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**The Effects of Partial Employment
Protection Reforms: Evidence from Italy**

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The Effects of Partial Employment Protection Reforms: Evidence from Italy

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Abstract

We combine matched employer-employee data with firms' financial records to study a 2001 Italian reform that lifted constraints on the employment of temporary contract workers while maintaining rigid employment protection regulations for employees hired under permanent employment contracts. Exploiting the staggered implementation of the reform across different sectoral collective bargaining agreements, we find that this policy change led to an increase in the incidence of temporary contracts but failed to raise employment significantly. The reform had both winners and losers. Firms appear to be the main winners as the reform was successful in decreasing labor costs, leading to higher profits. By contrast, young workers are the main losers since their earnings were substantially depressed following the policy change. Rent-sharing estimates show that workers on a temporary contract receive only 68% of the rents shared by firms with workers hired under a permanent contract, helping explain the post-reform labor cost reductions.

1 Introduction

Concerns over labor market flexibility have been at the center of the European political debate for the past three decades (e.g., Nickell, 1997). In response to the widespread belief that rigid employment protection laws (EPL) depress employment (OECD, 1994), 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, perhaps because of political constraints (Saint-Paul, 2000).

Economic theory delivers ambiguous predictions on the effects of such partial reforms of EPL. A number of studies have noted that such reforms 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). Some studies even suggest that partial reforms could end up increasing the bargaining power of incumbent workers, usually hired via permanent contracts, thus enhancing the "insider-outsider" gap and reducing overall efficiency (Bentolila and Dolado, 1994).

Empirical assessments of these policy changes and more generally of the role of EPL have mainly used cross-country research designs with aggregate data (Lazear, 1990; Bertola, 1990; Bertola and Rogerson, 1997; Garibaldi and Violante, 2005). A few recent studies have conducted within-country before-and-after studies, focusing on firm-level aggregates (e.g., Autor et al., 2007; Cappellari et al., 2012). While informative, an analysis of firm aggregates cannot directly address the effect of the reforms on job duration or on the rate of transition between temporary and permanent contracts, both of which are crucial to understanding the full impact of these institutional changes (Güell and Petrongolo, 2007; Cahuc et al., 2016). A firm-level analysis also ignores any distributional impact arising from the differential treatment of new vs. incumbent workers (Boeri, 2011).

In this paper, we use detailed Italian social security records matched with firms' financial data together with a difference-in-differences research design to provide a comprehensive empirical evaluation of an Italian reform signed into law on September 2001. This reform facilitated the creation of new 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: (i) how did the reform change the utilization of temporary contracts and

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overall employment; (ii) how did the policy impact the different parts of the labor market (jobs, firms, and workers); and (iii) were there heterogeneous effects across different worker and firm groups?

Contrary to the stated intent of the law (Biagi and Sacconi, 2001), we find that the reform had no effect on aggregate employment. We complement this null effect with evidence that shows how the post-reform 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. We then show that the reform generated both winners and losers. The primary winners appear to be firms. Even though value added per worker seems to be reduced, average labor costs fell substantially, leading to an increase in firms' profits. Young workers, by contrast, appear to be the main losers since their earnings were substantially depressed following the policy change, leading to a widening gap in earnings between young and old workers in the Italian labor market.

Our research design builds on the work of Cappellari et al. (2012) and exploits the staggered implementation of the reform across different sectoral collective bargaining agreements (*Contratti Collettivi Nazionali del Lavoro*; CCNLs henceforth) in an event-study framework. While Cappellari et al. (2012) rely on 8 CCNLs and survey information on firms' sectors to infer the passage of the reform, we exploit the fact that Italian matched employer-employee data directly report each worker's CCNL. We combine this information with novel data on the renewals of 181 Italian CCNLs to infer the reform status for around 58 million person-year observations, which are subsequently matched with firms' financial records. We show that outcomes follow parallel trends prior to the implementation of the reform, suggesting that observations from yet to be reformed CCNLs can be used to gauge counterfactual outcomes for observations from reformed CCNLs in the absence of the reform.

First, we analyze the effect of the reform on jobs. We find that this policy change led to an increase in the incidence of temporary contracts but failed to raise employment significantly. Consistent with the intended consequences of the law, we show that the reform fostered job creation and increased the share of new jobs signed under a temporary contract. However, we find that this rise in job creation was offset by the rate of separation for expiring temporary contracts, which increased by more than 20% after the reform. We rationalize this finding by noting that, while the reform made it easier for firms to create new jobs under a temporary contract, two key institutional elements remained in place in the Italian labor market: (i) firms can only keep workers under a temporary employment contract for a limited time, and (ii) permanent contract workers continue to have a high

level of employment protection. Extending the modeling framework of Cahuc et al. (2016), we show in the Appendix that the existence of these two restrictions combined with the more flexible regulation on the creation of temporary contracts makes firms more reluctant to change the employment status of workers from temporary to permanent. Specifically, we show that, after the reform, firms increasingly preferred separating from an incumbent temporary worker with an expiring contract to the alternative of converting the incumbent worker's temporary contract to a permanent one and therefore facing the risk of having to pay high firing costs. Difference-in-differences estimates show that, in fact, the entire observed increase in temporary job turnover is explained by the decline in temporary-to-permanent transitions occurring within the same employer. Conversely, we find that the reform significantly increased temporary-to-temporary (across employers) transitions as well as temporary-to-non-employment transitions. All these patterns are particularly pronounced among young workers.

Second, we analyze the effect of the reform on firms. We find that firms responded to the reform by increasing their share of temporary work employees without, however, expanding in size. This increase in the utilization of temporary workers maps into a rise in firms' profit margins (profits divided by value added) by approximately 8%. This increase in profits appears to be driven primarily by a reduction in firms' labor costs, rather than an increase in productivity. Our estimates suggest a gap in labor costs (compensation + labor taxes + benefits) between a permanent and a temporary employee of approximately €17,000. We estimate a negative and marginally significant effect of the reform on value added per worker. This latter finding is hard to reconcile with models where higher labor power leads to a lower level of firms' investment (Grout, 1984; Card et al., 2014; Jäger et al., 2019). Instead, it appears that reductions in employment protection might cause a reallocation effect toward jobs with lower value added per worker (Acemoglu, 2001; Cappellari et al., 2012). Our results therefore highlight a tradeoff between lower labor costs and lower productivity in firms' decisions to utilize temporary contracts (Weil, 2014).

Effects across firms are highly heterogeneous. Firms in the South of Italy, where informal labor markets appear well established, do not seem to have responded to the reform by increasing their share of temporary workers. Consequently, as expected, their financial accounts remain on average unchanged after the reform. We also find that firms with a high fraction of close-to-retirement workers complied with the policy change by significantly increasing their post-reform share of temporary workers. This suggests the existence of substitution patterns between old, permanent workers and young, temporary ones, consistent with recent evidence (Bovini and Paradisi, 2019). This substitution entails

significant labor cost reductions from the firm's perspective. Two other types of firms had a particularly large increase in their share of temporary workers in response to the reform. The first type is firms that paid a high wage premium pre-reform, as measured by standard two-way fixed effects decompositions à la Abowd et al. (1999). These firms are expected to have more incentives to use temporary workers as a way to reduce their relatively high labor costs (Katz and Krueger, 2019). In line with this prediction, we find that these firms experienced larger increases in their share of temporary workers as well as larger reductions in labor costs post-reform. This finding is consistent with Goldschmidt and Schmieder (2017) who show that high wage-premia firms in Germany are more likely to outsource part of their workforce. The second type of firms that significantly increased their share of temporary workers post-reform is firms in sectors associated with a low degree of employee hold-up power, as measured by the pre-reform sectoral average tenure (Garin and Silvério, 2019). The low degree of hold-up power suggests that these firms employ workers who are easier or less costly to replace. As expected, we find that these firms substantially increased their share of temporary workers, thereby decreasing labor costs and increasing profit margins.

Third, we examine the effect of the reform on workers. We start by focusing on incumbent temporary workers. These are relatively young individuals who were always employed with a temporary employment contract prior to the reform. We find that the reform made these workers experience significant earnings losses of around 7%. On the other hand, earnings of incumbent *permanent* workers did not change significantly. The earnings losses experienced by incumbent temporary workers can be explained by our theoretical framework that highlights the existence of temporary positions that in the pre-reform era were expected to be converted into permanent positions by employers but in the post-reform era were, instead, destroyed. Consistent with that, we find that the within-employer conversion rate of temporary jobs fell substantially (by around 36%) and overall job turnover increased. These patterns are particularly pronounced among younger workers. Another effect of the reform on incumbent temporary workers concerns job-sorting. Dustmann et al. (2020) find that following an increase in the minimum wage, workers are more likely to sort into higher productivity firms, which is consistent with the theoretical analysis of Acemoglu (2001). A partial EPL reform like the one studied in our paper may be interpreted as having the opposite effect on firms' labor costs than an increase in the national minimum wage has on them. Hence, mirroring Dustmann et al. (2020), we find that incumbent temporary workers disproportionately relocated into lower, rather than higher, productivity firms after the reform.

We complement the worker-level analysis by estimating the short- and medium-run

effects of entering the labor market following the passage of the reform on temporary contracts. By combining our difference-in-differences analysis with the empirical framework of Oreopoulos et al. (2012), we find that cohorts entering the labor market with a job already under the reform experienced immediate earnings losses of around 5%, relative to those who entered the labor market before the reform was implemented. This negative effect vanished only seven years following entry. Building on Lee (2009), we provide complementary evidence that suggests that these earnings losses are not due to selective entry or changes in the labor supply of young new entrants in the labor market. Instead, post-reform cohorts appear disproportionately less likely to obtain stable and high-paying jobs. This evidence on new entrants suggests that temporary employment contracts do not provide a "stepping stone" into the labor market. Instead, in line with Blanchard and Landier (2002), partial EPL reforms appear to foster the creation of highly precarious and fragile temporary jobs.

We conclude our analysis by examining the role of firm-specific pay policies in generating a wage gap between temporary and permanent workers, which can help explain the large reductions in firms' labor costs that we estimate following the reform. This analysis is also motivated by institutional features that suggest that temporary contract workers are largely underrepresented in both unions and firm-level wage agreements (Bentolila and Dolado, 1994; Montanari, 2002; Lani, 2013). Thus, we estimate a dynamic differential rent-sharing model based on within-person, within-employer transitions of workers from a temporary to a permanent contract. Our estimates suggest that differences in bargaining power explain around 79% of the raw difference in wage when moving from a temporary to a permanent contract within the same firm.

This paper contributes to several strands of the literature. 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., 2016; Cappellari et al., 2012; Cingano et al., 2016), a relationship characterized by mixed empirical results and one 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). More broadly, our firm-level results shed light on firm-level responses across various margins to an exogenous shock to labor market institutions and labor costs, an area that is receiving growing attention and has been primarily linked with minimum wage studies (Draca et al., 2011; Harasztosi and Lindner, 2019). Our result shows how a reduction in employment protection increases firms' profit margins, and suggests that changes to labor market institutions, such as reductions to EPL, might contribute to the decrease in the labor share (Krueger, 2018; Autor et al., 2020).

Our analysis of job flows provides new quasi-experimental evidence on 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 culled from administrative records allows us to decompose the responses of job creation and destruction to the reform. Moreover, it also allows us to test crucial predictions from our model such as the negative impact of the reform on the likelihood of converting temporary contracts to permanent ones within the same employer. Building on Dickens and Lang (1985) and Rebitzer and Taylor (1991), among others, who examine the existence and consequences of dual labor markets, we also document how partial reforms targeting only the employment protection of temporary contracts led to a widening gap in earnings between temporary and permanent workers, reinforcing the duality of the Italian labor market.

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 study the distributional impacts of these reforms across individuals and, in particular, between older and younger workers. We also provide new insights into the question of whether a temporary contract serves as a stepping stone 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, 2010; Oreopoulos et al., 2012), and is related to recent work of García-Pérez et al. (2019) who study the effect of a similar reform to the one analyzed here on Spanish high-school dropouts.

Finally, extending the analysis of Kahn (2016), we provide new evidence on the existence of a permanent contract premium in wages using an event-study design that focuses on the within-person, within-employer return of transitioning from a temporary to a permanent contract. We show that a major explanation for the large return associated with this transition is that firm-specific rents are not distributed equally between temporary and permanent workers within the same firm. This last finding contributes to a nascent literature that shows how firms are increasingly redrawing their boundaries, often by making use of alternative types of work arrangements, such as temporary employment contracts,

in an attempt to limit the degree of rent-sharing to a group of core employees (Kahneman et al., 1986; Weil, 2014; Goldschmidt and Schmieder, 2017; Katz and Krueger, 2019).

The rest of the paper is organized as follows. Section 2 explains the institutional framework behind the reform. Section 3 describes the data used for the empirical analysis. Section 4 explains the research design. Section 5 presents our main results on the effects of the reform. Section 6 discusses the winners and losers from this reforming process by estimating the effects of the reform on workers and firms. Section 7 presents the rent-sharing results. Finally, Section 8 concludes. The Appendix contains additional results as well as a theoretical model used to interpret some of our analysis.

2 Institutional Background

Historically, the permanent contract is the most typical form of employment contract in Italy. A permanent contract does not have a termination date.[1] Firms that wish to separate from a worker hired under a permanent contract have to pay high firing costs depending on firm size and worker tenure (Kugler and Pica, 2008).[2] Italian employers can also hire using a temporary contract, which has a termination date. When a temporary contract reaches its termination date, the employer can dismiss the worker without incurring 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).

Prior to the reform studied in this paper, Italian labor law stipulated that employment arrangements should be based on a permanent contract and a temporary contract could be adopted only 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 where firms could hire workers using temporary employment contracts, 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.[3]

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[1]Permanent employment contracts usually have a probationary period that varies across CCNLs with a typical length of around 6 months; see also the Employment Protection Legislation Database compiled by the ILO.

[2]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). See Bamieh (2016) and Jimeno et al. (2015) for recent papers that exploit variation in judicial enforcement to identify the effects of firing costs.

[3]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 to adopt such a contract, the temporary contract was *automatically* converted to a permanent contract. Moreover, similar to France (Cahuc et al., 2016), terminating a temporary contract before its expiration date was at least as costly as terminating a permanent one.

The regulation was later expanded with law 56/1987, which allowed CCNLs to specify additional clauses under which firms could hire workers on a temporary basis (Sestito, 2002).

The rules regarding temporary contracts fundamentally changed with decree 368, which was based on EU directive 1999/70/CE and was signed into law on September 6, 2001.[4] This reform replaced the strict inventory of cases under which temporary contracts were allowed with a general provision stating that employers can now rely on temporary employment for any "technical, production, organizational, and substitution" reason (Biagi, 2002). Importantly, regulations concerning the *renewal* of temporary employment contracts were not affected by decree 368/2001.[5]

The reform introduced by decree 368/2001 represents a fundamental milestone for Italian employment protection laws as it provided a de facto liberalization of the creation of temporary employment contracts (Tiraboschi, 2004). However, we stress that this reform represented only a *partial* reform. While employment protection was greatly reduced for temporary contracts after 2001, the employment protection associated with permanent contracts was unaffected by decree 368/2001 and in fact remained unchanged for almost 30 years. Figure C.1 captures this duality in the Italian labor market by plotting the evolution of the "Employment Protection Legislation Index" constructed by the OECD in Italy and other countries between 1985–2013.[6]

In conclusion, the Italian labor markets in the pre-reform era was characterized by (i) restrictions on the creation of temporary contracts, (ii) restrictions on the renewal of temporary contracts, and (iii) high employment protection granted to permanent contracts. Law 368/2001 essentially removed constraint (i) while maintaining constraints (ii) and (iii). According to the second-best theorem, it is then ex-ante unclear whether this reform necessarily leads to an increase in efficiency or employment levels (Blanchard and Landier, 2002). Appendix D formalizes this concept within a labor market equilibrium model based on Cahuc et al. (2016).

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[4]This reform followed a previous law (the "Treu Package"), signed in 1997, that introduced temporary work agencies. A third reform, decree 30 of 2003 (the "Biagi Law"), reformed the apprenticeship contract and introduced new temporary contracts limited to specific "projects" (Co.Co.Pro).

[5]Limits on the number of renewals are legislated by sectoral bargaining agreements. Typically, most temporary contracts can be renewed at most 2–3 times.

[6]Only more recently, and in particular with the introduction of the "Jobs Act" in 2015, has Italy reduced the employment protection of permanent employment contracts (Sestito and Viviano, 2018; Boeri and Garibaldi, 2018). Some of the new rules introduced by the Jobs Act, however, were repealed in 2018 by decree 87/2018 ("Decreto Dignità"), which for the first time *increased* employment protection under temporary employment contracts (Menegotto and Tomassetti, 2018). This illustrates more broadly how politically and economically salient reforms to employment protections were and continue to be, especially in Southern Europe.

3 Data

This section describes the data used for the empirical analysis. Section 3.1 introduces the social security records that represent our primary data source. Section 3.2 outlines the data collection on CCNLs, which is instrumental to our research design. Section 3.3 describes the income statement data used for the analysis of firms' outcomes. Summary statistics are presented in Section 3.4. Appendix A provides further details.

3.1 Matched Employer-Employee Data

Our main data source is derived from official social security records stored by the Italian Social Security Institute (*Istituto Nazionale Previdenza Sociale*, INPS).[7] This dataset provides the *complete* employment history for the period 1990–2013 of all private-sector workers who were employed at one point in time in a firm covered by the INVIND survey conducted by the Bank of Italy.[8]

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

This social security dataset (henceforth labeled as INPS-INVIND) is a matched employer-employee dataset that contains information on employment spells. Overall, INPS-INVIND contains information on around 7.5 million workers—roughly 40–50% of the universe of individuals present in INPS. For each employment spell that occurred in a given year, we have data on earnings, the number of days worked, the identity of the employer, as well as some demographic information on the employee.[9] As with most matched employer-employee data, attrition in these data can be caused by unemployment, non-participation, some forms of self-employment, or participation in the informal sector.

Critically for our purposes, INPS-INVIND provides administrative information on whether a job is under a temporary employment contract. We can distinguish between temporary contracts signed directly by the firm with the employee and those signed indirectly via a temporary work agency.[10] In our analysis, apprenticeships are not considered

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

[8]In the sequel, the data are balanced; that is, they provide the complete employment history also of all private sector workers who were not employed in a firm covered by the INVIND survey.

[9]Earnings include overtime payment, bonuses, and shift work. Earnings are converted to real euros (2010 CPI) and are top-coded at €400.000.

[10]Unfortunately, in the case of temporary workers employed through a temporary work agency, we are

temporary jobs. Since information on the type of employment contract is available only from 1998, we restrict our sample to the period 1998–2013. Finally, we use a version of INPS-INVIND that is collapsed down to the worker-year level, and that assigns job-related characteristics (CCNL, employer identity, type of employment contract, etc.) based on the job in which the worker earned the most in a given year.

3.2   Collective Bargaining Agreement (CCNL) Data

We also collect information on the year of renewal of each CCNL. INPS-INVIND provides administrative information on the CCNL associated with each job. *Centro Nazionale dell'Economia e del Lavoro* (CNEL) provides a digital archive of all CCNLs signed in Italy, from which we obtain information on the renewal years. We are able to match information on the history of renewals for 181 sectoral agreements, covering approximately 98% of all person-year observations in INPS-INVIND.[11] This matched sample is the one used in our analysis of local labor markets, jobs, and workers.

3.3   Firm Data

To study the effect of the reform on firms, we augment the baseline matched employer-employee data with two additional sources. First, we have administrative information from INPS—a file that we label as "Anagrafica"—on all Italian employers with at least one employee.[12] These data include information on national tax codes, monthly employment counts, geographical location, and additional information on firm-average earnings by different occupations.

We use tax codes to match information on income statements. From these statements, collected by CERVED, we obtain information on sales, value added, labor costs, and profits for the universe of Italian limited liability corporations.[13]

Overall, we are able to match balance-sheet information from CERVED for approximately 40 percent of the firm-year observations that we observe in our matched employer-

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unable to observe the user firm (Drenik et al., 2020).

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

[12]These data were originally collected at the Employer Identification Number (EIN)-year level. We collapsed this information at the firm-year level, using the national tax code (*codice fiscale*) reported by INPS as our definition of a firm. Although approximately 7% of firms have multiple EINs, INPS also records, within each parent firm, a "main EIN" identifier that identifies the headquarters of the parent firm.

[13]The CERVED dataset is derived from standardized reports that firms are required to file annually with the local Chamber of Commerce. It does not include private partnerships and sole proprietorships, which are characteristic of smaller firms.

employee dataset; see Table C.2 in the Appendix. As usual when working with matched employer-employee datasets and balance-sheet information (e.g., Card et al., 2014), we find that the matching rate improves substantially for larger firms.

3.4 Summary Statistics

Table 1 presents average characteristics across the two most popular contract types in the Italian labor market: temporary and permanent contracts.[14] The table distinguishes between the universe of person-year observations in INPS-INVIND (Columns 1 and 2) and the subset of the latter for which we have matched information on CCNLs (Columns 3 and 4).

The share of workers with a temporary contract is approximately 16%. 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 assumption that these workers are less likely to hold a college degree.[15] Temporary workers both earn and work substantially less than permanent workers. The gap in total yearly earnings between permanent and temporary workers is equal to roughly 17,000 real euros, which is 145% of the total yearly earnings of a temporary contract worker. Part of this gap is due to the difference in the total number of days worked in a year (175 for a temporary vs. 298 for a permanent worker). However, the gap in log daily wage remains substantial and equal to approximately 31 log points.[16]

Temporary workers are more likely to be employed in part-time positions. Only 12% of temporary jobs are obtained through a temporary work agency, which means that the vast majority of temporary contracts are signed directly by the firm with the employee. The average tenure of a temporary worker is approximately 1.8 years, which is significantly shorter than that of a permanent worker (6.5 years). Temporary workers are also more likely to hold multiple jobs within the year.[17]

Regarding workplaces, temporary workers tend to be employed in firms that are smaller and have lower value added per worker. Finally, we find that all reported statistics are virtually unchanged when we turn from the universe of person-year observations in INPS-

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[14]The labor contracts omitted from this table are apprenticeships and seasonal contracts.

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

[16]Boeri (2011) reports an hourly wage 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 largest in Sweden (45%) and smallest in the UK (6.5%). See also Kahn (2016) and Dias da Silva and Turrini (2015).

[17]We checked whether temporary workers simultaneously hold multiple job positions. This is reported in Appendix Table C.3: 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 are smaller for full-time workers and increase over time.

INVIND to the matched sample that contains information on the year of renewal of the associated CCNLs.

4 Research Design

Our research design is based on the staggered implementation of the reform across CCNLs. Section 4.1 describes in detail our key source of identifying variation. Section 4.2 introduces the baseline event-study regression framework used for assessing the impact of the reform.

4.1 Identifying Variation

Decree 368/2001—henceforth "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.[18,19] In effect, this means that some sectors are going to implement the new rules on the creation of temporary employment contracts before other sectors that may still be bound by their previous CCNL and, thus, by the previous legislation that allowed temporary contracts only under relatively limited circumstances. Appendix B provides a more detailed analysis of three of the most important CCNLs before and after the reform was passed.

We combine administrative micro-level information on the CCNL applied to each job in INPS with the digital archives of Italian CCNLs contained in CNEL to infer the reform status of 181 CCNLs. The year of implementation of the reform for a given CCNL is assigned on the basis of the date of the first new CCNL that was signed following the passage of the reform.

Most renewals of CCNLs that occurred following the passage of the reform were "regular," meaning that they were signed 2–3 years after the previous agreement, which is the typical duration of a CCNL. The overall distribution of renewal years is plotted in Panel (a) of Figure 1. Around 90% of the person-year observations are of CCNLs that imple-

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[18]CCNLs in Italy take a very distinctive form. The primary purpose of these CCNLs is to establish minimum wage floors within a particular occupation and sector. These minima thresholds (i) are equal between temporary and permanent contract employees, (ii) apply to all jobs signed under a specific CCNL regardless of the unionization status of the worker, and (iii) can be raised freely by firms. Besides setting these wage floors, CCNLs can also legislate on specific aspects, such as the maximum number of times that temporary contracts can be renewed. Even though they are not necessarily unique to a sector (e.g., there are some specific CCNLs for "handicraft," or *artigiani*, types of employers), CCNLs tend to overlap well with standard definitions of industries based, for instance, on ATECO codes. CCNLs can also sometimes be province-specific, although this is relatively rare (Boeri et al., 2019).

[19]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 that these maximum thresholds did not change significantly following the reform. See also Cappellari et al. (2012).

mented the reform by 2003. Movement of workers between jobs with different CCNLs and years of implementation of the reform is relatively low and does not appear to spike during the years in which the reform was implemented (see Table C.4).

Panel (b) of Figure 1 shows the key variation that we are leveraging in our research design. It reports the share of temporary jobs across two different CCNLs: one that implemented the reform early (in the food sector) and one that implemented it relatively late (in the metal handicraft sector).[20] We see that prior to 2001, both CCNLs have a roughly equal and flat trend in the share of temporary jobs. In 2002, the year in which the new legislation on temporary jobs was adopted in the food sector, we see a persistent jump in the share of temporary jobs. Conversely, the metal handicraft sector remains bound by the previous legislation up to 2005, when a new CCNL is signed and the new rules on temporary contracts are implemented. Accordingly, we observe a significant increase in the share of temporary jobs in the metal handicraft sector only after 2005.

4.2 Event Study

We recast the raw difference-in-differences variation plotted in Panel (b) of Figure 1 in an event-study framework. Our primary goal is to estimate how the reform impacted the share of temporary jobs and overall employment at the local labor market level, which we define as a combination of province and CCNL.[21] Our benchmark empirical model is given by

$$y_{cpt} = \eta_{cp} + \lambda_{pt} + \sum_{k=a}^{b} R_{ct}^k \theta_k + X_{cpt}^\top \beta + r_{cpt}, \tag{1}$$

where y_{cpt} represents either the share of temporary jobs or the logarithm of total employment in a given CCNL c, province p, and year t. The share of temporary jobs is calculated from the person-year panel described in Table 1. Employment counts are based on the data described in Section 3.3, which provide employment counts for the universe of private-sector employers in Italy. We cluster the standard errors at the local labor market (LLM) level and use the total number of person-year observations in the INPS-INVIND matched employer-employee dataset in a given cell as weights.[22]

The term R_{ct}^k is an event-study indicator (i.e., $R_{ct}^k \equiv \mathbf{1}\{t = t_c^* + k\}$, where t_c^* is the year of implementation of the reform in CCNL c). All our event-study indicators are binned at $a = -3$ and $b = 3$ and we always normalize θ_{-1} to 0. The coefficients θ_k when $k \geq 0$ therefore capture the effect of the reform on outcome y_{cpt}, k years after implementation

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[20]Note that the metal handicraft sector is different from the metal manufacturing sector studied in Appendix B.

[21]Appendix A.3 provides further details.

[22]We obtain similar results when weighting by the number of person-year observations in 2001.

of the reform relative to the year prior to implementation. When reporting our estimates, we refer to the coefficient θ_0 as the short-run (or at enactment) effect of the reform and θ_3 as the medium-run effect of the reform. Given our binning of event-study coefficients, medium run may be interpreted as 4+ years after the reform is implemented.

Identification of these coefficients hinges on the assumption that observations from CCNLs that have yet to implement the reform can be used as a counterfactual for observations from CCNLs that have already implemented the reform. Since trends in both employment and utilization of temporary contracts can vary enormously geographically even within a sector, equation (1) flexibly controls for CCNL by province, η_{cp}, and by province-specific time effects, λ_{pt}. The inclusion of η_{cp} means that we are computing changes in outcome y_{cpt} within a given LLM market. The inclusion of λ_{pt} allows us to construct potentially more realistic counterfactuals by comparing changes across CCNLs with different treatment statutes within a particular province. We also include in equation (1) a vector of controls in X_{cpt} that include fraction of female workers, fraction of Italian workers, and a quadratic term of average potential experience in a given LLM-year cell.[23] While the parallel trends assumption cannot be tested directly, we evaluate whether it holds in the years preceding the implementation of the reform by estimating the coefficients θ_k for $k < 0$.

5 The Impact of the Reform on Jobs

We now assess the effects of the reform on the incidence of temporary contracts, aggregate employment, and job flows. In Section 5.1 we document that the reform significantly increased the share of temporary contracts, but did not increase aggregate employment in a given local labor market. In Section 5.2 we show that the reform increased both job creation and job destruction, leading to an increase in the turnover rate of temporary jobs. The reform also significantly affected job-to-job transitions rates. Specifically, it reduced the probability of within-employer temporary-to-permanent job transitions while it significantly increased the probability of between-employer temporary-to-temporary job transitions. This section also shows how the reform led to a widening gap in earnings between permanent and temporary workers, reinforcing the duality of the Italian labor market.

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[23]We define potential experience as time passed since first entry into the labor market.

5.1 Share of Temporary Contracts and Employment

Panel (a) of Figure 2 plots the event-study coefficients $\{\theta_k\}$ from equation (1) when the dependent variable is the share of temporary jobs. These event-study coefficients are relatively flat and close to zero in the years prior to the reform, providing suggestive evidence in favor of the common-trend assumption. At enactment of the reform, there is a significant increase of about 1 percentage point in the share of workers covered by temporary contracts. The share of workers under a temporary contract increases by 4.3 percentage points in the medium run (i.e., 4+ years after the reform is implemented). To get a sense of the economic significance of these changes, Panel (b) reports the event-study coefficients depicted in Panel (a) rescaled by the share of workers employed under a temporary employment contract in the last year prior to implementation of the reform. In the medium run, the reform increased the share of temporary jobs by around 40% relative to the baseline pre-reform share of temporary contracts.

Next, we evaluate the results of the new legislation on employment, obtained when using log-employment in a CCNL × province × year as the dependent variable in equation (1). The event-study coefficients are also reported in Panel (b) of Figure 2. The trend in total employment prior to the reform appears stable. Following the reform, total employment continues to exhibit a flat profile with a point estimate that is negative but statistically insignificant. Therefore, the increase in the share of temporary jobs achieved through the reform does not appear to translate to a significant increase in employment. Our 95% confidence intervals reject medium-run increases in employment larger than 0.8%.

Our main results for the effects of the reform on the share of temporary jobs and on total employment are robust to several alternative specifications. Figure C.5 shows that the results are virtually unchanged when: (i) implementing changes in the definition of the year of implementation of the reform, t_c^*, (ii) focusing only on temporary jobs signed directly by the firm, (iii) estimating equation (1) without the inclusion of controls, (iv) limiting the analysis to the period 1998–2008, (v) considering alternative weighting schemes, and (vi) setting the metal manufacturing CCNL as a never-enacting sector.[24]

In conclusion, we find that the reform significantly raised the share of temporary contracts without increasing aggregate employment. We are not the first to find that an

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[24]The metal manufacturing CCNL is one of the most important CCNLs in the Italian economy (it covers around 25% of the jobs in our data). The CCNL that was signed after the passage of Decree 368/2001—signed in May 2003—presents a very peculiar situation. This CCNL states that both unions' and employers' representatives will reconvene in the following year to legislate on specific aspects that will allow the implementation of the reform on temporary contracts. It is unclear, however, whether the legislation on temporary employment agreements was actually implemented in the metal manufacturing sector (Santucci et al., 2008); see Appendix B.3 for details. Therefore, in this robustness check, we follow Cappellari et al. (2012) in letting the metal manufacturing CCNL represent a control group.

EPL reform can lead to null effects on aggregate employment. Figure C.4 and Table C.1 qualitatively relate our findings to those of other studies that have analyzed how various reforms to employment protection legislation mapped into changes in overall employment. Our null result can also be explained in a theoretical framework. Appendix D presents a model with endogenous creation of temporary/permanent contracts that rationalizes this result and echoes prior work by 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.

5.2 Job Creation, Job Destruction

The reform facilitated the conditions for hiring workers under a temporary employment contract. According to most models, including the one presented here in Appendix D, such facilitation is going to have a positive effect on job creation. However, two restrictions remained in place in the Italian labor market post-reform: (i) firms can only keep workers under a temporary employment contract for a limited time, and (ii) permanent contract workers continue to have a high level of employment protection.

The existence of these two labor market restrictions combined with the more flexible regulation on the creation of temporary contracts made firms more reluctant to change the employment status of workers from temporary to permanent, even if the match was of relatively high quality. Specifically, after the reform firms increasingly preferred separating from an incumbent temporary worker with an expiring contract and taking a chance with a newly hired temporary worker to the alternative of converting the incumbent worker's temporary contract to a permanent one and facing the risk of paying high firing cost in the future. This job destruction effect offsets the job creation effect highlighted above, and can ultimately lead to null or negative effects of the reform on aggregate employment.

Aggregate patterns suggest that both temporary job creation and destruction experienced enormous changes. Panel (a) of Figure C.2 shows that newly created temporary jobs represented only 5.7% of all jobs in 1998. Over the sample period, this share doubled to 11.4%. Panel (b) shows that the year-to-year turnover rate of temporary jobs also dramatically increased from 34% to 53%. Perhaps even more striking is the way the transition rates of temporary jobs evolved (Panel (c)), while the same rates remained fairly constant for permanent jobs (Panel d). In the first year of the sample period, about 30% of temporary jobs were converted to permanent jobs within the same employers. This share drops to around 12–13% in the most recent years. Conversely, the temporary job to temporary job transitions across different employers rose from 10% to around 25%.

We now isolate how much the reform contributed to these dynamics. The last part of this section will be devoted to understanding the evolution of the "contract gap," i.e., the difference in labor market outcomes between temporary and permanent jobs.

5.2.1 Measurement Framework

The empirical framework in this section estimates the event-study design shown in (1) but at the job level, i.e.,

$$y_{jcpt} = \eta_{cp} + \lambda_{pt} + \sum_{k=a}^{b} R_{ct}^k \theta_k + X_{jcpt}^\top \beta + r_{jcpt}, \tag{2}$$

where y_{jcpt} is an outcome for job (i.e., individual-firm match) j observed in CCNL c, province p, and year t.[25] X_{jcpt} controls for the same observable characteristics that we used for equation (1) but at the job level, namely, gender, dummy for Italian nationality, and a quadratic in potential experience of the worker associated with job j in year t.

To capture the effect of the reform on job creation, we estimate equation (2) by letting y_{jcpt} denote a dummy equal to 1 if job j corresponds to a new temporary job being created in year t. To capture the effect of the reform on job destruction, we estimate equation (2) by instead letting y_{jcpt} be a dummy equal to 1 if temporary job j observed in year t is no longer observed (under either a temporary or a permanent contract) in any subsequent year.[26]

Our model predicts that the reform will increase temporary job turnover. In particular, a key prediction is that some jobs that in the pre-reform era were expected to be converted from temporary to permanent will now be destroyed in the post-reform era.[27] These "complier" jobs are expected to be high-quality matches that lasted for a relatively long period of time but could not be renewed using another temporary contract because of the restriction on the number of renewals of temporary employment contracts. As a consequence, these jobs needed to be either destroyed or converted into permanent jobs by firms. To capture how the reform impacted the destruction rate of these high-quality,

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[25]We only focus on jobs that constituted the highest paying job of an individual in a given year (i.e., the job with the highest earnings in the year). The results in this section are robust to including all jobs observed in INPS-INVIND and/or to using a different time frame (quarterly instead of yearly).

[26]An alternative way to study the effect of the reform on the rate of job destruction would be to use a parametric hazard model. Instead, we use a stacked linear probability model for two reasons. First, it significantly simplifies the computation required, which is crucial given the size of our sample. Second, our methodology implies that the effects of the reform on all possible job flows are consistent with each other. In other words, the effect estimated on the job turnover rate is equal to the sum of the effects on across-employers transitions as well as the sum of the effects on within-employer transitions (in absolute terms); see Table 2.

[27]See the purple area in Figure D.1.

expiring temporary jobs, which one can view as the key unintended consequence of the reform, we estimate equation (2) restricting the sample to temporary jobs that in year t already existed for two or more years.[28] Henceforth, we will refer to such jobs as expiring temporary jobs.

Finally, to evaluate the impact of the reform on the contract gap, we report results from an augmented version of equation (2),

$$y_{jcpt} = \eta_{cp} + \lambda_{pt} + Perm_{jcpt}\theta + \sum_{k=a}^{b} R_{ct}^k \theta_k + \sum_{k=a}^{b} (R_{ct}^k \times Perm_{jcpt})\theta_k^P + X_{jcpt}^\top \beta + r_{jcpt}, \quad (3)$$

where $Perm_{jcpt}$ is an indicator equal to 1 if the observed job is under a permanent contract. The coefficients $\{\theta_k^P\}$ capture the evolution of the contract gap in outcome y_{jcpt} before and after the reform was implemented. Equation (3) is estimated on the sample of full-time jobs under either a temporary or a permanent contract.

5.2.2 Job Creation

The reform significantly increased the creation of temporary jobs. Panel (a) of Figure 3 shows the estimates from equation (2) when the outcome variable is an indicator equal to 1 if the observed job represents a *new* temporary job. Again, we find evidence in support of the parallel trends assumption: prior to the implementation of the reform the event-study coefficients exhibit a relatively flat profile. At enactment, the share of new temporary jobs is observed to increase by 1 percentage point. The share of new temporary jobs continues to grow post-reform by up to 3 percentage points, which is 42% of its pre-reform value. Panel (a) of Figure 3 shows that the evolution of the share of new temporary jobs closely tracks the evolution of the overall share of temporary jobs shown in Panel (a) of Figure 2. This indicates that the stock of temporary jobs is primarily increasing because of new temporary jobs being created in the Italian economy (as opposed to existing temporary jobs being renewed on a year-to-year basis by employers).

But who is filling these new temporary jobs? Figure C.6 shows that around 30% of these new temporary jobs are filled by previously non-employed individuals. Henceforth, the vast majority (70%) of the newly created temporary jobs are filled by previously employed individuals and are therefore primarily associated with (yearly) job-to-job transitions of

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[28]Ideally, we would like to analyze how the reform impacted the job destruction rate of expiring temporary contracts that could no longer be renewed. Unfortunately, we are unable to perfectly observe this in our data. We therefore impose this tenure restriction since most CCNLs in our sample period stipulate that employment relationships can last at most two or three years under a temporary employment contract.

workers. This is consistent with the overall evidence on the lack of effects on aggregate employment as shown in Figure 2. We further investigate how the reform influenced job-to-job transition rates in the next part.

5.2.3 Job Destruction

The reform also significantly increased the destruction of temporary jobs. Panel (b) of Figure 3 shows the impact of the reform on the year-to-year turnover rate of expiring temporary jobs. We find again evidence in favor of the parallel trends assumption, with pre-trends being relatively flat and centered around zero. At enactment, there is a significant jump of 4 percentage points (16% of the pre-reform year-to-year turnover rate of expiring temporary jobs), which increases to 5.5 percentage points (21% of the corresponding pre-reform year-to-year turnover rate) in the medium run.

An increase in the year-to-year job destruction rate of expiring temporary jobs can occur because of a *reduction* in either (i) the within-employer temporary-to-temporary transition (or renewal) rate or (ii) the within-employer temporary-to-permanent transition rate.[29] Panel (a) of Figure C.7 shows that the entire increase in the medium-run turnover rate of temporary jobs is driven by a reduction in the probability of observing temporary-to-permanent transitions occurring within the same employer.[30] We find a positive but insignificant effect on the probability of observing within-employer temporary-to-temporary transitions. The combination of these two results confirms that our specification is primarily targeting temporary jobs that, due to the restriction on the number of renewals, cannot be continued with a temporary contract and for which firms have therefore to decide between conversion and destruction. The key takeaway, as predicted by our model, is that firms in the post-reform era are significantly less inclined to convert these expiring temporary jobs into permanent positions within the firm.

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[29]Formally, another source that can lead to an increase in the destruction rate of temporary jobs is a reduction in within-employer transitions from temporary jobs into either seasonal jobs or apprenticeships. This type of transitions are, however, very rare in the data so we abstract from them here.

[30]Specifically, Panel (a) of Figure C.7 decomposes the increase in the turnover rate due to either (i) the within-employer temporary-to-temporary transition (or renewal) rate or (ii) the within-employer temporary-to-permanent transition rate by reporting the coefficient θ_3 from equation (2) when using as outcome a dummy equal to one if the temporary job observed in year t is renewed in $t+1$ or a dummy equal to one if the temporary job is converted to a permanent job in $t+1$, with both events occurring under the same employer from period t. Specifically, to construct these indicators, we leverage our longitudinal data and see whether the worker associated with the temporary job in year t is hired by the sample employer in $t+1$ under either a temporary (renewal) or permanent (conversion) contract.

5.2.4 Transitions

How did the post-reform Italian labor market absorb the increase in the rate of separation due to the reform? A temporary contract worker in year t whose job ended up being destroyed in year $t+1$ could either: (i) be hired by a new employer under a temporary contract, (ii) be hired by a new employer under a permanent contract, or (iii) remain non-employed.[31]

Panel (b) of Figure C.7 shows that the vast majority (86%) of individuals whose temporary job was destroyed going into year $t+1$ were able to find another temporary job with a different employer in year $t+1$, which suggests that the reform significantly increased job-to-job mobility (across temporary employment contracts), consistent with the job-creation evidence shown above. Interestingly, we also find that the share of transitions from a temporary job to a full year of non-employment significantly increased post-reform, while the reform had no impact on the transitions from temporary to permanent jobs across different employers.

Panel (a) of Table 2 reports the coefficient θ_3 from equation (2) for all observed temporary jobs (i.e., without restricting to expiring temporary jobs). Panel (b) presents the main results discussed in this section (i.e., restricting to expiring temporary jobs). The general patterns that we found when restricting to expiring temporary jobs are also found in the broader sample, but with smaller point estimates and overall magnitudes. In particular, in the medium run, the job-destruction rate increases by 3 percentage points ($\approx$ 9% of the pre-reform turnover rate) for all temporary jobs, but it increases by roughly 6 percentage points ($\approx 23\%$) for expiring temporary jobs.

Panel (c) further restricts to expiring temporary jobs where the worker is young (up to 25 years old). We find an extremely large and significant ($\approx 40\%$) increase in the associated turnover rate. A decomposition of this increase in the turnover rate suggests that younger workers post-reform were particularly less likely to experience a within-employer conversion to a permanent job: 73% of them found another temporary job with a new employer within the next year while 19% ended up being non-employed for a full year.[32] By contrast, Panel (d) shows that the reform had essentially a non-significant effect on all the transition rates of individuals with a permanent job.

In conclusion, the reform affected two key transitions. It significantly reduced the probability of within-employer temporary-to-permanent job transitions while it significantly increased the probability of between-employer temporary-to-temporary job transitions.

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[31]Panel (b) of Figure C.7 decomposes the overall increase in the turnover rate across these three different channels by reporting θ_3 from equation (2) when using as outcomes indicators corresponding to the transitions described in (i)–(iii) above.

[32]See also Figure C.7.

These two patterns are particularly evident for younger workers.

5.2.5 Duality of the Italian Labor Market

We now turn to the question of how much the reform impacted the duality of the labor market between temporary and permanent jobs. Panel (a) of Figure 4 plots the coefficients $\{\theta_k^P\}$ from equation (3) when the outcome is represented by total annual earnings. The coefficients shown in the figure therefore represent the evolution of the contract gap in earnings before and after the reform. The gap in earnings between temporary and permanent workers is relatively constant in the years leading to the reform. The pattern starts to diverge once the reform is introduced, with the gap in earnings increasing to almost 1830 euros, i.e., 21% of the adjusted gap in the last year prior to implementation (the outcome represented by the coefficient θ in equation (3)).

This divergence in the contract gap in earnings may be due to the fact that the reform (i) decreased incentives to create temporary jobs of longer duration and (ii) increased incentives to create temporary jobs of shorter duration.[33] As a result, we should expect differences in the number of days worked between permanent and temporary jobs to diverge after the reform. Panel (b) confirms this prediction by plotting results from (3) using as outcome days worked in a given year. Again, the pre-reform years are characterized by a fairly constant behavior. Upon implementation of the reform, there is a sharp increase in the contract gap in days worked by 6 days (11% of the adjusted gap in the pre-reform period) and it continues to grow significantly in the post-reform period by up to 21 days (or 30%).

Table 3 presents the results from Figure 4 and further plots the evolution of the contract gap across different subgroups. Column 2 shows that our baseline results are robust to including part-time jobs. Columns 3, 4 and 5 study the effects on the contract gap separately for men, women, and young workers (≤ 25 years of age), respectively. Specifically, we find that the gap in earnings increases more for women (38%) than men (16%), mirroring the increase in the contract gap in days worked (47% for women and 26% for men). Column 5 shows that the contract gap increased significantly for young workers following the implementation of the reform (36% in earnings and 48% in days worked).

In conclusion, this section shows that the increases in the employment protection gap between temporary and permanent jobs maps into an increase in the earnings gap between temporary and permanent jobs that appears to be primarily driven by decreases in the duration of temporary jobs, with effects being particularly pronounced in jobs filled by women and younger workers.

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[33]See Proposition 3 (part 2) in Section D and Figure D.1 for points (i) and (ii), respectively.

6 Winners and Losers

Having analyzed the main effects of the reform on aggregate employment and job flows, we now study the effects on firms and workers separately and show that the reform had both winners and losers. Section 6.1 shows that firms appear to be the main winners as the reform led to higher profits. By contrast, Section 6.2 shows that young workers are the main losers since their earnings were substantially depressed following the policy change.

6.1 Firms

Following the reform, firms experienced a higher degree of flexibility in managing their workforce. This flexibility appears to have entailed positive effects for firms. In particular, after the reform, firms' profits increased. This increase was driven by significant decreases in overall labor costs per worker that was not completely offset by decreases in value added per worker.

6.1.1 Measurement Framework

Our firm-level analysis is based on the sample of firms for which we are able to match income statements from CERVED (henceforth referred to as the INPS-INVIND-CERVED sample); see Section 3.3 and Appendix A.2 for further details. To assign a firm to a reform status, we look at the modal CCNL applied by the firm before the enactment of the reform. Thus, we restrict the analysis to firms already existing in the pre-reform era.[34]

We estimate an event-study specification akin to (1) but at the firm level,

$$y_{ft} = \psi_f + \lambda_{p(f),t} + \sum_{k=a}^{b} R_{ft}^k \theta_k + r_{ft}, \tag{4}$$

where y_{ft} is a firm-level outcome and $R_{ft} = \mathbf{1}\{t = t^*_{c(f,2001)} + k\}$ is an event-study indicator, $c(f,t)$ is a function that indicates the most common CCNL applied by firm f in year t, and t^*_c is the year of implementation of the reform for CCNL c. The terms ψ_f and $\lambda_{p(f),t}$ respectively denote firm and province by year fixed effects, where $p(f)$ is the province associated with firm f.[35] The results in (4) are weighted using inverse propensity score weights so as to match the pre-reform average firm-size and industry composition observed

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[34]The vast majority of firms that we analyze (95%) apply a single CCNL to its employees. However, especially among larger firms, it is possible to observe a given firm applying two or more different CCNLs to their workforce. The most common example is when a firm employs one or more managers since managers typically have their own separate CCNL.

[35]When firms have multiple establishments, we use the province where the firm's headquarters is located, which is indicated in the INPS records.

in the INPS-INVIND-CERVED sample to the one observed in our baseline INPS-INVIND data, collapsed at the firm level.

The inclusion of firm and province-by-year fixed effects implies that the difference-in-differences coefficients $\{\theta_k\}$ in (4) are identified by comparing within-employer changes in outcome y_{ft} before and after the reform with respect to a control firm within the same province but that in year t is still operating under the older, pre-reform legislation. We use the coefficients $\{\theta_k\}$ when $k < 0$ to assess the plausibility of the parallel trends assumption underlying equation (4).

6.1.2 Results

Panel (a) of Figure 5 reports the event-study coefficients from (4) using as the outcome the firm-level fraction of employees under a temporary employment contract. Prior to the reform, the event-study coefficients exhibit a flat profile centered around zero. Upon implementation of the reform, the fraction of temporary employees jumps significantly by 1 percentage point. The same fraction continues to grow by up to 5 percentage points, which is equal to 36% of the pre-reform firm-level share of temporary workers.

Panel (b) shows the event-study coefficients from Panel (a), but rescaled by the share of temporary workers observed in the pre-reform period. We then plot the event-study coefficients obtained when using log firm size as the outcome. In line with the LLM results (see Figure 2), the substantial increase in the fraction of temporary workers does not map into a significant firm-level increase in size.

How did firms' balance sheets change in response to the reform? Figure 6 reports two key firm-level margins that changed following the reform: labor costs and profit margins, defined as profits divided by value added per worker. A key pattern emerges: as firms increase their share of temporary employees, we observe (i) a reduction in labor costs per worker and (ii) an increase in the profit margins.

Table 4 further elaborates on the graphical evidence presented in Figures 5 and 6. Importantly, column 3 shows that the impact of the reform on value added per worker is negative but imprecisely estimated, while column 7 shows that the impact of the reform on log value added per worker is negative and marginally significant at -2%. This suggests that the reform did not result in an increase in the firm-worker match quality as predicted by models with match-quality learning (e.g., Nagypál, 2007; Faccini, 2014).[36] This result also implies that hold-up concerns (Grout, 1984) due to rigid employment protection granted

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[36]It is not surprising that the marginal effect of temporary contracts on match-quality learning is small since, as argued by Cahuc et al. (2016), permanent contracts tend to allow for a probationary period (typically 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).

to permanent contract employees are not predominant. Instead, our finding points to the fact that partial employment protection reforms may have played a role in the Italian productivity slowdown of the 1990s and 2000s (Cappellari et al., 2012; Daveri and Parisi, 2015).

A first-order effect of the reform is on labor costs per worker. Column 4 shows that they were reduced by up to 830 euros, which is equal to 3% of the pre-reform value. We obtain a similar magnitude when using the logarithm of labor costs per worker. As detailed in Appendix E, we can use the reform as an instrumental variable to assess the gap in firm labor costs between a temporary and a permanent contract employee. We estimate this gap to be approximately 17,000 euros.

The reform's effects on value added per worker and labor costs per worker map into a significant increase in profits. Column 5 shows that profits per worker increased by up to 790 euros, which is equal to an increase equal to 8.6% of the pre-reform value. Column 6 shows that the profit margin increases by 1 percentage point, for an overall increase of 8.3%. This result suggests that a rise in alternative work arrangements that imply more precarious forms of employment might have contributed to the decline in the labor share observed in several countries (e.g., Autor et al., 2020). Finally, column 9 shows that the reform did not have a significant effect on firm exit.

The evidence shows that firms appear to have complied with the reform primarily by substituting permanent workers with temporary employees, without increasing the overall number of workers, which is consistent with the theoretical predictions detailed in Appendix D as well as prior findings (Cappellari et al., 2012). This substitution 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 cost savings appear to be the predominant effect, as the evidence on post-reform profits suggests.[37]

6.1.3 Heterogeneity

We now assess the heterogeneity of the reform by estimating equation (4) separately for different subgroups of firms. Table 5 reports the results (based on coefficient θ_3 in (4)) for the following outcomes: share of workers under a temporary contract, log value added per worker, log labor costs per worker, and profit margins. We find that the reform had larger effects on firms (i) in Northern and Central Italy (i.e., areas where illegal work is

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[37]The tradeoff highlighted in Table 4 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.

less common), (ii) with an older workforce, (iii) with a high wage premium, and (iv) in sectors associated with employees that have low hold-up power.

Geography: Italy is a country with vast geographical differences (Malanima and Daniele, 2007). Northern Italy is traditionally more economically developed and industrialized than both Central and, particularly, Southern Italy. Panels (a)–(c) of Table 5 report the effects of the reform based on firms' location. In the North, we find that the reform had positive effects on the share of temporary workers and particularly large such effects on the profit margin, which appears to be driven by significant reductions in overall labor costs per worker. Firms in the Center experience comparable increases in their share of temporary workers and reductions in labor costs. Value added per worker decreases relatively more than in the North, but we should stress that the estimates on value added per worker remain relatively noisy and only marginally significant. These patterns eventually map into a null effect on the profit margin for firms in Central Italy. Interestingly, the reform does not appear to have an effect on the share of temporary workers in the South.[38] 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, profit margins and value added per worker. Panel (c) confirms that this is in fact the case.

Workforce Age: Panel (d) reports estimates on firms where prior to the implementation of the reform 25% or more of the workforce was 50 years of age or older. Such firms are therefore particularly likely to employ a consistent share of workers who are expected to be close to retirement post-reform. The evidence suggests that these firms substitute these older workers with temporary ones: the share of temporary employees jumps by around 45% post-reform. The substitutability between old permanent workers and young temporary workers is also found by Bovini and Paradisi (2019) in the context of an Italian pension reform that raised the retirement age in 2011. The increase in the share of temporary employees maps into lower labor costs per worker (of 5–6%) and higher profit margins. Even though estimates are imprecise, we do not find that firms with an older workforce experience lower value added per worker.

AKM Effects: Another category of firms that might have had a strong incentive to respond to the reform are those that, possibly for fairness or historical reasons, were

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[38]A possible explanation for this finding is that the South is traditionally associated with an informal labor market and, therefore, some firms in the South might still prefer to hire workers illegally instead of using legal temporary contracts (Di Porto et al., 2016).

forced to equally share rents with all their workers. A reform that facilitates the hiring of temporary contract workers (who are young, unlikely to be unionized, and hired for short periods) might allow some companies to reduce the wage premium or amount of rents shared with these workers (e.g., Abraham and Taylor, 1996; Katz and Krueger, 2019; Goldschmidt and Schmieder, 2017), a point that we further develop in Section 7. Consequently, firms paying relatively high premia to their workforce might have a strong incentive to hire under temporary contractual arrangements. We estimate firms' wage premia using the wage decomposition model of Abowd, Kramarz, and Margolis (1999) (AKM henceforth) for the years 1990–2001. We divide the AKM firm effects into quartiles and estimate equation (4) only for firms with an AKM effect in the fourth quartile. Panel (e) shows that these firms increased their share of temporary workers relatively more. This is in line with Goldschmidt and Schmieder (2017) who find that high AKM firms are more likely to outsource part of their workforce in an attempt to redraw the boundary of the firm and limit the sharing of firm-specific rents. We also find that high AKM firm effects are associated with a relatively large reduction in labor costs. Although estimates are imprecise, we find that the overall effect on the profit margin is positive.

Hold-Up Power: Finally, we turn to the question of how heterogeneity in employee hold-up power might have affected firms' response to the reform. Prior to the reform, the vast majority of the workforce was hired under a permanent employment contract and automatically covered by industry-wide CCNLs (see Section 7 for further details). The coexistence of these two institutional features is likely to have induced a significant degree of employee hold-up power. The latter feature, however, remains fundamentally unobserved and it is ex ante unclear how it varies across firms. We follow Garin and Silvério (2019) and measure worker hold-up degree by computing the average pre-reform worker tenure across CCNLs. We divide this measure into quartiles and report the effect for the lowest and highest quartiles (i.e., the highest degree of employee hold-up power, according to our proxy). Interestingly, Panel (f) shows that firms associated with CCNLs with a high degree of hold-up power do not appear to respond to the reform (i.e, their share of temporary workers remains flat post-reform). This zero first stage maps into (statistical) zero effects on other key firm-level margins, such as profits and labor costs. Conversely, firms associated with CCNLs that have lower hold-up power tend to respond quite strongly to the reform by significantly increasing their share of temporary employees. This effect maps into significantly lower labor costs per worker and lower value added per worker, but significantly higher profit margins.

6.2 Workers

We now investigate the impact of the reform from the worker's perspective. The staggered implementation of the law allows us to compare the impact of the reform on the career trajectories of individuals already present in the labor market in the pre-reform era (henceforth, incumbents). We then combine the research design of Oreopoulos et al. (2012) with our baseline difference-in-differences variation to compute the short- and long-run consequences of having entered the labor market under the new regime on temporary employment contracts. The general pattern of results is that the reform had a negative impact on earnings for new entrants as well as incumbent temporary workers. Incumbent permanent workers, instead, do not seem to be negatively affected.

6.2.1 Incumbents

We begin by analyzing the effect of the reform on incumbent workers (i.e., individuals already present in the labor market prior to the signing of the national reform in 2001). The focus on incumbent workers allows us to construct a worker-level measure of exposure to the reform based on the individual's CCNL in the pre-reform period, similarly to what we did in our firm-level analysis of Section 6.1.2.

We find that the reform led to significant earnings losses for incumbent temporary workers, while having almost no effect on incumbent permanent workers. The reform made incumbent temporary workers less likely to obtain a permanent contract with their employer. Instead, the reform made these workers more likely to relocate to other firms, particularly less productive ones. These effects are especially salient among young workers.

Measurement Framework

We estimate a version of equation (1) at the individual level that is given by

$$y_{it} = \eta_{LLM(i,2001)} + \lambda_{p(i,2001),t} + \sum_{k=a}^{b} R_{it}^k \theta_k + X_{it}^\top \beta + r_{it}, \tag{5}$$

where $R_{it} = \mathbf{1}\{t = t^*_{c(i,2001)} + k\}$ is an event-study indicator, $c(i,t)$ is a function that returns the CCNL of individual i in year t, and t^*_c is the year of implementation of the reform for CCNL c. Hence, in equation (5), individuals are treated according to when their 2001 CCNL implemented the reform. $\eta_{LLM(i,2001)}$ and $\lambda_{p(i,2001),t}$ denote local labor market (LLM) and province-by-year fixed effects based on the CCNL and province observed in 2001, respectively. The vector X_{it} controls for gender, Italian nationality and a quadratic in potential experience.

Equation (5) is estimated separately on individuals who had at least one employment spell in the pre-reform (1998–2001) and these employment spells were (i) always under a temporary contract (temporary incumbent workers), and (ii) always under a permanent contract (permanent incumbent workers). As we are primarily interested in analyzing the effect of the reform on the career progression of individuals, we restrict our analysis to individuals aged 40 and under in 2001.[39] The identification of θ_k is achieved using a difference-in-differences approach that compares individuals who in 2001 were employed under, say, a temporary contract but were subsequently covered by the reform in different years.[40,41]

This approach allows us to investigate whether the partial employment protection reform under investigation helped workers already present in the labor market climb up the job ladder toward more stable and higher-paying jobs (the stepping-stone hypothesis of, for instance, Booth et al., 2002), as opposed to "trapping" individuals in a sequence of precarious, lower-paying jobs (the precarious-trap hypothesis of, for instance, Blanchard and Landier, 2002).

Results

Figure 7 shows the effects of the reform on incumbent workers. Panel (a) reports the event-study coefficients from equation (5) estimated on the sample of incumbent temporary workers with annual earnings as the outcome. We find that differences in earnings between treated and control workers are relatively well balanced and statistically insignificant before the reform. Earnings differences started to emerge post-reform: individuals in the post-reform period earned 266 euros less (2.4% of the pre-reform average earnings of incumbent temporary workers) than individuals in the pre-reform period. This earnings gap continued to grow post-reform to 780 euros (or 7%).

What explains these earnings losses? In line with our model and the flow analysis of Section 5.2.3, Panel (b) of Figure 7 reports the effects of the reform on the probability of an incumbent temporary worker being converted to a permanent worker by the current employer. We find that post-reform this likelihood declined significantly by 9 percentage points (or 32% of the unconditional probability of a within-employer temporary-to-permanent transition in the pre-reform era).[42] In line with the duality results displayed

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[39]Our results are robust to the inclusion of all individuals in the sample. See Table C.7 in the Appendix.

[40]Clearly, individuals might move between different CCNLs and hence between reform statuses. This is why equation (5) primarily captures intention to treat type of effects. See also Table C.4.

[41]Appendix Table C.6 summarizes the key characteristics of incumbent temporary and permanent workers prior to the implementation of the reform.

[42]Results are robust to alternative assumptions on employment. Panel (a) of Figure C.8 shows that the results on earnings are virtually unchanged if we condition on incumbent temporary workers being

in Section 5.2.5, we also find highly asymmetric effects between temporary and permanent workers. Unlike incumbent temporary workers, the earnings of permanent incumbent workers covered by the reform appear to change little in event time.[43]

Table 6 presents the main results of this section for incumbent temporary workers and provides additional evidence. Column 1 of Panel (a) shows that the reform appears to have a small and only marginally significant *negative* effect on the probability of employment of incumbent temporary workers. Column 2 shows that earnings were reduced by up to 778 euros (or 7%).[44] Columns 3 and 5 show that some of the negative effects on earnings are captured by a significant reduction in days worked, even when we condition on workers being employed. Column 6 shows that the incumbent temporary workers are significantly less likely to remain with their current employer after the reform. As shown in Panel (b) of Figure 7, column 7 shows that this effect is entirely driven by the very large reduction in the within-employer probability of an incumbent temporary worker's contract being converted to a permanent contract following the reform.

The increase in job turnover caused by the reform raises the question of whether treated workers systematically relocated to lower productivity firms. This question is motivated by models such as that of Acemoglu (2001) where search frictions cause firms to create fewer jobs in capital-intensive firms relative to the social optimum. Acemoglu (2001) shows that an increase in the minimum wage can then cause a relocation of workers to more productive firms.[45] In a recent paper, Dustmann et al. (2020) find evidence in support of this prediction when analyzing the introduction of a national minimum wage in Germany. A reform that reduces EPL (like the one studied in our paper) may be interpreted as having the opposite effect on firms' labor costs than an increase in the national minimum wage has on them. Hence, mirroring Dustmann et al. (2020), we would expect incumbent temporary workers to relocate into lower, rather than higher, productivity firms after the reduction in EPL.

Column 8 of Table 6 shows estimates from equation (5) when the outcome is value added per worker of the current firm. In line with Acemoglu (2001) and Dustmann et al. (2020), our results suggest that the reform caused incumbent temporary workers to systematically relocate to lower-quality firms where value added per worker was 5000 euros

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employed. Individuals who became non-employed in a given year, but eventually reentered the sample in any subsequent years, are assigned earnings equal to 0 in the non-employment years.

[43]Panel (b) of Figure C.8 displays the impact of the reform on earnings across incumbent temporary and permanent workers.

[44]Consistent with the evidence shown in Figure C.8, conditional and unconditional estimates on earnings are relatively similar.

[45]Such a reallocation effect can also be rationalized by alternative models such as monopsonistic models of the type discussed in Card et al. (2018). See Dustmann et al. (2020) for a discussion.

lower (or 12% of the average value observed in the pre-reform period).

Panel (b) of Table 6 shows that the effects shown in Panel (a) are particularly pronounced among young individuals (i.e., those who were at most 25 years old in 2001). For instance, young workers' earnings losses are 12% of the average earnings observed in the pre-reform and their contract was 59% (12 percentage points) less likely to be converted by their employer to a permanent one.

The overall evidence presented in Table 6 can be interpreted through the lens of a model with heterogeneous job quality, as the one described in Appendix D. A significant fraction of incumbent temporary workers appears to be associated with key marginal temporary positions, i.e., positions that in the pre-reform era were expected to be converted to permanent positions by employers but in the post-reform were, instead, destroyed (depicted in purple in Figure D.1). This increase in reform-induced job-separation rates maps into a significant reduction in within-employer temporary-to-permanent transition rates and an increase in overall turnover, with treated incumbent temporary workers that relocating to lower-quality firms relative to control workers. Taken together, these events map into significant earnings losses, with effects that are particularly pronounced among younger workers.

6.2.2 New Entrants

Evaluating the impact of the reform on new entrants is more challenging as the quasi-experimental variation that we have exploited thus far requires workers to be already present in the labor market. Nevertheless, we show next that it is possible to combine our baseline difference-in-differences research design with the empirical model of Oreopoulos, Von Wachter, and Heisz (2012) in order to quantify the short- and medium-run effects of having *entered* the labor market after the reform has been implemented. Even though this analysis is subject to more limitations, we generally find that the reform negatively affected the earnings of new entrants both in the short- and in the medium-run.

<u>**Measurement Framework**</u>

We start by considering all individuals who entered the labor market between 1998 and 2005 and were not yet 30 years old at the moment of entry. We then collapse the data at the level of year of entry into the labor market (e), potential experience (ℓ), CCNL (c), province of entry (p), and calendar year (t). Following Oreopoulos, Von Wachter, and Heisz (2012), who study the impact of entering the labor market after a recession, our

main specification is given by

$$y_{ecpst} = \eta_{cp} + \lambda_{pt} + \phi_e + \chi_s + \sum_{s=1}^{7} \theta_s T_{ec} + r_{ecpst}, \tag{6}$$

where η_{cp} are LLM market fixed effects, λ_{pt} are calendar year fixed effects that are province-specific, ϕ_e and χ_s are year of entry and years of potential labor market experience fixed effects, respectively.[46] All results are weighted by the corresponding cell sizes.

$T_{ec} = \mathbf{1}\{e \geq t_c^*\}$ indicates whether cohort (e,c) entered the labor market after the reform was implemented. Therefore, the coefficients of interest, $\{\theta\}_{s=1}^{7}$, isolate how this condition at the moment of entry affects the experience profiles of workers. Each coefficient is identified by standard difference-in-differences arguments as the staggered implementation of the reform across CCNLs allows us to observe two individuals entering the labor market in the same year (and province) but with different CCNLs and, therefore, potentially different treatment regimes.

We assume that the variation that identifies $\{\theta\}_{s=1}^{7}$ is driven solely by the institutional changes caused by the reform, as opposed to differences in the labor supply of workers. This assumption would not hold if, for example, individuals selectively enter the labor market based on the reform status, since it would presumably generate unobserved differences between the pre- and post-reform cohorts. In such a case, we would expect to observe large shifts in the number of new entrants, with individuals disproportionately entering LLMs before they adopt the reform. We find, however, the number of new entrants to be flat around the reform, reinforcing our key identifying assumption.[47] We also find that pre-determined characteristics of workers are relatively well balanced between the pre- and post-reform cohorts.[48]

Results

Figure C.3 sets the stage for the analysis of new entrants by plotting annual earnings of workers entering the labor market between 1998 and 2005. There appears to be a dip

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[46]It is well known that cohort, potential experience, and year effects cannot be separately identified in equation (6). We therefore cluster together the year of entry effects for individuals entering the labor market between 1998 and 2000, using a similar strategy as Card and Lemieux (2001). This normalization does not affect our results.

[47]Figure C.9 shows that the logarithm of the number of new entrants appears flat in event time. Moreover, the evidence in Figure C.9 suggests that a key limitation of our design in equation (6)—the fact that we are able to measure employment outcomes only after an individual enters the labor market—is unlikely to severely bias our estimates. The evidence in Figure C.9 is also important by itself, as it shows that the reform did not significantly introduce more individuals into the labor market for the first time.

[48]Table C.8 shows the pre-determined observable characteristics of workers for both pre- and post-reform cohorts. We find most of the differences to be economically and statistically insignificant. Notably, post-reform cohorts are 1.5 percentage points less likely to be non-Italian and 0.16 years younger on average.

in the earnings profile occurring for workers entering after 2001 (i.e., after the national passage of the reform), particularly for cohorts entering between 2002 and 2004. Table 7 reports the coefficients θ_s from equation (6) that isolate the dynamic effects of entering the labor market. Column 1 shows that post-reform cohorts experience significant earnings losses of 430 euros in the first year they enter the labor market, which is equal to 5% of the average earnings of pre-reform cohorts in their first job. These earnings losses shrink but remain statistically significant over the experience profiles and do not converge to zero until seven years after entry.

The reduction in earnings experienced at the beginning of the career by post-reform cohorts is linked with a significant decrease in the probability of starting such a career with a permanent contract (see column 2). Importantly, post-reform cohorts also appear significantly less likely to be employed in a permanent job for up to four years after entry. Column 3 shows that these decreases in the probability of employment under or conversion to a permanent contract map into a decrease in the probability of remaining with the same employer, especially during the early stages of the career. Column 5 shows that differences in the extensive margin (employment vs. non-employment) across pre- and post-reform cohorts are also statistically and economically small. This suggests that the reform did not consistently help post-reform cohorts to secure a job following entry into the labor market. Finally, although estimates are somewhat imprecise, when we compare the quality of firms—as proxied by their value added per worker—between pre- and post-reform cohorts, we find that post-reform cohorts tend to be sorted into lower-quality firms, a difference that vanishes only seven years after entry.

The evidence shows that individuals who started a career under the reform suffered earnings losses both in the short and in the medium run, compared to control individuals who instead started their career with a job that still applied the old rules on temporary employment arrangements (and therefore were covered by the reform relatively late in their career). In addition, the post-reform cohorts were more likely to: (i) start with a temporary contract, (ii) remain "trapped" in a temporary job in the early part of their career, and (iii) be separated from current employers and relocated to lower-quality firms. Differences between pre- and post-reform cohorts in the probability of employment under a permanent contract only vanish five years after entry into the labor market and differences in earnings after seven years.

Robustness: We find that our results on new entrants are robust to a set of alternative implementations (see Table C.9). First, the qualitative results are unchanged when we control for share of females and Italian workers in a given cell. Second, they are un-

changed when we use only employed or full-time workers to calculate cell-specific means in equation (6). Third, the results on post-reform experience are basically unchanged when we introduce current LLM and current province-year fixed effects. This suggests that mobility across sectors and places does not appear to result in a rapid convergence for post-reform cohorts. Fourth, the results are qualitatively similar when we estimate an augmented version of equation (6) in which we interact province-by-calendar-year and entry-year fixed effects, which absorb any differences at the provincial level in the labor supply of new entrants over the experience profile.

Heterogeneity

Table C.10 investigates the heterogeneity of our baseline results shown in Table 7 by estimating equation (6) separately across different groups.

Gender: The earnings losses are particularly pronounced at the beginning of the career for male workers and these larger earnings losses appear to be linked to a significant decrease in the probability of starting a career with a permanent employment contract. The results for female workers are different. Post-reform cohorts have relatively small earnings losses that are only marginally statistically significant in the first year. Interestingly, differences between female pre- and post-reform cohorts in terms of earnings and probability of employment under a permanent contract both vanish almost immediately over the experience profile.

Age at Entry: A key limitation of our data is that we don't have information on education. We try to circumvent this limitation by looking at results separately for workers who were 21 or younger when they entered the labor market with a full-time job (and hence unlikely to be holding a college degree) and workers who were 24 or older when they entered (and hence more likely to be holding a college degree). Again, we find highly heterogeneous patterns. Post-reform cohorts that entered at an older age experienced large earnings losses at the outset of their career but were able to close the gap in earnings with the pre-reform cohorts. Again, this catch-up process in terms of earnings is highly correlated with the evolution of the probability of employment under a permanent contract. By contrast, post-reform cohorts that entered at a younger age experienced a very different pattern. Their probability of employment under a permanent contract is reduced not only at the start of their career but also 6-7 years after entry. Being "trapped" in temporary employment arrangements relatively late in their career then maps into significant losses in earnings for this group of workers that do not appear to converge to zero over the

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experience profile.

Predicted Earnings: Finally, we construct a predictor of earnings seven years after entry using information on the worker (gender, nationality, age at entry) and on the first job/employer (dummies for 5-digit sector codes, province indicators, deciles of firm size, and value added per worker). We fit this model to cohorts that entered before the national reform was passed (1998–2000) and extrapolate the predictions to later cohorts. We then estimate equation (6) separately for individuals that belong to the first and last terciles of these predictions. We find that at entry the earnings losses are disproportionately concentrated on individuals with the highest predicted earnings (a reduction of 7%). These individuals are also significantly less likely to start their career with a permanent contract (a decrease of 26%). However, they are able to exit the non-permanent contract state relatively fast with a consequent catch-up in terms of earnings. Conversely, the individuals with the lowest predicted earnings do not appear to suffer large earnings losses at entry. However, they appear to remain persistently employed in non-permanent jobs and this eventually maps into persistent earnings losses.

Final Discussion

A limitation of our design in equation (6) is the fact that we are able to measure employment outcomes only after an individual enters the labor market. The reform, however, may clearly impact this specific entry margin. Combining the methodology of Lee (2009) with additional assumptions, we show in Appendix F that we can set a bound on how large the change in the entry rate needs to be to eliminate the reform's effect on earnings displayed in Table 7. We find that the share of individuals who are able to find a first job in a given year in the reform counterfactual but not in the non-reform counterfactual needs to be 20.5% in order to compensate for the net present value of earnings losses reported in Column 1 of Table 7. This increase appears unrealistic, especially in light of the auxiliary evidence presented in Figure C.9 showing that the logarithm of the number of new entrants appears relatively flat in event time. All in all, the evidence suggests that the partial employment protection reform of 2001 caused present discounted value losses for young new entrants in the Italian labor market.

7 Bargaining Power of Temporary vs. Permanent Workers

We showed above that firms were among the main winners of the reform. Even though the reform led to a reduction in value added per worker, it also led to a sizable reduction in labor costs, with a positive net effect on firms' profits. How can firms reduce their labor costs by hiring temporary rather than permanent workers? To answer this question, we investigate the role of employer-specific pay policies and, in particular, differences in the amount of rents that firms share with permanent versus temporary workers.[49]

This analysis is motivated by the institutional features of the Italian labor market. Italian industrial relations between firms and workers 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. The second pillar consists of firm-level bargaining agreements that establish wage top-ups above contractual minimums. Firms can also distribute additional premiums and bonuses.[50]

Regarding the first pillar, the law explicitly forbids firms to discriminate between temporary and permanent contracts when establishing minimum wages. Under the second pillar, however, firms are legally allowed to split rents/premia/bonuses differently between permanent and temporary employees (Picchio, 2006). Montanari (2002) presents a case where temporary workers filed (and lost) a lawsuit against their employer who discriminated against them by allocating end-of-year bonuses exclusively to permanent workers. Relatedly, evidence indicates that temporary workers are not well represented by unions. For instance, 97% of all workers under the age of 35 who are registered in the largest union in Italy (CGIL) are under a permanent contract (Lani, 2013; Bentolila and Dolado, 1994).

To determine the extent to which firm-level bargaining accounts for the differences in wages between temporary and permanent workers, we rely on a model of differential rent-sharing.[51] Leveraging from within-person, within-employer transitions from temporary to permanent contracts, we find that around 79% of the raw return associated with such a

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[49]Several other economic forces may explain the existence of a gap in wages between temporary and permanent contract workers, including compensating differentials (Rosen, 1986), asymmetric information (Terviö, 2009), and efficiency wages (Bulow and Summers, 1986; Rebitzer and Taylor, 1991).

[50]Card et al. (2014) show that the median premium above minimum thresholds 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).

[51]The analysis that follows abstracts from an important channel that can also explain the existence of a contract gap: the sorting channel (i.e., the fact that temporary workers might be disproportionately concentrated into lower productivity firms).

transition can be explained by differences in bargaining power.

7.1 Measurement Framework

Let t_i^* denote the year in which individual i experiences for the first time in her career a temporary-to-permanent transition that occurred within the same employer. We begin by estimating the following event-study model:

$$w_{it} = \alpha_i + W_{it}^\top \phi + \sum_{k=a}^{b} \theta_k D_{it}^k + \varepsilon_{it}, \tag{7}$$

where w_{it} is the log daily wage paid by the employer who paid the most individual i in period t; α_i is an individual fixed effect; W_{it} is a vector of controls that includes a quadratic term in potential experience, year effects interacted with gender, a dummy for Italian worker, age at entry, and 1-digit industry code; and the event-time dummies $D_{it}^k = \mathbf{1}\{t = t_i^* + k\}$ define time relative to the year of the event t_i^*.

Equation (7) is estimated on the sample of individuals who at some point in their career experience a within-employer transition from a temporary to a permanent contract and were always employed with a temporary contract prior to this transition. Our interest lies in the coefficients $\{\theta_k\}$. These event-study coefficients capture the within-person evolution of wages before and after the employer converts the worker into a permanent position.

The inclusion of the large set of year effects interacted with observable characteristics, denoted by W_{it} in equation (7), implies that counterfactual wages for a worker who in year t moves from a temporary to a permanent contract within the same employer are constructed using a control worker with the same observable characteristics as the treated worker except that the control worker is still employed under a temporary contract in year t. Clearly, such counterfactual wages might be misspecified. We therefore use years prior to conversion to a permanent contract to assess the validity of these counterfactuals. These pre-trends are also somewhat useful for discussing alternative mechanisms such as learning models (Gibbons et al., 2005).

To investigate the importance of firms' wage-setting power in explaining the wage gap between temporary and permanent workers, we combine the event-study variation in equation (7) with a dynamic rent-sharing model, i.e.,

$$w_{it} = \alpha_i + W_{it}^\top \phi + \sum_{k=a}^{b} \gamma_k (S_{J(i,t),t}^k \times D_{it}^k) + \epsilon_{it}, \tag{8}$$

where S_{ij} is a measure of surplus per worker available at employer j and $J(i, t)$ is a function that returns the identity of the employer hiring individual i and time t. Specifications such

as (8) can be rationalized by simple bargaining models that assume different bargaining power across temporary and permanent contract workers.[52] We follow the rent-sharing literature and use log value added per worker as a proxy for $S_{iJ(i,t)}$; see also Card et al. (2018).

The coefficients $\{\gamma_k\}$ capture the evolution of the rent-sharing coefficients in event time. Our primary interest is assessing how these rent-sharing coefficients change once the same worker moves from a temporary to a permanent contract within the same employer. Note that γ_0 captures the rent-sharing coefficient in the first year in which the worker's temporary contract is converted to a permanent contract while γ_{-1} is the rent-sharing coefficient when the worker is on her last spell under the temporary employment contract. An Oaxaca decomposition of the adjusted wage change, $\Delta w_{it} \equiv w_{it} - w_{it-1}$, experienced at the time of the event based upon (8) is

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\begin{aligned}
\mathrm{E}[\Delta w_{it} - \Delta W'_{it}\phi|t = t^*(i)] &= \gamma_0\, \mathrm{E}[S_{J(i,t),t}|t = t_i^*] - \gamma_{-1}\, \mathrm{E}[S_{J(i,t),t-1}|t = t_i^* - 1] \quad (9)\\
&= \underbrace{\mathrm{E}[S_{J(i,t),t}|t = t^*(i)](\gamma_0 - \gamma_{-1})}_{\text{Bargaining Component}} + \underbrace{\gamma_{-1}\, \mathrm{E}[\Delta S_{J(i,t),t}|t = t^*(i)]}_{\text{Surplus Component}},
\end{aligned}
$$

where the first term captures how much of the within-person adjusted change in wage occurring at $t = t^*(i)$ is due to a change in the worker's bargaining power after moving from a temporary to a permanent contract while the second term captures the contribution due to change in the available surplus at the time of the event.[53]

In what follows, we start by reporting the within-person, within-employer event-study results of moving from a temporary to a permanent contract on wages using equation (7). Based on these results, we then try to assess the importance of firms' wage-setting power in explaining the difference in earnings between temporary and permanent workers, by estimating equation (8) and providing the associated Oaxaca decomposition highlighted in (9).

7.2 Results

The rate at which per-worker surplus maps into higher wages is vastly different across temporary vs. permanent workers. Figure C.10 shows a bin scatter plot of average log daily wages for temporary vs. permanent workers within different percentile bins of log

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[52]A version of this model is presented in Appendix D.5.5. Alternatively, a specification such as equation (8) can be obtained in wage-posting models where temporary workers' turnover rates with respect to firm-specific wage premiums differ from permanent workers' turnover rates. See Manning (2011) for an overview.

[53]The results are virtually identical to those of equation (9) when we use the alternative Oaxaca decomposition.

value added per worker. The contract gap in wages is basically null among low valued-added firms but grows as we look at more productive firms, consistent with a model of differential rent-sharing (Card et al., 2018). This evidence, however, is only descriptive. To account for high-wage workers sorting into more productive firms, we focus on within-employer, within-person transitions from a temporary to a permanent contract (henceforth abbreviated to $T \to P$) using the event-study design in equation (7), and plot the resulting estimates in Panel (a) of Figure 8.

Pre-trends leading to the transition $T \to P$ are relatively flat, suggesting that employer learning à la Gibbons et al. (2005) is not reflected in the pre-conversion wage dynamics.[54] When the worker obtains a permanent job, we observe a sudden increase of log-wages of around 6%.[55] Interestingly, the post-reform 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.

Panel (b) plots the rent-sharing coefficients γ_k in equation (8). While the worker is under a temporary contract, these rent-sharing coefficients exhibit a flat profile centered at 0.03. Once the worker is converted into a permanent contract, the rent-sharing coefficient jumps to approximately 0.05, consistent with prior estimates (Guiso et al., 2005; Card et al., 2014). Interestingly, the rent-sharing coefficients continue to grow following the conversion into permanent, consistent with models that assume firm-specific returns to seniority (Margolis, 1996; Abowd et al., 1999). Table 8 provides the Oaxaca decomposition of the wage change following the within-firm $T \to P$ transition. The difference in bargaining power associated with a temporary vs. a permanent contract explains around 79% of the unadjusted average wage increase experienced at the time of the transition. Conversely, the surplus component defined in equation (9) does not appear to have any explanatory power. Table C.11 shows that we obtain virtually identical results when focusing on full-time workers.

To sum up: Temporary contract workers receive about 68% of the rents traditionally shared by firms with permanent contract workers. The exclusion of temporary workers from firm-level agreements explains 79% of the wage contract gap and is consistent with the large decreases in firms labor costs documented in Section 6.1. All in all, the evidence therefore suggests that temporary workers are situated outside the traditional "boundary

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[54]Instantaneous learning where a worker is suddenly revealed to be highly productive and whose temporary contract is therefore converted to a permanent contract can also generate flat pre-trends prior to the conversion. Such instantaneous learning is hard to detect in typical datasets. However, a few empirical estimates suggest that employer learning occurs over a long period of time (Lange, 2007).

[55]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.11.

of the firm."

8 Conclusions

We analyze an Italian reform that relaxes several legal constraints on hiring temporary contract workers without affecting the employment protection granted to workers hired under permanent employment contracts. Even though the reform significantly increased the share of temporary employment contracts, it failed to increase aggregate employment since new temporary jobs were mostly substituting existing permanent ones. Nevertheless, the reform had both winners and losers. Firms were among the main winners. Driven by significant reductions in firms' labor costs, their profits and profit margins increased after this reform. The biggest losers were the young incumbent workers and the new entrants into the labor market, since they sustained the most significant earnings losses once the reform was implemented.

We believe that our findings can help inform the current policy debate both in the US (e.g., Irwin, 2017) and in Europe (e.g., Alderman, 2017), as temporary work and alternative employment arrangements are becoming increasingly popular (Weil, 2014). Nevertheless, many questions remain open. We are only able to capture effects for a limited number of years after the reform was introduced. Thus, future research is needed to understand the long-run effects. Moreover, the EPL reform studied in this paper is a partial one since it does not affect employment protection granted to workers hired under permanent contracts. Thus, future research is needed to study what happens when EPL reform is total, i.e., whether relaxing constraints on hiring under temporary contracts and reducing employment protection under permanent contracts generates different responses from the ones described in this paper.

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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	11,659	28,530	11,639	28,541
	(10,420)	(22,814)	(10,356)	(22,889)
Total Days Worked	175	298	175	299
	(111)	(101)	(111)	(101)
Log Daily Wage	4.10	4.41	4.10	4.41
	(0.41)	(0.53)	(0.40)	(0.53)
Age and Duration				
Age	32.2	39.8	32.2	39.8
Fraction under 30 yrs old	0.48	0.16	0.48	0.15
Fraction over 50 yrs old	0.06	0.16	0.06	0.16
Age at Entry	24.4	27.7	24.4	27.6
Tenure	1.8	6.5	1.8	6.5
Worker and Workplace Characteristics				
Female	0.41	0.29	0.42	0.32
Full Time	0.77	0.91	0.77	0.87
Employed via Temporary Work Agency	0.12	0.00	0.12	0.00
# of Jobs in the Year	1.7	1.2	1.7	1.3
Value Added per Worker	40,076	55,388	40,140	55,379
Firm Size	17.9	27.3	18.0	21.6
Number of Persons	3,117,592	5,009,195	3,088,994	4,963,242
Number of Person-Year Observations	9,287,049	46,493,228	9,122,129	45,403,514
Total Number of Person-Year Observations	59,054,022		57,755,111	

Note: This table provides summary statistics for the two most popular types of employment contracts available in the Italian labor market: temporary and permanent contracts (summary statistics for apprenticeships and seasonal contracts not shown). A worker-year cell is assigned to a temporary or a permanent contract and to a corresponding employer on the basis of the job that paid the most to the worker in that particular year. Columns 3-4 focus on person-year observations for which we have no missing information on the CCNL associated with the dominant job. Total earnings and total days worked refer to the sum of labor earnings and days worked, respectively, across all jobs in a given year. Earnings, log daily wages and value added per worker are in 2010 euros. Value added per worker is reported only for worker-year pairs where the associated employer has financial information collected in the CERVED sample. Number of persons refers to the number of individuals that at any point in their career had a temporary or permanent contract. Number of person-year observations counts the number of worker-year pairs assigned to either a temporary or a permanent contract. Total number of person-year observations report the total number of observations available in the data (counting individuals associated with temporary or a permanent contract in a given year as well as individuals associated with an apprenticeships or seasonal contract). Standard deviations are in parentheses. Source: INPS-INVIND, 1998-2013.

Table 2: The Impact of the Reform on Job Flows

	Turnover Rate	Within Employer Transitions		Across Employers Transitions		
		Temporary	Permanent	Temporary	Permanent	Non-Employed
	[1]	[2]	[3]	[4]	[5]	[6]
Panel (a): All Temp Jobs						
Medium-Run Effect of the Reform	0.0349***	-0.0055	-0.0307***	0.0318***	0.0031	0.0000
	(0.0065)	(0.0065)	(0.0046)	(0.0050)	(0.0035)	(0.0029)
Pre-Reform Mean	.37	.36	.27	.12	.16	.09
# of Job-Year Observations	7,515,085	7,515,085	7,515,085	7,515,085	7,515,085	7,515,085
Panel (b): Expiring Temp Jobs						
Medium-Run Effect of the Reform	0.0565***	0.0174	-0.0763***	0.0486***	-0.0051	0.0129**
	(0.0113)	(0.0135)	(0.0114)	(0.0083)	(0.0064)	(0.0056)
Pre-Reform Mean	.24	.26	.49	.07	.12	.04
# of Job-Year Observations	2,082,635	2,082,635	2,082,635	2,082,635	2,082,635	2,082,635
Panel (c): Expiring Temp Jobs, Young Workers						
Medium-Run Effect of the Reform	0.1006***	-0.0279*	-0.0759***	0.0738***	0.0076	0.0192***
	(0.0175)	(0.0166)	(0.0167)	(0.0136)	(0.0105)	(0.0064)
Pre-Reform Mean	.25	.28	.46	.09	.12	.04
# of Job-Year Observations	457,472	457,472	457,472	457,472	457,472	457,472
Panel (d): Permanent Jobs						
Medium-Run Effect of the Reform	-0.0079	0.0013*	0.0063	0.0006	-0.0138	0.0052
	(0.0125)	(0.0007)	(0.0125)	(0.0061)	(0.0122)	(0.0055)
Pre-Reform Mean	.2	.01	.78	.03	.12	.06
# of Job-Year Observations	42,507,190	42,507,190	42,507,190	42,507,190	42,507,190	42,507,190

Note: This table reports the coefficient θ_3 defined as ``Medium-Run Effect of the Reform'', which corresponds to the effect of the reform measured 4+ years after the enactment of the reform, see equation (2). Panel (a) estimates equation (2) on the observed sample of temporary jobs. Panel (b) estimates equation (2) only for temporary jobs that in year t already existed for two or more years. Panel (c) uses the same sample of Panel (b) but further conditions on temporary jobs where the worker is 25 or less years of age. Column 1 reports estimates from equation (2) when the dependent variable is an indicator equal to 1 if the job observed in year t is destroyed in year $t+1$. We also report how the reform impacted year-to-year individual transition rates within/across employers interacted with different contract types (temporary/permanent) for the type of jobs described in each panel. Pre-reform mean reports the average turnover rate (column 1) or transition rate (columns 2–6) observed in the pre-reform period. Effects on transitions into apprenticeships or seasonal contracts are not displayed. Standard errors are displayed in parentheses and are clustered at the LLM level.

Table 3: Dual Labor Markets and the Evolution of the Contract Gap

	Baseline	Including Part-Time	Female	Male	Age≤25
	[1]	[2]	[3]	[4]	[5]
Panel (a): Contract Gap in Days Worked					
Short-Run Effect of the Reform	5.71***	5.44***	5.61***	5.90***	9.23***
	(0.53)	(0.50)	(0.60)	(0.66)	(0.78)
Medium-Run Effect of the Reform	21.64***	24.13***	32.54***	18.19***	24.84***
	(1.20)	(1.07)	(1.15)	(1.56)	(1.30)
Pre-Reform Adjusted Gap	69.07	72.31	68.26	68.57	54.64
# of Job-Year Observations	46,414,861	54,362,154	12,085,130	34,328,578	4056418
Panel (b): Contract Gap in Earnings					
Short-Run Effect of the Reform	211.9***	95.41*	335.8***	188.6***	519.3***
	(49.72)	(49.05)	(48.99)	(61.74)	(52.82)
Medium-Run Effect of the Reform	1,831***	1,574***	2,689***	1,563***	1,708***
	(97.59)	(88.11)	(98.56)	(129.3)	(90.95)
Pre-Reform Adjusted Gap	8,897	8,379	7,121	9,523	4,098
# of Job-Year Observations	46,414,861	54,362,154	12,085,130	34,328,578	4,056,418

Note: This table reports the coefficient θ^P_0, —defined as the ``Short-Run Effect of the Reform"—and $\theta^P_3$, —defined as the ``Medium-Run Effect of the Reform"—from equation (3). The former refers to the effect of the reform at enactment, while the latter refers to the effect 4+ years after the implementation of the reform. In Panel (a), the outcome variable in equation (3) is annual days worked. Panel (b) presents results from equation (3) using annual earnings as outcome. In all columns, with the exception of column 2, equation (3) is estimated on the universe of full-time temporary and permanent jobs that represented the highest paying occupation of an individual in a given year. Column 2 adds part-time jobs as well. Column 3 estimates the baseline specification of column 1 but for females only, while column 4 for male only. Column 5 restricts the estimation to jobs where the associated worker is 25 years old or less. All models control for a quadratic in potential experience, Italian nationality and gender dummies. Pre-reform adjusted gap is the difference in a given outcome between permanent and temporary jobs as measured by the coefficient θ from equation (3). Standard errors are clustered at the LLM market level.

Table 4: The Effects of the Reform on Firms' Outcomes

	Share of Temp Contract Employees	Log Firm Size	Value Added per Worker	Labor Costs per Worker	Profits per Worker	Profit Margin	Log Value Added per Worker	Log Labor Costs per Worker	Firm Exit
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Short Run Effect of the Reform	0.0092***	-0.0015	-311	-396***	170	0.0040*	-0.0060	-0.0113***	0.0045
	(0.0025)	(0.0061)	(193)	(78)	(105)	(0.0022)	(0.0040)	(0.0027)	(0.0035)
Medium Run Effect of the Reform	0.0491***	-0.0047	-652	-831***	788***	0.0098**	-0.0203**	-0.0281***	0.0078
	(0.0067)	(0.0123)	(520)	(201)	(292)	(0.0042)	(0.0086)	(0.0065)	(0.0063)
Pre-reform Mean	.15	2.24	52,750	29,465	9,111	.12	3.78	3.29	.03
Observations	1,199,410	1,199,410	1,199,410	1,199,410	1,199,410	1,199,410	1,199,410	1,199,410	1,199,410

Note: This table reports the coefficient θ_0—defined as the ``Short-Run Effect of the Reform"—and $\theta_3$—defined as the ``Medium-Run Effect of the Reform"—from equation (4). The former refers to the effect of the reform at enactment, while the latter refers to the effect 4+ years after the implementation of the reform. The estimation sample— labeled as INPS-INVIND-CERVED—corresponds to all firms in our baseline matched employer-employee dataset for which we can match income statements contained in CERVED and that were already present in the labor market prior to the implementation of the reform. We use the dominant CCNL in the pre-reform period to define our event-study indicators. See text for details. Profit margin is defined as profits divided by value added. Value added, labor costs and profits are all in real terms using a 2010 CPI. Firm exit is a dummy equal to one if a particular firm-year corresponds to the last year in which we observe the firm in the data and, therefore, captures firm attrition in the estimation sample. All displayed results control for firm fixed effects and province-by-year fixed effects. Results are weighted using inverse propensity score weights designed to match the sector composition and firm-size pre-reform distribution observed in the INPS-INVIND-CERVED sample to the one observed in our original sample of firms. Standard errors are displayed in parentheses and clustered at the LLM level.

Table 5: Heterogeneity in the Firm-Level Analysis

	Share of Temp Contract Employees	Log Value Added per Worker	Log Labor Costs per Worker	Profit Margin
Panel (a): Firms in the North				
Medium-Run Effect of the Reform	0.0482***	-0.0192*	-0.0292***	0.0169***
	(0.00914)	(0.0116)	(0.0089)	(0.0060)
Pre-reform Mean	.14	3.86	3.35	.13
Observations	643,679	643,679	643,679	643,679
Panel (b): Firms in the Center				
Medium-Run Effect of the Reform	0.0589***	-0.0292*	-0.0286**	0.0035
	(0.0120)	(0.0155)	(0.0115)	(0.0069)
Pre-reform Mean	.18	3.74	3.26	.11
Observations	383,008	383,008	383,008	383,008
Panel (c): Firms in the South				
Medium-Run Effect of the Reform	0.0243	0.0037	-0.0158	-0.0030
	(0.0149)	(0.0209)	(0.0143)	(0.0099)
Pre-reform Mean	.13	3.59	3.13	.09
Observations	172,090	172,090	172,090	172,090
Panel (d): High-Fraction of Older Workers				
Medium-Run Effect of the Reform	0.0542**	0.00239	-0.0554***	0.0245*
	(0.0228)	(0.0268)	(0.0194)	(0.0132)
Pre-reform Mean	.12	3.87	3.39	.11
Observations	59,716	59,716	59,716	59,716
Panel (e): High-AKM Firm Effects				
Medium Run Effect of the Reform	0.0606***	-0.00479	-0.0354***	0.0103
	(0.0123)	(0.0164)	(0.0113)	(0.0075)
Pre-reform Mean	.14	4.04	3.53	.13
Observations	267,094	267,094	267,094	267,094
Panel (f): High Hold-up Power				
Medium Run Effect of the Reform	0.0145	0.0151	0.0021	0.0058
	(0.0141)	(0.0182)	(0.0092)	(0.0097)
Pre-reform Mean	.14	3.85	3.37	.12
Observations	93,877	93,877	93,877	93,877
Panel (g): Low Hold-up Power				
Medium Run Effect of the Reform	0.0555***	-0.0207*	-0.0297***	0.0192***
	(0.00957)	(0.0117)	(0.0092)	(0.0057)
Pre-reform Mean	.16	3.73	3.24	.11
Observations	587,975	587,975	587,975	587,975

Note: This table reports the coefficient θ_0—defined as the ``Short-Run Effect of the Reform"—and $\theta_3$—defined as the ``Medium-Run Effect of the Reform"—from equation (4). The former refers to the effect of the reform at enactment, while the latter refers to the effect 4+ years after the implementation of the reform estimated separately for each group described in Panel (a)-(g). The estimation sample—labeled as INPS-INVIND-CERVED—corresponds to all firms in our baseline matched employer-employee dataset for which we can match income statements contained in CERVED and that were already present in the labor market prior to the implementation of the reform. We use the dominant CCNL in the pre-reform period to define our event-study indicators. See text for details. Panel (a)-(b)-(c) estimate our baseline model separately for firms in the North, Center and South. In Panel (d), we estimate the event study conditional on firms that in the pre-reform period have 25% or more of their employees were 50 years or older. In Panel (e), we condition on firms where their estimated AKM effect—computed using only data from 1990-2001—belongs to the fourth quartile of the corresponding AKM firm effects distribution. In Panel (f), we condition on firms whose dominant CCNL has an average tenure that belongs to the last quartile of the corresponding person-year weighted distribution, calculated using pre-reform data only. In Panel (g), we condition on firms whose dominant CCNL has an average tenure that belongs to the first quartile of the corresponding person-year weighted distribution, calculated using pre-reform data only. All displayed results control for firm fixed effects and province-by-year fixed effects. Results are weighted using inverse propensity score weights designed to match the sector composition and firm-size pre-reform distribution observed in the INPS-INVIND-CERVED sample to the one observed in our original sample of firms. Standard errors are displayed in parentheses and clustered at the LLM level.

Table 6: The Effects of the Reform on Incumbent Temporary Workers

	Employed	Earnings	Days Worked	Conditional on Employment			Same Employer & Permanent Contract	Value Added per Worker
				Earnings	Days Worked	Same Employer		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel (a): Incumbent Temporary Workers								
Short Run Effect of the Reform	-0.0019	-266.2***	-7.078***	-348.0***	-7.982***	-0.0311***	-0.0397***	-3,990***
	(0.0033)	(83.39)	(1.269)	(86.99)	(1.130)	(0.00754)	(0.0061)	(879.9)
Medium Run Effect of the Reform	-0.0344**	-778.7**	-22.69***	-633.6**	-17.73***	-0.0312**	-0.0873***	-5,174***
	(0.0167)	(327.4)	(6.428)	(251.4)	(2.985)	(0.0144)	(0.0164)	(1822)
Pre-reform Mean Outcome	.97	11,054	170.1	11,434	176.0	.6	.27	41,817
# of Person-Year Observations	1,636,894	1,636,894	1,636,894	1,507,089	1,507,089	1,507,089	1,507,089	1,010,604
Panel (b): Incumbent Young Temporary Workers								
Short Run Effect of the Reform	-0.0071*	-420.9***	-8.756***	-456.1***	-9.277***	-0.0324***	-0.0397***	-3,760***
	(0.0037)	(108.9)	(1.442)	(106.9)	(1.357)	(0.00741)	(0.0063)	(984.4)
Medium Run Effect of the Reform	-0.0460***	-1,199.2***	-24.13***	-923.9***	-18.77***	-0.0472***	-0.117***	-5,970***
	(0.0131)	(384.2)	(5.349)	(320.2)	(3.522)	(0.0147)	(0.0176)	(2260)
Pre-reform Mean Outcome	.97	9,895	158.9	10,227	164.2	.59	.20	39,288
# of Person-Year Observations	709,053	709,053	709,053	651,959	651,959	651,959	651,959	435,749

Note: This table reports the coefficient θ_0—defined as the ``Short-Run Effect of the Reform"—and $\theta_3$—defined as the ``Medium-Run Effect of the Reform"—from equation (5). The former refers to the effect of the reform at enactment, while the latter refers to the effect 4+ years after the implementation of the reform. Panel (a) estimates this equation on the sample of incumbent temporary workers (i.e., workers that in the pre-reform period were always employed under a temporary employment contract and were 40 or less years of age in 2001). Outcomes from columns 1,2 and 3 do not condition on employment and we assign 0 days worked and 0 earnings to non-employment spells. See text for further details. Results from columns 4–8 condition on employment. Value added per worker is the average value added calculated for a given firm over the sample period; this measure is only available for firms belonging to the CERVED sample, see text for details. Same employer is a dummy equal to 1 if the worker did not switch employer. Same employer & permanent contract is a dummy equal to 1 if the worker remained with the same employer of the previous period and is currently hired under a permanent contract. Panel (b) estimates the specification in equation (5) using the sample of Panel (a) but further restricting it to individuals aged 25 or younger in 2001. We report the average in the pre-reform for a given panel and outcome for all columns. The pre-reform mean outcome reported in column 7 corresponds the unconditional share of individuals that in the pre-reform transitioned from a temporary into permanent job within the same employer in our overall sample (and restricted to workers 25 years of age or younger for Panel (b)). Standard errors displayed in parentheses and clustered at the LLM level.

Table 7: The Effects of Entering the Labor Market under the Reform on the Experience Profile

	Earnings	Permanent Contract	Same Employer	Same Employer & Permanent Contract	Employment	Value Added per Worker
	[1]	[2]	[3]	[4]	[5]	[6]
Reform x First Year of Entry	-429.2746***	-0.0746***	-0.0231***	-0.0129***	0.0000	-1.05e+03
	(63.1683)	(0.0042)	(0.0030)	(0.0029)	(.)	(1048.1898)
Reform x 2 Years after Entry	-311.7380***	-0.0185***	-0.0123***	-0.0095***	-0.0059**	-135.3027
	(58.8934)	(0.0033)	(0.0025)	(0.0026)	(0.0023)	(997.2775)
Reform x 3 Years after Entry	-216.3115***	-0.0114***	-0.0079***	-0.0080***	-0.0039*	-404.3455
	(58.4093)	(0.0030)	(0.0023)	(0.0025)	(0.0022)	(1046.2161)
Reform x 4 Years after Entry	-196.2133***	-0.0047*	-0.0033	-0.0102***	-0.0024	-3.39e+03***
	(60.8393)	(0.0028)	(0.0022)	(0.0024)	(0.0021)	(1141.4326)
Reform x 5 Years after Entry	-205.3657***	-0.0030	0.0013	-0.0113***	-0.0037*	-2.36e+03*
	(67.4090)	(0.0029)	(0.0022)	(0.0025)	(0.0020)	(1232.3639)
Reform x 6 Years after Entry	-166.0648**	-0.0018	0.0044*	-0.0136***	-0.0002	-2.87e+03**
	(75.7157)	(0.0033)	(0.0023)	(0.0026)	(0.0021)	(1333.3467)
Reform x 7 Years after Entry	-107.9406	0.0004	-0.0075***	0.0034	0.0050**	659.4812
	(87.5982)	(0.0037)	(0.0026)	(0.0025)	(0.0023)	(1457.6929)
Mean Outcome in First Year of Entry Pre-Reform Cohorts	8,425	.37	.44	.2	1	63,981
# of Cells	274,787	274,787	274,787	274,787	234,733	241,038

Note: This table reports the estimated coefficients θ_s from equation (6) that capture the dynamic effect of entry into the labor market under the new reform on temporary employment contracts. The sample includes all individuals who entered the labor market between 1998–2005 and that were 30 years old or less at the moment of entry. See text for details. Regression estimates are based on cell data at the level of year of entry into the labor market, local labor market (LLM) of entry (i.e., combination of province and CCNL at the time of entry) and year of potential experience in the labor market defined as current year minus year since first entering the INPS data. Coefficients in this table represent the interaction between a dummy for whether the first job is under the reform and dummies for year since the first entry into the labor market. The model controls for LLM, province of entry by year fixed effects, potential experience fixed effects and cohort fixed effects, properly normalized. See text for details. Columns in this table represent different outcomes. Permanent contract represents the share of workers under a permanent employment contract. Same employer is the share of workers that remain with the same employer across calendar years. Employment is the share of workers employed according to social security data (by definition, this is equal to 1 for all micro-observations in the micro data in the in the first year of entry). Standard errors are displayed in parentheses and clustered at the LLM level.

Table 8: Within-Employer, Within-Person Evidence of Differential Rent Sharing

	Last Year under Temp	First Year under Perm	Difference
	[1]	[2]	[3]
[1] Log Daily Wage	4.1167	4.1989	0.0821
Components			
[2] Log Value Added per Worker	3.9600	3.9556	-0.0044
[3] Rent-Sharing Coefficient	0.0345	0.0508	0.0163
	(0.0019)	(0.0019)	(0.0004)
Oaxaca Decomposition			
[4] Surplus Component	0.1366	0.1364	-0.0002
			[0.00]
[5] Bargaining Component	0.1364	0.2010	0.0646
			[0.79]

Note: This table reports estimates using the decomposition highlighted in equation (9). 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 average log daily wage. Row 2 reports the average log value added per worker. Row 3 reports the the rent-sharing coefficients from equation (8), as plotted in Panel (b) of Figure 7. Rows 4-5 report the Oaxaca decomposition terms defined in equation (9). The terms in square brackets in column 3 represent the percentage of the raw wage gap reported in row 1, column 3 that is explained by either the surplus or the bargaining component. Estimates are based on workers that start with a temporary contract and eventually transition to a permanent contract within the same employer and we have data on the value added per worker of their employer from CERVED. Number of person-year observations is 5,668,325 with 614,926 unique person identifiers. Standard errors, clustered at the LLM level, are shown in round brackets.

Figure 1: The Reform

(a) Year of Renewal

### (b) Staggered Implementation
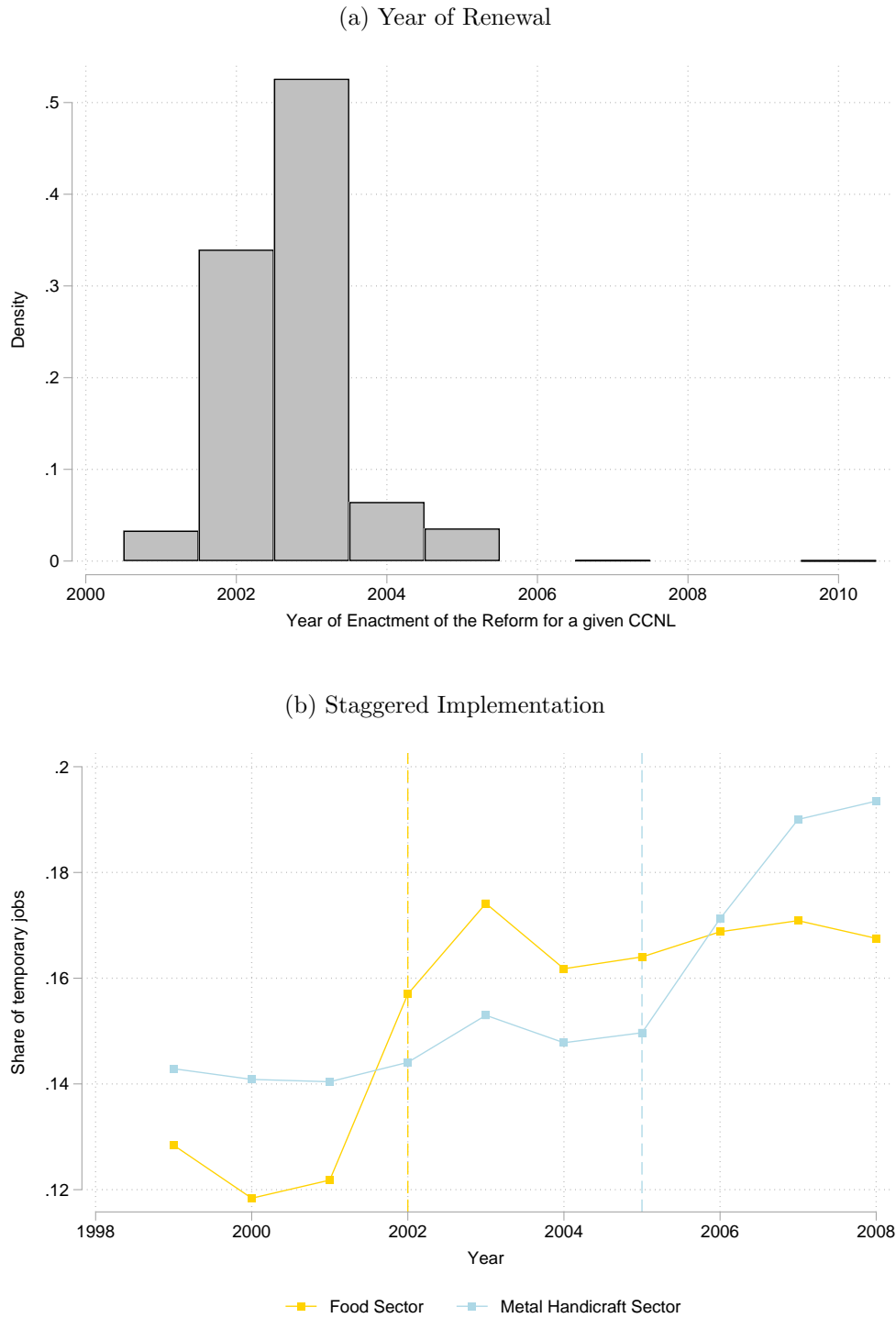
Note: Panel (a) shows the distribution of enactment years of the reform across CCNLs. See Section 4.1. Panel (b) shows the evolution of the share of temporary jobs in two CCNLs. The yellow line plots this share for the CCNLs in the Food sector, which implemented the reform in 2002. The light blue line corresponds to the Metal Handicrafts sector, which implemented the reform in 2005.

Figure 2: First Stage and Employment Effects of the Reform

(a) First Stage: Share of Temporary Jobs

Share of Temporary Jobs prior to the reform: .11
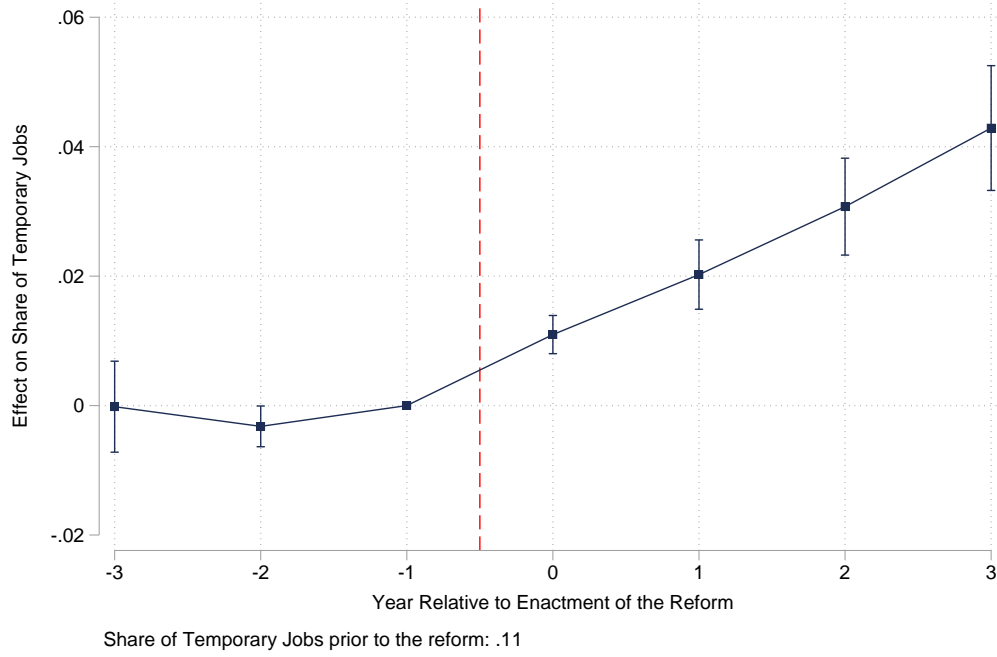
(b) Combined Effects: Share of Temp Jobs and Log Employment

■— Share of Temporary Jobs   ■--- Log Employment
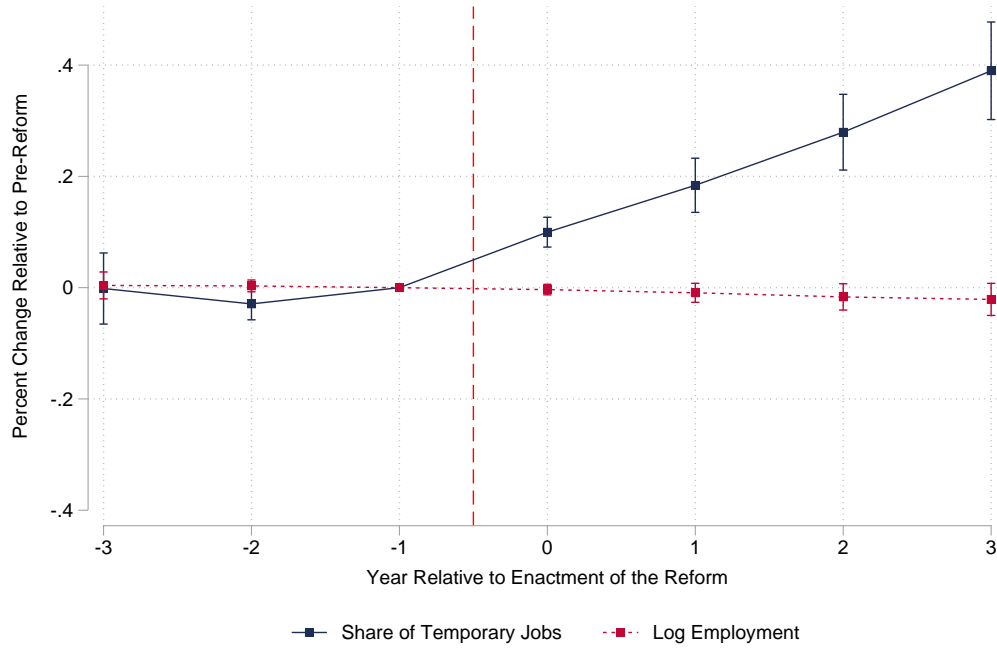
Note: Panel (a) reports the coefficients from equation (1) where the outcome variable is the share of temporary jobs. Panel (b) reports these coefficients scaled by the share of temporary jobs observed prior to the implementation of the reform as well the coefficients obtained when estimating (1) on the logarithm of total employment in a given CCNL × province × year cell. Estimation of (1) is based on 136,069 LLM by year observations with 9,845 unique LLMs. 95% confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 3: Job Creation, Job Destruction

(a) Share of New Temporary Jobs

Share of New Temporary Jobs prior to the reform: .07

(b) Turnover Rate of Temporary Jobs

Turnover Rate of Temporary Jobs prior to the reform: .24
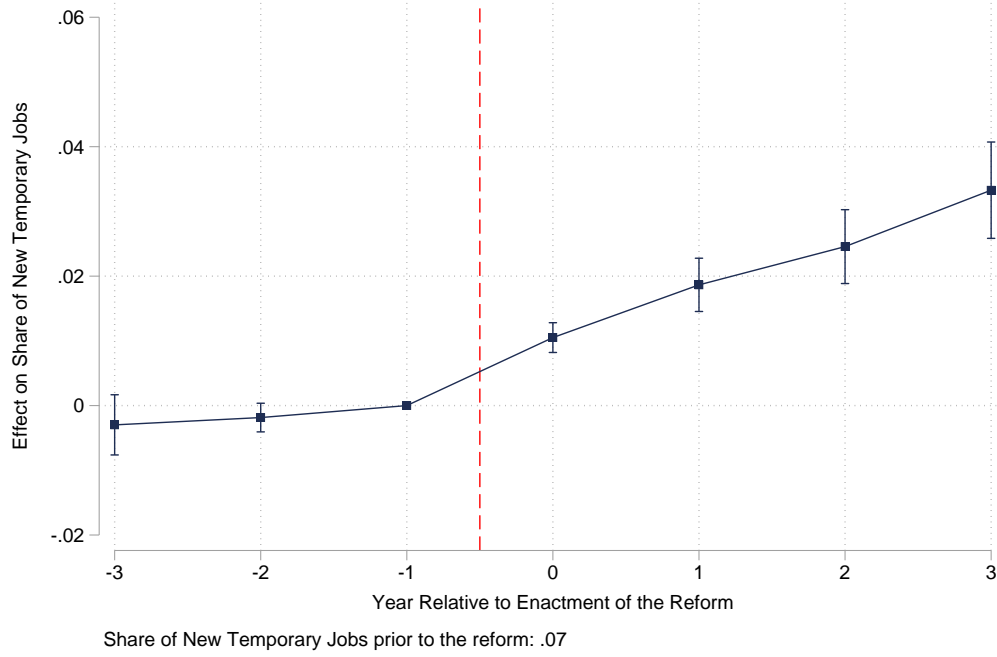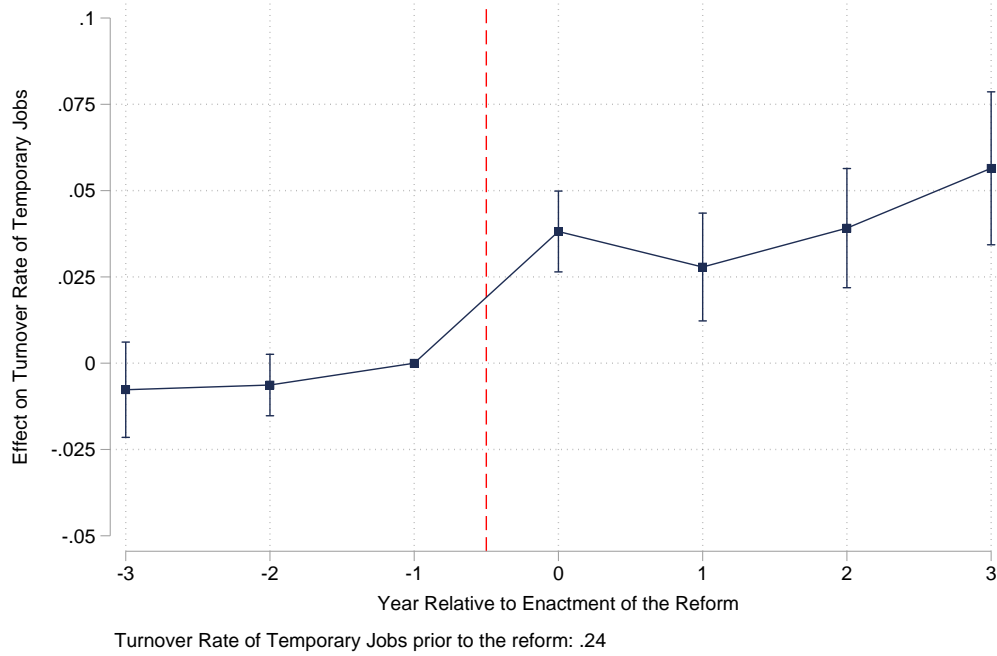
Note: This figure presents estimates from equation (2). In Panel (a), the outcome variable is an indicator variable for whether a given job j observed in year t is a new job created under a temporary employment contract. In Panel (b), the outcome variable is an indicator equal to 1 if the temporary job j observed in year t is destroyed in year $t+1$ (i.e., the match observed in period t is no longer observed in $t+1$ under a temporary or a permanent contract). The specification depicted in Panel (b) is estimated only on temporary jobs that in year t already existed for two years or more. 95% confidence intervals obtained after clustering the standard errors at the LLM level.

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Figure 4: Differences between Temporary and Permanent Jobs

(a) Annual Earnings

Gap in Earnings Permanent - Temporary prior to the reform: 8896.5

(b) Annual Days Worked

Gap in Days Worked Permanent - Temporary prior to the reform: 69.07000000000001
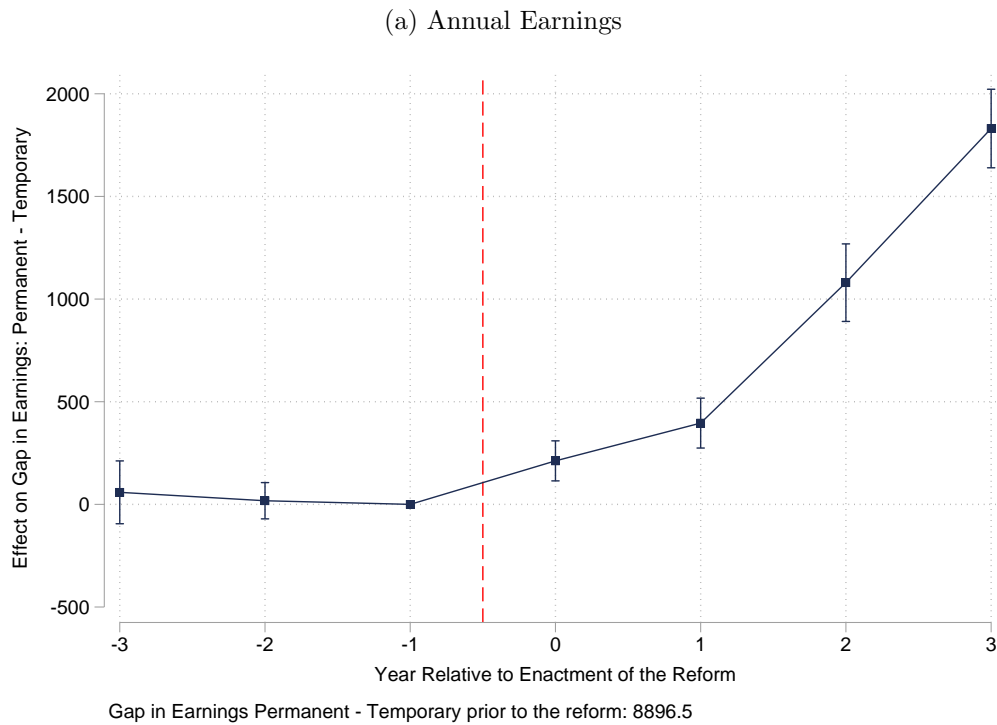
Note: Both panels display the coefficients $\{\theta_k^P\}$ from equation (3) on the outcome listed at the top of each figure. Each coefficient therefore represents the difference in a specific outcome between permanent and temporary jobs in event time. Equation (3) is estimated on the sample of full-time jobs that were under either a temporary or a permanent contract. Below each figure we report the adjusted difference in a given outcome between permanent and temporary jobs before the reform, as measured by the coefficient θ from equation (3). 95% confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 5: The Impact of the Reform on Firm-Level Share of Temps and Firm-Size
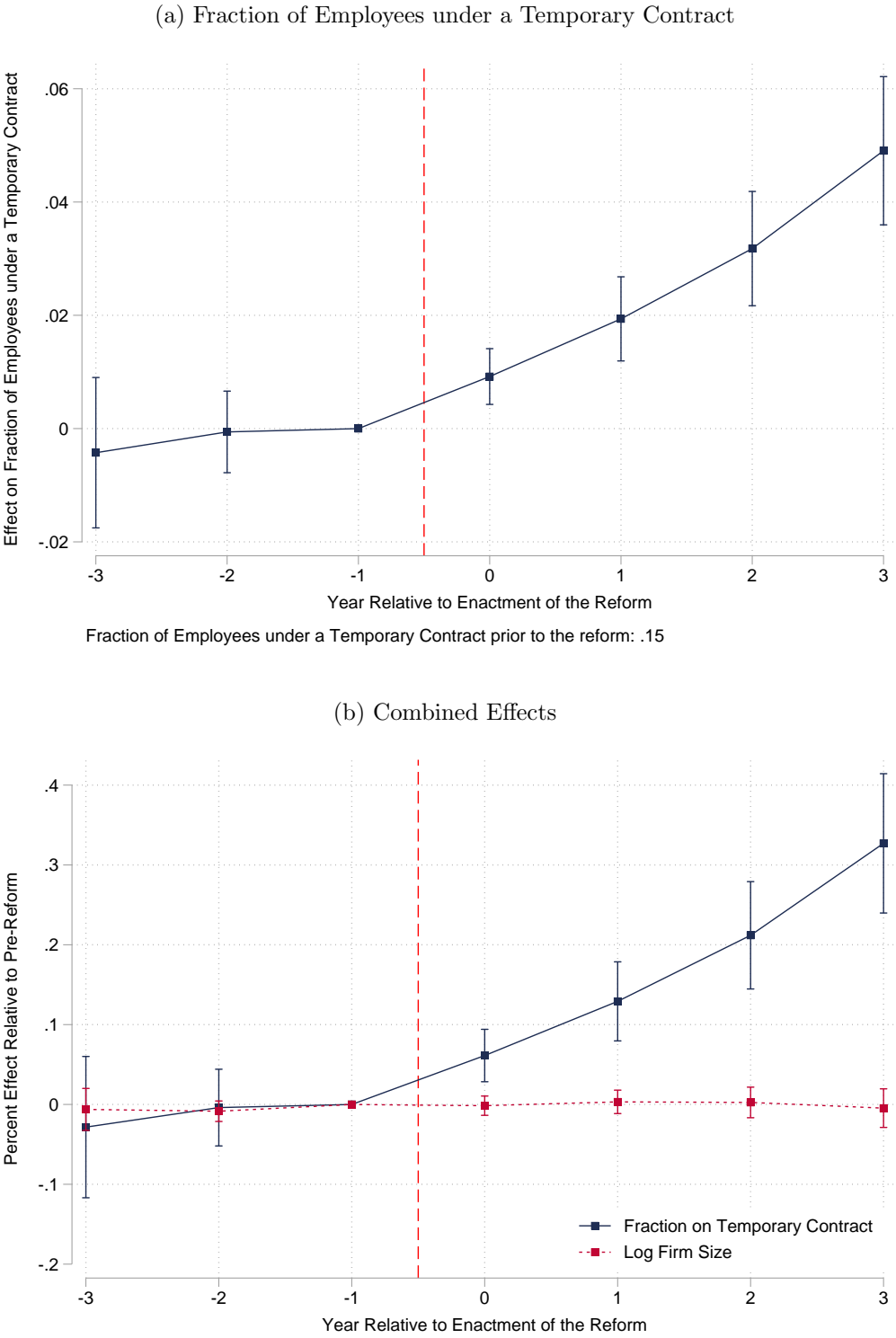
(a) Fraction of Employees under a Temporary Contract

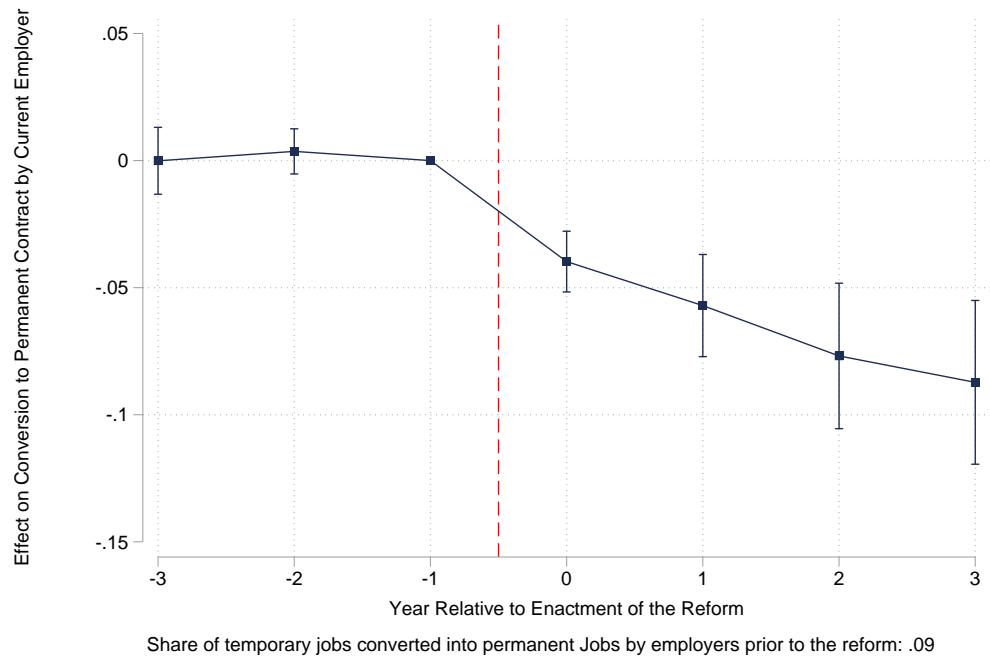Fraction of Employees under a Temporary Contract prior to the reform: .15

(b) Combined Effects

Note: This figure presents the event-study coefficients from the firm-level specification described in (4). Panel (a) shows estimates on fraction of employees under a temporary contract. Panel (b) rescales this coefficient by the fraction of temporary employees observed in the pre-reform period and reports also the event-study coefficients when the outcome is log firm size. 95% confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 6: The Impact of the Reform on Firm Labor Costs and Profit Margin
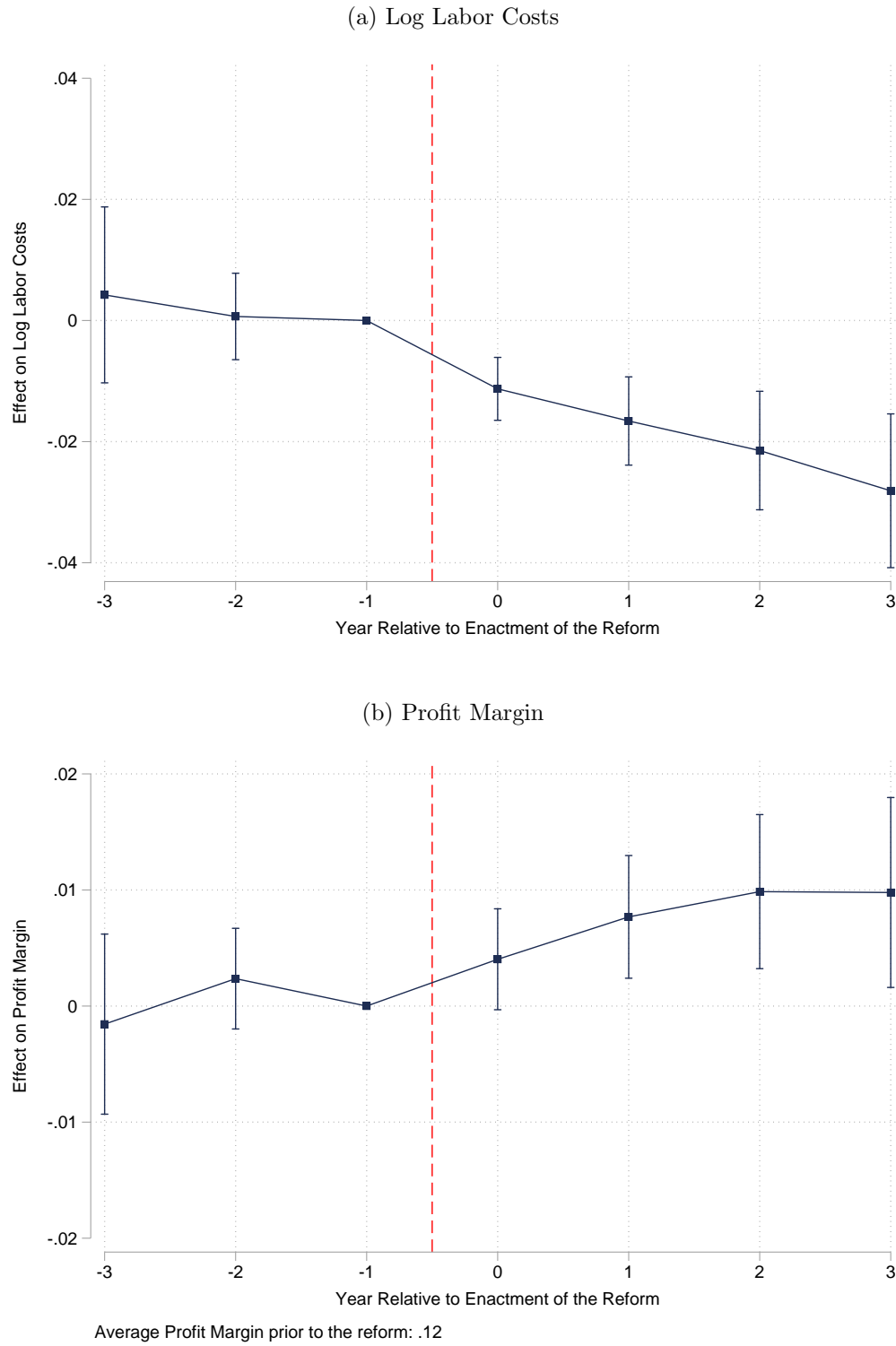
(a) Log Labor Costs

(b) Profit Margin

Average Profit Margin prior to the reform: .12

Note: This figure presents the event-study coefficients from the firm-level specification described in (4). Panel (a) shows the estimates on the logarithm of labor costs per worker. Panel (b) shows the effects on the profit margin, defined as profits divided by value added. 95% confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 7: Impact of the Reform on Incumbent Temporary Workers

(a) Annual Earnings

Earnings prior to the reform: 11433.53
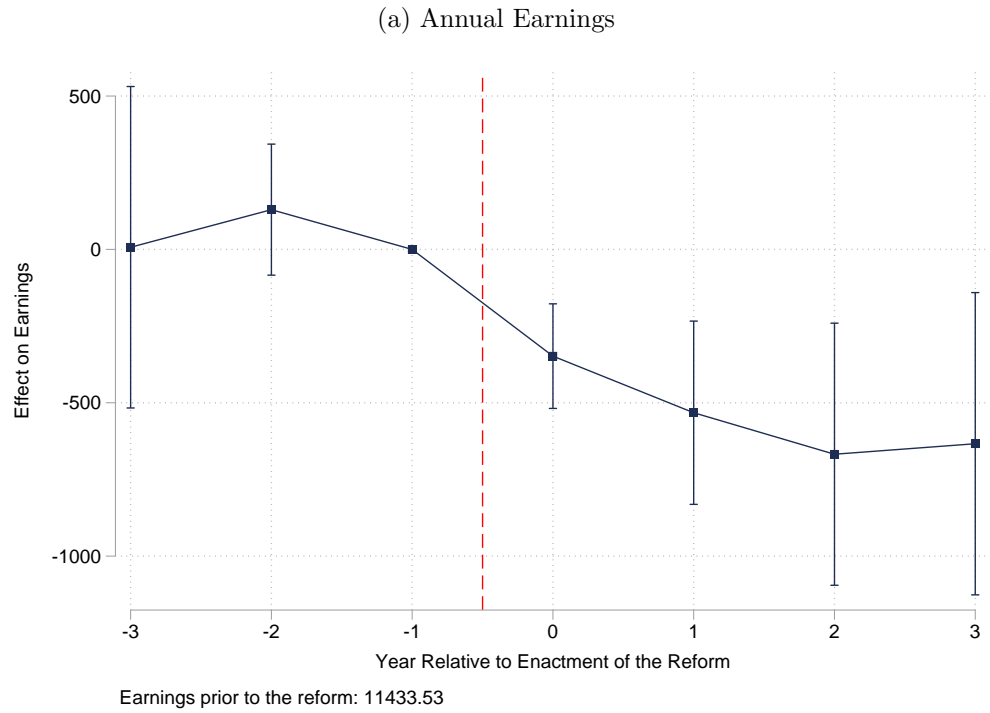
(b) Probability of Conversion from Temporary to Permanent within the Same Employer

Share of temporary jobs converted into permanent Jobs by employers prior to the reform: .09

Note: This figure displays the event-study coefficients when we estimate equation (5) on the sample of incumbent temporary workers (i.e., workers that in the pre-reform period, if employed, were always employed under a temporary employment contract and who were under the age of 40 in 2001). Panel (a) estimates (5) using as the outcome total annual earnings of individuals (conditional on being employed), while Panel (b) estimates equation (5) using as the outcome an indicator for whether the contract of an incumbent temporary worker was converted to a permanent contract by their current employer. Below this panel we report the aggregate share of temporary contracts that prior to the reform were converted to permanent contracts by employers. 95% confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 8: Within-Person, Within-Employer Transition from Temporary to Permanent Contract

(a) Event Study

(b) Rent-Sharing Coefficients
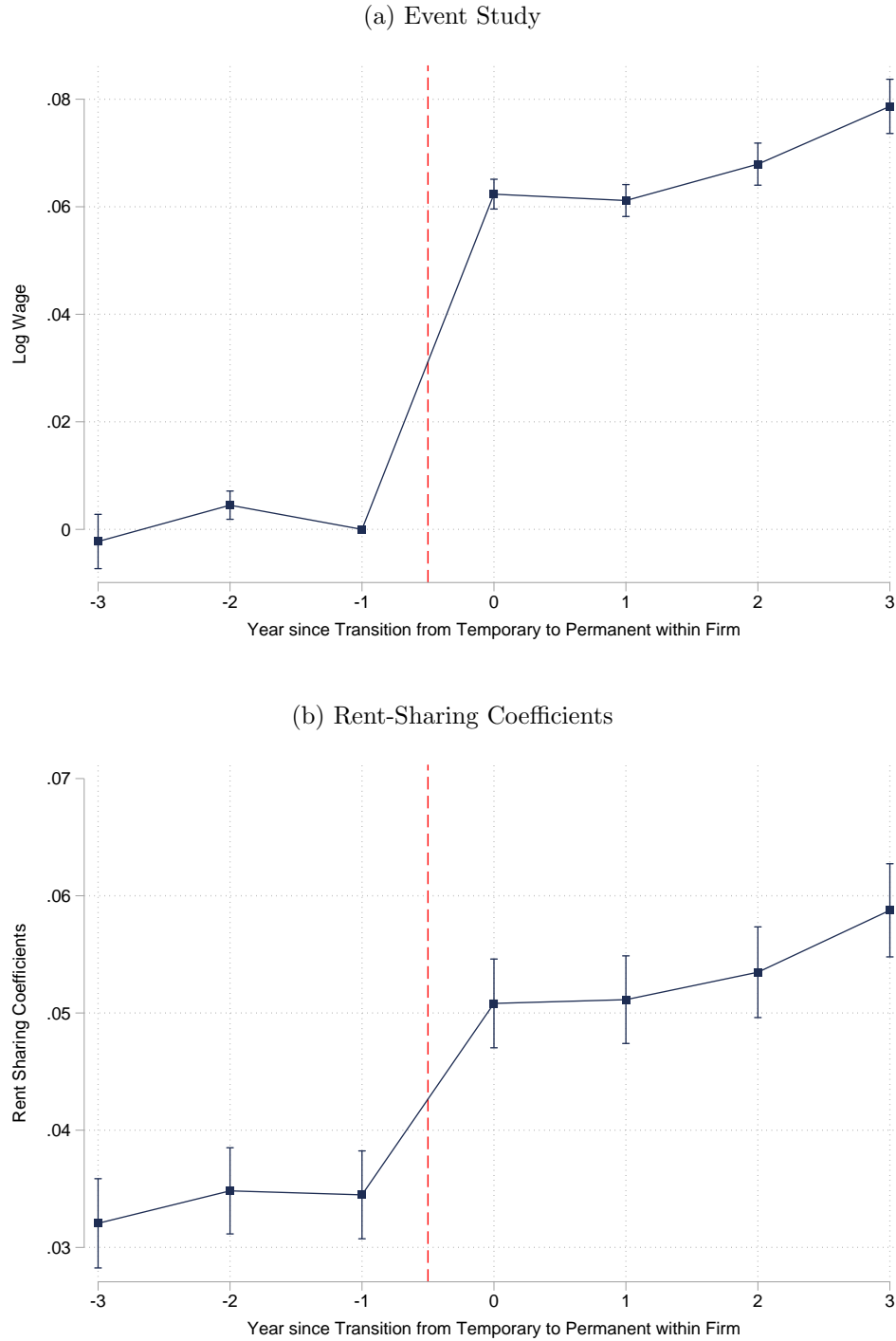
Note: Panel (a) reports the event-study coefficients from equation (7) where the outcome is log daily wages of a worker in her primary job in year t. In this regression, the event is defined as the year in which an employee transitions from a temporary to a permanent contract within the same employer. See text for details. Panel (b) reports the event-study coefficients interacted with log value added per worker. See equation (8) and text for details. All regression models control for worker fixed effects, a quadratic term in potential experience and year effects interacted with 1-digit sector codes, gender, age at entry, and Italian nationality. Estimations are based upon the sample of individuals who transitioned at least once from a temporary to a permanent contract within the same employer and were always employed under a temporary contract prior to this transition. 95% confidence intervals are obtained after clustering the standard errors at the LLM level.

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A   Data Appendix

Here we provide further details on the data used for our analysis.

A.1   Matched Employer-Employee Dataset

Our baseline information is derived from the INPS-INVIND matched employer-employee database, which provides the full employment history of individuals who at some point in their career were employed by a firm covered by the INVIND survey of the Bank of Italy for the period 1990–2013. Roughly 25% of the observed individuals were employed in a given year in an INVIND firm.

The raw data in INPS-INVIND is at the spell or job-year level. For each individual-year cell, we observe all the jobs associated with that individual. Information on each spell includes: identity of the employer, job start and separation dates, gross labor income (including bonuses and overtime), number of months/weeks worked in a year, months of employment, part-time vs. full-time status. These data are also combined with some information on the worker such as age, gender, and nationality (Italian vs. non-Italian). Information on the employment contract (apprenticeship, seasonal, temporary, and permanent) is available only from 1998. We therefore focus our analysis on the period 1998–2013. We also impose these additional restrictions: (i) we exclude public-sector employees (around 10% of the existing spell-year observations), (ii) we focus only on spells in which the worker is 16–64 years of age (dropping 0.12% of observations), and (iii) we exclude spells with an associated daily wage lower than 10 real euros (0.50%).

This leaves us with a sample of around 80 million spell-years. For our analysis, we work with a person-year version of the data in which we assign to each individual-year cell the job that paid the most in that particular year. Based on this "dominant" job, we then assign a temporary vs. permanent contract indicator to a particular worker-year observation, as well as other job-varying characteristics such as part-time/full-time status and, importantly, the associated CCNL. We use the latter to match information on the history of renewals of CCNLs collected by CNEL as discussed in Section 3.2. For the worker-level analysis, we measure the total labor market earnings of an individual in a given year, summed across all possible jobs. We do the same for days worked. Finally, we winsorize each of these measures at 1 and 99 percent.

A.2   Firms

In the complementary database that we labeled "Anagrafica" in Section 3.3, we have information on total employment (in each month) for all firms surveyed in the INPS-

INVIND matched employer-employee database as well as additional information such as sectoral code, number of establishments, and province of the main headquarters of a particular firm.

We construct a yearly measure of total employment by taking the median of the number of employees reported across months. We also calculate the share of temporary jobs as the fraction of employees hired by the firm in a given year under a temporary contract, using the micro spell-level data contained in INPS-INVIND.

Using the unique national tax identifier, we merge balance-sheet information for these firms using the CERVED database. The resulting set of matched firms are labeled INPS-INVIND-CERVED. The income statement variables that we use for the analysis are defined as follows:

- Labor costs: the cost paid by the employer to all employees. It comprises wages and salaries, social security contributions, severance package, retirement treatment, and other costs.

- Value added: the value that the firm was able to create from inputs with the production process. It is computed as: value of production – net purchases + variations of raw material stock – service and third-party asset costs. Value of production is defined as net revenue + variations in inventories of unfinished, semi-finished, and finished products + increase of asset value + operating grants.

- Profit: the value of the fiscal year profits before taxes. It is computed as: all operating revenues – all operating expenses + financial income - interest payments.

All income statements are in 2010 euros. We omit from the estimation firms that reported erratic or occasionally missing values in their main accounting variables as well as firms that reported abnormal year-to-year changes ($\geq 500\%$ change in absolute value). Finally, we winsorize value added, labor costs, and profits per worker at 1 and 99 percent.

Table C.5 describes our sample construction for the firm-level analysis. Column 1 reports the industry composition as well as average firm size of all the firms observed in INPS-INVIND that existed prior to the reform. The statistics refer to the pre-reform era. In column 2, we report aggregate statistics for these incumbent firms for which we have a match in the CERVED database. As expected, these firms tend to be larger and a small fraction of them are in the Handicraft sector. In column 3, we reweight the statistics of the INPS-INVIND-CERVED sample using a propensity score reweighting strategy. These weights are calculated to match the share of firms in each sector and firm-size pre-reform composition as reported in column 1. Specifically, using the baseline sample of firm-year observations shown in column 1, we estimate a logit of the matched-in-CERVED

indicator on CCNL fixed effects, 1-digit sector CSC codes[56] fixed effects and indicators for different firm-size thresholds ($\mathbf{1}\{size < 20\}$; $\mathbf{1}\{20 \geq size < 50\}$; $\mathbf{1}\{50 \geq size < 100\}$; $\mathbf{1}\{100 \geq size < 150\}$; $\mathbf{1}\{150 \geq size < 200\}$; $\mathbf{1}\{size > 250\}$). The inverse propensity score weights applied to the INPS-INVIND-CERVED sample appear to closely recover average firm size and industry composition observed in our baseline matched employer-employee dataset.

A.3   Local Labor Market Analysis

The LLM results are constructed as follows. Using the person-year panel built from our matched employer-employee dataset we collapse the share of temporary jobs in a given LLM, defined as the unique interaction between a province and a CCNL. Notice that this share corresponds to the share of temporary jobs that represented the dominant job for a given individual in a year. Using the information contained in "Anagrafica" we then calculated the total number of employees in a LLM. We exclude from the analysis LLMs that experienced more than a 100% change in total employment in absolute value. Finally, we winsorize employment at 1 and 99 percent. Our collapsed LLM by year data used to estimate (1) contains a total of 136,069 observations with 9,845 unique LLMs.

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[56]These industry codes are provided directly by INPS ("Codice Statistico Retributivo") and correspond to 4 main industries: Manufacturing, Handicraft ("Artigiani"), Banking/Insurance, and Retail and Services.

B A Case Study of Three Italian Collective Bargaining Agreements (CCNLs)

To better understand how the legislative framework regarding temporary work changed after the approval of Decree 368/2001, this Appendix examines in detail the text of the Retail, Food, and Metal Manufacturing sectors' CCNLs, which amount to more than 50% of the contracts registered in the INPS-INVIND administrative data.[57]

B.1 Retail Sector's CCNL

The Retail sector's CCNL signed on 09/22/1999 (i.e., *before* the reform was implemented) refers both to Law 56/1987 and to a specific list of circumstances allowing short-term contracts (Title VI, Section 25). This list approves hiring temporary workers only when there are "productive activity increases due to extraordinary orders" or for "substituting workers on leave."

The first CCNL signed *after* the reform was implemented exhibits important changes: the references to Law 56/1987 and to the special circumstances allowing temporary hiring were eliminated (Part III, Section 61).[58] This CCNL clearly states that the national law 368/2001 now provides the new rules under which it is possible to hire on a temporary employment contract. The CCNL only legislates on some complementary, specific aspects (e.g., when defining "new activities"—Section 64, Part III—or when fixing the maximum share of temporary contracts that firms are allowed to reach—Section 63, Part III).
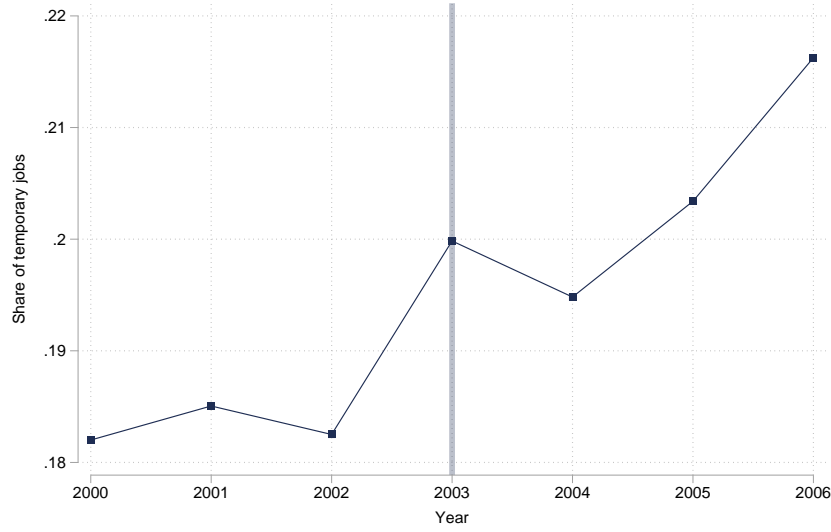
What is the evolution of the share of temporary workers in the Retail sector? While the share of temporary workers seems to remain relatively constant between 1999 and 2002, the time series exhibits a discontinuity in 2003, which coincides with the year in which the CCNL in this sector is renewed and, thus, with the year in which the first CCNL adopting Decree 368/2001 is signed. In particular, we observe a 4 percentage point increase in temporary contracts in the year of adoption of the new legislation—which amounts to 22% of the temporary workers' share observed before the reform.

To summarize, the Retail sector moves from a framework in which hiring temporary workers was based on a list of specific circumstances (based on Law 56/1987 guidelines) to a framework in which hiring temporary workers was allowed under any circumstances following the elimination of the special clauses listed under the former framework. We observe a strong increase in the share of temporary workers after 2001, in line with the

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[57]See the https://www.cnel.it/Archivio-Contratti/Contrattazione-Nazionale/Analisi-Avanzate.

[58]We refer, in particular, to the Retail and Services CCNL signed on 03/17/2003.

Figure B.1: Evolution of the Share of Temporary Workers in the Retail CCNL

hypothesis that the list of circumstances described in the pre-reform CCNLs was limited and that Decree 368/2001 liberalized the creation of temporary contracts to some extent.

B.2 Food Sector's CCNLs

The Food sector's CCNLs before the reform has a structure similar to that of the Retail sector presented in Section B.1. Before the approval of Decree 368/2001, the CCNLs associated with the Food sector stated clearly that an employment contract should typically be on a permanent basis.[59] Temporary work arrangements could only be allowed under specific circumstances, in line with the provisions of Law 56/1987.[60] Many of these specific circumstances refer to temporary substitution of workers, in which case firms had to specify the name of the substituted worker and the reasons for the substitution.

After the approval of Decree 368/2001, the structure of the CCNLs changed in a similar fashion to what we observed in the Retail sector. Importantly, unlike in the pre-reform agreements, the section dedicated to temporary employment no longer starts by stating that an employment contract will typically be on a permanent basis. Moreover, the list of

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[59]Specifically, we analyzed the following contracts: Food Industries ("Alimentari Industrie"), Food Handicraft ("Alimentari Artigiani"), Food Cooperatives ("Alimentari Cooperative"), and Food Small and Medium Enterprises ("Alimentari PMI"). The one with the highest coverage of workers is the Food Industries CCNL and this is the one we therefore analyzed in more detail.

[60]The Food Industry CCNL singles out circumstances such as: extraordinary activities outside the seasonal nature of the business (Art 18, point 1; Article 19), part-time worker substitutions, and the substitution of workers absent on leave or on holidays.

special circumstances under which it was possible to hire on a temporary basis is no longer specified.[61] Instead, similar to the Retail sector, post-reform CCNLs state that temporary employment contracts can be signed in accordance with the national legislation contained in Decree 368/2001.

Figure B.2 shows the development of the share of temporary workers in the Food Industry using the INPS-INVIND administrative data. In 2002, the year in which the new rules of Decree 368/2001 were implemented in this sector, we observe a 5.5 percentage point increase in the share of temporary workers—or 45% of the 2001 observed share.

Figure B.2: Evolution of the Share of Temporary Contract Workers in the Food Industry CCNLs

Note: This figure shows the share of temporary workers in the Food sector, as defined by the following CCNLs: Food Industries ("Alimentari Industrie"), Food Handicraft ("Alimentari Artigiani"), Food Cooperatives ("Alimentari Cooperative") and Food Small and Medium Enterprises ("Alimentari PMI").

B.3   Metal Manufacturing Sector's CCNL

The Metal Manufacturing CCNL ("Industrie Metalmeccaniche") is one of the most important CCNLs in Italy.[62] This agreement exhibits a very peculiar legal framework with respect to temporary contracts. While the CCNL signed before the approval of Decree 368/2001 lists a series of specific circumstances allowing temporary worker hiring (citing

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[61]The only exemption is the Food SME CCNL, which states that temporary contracts can be signed in accordance with Decree 368/2001. Moreover, this CCNL provides an additional list of circumstances under which it was possible to hire temporary workers. These circumstances appear to be the same as those listed in the CCNL signed *before* the reform was passed.

[62]This CCNL should not be confused with the Metal *Handicraft* CCNL that we analyzed in Section 4.1.

Law 56/1987), the following CCNL (signed on 05/07/2003) does not mention temporary work at all.[63] In contrast to the Food and Retail sector's CCNLs analyzed above, this agreement does not cite Decree 368/2001, but instead defers the regulation on temporary contracts to an agreement planned for 2004, which, however, is unclear whether it was fully implemented.[64]

We can therefore consider this CCNL almost as a control or "placebo" group, since we would expect the 2001 reform to have a small impact on the share of temporary workers in this sector. Figure B.3 seems to confirm this hypothesis: the share of short-term contracts in the Metal Manufacturing sector remains almost unchanged. In particular, there is no "jump" in the year of the CCNL's renewal (2003), in contrast to what occurred in the other two sectors examined above. These considerations led us to estimate an alternative empirical model in which the Metal Manufacturing sector's CCNL is a control group in all the periods of our event-study specification. The results, which are reported in Panel (f) of Figure C.5, show that our baseline LLM results are robust to this alternative specification.

Figure B.3: Evolution of the Share of Temporary Workers in the Metal Manufacturing CCNL
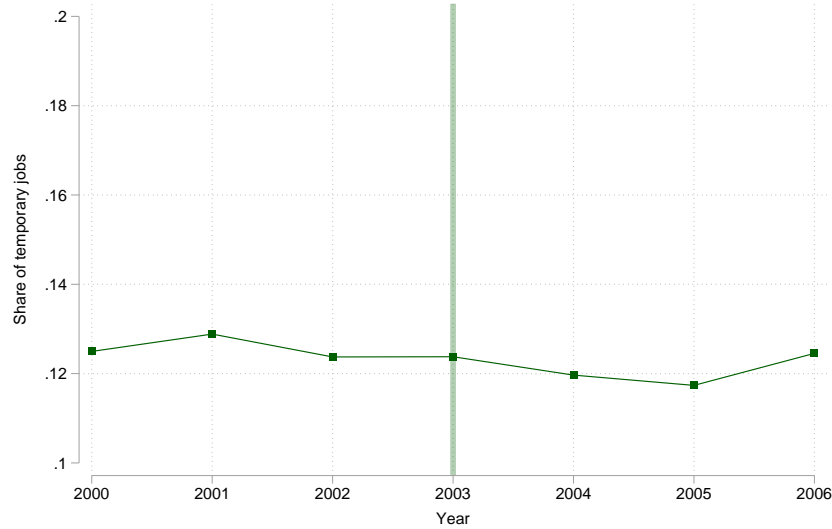
Note: This figure shows the share of temporary workers in the Metal Manufacturing sector ("Industrie Metalmeccaniche').

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[63]In particular, for the *pre-reform* CCNL, we refer to Section 1-BIS of the "Metal Manufacturing Sector: Industries" signed on 06/08/1999.

[64]According to Santucci et al. (2008) *"[...] the Metal Manufacturing Industry's CCNL leaves a large part of this sector without a national protocol on temporary work. The Metal Manufacturing Sector CCNL, which was signed after the approval of the 2001 reform, defers the regulation on short-term contracts to future agreements that have never been realized."* See also Cappellari et al. (2012).
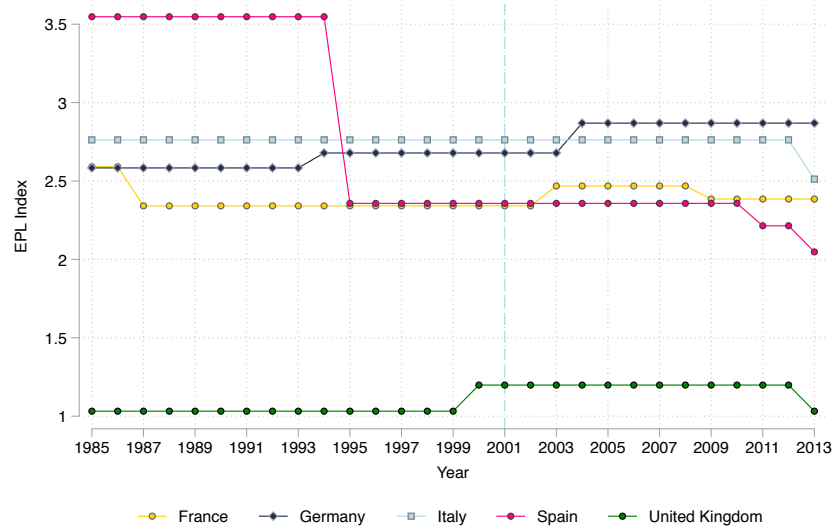
C   Additional Evidence

Figure C.1: Employment Protection Legislation

(a) Temporary Contracts
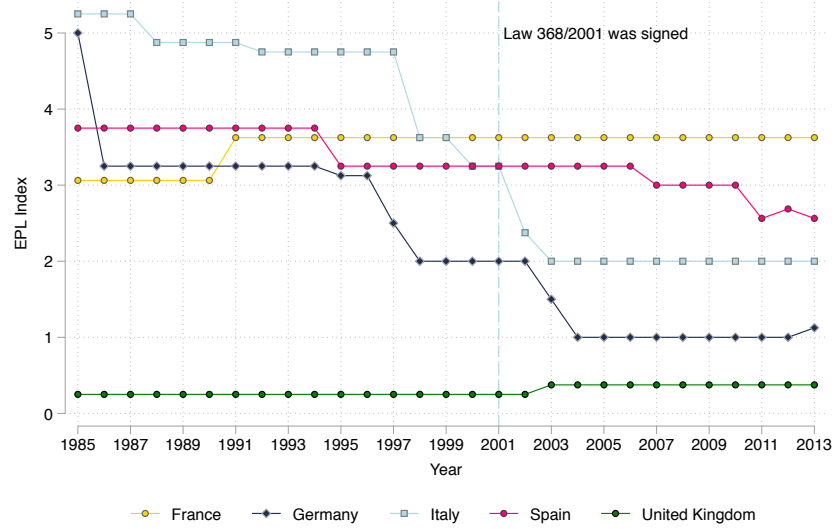
(b) Permanent Contracts

Figure C.2: Creation, Destruction and Transitions of Temporary Jobs

(a) Share of New Temporary Jobs

(b) Turnover Rate of Temporary Jobs

(c) Transitions of Temporary Jobs

(d) Transitions of Permanent Jobs

Note: Panel (a) reports the fraction of jobs that in a given year correspond to newly created temporary jobs. Panel (b) reports the turnover or churn rate of temporary jobs in a given year. Panels (c) and (d) report the transition rates based on the employment contract observed in period t. Panel (c) focuses on individuals that had a temporary job in year t and report the corresponding shares of individuals that in year $t+1$ are either: (i) under a temporary contract with a different employer relative to year t, (ii) under a permanent contract with a different employer relative to year t, (iii) under a temporary contract with the same employer of year t, (iv) under a permanent contract with the same employer of year t, or (v) no longer employed. Panel (d) is similar but for those with a permanent contract in year t.

Figure C.3: Descriptive Evidence on Entry Level Earnings
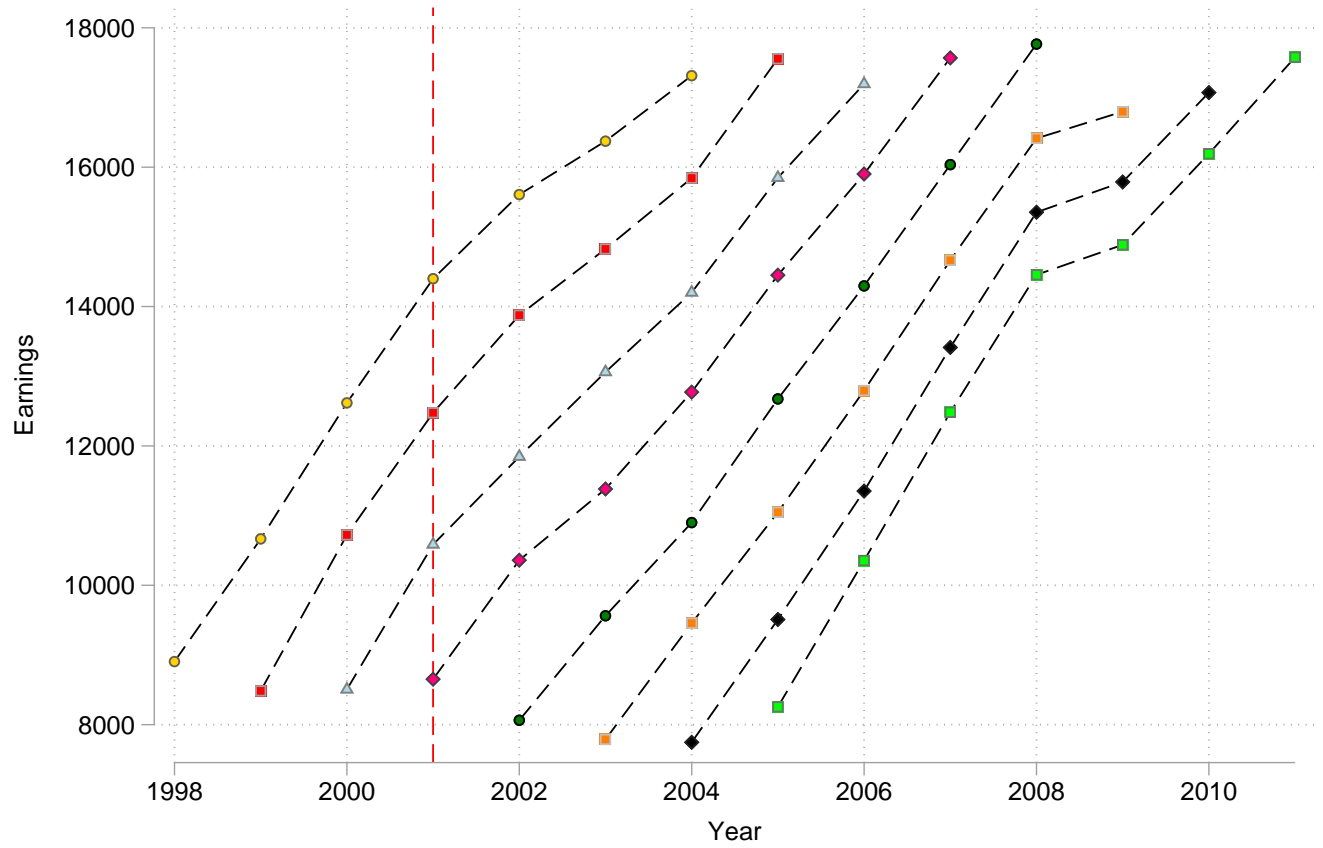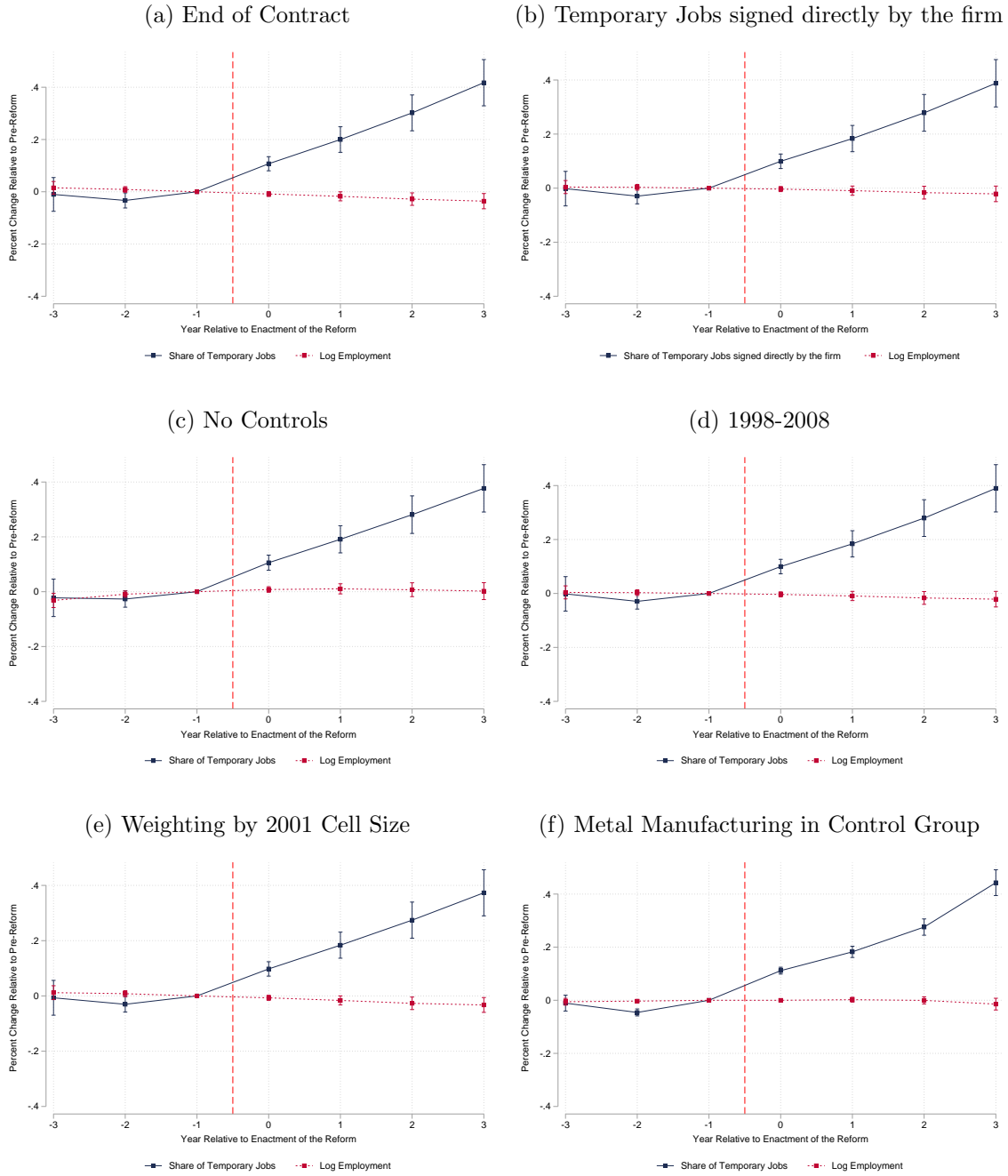
Note: This graph plots the annual earnings of workers entering the labor market between 1998 and 2005. It further focuses on those who were less than 30 years old at the time of entry.

Figure C.4: Increases to EPL and their Impact on Employment
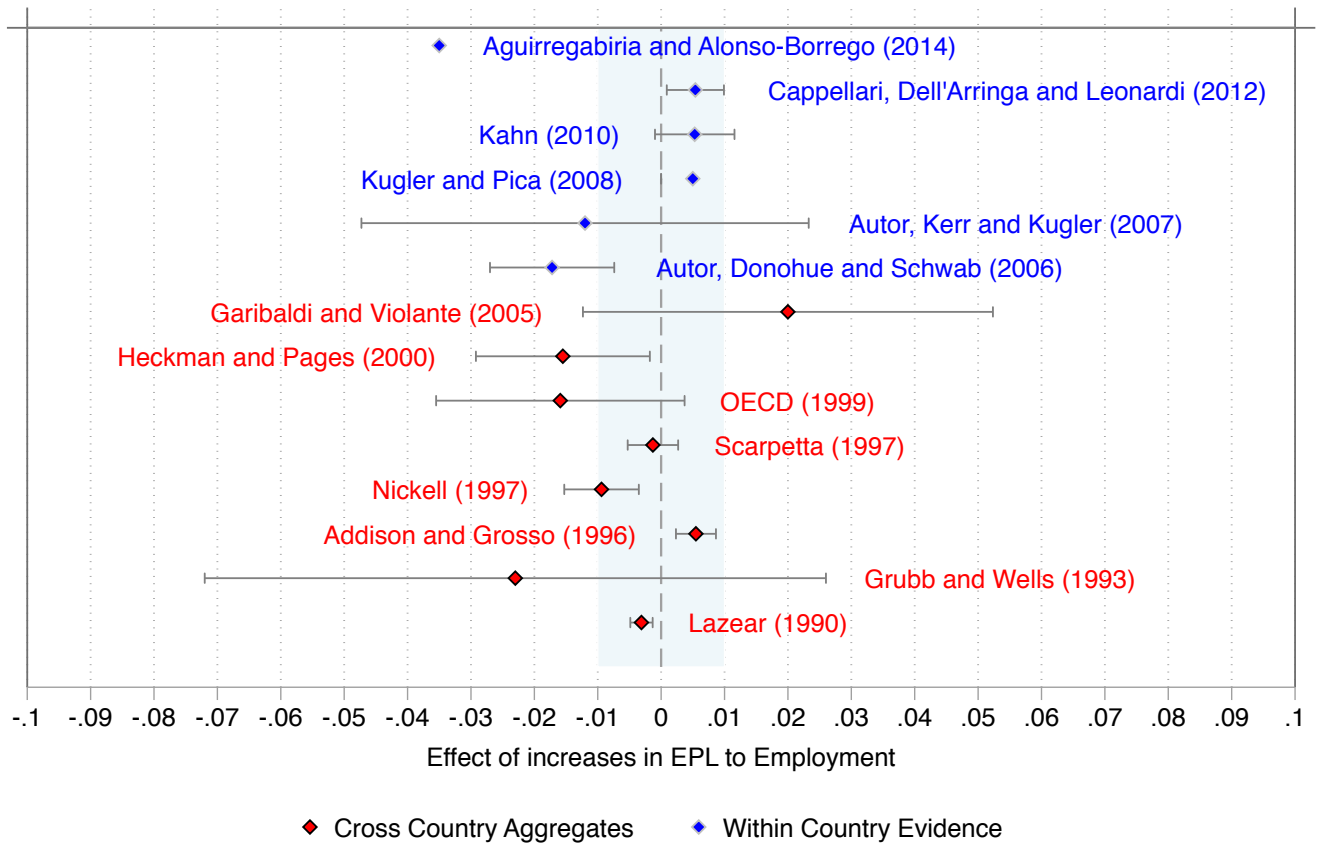
Note: This figure shows point estimates and 95% confidence intervals of the studies shown in the graph. See Table C.1 for details on the measures used to proxy EPL by each study and additional details concerning the estimation of the effects plotted in the figure.

Figure C.5: Robustness of Main Effect of the Reform

(a) End of Contract

(b) Temporary Jobs signed directly by the firm

(c) No Controls

(d) 1998-2008

(e) Weighting by 2001 Cell Size

(f) Metal Manufacturing in Control Group

Note: All panels display event study coefficients from equation (1). These event study coefficients are rescaled so that they capture the percentage change relative to pre-reform years. Panel (a) estimates model (1) letting the first year of implementation of the reform coincide with the year of expiration of pre-2001 CCNL. Panel (b) considers temporary jobs only those signed directly by the firm with the employee, therefore excluding temporary jobs obtained via a temporary work agency. Panel (c) estimates equation (1) without the inclusion of controls. Panel (d) estimates equation (1) using the sample period 1998-2008 as opposed to 1998-2013 (our baseline). Panel (e) weighs the regression estimates using the number of micro-observations in a given LLM market in the year 2001. Panel (f) estimates equation (1) setting the metal manufacturing sector as a never-enacting CCNL. 95% confidence intervals obtained after clustering the standard errors at the LLM level.
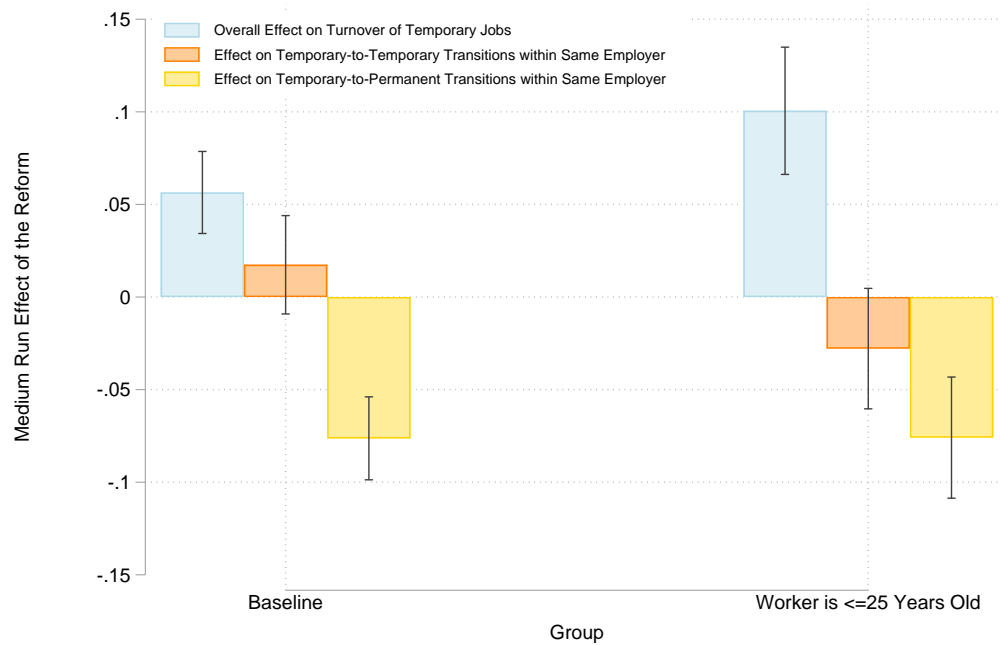
Figure C.6: Decomposition of the Increase in the Share of New Temporary Jobs
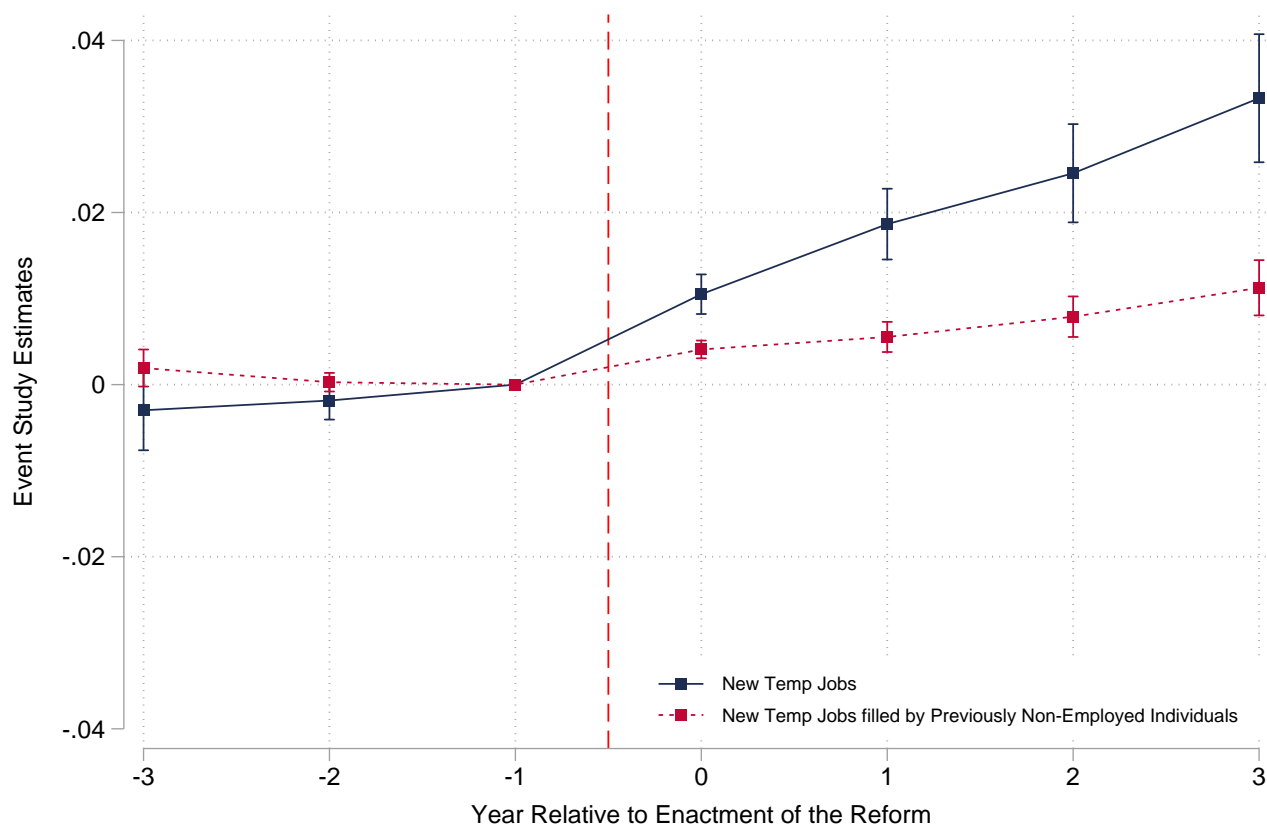
Figure C.7: Decomposition of the Increase in the Temporary Jobs Turnover Rate

(a) Decomposition #1

(b) Decomposition #2
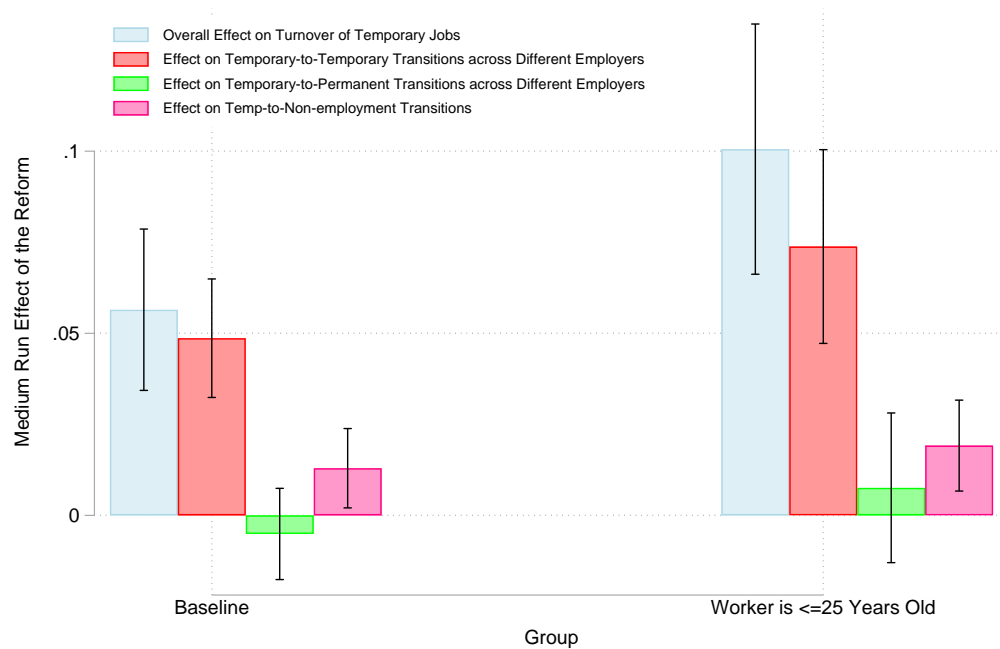
Note: This figure presents estimates from equation (2) on the sample of temporary jobs that in year t already existed for two years or more. Each bar represents the event study coefficient θ_3 on a different outcome that captures a particular transition rate as described in the legend of the figures. See text for details. 95% confidence intervals obtained after clustering the standard errors at the LLM level.

Figure C.8: Additional Evidence on Incumbents

(a) Extensive Margin

(b) Temporary vs. Permanent
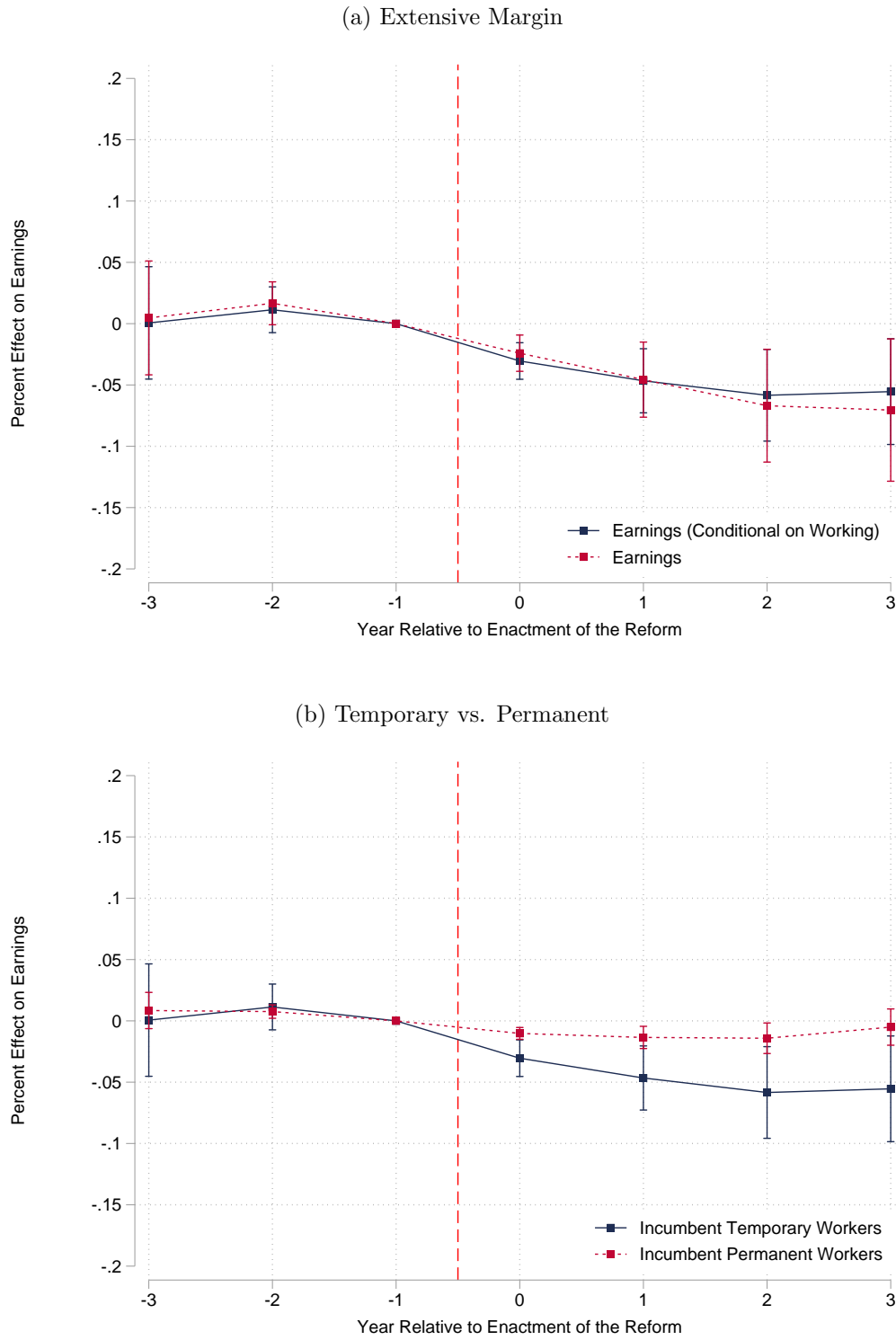
Note: This figure displays event study coefficients when estimating equation (5) on the sample of incumbent temporary workers (i.e., workers that in the pre-reform era (1998-2001), if employed, were always employed under a temporary employment contract and who are less than 40 years of age in 2001). Panel (a) displays the results when using as outcome earnings (assigning zero earnings to non-employment spells of individuals) and earnings conditional on employment. Panel (b) shows the event study coefficients when estimating equation (5) separately on incumbent temporary workers and incumbent permanent workers on (conditional on employment) earnings. All event study coefficients have been rescaled by the average level of earnings observed in the pre-reform era. 95% confidence intervals obtained after clustering the standard errors at the LLM level.

Figure C.9: Number of Entrants in Event Time
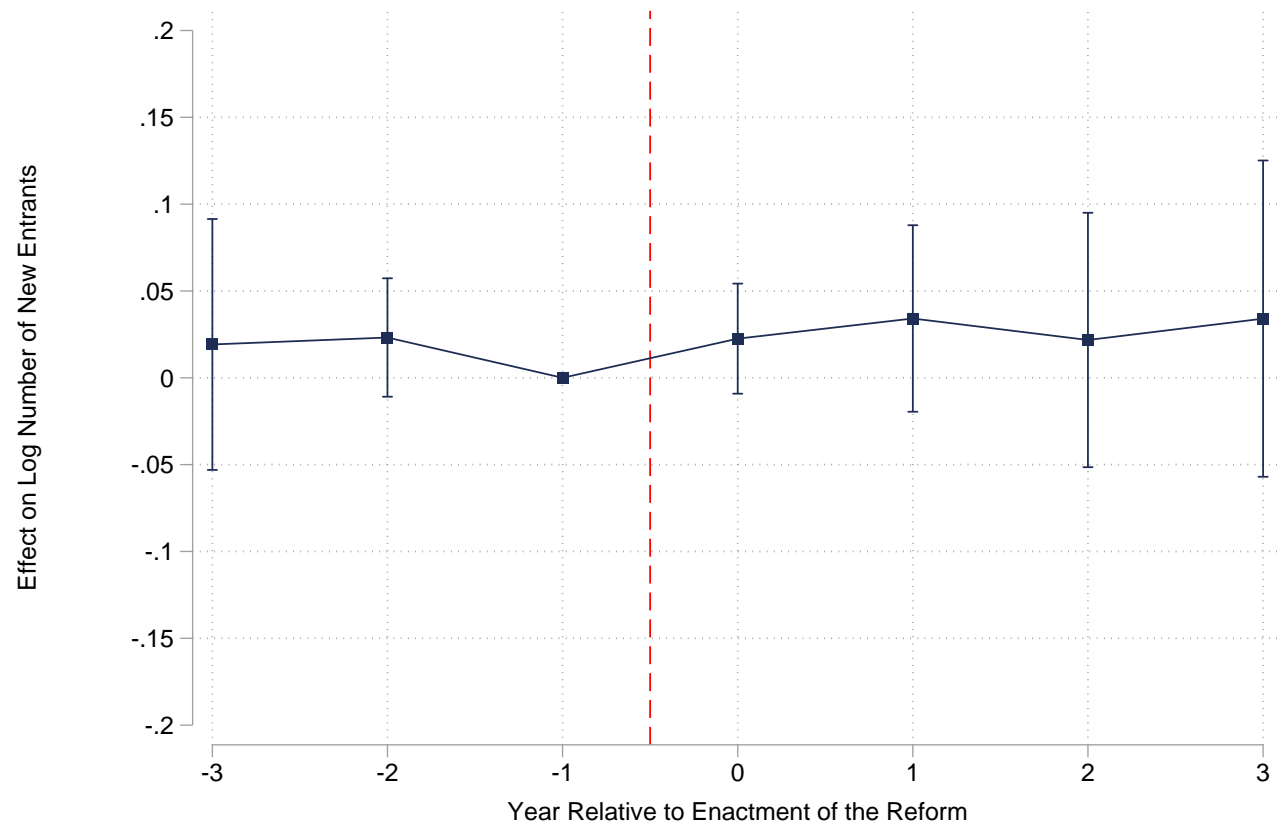
Figure C.10: Wages, Employment Contract and Value Added
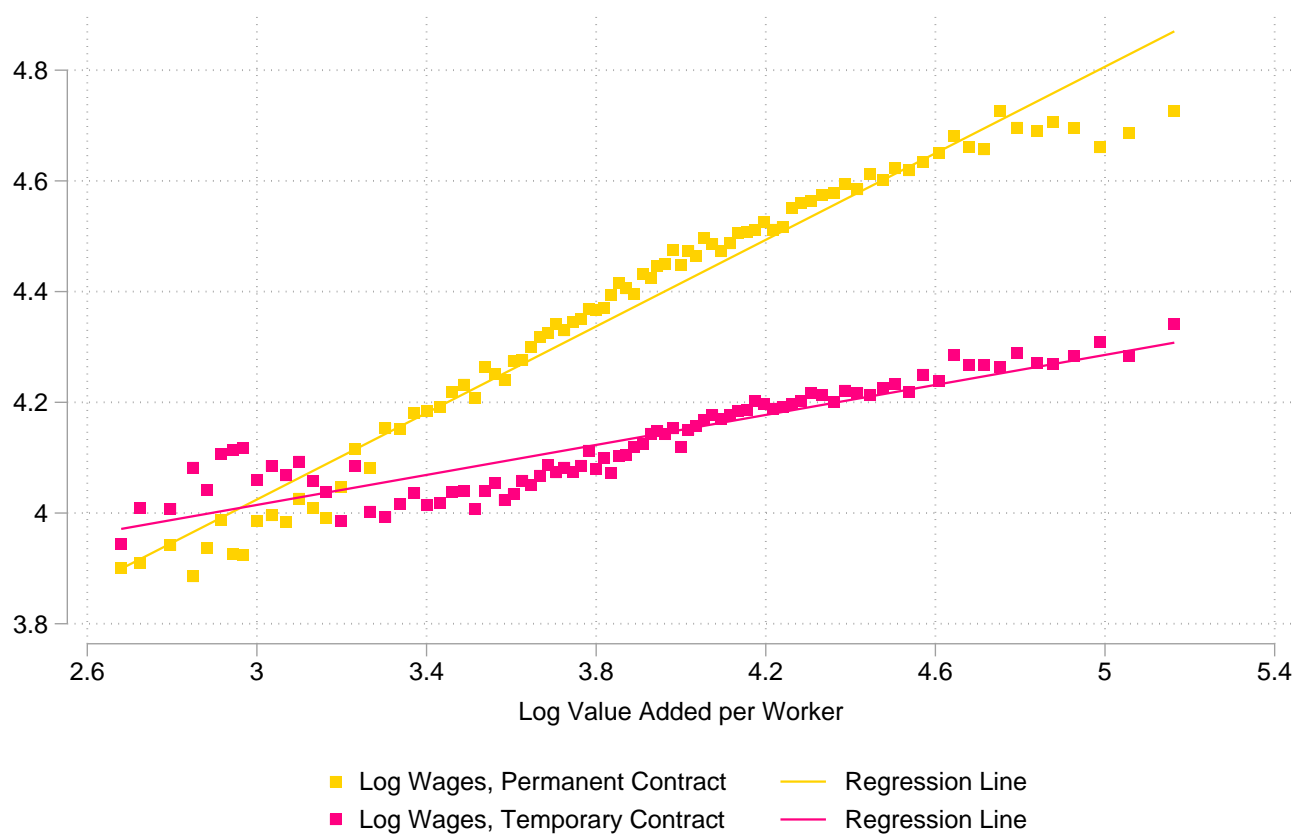
Table C.1: Empirical Estimates on the Impact of EPL on Employment

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	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 relationship 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	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	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.	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) + Annual Survey of Manufacturers (ASM, 1972-1999)	Variation in extent and adoption of employment protection measures - wrongful discharge laws - across US states. Difference in Differences framework	Table 5: adoption of the implied-contract exception is associated with a negative impact but imprecise effect on employment using ASM (other laws deliver mostly imprecise estimates)
Kugler and Pica (2008)	Random sample (1/90) from Italian 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	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.

Note: This table displays a list of studies that have analyzed the impact of reforms to EPL on aggregate employment.

Table C.2: Match with CERVED

	Universe of Firm-Year Observations in INPS-INVIND		Universe of Firm-Year Observations in INPS-INVIND matched in CERVED		Column 3 / Column 1 (x100)
	[1]	[2]	[3]	[4]	[5]
	# of Obs	%	# of Obs	%	
Firm Size					
<20	7,049,664	86.1	2,475,899	73.0	35.12
20-50	741,647	9.1	580,336	17.1	78.25
50-100	218,857	2.7	186,087	5.5	85.03
100-250	118,963	1.5	101,617	3.0	85.42
>250	57,095	0.7	46,692	1.4	81.78
# of Firm-Year Observations	8,186,226		3,390,631		41.42

Note: Column 1 corresponds to the set of firm-year observations that we observe in our matched employer-employee data over the sample period 1998-2013. Sample in column 3 represents the set of firm-year observations for which we can find a match in the CERVED database using a firm national tax identifier. See text for details.

Table C.3: 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 held 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, labeled as any worker who in a given month is employed under such contract. All statistics are person-month weighted.

Table C.4: 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 of reform 368/2001 in a given year.

Table C.5: Size and Sector Composition in the Pre-Reform

	Firms in INPS-INVIND existing before the reform	Firms in INPS-INVIND existing before the reform, matched with CERVED	Firms in INPS-INVIND existing before the reform, matched with CERVED -- reweighted
	[1]	[2]	[3]
Firm-Size	21.46	35.99	21.63
	(257.81)	(273.97)	(194.07)
Manufacturing	0.31	0.58	0.31
Handicraft	0.30	0.01	0.31
Banking	0.01	0.00	0.01
Retail and Services	0.39	0.41	0.38
Number of Observations	1,339,417	547,774	547,774

Note: This table reports average firm size as well as 1-digit sector composition for firms that were already operating in the pre-reform period. All reported statistics refer to the pre-reform era only. Firms in column 1 correspond to all the firms that we observe in the matched employer-employee data. In column 2, we report these statistics for those firm-year observations for which we are able to match balance sheet information from CERVED. In column 3, we reweigh these statistics using a propensity score reweighting designed to match the sector, and firm-size pre-reform composition as reported in column 1. Number of observations refer to the number of firm-year observations observations during the pre-reform. We use the CSC codes (Codice Statistico Contributivo) provided by INPS to assign 1-digit sector dummies. See text for details

Table C.6: Summary Statistics of Incumbent Workers

	Incumbent Temporary Workers [1]	Incumbent Permanent Workers [2]
Labor Market Outcomes		
Total Earnings	11,053	23,820
	(8083)	(12600)
Total Days Worked	170	283
	(110)	(68)
Log Daily Wage	4.14	4.33
	(0.33)	(0.44)
Age and Duration		
Age	26.1	31.7
Fraction Less than 30 yrs old	0.78	0.32
Age at Entry	22.8	23.8
Tenure	1.7	5.0
Worker and Workplace Characteristics		
Female	0.39	0.32
Full Time	0.82	0.90
Employed via Temporary Work Agency	0.02	0.00
# of Jobs in the year	1.5	1.20
Value Added per Worker	41,817	50,738
Firm Size	25.15	20.58
# of Individuals	129,143	1,129,685

Note: All summary statistics refer to the interval 1998-2001. Temporary (Permanent) incumbent workers are individuals born in 1961 or later that between 1998-2001, if employed, were always employed under a temporary (permanent) employment contract. Total earnings and total days worked refer to the sum of labor earnings and days worked, respectively, across all jobs in a given year. 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 per worker are in real 2010 euros. Value added per worker is reported only for workers where the associated employer has financial information collected in CERVED sample. See text for details.

Table C.7: Incumbent Temporary Workers, All Ages

	Employed	Earnings	Days Worked	Conditional on Employment				
				Earnings	Days Worked	Same Employer	Same Employer & Permanent Contract	Value Added per Worker
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Short Run Effect of the Reform	-0.0013	-194.7**	-6.659***	-292.7***	-7.703***	-0.0285***	-0.0399***	-3,980.5***
	(0.0032)	(81.89)	(1.202)	(84.96)	(1.080)	(0.00717)	(0.00586)	(883.7)
Medium Run Effect of the Reform	-0.0295*	-614.6*	-21.59***	-571.2**	-17.94***	-0.0305**	-0.0925***	-5,640.4***
	(0.0158)	(317.3)	(6.057)	(249.4)	(2.878)	(0.0136)	(0.0158)	(1833.6)
Pre-reform Average of the Outcome	.97	11,027.02	168.89	11,404.42	174.67	.6	.27	43,163.97
# of Person-Year Observations	1,740,914	1,740,914	1,740,914	1,602,529	1,602,529	1,602,529	1,602,529	1,078,512

Note: This table displays event study coefficients when estimating equation (5) on the sample of incumbent temporary workers, i.e. workers that in the pre-reform era (1998-2001), if employed, were always employed under a temporary employment contract but without restricting them to be 40 years or less by 2001 as done in the main text. Outcomes from columns 1-3 do not condition on employment and we assign 0 days worked and 0 earnings to non-employment spells. See text for further details. Results from columns 4-8 condition on employment. Value added per worker is the average value added calculated for a given firm over the sample period. This measure is only available for firms belonging to the CERVED sample. See text for details. Same employer is a dummy equal to 1 if the worker did not switch employer. Same employer & Permanent Contract is a dummy equal to 1 if the worker remained with the same employer and is under a permanent contract. The Pre-Reform Mean Outcome reported in column 7 corresponds the unconditional share of individuals that in the pre-reform have been converted from a temporary into permanent job by their employers in our overall observed sample. Standard errors displayed in round brackets and clustered at the LLM level.

Table C.8: Predetermined Characteristics between Pre- and Post-Reform Cohorts

	Pre-Reform Cohort- Mean	Adjusted Difference Between Pre- and Post-Reform Cohorts	P-value
	[1]	[2]	[3]
Female	0.3971	0.0023	0.451
	.	(0.0030)	
Foreign	0.1074	-0.0159	0
	.	(0.0037)	
Age at entry	22.2778	-0.1648	0
	.	(0.0257)	
South	0.1805	0.0055	0.1566
	.	(0.0039)	
Rome	0.0633	-0.0001	0.9442
	.	(0.0021)	.
Milan	0.0743	-0.0014	0.5782
	.	(0.0026)	

Note: This table compares predetermined characteristics of new entrants that entered the labor market between 1998-2005 and that were less than 30 years old at the time of entry. Column 1 reports the mean corresponding characteristic for workers that entered before the policy change took place. Column 2 reports the differences in differences regression coefficient associated with the reform. The dependent variable of this regression corresponds to the characteristic listed in each row and the underlying model controls for year and CCNL fixed effects. Standard errors are clustered at LLM 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, Milan and Rome are dummies equal to 1 if the individual lives in the South of Italy, Milan and Rome, respectively, at the moment of entry.

Table C.9: Robustness Analysis of New Entrants

	Adding Controls		Conditional on Employment		Conditional on Full-Time		Current LLM Fixed Effects		Prov of Entry x Year x Year of	
	Earnings	Perm	Earnings	Perm Contract	Earnings	Perm Contract	Earnings	Perm Contract	Earnings	Perm Contract
Reform x Year 1	-420.9367***	-0.0735***	-476.7897***	-0.0717***	-577.8144***	-0.0723***	-541.6122***	-0.0420***	-484.8620***	-0.0835***
	(63.2438)	(0.0041)	(59.8972)	(0.0043)	(64.1857)	(0.0044)	(56.9355)	(0.0037)	(66.0549)	(0.0056)
Reform x Year 2	-303.3886***	-0.0173***	-366.1165***	-0.0273***	-405.3417***	-0.0279***	-374.9968***	-0.0107***	-235.0392***	-0.0269***
	(58.9435)	(0.0032)	(55.7100)	(0.0036)	(58.5805)	(0.0037)	(50.1113)	(0.0033)	(61.7665)	(0.0036)
Reform x Year 3	-208.1448***	-0.0102***	-208.3198***	-0.0167***	-223.1080***	-0.0175***	-187.3251***	-0.0053*	-218.1408***	-0.0042
	(58.3376)	(0.0029)	(55.1700)	(0.0032)	(57.7085)	(0.0033)	(49.5823)	(0.0031)	(63.7933)	(0.0034)
Reform x Year 4	-188.3399***	-0.0035	-166.3222***	-0.0072**	-149.4988**	-0.0088***	-195.1481***	-0.0007	-191.0617***	0.0013
	(60.6081)	(0.0027)	(57.4501)	(0.0030)	(60.0791)	(0.0031)	(51.8412)	(0.0030)	(69.8181)	(0.0034)
Reform x Year 5	-197.9796***	-0.0018	-171.1952***	-0.0035	-133.3598**	-0.0045	-177.1265***	0.0010	-281.0294***	-0.0006
	(67.0132)	(0.0029)	(63.4755)	(0.0031)	(65.4586)	(0.0032)	(59.1891)	(0.0031)	(76.4651)	(0.0036)
Reform x Year 6	-158.9798**	-0.0005	-171.1385**	-0.0032	-92.6600	-0.0030	-137.4991*	-0.0035	-183.6027**	0.0004
	(75.2056)	(0.0032)	(71.5531)	(0.0034)	(74.1857)	(0.0035)	(75.1116)	(0.0037)	(81.7394)	(0.0036)
Reform x Year 7	-102.7208	0.0017	-160.4488*	-0.0010	-29.9357	-0.0010	-130.1785	-0.0055	21.7111	0.0020
	(87.1333)	(0.0037)	(83.5140)	(0.0039)	(88.0650)	(0.0040)	(96.8160)	(0.0045)	(90.2165)	(0.0037)
# of Cells	274,787	274,787	256,229	256,229	235,935	235,935	273,214	273,214	274,787	274,787

Note: This table reports the estimated coefficients θ_s from equation (6) which capture the dynamic effect of having entered the labor market under the new regime of temporary employment contracts. The sample includes all individuals that entered the labor market between 1998-2005 and that were 30 years old or less at the moment of entry. See text for details. Regression estimates are based on cell data at the level of year of entry in the labor market, local labor market of entry (i.e., combination of province and collective bargaining agreement at the moment of entry) and year of potential experience in the labor market defined as current year minus year since first entering the INPS data. Coefficients shown represent the interaction between a dummy for whether the first job is under the reform and dummies for year since the first entrance in the labor market. Baseline model control for LLM, province of entry by year fixed effects, potential experience fixed effects and cohort fixed effects properly normalized. See text for details. Columns in this table represent different specifications. In Adding Controls we add as controls cell share of female and Italian workers. Conditional on employment only calculates cell level averages among individuals that are formally employed in social security data. Conditional on full-time employment only calculates cell level averages among individuals that are formally employed in social security data and employed in a full time job. Current LLM fixed estimates the baseline model controlling for current LLM fixed effects as opposed to LLM of entry fixed effects. The last column estimates the baseline model replacing the province by year fixed effects with province by year by year of entry in the labor market fixed effects. Standard errors displayed in round brackets and clustered at the LLM level.

Table C.10: Heterogeneity Analysis on Entrants

	Men		Women		Age at Entry <=21		Age at Entry >=24		Low Predicted Earnings		High Predicted Earnings	
	Earnings	Perm Contract	Earnings	Perm Contract	Earnings	Perm Contract	Earnings	Perm Contract	Earnings	Perm Contract	Earnings	Perm Contract
Reform x Year 1	-641.8160***	-0.0856***	-155.7843*	-0.0588***	-138.1523**	-0.0487***	-912.4160***	-0.1162***	-88.2284	-0.0655***	-806.1886***	-0.1160***
	(78.1601)	(0.0051)	(86.0048)	(0.0053)	(69.8281)	(0.0045)	(118.6125)	(0.0067)	(67.0386)	(0.0051)	(126.0868)	(0.0067)
Reform x Year 2	-379.1788***	-0.0273***	-256.3740***	-0.0056	76.8071	-0.0025	-686.6022***	-0.0383***	-93.7773	-0.0123***	-507.8765***	-0.0425***
	(73.4585)	(0.0040)	(82.2897)	(0.0044)	(64.7223)	(0.0034)	(110.6197)	(0.0053)	(64.4965)	(0.0040)	(122.2296)	(0.0056)
Reform x Year 3	-232.9863***	-0.0191***	-181.4887**	-0.0010	-46.0753	0.0040	-265.5475**	-0.0286***	-109.2152*	-0.0038	-195.9963	-0.0293***
	(72.5376)	(0.0035)	(82.9513)	(0.0041)	(63.0943)	(0.0032)	(110.9194)	(0.0046)	(64.2782)	(0.0037)	(124.3294)	(0.0051)
Reform x Year 4	-202.2821***	-0.0097***	-80.6383	0.0016	-209.7911***	0.0006	-41.2837	-0.0082*	-130.0934*	-0.0052	-7.3606	-0.0053
	(75.2028)	(0.0033)	(86.8152)	(0.0040)	(65.2694)	(0.0032)	(118.1631)	(0.0045)	(67.5016)	(0.0037)	(132.8424)	(0.0049)
Reform x Year 5	-163.6972**	-0.0054	13.9772	0.0026	-468.2076***	-0.0050	217.7556*	0.0123***	-292.1073***	-0.0111***	270.6714*	0.0143***
	(82.4061)	(0.0034)	(94.5533)	(0.0042)	(71.5375)	(0.0035)	(130.5271)	(0.0047)	(74.0636)	(0.0039)	(144.8240)	(0.0050)
Reform x Year 6	-87.4128	-0.0040	130.4868	0.0065	-455.1374***	-0.0135***	297.6769**	0.0264***	-327.5717***	-0.0143***	526.3830***	0.0282***
	(92.2442)	(0.0038)	(104.8980)	(0.0047)	(80.4120)	(0.0039)	(147.2565)	(0.0053)	(83.2546)	(0.0044)	(160.2343)	(0.0053)
Reform x Year 7	45.7535	-0.0022	152.0947	0.0120**	-351.5391***	-0.0168***	341.7049**	0.0353***	-381.6914***	-0.0161***	676.4830***	0.0431***
	(107.4031)	(0.0044)	(120.2486)	(0.0053)	(93.8620)	(0.0046)	(170.5749)	(0.0060)	(96.6943)	(0.0051)	(182.4002)	(0.0060)
Mean Outcome in First Year of Entry Pre-Reform Cohorts	8,669.30	.38	8143.2	.35	6325.08	.26	11606.19	.49	5682.23	.33	11795.08	.45
# of Cells	274,787	274,787	256,229	256,229	235,935	235,935	273,214	273,214	274,787	274,787	274,787	274,787

Note: This table reports the estimated coefficients θ_s from equation (6) which capture the dynamic effect of having entered the labor market under the new regime of temporary employment contracts, estimated separately for the group of workers defined on top of each column. Regression estimates are based on cell data at the level of year of entry in the labor market, local labor market of entry (i.e., combination of province and collective bargaining agreement at the moment of entry) and year of potential experience in the labor market defined as current year minus year since first entering the INPS data. Coefficients shown represent the interaction between a dummy for whether the first job is under the reform and dummies for year since the first entrance in the labor market. We control for LLM, province of entry by year fixed effects, potential experience fixed effects and cohort fixed effects properly normalized. See text for details. Age at entry <=21 estimates the model using only individuals whose first job was full-time and they were less than 21 years of age at the moment entry (so unlikely to hold a college degree). Age at entry >=24 estimates the model using only individuals whose first job was full-time and they were 24 years of age or more at the moment entry (so more likely to hold a college degree). The last two columns are based on an auxiliary model where we predict earnings 7 years following entry using information on the worker (gender, nationality, age at entry) and on the first job (dummies for 5 digits sector codes, province and deciles of firm size and Value added per worker. We fit this model to cohorts that entered before the national reform was signed (1998-2000) and extrapolate the predictions to later cohorts and create tercile of these predictions with the third tercile representing the group labeled "high predicted earnings". Below each set of coefficients we report the average of a given outcome for the pre-reform cohorts in their first job. Standard errors displayed in round brackets and clustered at the LLM level.

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Table C.11: Within Employer, Within Person Evidence of Differential Rent Sharing -- Full Time Employees

	Last Year under Temp	First Year under Perm	Difference
	[1]	[2]	[3]
[1] Log Daily Wage	4.2182	4.2997	0.0815
Components			
[2] Log Value Added per Worker	4.0536	4.0452	-0.0083
[3] Rent Sharing Coefficient	0.0272	0.0427	0.0155
	(0.0019)	(0.0019)	(0.0004)
Oaxaca Decomposition			
[4] Surplus component	0.1103	0.1101	-0.0002
			[0.00]
[5] Bargaining component	0.1101	0.1727	0.0626
			[0.77]

Note: This table reports estimates from the decomposition highlighted in equation (9), estimated on full time workers only. 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 average log daily wage. Row 2 reports average log value added per worker. Row 3 reports the rent sharing coefficients from equation (8) as plotted in Panel (b) of Figure 8. Rows 4-5 report the Oaxaca decomposition terms defined in equation (9). The term in square bracket in column 3 represents 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 where we can observe the value added per worker of their employer in the CERVED dataset. Number of person year observations is 4,768,256 with 541,389 unique person identifiers. Standard errors, clustered at the LLM level, reported in round brackets.

D   Model

We now introduce a theoretical framework that illustrates how the reform described in this paper may influence the dynamics of job creation, duration, and destruction. We provide a qualitative analysis of this 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.

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., 2005).[65] 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 turnover rate 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 D.5.

D.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 exante 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.[66] The job type $\lambda \in [\underline{\lambda}; \bar{\lambda}]$

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

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

is drawn at random from a distribution with cdf $G(\lambda)$ and pdf $g(\lambda)$. The value of λ is observed exante 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 unproductive 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 converted to a permanent job with a newly bargained wage.[67] Writing a temporary contract implies a one-time administrative cost c_T paid by the firm. The reform described in Section 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.[68]

An alternative view is to consider temporary contracts as a screening device for learning about the true productivity of workers (Blanchard and Landier, 2002;Nagypál, 2007; 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).

D.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

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

[68]Dräger and Marx (2017) 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.

market and associated labor market equilibrium are derived in the next section. We impose the condition $f < U$ so that permanent jobs that become unproductive are always destroyed. As detailed in Appendix D.5, the surplus of creating a permanent job with an expected duration $1/\lambda$ is given by

$$S_P(\lambda) = (y - rU) \int_0^\infty e^{-(r+\lambda)\tau} d\tau - \int_0^\infty f\lambda e^{-(r+\lambda)\tau} d\tau. \tag{10}$$

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 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

$$S_T(\lambda, D) = \int_0^D \left(ye^{-\lambda\tau} - rU\right) e^{-r\tau} d\tau + \{\max[S_P(\lambda), 0]e^{-\lambda D}\}e^{-rD} - c_T. \tag{11}$$

In equation (11), 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 is discounted by e^{-rD}. The optimal duration is the one that maximizes the surplus of a temporary job and it is therefore given by

$$D^*(\lambda) = \begin{cases} \dfrac{1}{\lambda} \log \left(\dfrac{rU + \lambda f + (r + \lambda)c_P}{rU} \right) & \text{if } \lambda \leq \lambda_P \\[3mm] \dfrac{1}{\lambda} \log \left(\dfrac{y}{rU} \right) & \text{if } \lambda \geq \lambda_P, \end{cases} \tag{12}$$

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 exist 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 D.1, Panel (a) for a graphical description and Appendix D.5 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, whereas 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 jobs 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 reaches 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.

D.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.[69] In particular, we let $\gamma \in [0, 1]$ denote the fixed share of surplus retained by workers. Hence in this benchmark model temporary and

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[69]This timing is slightly different from Cahuc et al. (2016) who assume 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 D.4.

permanent workers are assumed to have the same bargaining power. Therefore, temporary workers will not be hired as a way for firms to extract a larger share of surplus from a given production opportunity by hiring temporary workers. This assumption is imposed to simplify the qualitative analysis of the model and it will be relaxed theoretically in Appendix D.5.5 and empirically in Section 7.

As detailed in Appendix D.5, the free-entry condition in the search market allows us to pin down the equilibrium value of θ from the following equation:

$$\kappa = q(\theta)(1 - \gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda)\right],\tag{13}$$

and the value of unemployment can be written as

$$rU = z + \kappa\frac{\gamma\theta}{(1 - \gamma)},\tag{14}$$

where z is the flow utility of being unemployed. Equation (14) 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.[70] 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 (13), where*

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

D.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.*

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[70]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.

3. An increase in temporary job destruction.

See Appendix D.5.3 for the proof.

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 D.5.3, we prove that the direct effect dominates the equilibrium feedback effect and so 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 to 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 (12).

Finally, the reform also affects the threshold λ_E that governs the substitution between temporary and permanent contracts at the time of job creation. A decrease in the cost of writing temporary contracts implies a direct effect: firms take advantage of the lower matching cost and so, 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 exante ambiguous and highly dependent on the distribution of production opportunities and other primitives of the model.[71]

The implications of the reform are summarized in Figure D.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 jobs are now destroyed (purple area); (iii) all temporary jobs have a shorter duration.

All these effects, combined with the substitution effect highlighted in the yellow area, counteract the increase in job creation and thus generate an a priori ambiguous effect on the steady-state employment rate. A direct corollary is that the impact of the reform

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[71]In Appendix D.5.3, we characterize when the direct effect dominates the equilibrium feedback effect and provide a sufficient condition.

on labor market efficiency is also ambiguous.[72] In Appendix D.5.4, we formally show that the post-reform increases in net output stemming from lower labor costs and the opening of new production opportunities (the intended consequences) can be potentially nullified by decreases in net output due to the post-reform substitution of permanent jobs with temporary ones, the destruction of temporary jobs that used to be converted into permanent jobs, 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 exante ambiguous effects, and quantify the distributional impacts of the reform on both firms and workers.

D.5 Model Derivations

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

D.5.1 Choice Rule

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

$$S_P(\lambda) = \Pi_P(\lambda) + V_P(\lambda) - U \tag{15}$$

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

$$\Pi_P(\lambda) = \int_0^\infty \left[\int_0^\tau [y - w(\lambda)]e^{-rt}dt - fe^{-r\tau} \right] \lambda e^{-\lambda\tau}d\tau, \tag{16}$$

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, $\Pr(\text{Job remains productive until } \tau) = e^{-\lambda\tau}$, hence the usage of the exponential distribution inside the outer integral. Similarly, we have that

$$V_P(\lambda) = \int_0^\infty \left[\int_0^\tau w(\lambda)e^{-rt}dt + Ue^{-r\tau} \right] \lambda e^{-\lambda\tau}d\tau. \tag{17}$$

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[72]To measure labor market efficiency we consider the steady-state surplus of the model, defined as the sum of production of filled jobs minus firing, matching and vacancy costs.

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

$$S_P(\lambda) = \frac{y - rU - \lambda f}{r + \lambda}. \tag{18}$$

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

$$S_T(\lambda, D) = \Pi_T(\lambda, D) + V_T(\lambda, D) - U \tag{19}$$

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

$$V_T(\lambda, D) = \int_0^D w(\lambda, D)e^{-r\tau}d\tau + \{\max[V_P(\lambda), U]e^{-D\lambda}\}e^{-rD} + U(1 - e^{-\lambda D})e^{-rD}. \tag{20}$$

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

$$S_T(\lambda, D) = \int_0^D [ye^{-\lambda\tau} - rU]e^{-r\tau} + \max\{S_P(\lambda), 0\}e^{-D(r+\lambda)} - c_T \tag{21}$$

The last two equations take D as given. The optimal duration of temporary contracts highlighted in equation (12) is obtained by taking the first order conditions from (21), for a given λ, that is

$$ye^{-\lambda D} - rU - (r + \lambda)e^{-\lambda D}\max\{S_P(\lambda), 0\} = 0 \tag{22}$$

It can be shown that the second order condition is satisfied and from (22) we obtain the optimal duration of temporary contracts, $D^*(\lambda)$, as displayed in equation (12), and the surplus of a temporary job is therefore given by $S_T(\lambda) = S_T(\lambda, D^*(\lambda))$. The function $D^*(\lambda)$ is continuous and increasing in the expected duration, $\frac{1}{\lambda}$ but decreasing in the value of unemployment, rU.

As noted by Cahuc et al. (2016), the model allows for three types of equilibrium (i) temporary jobs are created for all λ (which would occur when $c_T=0$) (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).

D.5.2 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

$$r\Pi_v = -\kappa + q(\theta)(1-\gamma) \int_{\underline{\lambda}}^{\lambda_T} S(\lambda) dG(\lambda) \tag{23}$$

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 (13) 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

$$rU = z + \theta q(\theta)\gamma \int_{\underline{\lambda}}^{\lambda_T} S(\lambda) dG(\lambda) \tag{24}$$

from which one obtains immediately (14) using the free entry condition.

D.5.3 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

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

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

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

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

where the first equation, (EC) identifies the Entry Condition. 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 (21) the FOC obtained from (22), 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 (14) 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

$$\frac{d\lambda_T}{dc_T} = \underbrace{-\frac{h_{c_T}^{TJCR}(\lambda_T, \theta; c_T)}{h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T)}}_{\text{Direct Effect}} + \underbrace{\frac{h_{\theta}^{TJCR}(\lambda_T, \theta; c_T)}{h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T)} \frac{h_{c_T}^{EC}(\theta; c_T)}{h_{\theta}^{EC}(\theta; c_T)}}_{\text{Equilibrium Feedback Effect}} \qquad (25)$$

    (1.2) Notice that

$$h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T) = y\frac{e^{-(r+\lambda_T)D^*(\lambda_T)}[(r+\lambda_T)D^*(\lambda_T) + 1] - 1}{(r+\lambda_T)^2} < 0$$

$$h_{c_T}^{TJCR}(\lambda_T, \theta; c_T) = -1 < 0$$

$$h_{\theta}^{TJCR}(\lambda_T, \theta; c_T) = -\frac{\partial U(\theta)}{\partial \theta}\left[\frac{r + \lambda_T(1 - e^{-rD^*(\lambda_T)})}{r + \lambda_T}\right] < 0$$

$$h_{c_T}^{EC}(\theta, c_T) = q(\theta)(1 - \gamma)[G(\lambda_T) - G(\lambda_E)] > 0$$

$$h_{\theta}^{EC}(\theta, c_T) = -(1-\gamma)q'(\theta)\left[\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda)\right] -$$
$$- q(\theta)(1-\gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda, \theta)dG(\lambda)\right.$$
$$\left. + \int_{\lambda_E}^{\lambda_T} h_{\theta}^{TJCR}(\lambda, \theta; c_T)dG(\lambda)\right] > 0 \qquad (26)$$

    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 (24). The last posi-

tive sign follows from the fact $h_\theta^{TJDR}(\lambda, \theta) < 0$ 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 (25) as

$$\frac{d\lambda_T}{dc_T} = -\frac{h_{c_T}^{TJCR}(\lambda_T, \theta; c_T)}{h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T)} - \frac{h_\theta^{TJCR}(\lambda_T, \theta; c_T)}{h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T)} \frac{d\theta}{dc_T} \tag{27}$$

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

$$-\frac{h_{c_T}^{TJCR}(\lambda_T, \theta; c_T)}{h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T)} - \frac{h_\theta^{TJCR}(\lambda_T, \theta; c_T)}{h_{\lambda_T}^{TJCR}(\lambda_T, \theta; c_T)} \frac{d\theta}{dc_T} < 0 \tag{28}$$

$$\iff 1 > \frac{d\theta}{dc_T} h_\theta^{TJCR}(\lambda_T; \theta; c_T).$$

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

$$-h_{\theta,\lambda}^{TJCR}(\lambda; \theta; c_T) = \frac{\partial U(\theta)}{\partial \theta} \frac{(-re^{-rD^*(\lambda)})}{(r + \lambda)^2} < 0 \tag{29}$$

Therefore we have that

$$-h_{\theta,\lambda}^{TJCR}(\lambda_T; \theta; c_T) < \int_{\lambda_E}^{\lambda_T} -\frac{h_\theta^{TJCR}(\lambda, \theta; c_T)dG(\lambda)}{G(\lambda_T) - G(\lambda_E)}dG(\lambda) \tag{30}$$

$$= E(-h_{\theta,\lambda}^{TJCR}(\lambda; \theta; c_T)|\lambda_E < \lambda < \lambda_T)$$

This implies that

$$\frac{d\theta}{dc_T} h_\theta^{TJCR}(\lambda_T; \theta; c_T) =$$

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

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

$$< 1$$

$$= Q.E.D.$$

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

$$\frac{d\lambda_P}{dc_T} = \frac{-r}{c_p + f} \frac{\partial U(\theta)}{\partial \theta} \frac{d\theta}{dc_T} > 0 \tag{31}$$

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

$$D^*(\lambda) = \begin{cases} \dfrac{1}{\lambda} \log\left(\dfrac{rU(\theta) + \lambda r + (r+\lambda)c_P}{rU(\theta)}\right) & \text{if } \lambda \leq \lambda_P \\[3mm] \dfrac{1}{\lambda} \log\left(\dfrac{y}{rU(\theta)}\right) & \text{if } \lambda \geq \lambda_P \end{cases} \tag{32}$$

which implies that, for a fixed λ,

$$\frac{dD^*(\lambda)}{dc_T} = \begin{cases} -\dfrac{1}{\lambda}\left(\dfrac{rU(\theta)}{rU(\theta) + \lambda r + (r+\lambda)c_P} \dfrac{\partial U(\theta)}{\partial \theta}\right) \dfrac{d\theta}{dc_T} > 0 & \text{if } \lambda \leq \lambda_P \\[3mm] -\dfrac{1}{\lambda}\left(\dfrac{rU(\theta)}{y} \dfrac{\partial U(\theta)}{\partial \theta}\right) \dfrac{d\theta}{dc_T} > 0 & \text{if } \lambda \geq \lambda_P \end{cases} \tag{33}$$

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,

$$\frac{\partial\lambda_E}{\partial c_T} = \underbrace{-\frac{h_{c_T}^{PJvsTJ}(\lambda_E, \theta; c_T)}{h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T)}}_{\text{Direct Effect}} - \underbrace{\frac{h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T)}{h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T)} \frac{d\theta}{dc_T}}_{\text{Equilibrium Feedback Effect}}. \tag{34}$$

(4.2) We have that

$$h_{\lambda_E}^{PJvsTJ}(\lambda_E, \theta; c_T) = \frac{-\lambda_E fr \frac{\partial D^*}{\partial\lambda}(\lambda_E)e^{-rD^*(\lambda_E)}}{(1 - e^{-rD^*(\lambda_E)})(r + \lambda_E)} \geq 0$$

$$h_{c_T}^{PJvsTJ}(\lambda_T, \theta; c_T) = -1 < 0$$

$$h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T) = -\lambda_E \frac{\partial U}{\partial\theta} \frac{(1 - e^{-rD^*(\lambda_E)})}{r + \lambda_E} < 0$$

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

$$h_{\theta}^{PJvsTJ}(\lambda_E, \theta; c_T) =$$

$$\frac{[G(\lambda_T) - G(\lambda_E)] \frac{\partial U(\theta)}{\partial \theta} \lambda_E \frac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_E}}{- \int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda, \theta) dG(\lambda) - \int_{\lambda_E}^{\lambda_T} h_{\theta}^{TJCR}(\lambda, \theta; c_T) dG(\lambda) - \frac{q'(\theta)}{q(\theta)} \bar{S}}$$

(see equation (30))

$$< \frac{[G(\lambda_T) - G(\lambda_E)] \frac{\partial U(\theta)}{\partial \theta} \lambda_E \frac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_E}}{- \int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda, \theta) dG(\lambda) - (G(\lambda_T) - G(\lambda_E)) h_{\theta}^{TJCR}(\lambda_T, \theta; c_T) - \frac{\bar{S} q'(\theta)}{q(\theta)}}$$

$$< \frac{\lambda_T \frac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_T}}{\frac{(r + \lambda_T (1 - e^{-rD^*(\lambda_T)}))}{r + \lambda_T}}.$$

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

$$\lambda_T < \frac{y - c_T r}{f + c_T}. \tag{35}$$

D.5.4 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.[73] 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

$$Y_P(\lambda, \theta, u, \lambda_T; c_P) = M_P(\lambda)y - u\theta q(\theta)g(\lambda)c_P - \lambda M_P(\lambda)f \tag{36}$$

  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

$$u\theta q(\theta)g(\lambda) = \lambda M_P(\lambda). \tag{37}$$

- $\lambda \in (\lambda_E, \lambda_P]$. 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

$$Y_{T_G}(\lambda, \theta, u, \lambda_T; c_P, c_T) = \tag{38}$$
$$\underbrace{M_{T_G}(\lambda)ye^{-\lambda D^*(\lambda)} - u\theta q(\theta)g(\lambda)c_T}_{\text{Net Production under temp contract}} + \underbrace{M_P(\lambda)y - \frac{M_{T_G}(\lambda)}{D^*(\lambda)}c_P e^{-\lambda D^*(\lambda)} - f\lambda M_P(\lambda)}_{\text{Net Production under perm contract}},$$

  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 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

$$\frac{M_{T_G}(\lambda)}{D^*(\lambda)} = u\theta q(\theta)g(\lambda) \tag{39}$$

$$\frac{M_{T_G}(\lambda)e^{-\lambda D^*(\lambda)}}{D^*(\lambda)} = \lambda M_P(\lambda) \tag{40}$$

- $\lambda \in (\lambda_P, \lambda_T]$. 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

$$Y_{T_B}(\lambda, \theta, u, \lambda_T; c_T) = M_{T_B}(\lambda)ye^{-\lambda D^*(\lambda)} - u\theta q(\theta)g(\lambda)c_T \tag{41}$$

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

where M_{T_B} is identified from

$$\frac{M_{T_B}(\lambda)}{D^*(\lambda)} = u\theta q(\theta)\frac{g(\lambda)}{G(\lambda_T)} \tag{42}$$

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

$$\mathcal{Y} = \int_{\underline{\lambda}}^{\lambda_E} Y_P(\lambda, \theta, u, \lambda_T; c_P)d\lambda + + \int_{\lambda_E}^{\lambda_P} Y_{T_G}(\lambda, \theta, u, \lambda_T; c_P, c_T)d\lambda$$

$$+ \int_{\lambda_P}^{\lambda_T} Y_{T_B}(\lambda, \theta, u, \lambda_T; c_T)d\lambda - \kappa\theta u q(\theta)$$

$$= M(\theta)\left[\int_{\underline{\lambda}}^{\lambda_E}\left(\frac{y}{\lambda} - f - c_P\right)g(\lambda)d\lambda + \int_{\lambda_E}^{\lambda_P}\left(\frac{y}{\lambda} - f - c_P\right)e^{-\lambda D^*(\lambda)}g(\lambda)d\lambda\right]$$

$$+ M(\theta)\left[\int_{\lambda_E}^{\lambda_T}\left(yD^*(\lambda)e^{-\lambda D^*(\lambda)} - c_T\right)g(\lambda)d\lambda - \kappa\right]$$

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

$$u = 1 - \int_{\underline{\lambda}}^{\lambda_E} M_P(\lambda)d\lambda + \int_{\lambda_E}^{\lambda_P} M_{T_G}(\lambda)d\lambda + \int_{\lambda_P}^{\lambda_T} M_{T_B}(\lambda)d\lambda$$

$$= \frac{1}{1 + M(\theta)\left[\int_{\underline{\lambda}}^{\lambda_E}\frac{1}{\lambda}g(\lambda)d\lambda + \int_{\lambda_E}^{\lambda_T} D^*(\lambda)g(\lambda)d\lambda + \int_{\lambda_E}^{\lambda_P}\frac{1}{\lambda}e^{-\lambda D^*(\lambda)}g(\lambda)d\lambda\right]} \tag{43}$$

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

$$\frac{d\mathcal{Y}^*}{dc_T} = \underbrace{\frac{dM(\theta)}{dc_T}\frac{\mathcal{Y}^*}{M(\theta)}}_{\text{A. Change to Employment}}$$

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

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

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

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

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

$$(44)$$

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 (D.5.3), 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 D.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 (44). Then, the reform (i.e. a reduction in c_T) will decrease net output if $(\lambda_E^*, \lambda_P^*, \lambda_T^*)$ are such that*

$$1 - \left[yD^*(\lambda_T^*)e^{-\lambda_T D^*(\lambda_T^*)} - c_T \right] \frac{g(\lambda_T^*)}{G(\lambda_T^*) - G(\lambda_E^*)} \frac{d\lambda_T^*}{dc_T} <$$

$$< \frac{g(\lambda_E^*)}{G(\lambda_T^*) - G(\lambda_E^*)} \left[yD^*(\lambda_E^*)e^{-\lambda_E^* D^*(\lambda_E^*)} - c_T \right] \frac{d\lambda_E^*}{dc_T} +$$

$$+ \frac{g(\lambda_P^*)}{G(\lambda_T^*) - G(\lambda_E^*)} \left(\frac{y}{\lambda_P^*} - f - c_P \right) e^{-\lambda_P^* D^*(\lambda_P^*)} \frac{d\lambda_P^*}{dc_T} + \tag{45}$$

$$+ \int_{\lambda_E^*}^{\lambda_T^*} ye^{-\lambda D^*(\lambda)}(1 - \lambda D^*(\lambda)) \frac{dD^*(\lambda)}{dc_T} \frac{g(\lambda)}{G(\lambda_T^*) - G(\lambda_E^*)} d\lambda -$$

$$- \int_{\lambda_E^*}^{\lambda_P^*} \left(\frac{y}{\lambda} - f - c_P \right) \lambda e^{-\lambda D^*(\lambda)} \frac{dD^*(\lambda)}{dc_T} \frac{g(\lambda)}{G(\lambda_T^*) - G(\lambda_E^*)} d\lambda.$$

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.

D.5.5 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 Section 7 and the empirical evidence shown in Section 7.2.

Such difference in bargaining power clearly provides another motive 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.

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 (10) and (11) are unchanged. Differential rent-sharing affects the value of creating a vacancy which is now given by

$$r\Pi_v = -\kappa + q(\theta)(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda) + q(\theta)(1-\gamma_T)\int_{\lambda_E}^{\lambda_T} S_T(\lambda)dG(\lambda). \tag{46}$$

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

$$rU = z + \frac{\theta\gamma_T\kappa}{1-\gamma_T} + \frac{\theta q(\theta)}{1-\gamma_T}(\gamma_P - \gamma_T)\int_{\underline{\lambda}}^{\lambda_E}\left(\frac{y - rU - \lambda f}{r+\lambda} - c_P\right)g(\lambda)d\lambda. \tag{47}$$

Notice that under no differential rent-sharing (i.e. $\gamma_T = \gamma_P$), equation (47) boils down to equation (14). Equation (47) 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 implies the creation of permanent employment contracts are captured by the last term in (47). Moreover, notice that

$$\frac{\partial U}{\partial \theta} > 0; \qquad \frac{\partial U}{\partial \lambda_E} > 0. \tag{48}$$

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 a higher rent.

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 define the endogenous thresholds that are used to solve for the endogenous quantities $(\theta, \lambda_E, \lambda_P, \lambda_E)$,

$$
h^{(EC2)}(\theta, \lambda_E; c_T) \equiv \kappa - q(\theta)(1 - \gamma_P) \int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda) dG(\lambda) +
$$

$$
- q(\theta)(1 - \gamma_T) \int_{\lambda_E}^{\lambda_T} S_T(\lambda; c_T) dG(\lambda) = 0 \tag{EC2}
$$

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

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

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

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 D.3. Based on this, one can plot the (EC2) and (PJvsTJ2) locuses in the (θ, λ_E) space, see Figure D.2, 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.

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 D.5.5.

- 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

$$
\frac{d\theta}{dc_T}\Big|_{\lambda_E \text{ is constant}} = -\frac{h_{c_T}^{(EC2)}(\theta, \lambda_E; c_T)}{h_\theta^{(EC2)}(\theta, \lambda_E; c_T)} < 0 \tag{49}
$$

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where

$$h_{c_T}^{(EC2)}(\theta, \lambda_E; c_T) = q(\theta)(1 - \gamma_T)[G(\lambda_T) - G(\lambda_E)] > 0$$

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

$$- q(\theta)\Big[(1 - \gamma_P)\int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR}(\lambda, \lambda_E, \theta)dG(\lambda)+$$

$$+ (1 - \gamma_T)\int_{\lambda_E}^{\lambda_T} h_{\theta}^{TJCR}(\lambda, \lambda_E, \theta; c_T)dG(\lambda)\Big] > 0$$

This implies that the (EC2) locus shifts upward in Figure D.2 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

$$\frac{d\lambda_E}{dc_T}\Big|_{\theta \text{ is constant}} = -\frac{h_{c_T}^{(PJvsTJ2)}(\theta, \lambda_E; c_T)}{h_{\lambda_E}^{(PJvsTJ2)}(\theta, \lambda_E; c_T)} > 0 \tag{50}$$

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 D.2 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 D.2 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 D.2, 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

$$\frac{d\lambda_P}{dc_T} = -\frac{1}{c_P + f}\left[\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\frac{d\theta}{dc_T} + \frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\frac{d\lambda_E}{dc_T}\right] \gtrless 0 \tag{51}$$

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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.

- The 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

$$\frac{d\lambda_T}{dc_T} = -\frac{h_{c_T}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)}{h_{\lambda_T}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)} - \frac{h_{\theta}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)}{h_{\lambda_T}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)}\frac{d\theta}{dc_T} - \frac{h_{\lambda_E}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)}{h_{\lambda_T}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)}\frac{d\lambda_E}{dc_T}$$
(52)

where

$$h_{\lambda_T}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T) = y\frac{e^{-(r+\lambda_T)D^*(\lambda_T)}[(r+\lambda_T)D^*(\lambda_T)+1]-1}{(r+\lambda_T)^2} < 0$$

$$h_{c_T}^{TJCR2}(\lambda_T, \lambda_E, \lambda_E\theta; c_T) = -1 < 0$$

$$h_{\theta}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T) = -\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\left[\frac{r+\lambda_T(1-e^{-rD^*(\lambda_T)})}{r+\lambda_T}\right] < 0$$

$$h_{\lambda_E}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T) = -\frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\left[\frac{r+\lambda_T(1-e^{-rD^*(\lambda_T)})}{r+\lambda_T}\right] < 0 \qquad (53)$$

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 D.5.3, we have that

$$\frac{d\theta}{dc_T}h_{\theta}^{TJCR2}(\lambda_T; \lambda_E, \theta; c_T) =$$

$$= \frac{-(G(\lambda_T) - G(\lambda_E))h_{\theta}^{TJCR2}(\lambda_T; \lambda_E, \theta; c_T)}{-\frac{1-\gamma_P}{1-\gamma_T}\int_{\underline{\lambda}}^{\lambda_E} h_{\theta}^{TJDR2}(\lambda, \lambda_E, \theta)dG(\lambda) - \int_{\lambda_E}^{\lambda_T} h_{\theta}^{TJCR2}(\lambda, \lambda_E, \theta; c_T)dG(\lambda) - \frac{q'(\theta)}{q(\theta)}\hat{S}}$$

$$< 1$$

where the second inequality follows from the derivations shown in equation (30) 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 $\frac{d\lambda_T}{dc_T} < 0$ since

$$1 > h_{\theta}^{TJCR2}(\lambda_T; \lambda_E, \theta; c_T) + h_{\lambda_E}^{TJCR2}(\lambda_T, \lambda_E, \theta; c_T)\frac{d\lambda_E}{dc_T} \qquad (54)$$

provided that $\frac{d\lambda_E}{dc_T} > 0$. 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 D.1: Choice between Contracts and Effect of the Reform

## (a) Choice Between Temporary and Permanent
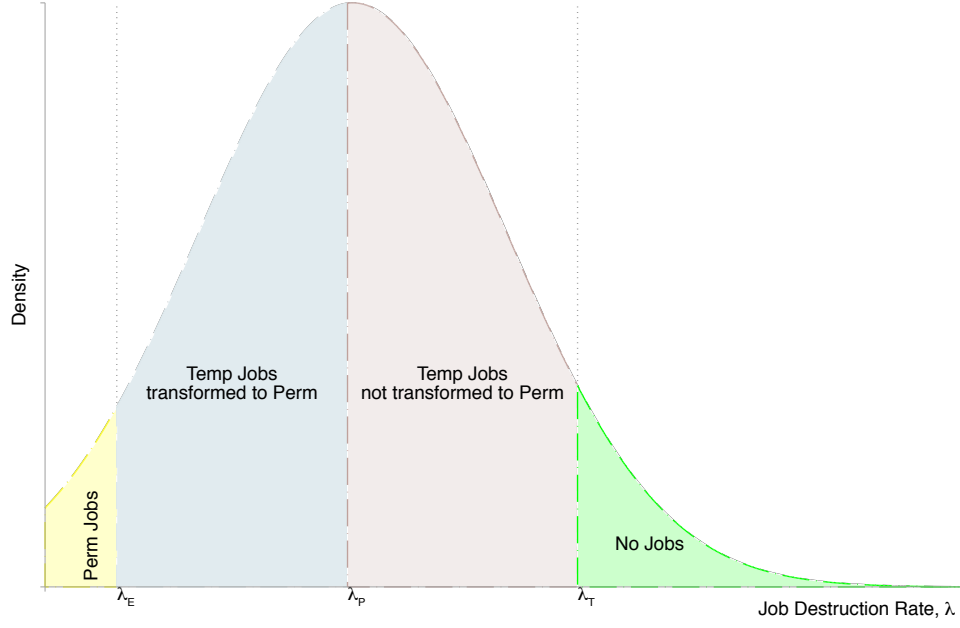

(b) Effect of the Reform
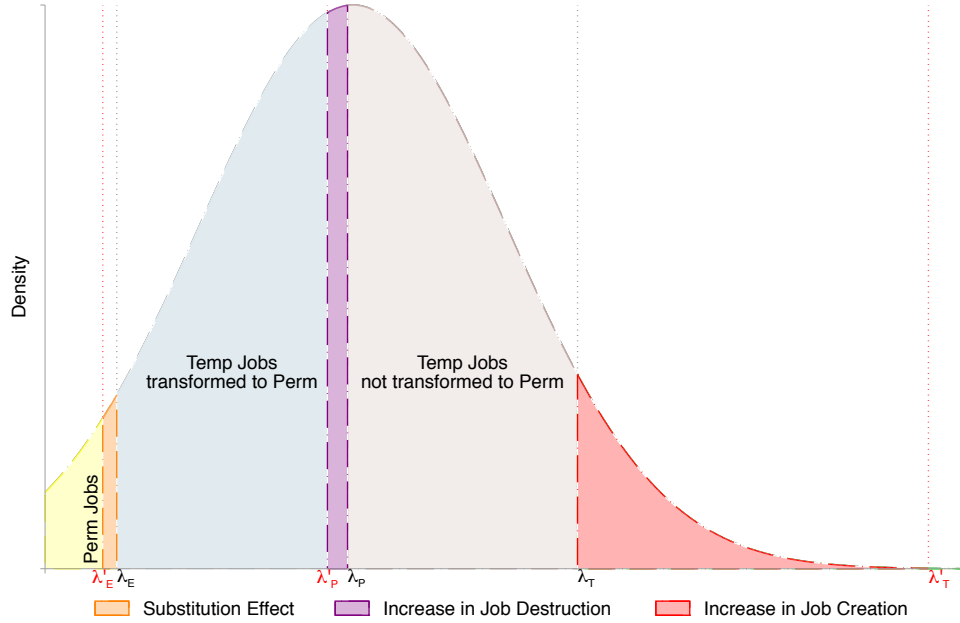
Note: Panel (a) shows the optimal choice rule between the creation of temporary and permanent contracts based on their associated surplus and an assumed distribution of production opportunities. The thresholds are defined as follows: $\lambda_P = \{\lambda : S_P(\lambda) = 0\}$, $\lambda_T = \{\lambda : S_T(\lambda) = 0\}$ and $\lambda_E = \{\lambda : S_P(\lambda) = S_T(\lambda)$, where S_P (S_T) is the surplus associated to the creation of a permanent (temporary) job. See Proposition 1 for details. 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 D.2: Equilibrium under Differential rent-sharing

(a) Equilibrium values of (θ, λ_E)
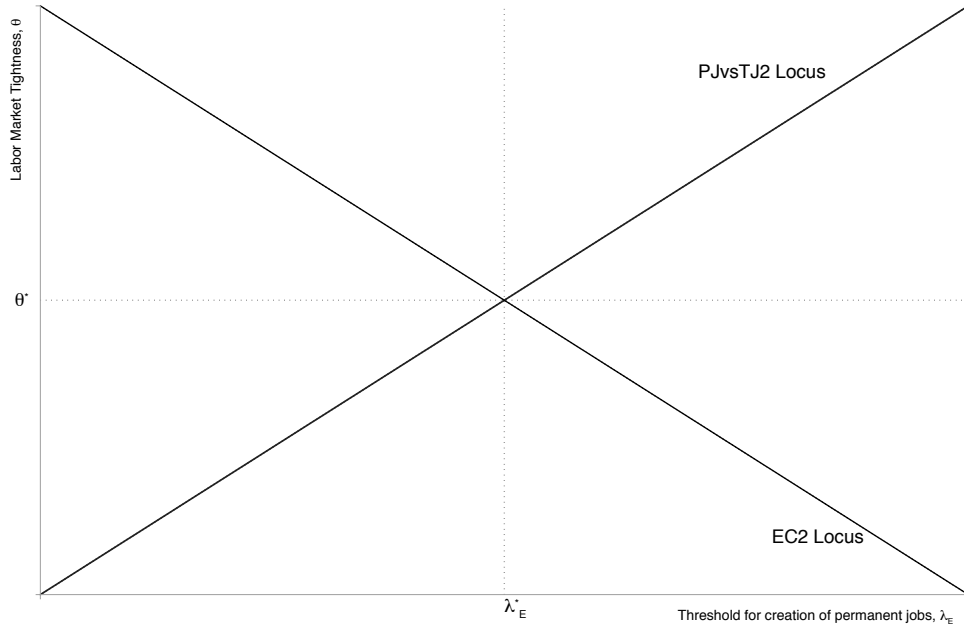
(b) Effect of the Reform
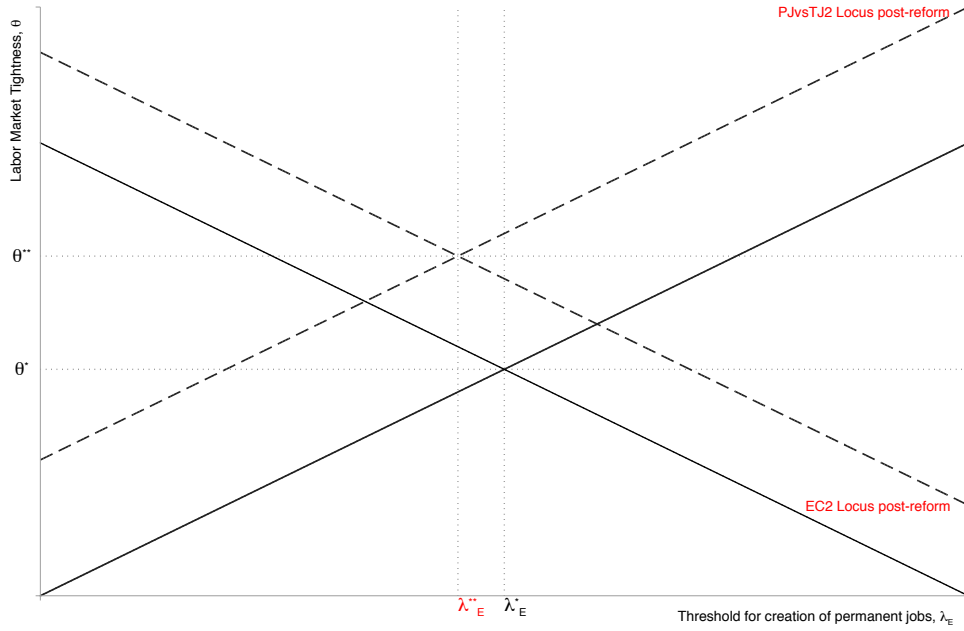
Note: Panel (a) shows how one can solve for equilibrium values of θ and λ_E using the entry condition (EC2) and the rule that defines the creation of permanent jobs (PJvsTJ2), as defined in Section D.5.5. Panel (b) shows how these equilibrium values are going to change following the introduction of a partial reform that facilitates the creation of temporary contracts ($\downarrow c_T$) without affecting the firing costs associated to permanent contracts, f.

E Labor Cost Analysis

Here we present a simple framework to interpret the estimates shown in Table 3 regarding labor costs per worker. Abstracting from the presence of seasonal contracts and apprenticeships, we can define the total labor costs for firm f in period t, denoted by $\tilde{C}_{ft}$,

$$\tilde{C}_{ft} = \tilde{w}_{ft}^P P_{ft} + \tilde{w}_{ft}^T T_{ft}, \tag{55}$$

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

Let $L_{ft} = P_{ft} + T_{ft}$ and define $c_{ft} = C_{ft}/L_{ft}$, $s_{ft} = T_{ft}/L_{ft}$. We can rewrite $\tilde{w}_{ft}^C = \phi^C + \xi_{ft}^C$, where ϕ^C is the average labor costs for contract $C \in \{T; P\}$. Then equation (55) becomes

$$c_{ft} = \phi s_{ft} + \xi_{ft} \tag{56}$$

where $\xi_{ft} = (\xi_{ft}^T - \xi_{ft}^P)s_{ft} + \xi_{ft}^P + \phi^P$ and $\phi = \phi^T - \phi^P$ is the average contract gap in labor costs between a temporary and a permanent contract. Parameter ϕ includes differences in terms of both direct wage compensation and labor taxes paid by the firm across contractual arrangements. Therefore, it provides a 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:

$$c_{jt} = \phi s_{jt} + \psi_j + \lambda_{p(j),t} + \varepsilon_{jt}. \tag{57}$$

An OLS estimation of (57) is likely to introduce bias in measuring ϕ, as the share of workers employed by the firm under a temporary contract, s_{jt}, could 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 (57) 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}, IV estimates of ϕ can be interpreted as an estimate of the average contract gap in labor costs between temporary and permanent workers.

F Bounding Analysis of Entrants

An important restriction of the empirical design of equation (6) is that we need to condition on individuals finding a first job. The reform, however, might increase the likelihood of obtaining a first job. This then creates a selection problem even under the assumption that the reform is randomly assigned (Manski, 1989). Following the methodology of Lee (2009), we evaluate how large the share of individuals who find a first job only after the reform (and not in the pre-reform scenario) needs to be in order to compensate for the net present value of earnings losses reported in column 1 of Table 7.

Let (E_{it}^R, E_{it}^{NR}) denote the potential employment status of individual i in year t in the counterfactual where this individual is exposed to the reform (R) or not (NR). Realized employment can be written as $E_{it} = E_{it}^R R_i + E_{it}^{NR}(1 - R_i)$, where R_i denotes reform status. Similarly, realized earnings can be written as $Y_{it} = Y_{it}^R R_i + Y_{it}^{NR}(1 - R_i)$. Our thought experiment aims to assess the effect of entering the labor market in year e (i.e., cohort e) under the reform on present discounted value earnings.

We impose the following assumptions: (i) $(Y_{ie+s}^R, Y_{ie+s}^{NR}, E_{ie}^R, E_{ie}^{NR}) \perp R_i$, where $s \in \{0, 1, \ldots, S\}$; (ii) $E_{ie}^R \geq E_{ie}^{NR}$; (iii) $Y_{ie+s}^R - Y_{ie+s}^{NR} = \Delta_s$; and (iv) the reform affects the probability of finding a job only in the first year of the search.[74] Assumption (i) states that reform status is as good as randomly assigned.[75] Assumption (ii) states that the reform is monotonically *increasing* the probability of finding a first job. Assumption (iii) imposes a constant treatment effect of the reform on Y_{ie+s}.

Under (i)–(iv), the PDV effect of the reform on earnings for individuals entering the labor market in year e at a given interest rate r is

$$PDV_{e,S}(r) = \underbrace{\frac{\Pr(E_{ie}^R > E_{ie}^{NR})}{\Pr(E_{ie} = 1 | R_i = 1)} \, \mathrm{E}[Y_{ie}^R | E_{ie}^R > E_{ie}^{NR}]}_{\text{Extensive margin effect at entry}} + \tag{58}$$

$$+ \underbrace{\frac{\Pr(E_{ie}^R = E_{ie}^{NR})}{\Pr(E_{ie} = 1 | R_i = 1)} \Delta_0}_{\text{Intensive margin effect at entry}} + \underbrace{\sum_{s=1}^{S} \frac{\Delta_s}{(1+r)^s}}_{\text{Post-entry treatment effects}} \quad .$$

The first group in (58) includes the "compliers" (i.e., individuals who are able to find a first

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[74]Assumption (iv) implies that people always find a job by the second year of the search. It is possible to extend this assumption, but the calculations are not as easy. However, given our findings that the number of new entrants appears flat in event time (see Figure C.9), we believe that our results are robust to alternative assumptions.

[75]Assumption (i) is imposed to simplify exposition. In our empirical setup, we assume that reform status is as good as randomly assigned after imposing a standard difference-in-differences structure as detailed in equation (6).

job in the first year of the search in the reform counterfactual but not in the non-reform one).[76] This creates an extensive margin effect at entry whose magnitude depends on the fraction of these compliers multiplied by their average level of earnings. The second group with $E_{ie}^R = E_{ie}^{NR}$ refers to the "always-takers" (i.e., individuals who would find a first job in the first year under both counterfactual scenarios). For these individuals, we need to compare earnings in the first year across reform status (captured by Δ_0). Finally, we need to sum the discounted income differences Δ_s for $s \geq 1$ following entry, as reflected by the third term in (58).

Our key data limitation is that we observe earnings only conditional on first entrance in the social security data. Hence, we can only compare earnings of treated and control individuals conditional on their having obtained a first job in a given year of entry c. This implies that we cannot identify Δ_s using observed average income differences

$$E[Y_{ie+s}|R_i = 1, E_{ie} = 1] - \mathrm{E}[Y_{ie+s}|R_i = 0, E_{ie} = 1] =$$
$$= \pi_e \left\{ \mathrm{E}[Y_{ie+s}^R|E_{ie}^R > E_{ie}^{NR}] - \mathrm{E}[Y_{ie+s}^R|E_{ie}^R = E_{ie}^{NR} = 1] \right\} + \Delta_s,$$

where $\pi_e = \dfrac{\Pr(E_{ie}^R > E_{ie}^{NR})}{\Pr(E_{ie} = 1|R_i = 1)}$ is the share of new entrants after the reform that wouldn't have found a job before the reform.

Under the assumption that the reform affects only the probability of finding a job and the average income of those who do find a job (i.e., there are no expected income differences between the compliers and always-takers), we can identify Δ_s and estimate the probability π_e needed to compensate for the estimated earnings losses.[77] We can then ask the question of how large the extensive margin response at entry caused by the reform—defined as π_e—needs to be in order to set to zero the PDV earnings losses estimated after entry into the labor market. That is, we solve for π_e from the following expression:

$$0 = \underbrace{\pi_e \left\{ \mathrm{E}[Y_{ie}|R_i = 0, E_{ie} = 1] - \Delta_0 \right\}}_{\substack{\text{Extensive margin} \\ \text{effect at entry}}} + \underbrace{(1 - \pi_e) \Delta_0}_{\substack{\text{Intensive margin} \\ \text{effect at entry}}} + \underbrace{\sum_{t=1}^{S} \frac{\Delta_s}{(1+r)^t}}_{\substack{\text{Post-entry} \\ \text{treatment effects}}}.$$

[76]These are individuals such that $E_{ie}^R > E_{ie}^{NR}$ who populate the red area in Figure D.1 in our model.

[77]The expected income assumption has two effects that go in opposite directions regarding our estimate of π_e. On the one hand, it implies that the income of compliers is higher than what we may expect given the results found throughout this paper. This implies that the reform's income effect through the increased entry rate is overestimated, leading to a lower π_e bound. On the other hand, the assumption implies that Δ_s is more negative than if compliers were actually less productive than always-takers, hence leading to a higher π_e bound.
An alternative methodology, closer to Lee (2009), would be to assume that compliers' income is distributed differently than always-takers' income. This would require us to impose a particular (and ad hoc) income structure to help us bound either Δ_s or the expected income of compliers. For simplicity, we abstract away from this alternative procedure here.

Using our estimates for earnings losses after entry and a discount rate of 3.5%, we find that the share π_e of individuals who find a first job only after the reform would need to be 20.5% in order to equalize the net present value of earnings before and after the reform. Figure C.9 shows that the logarithm of the number of new entrants appears flat in event time, which implies that the share of compliers is relatively small. More importantly, this suggests that our main results on earnings losses for new entrants are robust to a potential increase in the entry rate.