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The Impact of Temporary Contracts on Jobs, Firms and Workers: Evidence from Italy

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The Impact of Temporary Contracts on Jobs, Firms and Workers: Evidence from Italy

by

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Professor Patrick Kline, Chair
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Abstract

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Concerns over labor market flexibility have been at the center of the European political debate for the past three decades (see, e.g., Nickell, 1997). In response to the widespread belief that rigid employment protection laws (EPL) depress employment, many countries — including France, Spain, and Italy — undertook reforms that substantially relaxed legal constraints on the use of temporary employment contracts. Importantly, however, these reforms were often only *partial* in that the degree of employment protection granted to workers hired via permanent employment contracts remained unchanged, leading to a fundamentally dual labor market.

Economic theory delivers ambiguous predictions on the effects of such partial reforms. A number of studies have noted that such policy changes could in principle generate higher overall employment and improved labor market efficiency or alternatively they could lead to a substitution of permanent contracts with rotating temporary contracts and little or no net gain in employment (Bentolila and Saint-Paul, 1992; Cahuc and Postel-Vinay, 2002; Blanchard and Landier, 2002).

In this dissertation, my coauthors Diego Daruich, Sabrina Di Addario and I use detailed Italian social security records matched with firm financial data and a difference-in-differences research design to provide a comprehensive empirical evaluation of an Italian partial reform signed into law in 2001. This reform facilitated the usage of temporary contracts, while maintaining existing employment protections for workers with permanent contracts. Longitudinal data on jobs, firms, and workers permit us to answer three fundamental questions on the impact of this policy change: (1) How did the reform affect overall employment and labor income? (2) What factors contributed to the success or failure of the law in raising

employment and earnings? (3) Were there heterogeneous effects across different worker and firm groups?

In Chapter 1 and 2, we show that, contrary to the stated intent of the law (Biagi and Sacconi, 2001), the reform had little or no effect on aggregate employment, and led to a decline in average earnings. After the reform the Italian labor market became increasingly segmented: more workers were trapped in cycles of low-paid and fragile temporary jobs where the likelihood of transitioning from temporary to permanent jobs fell substantially. On the other hand, consistent with the intention of the law, average firm labor costs fell and mapped into significant increases in profits. The reform generated both winners and losers: its primary beneficiaries were firms, their shareholders and managers, as well as older incumbent workers. By contrast, the earnings of younger workers and new entrants were substantially depressed following the policy change and this widened the inter-cohort gaps in earnings among Italian workers.

In Chapter 3, we abstract from the effect of the reform and focus on the economic forces behind the substantial gap in daily wages between permanent and temporary workers. Informed by the large underrepresentation of temporary contract workers within unions, we investigate the role of employers' pay policies and the lower bargaining power of temporary contract workers. Exploiting within-person daily wage changes for workers who transitioned from a temporary to a permanent contract within the same employer, we find that temporary workers received only 66% of the rents traditionally shared by firms with workers employed under a permanent employment contract.

This dissertation is structured as follows. In Chapter 1, we begin by explaining the Italian institutional background and the 2001 reform that facilitated the creation of temporary employment contracts by firms. We then present a theoretical model to guide our empirical analysis. Chapter 1 concludes by showing how the reform impacted the dynamics of job creation, duration and destruction using Italian social security data.

In Chapter 2 we focus on the effects of the reform on the two fundamental actors operating in the labor market: firms and workers. A particular attention is devoted to analyze how the earnings profile of young workers have been affected, both in the short and in long run, by the introduction of the reform.

Chapter 3 presents our rent sharing estimates that quantify to what extent temporary contract workers have lower bargaining power within the firm compared to permanent contract workers.

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Contents

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Chapter 1

The 2001 Italian reform and its impact on jobs

1.1 Introduction

In this chapter, we introduce the 2001 Italian reform that facilitated the creation of temporary employment contracts while maintaining the employment protection granted to workers hired via permanent employment contracts. We then explain the social security data used in this dissertation to analyze the effects of this reform. Next, we introduce a simple search and matching model that can help us understand both the intended and unintended consequences that such partial reform to employment protection laws has on job flows and ultimately on the level of employment. This chapter concludes by presenting empirical evidence that confirms several predictions obtained from the theoretical model and sets the stage for the empirical analysis derived in Chapter 2 and 3 of this dissertation.

The research design used to analyze the effects of the reform builds on the work of Cappellari et al. (2012) and exploits the staggered implementation of the reform across different collective bargaining agreements (*Contratti Collettivi Nazionali del Lavoro*; CCNL henceforth). While Cappellari et al. (2012) rely on 8 CCNLs and survey information on firms' sector to infer the passage of the reform, we exploit the fact that Italian social security records directly report each worker's CCNL. These unique data allow us to account for the fact that firms can hire employees covered by different CCNLs (Card et al., 2014; Devicienti et al., 2016).

We combine this information with novel data on the renewals of 121 Italian collective bargaining agreements to infer the reform status for over 50 million person year observations, which in Chapter 2 and 3 are going to be subsequently matched with the universe

of financial records of Italian limited liability companies (CERVED, 2014). We show that outcomes follow parallel trends prior to the implementation of the reform, indicating that observations from CCNLs yet to be reformed can be used to gauge counterfactual outcomes for observations in reformed CCNLs in the absence of the reform. Relatedly, we show that there is relatively little endogenous sorting of workers across different CCNLs, and that the composition of new entrants before and after the reform is well balanced across a large set of observable individual characteristics.

To guide our empirical analysis we develop a search and matching model based on Cahuc et al. (2016) that captures the dynamic incentives of reforms to employment protection. The model suggests that partial EPL reforms can generate both intended and unintended consequences. The latter are driven by the increase in the turnover of temporary jobs as this type of policy change reduces incentives to sign longer temporary contracts as well as to convert workers originally hired under a temporary contract to a permanent position. Quantifying the extent of both intended and unintended consequences on the dynamics of job creation and destruction represents the main focus of this Chapter.

We find that, consistent with the intended consequences of the law, the reform fostered job creation and increased the share of new jobs signed under a temporary contract. Offsetting this rise in job creation, however, we find that the rate of separation for workers hired under a temporary contract increased after the reform. This change was primarily driven by decreases in the probability that a temporary contract is renewed and mapped into significant increases in the transition rate from a temporary contract in one year to non-employment in the next year. We also find that after the reform the majority of new jobs were filled by workers who came directly from another job (i.e., a job-to-job transition), rather than by workers coming from non-employment. Looking more closely at the jobs themselves we find that, after the passage of the reform, the total number of days worked under a temporary contract decreased by approximately 5%. This effect mapped into a significant increase in what we define as the "contract gap," i.e., differences in outcomes between permanent and temporary jobs. We observe an expansion of the contract gap in earnings of around 10% following the reform.

Our analysis of job flows provides new quasi-experimental evidence of the role of labor market flexibility and temporary contracts in the dynamics of job creation and destruction. This relationship has typically been studied either by comparing cross-country aggregates (Bertola, 1990; Bertola and Rogerson, 1997; Boeri, 1999; Bassanini and Marianna, 2009) or by relying on individual data based on surveys combined with selection on observables

techniques (Bover and Gómez, 2004; Gagliarducci, 2005; Güell and Petrongolo, 2007; Picchio, 2008). Focusing on individual transitions across employers and employment contracts permits us to decompose the responses of job creation and destruction to the policy change. Moreover, we can also test crucial predictions from our model such as the negative impact of the reform on the likelihood of converting temporary contracts to permanent ones. We also document how partial reforms targeting only the employment protection of temporary contracts map into a widening of the duality in key labor market outcomes across employment contracts. These findings connect to an older literature that examines the existence and consequences of dual labor markets (Dickens and Lang, 1985; Rebitzer and Taylor, 1991).

1.2 Institutional Background

In Italy, most workers are hired under permanent employment contracts. A permanent contract does not have a termination date. Firms that wish to separate from a worker hired under a permanent contract have to pay high firing costs depending on firm size and tenure (Kugler and Pica, 2008).[1] Italian employers can also using temporary contracts. These contracts are characterized by a termination date. Once the temporary contract reaches its termination date, the employer can dismiss the worker without incurring in any firing costs. Some types of temporary contracts are also associated with lower costs for firms in terms of social security contributions (Cappellari et al., 2012).

Traditionally, temporary contracts could only be adopted under specific circumstances (e.g., replacing a worker on sick leave). Employers had to provide to the social security agency a written notice demonstrating the existence of the particular circumstance justifying the use of the temporary contract. The list of admissible cases, as well as additional regulatory aspects such as temporary contract renewals and conversions to permanent contracts were regulated by a law dating back to 1962.[2]

The rules regarding temporary contracts changed with decree 368 signed into law on 09/06/2001 and based on EU directive 1999/70/CE. This reform replaced the strict inventory of cases under which temporary contracts were allowed with a general provision stating

[1] Firms can separate from permanent workers only if they can demonstrate in a labor court of law either financial difficulties (objective reason) or a breach of proper conduct by the worker (subjective reason).

[2] Typically, renewals of temporary contracts were highly regulated. If the limit on the number of renewals of a temporary contract was reached, or the employer failed to properly demonstrate the existence of a specific circumstance, the temporary contract was *automatically* converted to a permanent contract (Cappellari et al., 2012).

that employers can now rely on temporary employment for any "technical, production, organizational, and substitution" reason (Biagi, 2002).

Law 368/2001 therefore eased the utilization of temporary contracts by relieving firms of the hassle costs of demonstrating the existence of a specific circumstance justifying the usage of temporary contracts. This reform followed a previous law (the "Treu Package"), signed in 1997, that introduced temporary work agencies and liberalized apprenticeships contracts.[3] According to the Italian government of the time, the change to labor market institutions achieved with law 368/2001 would (i) increase firm performance, (ii) increase the country's employment rate, and (iii) provide better employment opportunities to younger generations (Biagi and Sacconi, 2001).

The Italian reform process between the 1990s and 2000s, for which decree 368/2001 represents a fundamental milestone, focused on facilitating hiring under flexible and increasingly deregulated temporary contracts without modifying the employment protection of permanent contracts. Figure A.1.1 shows that such an asymmetric reform was instrumental in establishing a de-facto dual labor market in employment protection (Boeri, 2011). The employment protection granted to temporary contracts was greatly reduced with the reform of 2001 while the employment protection associated with permanent contracts was relatively high and has remained unchanged for almost 30 years.[4]

1.3 Model

We now introduce a theoretical framework that illustrates how the reform described above may influence the dynamics of job creation, duration, and destruction. We provide a qualitative analysis of the policy change based on the matching setup of Cahuc et al. (2016) that endogenizes the choice of creating a job under a temporary vs. a permanent contract. We show that the reform:

1. Increases temporary job creation.

2. Increases temporary job destruction.

3. Decreases the duration of temporary jobs.

[3] A third reform, decree 30 of 2003 (the "Biagi Law"), introduced new, atypical contractual temporary arrangements.

[4] See Sestito and Viviano (2016) for the study of a 2015 Italian reform that relaxed the firing costs associated with permanent employment contracts.

The first two conclusions are common in the analysis of partial labor market reforms of employment protection (Bentolila and Saint-Paul, 1992; Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002; Alonso-Borrego et al., 2011).[5] The third prediction is new and stems from the fact that in the model the duration of a temporary contract represents an endogenous outcome.

Increases in the churning of temporary jobs represents an unintended consequence of this reform and one of the major motifs for why reducing the employment protection of temporary contracts but not of permanent ones can "backfire" and lead to lower employment, lower output and lower welfare for workers. The model makes it clear, however, that such backfiring of the reform represents only one possible scenario. Whether the unintended consequences of the reform are going to dominate the intended ones is ultimately an empirical question.

We now introduce the model, define the labor market equilibrium, and provide a qualitative analysis of the effect of the reform. All derivations are in Appendix C.1.

1.3.1 Setup

A continuum of risk-neutral, infinitely lived firms and workers with a common discount rate r operate in the labor market. There is no ex-ante heterogeneity across workers and their measure is normalized to 1. Firms use labor as the only input in production. All jobs produce the same amount of output $y > 0$ but differ in the Poisson rate λ at which they start producing zero output and hence become unproductive.[6] The job type $\lambda \in [\underline{\lambda}; \bar{\lambda}]$ is drawn at random from a distribution with cdf $G(\lambda)$ and pdf $g(\lambda)$. The value of λ is observed ex-ante by both workers and firms upon matching.

Firms and workers are assumed to bargain over the contract that maximizes the associated expected surplus. Two types of contracts can be created: permanent and temporary. Permanent contracts do not have a termination date and firms pay a red-tape cost f in case of separation. We extend the analysis of Cahuc et al. (2016) by allowing for heterogeneous matching costs, with c_P denoting the cost paid by the firm in writing a permanent contract (Pissarides, 2009). Temporary contracts endogenously specify a termination date. By assumption, a temporary contract cannot be renegotiated. If the job turns out to be unpro-

[5] This literature is connected with previous work that has shown how reductions in employment protection have ex-ante ambiguous impact on aggregate employment (Bertola, 1990; Bentolila and Bertola, 1990; Lazear, 1990).

[6] Allowing for more realistic productivity processes complicates, but does not alter, the main qualitative conclusions.

ductive before the termination date, the employer must continue to pay the worker until the termination date. At termination, the match can dissolve at zero cost or can be transformed into a permanent job with a new bargained wage.[7] Writing a temporary contract implies a one-time administrative cost c_T paid by the firm. The reform described in Section 1.2 is interpreted as an exogenous decrease in c_T.

The rationale for using temporary contracts stems from the combination of firing costs for permanent jobs and heterogeneity in the rate at which jobs become unproductive. Jobs with a very short expected duration are unlikely to be created under a permanent contract since such jobs will turn unproductive relatively fast, potentially forcing firms to pay the firing cost.[8]

An alternative view is to consider temporary contracts as a screening device to learn about the true productivity of workers (Blanchard and Landier, 2002; Faccini, 2014). However, as argued by Cahuc et al. (2016), learning models tend to ignore that permanent contracts allow for a probationary period (typically of 6 months in Italy) and the observed duration of a temporary contract is often shorter than that (the median duration is 4 months in our data). Moreover, learning motives are hard to reconcile with the observation that temporary contracts are more intensively used in sectors where production opportunities are relatively short (e.g., hotel or restaurant sector; see Bassanini and Garnero, 2013).

1.3.2 Choice Rule

We begin by characterizing the process of job creation and destruction for a fixed value of unemployment. The rate at which workers and firms are brought together in the search market and associated labor market equilibrium are derived next. We impose the condition $f < U$ so that permanent jobs that become unproductive are always destroyed. As detailed in Appendix C.1, the surplus of creating a permanent job with an expected duration $1/\lambda$ is given by

$$S_P(\lambda) = (y - rU) \int_0^\infty e^{-(r+\lambda)\tau} d\tau - \int_0^\infty f\lambda e^{-(r+\lambda)\tau} d\tau. \tag{1.1}$$

The first term of this equation gives the present discounted value (PDV) of the surplus generated from the job, which is equal to production y minus the flow value of the outside

[7] This implies that temporary contracts cannot be renewed ad infinitum, in line with Italian and other European institutional features. See also Section 5.3 of Cahuc et al. (2012).

[8] Dräger and Marx (2012) provide direct evidence for this point: using cross-country firm-level data, they show that the creation of temporary jobs responds positively to workload fluctuations especially in countries with strong duality in employment protection between permanent and temporary contracts.

option rU. This term is multiplied by the survival function $e^{-\lambda\tau}$ and is discounted by $e^{-r\tau}$. Recall that a job becomes unproductive at (random) date τ and shocks are assumed to arrive at a constant Poisson rate.

The surplus of a temporary job of type λ and duration D is given by

$$S_T(\lambda, D) = \int_0^D \left(ye^{-\lambda\tau} - rU\right) e^{-r\tau} d\tau + \{\max[S_P(\lambda), 0]e^{-\lambda D}\}e^{-rD} - c_T. \tag{1.2}$$

In equation (1.2), the surplus of a job is given by y times the survival function $e^{-\lambda\tau}$. Regardless of the date of destruction, a worker covered by a temporary contract remains employed up to D, which is why the term rU is not multiplied by the survival probability. A temporary job has an option value of becoming permanent, $\max[S_P(\lambda), 0]$, provided that the job remains productive up to the termination date D, an event that occurs with probability $e^{-\lambda D}$ and discounted by e^{-rD}. The optimal duration is the one that maximizes the surplus of a temporary job and it is therefore given by

$$D^*(\lambda) = \begin{cases} \dfrac{1}{\lambda}\log\left(\dfrac{rU + \lambda r + (r+\lambda)c_P}{rU}\right) & \text{if } \lambda \leq \lambda_P \\ \\ \dfrac{1}{\lambda}\log\left(\dfrac{y}{rU}\right) & \text{if } \lambda \geq \lambda_P \end{cases} \tag{1.3}$$

where λ_P is such that $S_P(\lambda_P) = 0$. The function $D^*(\lambda)$ is continuous and increasing in the expected duration of a job, $1/\lambda$, and decreasing in the value of unemployment. It follows that the optimal surplus is given by $S_T(\lambda) = S_T(\lambda, D^*(\lambda))$.

Proposition 1 *Let $\lambda_T = \{\lambda : S_T(\lambda) = 0\}$ and $\lambda_E = \{\lambda : S_P(\lambda) = S_T(\lambda)\}$. If $S_T(\lambda_P) > 0$, then there exists unique values $\lambda_T > \lambda_P > \lambda_E$ such that:*

1. *If $\lambda \in (0; \lambda_E]$, it is optimal to create permanent jobs.*
2. *If $\lambda \in (\lambda_E; \lambda_T]$, it is optimal to create temporary jobs.*
3. *If $\lambda \in (\lambda_E, \lambda_P]$, temporary jobs will be converted to permanent ones.*
4. *If $\lambda \in (\lambda_P; \lambda_T]$, temporary jobs will not be converted to permanent ones.*
5. *If $\lambda > \lambda_T$, no jobs will be created.*

See Figure 1.1, Panel (a) for a graphical description and Appendix C.1 for further details.

Heterogeneity in the arrival rate of shocks generates a tradeoff between the choice to create temporary contracts (which cannot be destroyed before the termination date) and permanent contracts (whose separation implies the payment of firing cost f). Accordingly, when $\lambda \in (0; \lambda_E]$ it is optimal to create a permanent contract while when $\lambda \in (\lambda_E; \lambda_T]$, this surplus is going to be higher under a temporary contract. For some types of jobs (i.e., those with $\lambda \in (\lambda_E, \lambda_P]$), it is optimal to create them as temporary jobs and eventually convert them to permanent at date D, provided that they have survived up to this date. On the other hand, jobs with $\lambda \in (\lambda_P; \lambda_T]$ generate negative surplus under a permanent contract and therefore are destroyed when the temporary contract hits expiration. The extent of such "precautionary firing" is crucially driven by the presence of firing costs $f > 0$. Jobs with a very short expected duration (i.e., those with $\lambda > \lambda_T$) generate negative surpluses regardless of the contract chosen and therefore will not be created. Finally, notice that a key corollary of this model is that differences in earnings between temporary and permanent contracts stem primarily from differences in duration. In the empirical analysis, we can test this prediction by exploiting micro-data on employment contracts, duration, and earnings.

1.3.3 Labor Market Equilibrium

There is a standard constant return to scale matching technology given by $m(u, v)$ where u represents unemployed workers and v vacancies. Let $\theta \equiv v/u$ denote labor market tightness. Jobs are filled with probability $q(\theta) = m(u, v)/v$ while job seekers find jobs at rate $\theta q(\theta)$.

Keeping a job open implies a cost for the firm of $\kappa > 0$. If there is a job contact, then both the firm and the worker learn the true value of λ and decide whether to create the job with wages determined by Nash bargaining.[9] In particular, we let $\gamma \in [0, 1]$ denote the fixed share of surplus retained by workers. Hence in this benchmark model temporary and permanent workers have the same bargaining power which rules out one additional potential reason to hire temporary workers: they can extract a smaller share of surplus from a given production opportunity. This assumption is imposed to simplify the qualitative analysis of the model and it is going to be relaxed theoretically in Appendix C.2 and empirically in Chapter 3.

[9]This timing is slightly different from Cahuc et al. (2016) which assumes the existence of a sunk cost paid by firms up front in order to draw a production opportunity. Our timing assumption does not modify the key implications of the original setup of Cahuc et al. (2016), while simplifying the qualitative analysis of Section 1.3.4.

As detailed in Appendix C.1, the free-entry condition in the search market allows us to pin down the equilibrium value of θ from the following equation:

$$\kappa = q(\theta)(1-\gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda)\right], \tag{1.4}$$

and the value of unemployment can be written

$$rU = z + \kappa\frac{\gamma\theta}{(1-\gamma)}, \tag{1.5}$$

where z is the flow utility of being unemployed. Equation (1.5) can therefore be used to substitute rU in the equation defining $(\lambda_E, \lambda_P, \lambda_T)$; note that (λ_T, λ_P) are decreasing in θ while λ_E is increasing in tightness.[10] Proposition 2 characterizes the labor market equilibrium given the choice rule defined in Proposition 1.

Proposition 2 *Given the conditions highlighted in Proposition 1, the labor market equilibrium in a context where both temporary and permanent jobs are created, provided that it exists, is unique and given by the quadruple* $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_T^*)$ *solving equation (1.4) and*

$$\begin{aligned}\lambda_P^* &= \{\lambda : S_P(\lambda) = 0\},\\ \lambda_T^* &= \{\lambda : S_T(\lambda) = 0\},\\ \lambda_E^* &= \{\lambda : S_P(\lambda) = S_T(\lambda)\}.\end{aligned}$$

1.3.4 Comparative Static Analysis

Proposition 3 *Given the labor market equilibrium defined in Proposition 2, a reduction in the cost of signing a temporary contract,* c_T*, implies:*

1. *An increase in the number of jobs created.*
2. *A decrease in the duration of a temporary job.*
3. *An increase in temporary job destruction.*

See Appendix C.1.3 for the proof.

[10] This follows from the fact that, for a given λ, permanent workers are more likely to be exposed to unemployment compared to temporary workers as temporary contracts cannot be terminated before the termination date. An increase in rU therefore pushes the optimal contract marginally toward more permanent contracts.

For a fixed θ, the reform opens up previously unexplored production opportunities that can now be explored via a temporary contract (i.e., λ_T increases). However, this direct effect is potentially offset by the fact that the reform increases labor market tightness, which improves the option value of search. The associated increase in the value of unemployment implies a decrease in λ_T. In Appendix C.1.3, we prove that the direct effect dominates the equilibrium feedback effect so that the reform is unambiguously associated with the creation of more jobs signed using a temporary employment contract.

The reform also shifts the incentives associated with the conversion of temporary contracts into permanent contracts. Higher labor market tightness and a decrease in the matching cost c_T increases the opportunity cost of converting a temporary contract into a permanent one. Hence the reform *unambiguously* reduces λ_P; i.e., post-reform we have an increase in the job destruction rate of temporary jobs. Moreover, a higher value of unemployment drives down the surplus associated with all jobs and therefore lowers the duration of temporary jobs, see equation (1.3).

Finally, the reform also affects the threshold λ_E that governs the substitution between temporary and permanent contracts at the moment of job creation. A decrease in the cost of writing temporary contracts implies a direct effect: firms take advantage of the lower matching cost so that, for a fixed θ, a lower fraction of jobs are now created under a permanent contract (i.e., there is a decrease in λ_E). This direct effect is again potentially offset by an increase in tightness, which implies an increase in rU and hence an *increase* in λ_E. In this case the overall effect on λ_E appears ex-ante ambiguous and highly dependent on the distribution of production opportunities and other primitives of the model.[11]

The implications of the reform are summarized in Figure 1.1, Panel (b), which is drawn assuming an overall negative effect of the reform on λ_E. Three forces drive up the separation rates of temporary jobs: (i) new temporary jobs of shorter expected duration are now created (red area); (ii) temporary jobs that used to be converted to permanent are now destroyed (purple area); (iii) all temporary jobs have shorter duration.

All these effects, combined with the substitution effect highlighted in the yellow area, counteract the increase in job creation generating an a-priori ambiguous effect on the steady-state employment rate. A direct corollary is that the impact of the policy change on labor market efficiency is also ambiguous.[12] In Appendix C.1.4, we formally show that the after

[11] In Appendix C.1.3, we characterize when the direct effect is going to dominate the equilibrium feedback effect and provide a sufficient condition.

[12] To measure labor market efficiency we consider the steady state surplus of the model, defined as the

reform increases to net output stemming from lower labor costs and the opening of new production opportunities (the intended consequences) can be potentially nullified by net output reductions due to the after reform substitution of permanent jobs with temporary ones, the destruction of temporary jobs that used to be converted into permanent, and the reduction in the duration of *all* temporary jobs (the unintended consequences).

The qualitative analysis provided here echoes the concerns of Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002) who warn that reforms facilitating the creation of temporary jobs, *without affecting the employment protection of incumbent/permanent workers*, can potentially lead to a scenario of higher unemployment, lower output and lower welfare, especially for younger workers. We next turn to the data to validate the predictions of the model, evaluate the ex-ante ambiguous effects and quantify the distributional impacts of this policy change on both firms and workers.

1.4 Data

Here we describe the data used for the empirical analysis of this disseration. Section 1.4.1 introduces the social security records that represent our primary data source. Section 1.4.2 outlines the data collection on collective bargaining agreements, which is instrumental to our research design.

1.4.1 Social Security Data

Our main data source is social security data from the Italian Social Security Institute (*Istituto Nazionale Previdenza Sociale*, INPS).[13] This dataset provides the *complete* employment history between 1990–2013 of all workers who were employed at one point in a firm covered by a survey run by the Bank of Italy, named INVIND.

The INVIND survey has been substantially improved over time both in terms of sample size and reference group. The survey started as being representative only of manufacturing firms with 50 or more employees. Starting in 1999, more industrial sectors were added (e.g., the energy sector). In 2001 also firms with 20–49 employees were included and finally in 2002 service firms were added to INVIND (Bank of Italy, 2008).

sum of production of filled jobs minus firing, matching and vacancy costs.

[13]With the exception of workers employed in some specific public sectors and self employment, nearly all employees in Italy are covered by INPS.

This social security data – henceforth labelled as INPS-INVIND – is a matched employer-employee dataset that contains information on employment spells. For each employment spell, we have data on the earnings, the number of days worked, work status (blue collar, white collar, mid-manager, manager), and demographic information on the employee (e.g., age, gender, nationality).[14] Unfortunately, education is not recorded. Critical for our purposes, INPS-INVIND provides administrative information on the type of contract (permanent vs. temporary) for each employment spell from 1998 on. The three main types of temporary contracts are: apprenticeships, temporary contracts obtained via a temporary work agency, and temporary jobs offered directly by the firm.[15] We restrict attention to workers who were always employed in the private sector.

Overall, we have information on approximately 128 million spell-year observations representing 15–20% of the population of Italian workers. As an administrative datasource, INPS-INVIND provides detailed information on the type of employment contract as well as on key outcomes such as earnings, that is free of measurement error. However, some important limitations should be stressed. As with most matched employer-employee data, it is not possible to distinguish between unemployment, non-participation, self-employment or participation in the informal sector, where the latter is fairly present, especially in the south of Italy (Mantegazza et al., 2014).

1.4.2 Collective Bargaining Agreement Data

As detailed in Section 1.5, our research design hinges on information on the years of renewals associated with each collective bargaining agreement (*Contratti Collettivi Nazionali del Lavoro*; henceforth CCNL). INPS-INVIND provides information on the CCNL associated to each job. Information on renewal years is provided by *Centro Nazionale dell'Economia e del Lavoro* (CNEL), which provides a digital archive of all collective bargaining agreements signed in Italy.

We can assign a year of renewals for about 121 sectoral agreements, covering approximately 93% of the person-year observations in INPS-INVIND.[16] This represents an improvement compared to previous studies that have used information on Italian collective

[14]Earnings include overtime payment, bonuses, shift work. Earnings are converted in real euros (1995 CPI) and are top-coded at 400.000€.

[15]This last category includes what Cappellari et al. (2012) define as collaborators).

[16]Unmatched cases correspond to cases where the CCNL in INPS-INVIND is not correctly spelled/reported and/or there is no clear cross-walk between INPS-INVIND codes and the CNEL archive.

bargaining agreements.[17] The corresponding subsample of INPS-INVIND with matched information on CCNLs' years of renewals will be denoted by INPS-INVIND-CNEL.

1.4.3 Summary Statistics

Table 1 presents characteristics of workers and firms across contract types. To derive these summary statistics, we collapse the information from INPS-INVIND to a person-year panel in which workers are assigned in a given year to either a temporary or a permanent contract according to the highest paid job. Table 1 distinguishes between the universe of person year observations in INPS-INVIND (Columns 1-2) and a subset of the latter with matched information on CCNLs (Columns 3-4).

The share of workers under a temporary contract is approximately 20%. Temporary workers in a given year earn and work substantially less compared to permanent workers. Defining the contract gap as the difference in outcomes between temporary and permanent workers, we see that the contract gap in log daily wage is approximately 33%.[18] Temporary workers are younger, more likely to be female, and more likely to enter the labor market at an earlier age, which is consistent with the idea that these workers are less likely to hold a college degree.[19]

Temporary workers are more likely to be employed in part-time positions. About 20% of temporary jobs are obtained via a temporary work agency and another 18% represent apprenticeships. This implies that the most common type of temporary contract is fixed term work arrangements signed directly by the firm (see Figure A.1.2).

The average tenure of a temporary worker is shorter than that of a permanent worker (0.84 vs. 5.43 years). Temporary workers are also more likely to transition between jobs in a given calendar year: 51% of temporary workers hold only one job in a given year (compared to 83% for permanent workers).[20]

[17] For example, Card et al. (2014) have information on 11 CCNLs; Cappellari et al. (2012) on 8 CCNLs; Adamopoulou and Zizza (2015) on 22 CCNLs.

[18] Boeri (2011) reports an hourly wage contract gap of approximately 24% for Italian male workers , after controlling for observable workers' characteristics and using European survey data. This gap is 29% in France, 27% in Germany and is the largest in Sweden (45%) and lowest in the UK (6.5%). See also Kahn (2016) and Dias da Silva and Turrini (2015).

[19] According to official figures from the Italian Ministry of Education, the average age at college graduation in 2005 is 28 years old. See also Garibaldi et al. (2012) and Malacrino and Saggio (2017).

[20] We also checked whether temporary workers simultaneously hold multiple job positions. This is reported in Appendix Table B.2. We find that the average number of jobs in a given month held by a worker employed with at least one temporary contract is approximately 1.02 while the same number is 1.01 for permanent workers. These numbers slightly increase overtime and are smaller for full time workers.

Regarding workplaces: temporary workers appear to be employed in smaller firms that tend to have lower value added per worker. Finally, we find that all reported statistics almost do not change when focusing on a subsample with information on the year of renewal of the associated CCNLs (See columns 3 and 4 of Table 1).

1.5 Research Design

Credible estimates of new regulations on temporary contracts hinge on the identification of a group of firms/jobs/workers similar to those affected by the new law in both observable and unobservable characteristics.

The variation associated with law 368/2001—henceforth simply labeled as the "reform"—provides a possible solution to this identification problem, since it was implemented in a staggered fashion across CCNLs. This generates sectoral and temporal variation that we will exploit in a difference-in-differences research design.

1.5.1 Implementation of the Reform

The reform establishes that new rules on the usage of temporary contracts should be implemented in a given sector only following the renewal of the associated CCNL which are typically renegotiated every two years (Biagi, 2002; Lozito, 2013).[21] We extend the research design of Cappellari et al. (2012), who rely on aggregate firm data on 8 CCNLs, by combining micro-information from social security records on collective bargaining agreements with information from CNEL on the time series of renewals of these agreements. Overall, we are able to infer the reform status associated with 121 CCNLs corresponding to approximately 50 million person-year observations in INPS-INVIND, as shown in Columns 3 and 4 of Table 1.

To assign the year of renewal to a given CCNL, we use the date of the first agreement signed following the passage of law 368/2001.[22] Most renewals around the time of the reform were regular in the sense that they were signed 2–3 years after the previous agreement.[23] The

[21]This occurred since the national law did not legislate on the maximum share of temporary contracts that firms could sign, leaving legislation on this subject to CCNLs. We find these maximum thresholds did not change significantly following the reform. See also Cappellari et al. (2012).

[22]For workers employed via temporary work agencies and a few other sectors, we let their year of renewal coincide with the year of passage of the reform, 2001. Our results are not affected by this particular choice.

[23]The person-year weighted share of contracts that were signed after the reform within three years from the previous agreement is 94%.

most important CCNL is "Metal Manufacturing" which covers around 23% of the person-year observations in INPS-INVIND-CNEL, followed by "Service", which covers around 16%. Movement of workers between jobs with different CCNLs and differente renewal years appears to be a relatively rare event: the share of such movers is around 6% and is relatively constant in the years in which the reform was implemented (see Table B.3). The overall distribution of renewal years is plotted in Figure 1.2, Panel (a). Around 90% of the person-year observations are under CCNLs that were renewed by 2003.

1.5.2 Event Study

Our research design hinges on the staggered implementation of the reform across CCNLs. In order to better gauge the key identifying variation, we begin by estimating how the share of jobs covered by a temporary contract changes following the reform using an event study regression

$$D_{cpt} = \eta_c + \lambda_{pt} + \sum_{k=a}^{b} R_{ct}^k \theta_k + X_{cpt}^\top \beta + r_{cpt}, \tag{1.6}$$

where D_{cpt} represents the share of temporary jobs in a given CCNL c, province p and year t. Common macroeconomic shocks occurring in province p and year t are captured by λ_{pt}. Time-varying characteristics such as fraction of females, foreign workers and average potential experience and age in a given cell are collected in X_{cpt}.[24] Standard errors are clustered at the province by CCNL level. In all our event-study specifications, we normalize $\theta_{-1} = 0$ and set $a = -3$ and $b = 3$.

Letting t_c^* denote the year of renewal of CCNL c following the passage of the 2001 reform, we have that $R_{ct}^k \equiv \mathbf{1}\{t = t_c^* + k\}$. Therefore, R_{ct}^k is an event-study indicator to denote whether the associated CCNL falls under the new regulations. The coefficients θ_k for $k \geq 0$ capture how the share of jobs covered by a temporary contract changes k years after implementation of the reform relative to the last year prior to its implementation. Identification of these coefficients hinges on the assumption that CCNLs yet to renew their collective bargaining agreement form a useful counterfactual for CCNLs under the new regime, after accounting for fixed differences between CCNLs, unobserved province specific common shocks, and time-varying observed compositional shifts as measured by the vector X_{cpt}.

[24] We define potential experience as years following the first appearance of the worker into the INPS's records.

This parallel-trend assumption can be assessed by evaluating the coefficients θ_j for $j < 0$. If these coefficients are significantly different from zero, it would mean that we cannot distinguish between the true effect of the reform and the prior, unobserved, dynamics that led to its implementation.

1.5.2.1 Results on Utilization of Temporary Contracts

Figure 1.2, Panel (b) plots the event-study coefficients $\{\theta_k\}$. These event-study coefficients are relatively flat and close to zero in the years prior to enactment of the reform, providing suggestive evidence in favor of the common-trend assumption. At enactment, there is a significant increase of about 1 percentage point in the share of workers covered by temporary contracts. Three years after the reform the share of workers under a temporary contract increases by about 3 percentage points, an increase of about 19% when benchmarked relative to the share of workers under a temporary contract in the pre-reform years (1998—2001). Figure B.1.1, Panel (a) shows that the bulk of the increase in the share of temporary contracts is concentrated in temporary work arrangements signed directly by the firm, whose share three or more years into the reform increased by approximately 33%.

Notice that the pre-2001 share of new jobs created is relatively high, as shown at the bottom of Figure 1.2. This is likely due to (i) the implementation of the Treu package in 1997 which introduced apprenticeships programs and legalized temporary work agencies (see Figure A.1.2), and (ii) the circumventions of pre-reform constraints on the possibility to hire under temporary contracts.

Figure B.1.1 assesses the robustness of Figure 1.2, Panel (b). We find that our results are robust to alterations to the set of controls, CCNLs used for estimation, and exclusion of particular groups of workers when constructing aggregates in a given cell (e.g. workers that moved between CCNLs that implemented the reform in different years).

1.6 Results

The model outlined in Section 1.3 predicts that, following the policy change, we should expect an increase in temporary job creation and destruction as well as a decrease in temporary job duration. In this section, we use the INPS micro-level information to confirm the predictions obtained from the model. We also document a widening of the contract gap in both earnings and days worked following the policy change.

1.6.1 Empirical Framework: Job Destruction and Creation

Our model highlights how the reform can have large effects on the separation rate of temporary jobs. We quantify this effect by leveraging from matched employer-employee data to determine whether a temporary job observed in a given year under the new policy regime is systematically more likely to be destroyed in the following year. We focus on all temporary jobs that correspond to the highest-paid occupation of a given worker in a given year and estimate the following event-study specification using this panel of temporary job-year observations:

$$Separated_{jt+1} = \sum_{k=a}^{b} R_{jt}^{k} \pi_k + X_{jt}^{\top} \beta + r_{jt}, \tag{1.7}$$

where $Separated_{jt+1}$ is a dummy equal to 1 if the temporary job observed in year t is destroyed in the following year. The event study indicators R_{jt}^{k} are dummy variables that determine whether the temporary job in year t is created under the new rules established by the reform. The vector $X_{jt}^{\top}$ controls for CCNL fixed effects, time effects that are province specific and characteristics of the worker such as gender, nationality, birth year fixed effects and a quadratic term in potential experience. The coefficients $\{\pi_k\}$ for $k \geq 0$ capture the effect of the reform in the year-to-year separation rate of temporary jobs, using temporary positions yet to be covered by the reform as a counterfactual.

Matched employer-employee data allow us to decompose this effect on separation across various dimensions. According to the model, increases in separation rates of temporary jobs may be driven by: (i) decreases in the likelihood of conversion of temporary jobs to permanent positions (purple area in Figure 1.1), (ii) decreases in the duration of temporary jobs, (iii) a compositional effect due to new temporary jobs of shorter expected duration being created after the reform (red area in Figure 1.1). We can measure how much of the potential increase in separation rate for temporary jobs is absorbed by the labor market, and consequently we can quantify the overall net effect of the reform on the destruction rate of temporary jobs. The effects mentioned above can be estimated succinctly by running the following Markov-type difference-in-differences model on the panel of temporary job-year observations:

$$\mathcal{S}_{jt+1}^{s} = \pi^{s} Post_{jt} + X_{jt}^{\top} \beta + r_{jt}, \tag{1.8}$$

where $Post_{jt}$ is a dummy equal to one if the CCNL associated with the temporary job j in year t is under the new policy regime and $\mathcal{S}_{jt+1}^{s}$ is an indicator associated with a potential state s in year $t+1$. These states are represented by the combination of contract status

(temporary, permanent) and whether the worker is employed under the same employer of year t (same, different), i.e. whether the match is destroyed in the following year. For instance when $s =$(permanent, same employer), $\mathcal{S}^s_{jt}$ represents an indicator equal to one if the temporary match observed in year t is transformed to a permanent contract in the following year. The final state corresponds to non-employment $s = (NE)$, i.e., when the temporary position in year t is destroyed and the associated employee does not result employed in the INPS archive in the following year.[25] Therefore, when $s = NE$, π^{NE} captures changes induced by the reform in the net job destruction rate of temporary jobs. To complement the analysis, we also estimate the Markov difference-in-differences model of equation (1.8) focusing this time on the panel of permanent job-year observations.

To measure the effects of the reform on job creation, the other key margin affected by the reform, we proceed in a similar way as when measuring impacts on job destruction. We begin by estimating the following event study on the panel of job-year observations in our data:

$$NewMatch^{TEMP}_{jt} = \sum_{k=a}^{b} R^k_{jt}\alpha_k + X^\top_{jt}\beta + r_{jt}, \tag{1.9}$$

where $NewMatch^{TEMP}_{jt}$ is an indicator equal to one if the job j observed in year t represents a new match created with a temporary contract. Therefore, the coefficients $\{\alpha_k\}$ in (1.9) measure the impact of the reform on the probability of observing new jobs created under a temporary contract.

Next, we focus on whether the reform is associated with significant changes in the exit rate from non-employment which is a more informative metric to quantify the *net* effect of the reform on job creation. Specifically, we estimate the following difference-in-differences model focusing again on the panel of job-year observations:

$$E^s_{jt} = \alpha^s Post_{jt} + X^\top_{jt}\beta + r_{jt}, \tag{1.10}$$

where E^s_{jt} is a dummy equal to 1 if job j observed in year t is created under contract $s \in \{Temp, Perm\}$ and such job is filled by an employee who was non-employed in year $t-1$. Hence, in equation (1.10), α^{Temp} captures changes induced by the reform in the probability to exit non-employment by obtaining a temporary job position.

[25] We assign the status non-employed in year t if the corresponding worker (i) is employed at least once in any years prior to t, (ii) is not employed according to social security records in year t (iii) is employed at least one year following t.

1.6.2 Results: Job Destruction, Job Creation

Figure 1.3, Panel (a) shows the estimates from equation (1.7). In the first year of implementation of the reform, we observe an increase in the probability of separation of around 2 percentage points. This effect does not appear to be the result of existing pre-trends which appear to be moderate in size and statistically insignificant overall.

The first row of Table 5, which reports the estimates from (1.8), decomposes the aggregate effect on separation shown in Figure 1.3. The increase in separation occurs primarily (72%) by way of a reduction in the probability of remaining employed under a temporary contract with the same employer. There is also a significant reduction in the probability of a temporary contract being converted to a permanent one by the original employer.

Hiring rates across different employers, however, absorb only 55% of the overall increase in separation rates. Consequently, we find a statistically significant impact of around 1 percentage point on the probability of transitioning from a temporary job to a full year of non-employment. This captures the positive effect of the reform on the net job destruction rate of temporary jobs as predicted by our theoretical model. We also estimate the same specification for permanent jobs (see row 2 of Table 5). Here, the reform does not appear to have modified separation rates significantly, neither in economical nor statistical terms.

Next, we turn to job creation. Figure 1.3, Panel (b) shows the impact of the reform on the probability of observing a new match under a temporary contract. Event coefficients in the years prior to the reform exhibit a flat profile. At enactment, the probability of observing a new job being created under a temporary contract jumps by around 1 percentage point and continues to grow in the post enactment years.

Exit rates from non-employment, however, are more informative in order to understand the net impact of the reform for the process of job creation. Estimates from equation (1.10) are reported in the last row of Table 5. The reform appears to increase the probability of observing a new job created under a temporary contract and filled by a previously non-employed worker by 0.6 percentage points. Interestingly, however, there appears to be evidence of substitution. The permanent job filling rate for workers previously non-employed decreases in the post-reform by approximately 0.3 percentage points, consistent with the substitution effect highlighted in Figure 1, Panel (b).

Finally, Table C.2 in the Appendix shows estimates equivalent to those shown in Table 5 but restricts the sample to matches in which the employee is 30 or less years old. Transition rates for permanent jobs held by young workers do not seem to experience any significant

changes post-reform. Temporary jobs held by young workers, on the other hand, are particularly less likely to be converted to a permanent one by the same employer following the policy change. There is no significant effect on the probability of being hired as a permanent worker by different employers and the net job destruction rate of these jobs is positive and significant. Job creation also significantly increases for this particular group, as the exit rate from non-employment into a temporary job grows after the reform and substitution effects in exit rates appear to be less pronounced compared to Table 5.

Overall, the reform tends to increases in both job creation and job destruction, as originally predicted by our model. Matched employer-employee data allow us to empirically decompose the sources driving these two main effects. Two key findings emerge: (i) about half of the increase in the separation rates in temporary jobs is not absorbed by the labor market in the following year and (ii) most of new temporary jobs created after the reform are associated with job-to-job transitions.

1.6.3 Temporary Job Duration and the Contract Gap

We next quantify the effect of the reform on the duration of temporary jobs. According to the model, the duration of temporary jobs is expected to decrease both because of a direct effect, since the reform decreases the incentives to sign temporary jobs of longer duration (see Proposition 3, part 2) and because of a composition effect as jobs of shorter expected duration are now getting created in the post reform under temporary contracts (see the green area in Figure 1, Panel (b)). To quantify the overall effect on the duration of temporary jobs, we estimate an event-study specification akin to (1.7) on the panel of temporary job-year observations where the outcome variable is the total number of days worked in year t by the employee hired under the corresponding temporary contract.

Figure 1.4, Panel (a) shows a significant decrease of around 5 days (around 2.5% of the pre-reform average) in the number of days worked in the first year of implementation of the reform that continues to grow and becomes around 10 days in the post-enactment years (a decrease of around 5%). Figure B.1.5 in the Appendix extends the findings reported in Panel (a) and shows the impact of the reform on the overall distribution of days worked under a temporary job.[26] The share of temporary jobs associated with significant shorter

[26]More specifically, we run difference in difference models based on equation (1.7) but where now the dependent variable is an indicator of number of days worked below a given threshold, as indicated in the x-axis of Figure B.1.5. We have also estimated similar regressions using the unconditional quantile methods of Firpo et al. (2009) and obtained similar qualitative results.

number of days worked is increasing sizeably post-reform. Figure 1.4, Panel (b) shows the corresponding impact of the reform on the annual earnings associated with temporary jobs. Using the pre-reform ratio between annual earnings and annual days worked reported at the bottom of Panel (a) and Panel (b), we see that almost the entire loss in earnings for temporary jobs post-reform can be explained by the corresponding reduction in days worked.

We next analyze how the reform affected the duality in labor market outcomes between permanent and temporary jobs, by estimating models of the following form on all job-year observation:

$$Y_{jt} = \theta_P P_{jt} + \sum_{k=a}^{b} R_{jt}^k \theta_k + \sum_{k=a}^{b} (R_{jt}^k \times P_{jt}) \theta_k^P + X_{jt}^\top \beta + r_{jt}, \tag{1.11}$$

where Y_{jt} in (1.11) measures either total earnings or days worked by the employee associated to job j in year t. The dummy P_{jt} is an indicator equal to 1 if the job is under a permanent contract. Thus, the event study coefficients $\{\theta_k^P\}$ capture how the reform affects the average gap between permanent and temporary jobs in the outcome Y_{jt} - see Figure 1.5.

The reform is associated with a widening of the contract gap. In terms of earnings, the gap grows by around 300 euros ($\approx$ 3% of the measured raw gap in the pre-reform) in the first year of implementation of the reform and grows to around 1000 euros ($\approx$ 10% of the pre-reform gap) three or more years following enactment. The widening of the contract gap in terms of annual earnings appears to be driven by changes in annual days worked: post-reform this gap grows by 8% at enactment and up to 13% in the post-reform years. Figure 1.4 highlights how the bulk of the widening of the contract gap appears to be driven by losses in both earnings and days worked, although the widening of the gap in later post-enactment years also appears to be driven by corresponding increases for permanent workers. This expansion of the contract gap implies a redistribution from younger to older workers, consistent with the within-firm evidence shown in Table 4. This is quantified in Figure B.1.6, which shows increases in the gap between old. vs. younger age groups in earnings following implementation of the reform (Rosolia and Torrini, 2007).

The widening of the contract gap does not appear to be explained by changes in the composition of workers with temporary jobs after the reform. Table C.1, Panel (b) in the Appendix shows that the reform remains associated with a significant widening of the contract gap after excluding new entrants and focusing only on continuously employed workers. Table C.1 also shows that the widening of the gap is not driven by part-time workers. In terms of potential labor market experience, increases in the contract gap are particularly pronounced for workers in their first job and remain economically significant for workers

with at least 5 years of potential experience. Table C.1 provides also evidence that the reform is associated with overall increases in the contract gap in terms of daily wages.

The results presented in this section complement the analysis in Table 5 and confirm the theoretical predictions of the model as post-reform temporary jobs tend to have shorter duration. We show that the widening in employment protection duality maps into an enlargement of the earnings gap between temporary and permanent jobs that appears to be primarily driven by decreases in the duration of temporary jobs.

1.7 Conclusions

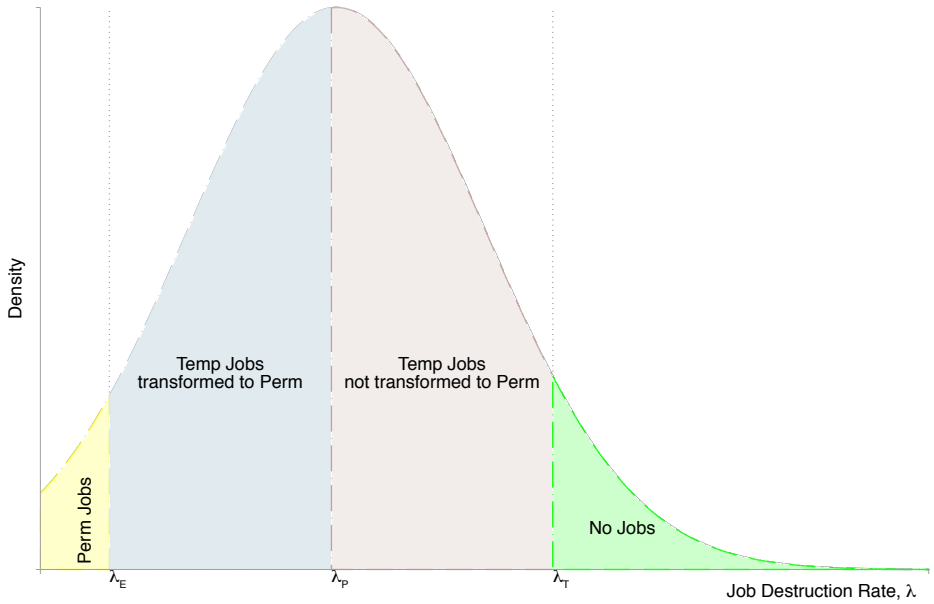
In this Chapter, we have analyzed an Italian reform that relaxed several legal constraints on hiring temporary contract workers without affecting the employment protection granted to workers hired under permanent employment contracts.

Consistent with the predictions of a search and matching model with two types of employment contracts and endogenous duration of temporary contracts, we find that this reform increased job creation. Job separation rates, however, increased in lockstep. After the reform, the duration of temporary contracts was significantly reduced in this mapped into a widening of the contract gap in both days worked and earnings between permanent contract workers and temporary ones. This increased the duality of the Italian labor market.

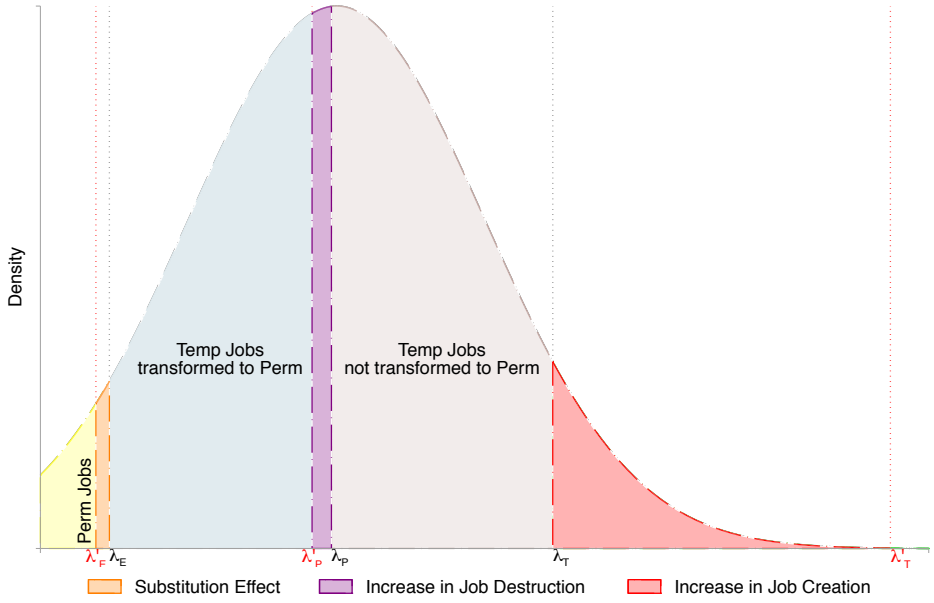
The next chapter is devoted to understand the consequences of this reform for both workers and firms.

1.8 Figures

Figure 1.1: Choice between Contracts and Effect of the Reform



(a) Choice Between Temporary and Permanent

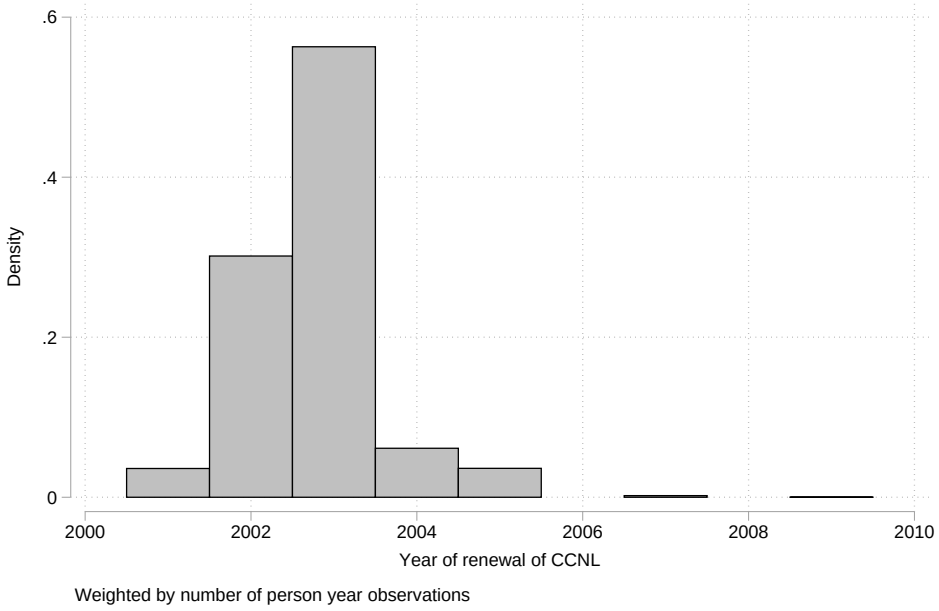


(b) Effect of the Reform

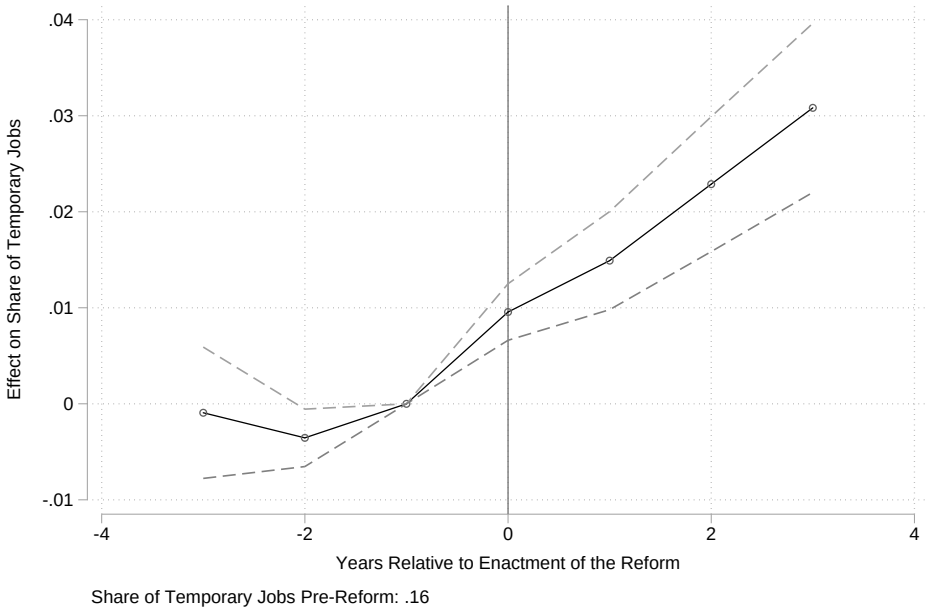
Note: Panel (a) shows the optimal choice rule between the creation of temporary and permanent contracts based on their associated surplus and an assumed distribution of production opportunities. See Proposition 1 for details and the definition of each threshold. Panel (b) plots equilibrium changes of $(\lambda_T, \lambda_E, \lambda_P)$ following the introduction of a reform that facilitates the creation of temporary jobs, see Proposition 2 and Proposition 3 for details. The new equilibrium thresholds are denoted in red in Panel (b).

Figure 1.2: The Reform

(a) Year of Renewal



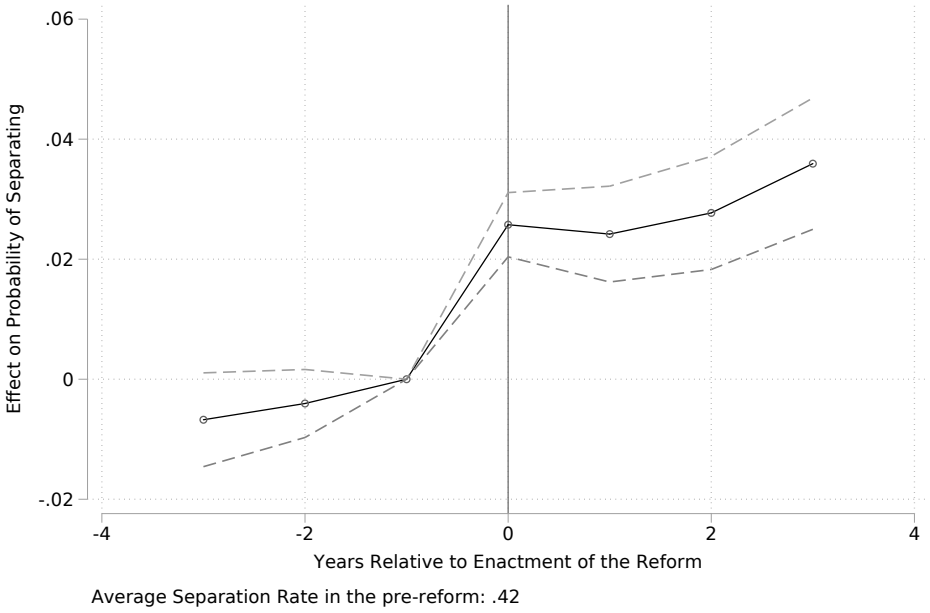
(b) Event Study



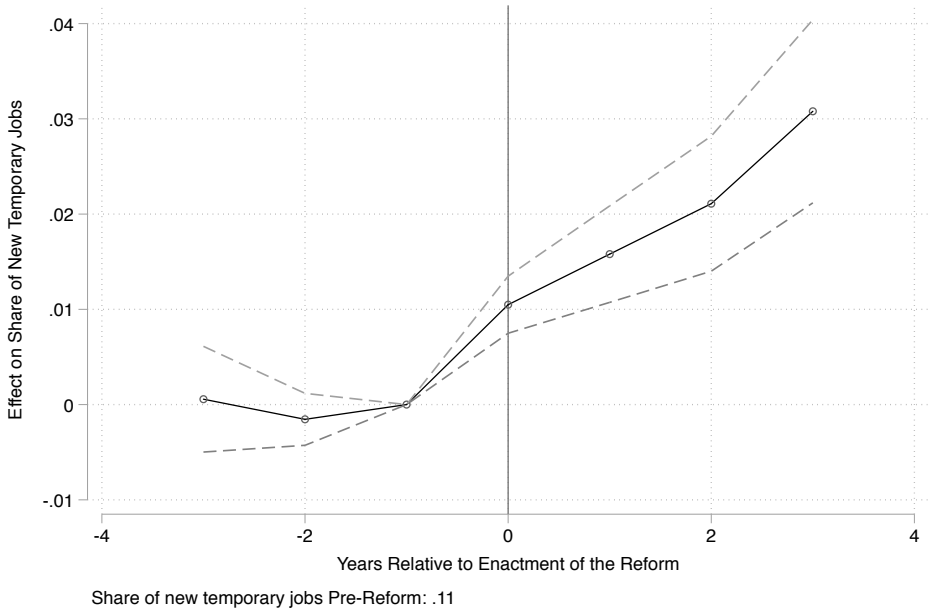
Note: Panel (a) shows the distribution of enactment years of the reform across CCNLs, see Section 1.5.1. Panel (b) shows event study estimates from (1.6). These event study estimates control for province by year and CCNL fixed effects as well as share of females, foreign, average potential experience and share of workers within 5 year interval age bins in a given CCNL x Province x Year cell. Estimates are weighted by the number of workers observed in a cell. The first year of enactment of the reform corresponds to year 0 of the graph. 95% confidence intervals obtained after clustering the standard errors at the CCNL x province level. Source: INPS-INVIND-CNEL.

Figure 1.3: Job Creation, Job Destruction

(a) Probability of Separation for Temporary Jobs



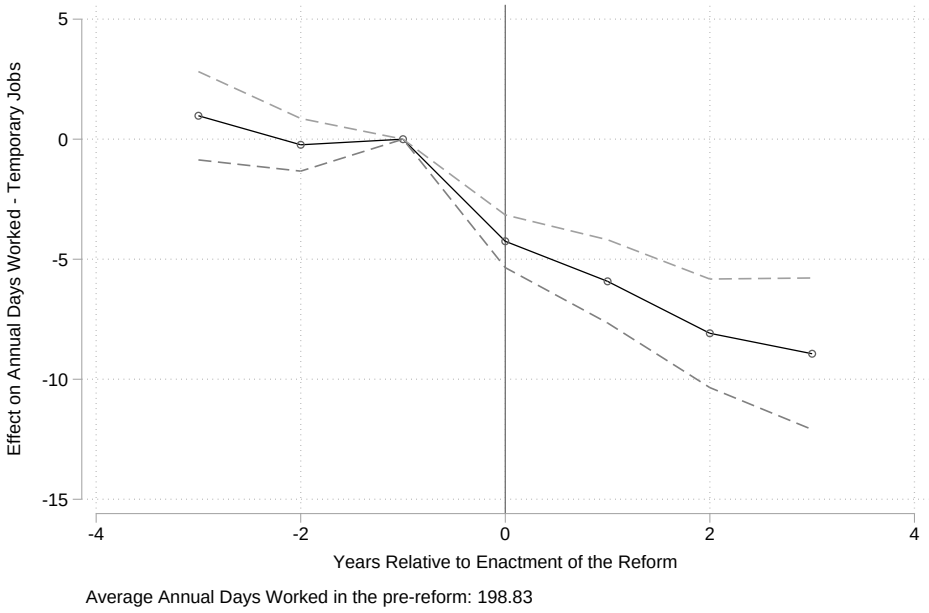
(b) New Jobs Created under a Temporary Contract



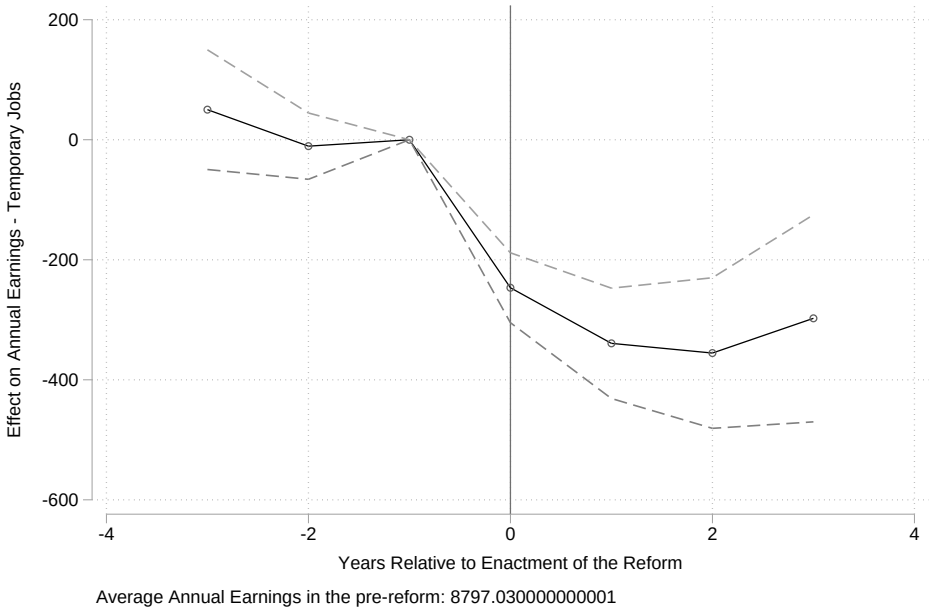
Note: Panel (a) presents estimates from the event study specification (1.7) on the panel of temporary jobs-year. The outcome variable is an indicator variable for whether the temporary job j observed in year t results destroyed in year $t+1$. Panel (b) presents estimates from equation (1.9). Here the unit of analysis corresponds to all job year observations observed in our social security data and the event study coefficients capture the impact of the reform in creating new jobs that are signed via a temporary contract. Both panels focus on jobs that represents the highest paid occupation in a given worker-year cell. See text for further details. 95% confidence intervals obtained after clustering the standard errors at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Figure 1.4: Duration and Earnings of Temporary Jobs

(a) Annual Days Worked



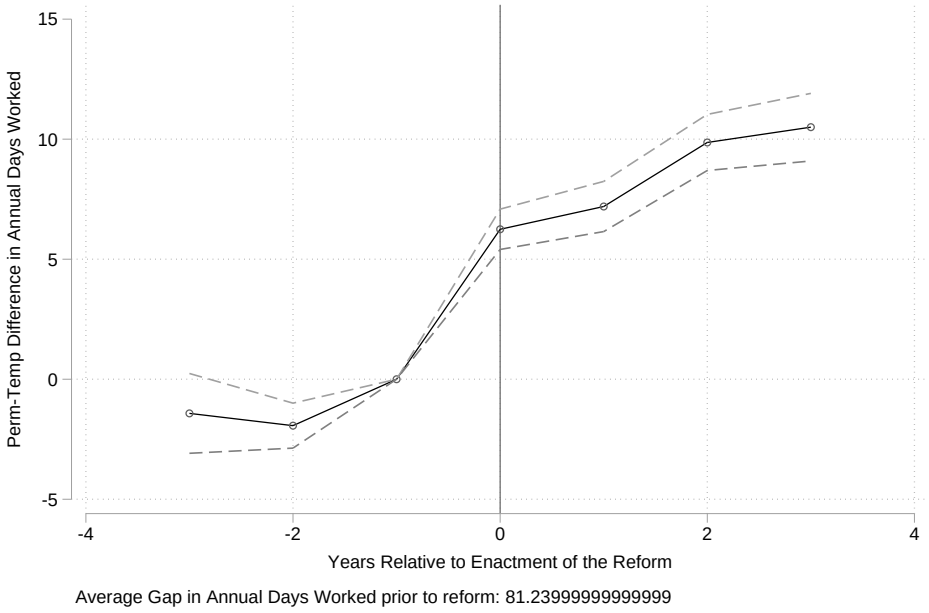
(b) Annual Earnings



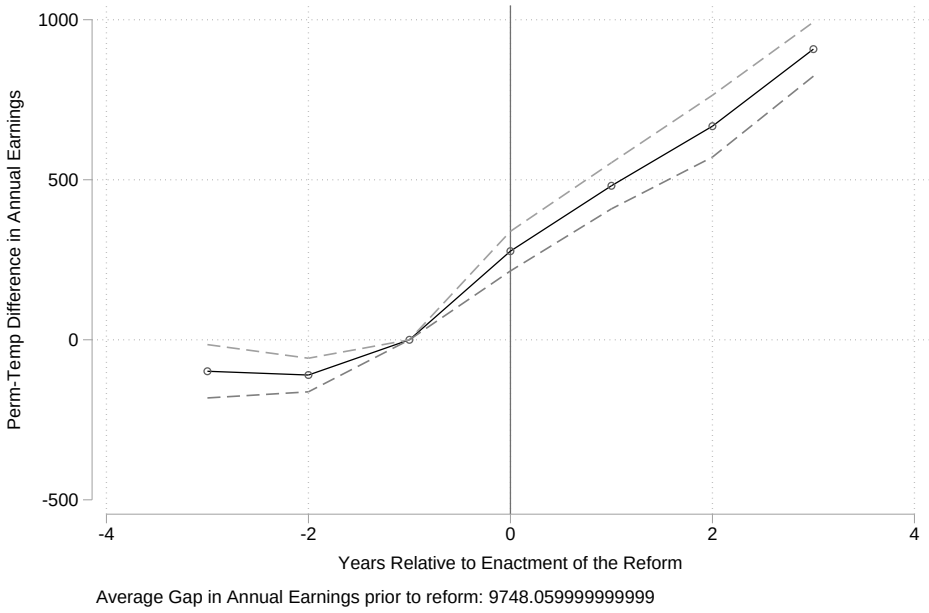
Note: Both Panels displays event study coefficients that summarize the impact of the reform on the outcome listed on top of each figure. Estimation is carried only for temporary jobs that represent the highest paid occupation for a given worker/year pair. In panel (a) the outcome variable is represented by the total number of days worked by the employee associated to the temporary contract. Similarly for earnings. The event study regression controls for workers' characteristics such as gender, nationality, birth dummies, a quadratic in potential experience as well as province by year and CCNL fixed effects. At the bottom of the figure, we provide the average of the outcome variable in the pre-reform years (1998-2001). 95% confidence intervals obtained after clustering the standard errors at the CCNL by Province level. Source: INPS-INVIND-CNEL.

Figure 1.5: Contract Gap in Days Worked and Earnings

(a) Annual Days Worked



(b) Annual Earnings



Note: Both Panels displays event study the coefficients, denoted as $\{\theta_k^P\}$ in equation (1.11), on the outcome listed on top of each figure. Data is represented by all jobs-year observed in the INPS-INVIND data and we focus only on jobs that correspond to the highest paid occupation within a given worker/year cell. At the bottom of the figure, we provide the raw gap in a particular outcome in the pre-reform years (1998-2001). 95% confidence intervals obtained after clustering the standard errors at the CCNL x Province level. Source: INPS-INVIND-CNEL.

1.9 Tables

Table 1: Summary Statistics

	INPS-INVIND		INPS-INVIND-CCNL	
	Temporary Contract	Permanent Contract	Temporary Contract	Permanent Contract
	[1]	[2]	[3]	[4]
Labor Market Outcomes				
Total Earnings	8,718.43	19,770.19	8,672.85	19,498.45
	(6671.58)	(15672.73)	(6577.14)	(15776.21)
Total Days Worked	191.24	283.85	191.24	283.36
	(106.24)	(64.73)	(106.05)	(65.08)
Log Daily Wage	3.7460	4.0826	3.7427	4.0699
	(0.3794)	(0.5182)	(0.3748)	(0.5171)
Age				
Age	30.70	39.19	30.73	39.08
Less than 30 yrs old	0.5619	0.1633	0.5594	0.1656
More than 50 yrs old	0.0406	0.1392	0.0406	0.1358
Age at Entry	23.78	24.93	23.79	24.92
Tenure	0.84	5.43	0.84	5.49
Worker and Workplace Characteristics				
Female	0.4234	0.3219	0.4222	0.3257
Full Time	0.7895	0.8716	0.7872	0.8683
Employed via Temporary Work Agency	0.2056	0.0026	0.2082	0.0028
Apprenticeship	0.1820	0.0000	0.1808	0.0000
One job in the year	0.5112	0.8279	0.5089	0.8272
Firm Size	29.99	35.92	29.47	35.89
Value Added per Worker (1000's real euros)	29.16	39.57	29.10	38.72
# of Person Year Obs.	53,616,784		50,064,176	
# of Persons	2,982,314	4,610,182	2,892,498	4,439,085

Note: This table provides summary statistics of the INPS-INVIND data described in Section 4.1 collapsed at the person year level. A worker is labelled as either temporary or permanent according to the job that paid the most in a given year. Column 3, 4 focus on workers for which we can match the CCNL of the worker with the CNEL archive, see Section 4.2. All labor market outcomes are measured conditionally on being employed in a given year. Total earnings refer to the sum of labor earnings (across all jobs) in a given year. Similarly for total days worked. Log daily wage refers to the real log daily wage on the job that paid the most in a given year. Earnings, log daily wages and value added are deflated using the 1995 CPI. Value Added per worker is calculated in thousands of real euros and reported only for workers employed in a firm that is matched with CERVED, see Table B1 for details. # of Persons refers to the number of individuals that at any point in their career had a temporary/permanent contract. All statistics are weighted by the number of person year observations. Standard deviation in parenthesis.

Table 5: Effect of the Reform on Job Flows

	Same Employer		New Employer		
	Temporary [1]	Permanent [2]	Non - Employed [3]	Temporary [4]	Permanent [5]
[1] **Transition Rates: Temporary Jobs**					
Post-Reform	-0.0169***	-0.0065***	0.0103***	0.0090***	0.0041***
	(0.0026)	(0.0022)	(0.0017)	(0.0019)	(0.0014)
# of Job-Year Observations	9,839,862	9,839,862	9,839,862	9,839,862	9,839,862
Pre-Reform Mean	.356	.22	.124	.175	.136
[2] **Transition Rates: Permanent Jobs**					
Post-Reform	0.0001	-0.0077	-0.0032	0.0007*	0.0100**
	(0.0001)	(0.0048)	(0.0022)	(0.0004)	(0.0047)
# of Job-Year Observations	40,224,313	40,224,313	40,224,313	40,224,313	40,224,313
Pre-Reform Mean	.004	.8	.042	.028	.125
[3] **Exit Rates from Non-Employment**					
Post-Reform				0.0066***	-0.0036***
				(0.0006)	(0.0012)
# of Job-Year Observations				50,064,175	50,064,175
Pre-Reform Mean				.041	.055

Note: Row 1 of this table presents estimates of the difference in differences coefficient π from equation (9) on an indicator corresponding to the state listed in each column of the Table. For instance, when looking at the column 1, row 1, the post reform coefficient captures the changes induced by the reform in observing a temporary job in year t being converted into a permanent job in year t+1 by the same employer of year t. Similarly for estimates in row 2 which capture effect of the reform on the transition rates associated to permanent jobs. Estimates in row 3 are based from equation (11). Here the post reform coefficient show the impact of the reform in creating new jobs under either a temporary (Column 4) or a permanent contract (Column 5) and the associated job is filled by a worker that was non-employed in the previous year, see text for details. Pre-reform mean reports the share associated to a given transition in the pre-reform era (1998-2001) for Row 1 and Row 2 while for Row 3 it reports the average exit rates from non-employment via either a temporary or a permanent contract in the pre-reform era. All the reported estimates focus on jobs that represent the highest paid occupation in a given worker-year cell. Estimates of the reform computed after controlling for province by year and CCNL fixed effects as well as worker level controls such as gender, nationality of birth (Italian vs. non-Italian), year of birth dummies and a quadratic in potential experience. Standard errors are clustered at the CCNL x Province level. Source:

Chapter 2

The 2001 Italian reform and its impact on firms, workers

2.1 Introduction

In this chapter, we document the impact of the partial reform to employment protection laws described in the previous chapter on firms and workers exploiting the staggered implementation of the policy across different collective bargaining agreements - see Section 1.5. In particular, we start by showing the effect of the reform on firms' balance sheet - value added, labor costs, profits. We investigate how these effects change across different groups of firms defined in terms of different age, wage and product market characteristics. The firm level analysis concludes by analyzing how the reform impacted patterns of within-firm inequality in pay. Next, we move into workers. We start by showing the impact of the policy on levels of employment, earnings and days worked. We then focus on two key subpopulations targeted by the reform: young workers entering the labor market and workers that at the time of the signing of the national reform were already employed under a temporary contract.

We find that firm profit margins, defined as profits divided by value added, increased following the policy change by approximately 5% with a cumulative effect of around €5.2bil. This increase in profits stemmed primarily from a decrease in labor costs per worker rather than increases in value added. Indeed, we estimate that the average gap in annual labor costs between temporary and permanent workers to be approximately €16,500. Average firm size did not change significantly after the reform, suggesting that firms primarily substituted permanent positions with temporary ones, although there is some indication that this change led to a decrease in value added. This highlights a tradeoff between lower labor costs and lower productivity in firms' decisions to utilize temporary contracts (Weil, 2014).

The decrease in labor costs and increased utilization of temporary contracts is also associated with a rise in within-firm earnings inequality. We find that the reform raised the within-firm standard deviation of earnings by approximately 4%. Moreover, the within-firm pay gap between young and older workers also increased by 4.5%. Interestingly, the overall wage bill paid to managers and their average compensation both increase after the reform. This suggests that the increase in profits following the policy change may have been partially redistributed to managers.

Regarding the impact of the reform on workers: We find that the reform has close to null effect on the probability to be employed, which is consistent with our theoretical framework and the previously described effects of the policy on the dynamics of job destruction and creation. On average, however, workers earned less after the reform, with a substantial rise in the pay gap between incumbent permanent workers, who had higher earnings following the reform, and incumbent temporary workers, who suffered an average earnings loss of up to 5% following the policy change. These losses are primarily driven to decreases in the probability that incumbent temporary workers were converted to permanent contracts by their employers after the reform.

Longitudinal data on workers permit us to isolate the dynamic effects of the reform on new entrants. Young individuals who entered the labor market after the reform earned between 3.5% to 7% less in the first year of entry compared to those who had entered in the pre-reform regime. These estimates persisted up to the 7th year following entry in the labor market and mapped into cumulative present discounted value losses ranging from €1,000 to €4,000 depending on the cohort analyzed. We show that these negative estimates were not due to compositional changes or selective entry of workers based on the reform status. Instead, post-reform entering cohorts were disproportionately more likely to be "trapped" during their career in temporary jobs where firms had fewer incentives to provide on-the-job training (Cabrales et al., 2014).

Our analysis of firms provides new evidence on the role of employment protection in the performance of firms (Autor et al., 2007; Dolado et al., 2012; Cappellari et al., 2012; Cingano et al., 2016), a relationship characterized by mixed empirical results and that has received a considerable amount of theoretical attention (Ljungqvist, 2002; Ichino and Riphahn, 2005; Lagos, 2006; Belot et al., 2007; Boeri and Garibaldi, 2007). We also establish a previously unexplored link between institutional reforms aimed at facilitating the creation of temporary work arrangements and increases in within-firm inequality (DiNardo et al., 1996; Card et al., 2013; Song et al., 2016).

Our results on workers contribute to the literature that examines the impact of partial labor market reforms on individual outcomes (see Boeri, 2011, for a review). We highlight in particular the distributional impacts of these reforms across individuals. We also provide new insights into the question of whether temporary contracts represent stepping stones into the labor market or a trap that hinders the development of the career of young workers, a question characterized by mixed empirical evidence (Booth et al., 2002; Blanchard and Landier, 2002; Ichino et al., 2008; Autor and Houseman, 2010). Our examination of the consequences of entering the labor market under the new policy regime connects to studies that analyze how entry conditions affect short- and long-term earnings (Kahn, 2010a; Oreopoulos et al., 2012), and is related to recent work of García-Pérez et al. (2016) who study a similar reform as the one analyzed here mainly targeted at Spanish high-school dropouts.

2.2 Data

The data used in this chapter follows closely the one described in Section 1.4. In particular, when analyzing the effect of the reform on workers we focus on individuals for which we can consistently measure their CCNL throughout their employment history. The data used for the firm level analysis is described next.

2.2.1 Firm Data

We augment the baseline matched employer-employee data with two additional sources. First, we have administrative information from INPS on all Italian employers that have at least one employee for the periods 1990–2013.[1] These data include information on national tax codes, number of workers employed in a given month, geographical location, and additional information on earnings by different work status.

We use tax codes to match information on income statements as collected by CERVED for the period 1998–2008. CERVED provides financial accounts for the universe of Italian limited liability corporations.[2] The CERVED dataset is derived from standardized reports that firms

[1]These data are originally collected at Employer Identification Number (EIN)-year level. We collapsed this information at the firm-year level, letting the national tax code (*codice fiscale*) reported by INPS to coincide with our definition of a firm. Approximately 7% of Firms have multiple EINs. INPS also records, within each parent firm, a"main EIN" identifier that essentially identifies the headquarters of the parent-firm.

[2]Hence, CERVED does not include private partnerships and sole proprietorships, typically used by smaller firms.

are required to file annually with the Chamber of Commerce and provides information on sales, value added, labor costs, profits, and return on equity (ROE).

Overall, for approximately 40 percent of the firm-year observations in INPS-INVIND-CNEL, we are able to match balance-sheet information from CERVED (see Table B.1 in the Appendix). As usual when working with matched employer-employee datasets and balance-sheet information (e.g., Card et al., 2014), the matching rate improves substantially for larger firms.

2.3 Research Design: Firms

We investigate the impact of the reform on firm-level outcomes by estimating the following difference-in-differences model:

$$Y_{ft} = \psi_f + \lambda_{Prov(f),t} + \theta Post_{ft} + \nu_{ft}. \tag{2.1}$$

Equation (2.1) is estimated on the sample of matched firms between INPS-INVIND and CERVED. In the above equation, Y_{ft} denotes an outcome of interest for firm f in period t; ψ_f represent firm fixed effects; $\lambda_{Prov(f),t}$ are time fixed effects that are province- specific, where $Prov(f)$ denotes the province of firm f. The variable $Post_{ft}$ is a dummy equal to 1 if firm f in period t is covered by the new rules concerning temporary contracts. Since reform status is assigned according to the year of renewal of the CCNL and firms may employ multiple workers with different CCNLs (and therefore with potentially different renewal years),[3] we assign the reform dummy by looking at the earliest year of renewal of the CCNL for a given firm. We also report estimates from (2.1) using only firms associated with a unique event year, which corresponds to roughly 83% of the overall matched sample. Table B.4 provides summary statistics on a variety of outcomes measured at the firm level between firms with unique event year and the overall sample of matched firms.

The coefficient of interest in (2.1) is θ. This difference-in-differences coefficient is estimated leveraging within-firm changes in the outcome across pre- and post-reform periods and by using firms (in the same province and year of treated firms) that are yet to be exposed to the reform in order to construct counterfactual outcomes.

To set the stage for the main firm-level analysis, Figure B.1.2 presents event-study estimates akin to the ones shown in Figure 1.2, focusing only on the sample of firms with

[3]The typical example corresponds to firms that have in their workforce some managers, as this type of occupation is typically associated with a unique collective bargaining agreement.

balance sheet information contained in CERVED and controlling for firm fixed effects. Despite changes to both the sample and specification used, the pattern remains robust: the share of workers in a firm who are hired under a temporary contractual arrangement exhibits a relatively flat profile in the pre-reform years, jumps in the year of implementation of the reform and continues to rise in the post-reform years.[4]

2.4 Results: Firms

Table 2 reports the coefficient θ from model (2.1) on different firm-level outcomes listed in each column. Table 2 has two panels. In Panel (a) estimates are computed on the sample of firms associated with a unique event year. Panel (b) reports estimates on the overall sample of matched firms between INPS-INVIND and CERVED.

Starting with Panel (a), Table 2 shows that the reform does not have a significant effect on sales and size. The effect on value added per worker is negative but insignificant, which suggests hold up concerns to be not particularly salient (Grout, 1984; Card et al., 2014). The policy change appears to have a negative and statistically significant impact on labor costs per worker which decrease by around 260 euros (about 1% of the pre-reform corresponding average). These effects combine into a positive effect for profits per worker. Profit margin, defined as profits divided by value added, increases by about 5% post-reform.

To better interpret magnitudes, one approach is to scale the coefficient reported on labor costs per worker with the impact of the reform on the share of temporary contracts hired by a firm, as shown in Figure B.1.2. As shown in Appendix D.1, this instrumental variable coefficient identifies the *contract gap* in labor costs between permanent and temporary workers in a subset of compliers firms that comply with the reform by increasing their share of workers under a temporary contract. We estimate this contract gap to be around 16,500 euros. This magnitude appears reasonable: from Table 1 we see that the earnings gap is around 11,000 euros (2/3 of the gap in labor costs) and temporary contracts typically imply lower severance payments and labor taxes for firms (Cappellari et al., 2012). Therefore, a 1 percentage point increase in the fraction of temporary workers is associated with a reduction

[4]The relatively different post-enactment magnitudes found between Figure B.1.2 and Figure 1.2, Panel (b) signal the presence of treatment effect heterogeneity in terms of firms' size. This is confirmed by Figure B.1.3 in the Appendix which plots the associated difference-in-differences coefficient from equation (2.1) on the share of temporary workers across different firms' sizes. We find that for firms with more than 100 employees the reform does not appear to have a significant effect. An explanation for this finding is that larger firms might have access to better resources and the know-how to deal with the bureaucratic burden associated with hiring temporary workers during the pre-reform era.

in labor costs per worker of around 165 euros per worker. The implied elasticity in an average pre-reform firm is 0.11. In terms of profits, a 1 percentage point increase in the fraction of temporary workers is associated with an increase in profits per worker of around 95 euros. The implied elasticity in an average pre-reform firm is 0.36.

The evidence presented thus far shows that firms appear to comply with the reform primarily by substituting permanent workers with temporary employees but without increasing the overall number of workers, consistent with the theoretical predictions of Section 1.3 and prior findings (Cappellari et al., 2012). This generates two effects: a positive effect on labor cost savings and a negative effect on value added. Taken together, these effects suggest that permanent and temporary workers are not perfect substitutes in production. Labor costs savings appear to be the predominant effect, as the evidence on post-reform profits suggests. The tradeoff between labor costs and quality of production is well described by Weil (2014) who notes that hiring decisions oriented toward temporary contracts typically highlight the tension between plant managers, who are more concentrated on product quality, and executives, who are focused instead on lowering costs.

Table 2 also shows that the reform is associated with positive effects on both firms' survival and creation. A possible interpretation here is that, thanks to the labor costs' savings induced by the reform, more firms are now able to survive and this also increases incentives for firms' creation. Table 2, Panel (b) shows that the main patterns of the estimates are confirmed when looking at the overall sample of firms. The negative effect on value added per worker is more precisely estimated in this sample and provides evidence that the policy change studied in this dissertation might have played a role in the Italian productivity slowdown (Daveri and Parisi, 2015), although the implied magnitude is not particularly large. The evolution of the significant estimated effects shown in Table 2 are plotted in Figure B.1.4.

2.4.1 Heterogeneity

We now assess the heterogeneity of the reform by interacting the reform indicator in (2.1) with a predetermined firm characteristic. We focus on the following outcomes: share of workers under a temporary contract, value added per worker, labor costs per worker and profits per worker.

Italy is a country with vast geographical differences (Malanima and Daniele, 2007). Northern Italy is traditionally more economically developed and industrialized compared

to both Central and especially Southern Italy. Table 3, Columns 1—3 report effects of the reform based on firms' location (north-center-south). In the North, we find that the reform has positive effects on the share of temporary workers and particularly large effects on profits, due to significant reductions in overall labor costs per worker. Firms in the Center experience larger increases in their share of workers under a temporary contract compared to firms in the North but the reduction in labor costs is somewhat smaller (possibly due to the higher degree of unionization in this area, which includes regions such as Emilia Romagna and Tuscany; see also La Valle, 2006). Interestingly, the reform does not appear to have an effect on the share of temporary workers in the South.[5] The South therefore implicitly provides a placebo test: in this region where the reform does not have a significant effect on the utilization of temporary workers, we should not expect to find any significant effects on labor costs and hence profits. Table 3, Column 3 confirms that this is in fact the case.

Table 3, Column 4 reports estimates on firms whose average employee's age in the pre-reform period belongs to the fourth quartile of the corresponding distribution. Such firms are therefore particularly likely to employ older workers possibly about to retire.[6] It appears that this type of firm reacts relatively more strongly to the policy change when evaluating changes to the share of temporary workers. This substitution of older workers with temporary workers implies large savings in terms of labor costs but at the same time significant decreases in value added, suggesting a far from perfect substitution between these two groups of workers.

Another group that might have a stronger incentive to respond to the reform are firms that, possibly due to fairness or historical reasons, are forced to share rents with *all* their workers. Facilitating the hiring of temporary contract workers, who are young, often not unionized, and hired for short periods, might allow some companies to reduce the amount of rents shared with these workers (Abraham and Taylor, 1996; Katz and Krueger, 2016; Goldschmidt and Schmieder, 2017). Consequently, firms paying relatively high premiums to their workforce might had a stronger incentive to hire under temporary contractual arrangements. Table 3, Column 5 provides some evidence regarding this particular hypothesis. We begin by estimating, for the years 1990—2001, firm wage premiums using the wage decomposition model of Abowed, Kramarz, and Margolis (1999) (AKM; see Appendix E.1 for details). The firm fixed effects estimated from the AKM model have been used elsewhere as a proxy for

[5] A possible explanation behind this finding is that the South is traditionally associated with a particularly developed labor black market and therefore some firms in the South might still prefer to hire "under the table" instead of using legal temporary contracts (Mantegazza et al., 2014).

[6] Median age within this group is around 50 years old. Results are similar when focusing on firms for which more than 50% of the workforce corresponds to workers age 55 or more.

the amount of firms' rents shared with all employees (Card et al., 2016; Goldschmidt and Schmieder, 2017). We divide these AKM firm effects into quartiles and consider firms with an AKM effect in the fourth quartile. In Table 3, Column 5 we see that this type of firm increase relatively more their share of workers under a temporary contract. Moreover, for this group of firm, the reform is associated with a large reduction in labor costs but also with a significant reduction in value added per worker, which ultimately maps into marginally significant effect on profits.

The last patterns of heterogeneity that we analyze are in terms of the product market structure. In Table 3, Column 6 we focus on firms that belong to a sector in which the total share of sales accrued to the 20 largest firms belongs to the first quartile of the corresponding distribution. Treatment effects for this group of firms belonging to a relatively more concentrated product market are relatively higher when it comes to the hiring of temporary workers. Moreover, for this group the reform is associated with significant reductions in labor costs per worker and these reductions ultimately map into increases in profits.

2.4.2 Within-Firm Pay Structure

Recent studies have documented the evolution of within-firm earnings inequality (Card et al., 2013; Song et al., 2016). However, these same studies are relatively silent about the sources driving the changes to the pay structure within the firm. Here we document that reforms of labor market institutions that facilitate the creation of temporary contracts are responsible for the increase in within-workplace earnings heterogeneity.

Table 4 presents estimates from model (2.1) on measures of within-firm inequality. The reform is associated with an increase in the within-firm standard deviation of earnings. Focusing on Panel (b), which comprises all the matched firms between INPS-INVIND and CERVED, we see the within-firm standard deviation in earnings increases by around 230 (a 4% increase relative to the pre-reform within-firm standard deviation in earnings). This increase in the within-firm pay structure is also seen when focusing on the ratio between the highest to lowest earner within the firm. Post-reform, this ratio grows by around 6%. Interestingly, also the within-firm gap in earnings between old and young workers increases post-reform by approximately 4.5%.

From Table 2, we know that the reform significantly reduced labor costs and increased firms' profits. This might set in motion some redistribution of resources within the same firm. Possible recipients of this redistribution include shareholders but also managers, who

represent the highest-ranked occupation in the Italian system. Some supporting evidence for this mechanism is provided in the remaining columns of Table 4. We see that the reform is associated with an increase in the total compensation paid to managers, which comprises wages, overtime pay, and bonuses. Part of this increase is driven by an extensive margin type of response as post-reform we see firms hiring more managers overall. When focusing on the average pay of managers, we see that post-reform earnings for managers increase by around 1400 euros (a change of around 3%), although this effect is statistically significant only in Panel (b). Finally, we notice that returns on equity, a measure particularly valuable for shareholders, appears to increase following implementation of the reform by approximately 1 percentage point.

Our results illustrate that the reform contributes to an increase in within-firm inequality. We quantify to what extent temporary workers might be excluded from firm-specific rents in Chapter 3.

2.5 Research Design: Workers

The first step in our analysis consists in estimating the average effect of the reform on employment rate, annual earnings, and annual days worked. We estimate the following difference-in-differences model:

$$Y_{it} = \eta_{CCNL(i,t)} + \lambda_{Prov(i,t),t} + \delta Post_{it} + X_{it}^{\top}\beta + \varepsilon_{it}, \tag{2.2}$$

where Y_{it} is a labor market outcome of interest for worker i in year t. CCNL fixed effects based on the highest paid occupation of worker i in year t are captured by $\eta_{CCNL(i,t)}$. Similarly for $\lambda_{Prov(i,t),t}$, which control for province by year fixed effects. The vector X_{it} controls for observable characteristics such as year of birth fixed effects, gender, nationality and a quadratic term in potential experience.

We estimate versions of (2.2) both conditionally on workers having positive earnings in a given year and unconditionally.[7] For workers with positive earnings in year t, the variable $Post_{it}$ is assigned on the basis of the CCNL applied to worker i in period t in her highest-paid occupation. For non-employed workers the assignment of the post-reform regime is based on the CCNL associated with the worker's last employment spell.

[7]The workers who are not employed in a year are assigned zero earnings and zero days worked (Jacobson et al., 1993).

We should stress two important caveats associated with estimation of (2.2). First, estimates on employment are going to be identified after excluding potential effects coming from new entrants. Second, endogenous sectoral relocation of workers across CCNLs might bias the interpretation of the difference-in-differences coefficient in (2.2).

Regarding the first caveat: Figure B.1.7 shows that the share of new entrants in the labor market does not change disproportionately once the reform is implemented. This represents an important finding in itself as the policy change does not appear to have significantly pushed into the labor market individuals yet to be employed; see also Section 2.7. As for the second caveat: Table B.4 shows that the fraction of workers moving between CCNLs with different years of renewal does not appear to be either large or increasing in the years when most CCNLs implemented the reform. Nevertheless, a simple way to address this second concern is to focus on incumbent workers who are employed in the year 2001, that is,

$$Y_{it} = \eta_{CCNL(i,2001)} + \lambda_{Prov(i,2001),t} + \delta^{INCU} Post_{CCNL(i,2001),t} + X_{it}^{\top}\beta + \varepsilon_{it}, \qquad (2.3)$$

where the assignment of the reform dummy is now fixed within each worker and depends on the CCNL associated with the employment spell in 2001. The same holds for the province by year and the CCNL fixed effects. Models such as (2.3) are common in labor economics as longitudinal-micro-level data are used to assess effects on workers' outcomes stemming from aggregate shocks (Autor et al., 2014) or changes in regulations (Walker, 2013).

2.6 Results: Workers, Overall Effects and Analysis on Incumbents

Table 6, Panel (a) provides difference-in-differences estimates from equation (2.2). The new policy regime has positive, but rather small, effects on employment. The essentially null effect on employment is a likely consequence of the large increases in the churning of temporary jobs, as shown in the flow analysis of Section 1.6. This finding nests the predictions of our model and is consistent with several other theoretical frameworks and empirical estimates, see Table B.5 and Figure A.1.3 in the Appendix. Average annual earnings are lower post-reform, with similar magnitudes when conditioning or not conditioning on workers with positive earnings in a given year. Both unconditional and conditional estimates also show a negative effect of the reform on days worked.

Panel (b) shows the impact of the the policy change on incumbents workers that are employed in the year when the reform was signed at the national level (2001). Interestingly,

we find a positive effect of the reform on incumbents' earnings. We find no effect on the employment rate but there are both conditional and unconditional declines in days worked. To better interpret these effects, we estimate model (2.3) by focusing only on workers whose employment spells, whenever observed, are *always* associated with a temporary contract in the pre-reform period (1998—2001). Within this group of incumbent temporary workers, the implementation of the policy change appears to trigger significant negative effects: employment drops by approximately 1 percentage point, earnings by approximately 3%, and days worked by approximately 6%—see also the event study on unconditional annual earnings shown in Figure 2.1, Panel (a).

A possible explanation for these negative effects is provided in Figure 2.1, Panel (b) which shows a large and significant decrease for incumbent temporary workers in the probability of being converted to permanent contracts in year $t+1$ by their employer of time t. These findings therefore suggest that an important fraction of temporary incumbent workers are employed in jobs that in our model corresponds to the purple area in Figure 1.1, Panel (b). Under the status quo, these workers were expected to be converted into a permanent contract. Instead, the policy change pushed a significant fraction of them into either non-employment or into temporary jobs with a new employer.[8]. In contrast, Table 6, Panel (d), shows that incumbent but permanent workers for the period 1998—2001 are actually benefiting from the reform in terms of higher annual earnings with small reductions in terms of days worked.

Table 6, Panel (a) suggests that the reform has negative effects on annual earnings, days worked and a positive but small effect on the likelihood to be employed. These aggregate results mask substantial heterogeneity: among incumbent workers, those employed with a temporary contract pre-reform appear particularly negatively affected while incumbent permanent workers actually experience an increase in earnings. These results suggest a redistribution going from temporary to permanent incumbent workers, and consequently from younger to older workers, in line with the findings of the previous two sections.[9]

2.7 Research Design: New Entrants

A key demographic group targeted by the reform is represented by young workers entering the labor market (Biagi and Sacconi, 2001). In this section we therefore examine the dynamic

[8]Table C.4 which estimates effect of the reform on the job flows for this particular group of workers

[9]The average age for incumbent temporary (permanent) workers is 27.7 (38.9) years in 2001.

impact of entering the labor market under the new regime on temporary contracts on the career trajectory of new entrants from their entry year up to their seventh year of potential experience in the labor market.

Recall that assignment to the new policy regime is conditional on employment. New entrants might therefore select their entry year as well as entry CCNL based on the reform status. Below, we show that possible unobserved changes in the composition of new entrants driven by the aforementioned selection mechanism do not appear to be a crucial concern.

If the reform status of a given CCNL correlates with the decision to enter the labor market then we would expect the reform to significantly impact the share of new entrants observed among employed individuals. Figure B.1.7 shows however that this does not appear to be the case, as the event study on the share of new entrants before and after the reform shows a relatively flat and insignificant profile overall. Moreover, Table C.3 shows that the observed characteristics of new entrants are relatively well balanced before and after the reform. Importantly, we do not find post-reform workers increasingly selecting into lower paying occupations, where the ranking of occupations is established using pre-reform data only. Similarly, if we construct a score of the predicted log wage based on various workers' observables using data prior to the policy change, we do not find that the average predicted score changes significantly for new entrants before and after the reform.

We consequently turn to the following regression framework to evaluate how entry in the labor market following the implementation of the reform affects earnings in each year following entry:

$$Y_i^\ell = \eta^\ell_{CCNL(i,e(i))} + \lambda^\ell_{g(i),e(i)} + \delta^\ell R_i + \varepsilon_i^\ell \qquad \text{for } \ell \in \{1,\dots,7\}. \tag{2.4}$$

In expression (2.4) ℓ measures years following entry in the labor market. The function $e(i)$ outputs for worker i the year of entry in the labor market so that $\eta^\ell_{CCNL(i,e(i))}$ controls for entry CCNL fixed effects. The term $\lambda^\ell_{g(i),e(i)}$ captures unrestricted interactions between year of entry and a specific worker group type defined in terms of gender, nationality, age at entry, and province.

The key variable of interest is $R_i = \mathbf{1}\{e(i) \geq t^*_{CCNL(i,e(i))}\}$ where $t^*_{CCNL(i,e(i))}$ denotes the year of passage of the reform in the entry CCNL of worker i. Therefore, R_i measures whether worker i's first job is in new policy regime on temporary contracts. Models akin to (2.5) have been used recently to assess the short- and long-run effects of entry conditions on labor market earnings for younger individuals (Oreopoulos et al., 2012). The identification

of δ^ℓ is driven by comparing earnings, after taking out common fixed differences across entry CCNLs, in the ℓth year following entry between two workers with the same group type and entry year but where one worker entered the labor market under the new policy while the other one was still covered by the old policy at time of entry.

To further test for issues of endogenous entry based on reform status and to account for heterogeneous treatment effects based on entry year and year of enactment of the reform, we augment model (2.4) as follows:

$$Y_i^\ell = \eta^\ell_{CCNL(i,e(i))} + \lambda^\ell_{g(i),e(i)} + \sum_{k=a}^{b} \delta_k^\ell R_i^k + \varepsilon_i^\ell \qquad \text{for } \ell \in \{1,\ldots,7\}. \tag{2.5}$$

where $R_i^k = \mathbf{1}\{e(i) \geq t^*_{CCNL(i,e(i))} + k\}$. Therefore, δ_k^ℓ for $k \geq 0$ captures how having entered the labor market $k \geq 0$ years following implementation of the reform affects labor market outcomes ℓ years after entry. Since we normalize $\delta^\ell_{-1} = 0$, the coefficients δ_k^ℓ are expressed relative to the cohort that entered the labor market one year prior to enactment of the reform in the corresponding CCNL of entry. We estimate equation (2.5) separately for each ℓ and we restrict our attention to workers that entered between 1998—2008 and were under 30 years of age.

2.8 Results: New Entrants

Figure 2.2 sets the stage for the regression analysis by plotting annual earnings of workers entering the labor market between 1998-2008. There appears to be a dip in the earnings profile occurring for workers entering after 2001 that continues to persist in the years following entry.

Figure 2.3, Panel (a) isolates the contribution of the reform to the earnings of new entrants at the time of their entry in the labor market. Notice that when $\ell = 1$, $\{\delta_k^1\}$ in equation (2.5) represent event study coefficients that indicate whether in the first occupation workers were covered under the reform ($k \geq 0$) or not. There is no evidence of an Ashenfelter's dip (Ashenfelter, 1978) in the earnings of new entrants leading up to the reform, confirming that issues of dynamic selection into treatment are not of the first order, while there is a significant downward jump of around 200 euros ($\approx 4\%$) for the cohort of individuals whose year of entry coincides with the first year of enactment of the reform that continues to persist and grow for subsequent cohorts.

Table 7, Panel (a) shows that the cohorts that enter in the post-reform labor market are associated with lower earnings compared to the cohort that entered one year before the implementation of the reform. These effects are relatively persistent up to the 7th year following entry. The negative estimates on earnings are qualitatively similar when we do not condition estimation on employed individuals. Figure B.1.8 quantifies the cumulative losses across cohorts whose first job was under the reform and the reform had been implemented in the entry CCNL for $k \geq 0$ years. These cumulative losses, when conditioning on positive earnings, range from 10% to 38% when benchmarked against the average earnings of the cohort that entered one year before enactment of the reform. Similar losses are reported when using unconditional estimates, see Figure B.1.8, Panel (b).

One possible explanation behind of these earning losses shown in Figure B.1.8 is that cohorts entering following the policy change are disproportionately more likely to be "trapped" in temporary jobs. Figure 2.3, Panel (b) shows the event study associated with the probability that the entering job is under a temporary contract signed directly by the firm. Post-enactment, this share increases by 3 percentage points ($\approx 7\%$ of the pre-reform mean) and continues to grow for subsequent cohorts entering post-reform. Table C.7 shows that the share of workers hired directly by the firm under a temporary contract remains significant essentially up to the fifth year following entry. Therefore, cohorts entering in the post-policy regime are: (i) more likely to be hired as temporary workers hired directly by the firm, (ii) this effect persists for several years following entry, and (iii) both (i) and (ii) are associated with decreases of contemporaneous and future earnings. One potential explanation is that these temporary jobs created by the firms directly are increasingly associated with less on-the-job training. Indeed, post-reform firms have fewer incentives to keep workers originally employed under a temporary contract and this in turn makes firms more reluctant to invest in their training. Cabrales et al. (2014) provide direct evidence of this point by showing how in OECD countries temporary contracts are associated with significant reductions in in on-the-job training. The effect is stronger for countries with a higher degree of dualism, like Italy.

2.8.1 Heterogeneity

We begin by computing PDV losses for workers that entered in a Local Labor Market (defined as a particular province and CCNL combination, LLM henceforth) not particularly affected by the reform. We then compare their losses to the losses of workers who instead entered a

LLM more affected by the reform. To measure the "intensity" of the reform, we compute the impact of the latter on the share of new temporary jobs created in each LLM and split this effect into quartiles. We then estimate an alternative version of (2.5) where we control for LLM entry fixed effects and we collapse the event dummies into a pre and post indicator and interact the latter with the quartile indicators defined above which are assigned on the basis of the LLM of entry of the worker. The PDV losses from this model are represented in Figure B.1.9. We can see that for workers entering in LLMs least affected by the reform, we do not find any significant loss in terms of PDV relative to cohorts entering in the pre-reform era. These losses become significant in the second-quartile and continue to grow up to the fourth quartile consistently with the evidence shown in Section 2.8.

Figure 2.4 reports additional heterogeneity effects. PDV losses are present also when we condition on full-time workers. Interestingly, PDV losses are particularly high for male workers while they are insignificant for women. There is a substantial amount of heterogeneity also across age at entry in the labor market, which represents our best proxy in our data for education. Workers entering at 20 years of age do not appear to experience significant PDV losses. On the other hand, workers entering the labor market at 26 years of age (and therefore more likely to hold a college degree) experience significant and economically large losses. Similarly, workers that entered in occupations in the fourth quartile of the pre-reform distribution of average pay incur the largest losses. On the other hand, workers employed in lower ranked occupations (corresponding to the first quartile of the pre-reform distribution) have slightly positive but insignificant overall cumulative effects.

Taken together, these findings suggest that the majority of the PDV losses associated with the reform are driven by workers belonging to the top of the predicted pre-reform wage distribution calculated using predetermined characteristics at entry such as gender, age and occupation. This pattern is consistent with recent descriptive evidence by Naticchioni et al. (2016) who show how more skilled Italians in the most recent cohorts have suffered a particularly severe earnings reduction.

The findings on entrants are consistent with the theory of temporary jobs representing a "precarious trap" rather than a "stepping stone" (Booth et al., 2002; Faccini, 2014) for young workers, as predicted by Blanchard and Landier (2002). García-Pérez et al. (2016) reach a similar conclusion looking at data from Spain, a country with a degree of dualism similar to that of Italy.

2.9 Conclusions

This chapter shows that the partial and asymmetric reform to employment protection laws described in the previous chapter generated asymmetric effects.

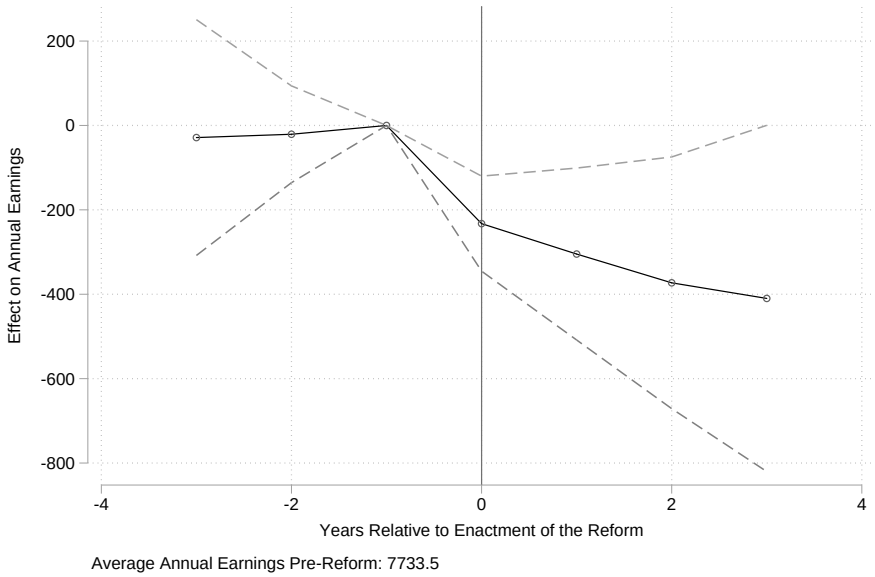
Firms' profits increase after this reform as do measures of within-firm inequality and managers' earnings. Significant increases in the job destruction rate of temporary jobs map into a close to null effect on the individual average probability to be employed following the policy change. Incumbent workers, hired in the pre-reform under a permanent contract, enjoy higher earnings while incumbent temporary workers and new entrants incur significant losses in earnings after the reform.

Although our findings for workers focus primarily on earnings, facilitating hiring under temporary employment contracts can also impact other key aspects of a young individual's life such as the decision to have a child, home ownership, and on-the-job injuries/fatalities. This might represent an interesting avenue for future research.

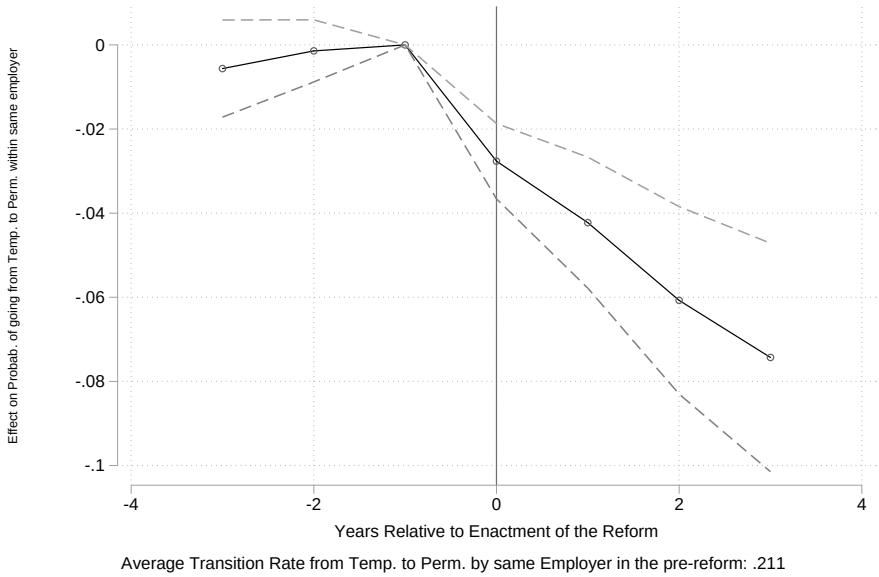
We believe our findings can better inform the current policy debate both in the US (Irwin, 2017) and in Europe (Alderman, 2017), as temporary work and alternative employment arrangements are becoming increasingly popular. Future research should be aimed at understanding if different reforms that combine the relaxation of constraints on hiring under temporary contracts with reductions in the employment protection of incumbent workers generate different responses than the ones estimated in this Chapter (Sestito and Viviano, 2016).

2.10 Figures

Figure 2.1: Impact of the Reform on Incumbent Temporary Workers



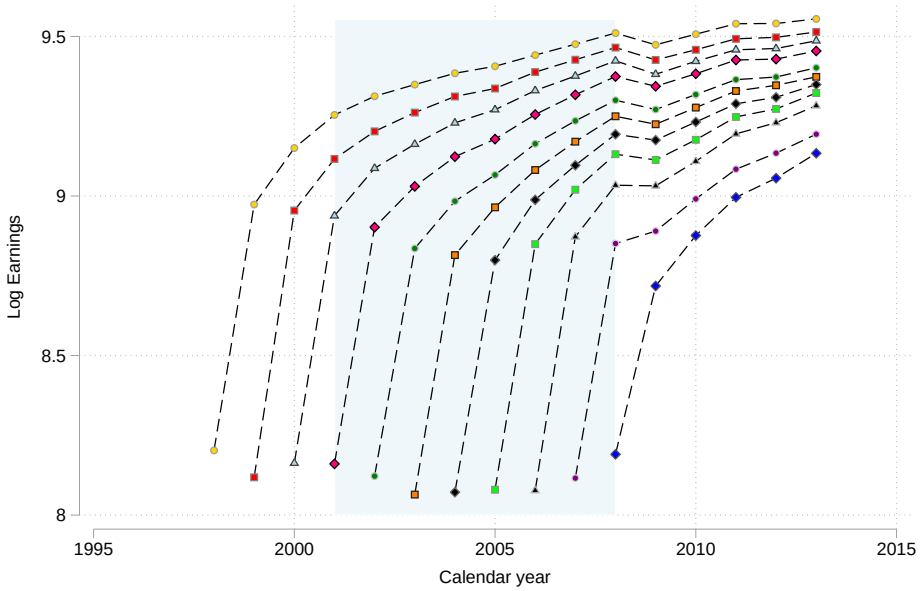
(a) Annual Earnings



(b) Probability of Converstion from Temporary to Permanent (Same Employer)

Note: This graph focuses on incumbent temporary workers, that is, workers that in the pre-reform era (1998-2001) were always employed under a temporary contract. We then run an event study on this group of workers where the event dummies are assigned based on the CCNL associated with the employment spell in 2001 and the outcome variable is listed on top of each panel. In Panel (b), the outcome variable is an indicator equal to 1 if the worker in $t+1$ is hired under a permanent contract by the same employer associated with the employment spell of year t. 95% confidence intervals obtained after clustering the standard errors at the CCNL x Province level. Source: INPS-INVIND-CNEL.

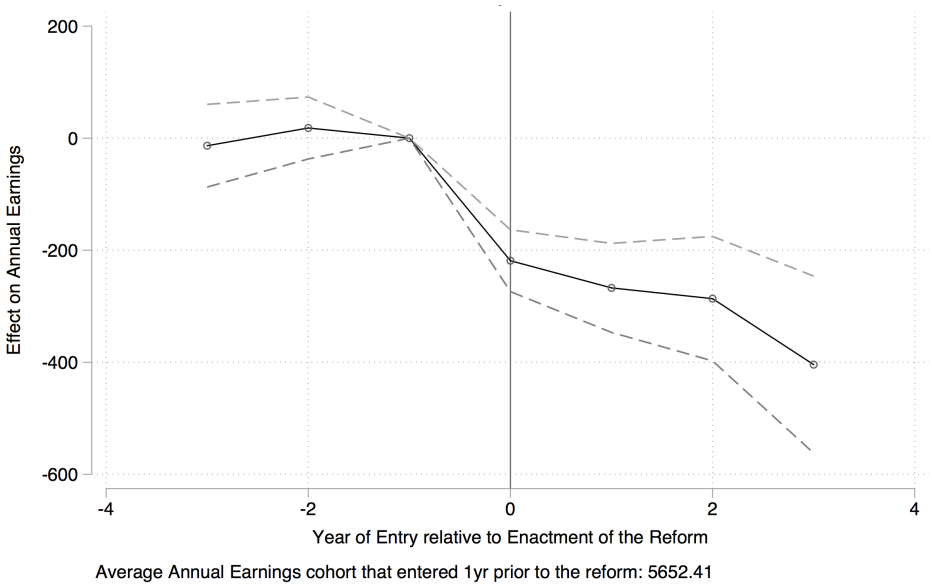
Figure 2.2: Descriptive Evidence on Entry Level Earnings



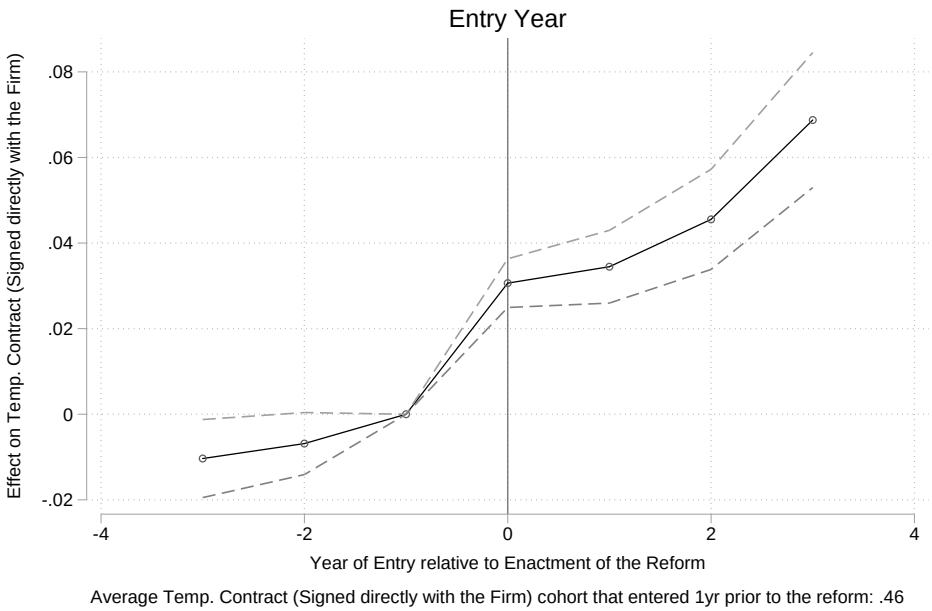
Note: This graph plots the log annual earnings of workers entering the labor market from 1998-2008 that were less than 30 years old at the time of entry. The shaded blue area corresponds to the years associated where different CCNLs enacted the reform. Source: INPS-INVIND-CNEL.

Figure 2.3: Impact of the Reform on New Entrants

(a) Annual Earnings

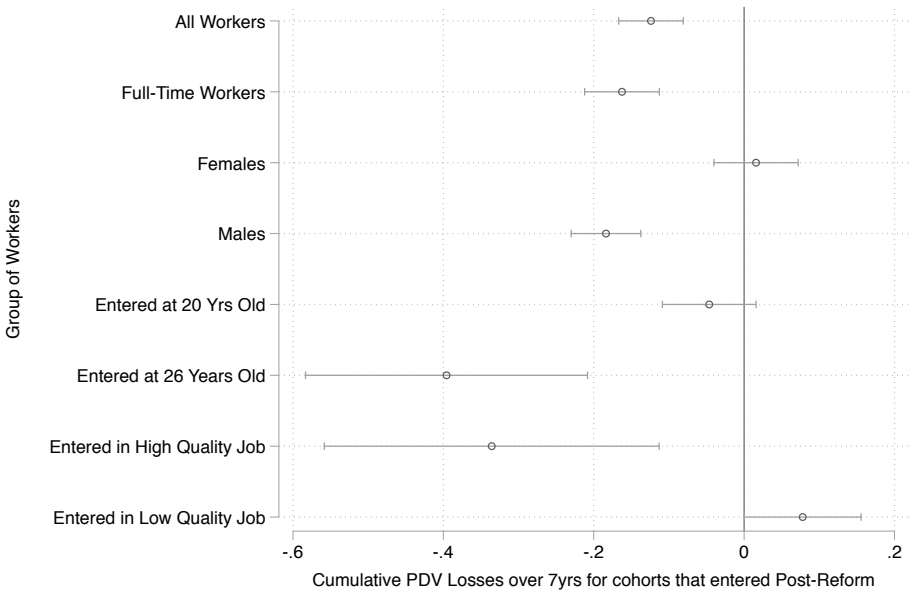


(b) Hired as a Temporary Directly by the Firm



Note: This graph plots the effect of the reform on new entrants' outcomes in their first year of entry, i.e. when $\ell = 1$ in equation (2.5). The dependent variable in Panel (a) is annual earnings. The dependent variable in Panel (b) is an indicator equal to 1 if the worker is hired under a temporary contract signed directly by the firm (excluding therefore temp jobs found via temporary work agency, internships and some specific types of external collaborations akin to consulting). The coefficient for the cohort that entered one prior to the passage of the reform, $\{\delta^1_{-1}\}$, is normalized to zero. See text for details. 95% confidence intervals obtained after clustering the standard errors at the entry CCNL x Province level. Source: INPS-INVIND-CNEL.

Figure 2.4: Heterogeneity in PDV across Group of Workers



Note: This graph plots the PDV values losses computed by running (2.4) where the treatment indicator R_i is interacted with the baseline characteristic reported in the graph. The estimated model controls for entry CCNL fixed effects, year of entry fixed effects interacted with worker type defined in terms of gender, nationality, age at entry, province and reported baseline characteristic when needed. PDV value calculations use an interest rate of 3% and are benchmarked relative to the average earnings of the cohort that entered one year prior to liberalization and belongs to the group reported in the graph. To construct an indicator of job quality, we proceed as follows: using only pre-reform data, we regress average log daily wages in the first seven years of potential labor market experience on unrestricted interactions of Age at Entry, Gender, Foreign, Province of Residence at entry as well as unrestricted interaction between work status and 5 digit industry dummies at the time of entry. The latter estimated fixed effects represent what we define as "Quality of the Job". High quality jobs represent jobs that belong to the fourth quartile of the corresponding distribution. Low quality job belongs to the first quartile of the corresponding distribution. Reported standard errors and 95% confidence intervals are computed via block bootstrap.

2.11 Tables

Table 2: Impact of the Reform on Firms' Outcomes

	# of Workers	Sales per Worker	Value Added per Worker	Labor Costs per worker	Profits per Worker	Profit margin	Firm Survival	Firm Creation
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel (a): Unique Event Year								
Post-Reform	0.7271	0.0665	-0.1891	-0.2683***	0.1518***	0.0055***	0.0241***	0.0847***
	(0.4429)	(0.5781)	(0.1183)	(0.0475)	(0.0577)	(0.0018)	(0.0022)	(0.0060)
Avg. Outcome Pre-Reform	16.06	167.87	32.71	20.56	3.58	.1	.95	.07
# of Firms	322,507	322,507	322,507	322,507	322,507	322,507	322,507	322,507
Panel (b): All Firms								
Post-Reform	0.5811	-0.6462	-0.2290**	-0.2421***	0.1446***	0.0042***	0.0213***	0.0723***
	(0.8866)	(0.5202)	(0.0987)	(0.0421)	(0.0505)	(0.0016)	(0.0018)	(0.0050)
Avg. Outcome Pre-Reform	21.36	163.58	32.62	20.54	3.61	.1	.96	.07
# of Firms	372,697	372,697	372,697	372,697	372,697	372,697	372,697	372,697

Note: This table shows difference in differences estimates of the effect of the reform on firms' outcomes, see equation (7) of the main text. Sample in Panel (a) corresponds to firms such that all their workforce renew their national contract (and therefore implemented the reform) in the same year while Panel (b) corresponds to the universe of firms in INPS-INVIND-CNEL matched with CERVED. For firms with multiple CCNLS, the post-reform dummy is assigned on the basis of the earliest renewal, see text for details. Sales, Value Added, Labour Costs and Profits are all expressed in thousands of real 1995 euros. # of Workers report the average number of monthtly emplooyes as reported by INPS. Profit margin is defined as profits divided by value added. Estimates on Column 7 and Column 8 are computed after balancing the firm-year panel. In this balanced panel, survive is a dummy equal to equal to zero whenever the firm in the corresponding year does not report any sales, value added nor profits, as reported in CERVED. Firm creation is a dummy equal to one if the associated year corresponds to the administrative date of creation of the firm (or of the main plant in cases of multiple EINs associated to the same firm). All models control for firm fixed effects and province by year fixed effects. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND-CNEL. Standard errors clustered at CCNL x Province level. Singleton observations (95,031 in Panel a; 101,079 in Panel b) are dropped from estimation.

Table 3: Heterogeneity on Firms' Outcomes

		Geography			*Pre-Reform Structure*		
	All	North	Center	South	Older Workforce	High AKM effect	Less Concentrated Sectors
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel (a): Fraction on a Temporary Contract							
Post-Reform	0.0161***	0.0142***	0.0257***	0.0095	0.0395***	0.0246***	0.0379***
	(0.0030)	(.0038)	(.0065)	(.0068)	(.0040)	(.0039)	(.0042)
Panel (b): Value Added per Worker							
Post-Reform	-0.1891	-0.2612	-0.3864	0.3811	-0.4415***	-0.6984***	-0.161
	(0.1183)	(.1588)	(.2214)	(.2748)	(.1518)	(.1875)	(.1504)
Panel (c): Labour Costs per Worker							
Post-Reform	-0.2683***	-0.3487***	-0.2163***	-0.0369	-0.6052***	-0.7757***	-0.3385***
	(0.0475)	(.0659)	(.0716)	(.1075)	(.0541)	(.0677)	(.0584)
Panel (d): Profits per Worker							
Post-Reform	0.1518***	0.2635***	-0.0915	0.084	0.202**	0.1538*	0.201***
	(0.0577)	(.074)	(.1163)	(.1319)	(.0807)	(.0819)	(.0739)
# of Firms	322,507	322,507	322,507	322,507	212,296	203,834	322,507

Note: This table reports the sum of the difference in differences coefficient described in equation (7) and the coefficient associated with the interaction term between the post reform indicator of equation (7) and a firm fixed binary characteristic as listed in each column of the table. Value added, labor costs and profits are expressed in thousands of real 1995 euros. The first column shows overall difference in differences from equation (7) for the outcome variables described in each panel. Firms in Column [5] are such that the average age of the worforce in the pre-reform periods belongs to the fourth quartile of the corresponding distribution. Firms in Column [6] have AKM effects that belong to the fourth quartile of the corresponding distribution. AKM effects are calculated using the largest connected set of firms from pre-reform data running from 1990-2001, see Appendix F for details. Firms in Column [7] belong to sectors (defined in terms of ATECO codes) in which the pre-reform total share of sales accrued to the 20 largest firms is in the first quartile of the corresponding distribution. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND-CNEL. Singleton observations dropped from estimation. Standard errors are clustered at CCNL x Province level. Estimation performed on the sample of firms with unique event year.

Table 4: Impact of the Reform on Within Firm Pay Structure

	Std. Of Earnings	Ratio: Top/Lowest Earner	Gap in Earnings Young vs. Old	Wage Bill: Managers	# of Managers	Avg. Earnings of Managers	ROE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel (a): Unique Event Year							
Post-Reform	92.4013***	0.1171**	210.6710**	13,315.24***	0.0128***	619.8087	0.0125***
	(33.1605)	(0.0533)	(102.6156)	(4260.2747)	(0.0041)	(390.6920)	(0.0032)
Avg. Outcome Pre-Reform	5932.56	4.55	5383.9	105,472	.11	43,322	.07
# of Firms	322,507	322,507	185,441	322,507	322,507	17,024	322,507
Panel (b): All Firms							
Post-Reform	230.2756***	0.3123***	256.3669***	36,456.5***	0.0398***	1434.8696***	0.0123***
	(31.2251)	(0.0565)	(89.9577)	(7857.8923)	(0.0081)	(325.0088)	(0.0027)
Avg. Outcome Pre-Reform	6076.44	5.37	5809.22	179,776	.18	46,275	.08
# of Firms	372,697	372,697	237,839	372,697	372,697	28,962	372,697

Note: This table shows difference in differences estimates on measures of within firm inequality, see equation (7) of the main text. Sample in Panel (a) corresponds to firms such that their workforce renew their national contract (and therefore implemented the reform) in the same year while Panel (b) corresponds to the universe of firms in INPS-INVIND-CNEL matched with CERVED. The outcome in Column 2 represents the ratio between the wage of the worker that earns the most and the worker that earns the least in a given firm year combination. The outcome in Column 3 is the average gap in earnings between employees age 40 or more vs. employees aged 30 or less. The outcome in Column 4 represents the total wage bill paid by the firm to its own managers (this measure is therefore equal to zero in the case in which the firm does not employ any managers). Estimates in Column 6 are for the subset of firms that employ at least one manager. All models control for firm fixed effects and province by year fixed effects. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND-CNEL. Singleton groups dropped from estimation. Standard errors are clustered at CCNL x Province level.

Table 6: Effect of the Reform on Workers' Outcomes

	Employment	Annual Earnings	Annual Days Worked	Annual Earnings (conditional on working)	Annual Days Worked (conditional on working)
	[1]	[2]	[3]	[4]	[5]
Panel (a): All Workers					
Post-Reform	0.0026**	-227.4803***	-0.9079***	-286.0140***	-1.9277***
	(0.0013)	(32.8396)	(0.2932)	(36.5815)	(0.2701)
# of Worker-Year Observations	44,201,457	44,201,457	44,201,457	41,660,198	41,660,198
Average Outcome Pre-Reform	.963	16685.21	260.311	17317.422	270.175
Panel (b): Incumbent Workers					
Post-Reform	0.0009	475.7296***	-3.4611***	456.8521***	-3.9987***
	(0.0007)	(66.8040)	(0.4244)	(66.1390)	(0.3246)
# of Worker-Year Observations	32,850,593	32,850,593	32,850,593	31,746,096	31,746,096
Average Outcome Pre-Reform	.99	17416.116	272.91	17594.65	275.708
Panel (c): Incumbent Temp Workers					
Post-Reform	-0.0113***	-243.6121***	-10.7760***	-188.4756***	-9.5045***
	(0.0025)	(75.2113)	(1.2739)	(66.2075)	(1.0307)
# of Worker-Year Observations	2,209,419	2,209,419	2,209,419	2,029,267	2,029,267
Average Outcome Pre-Reform	.976	7735.575	165.565	7927.215	169.667
Panel (d): Incumbent Perm Workers					
Post-Reform	0.0032	658.3718***	-0.6253	618.5525***	-1.5542***
	(0.0026)	(79.1984)	(0.4367)	(79.3368)	(0.2703)
# of Worker-Year Observations	23,422,774	23,422,774	23,422,774	22,859,129	22,859,129
Average Outcome Pre-Reform	.994	19688.538	289.185	19800.375	290.827

Note: Estimates in Panel (a) are based from equation (13) where each column list a particular outcome. Estimates in Panel (b)-(d) are based from equation (14). In particular, Panel (b) consists of workers that result employed with at least one day of work in 2001. Panel (c) consists of the workers in Panel (b) but that were always employed under a temporary contract in the pre-reform era: 1998-2001. Panel (d) consists of workers in Panel (b) that were always employed under a permanent contract in the pre-reform era: 1998-2001. Estimates across all panels focus on workers for which we can always match their CCNL with the CNEL archive. In Panel (a), the variable Post-Reform is a dummy equal to one if the CCNL associated to worker i in period t is under the new policy regime. For non employed individuals this dummy is assigned on the basis of the CCNL associated to the last employment spell. In Panel (b)-(d), the variable post-reform is assigned on the basis of the CCNL applied to a given worker in the year 2001. Employment is a dummy equal to 1 if the worker has reported at least one day of work to INPS in a given year and zero if the worker does not result employed in the INPS database in the corresponding year but results employed in any of the following years. A similar principle is applied to earnings and days worked, where missing values are replaced with zeros. In Column [3] and [5] we restrict the sample to workers that report positive earnings in a given year. All regressions control for province x year and CCNL fixed effects as well as worker micro controls such as gender, nationality (Italian vs. non-Italian), year of birth dummies and a quadratic in potential experience. In Panels (b)-(d), these Province by Year and CCNL fixed effects are defined with respect to the CCNL and the province associated to the employment spell measured in 2001. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Table 7: Dynamic Effect of The Reform for New Entrants

	Year Following Entry in the Labor Market						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A: Annual Earnings (Conditional on Working)							
Entered in the 1st Year of Enactment of the Reform	-218.7683***	-155.0625***	-158.5157***	-68.9004	-182.1512***	-129.9790***	-144.9972***
	(28.1485)	(42.3929)	(45.5389)	(42.5665)	(44.8490)	(43.7995)	(46.6382)
Entered in the 2nd Year of Enactment	-267.2777***	-244.7978***	-243.4170***	-241.6900***	-285.9301***	-267.7745***	-194.5710**
	(40.5473)	(70.7137)	(76.5237)	(70.2266)	(74.8421)	(74.8434)	(76.5012)
Entered in the 3rd Year of Enactment	-286.4935***	-369.6430***	-485.9216***	-463.4765***	-514.0049***	-386.9334***	-423.5988***
	(56.6896)	(107.3893)	(115.2004)	(109.6938)	(116.4714)	(112.8074)	(114.5189)
Entered in the 4th or later Year of Enactment	-404.2577***	-570.6097***	-667.9142***	-686.3711***	-717.8611***	-684.3223***	-654.2956***
	(80.6201)	(156.8952)	(176.5341)	(170.8084)	(177.6657)	(171.6595)	(167.0238)
Average Earnings - Entered 1 year prior to Reform	5652.41	9543.187	10733.526	11617.691	12504.128	13211.925	13998.69
Number of Observations	1,523,145	1,325,870	1,264,021	1,246,423	1,235,563	1,223,544	1,138,362
Panel B: Annual Earnings (Unconditional)							
Entered in the 1st Year of Enactment of the Reform	-218.7683***	-164.2035***	-152.7189***	-86.3152*	-165.9886***	-105.3970**	-120.9168**
	(28.1485)	(43.0934)	(46.7153)	(46.1642)	(45.9504)	(45.0709)	(47.3723)
Entered in the 2nd Year of Enactment	-267.2777***	-258.1952***	-222.6862***	-245.2507***	-259.8396***	-225.9337***	-144.4464*
	(40.5473)	(71.0175)	(77.0097)	(73.6522)	(75.1897)	(74.9847)	(77.2451)
Entered in the 3rd Year of Enactment	-286.4935***	-374.6382***	-459.2321***	-447.1004***	-463.8710***	-359.1505***	-388.3389***
	(56.6896)	(104.2702)	(114.3686)	(108.5966)	(109.2180)	(109.8134)	(113.6920)
Entered in the 4th or later Year of Enactment	-404.2577***	-570.8906***	-622.0788***	-633.4436***	-633.1121***	-611.4529***	-573.7486***
	(80.6201)	(152.3143)	(172.2671)	(165.0358)	(165.8477)	(164.9527)	(164.4476)
Average Earnings - Entered 1 year prior to Reform	5652.41	8258.36	8991.54	9751.06	10635.32	11520.05	12513.26
Number of Observations	1,523,145	1,519,681	1,484,907	1,453,704	1,420,020	1,379,096	1,265,072

Note: This table presents estimates from equation (16) where the dependent variable is annual earnings on all new entrants observed in the INPS-INVIND data between 1998-2008 that were less than 30 years old at the time of entry. Each row reports the coefficients δ from equation (16) for cohorts that entered in the post-reform labor market separately for each year following entry where the first column corresponds to the entry year in the labor market. These coefficients are expressed relative to the cohort that entered one year prior to implementation of the reform. Panel (a) conditions estimation on workers that have positive earnings in a given year. Panel (b) provides unconditional estimates where missing earnings for a given worker in year t are replaced with zero provided that the worker reports positive earnings for at least one period following year t. Each estimate is computed after controlling for CCNL of entry fixed effects, year of entry fixed effects interacted with worker type defined in terms of gender, nationality, age at entry and province. Standard errors are clustered at the entry CCNL x Province level. Source: INPS-INVIND-CNEL.

Chapter 3

Temporary contract workers and the boundary of the firm

3.1 Introduction

In this section, we abstract from the effect of the reform and focus on the contract gap in wages. The economic forces that can explain the existence of such a gap can be several, ranging from compensating differentials (Rosen, 1986), to asymmetric information (Terviö, 2009), to efficiency wages (Bulow and Summers, 1986; Rebitzer and Taylor, 1991). In this paper, we investigate a specific channel: employers' pay policies and differences in the amount of rents that firms share with permanent versus temporary workers.

This analysis is motivated by the institutional features of the Italian labor market. Italian industrial relations are based on a two-pillar system (Guiso et al., 2005). The first pillar consists of sectoral bargaining agreements that establish minimum wages for different occupational classes. Within the second pillar, unions and workers bargain with employers wage top-ups above contractual minimums. Firms can also distribute additional premiums and bonuses.[1]

Regarding the first pillar, the law explicitly forbids to discriminate between contract types when establishing minimum wages. Within the second pillar, however, firms are legally allowed to split rents/premiums/bonuses differently between permanent and temporary employees (Picchio, 2006; Montanari, 2002). Notably, evidence indicates that temporary work-

[1]Card et al. (2014) show that the median premium above minimums established at the CCNL level is around 24%. Using wage formation data in the metal products, machinery, and equipment industry, Guiso et al. (2005) report that in 1994 the average wage component due to firm-specific pay policies was around 23%. The latter grew to around 30% in 2009 according to the same data-source (Federmeccanica, 2009).

ers are not well represented by unions. For instance, 97% of all the workers registered to the largest union in Italy (CGIL) that are less than 35 years old are under a permanent contract (Lani, 2013; Bentolila and Dolado, 1994).

To determine the extent to which firm-level bargaining matters for the contract wage gap, we rely on a model of differential rent-sharing. Leveraging from within-person, within-employer transitions from temporary to permanent contracts, we find that around 75% of the raw return associated to such a transition can be explained by differences in bargaining power.

3.2 Research Design

We assume that the wage of a worker i, in period t can be expressed as

$$w_{it} = m_{it} + \gamma_{C(i,t)}\mathcal{S}_{i,J(i,t),t} \tag{3.1}$$

where m_{it} represents the outside option available to worker i in period t. The function $C(i,t) \in \{T, P\}$ returns the contract type of worker i in period t while $J(i,t)$ returns the identity of the employer of worker i in period t; $\mathcal{S}_{i,J(i,t),t}$ represents the surplus available for worker i in period t from the current employer $J(i,t)$. Finally, $\gamma_{C(i,t)}$ represents a contract-specific bargaining parameter.

An equation akin to (3.1) can be obtained within the heterogeneous bargaining power search and matching framework of Section C.2.[2]

We assume that the outside option can be decomposed to $m_{it} = \alpha_i + X_{it}^\top\beta + \tilde{\varepsilon}_{it}$ where α_i represents time-invariant skills portable across employers and contracts. The vector $X_{it}^\top\beta$ represents observable characteristics of the worker (such as experience) and $\tilde{\varepsilon}_{it}$ is a transitory component. The surplus term $\mathcal{S}_{J(i,t),t}$ is decomposed to $\mathcal{S}_{J(i,t),t} = S_{J(i,t),t} + A_{i,J(i,t),t}$ where $S_{J(i,t),t}$ is an average component that raises surplus for all employees at the firm and $A_{i,J(i,t),t}$ is a worker-specific component of the surplus available to the current employer that may arise because the employer particularly values the skills of worker i in period t.

We thus arrive at the following reduced-form equation for wages:

$$w_{it} = \alpha_i + X_{it}^\top\beta + \gamma_{C(i,t)}S_{J(i,t),t} + \varepsilon_{it} \tag{3.2}$$

where $\varepsilon_{it} = \tilde{\varepsilon}_{it} + \gamma_{C(i,t)}A_{i,J(i,t),t}$. We estimate (3.2) using a within person, within employer design. Specifically, we focus on all the workers who start on temporary contract and at one

[2]A representation akin to (3.1) can also be obtained in the wage posting model of Card et al. (2016) assuming imperfect substitution across employment contracts.

point in their career transition from a temporary to a permanent contract within the same employer. In this design, counterfactual wages for workers that in year t move from temporary to permanent are constructed using workers who are still employed under a temporary contract in year t and who will reach the permanent contract in subsequent years. We use log daily wage with the current employer as the outcome variable in (3.2). As a measure of observed surplus, S_{it}, we follow the rent-sharing literature and use log value added per worker (Card et al., 2016).

3.2.1 Decomposition

Let $t^*(i)$ denote the year of the first transition from a temporary to a permanent contract for worker i. According to (3.2), the within-person expected wage change $\Delta w_{it} = w_{it} - w_{it-1}$ at $t = t^*(i)$ is given by

$$E[\Delta w_{it}|\alpha_i, X_{it}, X_{it-1}, S_{J(i,t),t}, S_{J(i,t),t-1}, t = t^*(i)] = \Delta X_{it}^\top \beta + \gamma_P S_{J(i,t),t} - \gamma_T S_{J(i,t),t-1}, \quad (3.3)$$

provided that $E[\Delta\varepsilon_{it}|\alpha_i, X_{it}, X_{it-1}, S_{J(i,t),t}, S_{J(i,t),t-1}, t = t^*(i)] = 0$. The latter is the key assumption underlying the estimation of the bargaining coefficients $\{\gamma_P, \gamma_T\}$. Biased estimation of these coefficients can arise if, conditional on the observed skills X_{it} and α_i, the transition from a temporary to a permanent contract is systematically related to changes in unobserved skills, $\tilde{\varepsilon}_{it}$, or in the match component $\gamma_T A_{i,J(i,t),t}$. Although fundamentally untestable, we indirectly assess the presence of such a systematic relationship by evaluating wage patterns and the evolution of the bargaining coefficient prior to transitioning from a temporary to a permanent contract.

We use equation (3.3) to provide the following Oaxaca decomposition (Oaxaca, 1973; Fortin et al., 2011) at $t = t^*(i)$:

$$E[\gamma_P S_{J(i,t),t} - \gamma_T S_{J(i,t),t-1}|t = t^*(i)] = \underbrace{E[S_{J(i,t),t}|t = t^*(i)](\gamma_P - \gamma_T)}_{\text{Bargaining Component}} + \underbrace{\gamma_T E[\Delta S_{J(i,t),t}|t = t^*(i)]}_{\text{Surplus Component}} \quad (3.4)$$

In equation (3.4), the first term captures how much of the change in wage occurring at $t = t^*(i)$ is associated with a change in bargaining power. The second term measures how much of the return is captured by a change in the available surplus at the time of the event.[3]

[3]Results are virtually identical when using the alternative Oaxaca representation relative to equation (3.4).

3.3 Results

Figure 3.1 shows a bin scatter plot of average log daily wages for temporary vs. permanent workers within 100 percentile bins of log value added per worker. Notice that the rate at which potentially higher rents captured by higher log value added per worker are mapped into higher wages is significantly lower for workers under a temporary contract.

Clearly, the evidence in Figure 3.1 is only descriptive. To account for the sorting of high-ability workers into more productive firms, we focus on within-employer transitions of workers from a temporary to a permanent contract (henceforth abbreviated to $T \to P$). Figure 3.2, Panel (a) shows event-study estimates on the log daily wage of workers around such event. Recall that in this design workers are always employed under a temporary contract prior to obtain a permanent contract (i.e., when $t < t^*(i)$). Figure 3.2, Panel (a) shows that pre-trends leading to the transition $T \to P$ are relatively flat. This suggests that employer learning à la Gibbons and Katz (1991) is not reflected in the pre-event wage dynamics *while individuals are employed under a temporary employment contract*. When the worker obtains a permanent job, we observe a sudden increase of log wages of around 6%.[4] Interestingly, the post-event coefficients continue to grow in the years following the transition to a permanent contract, suggesting that returns to experience occur primarily while workers have a permanent contract.

Figure 3.2, Panel (b) plots the rent-sharing coefficient $\gamma_{C(i,t)}$ from an augmented version of (3.2) where log value added per worker at the current employer is interacted with event time dummies. While the worker is under a temporary contract, these rent-sharing coefficients exhibit a flat profile with an overall level centered at around 0.03. This suggests that $T \to P$ transitions are not primarily driven by an increasing comparative advantage captured by the unobserved time-varying match surplus defined as $\gamma_T A_{i,J(i,t),t}$. At the time of the event, the pattern of the rent-sharing coefficients changes significantly. There is a jump resulting into a rent-sharing coefficient that is now equal to approximately 0.05, in line with recent rent-sharing studies that focus on Italy (Guiso et al., 2005; Card et al., 2014). Notice that the rent-sharing coefficients appear to grow in the post-event years, suggesting that, contrary to what we found relative to the working years under a temporary contract, the experience accumulated following the transition $T \to P$ allows workers to obtain an increasing share of

[4] Unfortunately, we do not observe hours worked in INPS and so we cannot properly assess if the estimated return at $t = t^*(i)$ is due to changes in hours worked. We estimated the same model using full time workers and found very similar results, see Table C.6.

the surplus available at the firm.

Table 8 provides the Oaxaca decomposition shown in equation (3.4). The bargaining component explains around 75% of the unadjusted average wage increase observed at the time of the $T \to P$ transition. Conversely, the surplus component does not appear to have explanatory power.

A testable implication that follows from model (3.3) is that $T \to P$ transitions occurring within-firms that generate no significant surpluses should be associated with small returns for the worker. Testing this implication requires the identification of a set of "zero surplus" firms. To identify these firms, we follow the methodology of Card et al. (2016). In particular, we look for a threshold value κ such that firms with an observed surplus below κ are assumed to pay zero rents on average.[5]

We next run a regression of log daily wage on a permanent contract indicator, net surplus defined as $NS_{jt} = \max\{S_{jt} - \kappa, 0\}$, and an interaction between the latter and the permanent contract dummy, while controlling for worker fixed effects and additional time-varying workers' characteristics. In this regression, the coefficient on the permanent contract dummy measures the average $T \to P$ return for those workers employed in "zero surplus firms." Table C.5 in the Appendix shows that such coefficient is economically small and statistically indistinguishable from zero (we can reject returns higher than 1.8%). This suggests that workers obtaining a permanent contract in firms with close to zero rents do not receive significant returns once the contract is converted from temporary to permanent.[6]

3.4 Conclusions

The findings presented here show that economic profits are not split equally among employees hired under different types of employment contracts. Unions play an important role in the Italian wage setting system. However, temporary contract workers are usually poorly represented by unions. This generates in turn wide asymmetries in the degree of rent sharing between permanent contract workers and temporary ones. Relying on atypical, temporary

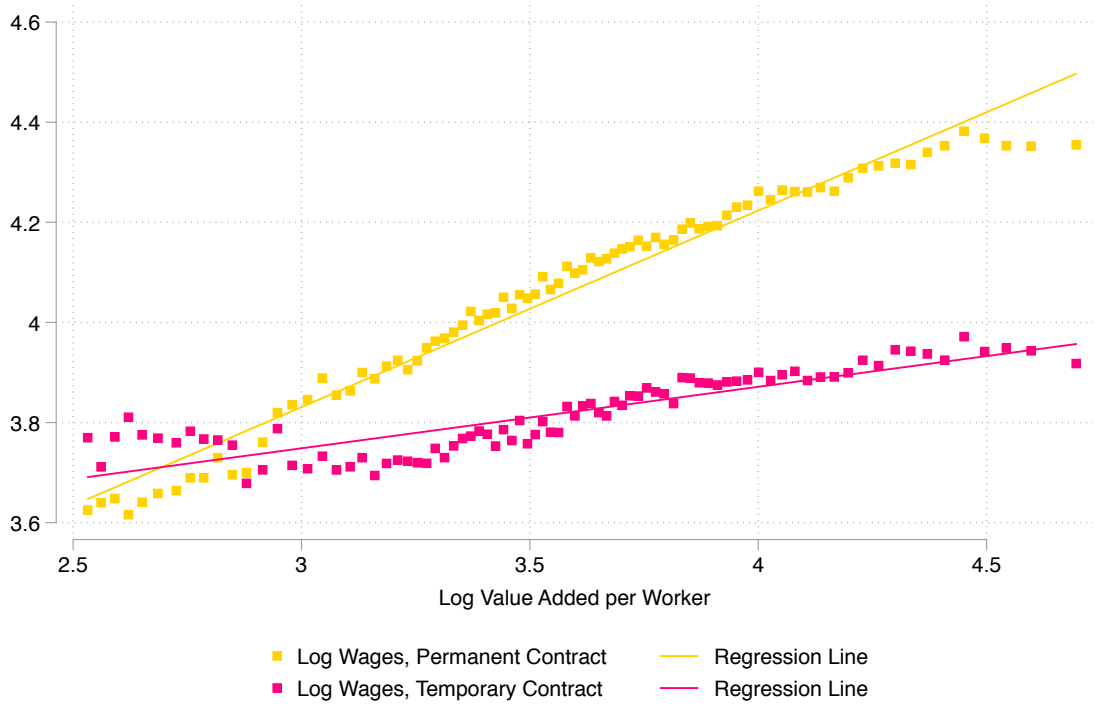
[5]The kink point is identified by fitting a series of bivariate regressions aimed at capturing the existence of a piecewise relationship between the AKM firm effects (computed separately for temporary and permanent workers) and our observed measure of surplus; see equation (17) of Card et al. (2016). The resulting set of zero-surplus firms are concentrated in small sectors with very low profits (e.g., fishing, hair dressing) and corresponds to roughly 3% of the matched person-year observations between INPS-INVIND-CNEL and CERVED.

[6]Results are similar when we replace κ as the average surplus available in firms belonging to the two lowest observed profitable sectors (fishing and hairdressing).

employment arrangements is therefore a way for firms to partially exclude workers from its boundary (Kline et al., 2017; Goldschmidt and Schmieder, 2017) and to ultimately to drive down labor costs, as shown in Table 4. To what extent this type of behaviour has contributed to increases in wage inequality and declines in the labor share observed in western economies is an interesting question for future research.

3.5 Figures

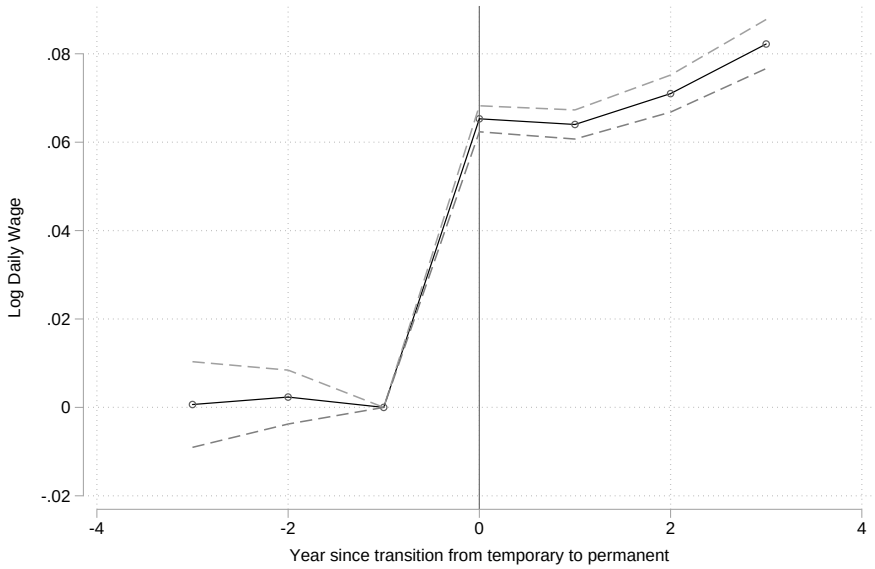
Figure 3.1: Wages, Contract and Value Added



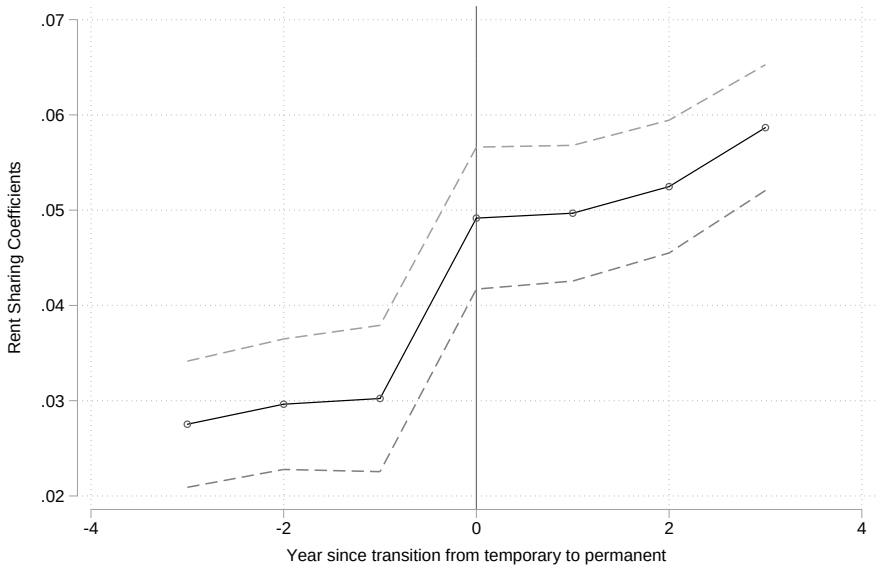
Note: This graph represents a bin scatter plot where we compute 100 percentiles of log value added per worker and report, within each bin, mean values of log daily wage for permanent workers and log daily wage for temporary workers, log value added per worker and share of workers under a temporary contract. Both value added and daily wages are expressed in real terms (CPI=1995). Source: INPS-INVIND-CNEL matched with CERVED.

Figure 3.2: Within-Person, Within-Employer Returns of Moving from Temporary to Permanent Contract

(a) Event Study



(b) Rent-sharing Coefficients in Event Time



Note: Panel (a) plots the event study coefficients on log daily wage and event time is defined in terms the first transition of a worker from a temporary to a permanent contract within the same employer. Panel (b) reports the coefficients of a regression of log daily wage on log value added per worker interacted with the same event dummies defined above. In estimating both panels we focus to all workers that start with a temporary contract and eventually transition to a permanent contract within the same employer and we have a measure of value added for such employers from CERVED, see Table B.1. In both panels, the event study regression model control for a quadratic term in potential experience, worker fixed effects, year effects interacted with gender, nationality, age at entry and 1 digit industry codes. 95% confidence intervals are reported after clustering the standard errors at the firm level.

3.6 Tables

Table 8: Within Employer, Within Person Evidence on Differential Rent Sharing

		Last Year under Temp	First Year under Perm	Difference
		[1]	[2]	[3]
[1]	Log Daily Wage	3.7849	3.8753	0.0904
	Components			
[2]	Log Value Added per Worker (Log Va)	3.6031	3.6018	-0.0012
[3]	Event Study Coefficient	0.0000	0.0589	0.0589
		(0.0000)	(0.0015)	(0.0015)
[4]	Rent Sharing Coefficient	0.0302	0.0492	0.0189
		(0.0038)	(0.0039)	(0.0005)
	Oaxaca Decomposition			
[5]	Surplus component	0.1089	0.1089	0.0000
				[0.00]
[6]	Bargaining component	0.1089	0.1771	0.0682
				[0.75]

Note: This table reports estimates from the decomposition shown in Section 7.1.1 of the paper. In particular, we investigate changes in the log daily wage of workers moving from temporary to a permanent contract within the same employer. Column 1 lists outcomes corresponding to the last year under a temporary contract for a worker, while Column 2 refers to the first year under a permanent contract. Column 3 presents differences between Column 2 and Column 1. The first row reports the average log daily wage of worker. Row 2 reports the average log value added per worker. Row 3 reports the event study coefficients plotted in Figure 8, Panel (a). Row 4 reports the rent sharing coefficients plotted in Figure 8, Panel (b). Both the estimates shown in Row 3 and 4 are computed after controlling for a quadratic term in potential experience, worker fixed effects and year effects interacted with gender, nationality and 1 digit industry codes. Rows 5-6 report the Oaxaca decomposition terms defined in equation (17) of the main text. The term in square bracket in column 3 represent the percentage that each component explains of the raw wage gap reported in Row 1, Column 3. Estimates based on workers that start with a temporary contract and eventually transition to a permanent contract within the same employer and we have a measure of value added for such employers. Number of person year observations is 5,340,083. Standard errors, clustered at the firm level, reported in round brackets. Source: INPS-INVIND-CNEL merged with CERVED.

Table C5: Returns at Zero Surplus Firms

Outcome Variable: Log Daily Wage	
Permanent	0.0051
	(0.0066)
Net Surplus	0.0138***
	(0.0052)
Net Surplus x Permanent	0.0514***
	(0.0050)

Note: This table shows the results of a regression in which the outcome variable is the log daily wage of a worker. In this regression, permanent is a dummy equal to 1 is the worker is under a permanent contract. Net surplus is a measure of net surplus based on log value added per worker calculated using the methodology of Card, Cardoso, Kline (2016), see text for details. Net Surplus x Permanent is an interaction between net surplus and the permanent dummy. The regression also controls for a quadratic term in potential experience, worker fixed effects and year effects interacted with gender, nationality and 1 digit industry codes. In this regression the permanent dummy captures the within person returns of moving from temporary to permanent at zero surplus firms. The estimates shown in this table are based on workers that start with a temporary contract and eventually transition to a permanent contract within the same employer and we have a measure of value added for such employers. Standard errors are clustered at the firm level. Number of person year observations is 5,340,083.

Table C6: Within Employer, Within Person Evidence on Differential Rent Sharing (Full Time Workers)

		Last Year under Temp	First Year under Perm	Difference
		[1]	[2]	[3]
[1]	Log Daily Wage	3.8926	3.9858	0.0932
	Components			
[2]	Log Value Added per Worker (Log Va)	3.7280	3.7247	-0.0032
[3]	Event Study Coefficient	0.0000	0.0637	0.0637
		(0.0000)	(0.0016)	(0.0016)
[4]	Rent Sharing Coefficient	0.0245	0.0427	0.0182
		(0.0038)	(0.0039)	(0.0005)
	Oaxaca Decomposition			
[5]	Surplus component	0.0913	0.0912	-0.0001
				[0.00]
[6]	Bargaining component	0.0912	0.1592	0.0679
				[0.73]

Note: This table reports estimates from the decomposition highligheted in Section 7.1.1 of the paper. In particular, we investigate changes in the log daily wage of workers moving from temporary to a permanent contract within the same employer, conditioning on workers who are always employed under a full time contract. Column 1 refers to the last year under a temporary contract for a worker, while Column 2 refers to the first year under a permanent contract. Column 3 presents differences between Column 2 and Column 1. The first row reports the average log daily wage of worker. Row 2 reports the average log value added per worker. Row 3 reports the event study coefficients plotted in Figure 8, Panel (a). Row 4 reports the rent sharing coefficients plotted in Figure 8, Panel (b). Both the estimates shown in Row 3 and 4 are computed after controlling for a quadratic term in potential experience, worker fixed effects and year effects interacted with gender, nationality and 1 digit industry codes. Rows 5-6 report the Oaxaca decomposition terms defined in equation (17) of the main text. The term in square bracket in column 3 represent the percentage of the raw gap that each component explains relative to the raw wage gap reported in Row 1, Column 3. Estimates based on workers that start with a temporary contract and eventually transition to a permanent contract within the same employer and we have a measure of value added for such employers. Number of person year observations is 3,759,564. Standard errors, clustered at the firm level, reported in round brackets. Source: INPS-INVIND-CNEL merged with CERVED.

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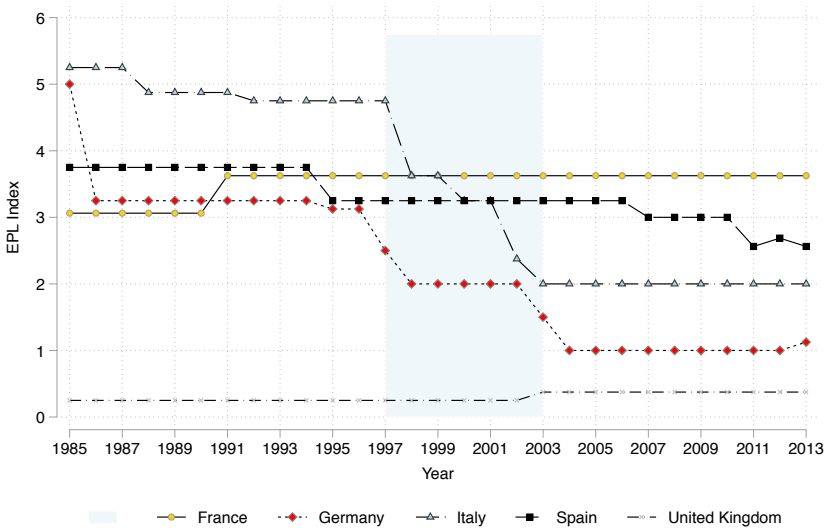
Appendix A

Additional Descriptive Evidence

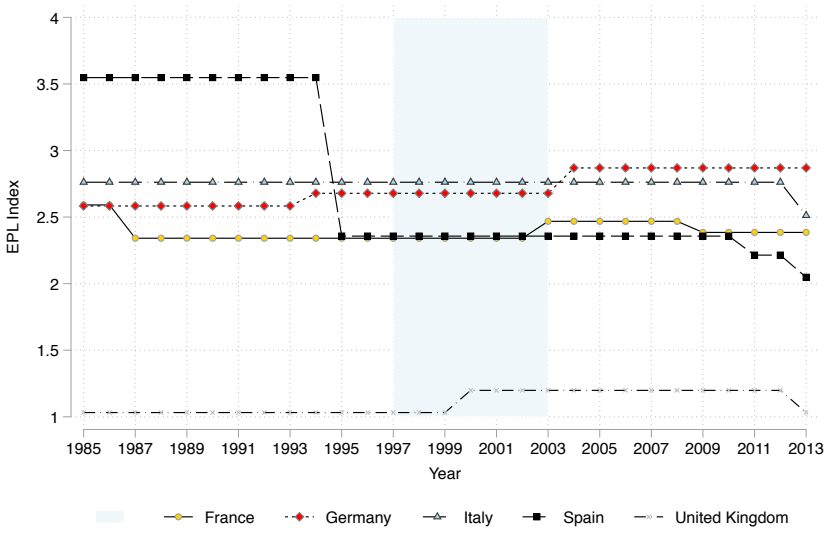
A.1 Figures

Figure A.1.1: Employment Protection Legislation

(a) Temporary Contracts

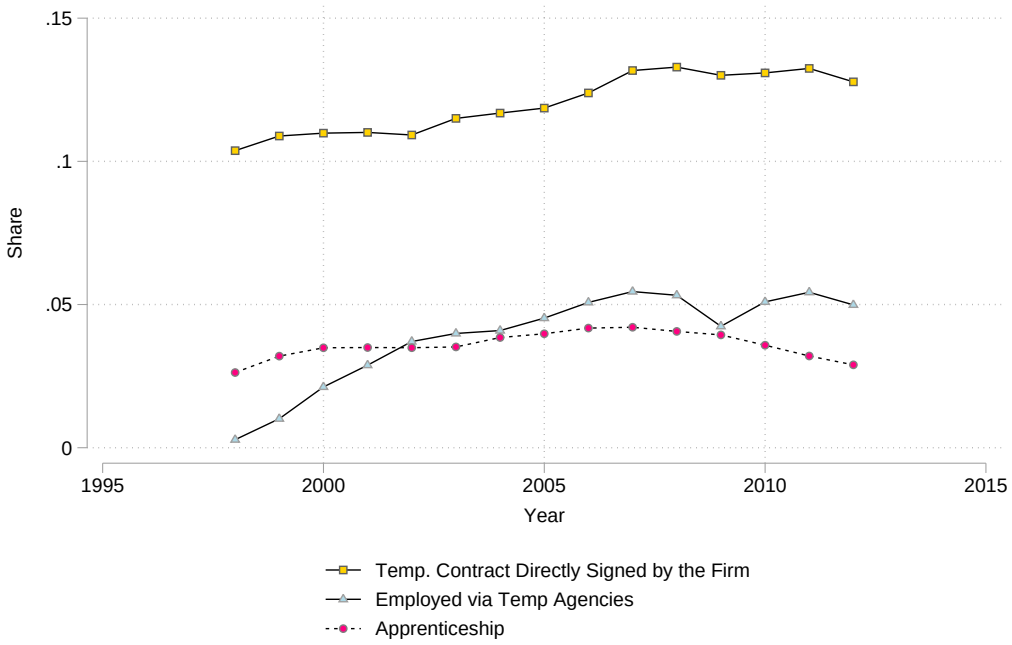


(b) Permanent Contracts



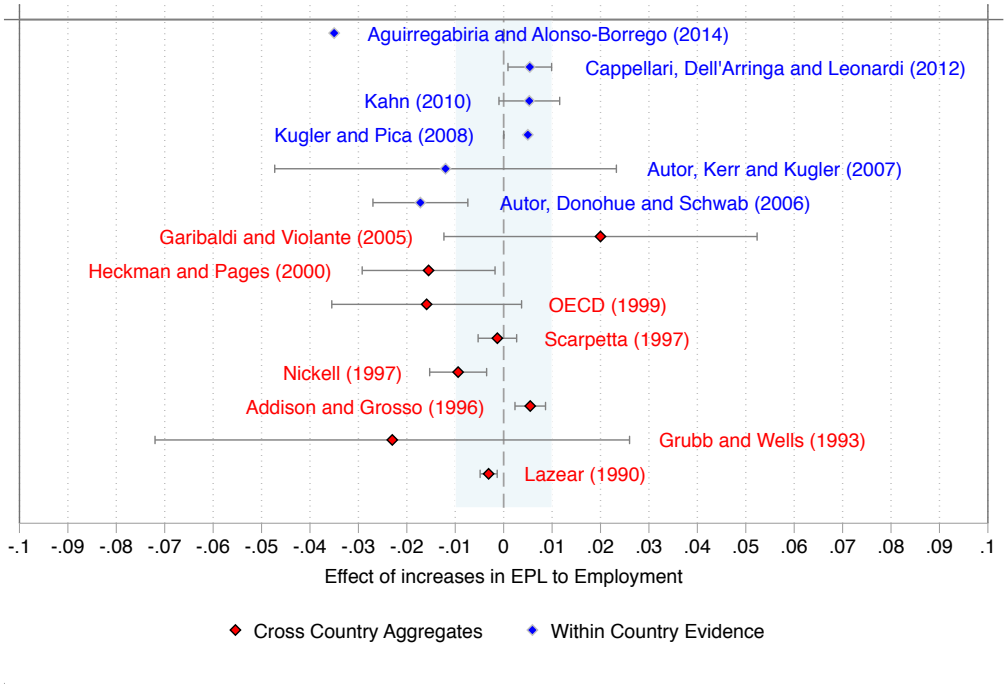
Note: The figure plots an index of the employment protection legislation (EPL) surrounding temporary and permanent contracts across countries and years. Panel (a) shows that the reform of 2001 is associated with a large drop in the EPL for temporary contracts (the previous drop in 1997 corresponds to the Treu Package). Panel (b), on the other hand, shows that the EPL of permanent workers in Italy is relatively high and remained unchanged for almost 30 years.

Figure A.1.2: Types of Temporary Contracts



Note: This figure plots the shares of the three type of temporary contracts: apprenticeships, employed via a temporary agency, temporary work employed directly by the firm (including collaborators). We use industry codes (in particular ATECO codes in combination with *codice statistici contributivi*, as described by INPS) to identify firms temporary work agencies. Source: INPS-INVIND-CNEL.

Figure A.1.3: Increases to EPL and their impact on Employment



Note: Figures shows point estimate and 95% confidence interval of the studies shown in the graph. See Table B.5. for details on the measures used to proxy EPL by each study and additional details concerning estimation of the effects plotted in the figure.

A.2 Tables

Table B1: Match with CERVED

	Firm Year Panel				
	INPS-INVIND-CNEL		INPS-INVIND-CNEL matched with info on Balance Sheet (CERVED)		Column 3 / Column 1 (x100)
	[1]	[2]	[3]	[4]	[5]
	# of Obs	%	# of Obs	%	
Number of Employees					
<20	4,850,580	87.1	1,551,938	73.1	31.99
20-50	467,150	8.4	359,039	16.9	76.86
50-100	139,594	2.5	118,075	5.6	84.58
100-250	76,781	1.4	65,309	3.1	85.06
>250	36,505	0.7	29,732	1.4	81.45
All	5,570,610		2,124,093		38.13

Note: Column (1) corresponds to the set of firm-year observations available in the INPS-INVIND-CNEL dataset from 1998-2008. Sample in Column [3] represents the set of firms for which we can find a match in the CERVED database using the national tax identifier (available only for Italian limited liability corporations). See text for details.

Table B2: Average number of jobs per year by type of main contract

All Workers				
	1998	**2003**	**2008**	**2013**
Permanent	1.005	1.007	1.010	1.012
Temporary	1.009	1.015	1.028	1.040
Only Full Time Workers				
	1998	**2003**	**2008**	**2013**
Permanent	1.003	1.003	1.004	1.004
Temporary	1.008	1.012	1.019	1.031

Note: This table reports the average number of jobs hold in a given month by workers employed either under a temporary or a permanent contract. To calculate the number of jobs, we use the original job spell level data provided by INPS to construct a person-month panel where we exploit the fact that INPS-INVIND provides detailed information on whether a given employee was employed in a particular month. From there we check how many different jobs a worker employed via a temporary in a given month, excluding months in which the worker transitioned between jobs. A temporary worker is therefore labelled as any worker who in a given month is employed under such contract. All statistics are person-month weighted.

Table B3: Migration between CCNLs

Year	Fraction of Movers
1999	0.0510
2000	0.0562
2001	0.0594
2002	0.0579
2003	0.0564
2004	0.0560
2005	0.0535
2006	0.0579
2007	0.0592
2008	0.0596

Note: This table reports the fraction of employed workers that switched jobs with different CCNLs and year of renewal in a given year. Source: INPS-INVIND-CNEL.

Table B4: Summary Statistics for Analysis at the Firm Level

	Unique Event Year	All matched Firms between INPS-INVIND and CERVED
	[1]	[2]
Fraction on Temporary Contracts	0.1739	0.175
# of Workers	16.6977	22.9787
	(190.7177)	(237.6814)
Age of the Firm	9.9154	10.0265
	(9.7205)	(9.7933)
Age of the Workforce	31.9817	32.0823
	(7.2607)	(7.2041)
Female	0.3396	0.3322
North	0.5931	0.5976
Center	0.2418	0.2389
South	0.1652	0.1635
Manufacturing	0.338	0.3454
Services	0.4299	0.3975
# of Managers	0.1382	0.2606
	(2.1043)	(4.0822)
Value Added per Worker	32.3321	32.3225
	(16.8007)	(16.8075)
Unique Event Year	1	0.8329
# of Firms	417,538	473,777
# of Firm Year Observations	1,797,098	2,124,093

Note: This table provides summary statistics for the firms in INPS-INVIND that are matched to the CERVED database, see text for details. Firms in column 1 are such that all their employees are employed under collective barganing agreements (CCNLs) that had renewed their national contract (and therefore implemented the reform) in the same year. Fraction of temporary contracts represent the fraction of employees that in a given firm and year combination were employed under a temporary contract. # of Workers report the average number of monthly employees of the firm as reported by INPS. Age of the firm represent the years since the year of creation of the firm. Value added per worker is in 1000 of real 1995 Euros. For multi plants firms, geographical location and sector are assigned according to the main plant, as identified by INPS. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND-CNEL. Source: INPS-INVIND-CNEL matched with CERVED.

Table B.5: Empirical Estimates on the impact of EPL on Employment

Paper	Data	Empirical Strategy	Finding
Lazear (1990)	20 OECD Countries, 1956-1984	Pooled OLS, Random effects. Different measures of EPL (number of months in advance to notify layoff vs. severance payments)	Table 3: stricter measures of EPL (via the number of months to notify separation) is associated with reduction in employment.
Grubb and Wells (1993)	11 EU Countries; 1989	OLS, simple cross section data in 1989. Construct own indicator of EPL using various indicators (temporary contracts, dismissal of regular workers, use of temporary work agency)	Figure 1, simple scatter plot using constructed measure of EPL show negative association between EPL and aggregate employment
Scarpetta (1996)	17 OECD Countries; 1983-1993	Random effects and feasible GLS. EPL index via OECD.	Table 1, Column 10: noisy relantionship between EPL and employment while controlling for other labor market institutions
Addison and Gross (1996)	Same as Lazear (1990) but dropped Portugal and added Finland.	Parallels Lazear (1990) but corrects and improves some of the measures used by Lazear (1990) for EPL (and corrections for outcome variables)	Opposite sign found with respect to the impact of Notice on aggregate employment showed in Table 3 of Lazear (1990).
Nickell (1997)	20 OECD Countries, 1983-1988 and 1989-1994.	OECD index to measure EPL. GLS random effects using two waves from the cross cross section.	Table 7: Panel (a), negative impact of EPL on employment (noisy and negative estimate on unemployment)
OECD (1999)	19 OECD Countries, 1985-1990, 1992-1997.	Random Effects, GSL. Also estimates on changes. Several additional controls of labor market institutions (e.g. degree of centralization of collective bargaining agreements)	Negative estimates of EPL on employment but statistically insignicant, see Table 2.10
Heckman and Pages (2000)	43 countries from Latin america and Caribbean, 1980-1997.	Fixed effects method. Used their own cardinal measure of employment protection, based on severance pay, notice interval, etc.	Table 6.c negative but imprecise estimate of EPL on employment, results vary by methodology (POLS vs. Random Effects)
Hunt (2000)	German detailed manufacturing industries 1977-1992.	Reform of 1985 that facilitated the creation of temporary employment contracts. Random effects model for coefficients on several variables (e.g. sales in t-1) interacted with treatment	Table 1: short term contracts did not affect employment adjustments.
Besley and Burgess (2004)	India: 1958-1992; state level data on sectoral output, employment, etc.	Evaluates within state "pro-labor" changes to industrial relations.	Pro workers regulations are associated with a 7 percent decrease in manufacturing employment (Table 5)
Kugler (2004)	Colombia: National household survey for June of 1988, 1992, and 1996.	1990 Colombian reform that reduced severance payments, widened the definition of "just" dismissals, extended the use of temporary contracts. Difference in Differences Framework	Use steady state calculations to infer impact on unemployment: reform reduced unemployment by 0.15 percentage point
Garibaldi and Violante (2005)	1960-2000; 17 OECD countries, see also Belot and van Ours (2004)	Controls for country Fixed effects using EPL constructed from Belot and van Ours (2004) - range from 0-3. Additional institutional variables.	Table 3: positive but imprecise effect of EPL on employment.
Autor, Donohue and Schwab (2006)	US Data from CPS 1978-1999.	Variation in extent and adoption of employment protection measures - wrongful discharge laws - across US states. Difference in Differences framework	Table 1: adoption of the implied-contract exception is associated with a negative impact on employment (other laws deliver mostly imprecise estimates)
Autor, Kerr and Kugler (2007)	US Firm Level data: Longitudinal Business Data (1976-1999) +	Variation in extent and adoption of employment protection measures - wrongful discharge laws - across US states. Difference in Differences	Table 5: adoption of the implied-contract exception is associated with a negative impact but imprecise effect on employment

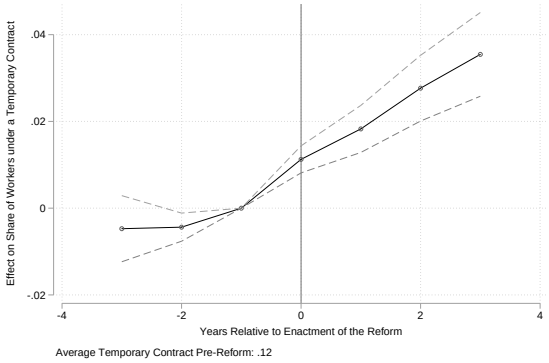
	Annual Survey of	framework	using ASM (other laws deliver
Kugler and Pica (2008)	Random sample (1/90) from Italial social security data 1986-1995.	1990 Italian reform that increased unjust dismissal costs for businesses below 15 employee. Difference in Differences framework.	Table 3: reform decreased both accessions and separations. Using steady state condition, little or no change to employment (<0.5 percentage point in employment)
Cappellari, Dell'Arringa and Leonardi (2012)	Survey Italian Firm Level data: 2004-2007	Variation in adoption of a law that facilitate the creation of temporary contracts (interpreted as decrease in EPL)	Table 5: negative effect of the reform on total employment, controlling for firm fixed effects.
Aguirregabiria and Alonso-Borrego (2014)	Spanish Firm Level data from 1982-1993.	Exploit 1984 reform that introduced fixed term contracts in Spain using structural model.	Table 6: estimates from the structural model suggests a positive effect of the reform on employment rate.

Appendix B

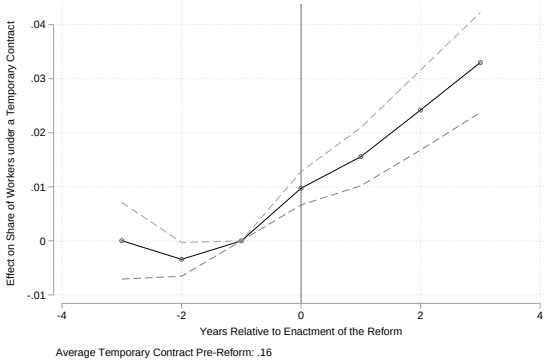
Additional Empirical Evidence

B.1 Figures

Figure B.1.1: Robustness of Effect of the Reform on Share of Temporary Contracts

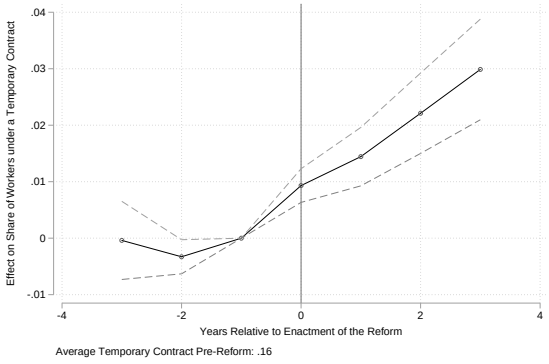


(a) Share on temp contract signed directly by firm

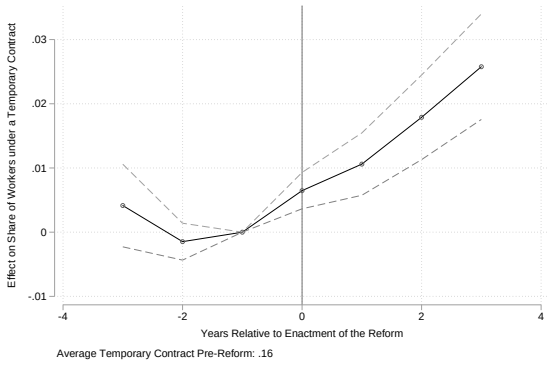


(b) No Late Agreements

(c) No specified agreement



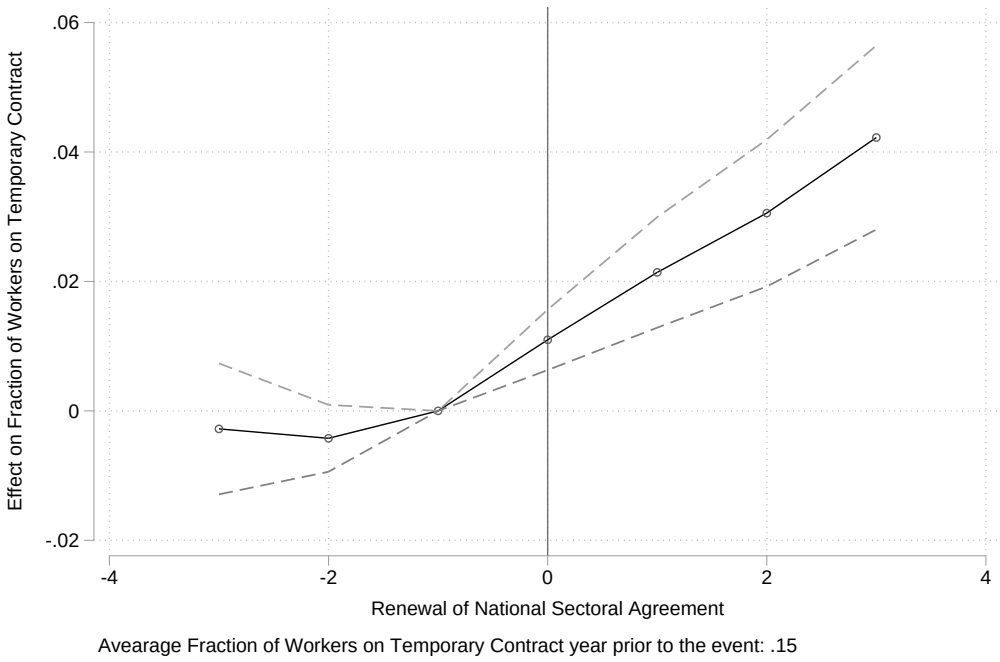
(d) No Managers



(e) No Movers

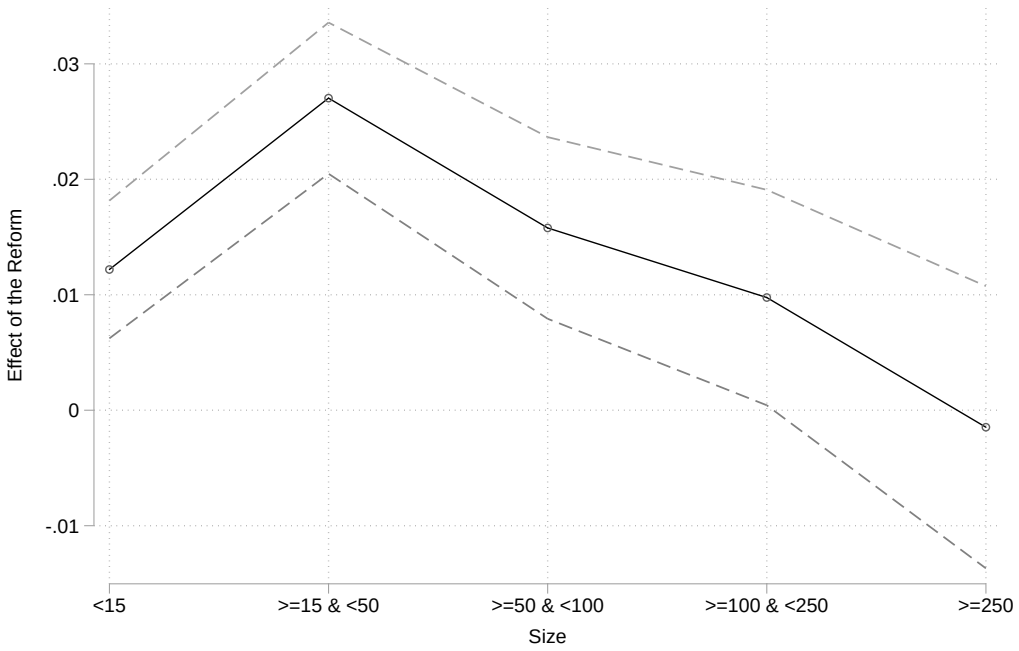
Note: Note: All Panels displays event study coefficients from equation (1.6) with the exception of Panel (a) which plots the event study coefficients when the dependent variable represents the share of jobs under a temporary contract signed directly by the firm. Panel (b) excludes CCNLs that were signed more than 3 years after the previous agreement. Panel (c) excludes CCNLs for which CNEL does not assign a specific CCNL (such as temporary work agencies). Panel (d) excludes CCNLs of managers - which in rare occasions might be employed with a temporary contract - from estimation. Panel (e) excludes workers that moved between CCNLs and year of renewals when computing aggregates in a given cell. 95% confidence intervals obtained after clustering the standard errors at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Figure B.1.2: Event Study Estimates at the Firm Level



Note: This figure presents firm level event study estimates where the dependent variable is the share of workers employed by a firm in a given year under a temporary contract. These estimates are computed on the sample of matched firms between INPS-INVIND and CERVED. The event study specification controls for firm fixed effect and time fixed effects that are province specific. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL matched with CERVED.

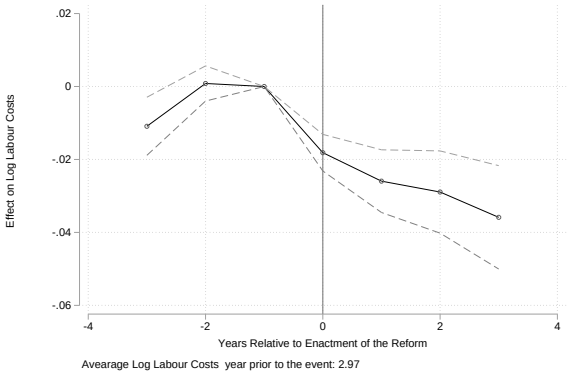
Figure B.1.3: Heterogeneity in Terms of Firm Size in Share of Temporary Workers



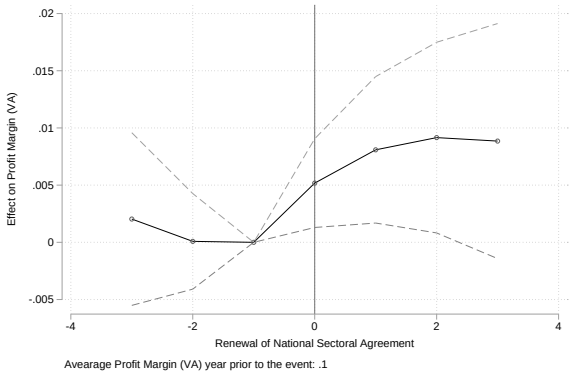
Note: This figure presents firm level difference in difference estimates from model (2.1) splitted by firm size where the dependent variable is the share of workers employed by a firm in a given year that are under a temporary contract. These estimates are computed on the sample of matched firms between INPS-INVIND and CERVED. Estimates computed controlling for firm fixed effect and time fixed effects that are province specific. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL matched with CERVED.

Figure B.1.4: Event Study on Firms' Outcomes

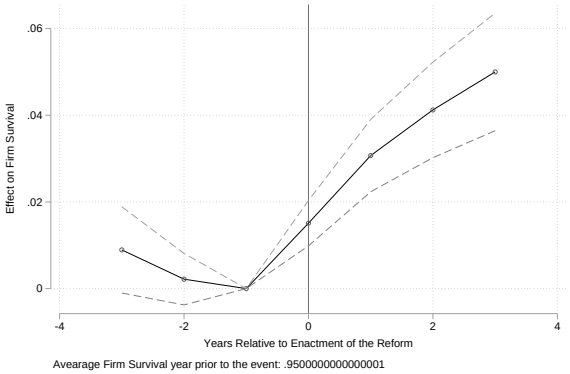
(a) Log Labor Costs per Worker



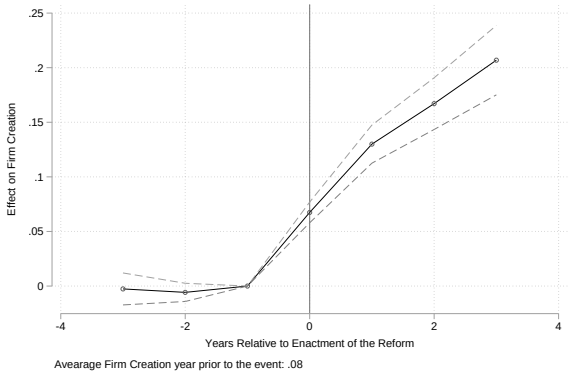
(b) Profit Margin



(c) Firms' Survival

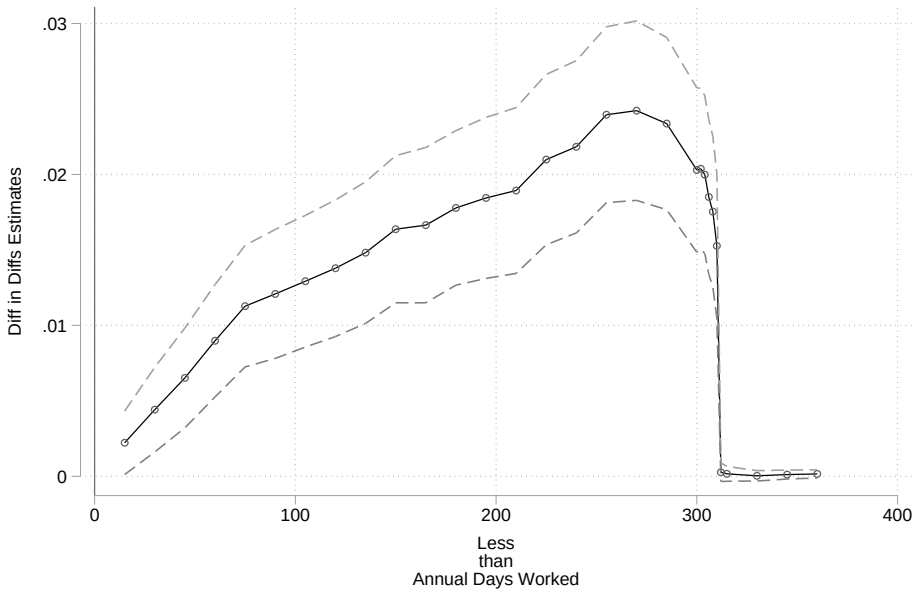


(d) Firms' Creation



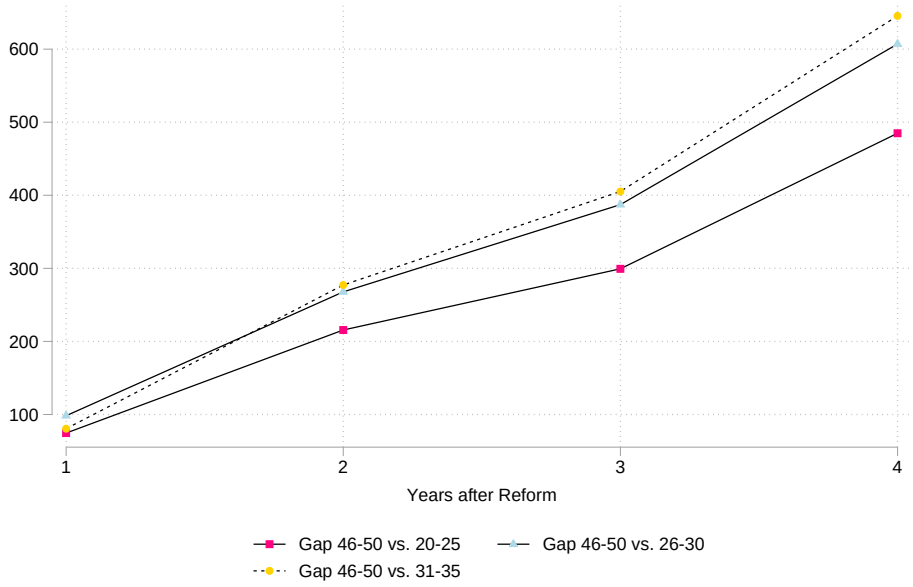
Note: All Panels displays event study coefficients obtained by expanding the original difference-in-differences model of equation (2.1), allowing for a different coefficient each year prior and following enactment of the reform (binning at -3 and 3). Estimates based on the sample of firms associated with a unique CCNL, see text for details. We use sampling weights to produce representative estimates for the corresponding population of firms in INPS-INVIND. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL matched with CERVED.

Figure B.1.5: Effect of the Reform on the Distribution of Days Worked



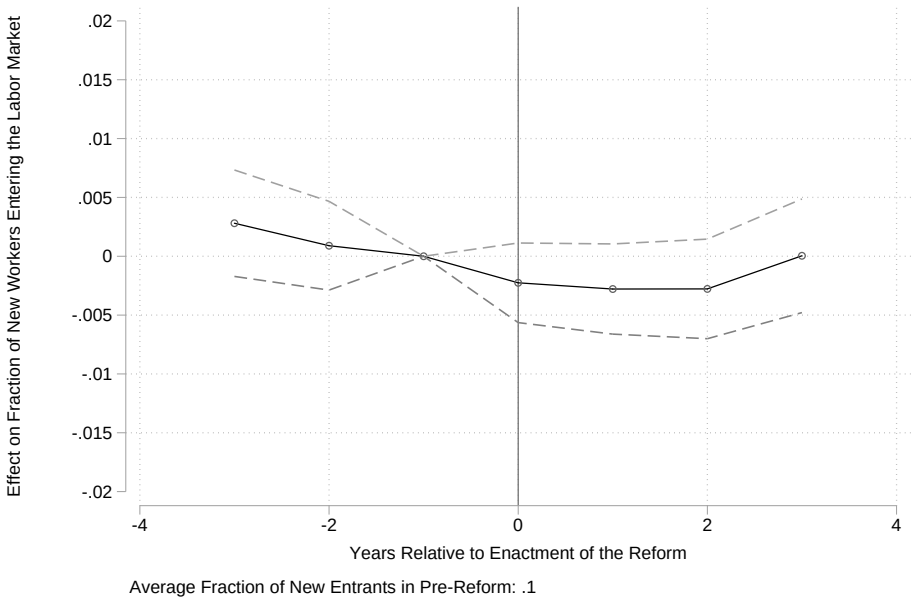
Note: This figure reports difference-in-differences estimates based from equation (1.7) collapsing the event dummies into a pre-post reform indicator. The dependent variable is represented by an indicator $\mathbf{1}\{Days_{it} \leq x\}$ where $Days_{it}$ is the total days worked by worker i in period t and x is a threshold value which is reported in the X-axis of the above graph. Each point in the graph therefore represents the difference-in-differences coefficient (and associated standard error) associated with increases in the share of temporary workers working less than x days in a given year. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Figure B.1.6: Redistribution from Young to Older Workers



Note: This figure provides evidence on how the reform affects the gap in earnings between older and younger workers. In particular, the graph shows coefficients obtained after interacting reform event dummies with age groups when the outcome variable is represented by annual earnings. Therefore, each point in the graph measures changes to the gap in earnings between age groups relative to the year prior to implementation of the reform. The baseline regression model controls for binned age groups (grouped in 5 years interval from 20 to 60 years old) fixed effects, gender, nationality, baseline event dummies, Province by year and CCNL fixed effects. Pre-reform gaps across age groups are 12820 euros (46-50 vs. 20-25) 8939 euros (46-50 vs. 26-30) and 5800 euros (46-50 vs. 31-35). Standard errors not reported in order to improve readability of the graph.

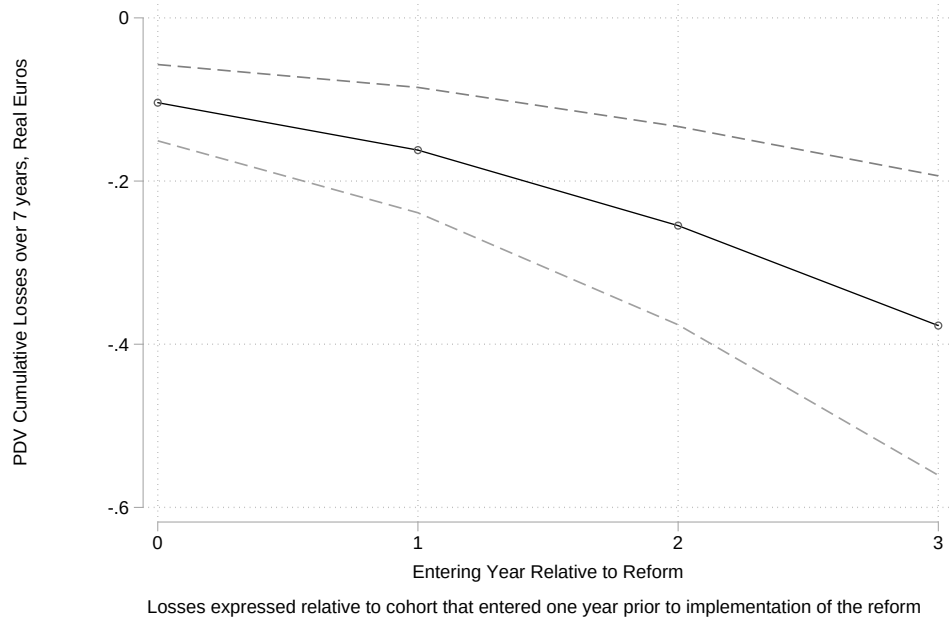
Figure B.1.7: Share of New Entrants and the Reform



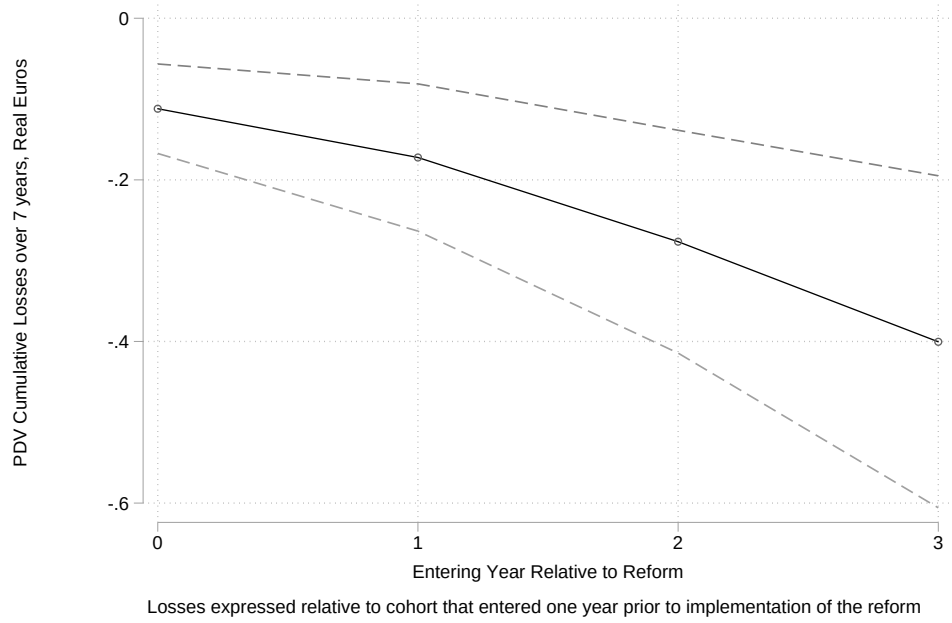
Note: This graph plots event study coefficients when the dependent variable is represented by the share of new entrants, expressed as a fraction with respect to overall employment, in a given CCNL by province by year cell. The event study regression model controls for province by year and CCNL fixed effects. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL

Figure B.1.8: Cumulative Losses: New Entrants

(a) Conditioning on Positive Earnings

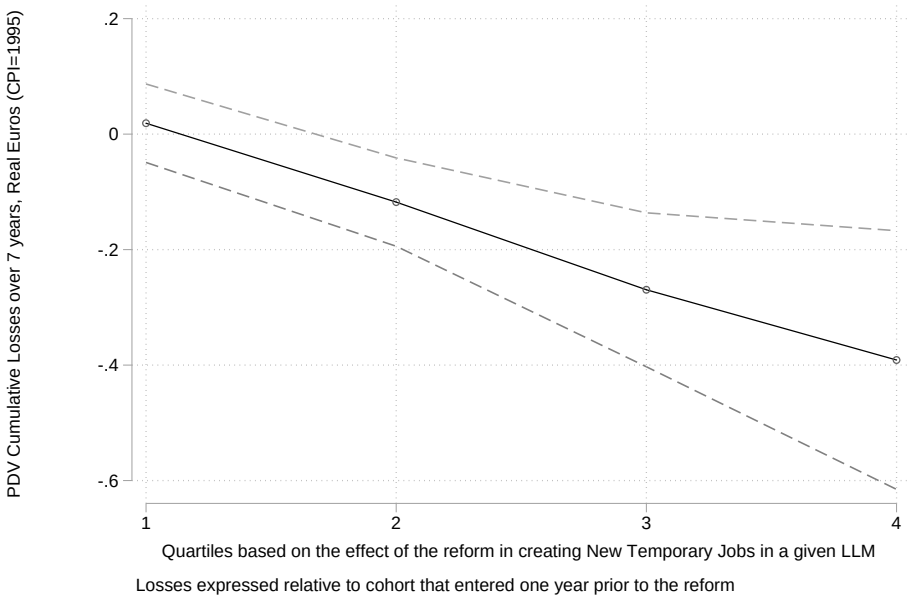


(b) Unconditional



Note: The figure shows present discounted value losses in terms of annual earnings for cohorts that entered the labor market $k \geq 0$ years following implementation of the reform using the estimated δ_k^ℓ from equation (2.5) and shown in Table 7. The cumulative losses are computed as $\sum_{\ell=1}^{7} \frac{\delta_k^\ell}{\bar{Y}_{-1}^\ell}(1+\tilde{\beta})^{1-\ell}$ where $\tilde{\beta} = 3\%$ and $\bar{Y}_{-1}^\ell$ is average earnings for the cohort that entered the labor market one year prior to enactment of the reform. Estimated δ_k^ℓ are computed conditioning on positive earnings in panel (a). Unconditional estimates are reported in Panel (b). Standard errors are computed via block bootstrap. Source: INPS-INVIND-CNEL.

Figure B.1.9: Cumulative Losses by LLMs



Note: This graph plots the PDV values losses computed by running (2.4) where the treatment indicator R_i is interacted with quartiles indicators. These quartiles indicators are defined based on the effect of the reform in creating new temporary jobs within the LLM of entry of a given worker. Each point in the graph shows the estimated PDV losses in conditional earnings for workers entering in one of the four possible type of LLM. PDV value calculations use an interest rate of 3% and are benchmarked relative to the average earnings of the cohort that entered one year prior to liberalization. The estimated model controls for entry LLM fixed effects, year of entry fixed effects interacted with worker type defined in terms of gender, nationality and age at entry. Reported standard errors and 95% confidence intervals are computed via block bootstrap.

B.2 Tables

Table C1: Effect of the Reform on the Contract Gap in Labor Market Outcomes

	Gap in Annual [1]	Gap in Annual Days [2]	Gap in Log Daily [3]
Panel (a): All Workers			
Enactment Year	276.9226***	6.2470***	0.0037***
	(26.2454)	(0.4330)	(0.0014)
3 Years After Reform	908.1303***	10.5004***	0.0176***
	(48.1505)	(0.8049)	(0.0022)
# of Observations	49,919,394	49,919,394	49,919,394
Gap in Pre-Reform Era	9475.26	79.93	.3
Panel (b): Excluding New Entrants and Previously Non-Employed			
Enactment Year	232.9747***	5.0926***	0.0031**
	(29.7759)	(0.4461)	(0.0014)
3 Years After Reform	831.9517***	8.9520***	0.0153***
	(44.8595)	(0.7316)	(0.0025)
# of Observations	42,559,502	42,559,502	42,559,502
Gap in Pre-Reform Era	10025.73	77.48	.32
Panel (c): Full Time Workers			
Enactment Year	360.1849***	6.3079***	0.0018
	(32.3498)	(0.4407)	(0.0011)
3 Years After Reform	1134.0119***	8.9317***	0.0162***
	(44.9371)	(0.7447)	(0.0018)
# of Observations	42,559,502	42,559,502	42,559,502
Gap in Pre-Reform Era	10025.73	77.48	.32
Panel (d): At least 5 yrs of Potential Experience			
Enactment Year	126.5341***	3.5304***	0.0033*
	(40.1014)	(0.4629)	(0.0019)
3 Years After Reform	501.6263***	3.9248***	0.0228***
	(56.0271)	(0.8477)	(0.0029)
# of Observations	40,468,047	40,468,047	40,468,047
Gap in Pre-Reform Era	9692.72	80.42	.28
Panel (e): New Entrants (<=30 Years Old at time of entry)			
Enactment Year	184.9483***	5.1316***	-0.0001
	(53.9651)	(1.1295)	(0.0051)
3 Years After Reform	335.3416***	7.6753***	0.0022
	(65.3314)	(1.3538)	(0.0050)
# of Observations	1,390,921	1,390,921	1,390,921
Gap in Pre-Reform Era	526.48	4.67	0.0019

Note: This table shows estimates from the contract gap event study specification as defined in equation (12), see text for details. Each column lists a different outcome for the model being estimated in equation (12) Gap in pre-reform era measures the raw gap in the outcome listed in each column in the years 1998-2001. Panel (b) excludes jobs associated to new entrants or taken by individuals that resulted non-employed in the pre-reform era. Panel (e) focus on workers entering the labor market for the first time while being also less than 30 years old. All regressions control for uninteracted event study coefficients, a dummy for the job on a permanent contract, province by year and CCNL fixed effects as well as workers' characteristics such as gender, nationality of birth (Italian vs. non-Italian), year of birth dummies and a quadratic in potential experience, see text for further details. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Table C2: Effect of the Reform on Job Flows (Young Workers)

	Same Employer		New Employer		
	Temporary [1]	Permanent [2]	Non - Employed [3]	Temporary [4]	Permanent [5]
[1] **Transition Rates: Temporary Jobs**					
Post-Reform	-0.0110***	-0.0107***	0.0102***	0.0091***	0.0025*
	(0.0028)	(0.0025)	(0.0020)	(0.0022)	(0.0015)
# of Job-Year Observations	8,014,184	8,014,184	8,014,184	8,014,184	8,014,184
Pre-Reform Mean	.372	.212	.109	.171	.135
[2] **Transition Rates: Permanent Jobs**					
Post-Reform	-0.0002	-0.0014	-0.0020*	-0.0003	0.0039
	(0.0003)	(0.0033)	(0.0011)	(0.0008)	(0.0031)
# of Job-Year Observations	13,907,849	13,907,849	13,907,849	13,907,849	13,907,849
Pre-Reform Mean	.008	.725	.065	.06	.142
[3] **Exit Rates from Non-Employment**					
Post-Reform				0.0072***	-0.0026*
				(0.0009)	(0.0015)
# of Job-Year Observations				21,922,033	21,922,033
Pre-Reform Mean				.096	.086

Note: Row 1 of this table presents estimates of the difference in differences coefficient π from equation (9) on an indicator corresponding to the state listed in each column of the Table. For instance, when looking at the column 1, row 1, the post reform coefficient captures the changes induced by the reform in observing a temporary job in year t being converted into a permanent job in year t+1 by the same employer of year t. Similarly for estimates in row 2 which capture effect of the reform on the transition rates associated to permanent jobs. Estimates in row 3 are based from equation (11). Here the post reform coefficient show the impact of the reform in creating new jobs under either a temporary (Column 4) or a permanent contract (Column 5) and the associated job is filled by a worker that was non-employed in the previous year, see text for details. Pre-reform mean reports the share associated to a given transition in the pre-reform era (1998-2001) while for Row 3 it reports the average exit rates from non-employment via either a temporary or a permanent contract in the pre-reform era. All the reported estimates focus on jobs that represent the highest paid occupation in a given worker-year cell and the associated worker is less than 30 years old. Estimates of the reform computed after controlling for province by year and CCNL fixed effects as well as worker level controls such as gender, nationality of birth (Italian vs. non-Italian), year of birth dummies and a quadratic in potential experience. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Table C3: Predetermined Characteristics and Entering Year Relative to Enactment of the Reform

	Pre Reform Average	Difference in Difference Coefficient	P-value
	[1]	[2]	[3]
Female	0.3853	-0.0042	0.446
		(0.0055)	
Foreign	0.1041	0.0073	0.0294
		(0.0033)	
Age at entry	22.8185	0.0626	0.2477
		(0.0539)	
South	0.1762	-0.0005	0.9253
		(0.0051)	
Rome	0.0585	0.0014	0.584
		(0.0026)	
Milan	0.0691	0.002	0.4562
		(0.0027)	
Commuter	0.3588	0.0026	0.4715
		(0.0036)	
Total Jobs in the year	1.359	0.0190	0.2323
		(0.0158)	
Welfare	0.0122	0.0001	0.8777
		(0.0008)	
Predicted Wage	3.67	-0.0028	0.56
		(0.0049)	
Quality of the Job	-0.0121	-0.0022	0.633
		(0.0047)	

Note: This table compares predetermined characteristics of new entrants that entered the labor market between 1998-2008 and that were less than 30 years old at the time of entry. Column 1 reports the average corresponding characteristic for workers that entered before the policy change took place. Column 2 reports the difference in difference regression coefficient associated to the reform. The depedent variable of this regression corresponds to the characteristic listed in each row and the regression control for year and CCNL fixed effects. The associated standard error is clustered at CCNL x Province level and the pvalue is reported in Column 3. Foreign is a dummy equal to 1 if the worker is born outside of Italy. South is a dummy equal to 1 if the worker lives in the South of Italy. Similarly, for Milan and Rome. Commuter is a dummy equal to 1 if the province of residence of the worker does not coincide with the province of the establishment that employed the worker in her first job. Predicted Wage is an index constructed as follows: using only pre-reform data, we regress average log daily wages in the first seven years of potential labor market experience on unrestricted interactions of Age at Entry, Gender, Foreign, Province of Residence at entry as well as unrestricted interaction between work status and 5 digit industry dummies at the time of entry. The latter estimated fixed effects represent what we define as "Quality of the Job" while the overall predicted coefficients from the aforementioned regression represent the overall "Predicted Wage". Source: INPS-INVIND-CNEL.

Table C4: Effect of the Reform on Job Flows (Incumbent Temporary Workers)

	Same Employer		New Employer		
	Temporary [1]	Permanent [2]	Non - Employed [3]	Temporary [4]	Permanent [5]
[1] **Transition Rates: Temporary Jobs**					
Post-Reform	-0.0068	-0.0519***	0.0089***	0.0345***	0.0153***
	(0.0059)	(0.0063)	(0.0024)	(0.0048)	(0.0027)
# of Job-Year Observations	801,139	801,139	801,139	801,139	801,139
Pre-Reform Mean	.408	.211	.05	.185	.146
[2] **Transition Rates: Permanent Jobs**					
Post-Reform	0.0014	-0.0072	0.0028*	-0.0015	0.0044
	(0.0009)	(0.0047)	(0.0017)	(0.0025)	(0.0033)
# of Job-Year Observations	1,229,758	1,229,758	1,229,758	1,229,758	1,229,758
Pre-Reform Mean	.004	.826	.014	.03	.126
[3] **Exit Rates from Non-Employment**					
Post-Reform				.0142***	-.0019
				(0.0009)	(0.0015)
# of Job-Year Observations				2,030,897	2,030,897
Pre-Reform Mean				.096	.086

Note: Row 1 of this table presents estimates of the difference in differences coefficient π from equation (9) on an indicator corresponding to the state listed in each column of the Table. For instance, when looking at the column 1, row 1, the post reform coefficient captures the changes induced by the reform in observing a temporary job in year t being converted into a permanent job in year t+1 by the same employer of year t. Similarly for estimates in row 2 which capture effect of the reform on the transition rates associated to permanent jobs. Estimates in row 3 are based from equation (11). Here the post reform coefficient show the impact of the reform in creating new jobs under either a temporary (Column 4) or a permanent contract (Column 5) and the associated job is filled by a worker that was non-employed in the previous year, see text for details. All the reported estimates focus on jobs that represent the highest paid occupation in a given worker-year cell and the associated worker represents an Incumbent temporary worker, meaning that the worker was always employed under a temporary contract in the pre-reform era (1998-2001). Pre-reform mean reports the share associated to a given transition in the pre-reform era for all incumbent workers while for Row 3 it reports the average exit rates from non-employment via either a temporary or a permanent contract in the pre-reform era, again for the population of incumbent temporary workers, see Table 6. Estimates of the reform computed after controlling for province by year and CCNL fixed effects as well as worker level controls such as gender, nationality of birth (Italian vs. non-Italian), year of birth dummies and a quadratic in potential experience. Standard errors are clustered at the CCNL x Province level. Source: INPS-INVIND-CNEL.

Table C7: Dynamic Effect of The Reform for New Entrants

	Year Following Entry in the Labor Market						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A: Hired as Temporary Directly by the Firm							
Entered 1st Year of Enactment of the Reform	0.0306***	0.0201***	0.0127***	0.0083***	0.0073***	0.0026	0.0015
	(0.0029)	(0.0031)	(0.0027)	(0.0026)	(0.0024)	(0.0022)	(0.0020)
Entered in the 2nd Year of Enactment	0.0345***	0.0253***	0.0209***	0.0120***	0.0074**	0.0037	0.0029
	(0.0043)	(0.0046)	(0.0033)	(0.0034)	(0.0030)	(0.0029)	(0.0027)
Entered in the 3rd Year of Enactment	0.0455***	0.0291***	0.0241***	0.0171***	0.0122***	0.0071**	0.0073**
	(0.0060)	(0.0058)	(0.0041)	(0.0043)	(0.0037)	(0.0035)	(0.0033)
Entered in the 4th or later Year of Enactment	0.0687***	0.0486***	0.0385***	0.0280***	0.0190***	0.0100**	0.0093**
	(0.0081)	(0.0076)	(0.0049)	(0.0050)	(0.0045)	(0.0043)	(0.0042)
of Temp hired directly - Entered 1 year prior to Reform	.461	.387	.301	.246	.225	.218	.199
Number of Observations	1,523,145	1,325,870	1,264,021	1,246,423	1,235,563	1,223,544	1,138,362

Note: This table presents estimates from equation (16) where the dependent variable is an indicator equal to one if the worker is employed in a given year via a temporary contract signed directly by the firm (excluding therefore temp jobs found via temporary work agency, internships and some specific types of external collaborations akin to consulting) and the sample corresponds on all new entrants observed in the INPS-INVIND data between 1998-2008 that were less than 30 years old at the time of entry. Each row reports the coefficients δ from equation (16) for cohorts that entered in the post-reform labor market separately for each year following entry where the first column corresponds to the entry year in the labor market. These coefficients are expressed relative to the cohort that entered one year prior to implementation of the reform. Estimation on workers that have positive earnings in a given year. Each estimate is computed after controlling for CCNL of entry fixed effects, year of entry fixed effects interacted with worker type defined in terms of gender, nationality, age at entry and province. Standard errors are clustered at the entry CCNL x Province level. Source: INPS-INVIND-CNEL.

Appendix C

Model Derivation

C.1 Model Derivations

In this Appendix we provide further details concerning the conceptual framework analyzed in Section 1.3 and based on Cahuc et al. (2016).

C.1.1 Choice Rule

The surplus associated to creating a permanent job with expected duration $1/\lambda$ is given by

$$S_P(\lambda) = \Pi_P(\lambda) + V_P(\lambda) - U \tag{C.1}$$

where Π_P represents the value for a firm of creating a permanent job and V_P is the value to the worker. In particular, we have that

$$\Pi_P(\lambda) = \int_0^\infty \left[\int_0^\tau [y - w(\lambda)]e^{-rt}dt - fe^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau - c_P, \tag{C.2}$$

where $\int_0^\tau [y - w(\lambda)]e^{-rt}dt$ represents the discounted flow of profits (production minus the wage, $w(\lambda)$) up to random date τ, when the job no longer becomes productive and firms have to pay the firing cost f. Given the Poisson distributional assumption, Pr(Job remains productive until τ)=$e^{-\lambda\tau}$, hence the usage of the exponential distribution inside the outer integral. Similarly, we have that

$$V_P(\lambda) = \int_0^\infty \left[\int_0^\tau w(\lambda)e^{-rt}dt + Ue^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau. \tag{C.3}$$

We can arrange $S_P(\lambda)$ as follows

$$S_P(\lambda) = \frac{y - rU - \lambda f}{r + \lambda} - c_P. \tag{C.4}$$

The surplus of creating a temporary contract of duration D and type λ is given by

$$S_T(\lambda, D) = \Pi_T(\lambda, D) + V_T(\lambda, D) - U \tag{C.5}$$

where $\Pi_T(\lambda, D)$ represents the value for a firm of creating a temporary job, $V_P(\lambda, D)$ is the value to the worker and U is the value of unemployment. In particular, we have that

$$\Pi_T(\lambda, D) = \int_0^D [ye^{-\lambda\tau} - w(\lambda, D)]e^{-r\tau}d\tau + \{\max[\Pi_P(\lambda), 0]e^{-D\lambda}\}e^{-Dr} - c_T.$$

Temporary contracts have to pay the wage $w(\lambda, D)$ up to the termination date D. However, production may drop to zero before D, which is why y is multiplied by the survival function while the wage term is only properly discounted. Provided that production survives up to date D, firms have the option to convert the

contract into permanent and obtain profits Π_P or destroy the match (in which case they obtain 0). Similarly, for the worker

$$V_T(\lambda, D) = \int_0^D w(\lambda, D)e^{-r\tau}d\tau + \{\max[V_P(\lambda), U]e^{-D\lambda}\}e^{-rD} + U(1 - e^{-\lambda D})e^{-rD}. \tag{C.6}$$

We can rearrange $S_T(\lambda, D)$ as follows

$$S_T(\lambda, D) = \int_0^D [ye^{-\lambda\tau} - rU]e^{-r\tau} + \max\{S_P(\lambda), 0\}e^{-D(r+\lambda)} - c_T \tag{C.7}$$

The last two equations take D as given. The optimal duration of temporary contracts highlighted in equation (1.3) is obtained by taking the first order conditions from (C.7), for a given λ, that is

$$ye^{-\lambda D} - rU - (r + \lambda)e^{-\lambda D}\max\{S_P(\lambda), 0\} = 0 \tag{C.8}$$

It can be shown that the second order condition is satisfied and from (C.8) we obtain the optimal duration of temporary contracts, $D^*(\lambda)$, as displayed in equation (1.3), and the surplus of a temporary job is therefore given by $S_T(\lambda) = S_T(\lambda, D^*(\lambda))$. The function $D^*(\lambda)$ is continuos and increasing in the expected duration, $\frac{1}{\lambda}$ but decreasing in the value of unemployment, rU.

Proposition 1: As noted by Cahuc et al. (2016), the model allows for three types of equilibrium (i) only temporary jobs are created (which would occur c_P=0) (ii) only permanent jobs are created (which happens when $S_T(\lambda_P) < 0$ (iii) both temporary and permanent jobs are created (which happens when $S_T(\lambda_P) > 0$). Proposition 1 corresponds to the latter case (see Proposition 1, case 3 of Cahuc et al., 2016). The proof for existence of such a choice rule is contained in Section A.2.4 of Cahuc et al. (2016).

C.1.2 Labor Market Equilibrium

We now provide further details necessary to properly define the labor market equilibrium highlighted in Proposition 2. Recall that firms obtain a fixed share $1-\gamma$ of the surplus generated from the job and workers the remaining share γ. The flow cost of keeping a vacancy open is κ. This implies that the value of keeping a vacancy open is given by

$$r\Pi_v = -\kappa + q(\theta)(1 - \gamma)\int_{\underline{\lambda}}^{\lambda_T} S(\lambda)dG(\lambda) \tag{C.9}$$

where $S(\lambda)$ denotes the surplus generated from a newly filled job of type λ. Free entry in the search market implies $\Pi_v = 0$ from which one obtains equation (1.4) in the main text.

Proceeding similarly, the value of unemployment, assuming a flow utility of being out of employment equal to z, is given by

$$rU = z + \theta q(\theta)\gamma \int_{\underline{\lambda}}^{\lambda_T} S(\lambda) dG(\lambda) \tag{C.10}$$

from which one obtains immediately (1.5) using the free entry condition.

C.1.3 Comparative Statics

Before proceeding with the proof of Proposition 3, it is useful to restate the key equations characterizing the labor market equilibrium underlined in Proposition 2

$$h^{EC}(\theta; c_T) \equiv \kappa - q(\theta)(1-\gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda) dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda; c_T) dG(\lambda)\right] = 0 \tag{EC}$$

$$h^{TJCR}(\lambda_T, \theta; c_T) \equiv \frac{y - rU(\theta)}{r + \lambda_T} + \lambda_T \frac{U(\theta)(e^{-rD^*(\lambda_T)} - 1)}{r + \lambda_T} - c_T = 0 \tag{TJCR}$$

$$h^{TJDR}(\lambda_P, \theta) \equiv \lambda_P - \frac{y - rc_P - rU(\theta)}{c_P + f} = 0 \tag{TJDR}$$

$$h^{PJvsTJ}(\lambda_E, \theta; c_T) \equiv \frac{\lambda_E U(\theta)(e^{-rD^*(\lambda_E)} - 1)}{r + \lambda_E} + \frac{\lambda_E f}{\lambda_E + r} + c_P - c_T = 0 \tag{PJvsTJ}$$

where the first equation, (EC) identifies the Entry Condition from which one obtains the equilibrium value of θ. The second equation characterizes the Temporary Job Creation Rule (TJCR), which recall is pinned down by the job type, λ_T, that makes the surplus of a temporary job equal to 0. This equation is obtained after replacing in (C.7) the FOC obtained from (C.8), i.e, $ye^{-\lambda D} - rU = (r + \lambda)e^{-\lambda D} \max\{S_P(\lambda), 0\}$. The same substitution is used for the rule characterizing the threshold, λ_E, above (below) which temporary (permanent) jobs will be created, see equation (PJvsTJ). Finally, we have the expression that rules the Temporary Job Destruction Rule (TJDR).

Using (1.5) to substitute for the value of unemployment, one obtains a system of four equations and four unknowns $(\lambda_E, \lambda_T, \lambda_P, \theta)$. For the comparative static analysis that is going to follow, it is important to notice that $U(\theta)$ is an increasing function of θ, i.e, a tighter labor market yields better employment opportunities for the unemployed. This conversely makes (λ_T, λ_P) decreasing functions of θ since better outside options make both firms and workers more demanding on how durable a given match has to be. On the other hand, λ_E is an increasing function of θ.

Proof of Proposition 3: We prove each statement listed in Proposition 3 separately. All the following derivations are computed at the labor market equilibrium defined in Proposition 2.

1. *Increase in the number of jobs*: Here we analyze how λ_T moves in response to a marginal change in c_T. This is done in three separate steps.

   1.1) Begin by noticing that, since $S_T(\lambda_T) = 0$, from the (EC) locus, we have that $\dfrac{\partial\theta}{\partial\lambda_T} = 0$. Therefore, letting $h_x(x,y)$ denote the partial derivative of function $h(x,y)$ with respect to the first argument, we have that

$$\frac{d\lambda_T}{dc_T} = \underbrace{-\frac{h^{TJCR}_{c_T}(\lambda_T,\theta;c_T)}{h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T)}}_{\text{Direct Effect}} + \underbrace{\frac{h^{TJCR}_{\theta}(\lambda_T,\theta;c_T)}{h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T)}\frac{h^{EC}_{c_T}(\theta;c_T)}{h^{EC}_{\theta}(\theta;c_T)}}_{\text{Equilibrium Feedback Effect}} \tag{C.11}$$

   1.2) Notice that

$$\begin{aligned}
h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T) &= y\frac{e^{-(r+\lambda_T)D^*(\lambda_T)}[(r+\lambda_T)D^*(\lambda_T)+1]-1}{(r+\lambda_T)^2} < 0\\
h^{TJCR}_{c_T}(\lambda_T,\theta;c_T) &= -1 < 0\\
h^{TJCR}_{\theta}(\lambda_T,\theta;c_T) &= -\frac{\partial U(\theta)}{\partial\theta}\left[\frac{r+\lambda_T(1-e^{-rD^*(\lambda_T)})}{r+\lambda_T}\right] < 0\\
h^{EC}_{c_T}(\theta,c_T) &= q(\theta)(1-\gamma)[G(\lambda_T)-G(\lambda_E)] > 0\\
h^{EC}_{\theta}(\theta,c_T) &= -(1-\gamma)q'(\theta)\left[\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda)\right] -\\
&\quad - q(\theta)(1-\gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} h^{TJDR}_{\theta}(\lambda,\theta)dG(\lambda) + \int_{\lambda_E}^{\lambda_T} h^{TJCR}_{\theta}(\lambda,\theta;c_T)dG(\lambda)\right] > 0
\end{aligned}$$

   where the first negative sign follows from the envelope theorem and the fact that $e^{-x} < \frac{1}{x+1}$. The third negative sign follows from (C.10). The last positive sign follows from the fact $h^{TJDR}_{\theta}(\lambda,\theta) < 0$ and $q'(\theta)$ is decreasing in θ. Therefore the direct effect is negative (or equivalently a decrease in c_T implies an increase in λ_T) while the feedback equilibrium effect is positive. Since from (EC) we obtain that labor market tightness is inelastic with respect to $(\lambda_E,\lambda_T,\lambda_P)$, we can rearrange (C.11) as

$$\frac{d\lambda_T}{dc_T} = -\frac{h^{TJCR}_{c_T}(\lambda_T,\theta;c_T)}{h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T)} - \frac{h^{TJCR}_{\theta}(\lambda_T,\theta;c_T)}{h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T)}\frac{d\theta}{dc_T} \tag{C.12}$$

   1.3) We now prove that the direct effect dominates the general equilibrium effect, that is

$$-\frac{h^{TJCR}_{c_T}(\lambda_T,\theta;c_T)}{h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T)} - \frac{h^{TJCR}_{\theta}(\lambda_T,\theta;c_T)}{h^{TJCR}_{\lambda_T}(\lambda_T,\theta;c_T)}\frac{d\theta}{dc_T} < 0 \iff 1 > \frac{d\theta}{dc_T}h^{TJCR}_{\theta}(\lambda_T;\theta;c_T). \tag{C.13}$$

We can rearrange the RHS of the last expression as follows

$$\frac{d\theta}{dc_T}h_\theta^{TJCR}(\lambda_T;\theta;c_T) = \frac{-(G(\lambda_T)-G(\lambda_E))h_\theta^{TJCR}(\lambda_T,\theta;c_T)}{-\int_{\underline{\lambda}}^{\lambda_E} h_\theta^{TJDR}(\lambda,\theta)dG(\lambda) - \int_{\lambda_E}^{\lambda_T} h_\theta^{TJCR}(\lambda,\theta;c_T)dG(\lambda) - \frac{q'(\theta)}{q(\theta)}\bar{S}}$$

where $\bar{S} = \int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda)$. Now focus on $-h_\theta^{TJCR}(\lambda;\theta;c_T)$ and notice that

$$-h_{\theta,\lambda}^{TJCR}(\lambda;\theta;c_T) = \frac{\partial U(\theta)}{\partial\theta}\frac{(-re^{-rD^*(\lambda)})}{(r+\lambda)^2} < 0 \tag{C.14}$$

Therefore we have that

$$-h_{\theta,\lambda}^{TJCR}(\lambda_T;\theta;c_T) < \int_{\lambda_E}^{\lambda_T} -\frac{h_\theta^{TJCR}(\lambda,\theta;c_T)dG(\lambda)}{G(\lambda_T)-G(\lambda_E)}dG(\lambda) = E(-h_{\theta,\lambda}^{TJCR}(\lambda;\theta;c_T)|\lambda_E<\lambda<\lambda_T) \tag{C.15}$$

This implies that

$$\begin{aligned}
\frac{d\theta}{dc_T}h_\theta^{TJCR}(\lambda_T;\theta;c_T) &= \frac{-(G(\lambda_T)-G(\lambda_E))h_\theta^{TJCR}(\lambda_T,\theta;c_T)}{-\int_{\underline{\lambda}}^{\lambda_E} h_\theta^{TJDR}(\lambda,\theta)dG(\lambda) - \int_{\lambda_E}^{\lambda_T} h_\theta^{TJCR}(\lambda,\theta;c_T)dG(\lambda) - \frac{q'(\theta)}{q(\theta)}\bar{S}} \\
&< \frac{-(G(\lambda_T)-G(\lambda_E))h_\theta^{TJCR}(\lambda_T,\theta;c_T)}{-\int_{\underline{\lambda}}^{\lambda_E} h_\theta^{TJDR}(\lambda,\theta)dG(\lambda) - (G(\lambda_T)-G(\lambda_E))h_\theta^{TJCR}(\lambda_T,\theta;c_T) - \frac{q'(\theta)}{q(\theta)}\bar{S}} \\
&< 1 \\
&= Q.E.D.
\end{aligned}$$

2. *Increase in temporary job destruction*: We now show that a reduction in c_T implies a decrease in λ_P. In particular, total differentiating the expression for λ_P implied by equation (TJDR), we have that

$$\frac{d\lambda_P}{dc_T} = \frac{-r}{c_p+f}\frac{\partial U(\theta)}{\partial\theta}\frac{d\theta}{dc_T} > 0 \tag{C.16}$$

3. *Decrease in the duration of a temporary job*: This follows recalling the expression for $D^*(\lambda)$

$$D^*(\lambda) = \begin{cases} \frac{1}{\lambda}\log\left(\frac{rU(\theta)+\lambda r+(r+\lambda)c_P}{rU(\theta)}\right) & \text{if } \lambda \le \lambda_P \\ \frac{1}{\lambda}\log\left(\frac{y}{rU(\theta)}\right) & \text{if } \lambda \ge \lambda_P \end{cases} \tag{C.17}$$

which implies that, for a fixed λ,

$$\frac{dD^*(\lambda)}{dc_T} = \begin{cases} -\frac{1}{\lambda}\left(\frac{rU(\theta)}{rU(\theta)+\lambda r+(r+\lambda)c_P}\frac{\partial U(\theta)}{\partial\theta}\right)\frac{d\theta}{dc_T} > 0 & \text{if } \lambda \le \lambda_P \\ -\frac{1}{\lambda}\left(\frac{rU(\theta)}{y}\frac{\partial U(\theta)}{\partial\theta}\right)\frac{d\theta}{dc_T} > 0 & \text{if } \lambda \ge \lambda_P \end{cases} \tag{C.18}$$

4. *Ambiguous effects on* λ_E: We next show what happens in response to the reform to λ_E. As described in the main text, the effect of the reform on this particular thresholds appear ambigous, as the math below and different calibration of the model (not reported) show.

4.1) Since $S_P(\lambda_E) = S_T(\lambda_E)$, we have that from the entry condition (EC) $\dfrac{\partial \theta}{\partial \lambda_E} = 0$. Therefore,

$$\frac{\partial \lambda_E}{\partial c_T} = \underbrace{-\frac{h_{c_T}^{PJvsTJ}(\lambda_E, \theta; c_T)}{h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T)}}_{\text{Direct Effect}} \underbrace{-\frac{h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T)}{h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T)} \frac{d\theta}{dc_T}}_{\text{Equilibrium Feedback Effect}}. \tag{C.19}$$

4.2) We have that

$$\begin{aligned}
h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T) &= \frac{-\lambda_E f r \frac{\partial D^*}{\partial \lambda}(\lambda_E) e^{-rD^*(\lambda_E)}}{(1 - e^{-rD^*(\lambda_E)})(r + \lambda_E)} \geq 0 \\
h_{c_T}^{PJvsTJ}(\lambda_T, \theta; c_T) &= -1 < 0 \\
h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T) &= -\lambda_E \frac{\partial U}{\partial \theta} \frac{(1 - e^{-rD^*(\lambda_E)})}{r + \lambda_E} < 0
\end{aligned}$$

where the first inequality follows from the fact that the duration of a temporary contract is a decreasing function of λ and that λ_E is such that $U = \dfrac{f}{1 - e^{-rD^*(\lambda_E)}}$. Hence the direct effect implies that a *decrease* in c_T is associated with a decrease in λ_E and the equilibrium feedback effect on the other hand implies an increase in λ_E following a decrease in c_T.

4.3) Given the above, the high level condition that ensures that the direct effect dominates the equilibrium feedback effect is given by

$$-\frac{h_{c_T}^{PJvsTJ}(\lambda_E, \theta; c_T)}{h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T)} - \frac{h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T)}{h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T)} \frac{d\theta}{dc_T} > 0 \iff 1 > h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T) \frac{d\theta}{dc_T}$$

In order to derive a more primitive sufficient condition, one can notice the following

$$\begin{aligned}
h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T) &= \frac{[G(\lambda_T) - G(\lambda_E)] \dfrac{\partial U(\theta)}{\partial \theta} \lambda_E \dfrac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_E}}{- \int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda, \theta) dG(\lambda) - \int_{\lambda_E}^{\lambda_T} h_{\theta}^{TJCR}(\lambda, \theta; c_T) dG(\lambda) - \dfrac{q'(\theta)}{q(\theta)} \bar{S}} \\
\text{(see equation (C.15))} \; &< \frac{[G(\lambda_T) - G(\lambda_E)] \dfrac{\partial U(\theta)}{\partial \theta} \lambda_E \dfrac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_E}}{- \int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda, \theta) dG(\lambda) - (G(\lambda_T) - G(\lambda_E)) h_{\theta}^{TJCR}(\lambda_T, \theta; c_T) - \dfrac{\bar{S} q'(\theta)}{q(\theta)}} \\
&< \frac{\lambda_T \dfrac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_T}}{\dfrac{(r + \lambda_T (1 - e^{-rD^*(\lambda_T)})}{r + \lambda_T}}.
\end{aligned}$$

Therefore, given the equilibrium conditions (PJvsTJ) and (TJDR) which implies $U = \frac{f}{1-e^{-rD^*(\lambda_E)}}$9 and $\frac{y-rU(\theta)}{r+\lambda_T} - c_T = \lambda_T \frac{U(\theta)(e^{-rD^*(\lambda_T)}-1)}{r+\lambda_T}$, one sufficient condition that ensures that the inequality above is below 1 is that, in equilibrium, λ_T satisfies

$$\lambda_T < \frac{y - c_T r}{f + c_T}. \tag{C.20}$$

C.1.4 Labor Market Efficiency

To evaluate the effect of the reform on labor market efficiency, we start by computing the total steady state surplus when both temporary and permanent contracts are created. Total surplus is defined as total output produced by filled jobs minus the cost of job vacancies, contracting and firing costs.[1] To construct such measure of *net* output, we partition the analysis according to the relevant ranges of λ where $\lambda \in [\underline{\lambda}, \lambda_T]$.

- $\lambda \in [\underline{\lambda}, \lambda_E]$. In this range permanent jobs are created. The net production provided by jobs for a given $\lambda \in [\underline{\lambda}, \lambda_E]$ is given by

$$Y_P(\lambda, \theta, u, \lambda_T; c_P) = M_P(\lambda)y - u\theta q(\theta)g(\lambda)c_P - \lambda M_P(\lambda)f \tag{C.21}$$

  where $M_P(\lambda)$ is the mass of permanent jobs which is pin down by the steady state equation that equates the entries and exits for these type of jobs

$$u\theta q(\theta)g(\lambda) = \lambda M_P(\lambda). \tag{C.22}$$

- $\lambda \in (\lambda_E, \lambda_P]$. In this range jobs are created as temporary but, provided that they survived up to their termination date, they are going to be converted as permanent at expiration. Intuitively, we call these the "good" temporary jobs. The net production provided by such jobs is given by

$$Y_{T_G}(\lambda, \theta, u, \lambda_T; c_P, c_T) = \underbrace{M_{T_G}(\lambda)ye^{-\lambda D^*(\lambda)} - u\theta q(\theta)g(\lambda)c_T}_{\text{Net Production under temp contract}} + \underbrace{M_P(\lambda)y - \frac{M_{T_G}(\lambda)}{D^*(\lambda)}c_P e^{-\lambda D^*(\lambda)} - f\lambda M_P(\lambda)}_{\text{Net Production under perm contract}}, \tag{C.23}$$

  where the first (second) part provides the net production associated with a job of type λ signed under a temporary (permanent) contract. Notice that the payment of the matching cost c_T occurs regardless of whether the job survives up to a specific date while payment of c_P occurs conditional on survival

[1] Following Cahuc et al. (2016), we exclude home production from our calculations of aggregate net output.

and hence this term is multiplied by the survival function, $e^{-\lambda D^*(\lambda)}$. The equality between entries and exists for this type of jobs imply that

$$\frac{M_{T_G}(\lambda)}{D^*(\lambda)} = u\theta q(\theta) g(\lambda) \tag{C.24}$$

$$\frac{M_{T_G}(\lambda) e^{-\lambda D^*(\lambda)}}{D^*(\lambda)} = \lambda M_P(\lambda) \tag{C.25}$$

- $\lambda \in (\lambda_P, \lambda_T]$. In this range, jobs are created as temporary and are subsequently destroyed at termination. Therefore, the net production implied by these jobs is given by

$$Y_{T_B}(\lambda, \theta, u, \lambda_T; c_T) = M_{T_B}(\lambda) y e^{-\lambda D^*(\lambda)} - u\theta q(\theta) g(\lambda) c_T \tag{C.26}$$

where M_{T_B} is identified from

$$\frac{M_{T_B}(\lambda)}{D^*(\lambda)} = u\theta q(\theta) \frac{g(\lambda)}{G(\lambda_T)} \tag{C.27}$$

Combining the above equations, we obtain that the net surplus associated with this economy, denoted as $\mathcal{Y}$, is given by

$$\begin{aligned}
\mathcal{Y} &= \int_{\underline{\lambda}}^{\lambda_E} Y_P(\lambda, \theta, u, \lambda_T; c_P) d\lambda + \int_{\lambda_E}^{\lambda_P} Y_{T_G}(\lambda, \theta, u, \lambda_T; c_P, c_T) d\lambda + \int_{\lambda_P}^{\lambda_T} Y_{T_B}(\lambda, \theta, u, \lambda_T; c_T) d\lambda - \kappa\theta u q(\theta) \\
&= M(\theta) \left[\int_{\underline{\lambda}}^{\lambda_E} \left(\frac{y}{\lambda} - f - c_P \right) g(\lambda) d\lambda + \int_{\lambda_E}^{\lambda_P} \left(\frac{y}{\lambda} - f - c_P \right) e^{-\lambda D^*(\lambda)} g(\lambda) d\lambda \right] \\
&+ M(\theta) \left[\int_{\lambda_E}^{\lambda_T} \left(y D^*(\lambda) e^{-\lambda D^*(\lambda)} - c_T \right) g(\lambda) d\lambda - \kappa \right]
\end{aligned}$$

where $M(\theta) = \theta u q(\theta)$ and the last term in the first equation corresponds to the total cost of vacancies posted which in steady state is given by the number of jobs created. Finally, notice that the derivations above imply that the steady state unemployment rate can be derived as

$$\begin{aligned}
u &= 1 - \int_{\underline{\lambda}}^{\lambda_E} M_P(\lambda) d\lambda + \int_{\lambda_E}^{\lambda_P} M_{T_G}(\lambda) d\lambda + \int_{\lambda_P}^{\lambda_T} M_{T_B}(\lambda) d\lambda \\
&= \frac{1}{1 + M(\theta) \left[\int_{\underline{\lambda}}^{\lambda_E} \frac{1}{\lambda} g(\lambda) d\lambda + \int_{\lambda_E}^{\lambda_T} D^*(\lambda) g(\lambda) d\lambda + \int_{\lambda_E}^{\lambda_P} \frac{1}{\lambda} e^{-\lambda D^*(\lambda)} g(\lambda) d\lambda \right]}
\end{aligned} \tag{C.28}$$

so that the unemployment rate is decreasing in the rate of arrival of offers and in the duration of temporary contracts.

Let $\mathcal{Y}^*$ denote net aggregate production at the labor market equilibrium $(\lambda_E^*, \lambda_P^*, \lambda_T^*, \theta^*)$ defined in Proposition 2. We next show that the reform affects labor market efficiency via the following channels

$$\frac{d\mathcal{Y}^*}{dc_T} = \underbrace{\frac{dM(\theta)}{dc_T}\frac{\mathcal{Y}^*}{M(\theta)}}_{\text{A. Change to Employment}} + \underbrace{g(\lambda_E)M(\theta)\left[\left(\frac{y}{\lambda_E} - f - c_P\right)(1 - e^{-\lambda_E D^*(\lambda_E)}) - (yD^*(\lambda_E)e^{-\lambda_E D^*(\lambda_E)} - c_T)\right]\frac{d\lambda_E}{dc_T}}_{\text{B. Change to Net Output from Substitution of Perm with Temp}} +$$

$$+ \underbrace{M(\theta)g(\lambda_P)\left(\frac{y}{\lambda_P} - f - c_P\right)e^{-\lambda_P D^*(\lambda_P)}\frac{d\lambda_P}{dc_T}}_{\text{C. Change to Net Output from Reduction in Conversion of Temp into. Perm}}$$

$$+ \underbrace{M(\theta)(yD^*(\lambda_T)e^{-\lambda_T D^*(\lambda_T)} - c_T)g(\lambda_T)\frac{d\lambda_T}{dc_T}}_{\text{D. Changes to Net Output from New Temporary Jobs being Created}} +$$

$$+ \underbrace{M(\theta)\left[-\int_{\lambda_E}^{\lambda_P}\left(\frac{y}{\lambda} - f - c_P\right)\lambda e^{-\lambda D^*(\lambda)}\frac{dD^*(\lambda)}{dc_T}g(\lambda)d\lambda + \int_{\lambda_E}^{\lambda_T} ye^{-\lambda D^*(\lambda)}(1 - \lambda D^*(\lambda))\frac{dD^*(\lambda)}{dc_T}g(\lambda)d\lambda\right]}_{\text{E. Changes to Net Output from Changes in Duration of Temporary Contracts}} +$$

$$- \underbrace{M(\theta)(G(\lambda_T) - G(\lambda_E))}_{\text{F. Changes to Matching Costs}}. \tag{C.29}$$

A. The reform affects the steady state exit rate from non-employment. If such effect is positive then we are going to have a partial increase in net output, as more people are now producing in this economy (assuming $\mathcal{Y}^*$ to be positive).

B. The reform also changes the incentives to hire permanent vs. temporary: If λ_E decreases, then some marginal jobs that in the status quo were created as permanent are now created as temporary. A fraction $(1 - e^{-\lambda_E D^*(\lambda_E)})$ of these temporary jobs created in the post reform equilibrium will not survive and this term captures the associated net loss in output.

C. The reform gives incentives to destroy temporary jobs that in the status quo used to be converted into permanent jobs. This term captures the associated loss in output.

D. The reform opens up new production opportunities unavailable in the status quo. This term captures the corresponding increase in net output.

E. The reform affects the duration of temporary jobs. As shown in Appendix (C.1.3), the reform decreases the duration of temporary jobs and hence lowers their overall expected productivity over the lifetime of a job. Lower duration, however, also increases the probability that temporary jobs survive at expiration, this is captured by the first term of this expression.

F. This captures the cost savings associated with the decrease in the matching cost c_T.

The reform can improve labor market efficiency via the following channels: an increase to aggregate employment (Effect A), the creation of new production opportunities that after the reform can be explored via temporary contracts (Effect C), reductions in matching costs (Effect F). However, the reform also lowers the incentives to convert temporary contracts into permanent ones (Effect C). The corresponding losses to net output from the increased churn of temporary jobs have to be added to the potential losses stemming from substitution of permanent jobs with temporary ones (Effect B) and reductions in the duration of temporary contracts (Effect E).

Similarly to what described in the text concerning the effect on aggregate employment, it follows that this reform delivers ambiguous results also on labor market efficiency. The proposition below summaries a possible scenario where the reform *lowers* labor market efficiency

Proposition 4 *Suppose that the equilibrium thresholds* $(\lambda_E^*, \lambda_P^*, \lambda_T^*)$ *move in response to the reform as in Figure 1.1 and that the policy change does not increase aggregate employment and maps into losses in net production for Effect B and Effect E in equation (C.29). Then, the reform (i.e. a reduction in* c_T*) will decrease net output if* $(\lambda_E^*, \lambda_P^*, \lambda_T^*)$ *are such that*

$$\begin{aligned} 1 - \left[yD^*(\lambda_T^*) e^{-\lambda_T D^*(\lambda_T^*)} - c_T \right] \frac{g(\lambda_T^*)}{G(\lambda_T^*) - G(\lambda_E^*)} \frac{d\lambda_T^*}{dc_T} < & \frac{g(\lambda_E^*)}{G(\lambda_T^*) - G(\lambda_E^*)} \left[yD^*(\lambda_E^*) e^{-\lambda_E^* D^*(\lambda_E^*)} - c_T \right] \frac{d\lambda_E^*}{dc_T} + \\ & + \frac{g(\lambda_P^*)}{G(\lambda_T^*) - G(\lambda_E^*)} \left(\frac{y}{\lambda_P^*} - f - c_P \right) e^{-\lambda_P^* D^*(\lambda_P^*)} \frac{d\lambda_P^*}{dc_T} + \\ & + \int_{\lambda_E^*}^{\lambda_T^*} y e^{-\lambda D^*(\lambda)} (1 - \lambda D^*(\lambda)) \frac{dD^*(\lambda)}{dc_T} \frac{g(\lambda)}{G(\lambda_T^*) - G(\lambda_E^*)} d\lambda - \\ & - \int_{\lambda_E^*}^{\lambda_P^*} \left(\frac{y}{\lambda} - f - c_P \right) \lambda e^{-\lambda D^*(\lambda)} \frac{dD^*(\lambda)}{dc_T} \frac{g(\lambda)}{G(\lambda_T^*) - G(\lambda_E^*)} d\lambda. \end{aligned} \tag{C.30}$$

That is, the increase in net output stemming from lower matching costs and the opening of new production opportunities is offset by the reductions in net output obtained from the after reform substitution of permanent jobs with temporary, the destruction of temporary jobs that used to be converted into permanent, and the reduction in the duration of temporary jobs.

C.2 Extension: Differences in Bargaining Power

In this subsection we provide an extension to the model by allowing the Nash bargaining coefficient, γ, to be different across employment contracts. Specifically, we assume that temporary contract workers have lower

bargaining power than permanent contract ones. This assumption is motivated by the institutional details described in Chapter 3 and the empirical evidence shown in Section 3.3.

Such difference in bargaining power clearly provides another motif for firms to utilize temporary contracts as workers hired under this type of employment contract can extract a lower portion of the surplus generated from a given match. We next evaluate the consequences of the reform under heterogeneous bargaining coefficients in an equilibrium where both temporary and permanent contracts are created.

C.2.0.1 Derivations

Let γ_C denote the Nash bargaining coefficient if a job is created under an employment contract of type $C \in \{P, T\}$ with $\gamma_T < \gamma_P$. It is easy to see that for a given value of unemployment, the derivations of the surpluses in equation (1.1) and (1.2) are unchanged. Differential rent-sharing affects the value of creating a vacancy which is now given by

$$r\Pi_v = -\kappa + q(\theta)(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + q(\theta)(1-\gamma_T)\int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda). \tag{C.31}$$

Using the free entry condition, the value of unemployment is given by

$$rU = z + \frac{\theta\gamma_T\kappa}{1-\gamma_T} + \frac{\theta q(\theta)}{1-\gamma_T}(\gamma_P - \gamma_T)\int_{\underline{\lambda}}^{\lambda_E}\left(\frac{y - rU - \lambda f}{r+\lambda} - c_P\right)g(\lambda)d\lambda. \tag{C.32}$$

Notice that under no differential rent-sharing (i.e. $\gamma_T = \gamma_P$), equation (C.32) boils down to equation (1.5). Equation (C.32) states that the value of unemployment depends on θ but also on an additional endogenous quantity, λ_E. To understand the intuition behind this result recall that, when $\gamma_P = \gamma_T$, marginal jobs with shock arrival rates $\lambda = \lambda_E$ provides the same utility to the worker irrespective of the employment contract. Under differential rent-sharing, on the other hand, individuals obtain a smaller gain when these marginal jobs are created under a temporary contract. The extra rents obtained by workers when the exogenous draw of production opportunity imply the creation of permanent employment contracts are captured by the last term in (C.32). Moreover, notice that

$$\frac{\partial U}{\partial \theta} > 0; \qquad \frac{\partial U}{\partial \lambda_E} > 0. \tag{C.33}$$

The first condition is the standard result that the value of unemployment increases with labor market tightness. The second condition, which is going to be true in equilibrium, states that if firms are creating more permanent jobs (i.e. an increase λ_E) then the value of unemployment raises because individuals are more likely to exit non-employment by obtaining a permanent job where they can extract an higher rent.

C.2.0.2 Equilibrium

To analyze the effect of the reform under a model that assumes differential rent-sharing, it is useful to write down the entry condition and the equations that defines the endogenous thresholds that are used to solve for the endogenous quantities $(\theta, \lambda_E, \lambda_P, \lambda_E)$,

$$h^{(EC2)}(\theta, \lambda_E; c_T) \equiv \kappa - q(\theta)(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) - q(\theta)(1-\gamma_T)\int_{\lambda_E}^{\lambda_T} S_T(\lambda; c_T)dG(\lambda) = 0 \quad \text{(EC2)}$$

$$h^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T) \equiv \frac{y - rU(\theta, \lambda_E)}{r + \lambda_T} + \lambda_T \frac{U(\theta, \lambda_E)(e^{-rD^*(\lambda_T)} - 1)}{r + \lambda_T} - c_T = 0 \quad \text{(TJCR2)}$$

$$h^{TJDR2}(\lambda_P, \lambda_E, \theta) \equiv \lambda_P - \frac{y - rc_P - rU(\theta, \lambda_E)}{c_P + f} = 0 \quad \text{(TJDR2)}$$

$$h^{(PJvsTJ2)}(\lambda_E, \theta; c_T) \equiv \frac{\lambda_E U(\theta, \lambda_E)(e^{-rD^*(\lambda_E)} - 1)}{r + \lambda_E} + \frac{\lambda_E f}{\lambda_E + r} + c_P - c_T = 0 \quad \text{(PJvsTJ2)}$$

where we made explicit the dependence of the value of unemployment on both (θ, λ_E).

To solve for equilibrium, one can start by noticing that (EC2) and (PJvsTJ2) provide a system of equations in two unknowns (θ, λ_E). In particular, from the (EC2) locus one can verify that labor market tightness is a decreasing function of λ_E. Intuitively, an higher λ_E lowers the expected profitability of creating a vacancy when $\gamma_P > \gamma_T$. Conversely, the (PJvsTJ2) locus makes labor market tightness an increasing function of λ_E, as described in Section 1.3.3. Based on this, one can plot the (EC2) and (PJvsTJ2) locuses in the (θ, λ_E) space, see Figure C.2.1, Panel (a). The intersection of the two locuses provides the equilibrium values of (θ, λ_E) which can be plugged in (TJDR2) and (TJCR2) to solve for (λ_P, λ_T) respectively.

C.2.0.3 Effect of the Reform

Next we analyze the effect of the reform. All the following differentiations are evaluated at the solution of the system of equations defined in Section C.2.0.2.

- We begin by considering the consequences of a reduction in c_T for labor market tightness, θ, and the threshold that defines the creation of permanent jobs, λ_E. From the (EC2) locus, we have that

$$\frac{d\theta}{dc_T}|_{\lambda_E \text{ is constant}} = -\frac{h^{(EC2)}_{c_T}(\theta, \lambda_E; c_T)}{h^{(EC2)}_{\theta}(\theta, \lambda_E; c_T)} < 0 \quad \text{(C.34)}$$

where

$$h_{c_T}^{(EC2)}(\theta,\lambda_E;c_T) = q(\theta)(1-\gamma_T)[G(\lambda_T)-G(\lambda_E)] > 0$$
$$h_{\theta}^{(EC2)}(\theta,\lambda_E;c_T) = -q'(\theta)\left[(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + (1-\gamma_T)\int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda)\right] -$$
$$- q(\theta)\left[(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda,\lambda_E,\theta)dG(\lambda) + (1-\gamma_T)\int_{\lambda_E}^{\lambda_T} h_{\theta}^{TJCR}(\lambda,\lambda_E,\theta;c_T)dG(\lambda)\right] > 0$$

This implies that the (EC2) locus shifts upward in Figure C.2.1 following a *decrease* in c_T: for a given λ_E, we have an increase in job creation due to the lower matching cost in creating a temporary contract. Notice that, differently from the benchmark case where after reform changes to θ were independent of γ, now smaller values of γ_T will command larger increases to θ after the policy change, as firms will increasingly take advantage of the lower rent-sharing associated to temporary contracts.

From the (PJvsTJ2) locus, we see that

$$\frac{d\lambda_E}{dc_T}|_{\theta \text{ is constant}} = -\frac{h_{c_T}^{(PJvsTJ2)}(\theta,\lambda_E;c_T)}{h_{\lambda_E}^{(PJvsTJ2)}(\theta,\lambda_E;c_T)} > 0 \tag{C.35}$$

where

$$h_{\lambda_E}^{(PJvsTJ2)}(\lambda_E,\theta;c_T) = \frac{-\lambda_E f r \frac{\partial D^*}{\partial \lambda}(\lambda_E)e^{-rD^*(\lambda_E)}}{(1-e^{-rD^*(\lambda_E)})(r+\lambda_E)} > 0$$
$$h_{c_T}^{PJvsTJ}(\lambda_T,\theta;c_T) = -1 < 0$$

This implies that the (PJvsTJ2) locus shifts to the left in Figure C.2.1 following a *decrease* in c_T: for a given θ, firms are less willing to create permanent jobs due to the lower matching costs in creating temporary contracts. Figure C.2.1 shows the consequences of the reform for the new equilibrium values of (θ,λ_E): labor market tightness is going to unambiguously increase. Similar to the benchmark case, however, the overall effect of λ_E is ex-ante ambiguous. See the graphical illustration in Figure C.2.1, Panel (b).

- Now we evaluate how the policy change affects λ_P. Recall that λ_P is identified from (TJDR2) for given (θ,λ_E). Total differentiation of (TJDR2) gives us

$$\frac{d\lambda_P}{dc_T} = -\frac{1}{c_P+f}\left[\frac{\partial U(\theta,\lambda_E)}{\partial\theta}\frac{d\theta}{dc_T} + \frac{\partial U(\theta,\lambda_E)}{\partial\lambda_E}\frac{d\lambda_E}{dc_T}\right] \gtrless 0 \tag{C.36}$$

The effect on λ_P is now ex-ante ambiguous as the reform increases θ but has an ambiguous effect on λ_E. The intuition is as follows: for a fixed λ_E, increased job creation from the lower matching costs raises the opportunity costs of converting a temporary job into a permanent one. However, if the reform decreases λ_E, this in turn will also decrease the value of unemployment for workers. This latter effect counteracts the increases to the value of unemployment driven by a larger θ and maps into an ex-ante ambiguous effect on λ_P.

- Last part of this qualitative analysis concerns the effect of the reform on λ_T. Recall that this threshold is identified from (TJCR2) given values (θ, λ_E). Total differentiation of (TJCR2) gives us

$$\frac{d\lambda_T}{dc_T} = -\frac{h^{TJCR2}_{c_T}(\lambda_T, \lambda_E, \theta; c_T)}{h^{TJCR2}_{\lambda_T}(\lambda_T, \lambda_E, \theta; c_T)} - \frac{h^{TJCR2}_{\theta}(\lambda_T, \lambda_E, \theta; c_T)}{h^{TJCR2}_{\lambda_T}(\lambda_T, \lambda_E, \theta; c_T)}\frac{d\theta}{dc_T} - \frac{h^{TJCR2}_{\lambda_E}(\lambda_T, \lambda_E, \theta; c_T)}{h^{TJCR2}_{\lambda_T}(\lambda_T, \lambda_E, \theta; c_T)}\frac{d\lambda_E}{dc_T} \quad \text{(C.37)}$$

where

$$\begin{aligned}
h^{TJCR2}_{\lambda_T}(\lambda_T, \lambda_E, \theta; c_T) &= y\frac{e^{-(r+\lambda_T)D^*(\lambda_T)}[(r+\lambda_T)D^*(\lambda_T)+1]-1}{(r+\lambda_T)^2} < 0 \\
h^{TJCR2}_{c_T}(\lambda_T, \lambda_E, \lambda_E\theta; c_T) &= -1 < 0 \\
h^{TJCR2}_{\theta}(\lambda_T, \lambda_E, \theta; c_T) &= -\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\left[\frac{r+\lambda_T(1-e^{-rD^*(\lambda_T)})}{r+\lambda_T}\right] < 0 \\
h^{TJCR2}_{\lambda_E}(\lambda_T, \lambda_E, \theta; c_T) &= -\frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\left[\frac{r+\lambda_T(1-e^{-rD^*(\lambda_T)})}{r+\lambda_T}\right] < 0
\end{aligned} \quad \text{(C.38)}$$

We next show that, if the reform decreases the value of λ_E, then λ_T unambiguously increases. Proceeding in a similar way as in Section C.1.3, we have that

$$\begin{aligned}
\frac{d\theta}{dc_T}h^{TJCR2}_{\theta}(\lambda_T; \lambda_E, \theta; c_T) &= \frac{-(G(\lambda_T)-G(\lambda_E))h^{TJCR2}_{\theta}(\lambda_T; \lambda_E, \theta; c_T)}{-\frac{1-\gamma_P}{1-\gamma_T}\int_{\underline{\lambda}}^{\lambda_E} h^{TJDR2}_{\theta}(\lambda, \lambda_E, \theta)dG(\lambda) - \int_{\lambda_E}^{\lambda_T} h^{TJCR2}_{\theta}(\lambda, \lambda_E, \theta; c_T)dG(\lambda) - \frac{q'(\theta)}{q(\theta)}\hat{S}} \\
&< 1
\end{aligned}$$

where the second inequality follows from the derivations shown in equation (C.15) and $\hat{S} = \frac{1-\gamma_P}{1-\gamma_T}\bar{S}_P + \bar{S}_T$ with $\bar{S}_T = \int_{\lambda_E}^{\lambda_T} S_T(\lambda, \lambda_E, \theta; c_T)dG(\lambda)$ and $\bar{S}_P = \int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda, \lambda_E, \theta)dG(\lambda)$. The inequality above implies that $\dfrac{d\lambda_T}{dc_T} < 0$ since

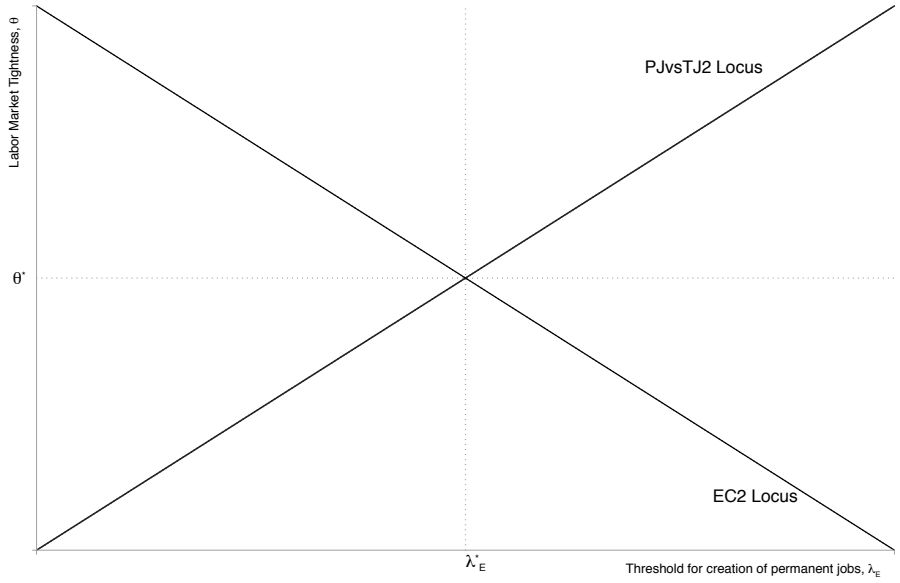
$$1 > h^{TJCR2}_{\theta}(\lambda_T; \lambda_E, \theta; c_T) + h^{TJCR2}_{\lambda_E}(\lambda_T, \lambda_E, \theta; c_T)\frac{d\lambda_E}{dc_T} \quad \text{(C.39)}$$

provided that $\dfrac{d\lambda_E}{dc_T} > 0$. Therefore, the reform unambiguously increase λ_T, as in the benchmark case. Here differential bargaining power weakens the equilibrium feedback effect coming from an increase

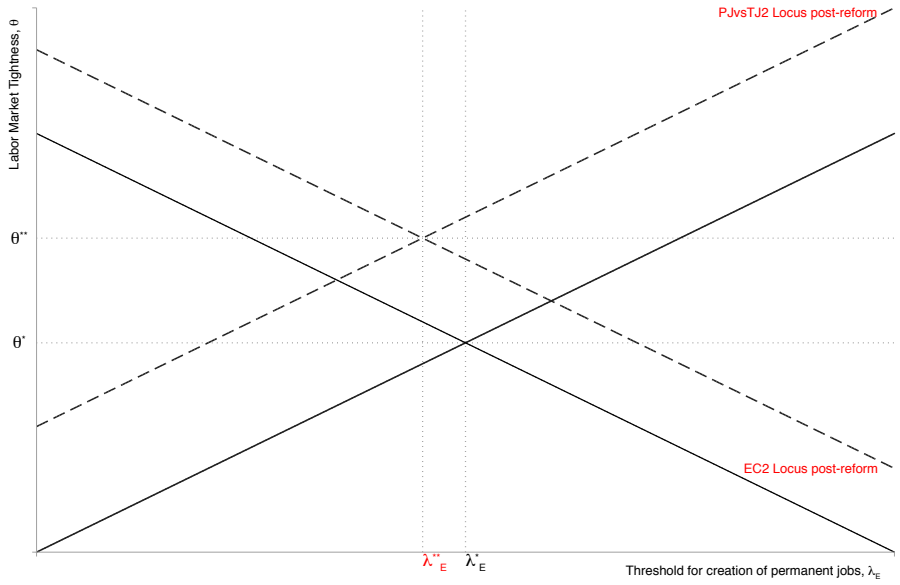
in labor market tightness as the value of unemployment when fewer permanent jobs can be created in equilibrium.

Figure C.2.1: Equilibrium under Differential rent-sharing

(a) Equilibrium values of (θ, λ_E)



(b) Effect of the Reform



Note: Panel (a) of this figure shows how one can solve for equilibrium values of θ and λ_E using the entry condition (EC2) and the rule that defines the creation of permanent jobs (PJvsTJ2), defined in Section C.2.0.2. Panel (b) shows how these equilibrium values are going to change following the introduction of a partial reform that facilitates the creation of temporary contracts ($\downarrow c_T$) without affecting the firing costs associated to permanent contracts, f.

Appendix D

Labor Costs Analysis

D.1 Labor Costs Analysis

Here we present a simple framework to interpret the estimates shown in Table 2 regarding in particular labor costs per worker. Start by noticing that labor costs, denoted as $\tilde{C}_{jt}$, can be written as

$$\tilde{C}_{jt} = \tilde{w}^P_{jt} P_{jt} + \tilde{w}^T_{jt} T_{jt} \tag{D.1}$$

where $\tilde{w}^P_{jt}$ ($\tilde{w}^T_{jt}$) is the average labor cost paid by the firm for workers under a permanent (temporary) contract and P_{jt} (T_{jt}) is the total number of workers hired by the firm under a permanent (temporary) contract.

Let $L_{jt} = P_{jt} + T_{jt}$ and define $c_{jt} = C_{jt}/L_{jt}$, $s_{jt} = T_{jt}/L_{jt}$. We can rewrite $w^C_{jt} = \phi^C + \xi^C_{jt}$ for $C \in \{T; P\}$ so that (D.1) becomes

$$c_{jt} = \phi s_{jt} + \xi_{jt} \tag{D.2}$$

where $\xi_{jt} = (\xi^T_{jt} - \xi^P_{jt}) s_{jt} + \xi^P_{jt} + \phi$ and $\phi = \phi^T - \phi^P$ is the average contract gap in labor costs between temporary and permanent contract. This structural parameter includes differences in terms of both direct wage compensation and labor taxes paid by the firm across contractual arrangements and therefore provides an useful benchmark to assess how much firms might potentially save when hiring temporary workers. Decomposing ξ_{jt} as $\xi_{jt} = \psi_j + \lambda_{p(j),t} + \varepsilon_{jt}$, we obtain the following equation

$$c_{jt} = \phi s_{jt} + \psi_j + \lambda_{p(j),t} + \varepsilon_{jt} \tag{D.3}$$

OLS estimation of (D.3) is likely to introduce bias in measuring ϕ, as the share of workers employed by the firm under a temporary contract, s_{jt}, might be correlated with unobserved changes in the labor cost structure of a given firm, even after controlling for fixed unobserved characteristics of the firm and province specific macroeconomic shocks. A possible solution is represented by IV estimation of (D.3) using the reform indicator as an instrument for s_{jt}. Under the assumption that the enactment reform, conditional on firm and province by year fixed effects, is uncorrelated with unobserved determinants of labor costs of firms captured by ε_{jt}, then IV estimates of -ϕ can be interpreted as an estimate of the average contract gap in labor costs between permanent and temporary workers.

Appendix E

AKM Effects

E.1 AKM Effects

We provide details concerning estimation of the AKM effects used in Table 3 to characterize the heterogeneous effects of the reform in terms of firms' pay structure. Using the INPS-INVIND data for the years 1990-2001 (28,494,774 person year observations) , we estimate the following AKM model

$$w_{it} = \alpha_i + \psi_{J(i,t)} + X_{it}^{\top}\beta + r_{it} \tag{E.1}$$

where w_{it} represents the log daily wage of worker i in period t; α_i represents a time invariant component of ability portable between employers for worker i; $J(i,t)$ is a function reporting the identity of the firm employing worker i in period t and so $\psi_{J(i,t)}$ are firm specific relative pay premiums; X_{it} controls for education (proxied by year of entry in the labour market) interacted with experience and time fixed effects; r_{it} is an unobserved time-varying error capturing shocks to human capital, person-specific job match effects, and other factors.

Identification of the AKM model hinger on a strict exogeneity assumptions that says that workers' move are, conditional on workers and firms fixed effects, uncorrelated with the time-varying residual components of wages. Card et al. (2013) and (Macis and Schivardi, 2016)provide supportive evidence in favour of this assumption for the German and Italian context.

The estimated firms fixed effects - often defined as "AKM effect" - provides a measure of the wage premium paid by each firm across its own workforce. Based on these estimated firm effects, we define firms as having an "High AKM effect" according to whether their associated firm effect is belongs to the fourth of the corresponding estimated distribution of AKM effects.

Finally, notice that estimates of the firm and worker effects in (E.1) are separately identified within a connected set of firms that are linked through by workers' moves (Card et al., 2013), which leaves us with 28,044,493 person year observations and in particular 754,407 firm effects that we subsequently merge into the INPS-INVIND-CNEL sample merged with balance sheet information coming from CERVED. In estimating (E.1), we let the definition of firm coincide with the EIN provided by INPS. For multi-plants establishment their associated firm effect is calculated as the average of the corresponding AKM effects for each associated plant.