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 the sectoral mobility of workers, using matched employer-employee data from the Italian provinces of Treviso and Vicenza. First, I show that inter-industry matches—where a worker's prior experience is in a different industry—are of lower quality, as measured by job duration and wage levels. I then relate this empirical finding to the theoretical framework of Pries and Rogerson (2005), which shows that stricter employment protection leads to higher recruitment standards on match quality. Building on this, I test whether stricter EPL results in fewer inter-industry matches. Exploiting Italy's 1990 reform (Law 108/1990), which increased severance pay requirements for small firms, a Difference-in-Differences analysis reveals a 1.3–2.0 percentage point reduction in the probability of inter-industry hiring. 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 in 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) use two different models that investigates the relation between employment protection and recruitment selectiveness. 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 empirical literature and investigates 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 an increase in Employment Protection Legislation. 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. The time window is 1975-2001.

The theoretical framework builds on Pries and Rogerson (2005). They use a search and matching model, with imperfect information about match productivity. Employers and potential employees observe an *ex-ante* signal π of match productivity and they decide whether to form a match or not.

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

The quality of the match is then revealed during the match. If the revealed match type is of low-productivity, the employer dismisses the worker. If employment protection increases, both the worker and the firm have a higher threshold on the match quality signal in order to form a match. Indeed, with higher match quality is lower the probability of paying the dismissal costs.

In order to connect Pries and Rogerson (2005) with my question, one needs to observe whether inter-industry matches have different match quality with respect to intra-industry matches. To empirically test this assumption, I use job duration and wage levels as measures of match quality (Belot, Liu, and Triantafyllou 2024). I show that, in Italy between 1975 and 1990, inter-industry matches are less stable and have lower wages. These measures of match quality are highly correlated with productivity and employer satisfaction (Pellizzari 2011). As a result, with an increase in employment protection, employers will look for matches of better quality and thus will be less prone to hire from other industries.

To show this, 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 Difference in Differences approach. The treated (control) group is composed of firms with up to (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 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%. 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 details the theoretical framework, Section 4 presents the institutional background of employment protection in Italy and on the 1990 change. Section 5 presents the main data source, and some descriptives about workers, firms, and inter-industry mobility of workers, Section 6 tests the main assumption of the theoretical framework, Section 7 explains the empirical specification, Section 8 presents the results, and Section 9 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* labor market measures (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 the employment rate by 0.34 to 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 that low labor market flexibility in Europe does not translate in 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

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

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 to use microdata (in this case, US Current Population Survey) to investigate the effects of EPL. In particular, they exploit the staggered nature of the application of EPL laws in US states and show negative impact of stricter EPL on the employment rate, with more pronounced effects for female, younger, and less-educated workers. Other studies that exploit the staggered adoption of EPL changes in the US are, for example, Acharya, Baghai, and Subramanian (2014), Bai, Fairhurst, and Serfling (2020), and Beuselinck, Markarian, and Verriest (2021).

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, policy changes that have beeen extensively investigated are the Spanish liberalization of fixed-term contracts in 1984 (García-Pérez, Marinescu, and Vall Castello 2019), the UK Reform that increased EPL for short-tenured workers in 1999 (Marinescu 2009), the Swedish Reform that relaxed the LIFO rule for collective lay-offs in 2001 (Lindbeck, Palme, and Persson 2006, Olsson 2009, Olsson 2017, Bjuggren 2018, and Butschek and Sauermann 2022[3]), the Act of December 2013 in Belgium which harmonized notice periods for blue- and whit-collar workers (Caggese et al. 2022 and Alpysbayeva and Vanormelingen 2022), and the Italian Jobs Act in 2015 that, among other things, introduced a contract with job security increasing in tenure (Boeri and Garibaldi 2019 and 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.

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

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 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.

To my knowledge this is the first work to study the effect of EPL on inter-industry flows through the channel of recruitment selectiveness. Bassanini and Garnero (2013) uses cross-country aggregate flows and shows that an increase in EPL decreases the number of job-to-job transitions within the same industry, without affecting the number of job-to-job transitions across industries. They also do not find significant effect of job-to-jobless transitions. However, their analysis is different from ours because they do not analyze incidence of inter-industry matches on total hires and because they use cross-country data, which involves different types of reforms which increased EPL.

This work also builds on the literature on measurement of match quality. Indeed, this is needed to build a bridge between the literature on recruitment and employment protection on one side and flows of workers across industries on the other side. Section 6 includes a review of this strand of literature. The review builds on the work of Belot, Liu, and Triantafyllou (2024).

3 Theoretical Framework

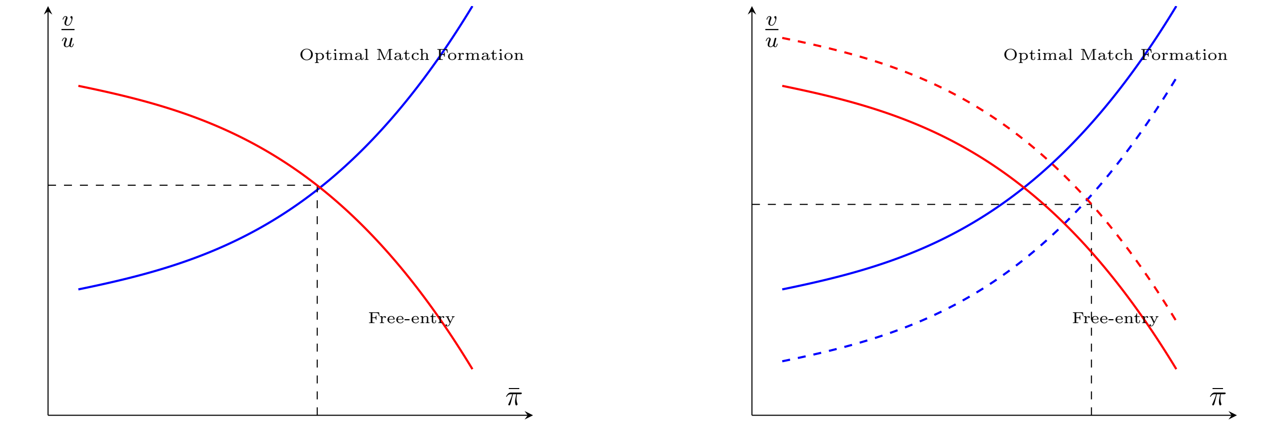
The theoretical framework of this work builds on Pries and Rogerson (2005)[4]. They use a search and matching model with imperfect information. The potential employee and the firm do not know *ex-ante* the match productivity. They know that it can be either high (y^g) or low (y^b), with $y^g > y^b$. They both observe a signal π about the quality of the match: they share the expectation that the match is of good quality with probability π. If the match is created, the actual match productivity may be revealed throughout the match: each period, it can either be revealed (firm and worker know whether productivity is y^g or y^b) or not (firm and worker continue to share the prior π on productivity being y^g)[5].

The equilibrium is found on a $\frac{v}{u}$-$\bar{\pi}$ space, where $\frac{v}{u}$ is the vacancy-to-unemployment ratio, while $\bar{\pi}$ is the minimum level of *ex-ante* probability of good-quality match required to form a match (see Figure 1a). The two curves are the optimal match formation and the free-entry curve. The first is a monotonic positive relationship between $\bar{\pi}$ and $\frac{v}{u}$, while the second is a monotonic negative relationship between $\bar{\pi}$ and $\frac{v}{u}$.

The intuition is as follows. On one hand, the higher $\bar{\pi}$, the more likely the match is profitable, and thus the larger the number of vacancies (Optimal Match Formation curve). At the same time, though, an

4. The notation in this paragraph follows the notation of their paper
5. This is modeled with the observed productivity y being equal to the actual productivity $\bar{y}$ plus noise. In other words, $y = \bar{y} + \epsilon_t$, where ϵ is uniformly distributed between $-\omega$ and ω. ω is such that $\omega > \frac{y^g - y^b}{2}$. Thus, if the observed productivity y is high enough to rule out the possibility that the match is of bad quality, the match quality is revealed. The same happens if the observed productivity y is low enough to rule out the possibility that the match is of good quality. The match quality is not revealed if $y \in [y^g - \omega, y^b + \omega]$. It may be revealed during the following period, depending on the realization of ϵ_{t+1}.

increase in the reservation *ex-ante* match quality decreases the number of acceptable matches, making job creation less attractive (Free-entry curve).



(a) *Pries and Rogerson model, with dismissal costs at d.*

(b) *Pries and Rogerson model, with higher dismissal costs $d' > d$.*

Figure 1: *Equilibrium of Pries and Rogerson on the $\frac{v}{u}$-$\bar{\pi}$ space. Example of increase in firing costs d. The first chart is taken from the paper (Figure 1).*

With an increase in firing costs, the minimum *ex-ante* match quality needed to form a match $\bar{\pi}$ increases to $\bar{\pi}' > \bar{\pi}$ (Figure 1b). In other words, a match with $\tilde{\pi} \in (\bar{\pi}, \bar{\pi}')$ that would have been formed under the coarser EPL regime is not formed anymore. The optimal match formation curve shifts downward because firms anticipate that they will pay higher dismissal costs and form fewer matches. Free-entry curve shifts upward because, keeping $\bar{\pi}$ constant, the gain from job creation increases, as the outside option decreases (by the increase in d). As a result, $\bar{\pi}$ unambiguously increases.

Now, in order to connect the model by Pries and Rogerson (2005) to the inter-industry mobility question, one needs to assume that the *ex-ante* match productivity is, *ceteris paribus*, lower if the potential employee's previous experience was in another industry. In the empirical section, I will test this hypothesis exploiting widely used measures of match quality. This hypothesis is based on the industry-specificity of human capital. This means that a worker accumulates human capital that increases her productivity if she keeps working in the same industry. At least a part of the human capital that she accumulates during her experience in the industry adds nothing to her productivity in other industries. The existence of industry-specific human capital has been shown empirically by multiple studies (e.g., Neal 1995; Weinberg 2001; Sullivan 2010)[6].

Notice that another assumption needs to be done. Indeed, it is not enough to assume industry-specific human capital, but one needs to assume that this translates into the prior of jobseekers and firms. In

6. Other authors, in particular Kambourov and Manovskii (2009), argue that when occupational specificity of human capital is taken into account, the relevance of industry-specificity is lower, but they do not question its existence.

other words, employer and potential employees know that, on average, inter-industry matches are of lower quality and statistically discriminate (Aigner and Cain 1977) potential employees on the basis of this.

If the selectivity on *ex-ante* match productivity increases, it is less likely that an inter-industry candidate is preferred to an intra-industry candidate. As a result, if all these assumptions hold, with an increase in firing costs, the probability of inter-industry matches decreases.

With the available data, I am able to test whether, on average, inter-industry matches are of lower quality, exploiting widely used measures of match quality. I am not able to test whether this is known by the employer and the potential employee. Using a DiD approach, I am able to show that the main conclusion of this theoretical framework holds. Match quality measures are discussed in Section 6, empirical specification of the DiD in Section 7, and results in Section 8.

4 Institutional Background

4.1 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[7] and 1989-1990, accepted by the Constitutional Court[8]). 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.

4.2 International Comparison

Compared to other countries, the level of employment protection in Italy 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).

7. Decision 27, 1982 [Constitutional Court website]
8. Decision 65, 1990 [Constitutional Court website]

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 to 5 countries (Australia, Finland, Norway, Sweden, and Switzerland). Figure 3 shows the ranking of Employment Protection strictness in 1990 and 2019.

The OECD index is periodically updated (see 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). Figure 4 shows the evolution of the index in Italy, Spain, Portugal, Germany, France, and United States between 1990 and 2019.

5 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 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. Each 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 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 through 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 latter 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 is provided in Table 4. `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[9]) 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.

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[10]. Notice that, since the data originate from administrative procedures, it does not come with

9. 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)

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

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 assume that she is not retired if she appears in the dataset again 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[11]. 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 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. Again, this does not mean that she is retiring, as she could become self-employed, inactive, unemployed and then retiring, etc. To organize the dataset as described, I follow the following criteria to attribute an employment status for each quarter:

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.

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

- 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 (e.g., she worked 1 month in firm j and one month in firm k), the worker is considered working the entire quarter in the company where she earned the highest weekly wage

5.1 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 is not part of the *original dataset* and 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. These firms are not considered in the causal analysis, to avoid selection bias.

Table 5 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 hundreds of employees, because it is computed winsorizing the employment level. Observing the average firm size, one can appreciate the significance of a reform aimed at firms with fewer than 15 employees, as it is the case with the reform examined in this paper. 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[12]. 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).

12. 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 does not allow us to draw conclusions.

5.2 Workers

The workers included in the sample are 3,650,312. A worker enters in the dataset if she 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 5 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 (Kalleberg 2000).

5.3 Inter-Industry Mobility of Workers

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 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 1, 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. To build the table, 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 more frequently 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.

5.4 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.

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 1: *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.

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 6 plots the activity, employment, and unemployment rate for Veneto and Italy (for the entire population, young people, and 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).

6 Match Quality

With the available data described in the previous section, I am able to measure match quality with widely-used measures. This is needed to test the assumption that inter-industry matches are, on average, of lower quality.

In the literature, researchers have suggested multiple measures of match quality. Belot, Liu, and Triantafyllou (2024) provide an extensive review, that I use as point of reference of this paragraph.

Match quality can be measured through surveys to workers and/or employers, or exploiting data on wages, job duration, and education. The availability of employer-employee matched data allows the researchers to estimate match effects as well (Woodcock 2015), exploiting workers switching firms throughout their working history.

Ferreira and Taylor (2011) employ factor analysis to combine multiple survey questions answered by workers into a single match quality index. In particular, they exploit the British Household Panel Survey and they show that their measure correlated negatively with the probability of separation and positively with wages. They also show that average match quality follows a J-shaped curve: it decreases in the first 5 years, and then increases afterwards. The short-term findings are consistent with the US-based evidence provided by Belot, Liu, and Triantafyllou (2024) (who use NLSY79 data).

Some studies use survey-based measures of match quality as dependent variable, to study the impact of various labor market phenomena on match quality (e.g., Zhang, Salm, and Soest 2021 studies on-the-job training; Barmby, Bryson, and Eberth 2012 studies accumulation of human capital).

In addition to surveys. researchers use wage and job duration to measure match quality. In particular, the match quality is increasing in wage level and job tenure. A higher wage signals a better match quality as it is part of the surplus deriving from the match (i.e., the higher the surplus, the higher the wage) (Le Barbanchon 2016). On the other hand, job tenure increases with match quality as the quality is revealed throughout the match (Jovanovic 1979, Pries and Rogerson 2005): the fact that the match is not terminated means that the job quality was revealed and the parties decided not to separate.

There is an extensive literature, well-documented in Belot, Liu, and Triantafyllou (2024), that adopts these solutions to investigate how match quality is impacted by job search policies, namely Unemployment Insurance (e.g., Card, Chetty, and Weber 2007; Le Barbanchon 2016; Nekoei and Weber 2017) and active labor market policies (e.g., Gaure, Røed, and Westlie 2012 and Crépon, Ferracci, and Fougère 2012). The same solutions are adopted by Simon and Warner (1992) to estimate the impact of job applicant referrals on match quality.

Pellizzari (2011) shows that wages and job duration are not only measures of job quality for the employee's side, but also for the employers' side. Indeed, he shows that higher recruitment effort leads to employer's satisfaction on one side and to higher wages and more stable matches on the other side. This supports my point, as I am using wage and job duration to detect the signal on the expected quality of an inter-industry match that employers are likely to receive.

6.1 Job Duration

In this subsection, I investigate whether inter-industry matches exhibit higher risk of separation. To do this, I perform a multivariate survival analysis, controlling for multiple potential confounders and fixed effects. I show that inter-industry matches are more likely to be terminated during the first 5 years of the contract. This is especially true for men and for workers on an open-ended contract, that is indeed the type of contract affected by the 1990 reform.
This subsection benefits from the indications provided by Miller (2008) on how to report effectively hazard models' results.

6.1.1 Methodology

I use a Cox proportional hazard model. I only consider the first 5 years (20 quarters) on the job (as in Card, Chetty, and Weber 2007). The model allows the researcher to investigate the duration of an event (in this case, of a match) conditioning the estimates on a set of covariates. I show that results are robust to the introduction of multiple controls (weekly wage, number of employees, worker's age, duration of previous job), and fixed effects (qualification, province, contract, gender, and industry). I also provide a set of heterogeneity results.

The proportional hazard model is created to deal with continuous data. The process studied here is not continuous, as the failure time is daily. Moreover, I only observe quarterly changes. As a result, failures are tied at the quarterly level. To address this issue, I use the Efron estimation method (Efron 1977)[13].

A censored proportional hazard model is built on three assumptions (Wilson 2018):

- Independent Observations, conditional on the covariates. To reduce the risk of violating this assumption, I include the covariates that may influence job duration. Due to data contraints, I am not able to construct some variables that may be useful. For example, I am not able to compute the average job duration per worker, as I do not observe the entire working history of individuals

13. I estimate the model using the STATA `stcox` command. Details on data preparation, syntax, and options can be found in the Stata Manual (`stata.com/manuals/ststcox.pdf`)

- Censoring is independent from the time of the event. This is the case, because all matches are censored 5 years from start date, if they last more than 5 years

- The hazard functions of each strata are proportional. This means that ratio between the hazard function of the strata should not depend on t. This is the case if and only if if the coefficient of the strata dummy is time-invariant[14]. To test whether the assumption holds, I show that the $-\log[-\log(\text{Survival Probability})$ - $\log t$ plots for the two strata (inter- and intra-industry matches) are parallel (Figure 10)[15].

The model that I estimate include variables at the match-, firm-, and worker-level:

$$h(t|\phi_{ij}, \underline{A}_{ij}, \underline{B}_i, \underline{C}_j) = h_0(t) \exp\left\{\delta\phi_{ij} + \alpha^T \underline{A}_{ij} + \beta^T \underline{B}_i + \gamma^T \underline{C}_j\right\} \tag{1}$$

where i is the subscript for the worker, j is the subscript for the firm. ϕ_{ij} is a dummy that takes value 1 if the match is inter-industry and 0 if the match is not inter-industry. δ is thus the coefficient of interest. $\underline{A}_{ij}$ is a set of variables at the match-level, $\underline{B}_i$ is a set of variables at the worker-level, and $\underline{C}_j$ is a set of variables at the firm-level. In the estimation, I also estimate time-varying coefficients, including the interaction between each variable and t.

Variables Included The dataset includes match-, firm, and worker-level variables. Match-level variables are duration in quarters, average weekly wage (throughout the match), wage quarterly growth rate, type of contract (open ended or fixed term), and qualification (blue collar, white collar, manager, part-time blue collar, part-time white collar, and apprentice). Wage quarterly growth rate is obtained as follows:

$$\text{Wage Quarterly Growth Rate} = \left(\frac{w_T}{w_1}\right)^{\frac{1}{T}} - 1$$

where T is the duration of the match, in quarters, and w_1 (w_T) is the wage level on the first (last) quarter of the match. The rate is adjusted at the quarterly frequency to prevent multicollinearity issues. Indeed, alternatives such as $w_T - w_1$, $\log(w_T) - \log(w_1)$, or $(w_T/w_1) - 1$ would have been more correlated with the duration of the match.

Firm-level variables are industry code (2-digit ATECO 91), number of employees, and province. Worker-level variables are age, gender, duration of previous job, and unemployment spell time. Duration of previous job cannot be computed for all individuals, as for some individuals I do not observe

14. Consider the dummy Z, which distinguishes between two strata ($Z = 0$ and $Z = 1$). The Cox model is $h(t|Z, X) = h_0(t) \exp\{\beta Z + \gamma X\}$, where X is a set of additional covariates. If the coefficient β is time-invariant, the hazard ratio between the two strata is $h(t|Z = 1, X)/h(t|Z = 0, X) = \exp\{\beta + \gamma X\}/\exp\{\gamma X\}$, which is, in turn, time invariant.

15. Sestelo (2017) provides a clear proof which shows that, if the proportionality assumption holds, the difference between the $-\log(-\log)$ transformation for each strata does not depend on t.

the first quarter in the previous job. Unemployment spell time is the duration, in quarters, of the unemployment spell that ended with the match in the dataset.

Sample Selection In this section I am only showing descriptive facts about inter-industry matches. No exogenous change is needed to run the estimation. The choice of the time window is thus driven by three priorities. (i) it is better not to include any major policy change in terms of employment protection as it may distort the preferences of employers towards different types of matches. (ii) Since the aim of this analysis is instrumental to the evaluation of the impact of the 1990 reform, it is better to consider pre-reform years. (iii) The duration of the matches should not be distorted by the dataset's start and end dates. (iv) The dataset needs to be suitable for a feasible empirical analysis on Stata. As a result, I consider workers' histories from 1975 to 1989. For the sake of computability, I keep a random sample of workers, keeping 1/3 of the workers in the dataset. The analysis is at the match level. A match is included in the dataset if (i) it is not the first match of the workers in the dataset (i.e., I am able to observe at least one quarter in the previous match), (ii) at the end of the match, the worker is older than 18 and younger than 64, (iii) the firm is resident in the provinces of Treviso and Vicenza, (iv) within the time span of the dataset, the firm has always had more than 2 but less than 28 employees.

Lastly, I only consider matches that ended between 1983Q4 and 1989Q4. The latter constraint is imposed to avoid the reaction to the 1990 reform. The former, instead, is to have a dataset with a stationary average match duration. Indeed, given that I only select matches for which the initial date is observed in the dataset, matches that end sooner are, on average, shorter. The average duration stabilizes between 11 and 12 quarters starting from 1983Q4 (Figure 9).

Descriptives Figure 7 shows the age distribution of workers who experience a separation, distinguishing between separation from an inter-industry match and from intra-industry match. It is compared with the age distribution of the entire working population. The distinction by gender is also provided. Young workers separate more often than older workers, and this is especially true for women.

6.1.2 Results

Table 6 reports the estimates for the entire sample, gradually introducing controls. Table 7 reports the results by categories, estimating the model separately for men and women, blue- and white-collar workers, small and big firms, fixed-term and open-ended contracts. In Table 6, Column (1) includes only the Inter-Industry match dummy. Column (2) adds this dummy along with several others

(gender, contract type, qualification, 2-digit ATECO 91, and province). These additional controls are also included in Columns (3) through (7). From Column (3) onward, the controls are included both as standalone variables and interacted with t (the match duration) to capture the potentially changing influence of these variables on the likelihood of match termination over time.
Tables report the estimate of the components of vectors α, β, and γ, and of scalar δ. When reading the results, it might be more intuitive to consider the hazard ratio, computed as $\exp\{\delta\}$ (with any of the coefficients). The hazard ratio is the ratio of the instantaneous probability of failure of two groups that differ only by a one-unit increase in the variable of interest for the group in the numerator.

The coefficient of interest is δ, i.e., the coefficient of the inter-industry dummy. Inter-industry matches are consistently less stable than intra-industry matches, as the estimates varies between 0.31 and 0.35 depending on the specifications. This magnitude translate in an hazard ratio of $\exp\{0.31\} = 1.36$. The effect is larger for men then women, and for Fixed Term than Open Ended (see, respectively, Columns (1) and (2) and Columns (7) and (8) from Table 7). Figure 11 plots the hazard estimates by type of match (inter- or intra-industry). If one takes job duration as match quality indicator, they can conclude that inter-industry matches' quality is lower.

The duration of the match is also related to the level of wages, and by the wage growth rate during the match. In particular, increasing by 1 the log weekly average wage (i.e., for example going from 500 to 1,350), the instantaneous probability of failure decreases by 45% (i.e., the hazard ratio is $\exp\{0.37\} = 1.45$). Also, increasing by 1% the wage quarterly growth rate, the hazard probability decreases by 2% (i.e., the hazard ratio is $\exp\{-0.02\} = 0.98$). These facts are consistent with the studies which show a negative relation between wages and labor mobility (e.g., Clark 2001, Pellizzari 2011). Moreover, the fact that the two measures of job quality employed in this study are correlated is an encouraging finding.

6.2 Wages

Figure 12 shows that, on average, the increase in wage from an inter-industry match is significantly lower with respect to the increase from an intra-industry match. This is particularly true for White-Collar workers. On average, women lose money from an inter-industry match, while men gain. On average, blue collar workers gain from both an inter- and intra-industry match, while white-collar workers gain from an intra-industry match, but lose from an inter-industry match (even though the mean is not significantly different from 0). This fact may not be indicative of lower match quality. Indeed, there are multiple potential channels that can explain the phenomenon. It could be explained by inter-industry changes going only towards industries with lower wages. However, Figure 13 shows that this is not the case, as the pattern holds within industries as well.

To eliminate additional possible confounding effects, I regress entry log wages of new matches against a set of dependent variables and fixed effects. Dependent variables are log of wage in previous match, number of employees, unemployment spell, and the interaction between log of wage in previous match and duration of unemployment spell. Fixed effects are at the gender-, province, 2-digit ATECO 91-, and qualification-levels. The regression is:

$$
\begin{aligned}
\log(w_{i,j,m,t}) = {} & \alpha + \beta\phi_{i,j,m} + \gamma \log(\text{Final Wage}_{i,j,m-1}) \\
& + \gamma \text{N Employees}_{j,t} + \delta \text{US}_{i,m} + \eta \text{US}_{i,m} \times \log(w_{i,j,m,t}) + \\
& + \theta_{\text{Qualification}(i,j,m,t)} + \iota_{\text{Gender}_(i)} + \kappa_{\text{Industry}(j)} + \tau_t + \rho_{\text{Province}(j)} + \varepsilon_{i,j,m,t}
\end{aligned}
\tag{2}
$$

where i, j, m, and t are the subscripts for, respectively, worker, firm, number of match between worker i and firm j, and quarter. It is important to identify the number of match between i and j because they could match multiple times during a worker's history. $\phi_{i,j,m}$ takes value 1 is the match is inter-industry and value 0 if the match is intra-industry, Final $\text{Wage}_{i,j,m-1}$ is the wage of worker i at the end of previous match, N $\text{Employees}_{j,t}$ is the number of employees of firm j at time t, $\text{US}_{i,m}$ is the duration, in quarters, of the unemployment spell that preceded the match m, $\varepsilon_{i,j,m,t}$ is the error term. Only the first observation of each match is considered. Thus, t is actually a function of j, m, i.

Results are shown in Table 8. The results are robust to the gradual introduction of all controls and fixed effects. The preferred specification is the one with all covariates (column 10). On average, the wage of an inter-industry match is 4% lower with respect to an intra-industry match. Notice that the estimate would be even larger if industry FEs were not considered (columns (1) to (5)). The coefficients of the other variables are consistent with the literature. Men earn, on average, 15% more than women (consistent with Zizza 2013, Rustichelli et al. 2007), blue-collar workers 25% more than apprentices, white-collar workers 10% more than blue-collar workers, and managers 25% than white-collar workers.

This evidence allows me to conclude that, on average, the match quality of inter-industry matches is lower than the match quality of intra-industry matches, when match quality is measured by wage levels.

7 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[16] had to reinstate the unfairly dismissed worker and pay them compensation. On May 10th, 1990, the Italian Parliament approved 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 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[17], (b) are regulated by an open-ended contract, (c) are not the first match of the worker observed in the dataset, (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 difference-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 behavior (Leonardi and Pica 2013).

The general form of the specification that I employ is

$$I = \tau_t + \gamma_k + \phi_r + \beta' X_{ijt} + \delta_1 D_j^S + \delta_2 (D_j^S \times Post_t) \tag{3}$$

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 it is not possible to 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

16. The threshold is decreased to 5 employees for agricultural firms. However, they are not included in the dataset.
17. 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.

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:

$$\Pr\left\{\text{Sector Change}|X_{ijt}, D_j^S, Post_t\right\} = \tau_t + \gamma_k + \phi_r + \beta' X_{ijt} + \delta_1 D_j^S + \delta_2(D_j^S \times Post_t) \tag{4}$$

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[18]. 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.

8 Results

I run regression 4 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

18. 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 $\beta_1 = ... = \beta_0$.

between 3 and 28 employees (Tables 2 and 3). For robustness, I also provide results for a sample that only include firms between 10 and 20 employees (Tables 9 and 10). Figure 2 plots the probability of inter-industry new matches for treated and control groups and shows that the Parallel Trend Assumption is satisfied.

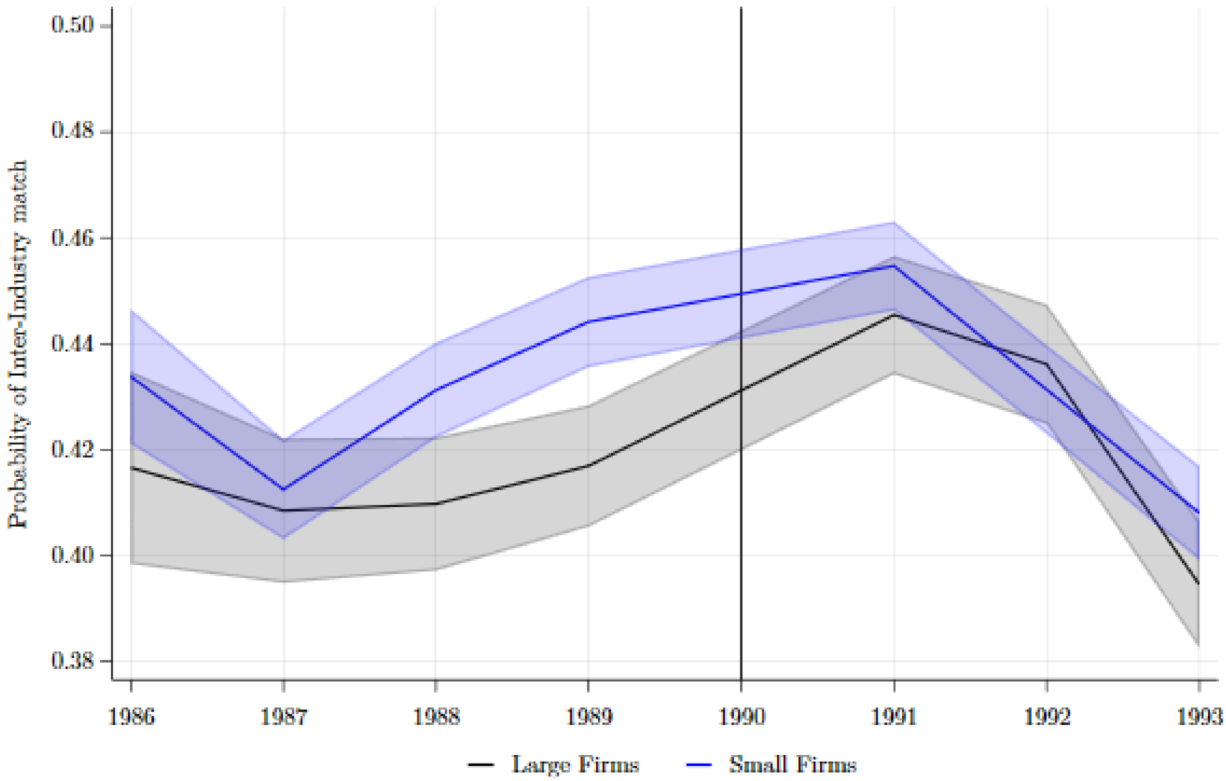


Figure 2: *Probability of Inter-Industry match for Large and Small Firms between 1986 and 1993. Small Firms are those that have between 3 and 15 employees. Large firms are those that have between 16 and 28 employers. The probability is computed as the ratio between the inter-industry new-matches and 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 2 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[19]. 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 the Job to Job dummy and Sector fixed effects. The Job to Job dummy 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

19. 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 (0.004;0.020)	0.010 (0.002;0.019)	-0.004 (-0.012;0.004)	0.018 (0.010;0.026)	0.015 (0.006;0.023)	0.000 (-0.007;0.008)
Post × Treat	-0.019 (-0.031;-0.007)	-0.018 (-0.030;-0.006)	-0.012 (-0.023;-0.001)	-0.014 (-0.026;-0.003)	-0.013 (-0.024;-0.001)	-0.012 (-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 (0.001;0.002)			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 $\in [0, 1]$	100.00	100.00	99.97	100.00	100.00	99.97

Table 2: *Results for Regression 4. 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	0.012 (0.003;0.020)	0.007 (-0.002;0.015)	0.011 (0.003;0.019)	0.018 (0.009;0.026)	0.012 (0.004;0.020)	0.012 (0.004;0.020)
Post × Treat	-0.019 (-0.031;-0.007)	-0.017 (-0.029;-0.006)	-0.015 (-0.027;-0.004)	-0.014 (-0.026;-0.003)	-0.012 (-0.024;-0.001)	-0.013 (-0.025;-0.002)
Post	0.020 (0.011;0.030)	0.024 (0.014;0.034)	0.018 (0.008;0.027)	0.016 (0.007;0.025)	0.020 (0.011;0.030)	0.011 (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 3: *Results for Regression 4. 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.*

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 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[20]. 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

20. 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.

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 3 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 probit estimation of the model (Table 3) leads to very close point estimates (between -1.4% and -1.9%).

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 and probit model are provided, respectively, in Tables 9 and 10. 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).

8.1 Workers' outside option as possible confounding factor

The Diff-in-Diff shows a reduction in industrial mobility of workers following an increase in the strictness of Employment Protection Legislation. While this result is robust to multiple specifications, estimation methods, and sample definitions, the channel that I identify may not be the only channel in action. In particular, another channel might be in action: employers may anticipate that workers in inter-industry matches would be more likely to litigate in case of dismissal. As a result, *ceteris paribus*, it would be more costly to fire them with respect to workers in a intra-industry match. While I do not have data on litigations, I can show empirically that this effect is likely not to be particularly large.

The specific design of the 1990 reform makes the firing cost dependent on whether the dismissed worker accepts the dismissal or decides to litigate. Notice that, in most of the cases, the litigation did not arrive to a courthouse, but it was settled privately between the parties (Galdon-Sanchez and Guell 2000)[21].

21. Even if the parties do not arrive to the judicial stage, an increase of severance pay for cases that are judged unfair

Now, if a category of workers have higher probability of litigation, the expected firing cost for the category is higher. In other words, an increase in firing costs is more relevant for the workers who have a higher probability of litigation. Moreover, the workers who are more likely to litigate are those who face worse labor market conditions (Donohue III and Siegelman 1990 and Ichino, Polo, and Rettore 2003). Figure 14 shows that there is no significant difference between inter- and intra-industry matches in terms of length of unemployment spell after the end of the match. While this evidence does not rule out the possibility of the probability of litigation channel, it shows that the conditions faced on the labor market after an inter- or intra-industry matches are not extremely different. As a result, I do not expect large difference in probability of litigation.

9 Conclusions

This work explores the effects of an increase in Employment Protection (EPL) on inter-industry worker mobility. The theoretical framework builds on Pries and Rogerson (2005). They use a search and matching model with imperfect information and show how stricted employment protection is associated with higher recruitment selectiveness.

Using an Italian employer-employee matched dataset, I show that, at least between 1975 and 1990, inter-industry matches are of lower quality with respect to intra-industry matches, when match-quality is measured by job duration and wage levels. In particular, a survival analysis shows that the hazard ratio of inter-industry matches to intra-industry matches is 1.36: during each quarter of the first 5 years, an inter-industry match is 30% more likely to be terminated (either by the employer of the employee). Moreover, I find that the wage of an inter-industry match is on average 4% lower with respect to the wage of an intra-industry match, after controlling for a number of covariates.

These facts, linked with Pries and Rogerson (2005) framework, leads to the prediction that an increaase in EPL leads to fewer inter-industry matches. To show this, 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. 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.

These findings contribute to the broader literature on EPL by emphasizing its influence not only on

increases the payment agreed in private settlements (Galdon-Sanchez and Güell 2003).

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.

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A Appendix

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 4: *Variables' list in the three datasets.*

		(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,	27.6	49.7	5.4	4.1	651.5	636.7	3.4	6.8
	hunting			(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	39.1	62.0	3.1	2.8	497.6	538.5	5.3	9.1
	restaurants			(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	40.6	63.2	3.7	3.6	557.1	545.7	7.3	6.4
	see caption			(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	42.9	54.3	5.5	4.8	545.2	667.8	4.8	4.4
	see caption			(6.6)	(5.6)	(204.8)	(343.1)		
Total								70,367	487,038

Table 5: *Descriptive Statistics for firms in the sample between 1986 and 1994.*

This table reports descriptive statistics for the time span of 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fixed Coefficients							
Inter-Industry Matches	0.308***	0.311***	0.307***	0.315***	0.320***	0.311***	0.346***
	(0.276;0.341)	(0.276;0.346)	(0.273;0.342)	(0.281;0.350)	(0.285;0.355)	(0.276;0.346)	(0.310;0.383)
$\log(\bar{w})$			-0.364***	-0.383***	-0.359***	-0.323***	-0.342***
			(-0.445;-0.283)	(-0.465;-0.301)	(-0.442;-0.276)	(-0.407;-0.240)	(-0.426;-0.257)
Age				0.000	-0.000	-0.000	0.001
				(-0.001;0.001)	(-0.001;0.001)	(-0.001;0.000)	(-0.000;0.002)
Wage Quarterly Growth Rate					-0.020***	-0.020***	-0.019***
					(-0.026;-0.015)	(-0.026;-0.015)	(-0.025;-0.014)
Duration of Unemployment Spell						0.020***	0.017***
						(0.015;0.024)	(0.012;0.021)
Duration Previous Match							0.004**
							(0.001;0.008)
Time-Varying Coefficients							
$\log(\bar{w}) \times t$			0.004	0.009*	0.007	0.004	0.003
			(-0.006;0.014)	(-0.001;0.019)	(-0.004;0.017)	(-0.006;0.015)	(-0.008;0.014)
Age $\times\ t$				-0.001***	-0.001***	-0.001***	-0.001***
				(-0.001;-0.001)	(-0.001;-0.001)	(-0.001;-0.001)	(-0.001;-0.001)
Wage Quarterly Growth Rate $\times\ t$					0.002***	0.002***	0.002***
					(0.001;0.002)	(0.001;0.002)	(0.001;0.002)
Duration of Unemployment Spell $\times\ t$						-0.001***	-0.001***
						(-0.002;-0.000)	(-0.002;-0.001)
Duration Previous Match $\times\ t$							0.000
							(-0.000;0.001)
Dummy Controls							
Gender		✓	✓	✓	✓	✓	✓
Contract Type		✓	✓	✓	✓	✓	✓
Qualification		✓	✓	✓	✓	✓	✓
2-digit ATECO 91		✓	✓	✓	✓	✓	✓
Province		✓	✓	✓	✓	✓	✓
N. Obs.	17,373	17,373	17,373	17,373	17,373	17,373	15,702

Table 6: *Estimation of Coefficients from Equation 1.*

Efron method is used, to account for tied failures. Estimation is performed using Stata `stcox` *command. A match is included in the analysis if (i) it is not the first match of worker i that is observed in the dataset, (ii) firms j is resident in the provinces of Treviso and Vicenza. A match is inter-industry if i's previous experience is in a firm which operates in a different 2-digit ATECO 91 industry.* w_T *is worker's i weekly wage received from firm j during last quarter of the match, in EUR 2003. Age is in quarters,* w_1 *is worker's i weekly wage received from firm j during first quarter of the match, duration of unemployment spell is the duration of the spell before the match included in the dataset, duration previous match can be computed only in case one is able to know the duration of the match from the dataset (for 15,702 matches out of 17,373). Contract type can be Open Ended or Fixed Term, qualification can be Apprentice, Blue-Collar, White-Collar, Manager, part-time Blue-Collar, part-time White Collar, and part-time Manager. Confidence intervals based on bootstrapped SEs are reported within (brackets) at the 95% confidence level. *** indicates significance at the 1%, ** at 5%, and * at 10%.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Gender		Qualification		Firm Size		Type of Contract	
	Men	Women	Blue-Collar	White-Collar	Small Firms	Big Firms	Fixed Term	Open Ended
Fixed Coefficients								
Inter-Industry Matches	0.498***	0.067	0.404***	0.004	0.347***	0.167	0.251***	0.355***
	(0.451;0.545)	(0.005;0.129)	(0.363;0.445)	(-0.082;0.090)	(0.292;0.401)	(-0.072;0.406)	(0.121;0.381)	(0.317;0.394)
$\log(\bar{w})$	-0.408***	-0.426***	-0.407***	-0.056**	-0.133	-0.712	-1.265***	-0.287***
	(-0.527;-0.289)	(-0.553;-0.299)	(-0.503;-0.310)	(-0.224;0.113)	(-0.261;-0.005)	(-1.331;-0.094)	(-1.632;-0.897)	(-0.374;-0.200)
Age	0.003***	-0.005***	0.002***	-0.010***	0.000	-0.000	-0.017***	0.001
	(0.002;0.004)	(-0.006;-0.003)	(0.001;0.003)	(-0.012;-0.008)	(-0.001;0.002)	(-0.005;0.005)	(-0.025;-0.008)	(-0.000;0.001)
Wage Quarterly Growth Rate	-2.330***	-1.382***	-2.330***	0.096	-2.687***	-0.479	-1.594	-2.048***
	(-3.078;-1.582)	(-2.255;-0.509)	(-2.949;-1.711)	(-1.291;1.482)	(-3.559;-1.815)	(-4.096;3.137)	(-3.593;0.404)	(-2.648;-1.449)
Duration of Unemployment Spell	0.016***	0.023***	0.016***	0.019***	0.014***	0.014	-0.002**	0.019***
	(0.010;0.023)	(0.016;0.030)	(0.011;0.021)	(0.008;0.030)	(0.007;0.021)	(-0.027;0.054)	(-0.021;0.017)	(0.014;0.024)
Duration Previous Match	0.002	0.010***	0.005**	0.005	0.003	0.012	0.016	0.004**
	(-0.003;0.006)	(0.005;0.015)	(0.001;0.009)	(-0.004;0.013)	(-0.002;0.008)	(-0.005;0.029)	(0.002;0.030)	(0.001;0.008)
Time-Varying Coefficients								
$\log(\bar{w}) \times t$	0.005	0.010	-0.003	0.007	-0.011	0.125**	0.162***	-0.003
	(-0.012;0.022)	(-0.006;0.026)	(-0.016;0.010)	(-0.013;0.026)	(-0.027;0.004)	(0.025;0.225)	(0.056;0.268)	(-0.014;0.008)
Age $\times$ t	-0.001***	-0.000***	-0.001***	0.000	-0.001***	-0.001**	0.002**	-0.001***
	(-0.001;-0.001)	(-0.000;-0.000)	(-0.001;-0.001)	(-0.000;0.000)	(-0.001;-0.000)	(-0.001;-0.000)	(0.000;0.003)	(-0.001;-0.001)
Wage Quarterly Growth Rate $\times$ t	0.214***	0.215***	0.222***	0.127	0.236***	-0.186	0.218	0.201***
	(0.149;0.279)	(0.088;0.342)	(0.167;0.276)	(-0.105;0.358)	(0.165;0.307)	(-0.881;0.509)	(-0.226;0.663)	(0.148;0.253)
Duration of Unemployment Spell $\times$ t	-0.001***	-0.001***	-0.002***	0.000	-0.001	-0.002	0.002	-0.002***
	(-0.002;-0.000)	(-0.003;-0.000)	(-0.002;-0.001)	(-0.001;0.002)	(-0.002;0.000)	(-0.010;0.005)	(-0.003;0.007)	(-0.002;-0.001)
Duration Previous Match $\times$ t	0.000	0.000	0.000	0.001	0.001*	-0.001	-0.004**	0.000*
	(-0.000;0.001)	(-0.000;0.001)	(-0.000;0.001)	(-0.000;0.001)	(-0.000;0.001)	(-0.004;0.001)	(-0.007;-0.000)	(-0.000;0.001)
Dummy Controls								
Gender			✓	✓	✓	✓	✓	✓
Contract Type	✓	✓	✓	✓	✓	✓		
Qualification	✓	✓			✓	✓	✓	✓
2-digit ATECO 91	✓	✓	✓	✓	✓	✓	✓	✓
Province	✓	✓	✓	✓	✓	✓	✓	✓
N. Obs.	9,502	6,200	12,677	3,025	6,991	460	1,267	14,435

Table 7: *Heterogeneity Results for Survival Analysis. Main results are shown in Table 6. Details about observations included in paragraph 6.1.1 and in caption of Figure 6. Big firms are firms with more than 15 employees during the time window of the dataset, small firms are firms with less than 15 employees.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Constant	6.267***	2.819***	2.825***	2.869***	2.388***	2.662***	3.500***	3.703***	3.656***	3.658***
	(6.264;6.271)	(2.789;2.848)	(2.796;2.855)	(2.839;2.899)	(2.346;2.430)	(2.577;2.747)	(3.419;3.581)	(3.624;3.782)	(3.577;3.735)	(3.579;3.737)
Inter-Industry Match	-0.022***	-0.022***	-0.021***	-0.023***	-0.024***	-0.040***	-0.035***	-0.041***	-0.040***	-0.041***
	(-0.027;-0.017)	(-0.026;-0.018)	(-0.025;-0.017)	(-0.027;-0.019)	(-0.028;-0.021)	(-0.044;-0.036)	(-0.039;-0.032)	(-0.045;-0.037)	(-0.044;-0.037)	(-0.044;-0.037)
$\log(w_{m-1})$		0.553***	0.541***	0.537***	0.615***	0.571***	0.410***	0.365***	0.370***	0.369***
		(0.548;0.558)	(0.536;0.546)	(0.532;0.542)	(0.608;0.621)	(0.564;0.577)	(0.403;0.417)	(0.358;0.372)	(0.363;0.376)	(0.363;0.376)
N. Employees			0.005***	0.005***	0.005***	0.005***	0.005***	0.004***	0.004***	0.004***
			(0.005;0.006)	(0.005;0.006)	(0.005;0.006)	(0.005;0.006)	(0.004;0.005)	(0.004;0.005)	(0.004;0.005)	(0.004;0.005)
Unemployment Spell				-0.010***	0.257***	0.233***	0.161***	0.142***	0.147***	0.147***
				(-0.011;-0.009)	(0.241;0.274)	(0.217;0.250)	(0.146;0.176)	(0.128;0.157)	(0.133;0.162)	(0.133;0.162)
Unemployment Spell $\times \log(w_{m-1})$					-0.043***	-0.040***	-0.028***	-0.025***	-0.026***	-0.026***
					(-0.046;-0.041)	(-0.042;-0.037)	(-0.030;-0.025)	(-0.027;-0.023)	(-0.028;-0.024)	(-0.028;-0.024)
Qualification										
default: apprentice										
Blue-Collar							0.245***	0.251***	0.250***	0.250***
							(0.240;0.250)	(0.246;0.256)	(0.245;0.255)	(0.245;0.255)
Manager							0.624***	0.610***	0.607***	0.607***
							(0.594;0.653)	(0.582;0.639)	(0.578;0.635)	(0.578;0.636)
White-Collar							0.308***	0.356***	0.354***	0.354***
							(0.301;0.315)	(0.349;0.362)	(0.347;0.361)	(0.347;0.361)
Gender										
default: woman										
Man								0.150**	0.149**	0.149**
								(0.145;0.154)	(0.145;0.153)	(0.145;0.154)
Other FEs										
2-digit ATECO 91						✓	✓	✓	✓	✓
Quarter									✓	✓
Province										✓
Adj. R^2	0.00	0.42	0.44	0.44	0.45	0.48	0.55	0.58	0.58	0.58
N. Obs.	71,993	71,993	71,993	71,993	71,993	71,993	71,993	71,993	71,993	71,993

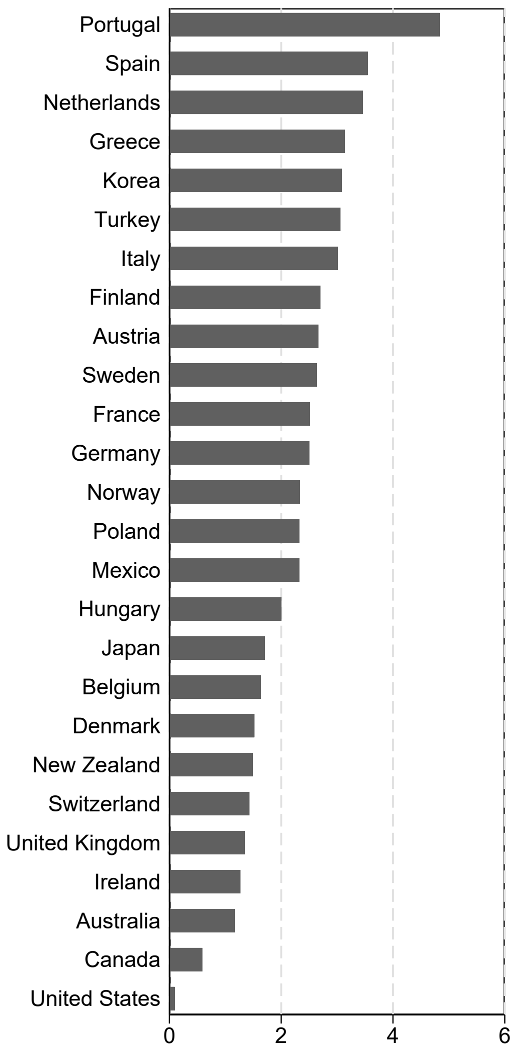
Table 8: *Estimates of Regression 8. Dependent variable is the logarithm of the entry wage of new matches. Each observation in the dataset is a new match, between 1986Q1 and 1989Q4 A new match is included in the sample if (i) it is not the first match observed for each individual and (ii) the firms is resident in the provinces of Treviso and Vicenza. A match is labeled as inter-industry if the worker's previous job was in a firm which operated in a different 2-digit ATECO 91 industry. Wages are weekly, in EUR 2003 units. Unemployment spell is the number of quarters the worker remained unemployed before being hired.*

	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

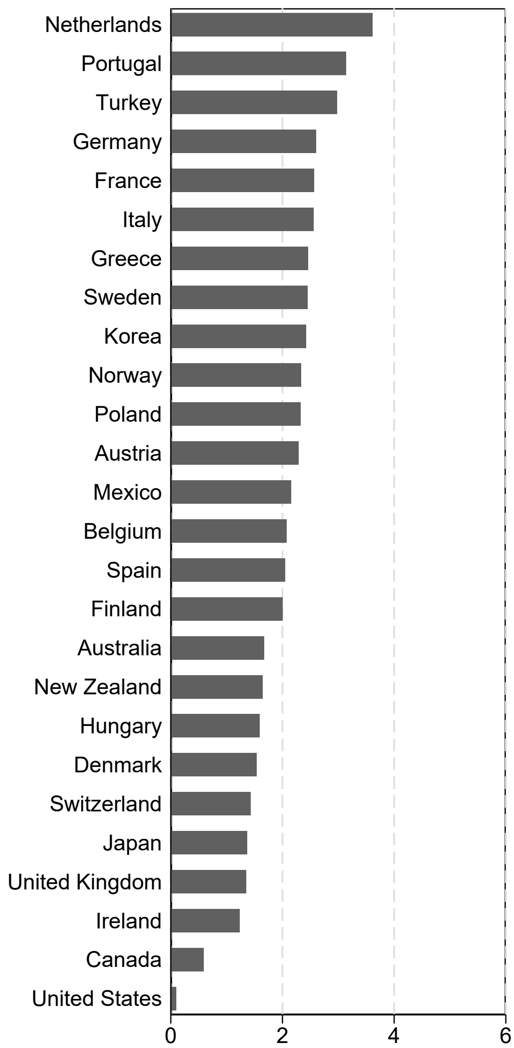
Table 9: *Results for Regression 4. Estimation of Linear Probability Model. A match is included in the sample if (i) it is not the first match observed for each individual, (ii) the firms is resident in the provinces of Treviso and Vicenza, (iii) the firm has between 10 and 20 employees. Pre-treatment 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 10: *Results for Regression 4. Estimation of Probit Model. A match is included in the sample if (i) it is not the first match observed for each individual, (ii) the firms is resident in the provinces of Treviso and Vicenza, (iii) the firm has between 10 and 20 employees. Pre-treatment years 1986-1989, Post-treatment 1991-1994. Year FEs always included.*



***(a)** 1990*



***(b)** 2019*

Figure 3: *OECD Employment Protection Index in 1990 and 2019. Strictness of protection against individual dismissals of regular workers (EPR).*

The figure shows the levels of the OECD Employment Protection Index by country, in 1990 and 2019. The index shown is EPR (strictness of protection against individual dismissals). Scale is from 0 to 6. Details on the index are included in the caption of Figure 4. A similar chart is included in Cahuc, Carcillo, and Zylberberg (2014). Data is from the database OECD Indicators of Employment Protection.

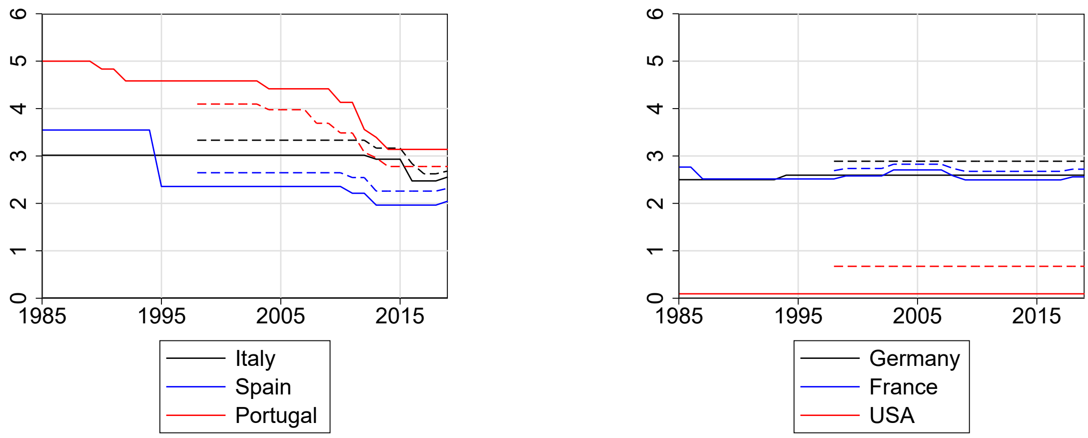


Figure 4: *OECD Employment Protection Index for Italy, Spain, Portugal, Germany, France, and United States.*

The figure shows the evolution of the OECD Employment Protection indexes by country, from 1985 and 2019. Solid line is strictness of protection against individual dismissals of regular workers (EPR), dashed line (EPRC) is a summary of EPR and strictness of protection due to additional regulations on collective dismissals (EPC). EPR, EPC, and EPRC are on a scale from 0 to 6. EPR is available from 1985, EPC and EPRC from 2008. Details on methodology are explained in details in Grubb and Wells (1993), Myant and Brandhuber (2016). Indexes are based on reporting by national governments and OECD Secretariat. EPR is based on nine items on administrative procedures for individual dismissal, required notice and severancy pay for individual dismissals, and conditions under which individual dismissals are fair or unfair. EPC covers the additional costs in case of collective dismissals. Data is from the database OECD Indicators of Employment Protection.

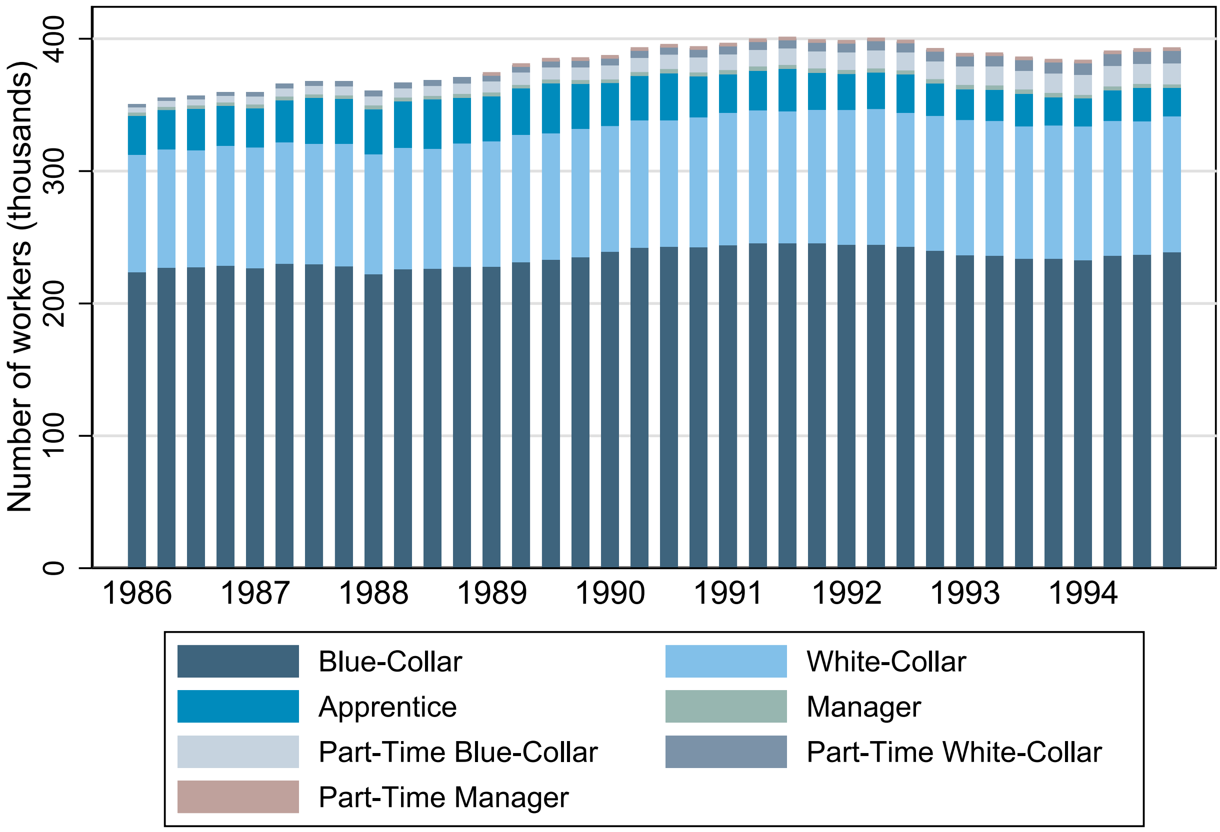
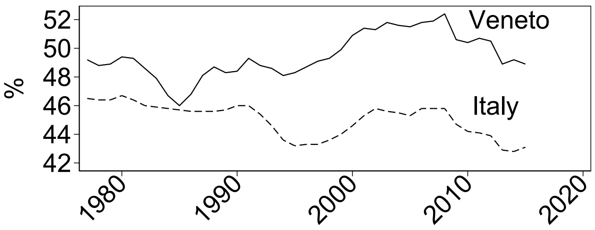
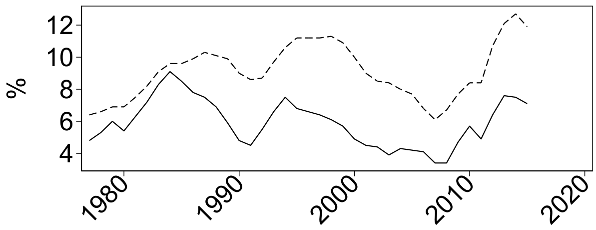


Figure 5: *Number of active workers by qualification and quarter.*

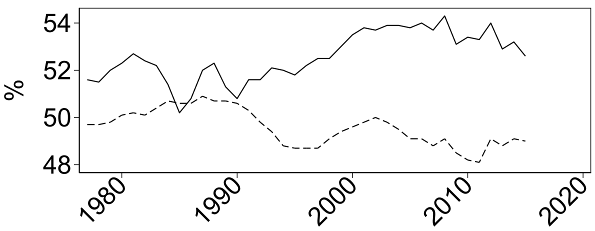
The figure shows the number of workers by qualification and by quarter. Qualification is coded in variable `qualif` *in the raw dataset. Only firms active in the provinces of Treviso and Vicenza are included. Workers with non-specified qualifications are excluded from the sample.*



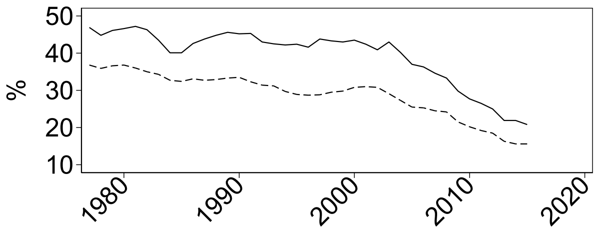
(a) Employment Rate



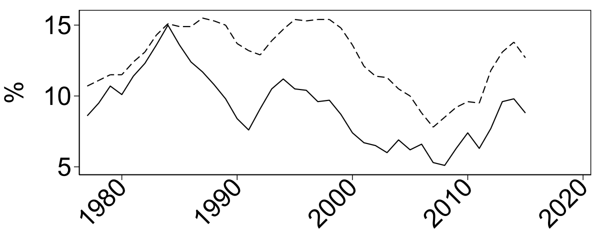
(b) Unemployment Rate



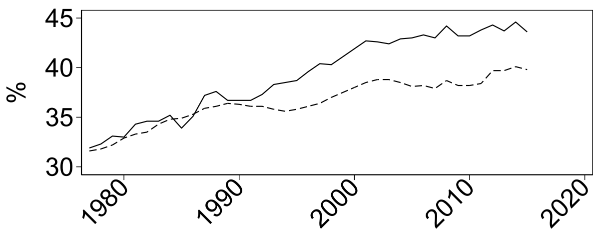
(c) Activity Rate



(d) Youth Employment Rate



(e) Female Unemployment Rate

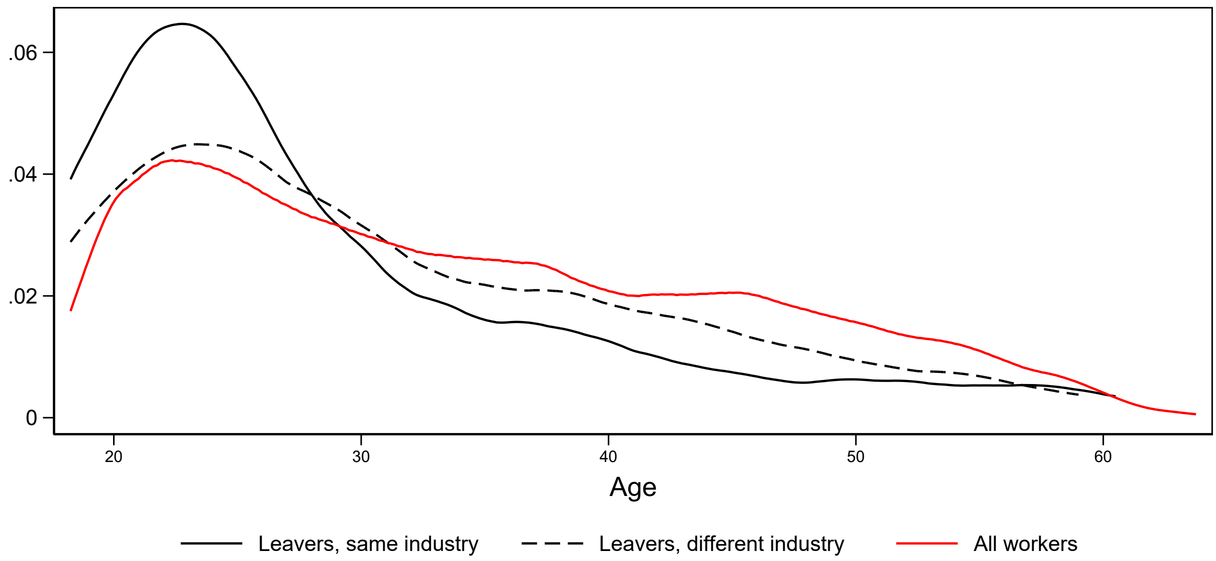


(f) Female Activity Rate

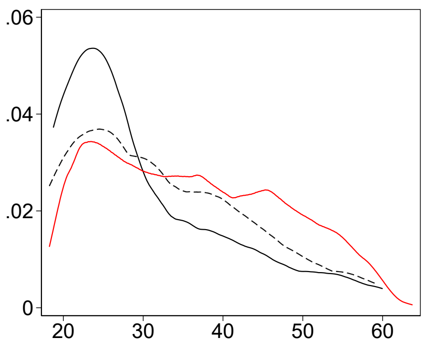
Figure 6: *Labor Macro variables in Veneto (solid line) and Italy (dashed line)*

.

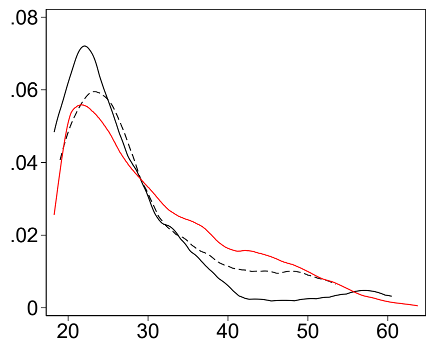
This figure shows employment, unemployment, activity, youth employment, female unemployment, and female activity rates in Veneto (solid line) and Italy (dashed line). Annual frequency, between 1977 and 2015. Youth defined as 15-24. Data from ISTAT.



(a) Men and Women



(b) Men



(c) Women

Figure 7: *Distribution of the age of workers who experience separation from second or further match observed in the dataset, at the time of separation.*

Distribution of the age of workers who experience separation from second or further match observed in the dataset, at the time of separation. Snapshot taken on 1985Q1. Distinction between inter-industry and non-inter-industry matches. The age distribution of all employed individuals is also plotted. A match is included in the sample if (i) it is not the first match observed for each individual, (ii) the firms is resident in the provinces of Treviso and Vicenza, (iii) the firm has had at least 3 and maximum 28 employees between 1982 and 1998, (iii) the match duration is higher than or equal to 4 quarters. A match is defined inter-industry if the previous employment of the individual was at a firm operating in a different 2-digit ATECO industry code.

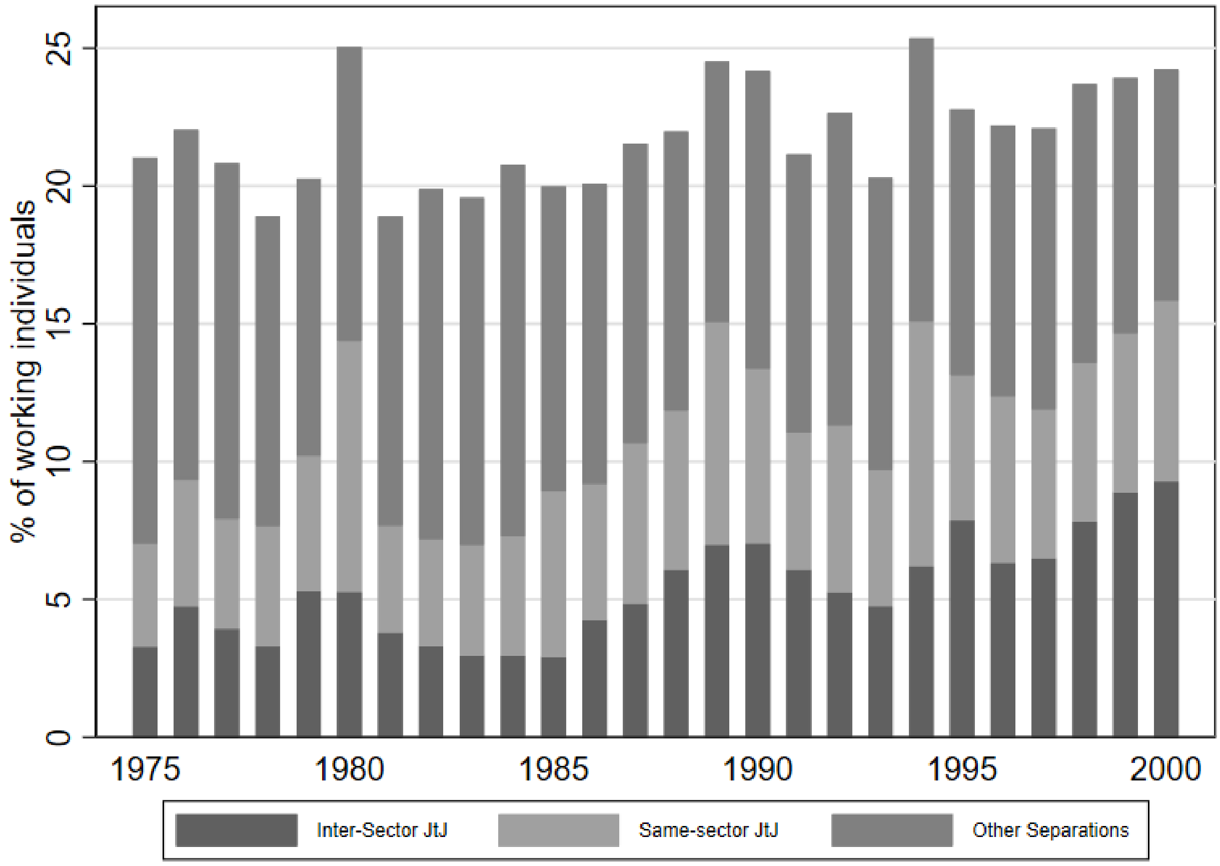


Figure 8: *Proportion of Workers that had a separation within the year.*

This figure shows the proportion of workers who experience a separation, by 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.

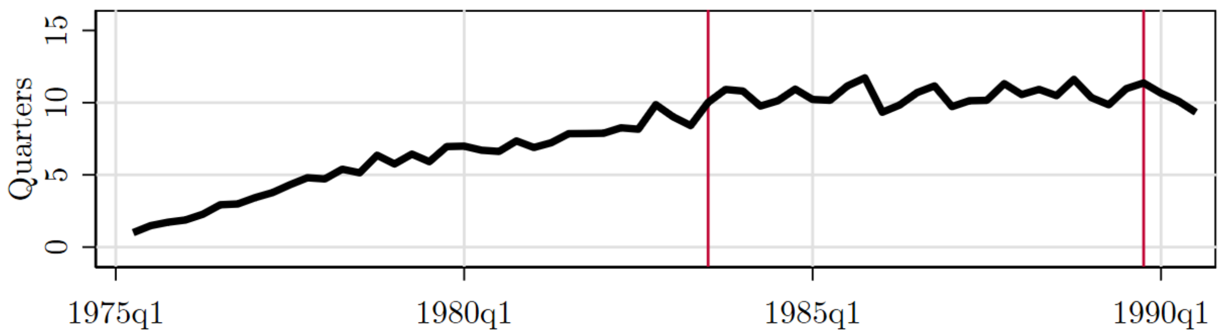


Figure 9: *Average Duration of Match in the dataset built for Survival Analysis.*

First red line on 1984Q4, second on 1989Q4. Workersà histories are considered from 1975 to 1990. A match is included if (i) it is not the first match of the workers in the dataset, (ii) at the end of the match the worker is older than 18 and younger than 64, (iii) the firm is resident in the provinces of Treviso and Vicenza, (iv) within the time span of the dataset, the firm has always had more than 2 but less than 28 employees. A random sample (1/3) of the dataset is considered, for the sake of computability.

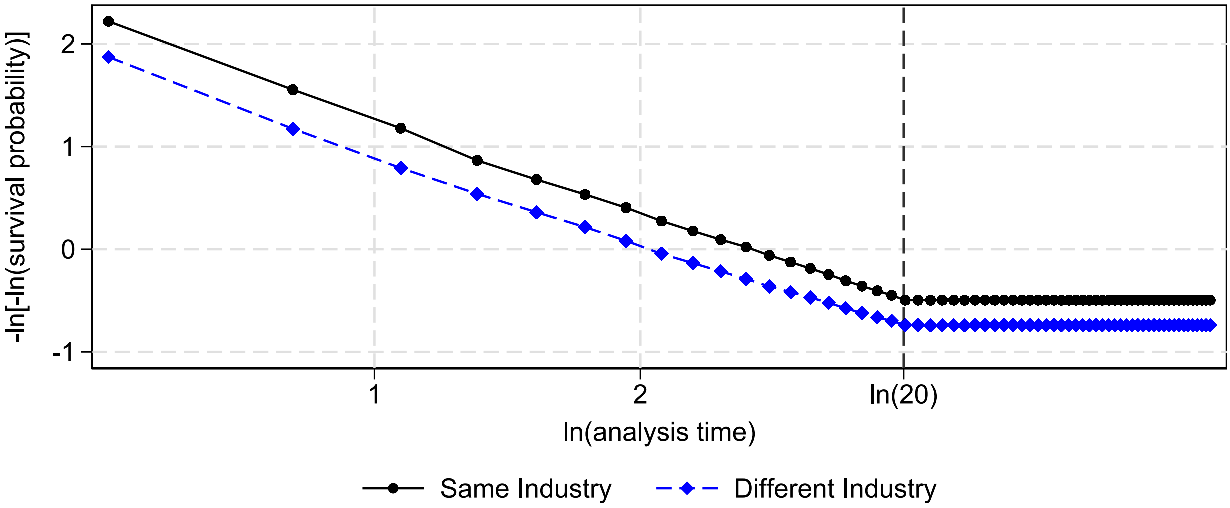


Figure 10: *Plot of* $-\log[-\log(\textit{Survival Probability})]$ *and* $\ln(t)$*, for Intra- and Inter-Industry matches.* t *is in quarters. 20 is the censoring time for all objects. Dataset used is explained in paragraph 6.1.1.*

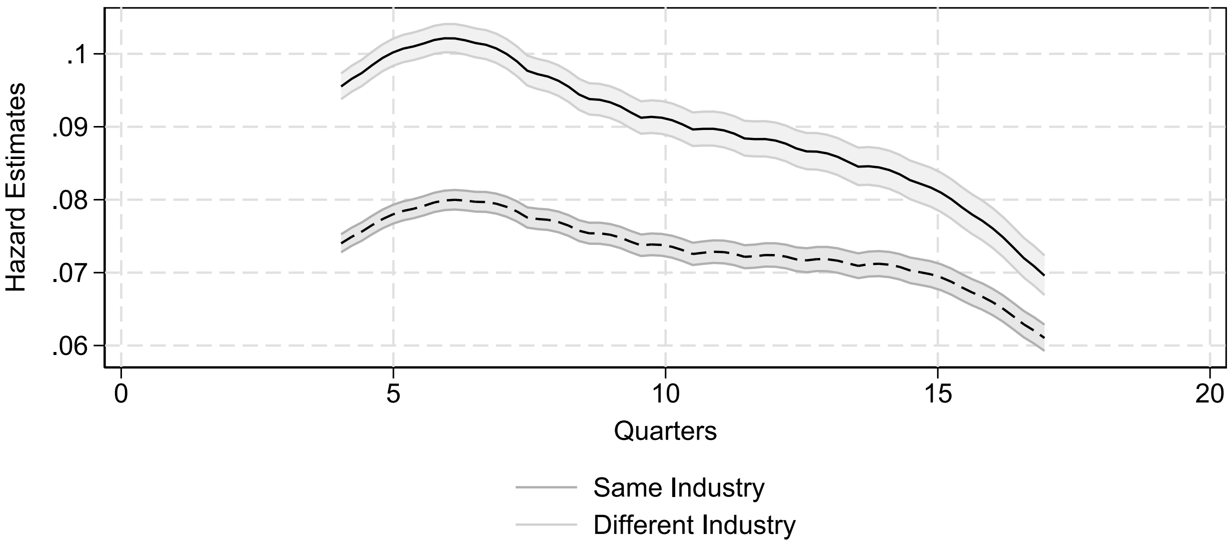
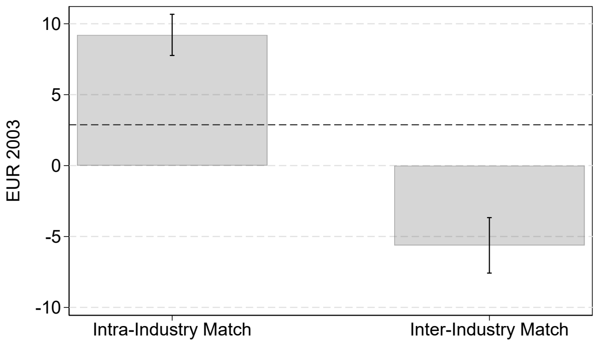
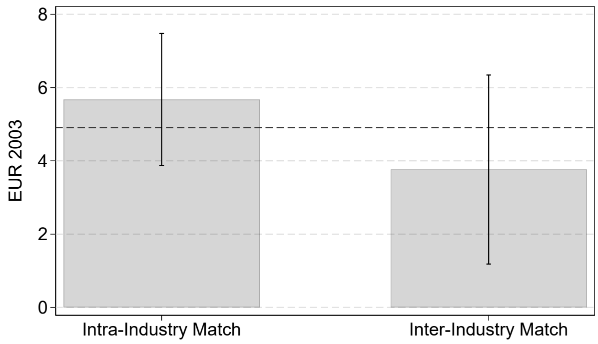


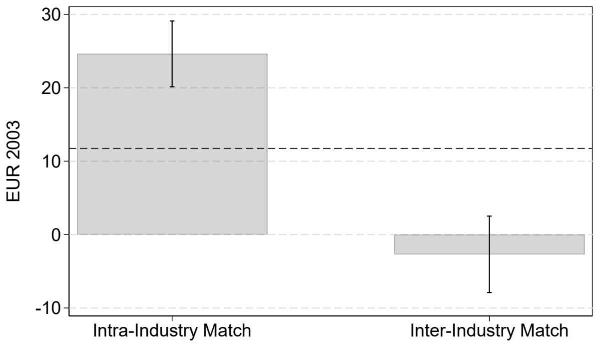
Figure 11: *Hazard Function estimates of match duration, using the dataset described in paragraph 6.1.1. Estimates plotted in this chart do not include covariates, and thus visualize the results of Column (1) from Table 6.*



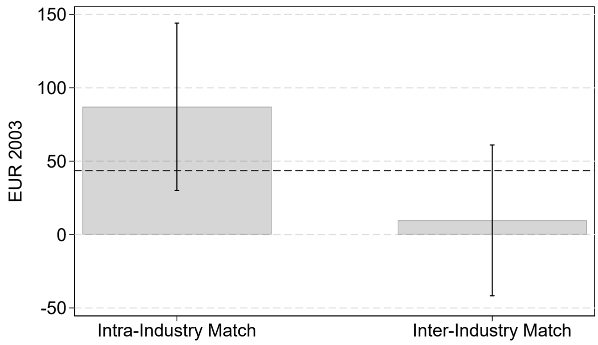
(a) *All Workers*



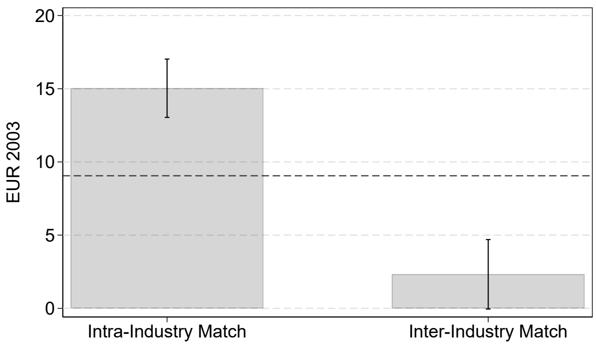
(b) *Blue-Collar Workers*



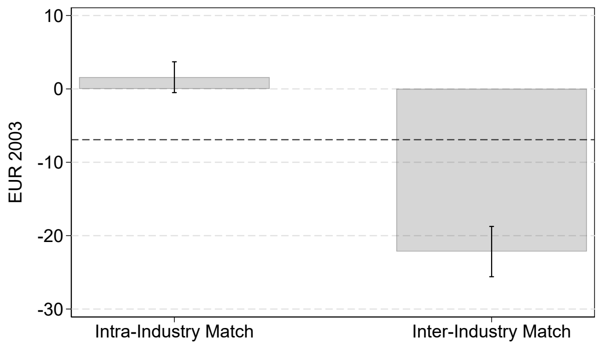
(c) *White-Collar Workers*



(d) *Managers*



(e) *Men*



(f) *Women*

Figure 12: *Mean of difference between weekly wage in previous job and weekly wage in current job, by category of match (inter-industry and intra-industry).*

The chart is provided for the entire sample, and for 5 subsets. The dotted line indicates the mean without distinguishing for categories of match. A match is included in the computation if (i) it is not the first match observed for each individual, (ii) the firm is resident in the provinces of Treviso and Vicenza. A match is defined inter-industry if the previous employment of the individual was at a firm operating in a different 2-digit ATECO industry code. The time window is from 1986q2 to 1989q4.

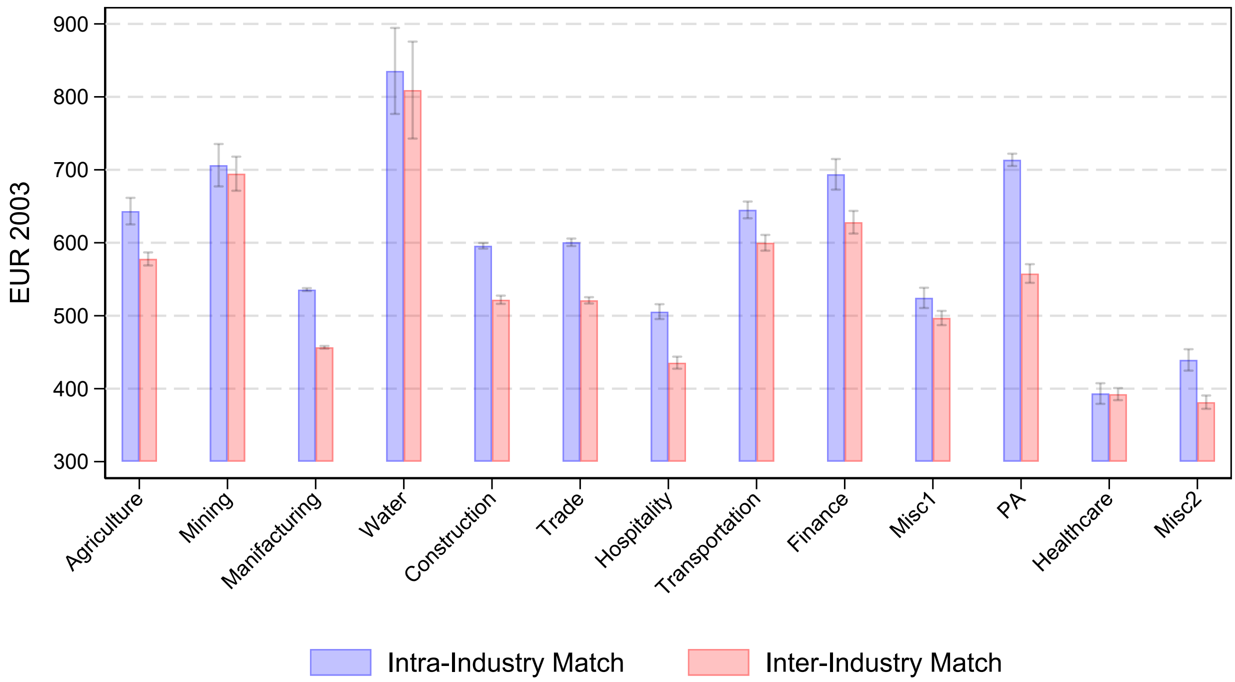
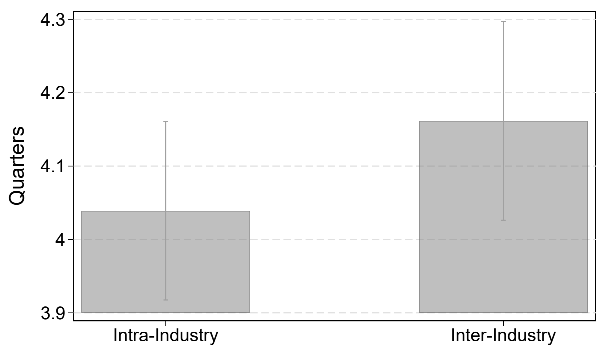
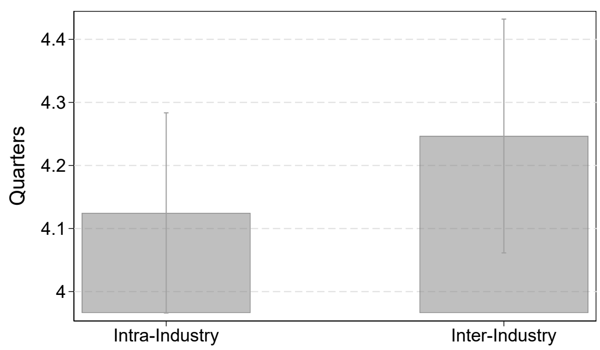


Figure 13: *Mean of entry wages, by category of match (inter-industry and intra-industry) and by 1-digit ATECO 91 industries.*

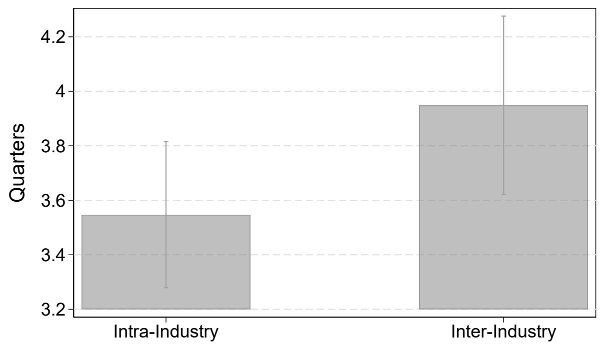
A match is included in the computation if (i) it is not the first match observed for each individual, (ii) the firm is resident in the provinces of Treviso and Vicenza. A match is defined inter-industry if the previous employment of the indiviudal was at a firm operating in a different 2-digit ATECO industry code. The time window is from 1986Q2 to 1989Q4.



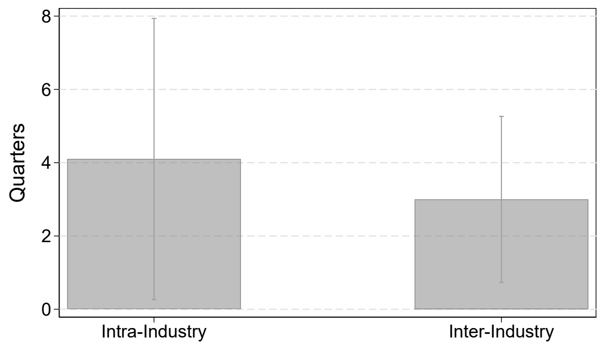
(a) *All Workers*



(b) *Blue-Collar Workers*



(c) *White-Collar Workers*



(d) *Managers*

Figure 14: *Length of Unemployment Spell after Intra- or Inter-Industry Match.*

The figure shows the average length of unemployment spell after an inter- or intra-industry match between 1975 and 1989. An unemployment spell is included if it follows a match that (i) is not the first match observed for each individual (ii) the firm is resident in the provinces of Treviso and Vicenza. A match is defined inter-industry if the previous employment of the individual was at a firm operating in a different 2-digit ATECO industry code. Job-to-job transitions are excluded.