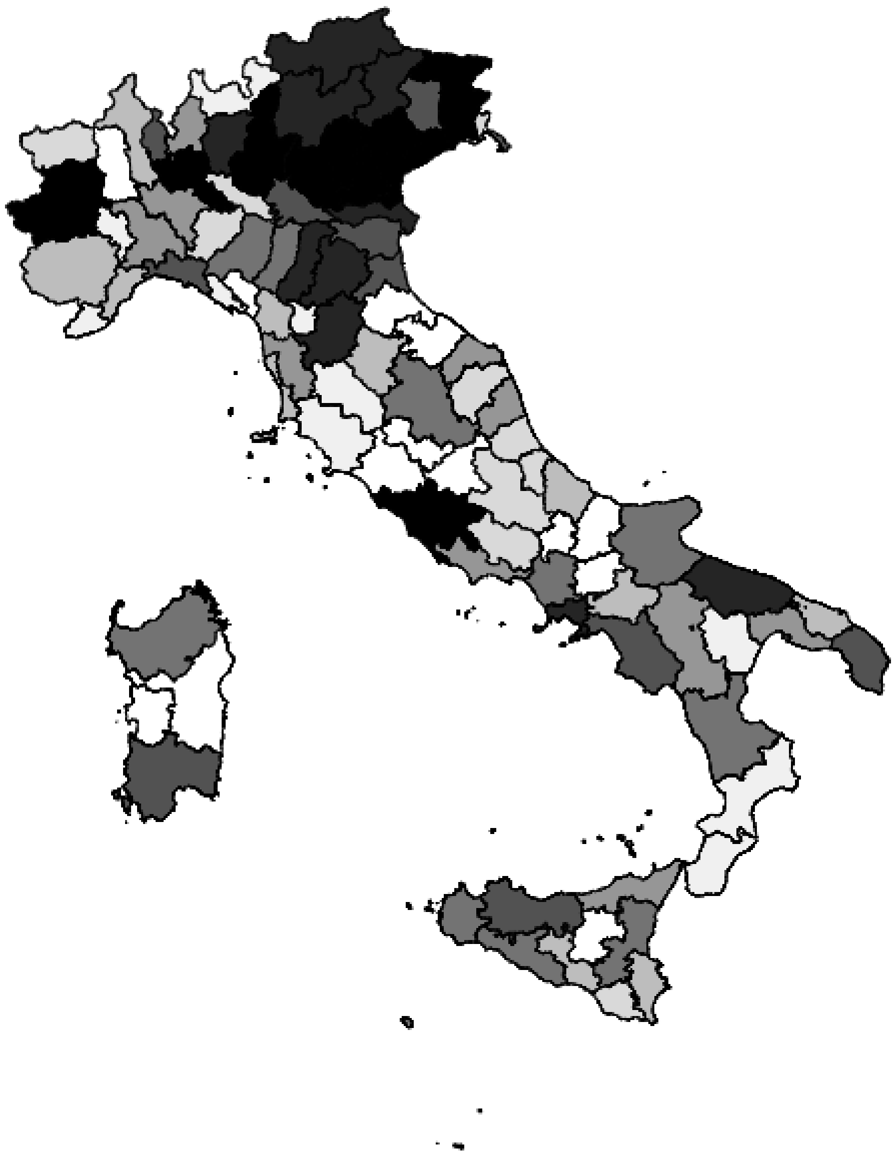
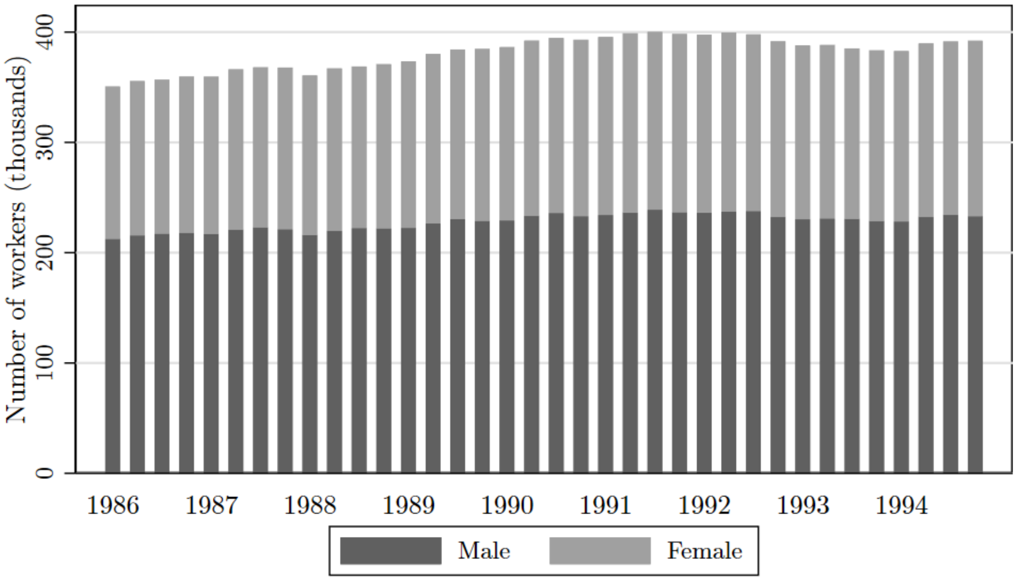
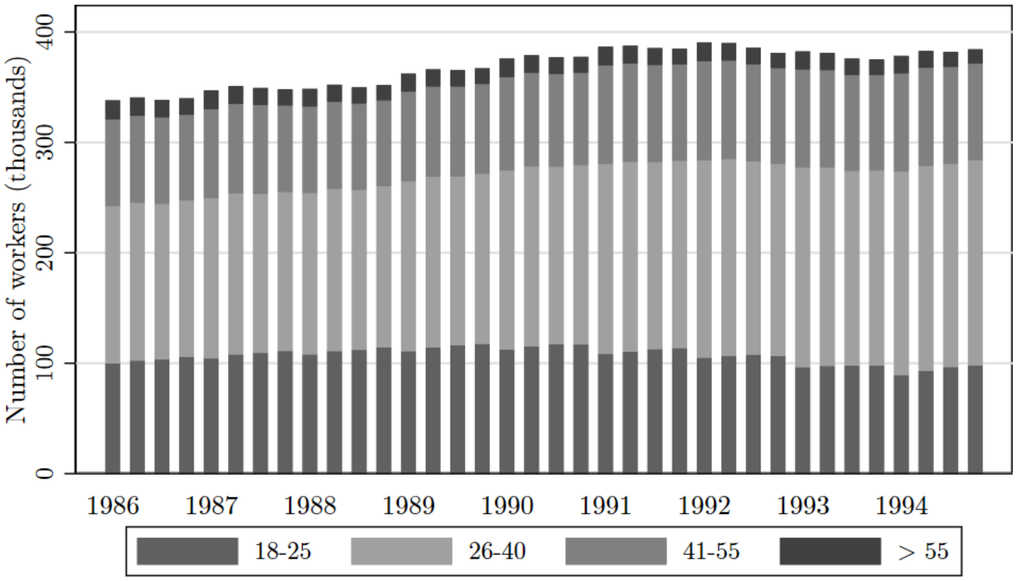


Figure 6: *Distribution of firms in the dataset by province. 9 nuances of grey correspond to the 9 quantiles of the number of firms in each province. Provinces borders are at 1991 and are provided by Istat.*

B Tables



(a) Number of active workers by gender



(b) Number of active workers by age class

Figure 7: *Number of active workers by gender and by age class. Only firms active in the provinces of Treviso and Vicenza are included. Workers with non-specified age, or with age lower than 18 are removed from the sample (3.4% of the sample) in Figure 7b. The total number of workers active for at least one quarter in the provinces of Treviso and Vicenza between 1986 and 1994 is 726,959.*

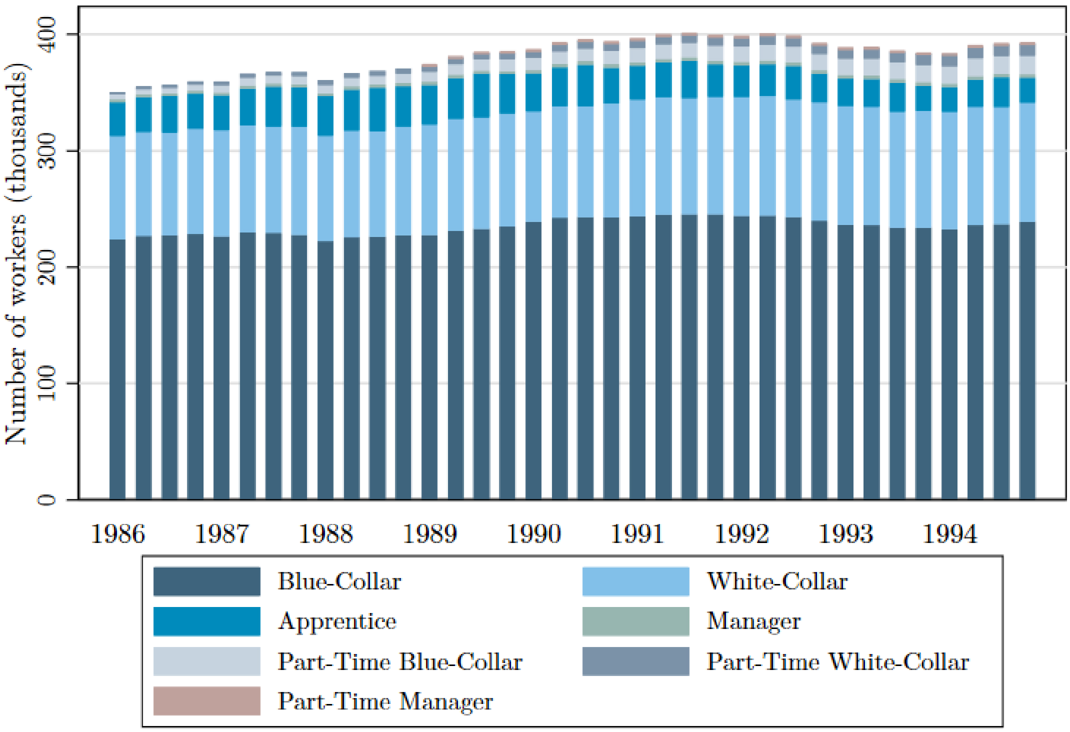


Figure 8: *Number of active workers by qualification and quarter. Only firms active in the provinces of Treviso and Vicenza are included. Workers with non-specified qualifications are excluded from the sample. Figure 9 is the same chart, but total number of workers by quarter is normalized to 1.*

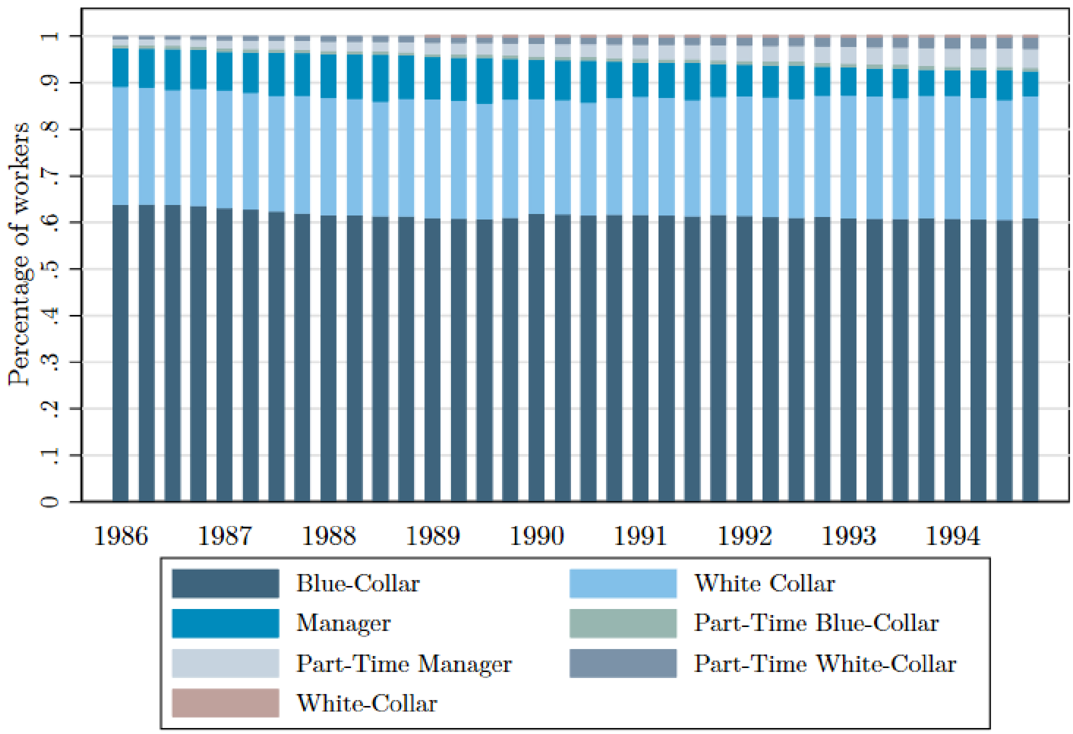


Figure 9: *Same as Figure 8, but total number of workers by quarter normalized to 1.*

Variable	Label	Notes
	azien dataset	
matr_az	Firm Code	10-figure INPS code
rag_soc	Business Name	
att_econ	Economic activity	
indirizzo	Address	
cap	Postal Code	
comune	Town	
prov	Province	
csc	"Codice Statistico Contributivo"	
data_cost	Date of Establishment	
data_sosp	Date of Suspension	
data_cess	Date of Termination	
cod_fis	Fiscal Code	Useful to merge with external datasets
part_iva	VAT Code	
cod_com	Code of Town	
ateco81	Sector Code, 1981	
ateco91	Sector Code, 1991	The data provider suggests not to use it
artig	1 if artisan firm, 0 otherwise	
data_in	First day of activity	
data_out	Last day of activity	
dip_in	Number of employees at data_in	
dip_out	Number of employees at data_out	
mes_sosp	Number of months of suspended activity	
num_sosp	Number of suspensions	
	anagr dataset	
cod_pgr	Worker Code	
sesso	Gender	
com_n	Place of Birth	
prov_n	Province of Birth	
naz	Nationality	
com_r	Place of Residence	
prov_r	Province of Residence	
anno_n	Year of Birth	
	contr dataset	
cod_pgr	Worker Code	
matr_az	Firm Code	10-figure INPS code
anno	Year	
mesi_r	Paid months	12-figures string. "1" if paid, "0" if non-paid
sett_r	Number of paid weeks	
gior_r	Number of paid days	
retrib03	Total wage	
contrat	CCNL INPS Code	
livello	Level within CCNL	
qualif	Level	(Wh)ite-, blue-collar, (middle) manager) × (Part-, Full-Time)

Table 8: *Variables' list in the three datasets.*

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.015	0.014	0.005	0.019	0.017	0.010
	(0.004;0.027)	(0.002;0.025)	(-0.007;0.016)	(0.007;0.030)	(0.005;0.028)	(-0.001;0.021)
Post × Treat	-0.015	-0.013	-0.012	-0.018	-0.017	-0.020
	(-0.032;0.002)	(-0.030;0.004)	(-0.028;0.005)	(-0.035;-0.001)	(-0.034;0.000)	(-0.036;-0.004)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;0.000)			(-0.000;0.000)
Years of Firm's Activity			0.001			0.002
			(0.001;0.002)			(0.002;0.002)
Year FEs	✓	✓	✓	✓	✓	✓
Job to Job FEs		✓	✓		✓	✓
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
Adj. R^2	0.0031	0.0212	0.1495	0.0186	0.0279	0.1574
N. Obs.	51,261	51,261	51,246	51,261	51,261	51,247
% Fitted $\in [0,1]$	100.00	100.00	99.97	100.00	100.00	99.97

Table 9: *Results for Regression 2. Estimation of Linear Probability Model. All new matches included. Samples includes firms between 10 and 20 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.022	0.017	0.008	0.026	0.022	0.016
	(0.008;0.036)	(0.003;0.030)	(-0.005;0.021)	(0.013;0.039)	(0.009;0.035)	(0.003;0.028)
Post × Treat	-0.021	-0.019	-0.020	-0.027	-0.026	-0.031
	(-0.041;-0.001)	(-0.039;0.001)	(-0.038;-0.001)	(-0.047;-0.007)	(-0.045;-0.006)	(-0.049;-0.013)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;0.000)
Years of Firm's Activity			0.003			0.003
			(0.002;0.003)			(0.003;0.003)
Year FEs	✓	✓	✓	✓	✓	✓
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
Adj. R^2	0.0129	0.0212	0.1916	0.0420	0.0473	0.2142
N. Obs.	38,011	38,011	37,992	38,011	38,011	37,992
% Fitted $\in [0,1]$	100.00	100.00	99.95	100.00	100.00	99.95

Table 10: *Results for Regression 2. Estimation of Linear Probability Model. Only job-to-job new matches included. Samples includes firms between 10 and 20 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.015	0.012	0.013	0.018	0.016	0.016
	(0.003;0.027)	(0.000;0.024)	(0.001;0.025)	(0.007;0.030)	(0.004;0.028)	(0.004;0.028)
Post × Treat	-0.014	-0.013	-0.012	-0.018	-0.017	-0.020
	(-0.032;0.003)	(-0.031;0.004)	(-0.030;0.005)	(-0.035;-0.001)	(-0.034;-0.000)	(-0.036;-0.003)
Post	0.019	0.026	0.020	0.017	0.022	0.012
	(0.007;0.032)	(0.013;0.038)	(0.008;0.033)	(0.004;0.029)	(0.010;0.034)	(-0.000;0.024)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;-0.000)
Years of Firm's Activity			0.001			0.001
			(0.001;0.001)			(0.001;0.001)
Job to Job FEs		✓	✓		✓	✓
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
N. Obs.	51,261	51,261	51,261	51,261	51,261	51,261

Table 11: *Results for Regression 2. Estimation of Probit Model. All new matches included. Samples includes firms between 10 and 20 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*

	ATECO 81 2-digit			ATECO 91 2-digit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.022	0.016	0.020	0.026	0.022	0.024
	(0.008;0.035)	(0.003;0.030)	(0.006;0.033)	(0.013;0.039)	(0.008;0.035)	(0.011;0.037)
Post × Treat	-0.021	-0.019	-0.015	-0.027	-0.025	-0.026
	(-0.041;-0.001)	(-0.039;0.001)	(-0.035;0.005)	(-0.046;-0.007)	(-0.045;-0.006)	(-0.045;-0.007)
Post	0.016	0.030	0.020	0.006	0.017	0.005
	(0.002;0.031)	(0.016;0.045)	(0.006;0.035)	(-0.008;0.020)	(0.003;0.031)	(-0.009;0.019)
Wage		-0.000	-0.000		-0.000	-0.000
		(-0.000;-0.000)	(-0.000;-0.000)		(-0.000;-0.000)	(-0.000;-0.000)
Δ Wage			-0.000			-0.000
			(-0.000;-0.000)			(-0.000;-0.000)
Years of Firm's Activity			0.002			0.002
			(0.002;0.003)			(0.002;0.003)
Gender FEs	✓	✓	✓	✓	✓	✓
City FEs			✓			✓
Sector FEs			✓			✓
N. Obs.	38,011	38,011	38,011	38,011	38,011	38,011

Table 12: *Results for Regression 2. Estimation of Probit Model. Only job-to-job new matches included. Samples includes firms between 10 and 20 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*