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 contracts. Exploiting the staggered implementation of the reform across different collective bargaining agreements, we find that this policy change led to an increase in the share of temporary contracts but failed to raise employment. The reform had both winners and losers. Firms are 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 temporary workers receive only two-thirds of the rents shared by firms with permanent workers, helping explain most of the labor costs and earnings reductions caused by the reform.

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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 legislation (EPL) depresses 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 employment protection granted to permanent-contract workers remained unchanged, perhaps due to political constraints (Saint-Paul, 2000).

Economic theory delivers ambiguous predictions on the effects of partial EPL reforms. Such reforms could generate higher 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 employment gain (Bentolila and Saint-Paul, 1992; Cahuc and Postel-Vinay, 2002; Blanchard and Landier, 2002). Partial reforms may even increase the bargaining power of incumbent workers, usually hired via permanent contracts, thus enhancing the "insider-outsider" gap and reducing efficiency (Bentolila and Dolado, 1994).

Empirical assessments of these reforms, 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 with firm-level aggregate data (e.g., Autor et al., 2007; Cappellari et al., 2012). While informative, an analysis of firm aggregates cannot directly address why for instance the stock of temporary jobs might be increasing—is it because temporary jobs are being renewed more often or because new jobs are now created with temporary contracts? A firm-level analysis also ignores any distributional impact that these reforms might have across different groups of 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 a 2001 Italian reform. This reform—first studied by Cappellari et al. (2012) using firm-level data—facilitated the creation of new temporary contracts while maintaining existing employment protections for workers with permanent contracts. Matched employer-employee data allow us to study in detail the reform's effects on job creation/destruction and their mapping into employment. The richness of our data is also particularly useful to assess the reform's unequal effects across different groups (e.g., firms vs. workers or young vs. older workers) and provide further insights on the relevant theoretical mechanisms behind the reform's main effects.

Contrary to the stated intent of the law (Biagi and Sacconi, 2001), we find that the reform had no effect on employment. While the reform did increase the creation of temporary jobs, it also increased job destruction. In particular, more workers were trapped in cycles of low-paid and fragile temporary jobs with a substantially reduced likelihood of transitioning from temporary to permanent jobs. Despite its null effect on employment, the reform still generated both winners and losers. The primary winners appear to be firms as labor costs fell substantially, leading to an increase in profits after the reform. Young workers, by contrast, appear to be the main losers since their earnings were substantially depressed, leading to a widening gap in earnings between young and old workers in the Italian labor market.

Our research design builds on Cappellari et al. (2012) and exploits the staggered implementation of the reform across different sectoral collective bargaining agreements (*Contratti Collettivi Nazionali del Lavoro*; henceforth CCNLs) in an event study framework. While Cappellari et al. (2012) rely on 8 CCNLs and survey information on firms' sectors to infer the reform's passage, 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 before the reform's implementation, suggesting 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 aggregate (i.e., economy-wide) effect of the reform on the dynamics of job creation/destruction and employment. In line with the intended consequences of the law, the reform fostered job creation and increased the share of new jobs signed under a temporary contract. However, 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. The entire observed increase in temporary job destruction is, in fact, explained by the decline in temporary to permanent transitions occurring within the same employer. Relatedly, most of the new temporary jobs created after the reform are actually going to previously employed workers rather than to previously non-employed ones. The general pattern therefore suggests that the share of temporary jobs increases after the reform because existing workers are more likely to bounce from an existing temporary job to a new temporary job with a different employer, without increasing employment. These negative effects are particularly pronounced among young workers, the group that was supposed to benefit the most by the reform (Biagi and Sacconi, 2001).

Second, we analyze the reform's effect on firms. We find that incumbent firms responded to the reform by increasing their share of temporary employees without, however, expanding in size. These within-firm effects are quantitatively similar to the estimates at an aggregate level, thus suggesting that firm entry and exit was not a relevant margin for the policy's effects. While firm size was unaffected, this increase in the utilization of temporary workers, however, did map into a sizable rise in firms' profit margins (profits divided by value added), of approximately 8%. This increase in profits appears to be driven primarily by a reduction in firms' labor costs rather than by an increase in productivity. In fact, productivity (measured as log value added per worker), if anything, decreased due to the reform. 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., 2021). Instead, as detailed below, it appears that reductions in employment protection might cause a reallocation effect toward jobs with lower value added per worker (Acemoglu, 2001; Dustmann et al., 2021).

The firm-level analysis also shows that effects across firms are highly heterogeneous, which helps shed light on the mechanisms behind the reform's effects. We find that the reform had larger temporary employment, labor costs, and profits effects on firms with (i) high pre-reform labor costs per worker, (ii) relatively low turnover costs, (iii) a high firm-wage premia, and (iv) a low expected survival rate. This is not surprising since these are probably the firms with the largest incentives to react to the reform. The negative effect on productivity, however, is concentrated only among group (iv)—i.e., firms with low ex-ante probability of surviving—which also tend to be low-quality firms (with low-profit margins, value added per worker, and labor costs per worker). The fact that productivity did not decrease among the groups (i)–(iii) listed above suggests that neither replacing relatively expensive workers with cheaper (temporary) ones nor reductions in the wage premium (likely because of differential rent-sharing) seem to explain the observed productivity decrease for the average firm. Instead, productivity appears to have declined particularly among firms with a low expected survival rate as the reform enabled the creation of low-quality jobs that would not have existed otherwise.

Third, we examine the reform's effect on workers. We start by focusing on incumbent temporary workers, i.e., relatively young individuals who were always employed with a temporary contract before the reform. We find that the reform made these workers experience significant earnings losses of around 7%, most of which is explained by reductions in daily wages (rather than days worked). Conversely, the earnings of incumbent *permanent* workers were not significantly affected. Thus, the reform led to an increase in the labor market duality between temporary and permanent workers (and therefore also between young and older workers).

But why did incumbent temporary workers experience significant daily wage losses? We show that incumbent temporary workers appear to be employed in marginal temporary jobs that, in the pre-reform period, were expected to be converted into permanent positions by their employer. The reform's arrival reduced firms' likelihood to convert temporary jobs into permanent ones by almost 32% (60% for workers aged 25 or less) and, consequently, incumbent temporary workers reallocated into lower-quality jobs. These negative reallocation effects induced by partial EPL reforms are thus symmetric to the ones of Dustmann et al. (2021), who show how an increase to the minimum wage in Germany (a reform that increases labor costs, contrary to the one studied here) helped workers sort into higher-quality jobs.

To complete the analysis on the reform's effect on young individuals in general, however, we also must understand its effects on *new* workers, not just on young incumbent workers. We thus evaluate the short- and medium-run effects of entering the labor market following the reform's passage. By combining our difference-in-differences analysis with the empirical framework of Oreopoulos et al. (2012), we find that cohorts entering the labor market after 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. The earnings losses are not due to selective entry or changes in the composition of new entrants. Instead, post-reform cohorts appear disproportionately less likely to obtain stable and high-paying jobs. This suggests that temporary employment contracts created as a result of a partial EPL reforms do not represent stepping stones into the labor market (Booth et al., 2002; Autor and Houseman, 2010; García-Pérez et al., 2019). Instead, in line with Blanchard and Landier (2002), these reforms appear to mainly foster the creation of highly precarious and fragile temporary jobs.

We conclude our analysis by evaluating a potential driver of the reform-induced large reduction in both firms' labor costs and temporary workers' earnings—two of our main results. Motivated by institutional features that suggest temporary contract workers are largely underrepresented in both unions and firm-level wage agreements (Bentolila and Dolado, 1994; Montanari, 2002; Lani, 2013), we focus on the role of firm-specific pay policies that may discriminate between temporary and permanent workers. Extending the analysis of Kahn (2016), we provide new evidence on the existence of a permanent contract premium in wages (of between 6% and 8%) using an event study design that focuses on the within-person, within-employer return of transitioning from a temporary to a permanent contract. In line with recent results from Goldschmidt and Schmieder (2017) and Drenik et al. (2021), we find that almost all of wage return associated with a temporary to permanent within-employer conversion is explained by rents not being distributed equally between temporary and permanent workers within the same firm. The resulting estimated difference in the relative bargaining power of temporary versus permanent workers explains over half of the reduction in firm labor costs and three-quarters of incumbent temporary workers' earnings losses.

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 must pay high firing costs depending on firm size (above/below 15 employees) 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 firm costs 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 a written notice to the social security agency 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 from 1962.[3] 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 set of cases which allowed temporary contracts with a general provision 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]

Decree 368/2001—henceforth "the reform"—represents a fundamental milestone for Ital-

[1]Permanent employment contracts usually have a probationary period that varies across CCNLs, with a typical length of around six months; see also the EPL Database compiled by the International Labour Organization.

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

[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 (typically two to three) are legislated by sectoral bargaining agreements.

ian EPL 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 the reform and in fact has remained unchanged for almost 30 years.[6]

In conclusion, the Italian labor market in the pre-reform period 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. The reform 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 productivity or employment levels, as discussed in more detail in the next section.

3 Economic Impact of Partial Reforms to EPL

This section discusses how the partial reform to EPL studied here might affect key economic outcomes such as employment, productivity, and earnings. The general message is that economic theory tends to deliver ambiguous predictions.

Employment. The reform is expected to have a positive effect on both job creation and job destruction and thus, ultimately, an ambiguous effect on employment. Appendix E presents a theoretical model that formalizes these predictions, which are shared by several other models that have analyzed partial EPL reforms (e.g., Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002; Cahuc et al., 2016). The intuition is the following. The reform facilitated the creation of temporary employment contracts, which has a positive effect on job creation. However, two restrictions remained in place in the post-reform: (i) firms can only keep workers under a temporary employment contract for a limited time, and (ii) permanent 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 make firms also 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 would increasingly prefer 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. Thus, the reform is also going to increase the destruction rate of temporary jobs. This positive effect on job destruction is in parallel with the positive effect on job creation highlighted above and can ultimately lead to null or negative effects of

[6] Online Appendix Figure G1 captures this duality in the Italian labor market by plotting the evolution of the EPL Index constructed by the OECD in Italy and other countries between 1985–2013.

the reform on aggregate employment.

Productivity. The reform's effect on productivity is also theoretically ambiguous. On the one hand, by reducing the cost of creating jobs, this reform can help firms screen and learn about the quality of workers, thus raising match quality and productivity (Faccini, 2014). A more flexible regime on employment protection may also reduce workers' hold-up power, thus raising the share of revenue kept by firms and increasing their incentives to invest in productivity-enhancing technologies or inputs (Grout, 1984; Card et al., 2014; Jäger et al., 2021). On the other hand, higher temporary job destruction rates (due to laxer EPL) might reduce on-the-job training, leading to lower human capital and productivity (Cabrales et al., 2017; Doepke and Gaetani, 2020). Finally, by reducing the cost of job creation, the reform might increase the share of lower-quality jobs that are created. As in Acemoglu (2001), search frictions and general equilibrium forces (i.e., it is harder to find workers with more low-quality jobs being posted) might also lead to the creation of fewer high-quality jobs in response to the reform.

Workers' Earnings and Firms' Labor Costs. If temporary workers tend to keep a lower proportion of rents than permanent workers (e.g., Guiso et al., 2005; Card et al., 2014), the reform's net effect on the prevalence of temporary contracts (as discussed above) can have an effect on workers' earnings. In particular, if, for example, the reform tends to increase the share of temporary workers, we may expect earnings to be reduced. Alternatively, if the reform is able to promote permanent employment, earnings may increase. In addition, the reform's ambiguous effect on productivity enhances even more the ambiguity of its impact on earnings.

Moreover, rent-sharing might change after the reform if outside options and bargaining power are affected. As previously mentioned, for example, some studies suggest that partial EPL reforms could end up increasing the bargaining power of incumbent workers, usually hired via permanent contracts, thus enhancing the insider-outsider gap (Bentolila and Dolado, 1994). On the other hand, if aggregate employment or productivity increased, outside options for all workers might improve, increasing their bargaining power and wages. As is clear by this discussion, this indeterminacy on the reform's impact on labor earnings also leads to theoretically ambiguous effects on firms' labor costs and profit margins.

4 Data

Having described the reform's institutional background and potential effects, we now present the data used for the empirical analysis. Section 4.1 introduces the social security data. Section 4.2 outlines the data collection on CCNLs, which is key to our research design. Section 4.3 describes the income statement data used for the firm analysis, and Section 4.4 presents summary statistics. Appendix A provides further details.

4.1 Matched Employer-Employee Data

The matched employer-employee data used for the analysis 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 by a firm covered by the INVIND survey conducted by the Bank of Italy. This social security dataset (henceforth INPS-INVIND) is a matched employer-employee dataset that contains information on employment spells. INPS-INVIND covers around 7.5 million workers—roughly 40%–50% of the universe of individuals present in the INPS's records. For each employment spell in a given year, we have data on earnings, number of days worked, and the employer's identity as well as some demographic information on the employee.[8]

Critically, INPS-INVIND provides administrative information on whether a job is under a temporary employment contract.[9] In our analysis, apprenticeships are not considered 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 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.

4.2 CCNL Data

We also collect information on each CCNL's renewal year. INPS-INVIND provides administrative information on the CCNL associated with each job. The *Centro Nazionale dell'Economia e del Lavoro* (CNEL) provides a digital archive of all CCNLs signed in Italy, from which we obtain the renewal years. We can match information on the history of renewals for 181 sectoral agreements, covering approximately 98% of all person-year observations in INPS-INVIND.[10]

4.3 Firm Data

To study the reform's effect on firms, we augment the baseline matched employer-employee data with two additional sources. First, we have administrative information on all Italian em-

[7]Excluding self-employed and some specific public sectors workers, nearly all workers are covered by INPS.

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

[9]While we can distinguish between temporary contracts signed directly by the firm with the employee and those signed indirectly via a temporary work agency, in the latter we cannot observe the user firm (Drenik et al., 2021). We highlight, however, that 88% of temporary workers are hired directly by the user firm, so we observe the user firm for the vast majority of the temporary workers in our data.

[10]Unmatched cases occur when 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.

ployers with at least one employee, a file that we label as "Anagrafica".[11] These data include information on the national tax identifier, monthly employment counts, geographical location, and additional information on firm average earnings by different occupations.

We use the tax identifier 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.[12] Overall, we can match balance sheet information from Cerved for approximately 40% of the firm-year observations that we observe in our matched employer-employee dataset (see Table G.1 in the Online Appendix). As usual, when working with matched employer-employee datasets and balance sheet information (e.g., Card et al., 2014), the matching rate improves substantially for larger firms.

4.4 Summary Statistics

Table 1 presents average characteristics for temporary and permanent contracts. The share of workers with a temporary contract is approximately 16%. The vast majority of temporary contracts are signed directly by firms—only 12% of temporary jobs are obtained through a temporary work agency. Relative to permanent workers, temporary workers are younger, more likely to be female, to work on part-time contracts, to enter the labor market at an earlier age. Temporary workers also tend to have a shorter tenure (1.8 vs 6.5 years) and are slightly more likely to hold multiple jobs within the year.[13] 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 versus 298 for a permanent worker). However, the gap in log daily wage remains substantial and equal to about 31 log points. Regarding workplaces, temporary workers tend to be employed in firms that are smaller and have lower value added per worker. Finally, all reported statistics are virtually unchanged when we turn from the universe of person-year observations in INPS-INVIND to the matched sample that contains information on the associated CCNLs' renewal year.

5 Research Design

The research design used in this paper leverages administrative data on the CCNL applied to a given job with information on the renewals of these CCNLs. CCNLs in Italy take a very

[11]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 the INPS as our definition of a firm. Although approximately 7% of firms have multiple EINs, the INPS also records, within each parent firm, a "main EIN" identifier for the parent firm's headquarters.

[12]The Cerved dataset is derived from standardized reports that firms are required to file annually. It excludes private partnerships and sole proprietorships, which are characteristic of smaller firms.

[13]See Online Appendix Table G.2 for the average number of jobs in a month.

distinctive form. Their primary purpose 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 worker's unionization, and (iii) firms can provide "top-ups" above them (Card et al., 2014). However, CCNLs can also legislate on several other aspects, such as the maximum number of times that temporary contracts can be renewed or regulations on hours worked.[14]

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.[15] In effect, this means that some sectors are going to implement the new rules on temporary 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. To exploit these differences across CCNLs, we combine administrative micro-level information on the CCNL applied to each job in the INPS's records with the digital archives of Italian CCNLs contained in the CNEL archive. Using this information, we can infer the reform implementation year of 181 CCNLs—plotted in Panel (a) of Figure 1—as the date of the first CCNL renewal signed after the reform was passed.[16]

To better understand how the legislative framework regarding temporary work changed after the reform's approval, Appendix B examines in detail the text of the retail, food, and metal manufacturing sectors' CCNLs, which are some of the most prevalent contracts registered in the INPS-INVIND administrative data. The retail and food sectors provide good examples of the idea behind the research design. CCNLs signed before the reform stated clearly that employment contracts should typically be on a permanent basis and listed only particular circumstances under which it was possible to hire on a temporary basis. CCNLs signed after the reform no longer state that an employment contract will typically be on a permanent basis. Moreover, the list of special circumstances that allow hiring on a temporary basis is typically no longer specified in the post-reform CCNL.

Panel (b) of Figure 1 illustrates the key variation leveraged by our research design. This figure 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

[14]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 typically overlap with standard definitions of industries based, for instance, on ATECO codes. In some occasions, CCNLs' provisions might be province specific, although this is not particularly common (Boeri et al., 2021).

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

[16]Note that in our main design all CCNLs are eventually going to renew and thus implement the reform. More importantly, all CCNLs' renewals tend to occur in a relatively short window of time. Appendix C.2 presents several robustness checks designed to assess how this impacts our main estimates.

handicraft sector).[17] Both CCNLs have a roughly similar trend in the share of temporary jobs before 2001. In 2002, when 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.

There are two main potential concerns with our research design. First, one may worry that unions and employers organizations could endogenously choose when to renew based on the expected effects of the reform. We do not find evidence of this. In fact, only 15% of CCNLs that occurred after the reform was passed were "irregular," meaning they were signed over two years after the previous agreement, which is the typical duration of CCNLs during our sample period (Brandolini et al., 2007; D'Amuri and Nizzi, 2018). These irregular cases seem to be driven by historical reasons, and thus their longer duration does not appear endogenous to the reform: 99% of CCNLs that took longer than two years to renew after the reform also took more than two years to renew in the years right before the reform. The share is more than 97% when looking at CCNLs that took more than three years to renew.[18] Nevertheless, as a robustness check, Appendix C.3 shows that the results are unchanged if we use the ending date of the previous agreement (rather than the initial date of the new agreement) as the year of the reform's enactment.

Second, since the year of the reform's implementation coincides with the associated CCNL's renewal, our estimates may mix the reform's effect with the effects of the CCNL renewal (e.g., the update of the associated wage floors). Appendix C.3, however, presents several analyses—including a placebo exercise—that strongly suggest this is not a concern for our results.

6 Econometric Framework

This section describes how the staggered implementation of the reform is used to quantify its effects on the three fundamental facets of the labor market: jobs, firms, and workers.

6.1 Job Creation, Job Destruction, and Employment

As described in Section 3, the reform is expected to increase the share of temporary jobs. It is also expected to increase both temporary job creation and job destruction, thus having an ambiguous effect on employment. To test these predictions and quantify the reform's effect on aggregate employment, we cast the raw difference-in-differences variation plotted in Panel (b)

[17] Note that the metal *handicraft* sector is different from the metal *manufacturing* one in Appendix B.

[18] All these statistics are person-year weighted. The largest CCNL that enacted relatively late in our analysis is the metal handicraft CCNL that renewed in 2005 even though the last agreement was signed in 2000. However, this longer gap between agreements is common for this CCNL, which has typical gaps of four to five years.

of Figure 1 in the following event study framework:

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

where y_{cpt} is an outcome—such as the share of temporary jobs or log of total employment—observed in CCNL c, province p, and year t. The vector of controls X_{cpt} includes the fraction of female workers, the fraction of Italian workers, and a quadratic term of average potential experience (i.e., years passed since first entry into the labor market). To account for serial correlation of the error term within a given local labor market (LLM)—which we define as a unique CCNL-province combination—the standard errors in (1) and in all our subsequent regressions are clustered at the LLM level. The regression uses as weights the total number of workers present in a given LLM × year cell.

The event study indicators R_{ct}^{k} are our treatment of interest as they capture time elapsed from the reform's passage; i.e., $R_{ct}^{k} \equiv \mathbf{1}\{t = t_c^* + k\}$, where t_c^* is the year of the reform's implementation in CCNL c. All our event study indicators are binned at $a = -3$ and $b = 3$, and we always normalize θ_{-1} to 0.[19] The coefficients θ_k when $k \geq 0$ thus capture the reform's effect on outcome y_{cpt}, k years after the reform's implementation relative to the year before implementation. When reporting our estimates throughout the paper, 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 four-plus years after the reform is implemented.

In equation (1), and similarly for our subsequent regressions, identification of θ_k hinges on the assumption that CCNLs yet to implement the reform ("control" CCNLs) can be used as a counterfactual for CCNLs that have implemented it ("treated" CCNLs). Although this identifying assumption cannot be tested directly, our analysis leverages data from before the reform and rich econometric specifications to maximize its plausibility. Specifically, the inclusion of province-specific time effects controls for unobserved shocks specific to a given province. This allows us to construct potentially more realistic counterfactuals where the identification of θ_k is based on contrasting the outcomes of treated CCNLs to control CCNLs within a given province × year cell. As a further check, we also evaluate whether the parallel trend assumption holds in the years leading up to the reform by computing the coefficients θ_k for $k < 0$.[20]

Sample. The share of temporary jobs, share of new temporary jobs, and destruction rate of temporary jobs are all calculated from the person-year panel described in Table 1. The share of temporary jobs is defined as the fraction of jobs observed in a given LLM × year that are

[19] Binning helps avoid collinearity issues due to the lack of never-treated units (Borusyak et al., 2021).

[20] In Appendix C.2 we show that our estimates from the two-way fixed effects model specified in equation (1) are fairly similar to the ones obtained by running recently proposed estimators designed to handle treatment effects heterogeneity in event-study research designs (e.g., Sun and Abraham, 2020; Borusyak et al., 2021).

under a temporary contract. When assessing the effects of the reform on job creation, our outcome of interest is the fraction of jobs that are new temporary jobs, i.e., temporary jobs where the corresponding firm-worker match is observed for the first time in the INPS-INVIND data. To assess the impact on job destruction, we look at the job destruction rate of expiring temporary jobs.[21] That is, we look at all expiring temporary jobs in year t and compute the fraction of these jobs that ended up being destroyed in year $t+1$ (the worker-firm match is no longer observed under either a temporary or a permanent contract). To measure the effects on employment, we use the data described in Section 4.3 that contain employment counts for the universe of private-sector employers in Italy (see Appendix A.3 for details).

6.2 Firms

After assessing the reform's effect on aggregate margins (e.g., share of temporary jobs and total employment), we evaluate how the reform affected firms and workers separately. The effects on firms are estimated using an event study specification akin to (1) at the firm level:

$$y_{ft} = \psi_f + \lambda_{p(f),t} + \sum_{k=a}^{b} R_{ft}^k \theta_k + r_{ft}, \tag{2}$$

where y_{ft} is a firm-level outcome and $R_{ft} = \mathbf{1}\{t = t^*_{c(f,2001)} + k\}$ is an event study indicator, with $c(f, 2001)$ being a function that returns the modal CCNL applied by firm f before the reform was implemented. 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.[22] The inclusion of firm and province-by-year fixed effects implies that the difference-in-differences coefficients $\{\theta_k\}$ in (2) 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.

Sample. The analysis is performed on the sample of firms for which we can match Cerved's income statements (henceforth, the INPS-INVIND-Cerved sample). The results are weighted using inverse propensity score weights so as to match the pre-reform average firm size and industry composition observed in the INPS-INVIND-Cerved sample to the one observed in our baseline INPS-INVIND data, collapsed at the firm level (see Section 4.3 and Appendix A.2 for further details). To determine when a firm implemented the reform, equation (2) uses the modal CCNL applied by the firm in 2001 and uses this CCNL's enactment year to determine

[21] Economic theory (including our model in Appendix E) delivers sharp predictions of the on-the-job destruction rate of temporary contracts that could no longer be renewed by the firm. Unfortunately, we cannot perfectly observe the expiration date of each temporary contract in our data. We thus define expiring temporary jobs as jobs that in year t already existed for two or more years as most CCNLs in our sample period stipulate that employment relationships can last at most two or three years under a temporary employment contract.

[22] When firms have multiple establishments, we use the province where the firm's headquarters is located.

whether a firm-year observation is in the pre- or post-reform period.[23,24] It follows, then, that the firm analysis is restricted to incumbent firms that already existed in the pre-reform period.

6.3 Workers

The analysis at the worker level is divided into two parts. We begin by introducing the regression framework used to analyze the impact of the reform on incumbent workers. Focusing on incumbent workers allows us to construct a worker-level measure of exposure to the reform in a similar way as in the firm-level analysis. We then present the econometric framework for new labor market entrants. This represents, perhaps, the group of workers most heavily targeted by the reform since the first job that young workers obtain in the labor market is usually a temporary one. However, since we only have information on new entrants *following* their entry in the labor market, it is hard to create an ex-ante measure of "exposure" for new entrants as we did for incumbent workers. Section 6.3.2 thus presents an augmented econometric model, similar in spirit to the one used by Oreopoulos, Von Wachter, and Heisz (2012), to assess the short- and medium-run effects of entering the labor market following the reform's passage.

6.3.1 Incumbents

When evaluating effects on incumbent workers, we implement an event study design similar to (2) at the individual level:

$$y_{it} = \eta_{LLM(i,2001)} + \lambda_{p(i,2001),t} + \sum_{k=a}^{b} R_{it}^{k}\theta_k + X_{it}^{\top}\beta + r_{it}, \tag{3}$$

where $R_{it}^{k} = \mathbf{1}\{t = t^{*}_{c(i,2001)} + k\}$ is an event study indicator and $c(i,t)$ is a function that returns the CCNL of individual i in year t. Hence, in equation (3), individuals are treated according to when their 2001 CCNL implemented the reform. $\eta_{LLM(i,2001)}$ and $\lambda_{p(i,2001),t}$ denote 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. 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.[25]

[23] Almost all 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 managers since they typically have their own separate CCNL.

[24] It is relatively rare (less than 3%) to observe firms switching CCNLs that would entail different years of the reform's implementation. Similarly, workers' movements between jobs with different CCNLs and years of the reform's implementation is relatively low and does not appear to spike during the years in which the reform was implemented (see Table G.3). The possibility of firms selecting different CCNLs was virtually non-existent during the 2000s, the key period analyzed in this paper. Only after 2011–2012 has it become more common to observe firms "opting out" from major CCNLs in favor of new and, often, ad hoc CCNLs (Lucifora and Vigani, 2019).

[25] Clearly, individuals might move between different CCNLs and hence between reform statuses. This is why equation (3) primarily captures intention to treat type of effects. See also Table G.3.

Sample. We estimate Equation (3) separately on incumbent permanent workers and incumbent temporary workers. The latter correspond to individuals always employed with a temporary contract over the pre-reform period 1998–2001. The former are those always employed with a permanent contract during the same period. Although our results are robust to including all individuals, we are primarily interested in analyzing the reform's effect on individuals' career progression so we restrict our analysis to those aged 40 and under in 2001.[26]

6.3.2 New Entrants

Following Oreopoulos, Von Wachter, and Heisz (2012), the regression model used to analyze the reform's effect on the career evolution of new labor market entrants reads as follows:

$$y_{ecpst} = \eta_{cp} + \lambda_{pt} + \phi_e + \chi_s + \sum_{s=1}^{7} \theta_s T_{ec} + r_{ecpst}, \tag{4}$$

where e refers to the year of entry into the labor market, c is the CCNL at the time of entry, p is the province of entry so that η_{cp} are LLM market (at entry) fixed effects, λ_{pt} are calendar year fixed effects that are province specific, and ϕ_e and χ_s are year of entry and years of potential labor market experience fixed effects, respectively.[27] $T_{ec} = \mathbf{1}\{e \geq t_c^*\}$ 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 workers' experience profiles. Each coefficient $\{\theta\}_{s=1}^{7}$ is identified by standard difference-in-differences arguments since the reform's staggered implementation 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.[28] Key to the interpretation of the results is that new entrants do not select their CCNL of entry based on the reform status. Section 8.2.2 presents some complementary evidence consistent with this identifying assumption.

Sample. We consider all individuals who entered the labor market between 1998 and 2005 and were not yet 30 years old at the moment of entry. Following Oreopoulos, Von Wachter, and Heisz (2012), we collapse these data into cells defined by year of entry into the labor market (e), potential experience (ℓ), CCNL at entry (c), province of entry (p), and calendar year (t). The results are weighted by cell size.

[26] The key differences between incumbent temporary and permanent workers in this sample (as reported in Online Appendix Table G.4) are similar to those reported for the general population in Table 1.

[27] It is well known that cohort, potential experience, and year effects cannot be separately identified in equation (4). 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.

[28] This is similar to the strategy pursued by Oreopoulos, Von Wachter, and Heisz (2012) who compared graduating cohorts characterized by different levels of unemployment upon entering the labor market.

7 Job Creation, Job Destruction, and Employment

This section quantifies the reform's effects on the share of temporary jobs, temporary job creation/destruction, and employment. Many papers have shown how partial reforms to EPL impact employment or the share of temporary jobs (e.g., Cappellari et al., 2012); see Appendix Figure G2 for a review of estimates of EPL on employment. Yet, most of these studies are based on aggregate data and thus cannot fully explain how, for instance, changes to EPL affect the stock of temporary jobs. Is the stock of temporary jobs rising because new temporary jobs are being created, or is it because existing ones are being renewed? Are existing employees filling up these jobs, or are these jobs being taken by new labor market entrants? Similarly, any study based on aggregate firm or sector data is typically unable to show effects on the destruction rate of existing temporary jobs. The latter, however, represents the crucial unintended consequence of the reform and is instrumental to rationalize effects on employment, as explained in Section 3. Finally, aggregate data are typically unable to isolate the effects on the employment of young workers, who represent the category most affected by this reform. The availability of matched employer-employee data permits us to zoom into these important margins in a comprehensive way and represents a key novelty of the results presented in this section.

Share of Temporary Contracts. In line with the theoretical predictions explained in Section 3, we find that the reform increased the share of temporary contracts. Panel (a) of Figure 2 plots the event study coefficients $\{\theta_k\}$ from equation (1) when the dependent variable represents the share of temporary jobs. These event study coefficients are relatively flat and close to zero in the years before the reform, providing suggestive evidence in favor of the common-trend assumption. At the reform's enactment, there is a significant increase of about 1 percentage point in the share of jobs covered by temporary contracts. The effect is even larger over time, increasing to around 4 percentage points in the medium run, which amounts to an increase of around 36% relative to the pre-reform share of temporary contracts.

Job Creation. The reform significantly increased the creation of temporary jobs as shown in Panel (b) of Figure 2. Again, we find evidence in support of the parallel trends assumption: before the reform's implementation, the event study coefficients exhibit a relatively flat profile. At enactment, the share of new temporary jobs increases by 1 percentage point. The share of new temporary jobs continues to grow post-reform by up to 3 percentage points, which is around 43% of its pre-reform value. By comparing Panels (a) and (b) of Figure 2, we see that the stock of temporary jobs is therefore primarily increasing as a result 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).

Who is filling these new temporary jobs? Online Appendix Figure G3 shows that around

30% of these new temporary jobs are filled up by individuals who were previously non-employed. Thus, around 70% of these new jobs are being taken by workers doing job-to-job transitions. This suggests that the share of new temporary jobs is not increasing because workers are entering the labor market and filling up these new temporary jobs. Instead, the share of new temporary jobs is increasing because, after the reform, existing workers are disproportionately more likely to bounce from an existing job to a temporary job with a different employer.[29]

Job Destruction. Consistent with the above discussion, Panel (c) of Figure 2 shows that the reform significantly increased the destruction rate of expiring temporary jobs. We again find 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 destruction rate of expiring temporary jobs), which increases to 5.5 percentage points (21% of the corresponding pre-reform year-to-year destruction rate) in the medium run. The analysis presented in Appendix D.1 and Table D1 shows that the increase in the destruction rate of temporary jobs shown in Panel (c) is entirely explained by a reduction in the within-firm conversion of temporary contracts into permanent ones and is particularly pronounced among temporary jobs held by young workers. Thus, as we further elaborate in the next sections, the reform seems to have increased the job destruction rate by reducing the probability that workers (particularly young ones) move up the job ladder within the firm.

Employment. Given that the reform increased both job creation and destruction, one may wonder what the net effect on aggregate employment is. Panel (d) of Figure 2 shows that the effect on log employment is statistically indistinguishable from zero. Following the reform, total employment exhibits a flat profile (as in the pre-reform period) with a point estimate that is negative but statistically insignificant. Our 95% confidence intervals reject medium-run increases in employment larger than 0.8%. Therefore, the increase in the share of temporary jobs (and job creation) achieved through the reform does not appear to translate into a significant increase in employment. Importantly, Panel (d) also shows that the reform did not lead to an increase in employment of young workers, which perhaps represented its primary goal (Biagi and Sacconi, 2001). All in all, the reform appears to have failed to raise employment.

Robustness. Our key results on the reform's effects on the share of temporary contracts and total employment are robust to a series of potential concerns. First, in our main design, all units are eventually treated and do so in a relatively short window of time. To assess how this impacts our estimates, Appendix C.1 shows that we obtain very similar results if the metal manufacturing sector is assumed to be a "control" CCNL that did not implement the reform as

[29] The analysis presented in Appendix D.1 complements this result and shows in particular that the reform significantly increased temporary-to-temporary job mobility across different employers; see Table D1.

argued by some labor law scholars (Santucci et al., 2008) and discussed in Appendix B.3.

Second, recent papers (e.g., Goodman-Bacon, 2021) have warned against identification of average treatment effects from two-way fixed effects models—as in equation (1)—in the presence of treatment effects heterogeneity. Appendix C.2 demonstrates that we obtain virtually the same event study estimates based on two alternative estimators designed to deal with these issues (Sun and Abraham, 2020; Borusyak et al., 2021).

Finally, Appendix C.3 shows that our main conclusions are robust to (i) using an alternative definition of the year of the reform's implementation, (ii) estimating the incidence of the reform focusing only on temporary jobs signed directly by the firm, (iii) using only "balanced" sectors already existing before 2001, and (iv) running the analysis without controls.

8 Winners and Losers

Having analyzed the reform's main effects on the creation and destruction of temporary jobs as well as on total employment, we now show that there were both winners and losers stemming from this policy change. Section 8.1 shows that firms appear to be the main winners as the reform led to higher profits. By contrast, Section 8.2 shows that young workers are the main losers since their earnings were substantially depressed. Those who already had a permanent job instead appear to be essentially unaffected by the reform.

8.1 Firms

Panel (a) of Figure 3 shows estimates from equation (2) using as an outcome the fraction of temporary workers within the firm. The event study coefficients exhibit a flat profile centered around zero before the reform. Upon the reform's implementation, 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 33% of the pre-reform firm-level share of temporary workers. Panel (b) shows that the substantial increase in the fraction of temporary workers does not map into a significant firm-level increase in size. Note that both of these effects are not only qualitatively but also quantitatively similar to those at the LLM level, as displayed in Figure 2. This suggests that the main aggregate effects are *not* driven by firm entry and exit (e.g., entry of new firms that hire more temporary workers than incumbent firms do). More importantly for the interpretation of the rest of our results, it suggests that incumbent firms can be used to evaluate the reform's effects on the production sector as a whole.

Having confirmed that the reform had a positive effect on firms' share of temporary workers, we now study the reform's impact on firm-level outcomes. The last two panels of Figure 3 report two key margins that changed following the reform: labor costs and profit margins, defined as profits divided by value added. A key pattern emerges: as firms increase their share of temporary workers, (i) labor costs per worker decrease and (ii) profit margins increase.

Table 2 further elaborates on this graphical evidence. While column 3 shows that the impact of the reform on value added per worker is negative but imprecisely estimated, column 7 shows that the medium-run impact of the reform on *log* value added per worker is negative and marginally significant at −2%. This has implications for two important issues that are typically theoretically undetermined. First, it suggests 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). It is not surprising, however, 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 six months in Italy) and the observed duration of a temporary contract is often shorter than that (the median duration in our data is four months). Second, this result also implies that hold-up concerns (Grout, 1984) due to rigid employment protection granted to permanent contract employees are not predominant. If they were, we would expect to see an increase in productivity after the reform. 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 (Daveri and Parisi, 2015).

A first-order effect of the reform is on labor costs. Column 4 shows they were reduced by 830 euros per worker in the medium run, which is equal to 3% of the pre-reform value.[30] Even though value added per worker decreased, this effect on labor costs led to a significant increase in profits. Column 5 shows that profits per worker increased by 790 euros, around 8.6% of the pre-reform value. Similarly, column 6 shows the profit margin increased by 1 percentage point, for an overall increase of 8.3%. One may worry that changes in aggregate firm exit patterns might drive the results. Column 9, however, shows that the reform did not have a significant effect on firm exit, suggesting that such worries may not be warranted.[31]

To summarize, our evidence shows that firms appear to have complied with the reform primarily by substituting permanent workers with temporary ones, without increasing the overall number of workers. More novel relative to previous findings, we find that this substitution generates two effects: reduced labor cost and somewhat reduced productivity. Labor cost savings appear to be the predominant effect, as firm profits increased after the reform. These results thus contribute to the emerging literature trying to explain the decline in the labor share (e.g., Autor et al., 2020). As labor costs decreased more than value added, our findings suggest that EPL reforms and, more generally, the increased prevalence of more precarious forms of em-

[30] We obtain a similar magnitude when using the logarithm of labor costs per worker.

[31] Even if aggregate exit did not change, it is also possible for changes in the composition of firms to drive the results of, for example, increasing profits. This would happen if, after the reform, the most profitable firms were less likely to exit and the least profitable ones were more likely to exit. The heterogeneity analysis below, however, shows that low-profit firms are actually (if anything) less likely to exit after the reform.

ployment might have contributed to the worldwide decline in the labor share.

Heterogeneity

For which type of firms are the effects on the fraction of temporary employees, productivity, labor costs, and profits particularly pronounced? To answer this question, which can shed light on the mechanisms behind the results, we estimate equation (2) separately for different subgroups of firms. These subgroups are defined in terms of pre-reform characteristics that are expected to impact the likelihood of hiring temporary workers. We find that the reform had larger temporary employment, labor costs, and profits effects on firms with (i) high pre-reform labor costs per worker, (ii) relatively low turnover costs, (iii) a high wage premium, and (iv) a low expected survival rate. The negative effect on productivity instead seems to be primarily concentrated among low-quality firms (with low-profit margins, value added per worker, and labor costs per worker) that have a particularly low ex-ante probability of surviving.

Labor Costs per Worker. Firms with high labor costs per worker in the pre-reform period might be particularly inclined to respond to the reform by hiring more temporary workers as this can potentially drive down their labor costs. Table 3, Panel (a) confirms this prediction. We compute quartiles of the pre-reform labor costs per worker and re-estimate equation (2) for firms belonging to the fourth quartile, i.e., those with the highest labor costs. The share of temporary workers increases disproportionately more for these firms (the point estimate is around 20% larger than what we find in the baseline), which maps into a large reduction in labor costs per worker (of 4.9% versus 2.8% for the average firm). Despite this large change in labor costs, we find a null effect on log value added per worker.

The fact that productivity did not decrease suggests that replacing relatively expensive workers with cheaper (temporary) ones does not seem to explain the observed productivity decrease for the average firm. Given the relatively large reduction in labor costs, however, we do find a significant increase in the profit margin of around 3 percentage points, about 21% of the pre-reform value (versus 8% for the average firm).[32] Thus, firms with high labor costs are more likely to react to and benefit from the reform, but the replacement of relatively expensive workers does not seem to explain the productivity decline.

Turnover Costs. Another key margin that might affect a firm's reaction to the reform is its underlying turnover costs. If these costs are high, it becomes challenging for firms to substitute existing permanent workers with temporary ones. However, measuring the impact of the reform across firms with different underlying turnover costs is challenging since turnover costs

[32] This is consistent to what has been recently found by Acabbi and Alati (2021), who studies the same reform and shows that firms that had a high share of permanent employees experienced larger profit margin increases.

are fundamentally unobserved.[33] Thus, we proxy turnover costs in two complementary ways.

Following Garin and Silvério (2019), we first measure turnover costs using the average pre-reform worker tenure across CCNLs.[34] We divide this firm-size-weighted measure into quartiles and report here the effect for the highest quartile. Interestingly, Panel (b) shows that firms associated with CCNLs with a high turnover cost 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.[35] Thus, high turnover costs seem to be a barrier for firms to react to the reform by increasing their share of temporary workers.

To study the impact on employers with relatively low turnover/replacement costs, we focus instead on firms with a sizable part of their workforce being close to retirement age in the post-reform period.[36] These firms should have lower replacement costs (since there are no firing costs associated with retirement) and are more likely, once covered by the reform, to substitute the retiring worker with a new employee on a temporary contract. Panel (c) of Table 3 shows the results when focusing on firms that before the reform had 25% or more of employees aged 50 or older. The evidence suggests that these firms do indeed substitute older workers with temporary ones: the share of temporary employees jumps by around 45% post-reform (compared to 33% for the average firm). The increase in the share of temporary employees maps into lower labor costs per worker (of 5.5% versus 2.8% for the average firm) and higher profit margins. Even though the estimates are imprecise, we do not find that firms with an older workforce experienced a reduction in value added per worker.[37] Thus, while firms with older workers do respond more to the reform (and experience larger increases in profit margins), the replacement of experienced workers with younger ones does not seem to drive the productivity decline.

[33] There are several sources of turnover costs. It is particularly costly to terminate jobs that requires significant investment on firm-specific human capital (Jovanovic, 1979a). Similarly, ex-post realized matches in environments characterized by unknown ex-ante productivity and search frictions are also costly to destroy (Jovanovic, 1979b). Finally, institutional factors, such as unions, might also affect employers' turnover costs.

[34] This measure may also proxy other sector characteristics (e.g., unionization). The important element for our purpose, however, is that it captures how difficult it is for a firm to replace its workers.

[35] Note that this result provides further validation to our research design and, in particular, addresses the potential issue that the year of the reform's implementation coincides with the year of renewal of CCNLs. If our baseline effects captured the effects of these renewals instead of the reform, we would expect to continue to find effects on labor costs and profit margins even among those firms that cannot respond to the reform by adjusting their share of temporary workers due to high turnover costs. See more details in Appendix C.3.

[36] We find qualitatively similar results when using firms associated with CCNLs in the bottom tenure quartile.

[37] This relates to the recent literature on worker- and firm-level consequences of changes to the retirement age (Bovini and Paradisi, 2019; Boeri et al., 2021; Carta et al., 2021). In particular, our findings are qualitatively consistent with Carta et al. (2021), who finds that the 2011 Italian reform that increased the retirement age (and thus reduced the replacement of older employees with younger ones) had no significant effect on labor productivity.

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 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 we further develop in Section 9. Consequently, firms paying relatively high premia to their workforce might have a strong incentive to hire using temporary contracts.

We estimate firms' wage premia using the wage decomposition model of Abowd et al. (1999) (henceforth AKM) for the years 1990–2001. We divide the AKM firm effects into quartiles and estimate equation (2) only for firms with an AKM effect in the top quartile. Panel (d) shows that these firms increased their share of temporary workers relatively more (by 43% compared to 33% in the baseline). 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 these firms are associated with a relatively large reduction in labor costs (by 3.5% versus 2.8% for the average firm). Although estimates are imprecise, we find that the overall effect on the profit margin is positive. While high-premia firms do seem to react more to the reform, the effect on these firms' productivity is, once again, null, suggesting that reductions in wage premiums (likely because of differential rent-sharing) are not the main driver of the productivity decline.

Low Expected Survival Probability. The reform might have given the opportunity to firms that in the pre-reform counterfactual were expected to disappear to actually survive in the post-reform period, probably by taking advantage of the lower labor costs associated with temporary workers. To test for this prediction, we first estimate a pre-reform survival model by regressing an indicator for firm death on various firm characteristics.[38] We re-estimate equation (2) for firms belonging to the top quartile of the associated predicted values, i.e., those with a relatively high probability of dying. Based on the pre-reform means reported in Panel (e) of Table 3, we find that these firms had relatively low productivity, low labor costs, and close to zero profit margins. Though somewhat imprecise, we find that the cumulative exit rate of these lower quality firms, once covered by the reform, is around 5 percentage points smaller relative to similar firms still under the pre-reform regime (Online Appendix Figure G5).

Going back to Table 3, Panel (e) reports that these firms increased their share of temporary workers after the reform, with a particularly large decrease in labor costs per worker (of 6.4% versus 2.8% for the average firm). Interestingly, this is the single subgroup (among our analysis)

[38]These are profit margin, log value added per worker, log labor costs, average earnings, average age, female worker share, foreign worker share, and province and CCNL fixed effects.

with a relatively large, negative, and statistical significant effect on value added per worker (of 6.8%). Thus, low-quality firms (with low profit margins, value added per worker, and labor costs per worker as well as low ex-ante survival probability) seem to be the main drivers of the productivity decline. In line with the theoretical prediction from Section 3, the reform appears to have enabled the existence of low-quality jobs that would not have existed otherwise.

8.2 Workers

We now investigate the impact of the reform on workers. Section 8.2.1 analyzes the reform's impact on incumbent temporary workers, i.e., individuals already present in the labor market in the pre-reform period with a temporary contract. Section 8.2.2 then computes the short- and long-run consequences of having entered the labor market under the new regime on temporary employment contracts. The general pattern of the 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 by the reform.

8.2.1 Incumbents

How did the reform impact individuals already present in the labor market and employed with a temporary contract in the pre-reform period? Figure 4 reports the event study coefficients after fitting equation (3) on incumbent temporary workers. Panel (a) focuses on employment effects, so the outcome is an indicator variable equal to one if the worker is employed in a given year. The short-run effect on employment seems to be basically zero, while the medium-run effect is negative and amounts to around 3 percentage points. Our results therefore suggest that the reform was not only ineffective at increasing aggregate employment but also unsuccessful at increasing the likelihood of employment for workers already present in the labor market with a temporary contract and thus particularly affected by the new legislation.

Panel (b) reports the reform's impact on the annual total labor market earnings of incumbent temporary and permanent workers, rescaled by the corresponding pre-reform average earnings.[39] Differences in earnings between treated and control workers are relatively well balanced and statistically insignificant before the reform. Earnings differences among temporary workers start to emerge once the reform is passed: one year following its implementation, individuals covered by the reform earn around 2.4% less than individuals whose pre-reform CCNL is yet to implement the new legislation on temporary contracts. This earnings gap continues to grow post-reform to around 6%. The results are very similar when not conditioning on employment, as shown in Appendix Figure G6. Thus, it does not appear that the nega-

[39] Annual earnings is defined as the sum of labor earnings received by an individual. Therefore, if a worker had two or more jobs in a given year, we sum the earnings received from all the jobs. We assign zero earnings to those individuals who are non-employed in a given year and eventually reentered the sample in any subsequent year.

tive earnings effects depicted in Panel (b) are primarily driven by the reform's effect on the extensive margin displayed in Panel (a); see also Table 4, column 2 and column 3.

While the reform has a clear, negative effect on incumbent temporary workers, Panel (b) of Figure 4 also shows that the same reform leads to economically and statistically insignificant medium-run effects for incumbent permanent workers, further providing validation of our research design.[40] Permanent workers, while clearly affected by the CCNL renewal, are arguably less affected by the reform given their associated level of employment protection. The fact that these workers do not experience significant changes in earnings after the renewal of the associated CCNL therefore suggests our research is indeed capturing the effects of the reform as opposed to more general effects of the CCNL renewal. Based on Panel (b), we thus conclude that the new legislation on temporary contracts has raised considerably the duality between temporary and permanent contracts (and therefore between young and older workers) in key labor market outcomes, such as labor earnings.[41]

But what explains the negative earnings effects for incumbent temporary workers? One first potential explanation is that workers under the new reform may work fewer days within the year. To account for that, Figure G7, Panel (a) shows effects on the log daily wage associated with the primary job of a given year. For this particular outcome, we find evidence of an upward trend in the pre-event years with a potential discontinuity once the reform is passed. To account for these pre-trends, we follow an approach similar to Dustmann et al. (2021) and Gruber et al. (2021) and estimate a linear trend using pre-reform periods only. We then use the deviations of the log daily wage of incumbent temporary workers from this trend extended to post-reform years as the outcome of interest. The results are displayed in Panel (c) of Figure 4. Once we account for the linear pre-trend, the pre-reform coefficients appear essentially flat and statistically insignificant. Once the reform is passed, we observe a negative effect of the reform on log daily wages. The effect at enactment is around −1% and grows to around −6% in the medium run. How much of the reduction in earnings (column 3 in Table 4) does this explain? Daily wages account for about one-third of the short-term effect (−3%) and basically all of the medium-term effect (−6%) on earnings. Based on this evidence, we conclude that most of the aggregate negative earnings effects appear to be driven by changes in daily wages rather than by changes in days worked.[42]

[40] This is in line with what is displayed in the bottom panel of Appendix Table D1: the reform does not appear to have any economically meaningful impact on the transition rates of permanent workers.

[41] This fact holds not only when comparing incumbent workers but also when considering the universe of full-time temporary versus permanent jobs. See Online Appendix Figure G4.

[42] Online Appendix Figure G8 displays some robustness analysis where we account for the differential pre-trends in alternative ways (e.g., estimating province- or worker-specific linear trends based on observed characteristics at the time of entry in the labor market). Results are shown in Panel (a) of Online Appendix Table G.5. This table also analyzes the robustness of our results with respect to alternative measures of log daily wages by considering, instead of the daily wage from the primary occupation, the daily wage obtained by dividing the total

As a possible explanation for why wages are negatively affected by the reform, we show next that incumbent temporary workers, once covered by the reform, are systematically less likely to (i) work under a permanent contract with their employers and (ii) relocate into a high-value-added employer. First, Panel (d) of Figure 4 shows that, after the reform, there is a 9 percentage point decline in the likelihood of firms converting temporary jobs into permanent ones. This effect corresponds to roughly 32% of the unconditional probability of a within-employer temporary-to-permanent transition in the pre-reform period. Table 4 (columns 1, 5, and 6) further shows that this decrease in the likelihood to observe within-employer temporary-to-permanent transitions entirely explains the overall increase in the job destruction rate due to the reform, thus confirming a key theoretical prediction described in Section 3.[43]

Second, column 7 shows that the reform caused incumbent temporary workers to systematically relocate to lower-quality firms, in which the mean value added per worker is 5,000 euros lower (or 12% of the average value observed in the pre-reform). This result is in line with the recent findings of Dustmann et al. (2021). They show that an increase in the minimum wage in Germany—i.e., a reform that raised instead of decreased labor costs as the one analyzed in our paper—makes workers relocate to higher-quality establishments. In line with their results, we find that this partial EPL reform made workers relocate into lower-quality employers.

We conclude this section by displaying the effects of the reform on young workers, defined as individuals who were at most 25 years old in 2001. Our focus on young individuals is motivated by the Italian legislators who argued that this demographic group should have benefited the most from the new regime on temporary contracts (Biagi and Sacconi, 2001). The results in Panel (b) of Table 4 contradict this prediction. Young incumbent temporary workers experience earnings losses of around 12% of the average earnings observed in the pre-reform, almost twice as large as those for incumbent temporary workers in general. Once again, the negative earnings losses hold both unconditionally and when conditioning on employment.

In line with the mechanisms above, we find that these larger earnings losses are driven by a significant reduction—of around 12 percentage points—of the within-employer conversion of temporary contracts into permanent positions. This corresponds to around 59% of the pre-reform conversion rate, which is twice as large as the percent effect for incumbent temporary workers in general. Interestingly, for this group of workers, the negative percent effects of conditional earnings are slightly larger than the ones on log daily wages, thus suggesting that the negative effect on earnings is driven by responses along both extensive and intensive margins.

earnings of an individual in a given year (potentially summing the earnings obtained from multiple jobs in a year) by the corresponding measure of total number of days worked, or by considering only the log daily wage from full-time jobs. Overall, across these multiple checks, we find results similar to the ones displayed in Table 4.

[43] We obtain an identical result when considering the universe of temporary jobs observed in INPS-INVIND. See, in particular, the analysis on job flows presented in Appendix D.1.

To summarize, we believe the overall evidence presented in Table 4 can be interpreted through the lens of a model with heterogeneous job quality, as the one described in Appendix E. A significant fraction of incumbent temporary workers appears to be associated with key marginal temporary positions, i.e., positions that in the pre-reform period were expected to be converted to permanent positions by employers but in the post-reform period were instead destroyed. 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 job destruction, with treated incumbent temporary workers relocating to lower-quality employers relative to control workers. Taken together, these events map into significant earnings losses, with effects that are particularly pronounced among younger workers.

A caveat, however, should follow. By focusing on incumbent workers, we might be artificially selecting on a group of workers who are more likely to face the negative consequences of the reform. The reform may also permit the opening of new jobs that were previously unavailable, which might be particularly beneficial for new entrants of the labor market. We evaluate whether that is the case in the next section.

8.2.2 New Entrants

We now present the results from the research design described in Section 6.3.2. The key identification assumption underlying this section is that the coefficients $\{\theta\}_{s=1}^{7}$ from equation (4) solely identify changes in outcomes due to the reform as opposed to differences in the labor supply of workers entering the labor market in different years. This assumption would not hold if, for example, individuals selectively enter the labor market based on the reform status, since this 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 CCNLs before they adopt the reform. Online Appendix Figure D3 shows, however, that the number of new entrants is flat around the reform, reinforcing our key identifying assumption. This is consistent with the evidence presented in Panel (d) of Figure 2 that shows no effects of the reform on the employment of young workers. We also find that pre-determined characteristics of workers are relatively well balanced between the pre- and post-reform cohorts (see Table G.6). Thus, we conclude that pre- and post-reform cohorts appear relatively similar in terms of observable characteristics, and we find no significant evidence that the reform altered the entry patterns of young workers.

Having confirmed the credibility of the required assumptions, Table 5 displays the main results regarding the dynamic effects of entering the labor market under the new regime on temporary employment contracts. 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 losses shrink over the experience profile but remain statistically significant until seven years after entry.

The reduction in earnings experienced 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 the reform decreased the probability of remaining with the same employer, especially during the early stages of the career. A sizable part of the post-entry-reduced probabilities of permanent-contract employment and of remaining with the same employer seems to be explained by the reduced probability of being converted into a permanent worker by the current employer (column 4).

Column 5 shows that differences in the extensive margin (employment versus non-employment) across pre- and post-reform cohorts are also statistically and economically small. This suggests the reform did not consistently help post-reform cohorts 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—in line with the effect on incumbent temporary workers.

To summarize, we find that individuals who started their careers under the reform suffered earnings losses both in the short and in the medium run, relative 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 careers). In addition, the post-reform cohorts were also 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.[44]

A limitation of our design is the fact that we can 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 D.2 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 5. We find that the share of individuals who can find a first job in a given year in the reform counterfactual but not in the non-reform counterfactual needs to be 20.5% to compensate for the net present value of earnings losses reported in column 1 of Table 5. This increase appears unrealistic, especially in light of the auxiliary evidence presented in Figure D3 showing that the logarithm of the

[44]Appendix Table G.7 shows that similar results when augmenting the set of controls, conditioning only on full-time workers and controlling for province-by-year fixed effects interacted with cohort of entry. It also shows that mobility across sectors and places does not appear to result in a rapid convergence for post-reform cohorts, as we still obtain negative earnings effects when we use *current* LLM and province-year fixed effects.

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 sizable earnings losses for young new entrants in the Italian labor market.

9 A Bargaining Power Explanation

The previous section showed that firms are among the main winners of the reform. Even though the reform leads to a reduction in value added per worker, it also causes a sizable reduction in labor costs, with a positive net effect on firms' profits. Incumbent temporary workers instead were among the main losers due to their large reductions in earnings. But how can firms reduce their labor costs by hiring temporary rather than permanent workers? Similarly, what explains the earnings losses of incumbent temporary worker earnings? To answer these questions, we investigate the role of employer-specific pay policies and, in particular, the differences in the amount of rents that firms share with permanent versus temporary workers.

Section 9.1 begins by leveraging from within-person, within-employer transitions from temporary to permanent contracts to show that the same worker's wage is at least 6% higher if hired by her current employer with a permanent rather than a temporary contract. Section 9.2 shows that most of the wage effect associated with the temporary-to-permanent conversion can be explained by a simple model of differential rent-sharing and that relative bargaining power differences of temporary versus permanent workers can account for 52% of the reform's reduction in firm labor costs and for 77% of incumbent temporary workers' earnings losses.

9.1 Within-Firm, Temporary to Permanent Transitions

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.[45] While the law explicitly forbids firms from discriminating between temporary and permanent contracts when establishing minimum wages in the first pillar, firms are legally allowed to split rents/premia/bonuses differently between permanent and temporary employees (Picchio, 2006).[46]

[45]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).

[46]Montanari (2002) describes a law case where temporary workers filed (and lost) a lawsuit against their employer that discriminated against them by allocating end-of-year bonuses exclusively to permanent workers. Relatedly, the 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).

This discrimination seems apparent in the raw wage data. The rate at which per-worker surplus maps into higher wages is vastly different across temporary versus permanent workers. Online Appendix Figure G9 shows a bin scatter plot of average log daily wages for temporary versus permanent workers of firms within different percentile bins of log value added per worker. The difference in wages is basically null among low valued-added firms but grows with firms' productivity, consistent with a model of differential rent-sharing (Card et al., 2016). This evidence, however, is only descriptive. To account for sorting of higher-skilled individuals into more productive firms, we focus on within-employer, within-person transitions from a temporary to a permanent contract (henceforth abbreviated as $T \to P$). Let t_i^* denote the year in which individual i experiences for the first time in her career such an event. The wage effect associated with $T \to P$ transitions is estimated from the following event study:

$$w_{it} = \alpha_i + W_{it}^\top \phi + \sum_{k=a}^{b} \theta_k D_{it}^k + \varepsilon_{it}, \tag{5}$$

where w_{it} is the log daily wage paid by the employer that 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 one-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^*.

To reduce the concern that only the best workers are selected into a permanent position, we estimate equation (5) on the sample of individuals who at some point in their career experience a $T \to P$ transition and were always employed with a temporary contract before this transition. Equation (5) thus compares similar workers on a temporary contract who eventually obtain a conversion to a permanent position. It follows that the key source of identification for the coefficients of interest, $\{\theta_k\}$, is that the $T \to P$ conversion is obtained in different years for different workers. These coefficients are shown in Panel (a) of Figure 5. Pre-trends leading to the transition $T \to P$ are relatively flat and economically insignificant. This is important because it provides evidence in favor of the parallel trend assumption underlying equation (5). In particular, workers who are yet to experience a $T \to P$ transition in year t seem to provide a valid counterfactual for workers converted to a permanent contract in year t.

The main result from this analysis is that when workers obtain a permanent job, their log daily wages suddenly increase by around 6%.[47] Interestingly, these continue to grow (to around 8%) in the years following the transition to a permanent contract, suggesting that returns to experience are larger while workers are on a permanent contract. The next section shows that a differential rent-sharing model, together with the estimated bargaining power differences

[47]We do not observe hours worked so we cannot assess if the estimated return is due to changes in hours worked. However, results are almost unchanged if we focus on full-time workers (Online Appendix Table G.8).

across contracts, can explain a significant amount of both the instantaneous return associated with a within-firm conversion into a permanent contract and the reform's effects on firm labor costs and temporary workers' earnings.

9.2 Differences in Bargaining Power and The Reform's Effects

To understand how firms share surpluses with workers while on a temporary versus a permanent contract, we fit a dynamic rent-sharing model,

$$w_{it} = \alpha_i + W_{it}^{\top}\phi + \sum_{k=a}^{b} \gamma_k (S_{J(i,t),t}^{k} \times D_{it}^{k}) + \epsilon_{it}, \tag{6}$$

where $S_{j,t}$ is a measure of surplus per worker available at employer j at time t and $J(i,t)$ is a function that returns the identity of the employer hiring individual i and time t. We follow the rent-sharing literature and use log value added per worker as a proxy for $S_{J(i,t),t}$ (e.g., Card et al., 2016). Our primary interest is assessing how rent-sharing changes when the same worker is moved by her employer into a permanent contract. This is captured by the ratio $\frac{\gamma_{-1}}{\gamma_0}$, which we interpret as a measure of the relative bargaining power of temporary workers.[48]

Panel (b) of Figure 5 plots the coefficients γ_k in equation (6). While the worker is under a temporary contract, these coefficients exhibit a flat profile centered at 0.034. Once the worker is converted into a permanent contract, γ jumps to approximately 0.051, consistent with prior estimates (Guiso et al., 2005; Card et al., 2014). This suggests a relative bargaining power equal to 0.67 (0.034/0.051). Therefore, wages of temporary workers are only 67% as responsive to surpluses as those of permanent workers.[49] A possible explanation behind the pattern displayed in Figure 5 is that firms are experiencing a positive productivity shock at the time of the $T \longrightarrow P$ transition—which could imply that the contract itself is not the only driver of the wage change. To understand the importance of this channel, we apply a Oaxaca Decomposition to the average rent-sharing effect observed at the moment of conversion, i.e.,

$$\begin{aligned} \mathrm{E}[\gamma_0 S_{J(i,t),t} | t = t_i^*] - \mathrm{E}[\gamma_{-1} S_{J(i,t),t} | t = t_i^* - 1] = {} & \underbrace{(\gamma_1 - \gamma_0)\,\mathrm{E}[S_{J(i,t),t} | t = t_i^*]}_{\text{Bargaining Component}} + \\ & \underbrace{\gamma_0 \{\mathrm{E}[S_{J(i,t),t} | t = t_i^*] - \mathrm{E}[S_{J(i,t),t} | t = t_i^* - 1]\}}_{\text{Surplus component}}, \end{aligned}$$

where the surplus component captures how much of the change in value added per worker explains the reported wage change. The bargaining component instead captures the importance of relative differences in bargaining power. As shown in Table 6, changes in value added

[48] Note that γ_{-1} is the rent-sharing elasticity estimated among temporary workers in their last employment spell before obtaining a permanent job, while γ_0 is the elasticity for the same group of workers in their first year with a permanent job. While these elasticities do not directly translate into rent-sharing coefficients (e.g., Manning, 2011), their ratio can be shown to be equal to the ratio of rent-sharing coefficients in standard models.

[49] Kline et al. (2019) find differential pass-through of firm-surplus to wages between existing workers and new hires. Drenik et al. (2021) find a relative bargaining power of 0.50 for workers hired by a user firm via a temporary work agency relative to workers directly hired by the user firm.

seem to explain little of the observed wage effect. Conversely, differences in bargaining power account for 80% of the raw wage increase associated with a $T \rightarrow P$ transition.[50]

We conclude by quantifying the role of bargaining power differences between temporary and permanent workers in explaining the reduced-form effects of the reform displayed in Section 8. We assume throughout that workers' earnings are equal to $y_c = b + \alpha_c \times S$, where c denotes whether the contract is temporary or permanent and highlights that workers have different bargaining powers depending on their contracts. The ratio of temporary to permanent bargaining powers, $\frac{\alpha_T}{\alpha_P}$, is equal to 0.67, based on the evidence of Figure 5(a). For simplicity, it is assumed that the reform does not affect this share nor the surplus level. Instead, the reform is assumed to only affect the share of temporary contracts at each firm as well as the transition probability of incumbent workers (e.g., to a permanent contract with the same employer).[51]

In Table 2 of Section 8.1, we reported that firms' labor costs per worker were reduced by 2.81% after the reform (in the medium run), while the share of temporary workers at the firms increased by 4.91%. Using $\frac{\alpha_T}{\alpha_P} = 0.67$, a back-of-the-envelope calculation suggests that our estimate of the relative bargaining power ratio between temporary and permanent workers would generate a labor cost reduction of 1.44%, explaining approximately half (52%) of the observed reform-induced labor cost reduction.

Proceeding in a similar way for the workers' analysis, in Table 3 of Section 8.2, we reported that incumbents' temporary workers earnings decreased by around 7.0% after the reform and the probability of within-firm temporary-to-permanent transitions also decreased by 8.7 percentage points. Together with $\frac{\alpha_T}{\alpha_P} = 0.67$, these reduced-form effects generate earnings reductions of about 5.4%. Thus, differences in the relative bargaining power of temporary workers explain about three-quarters (77%) of the reform-induced earnings reduction of incumbent temporary workers—with the remaining part being driven by negative reallocation effects.

All in all, these calculations therefore suggest that differences in the bargaining power between temporary and permanent workers can explain a large portion of both the within-firm reductions in labor costs as well as the earnings losses induced by the reform.

[50] Appendix D.3 discusses other possible interpretations (e.g. differences in the job tasks between temporary vs. permanent contracts) that could account for a wage return following a $T \rightarrow P$ transition. While the data and research design does not permit us to completely rule out other possible theories, Appendix D.3 suggests that temporary workers are often simply paid according the CCNL's minima while being excluded from wage top-ups offered via firm-level agreements by more productive employers. See also Montanari (2002).

[51] See Appendix D.4 for details. The calculations below also require information on labor shares, which we observe in the data, and on the level of bargaining power of permanent workers, which we assume to be 0.5 as is standard in the literature (e.g., Pissarides, 2009; Gertler and Trigari, 2009).

10 Conclusions

As temporary work and alternative employment arrangements are becoming increasingly popular (Weil, 2014), a policy debate is currently occurring both in the US (e.g., Irwin, 2017) and in Europe (e.g., Alderman, 2017) regarding the benefits of allowing, or even promoting, such type of work. These employment arrangements are typically promoted with the idea that they help create job opportunities, particularly among young workers (Biagi and Sacconi, 2001). Our results cast doubt on the validity of this idea.

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—including, in particular, employment among young workers. As a result, new temporary jobs mostly substituted existing permanent ones. Although employment did not change, the reform had both winners and losers. Firms were among the main winners. While the reform led to reductions in productivity, labor costs were reduced even more, leading to increases in both profits and profit margins. The negative effect on productivity was concentrated among low-productivity firms that had a particularly low ex-ante probability of surviving without the reform.

Young incumbent temporary workers and new labor market entrants were the main losers since they sustained the most significant earnings losses once the reform was implemented, of between 5% and 12%. What explains these losses? There are two main factors. First, these workers were significantly less likely to obtain permanent, stable jobs after the reform. Second, workers with temporary contracts are systematically paid less than if they had a permanent contract, most of which seems to be explained by a simple contract-specific rent-sharing model. These two elements explain about three-quarters of the earnings decrease.

All in all, our findings suggests that several changes may be needed for policies to promote employment through the creation of temporary or alternative employment. These policies may need guardrails that limit the capacity of new alternative jobs to replace old, stable jobs. Given that permanent contracts typically have probationary periods, one needs to first understand what problem alternative contracts are solving. If the problem is actually that some tasks only last a relatively short amount of time (e.g., replacing a worker on maternity leave), other constraints may be needed to prevent firms from making permanent tasks into rotating temporary contracts. One option may be to equalize the labor costs of alternative and permanent workers. California's recent independent contractor law (AB5) aims to do this by forcing firms to pay full benefits to workers performing tasks in the hiring company's main area of business. Future research may be needed to see if such restraints are successful without reverting to the original problem of firms not being able to hire workers for actual temporary tasks.

Data Availability Statement

The data underlying this article cannot be shared publicly, as the paper uses confidential administrative data. However, instructions on how to obtain these data along all replications scripts, including detailed explanations of data construction, are available at the following DOI: 10.5281/zenodo.7311906 (https://doi.org/10.5281/zenodo.7311906)

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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 based on 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 who at any point in their career had a temporary or permanent contract. The 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 a temporary or a permanent contract in a given year as well as individuals associated with an apprenticeship or seasonal contract). Standard deviations are in parentheses. Source: INPS-INVIND, 1998–2013.

Table 2: 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 (2). The former refers to the effect of the reform at enactment, while the latter refers to the effect four-plus years after the reform was implemented. The estimation sample— labeled 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 before the reform was implemented. We use the dominant CCNL in the pre-reform period to define our event study indicators. See Section 6.2 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 observation corresponds to the last year in which we observe the firm in the data. All displayed results control for firm fixed effects and province-by-year fixed effects. The results are weighted using inverse propensity score weights designed to match the sector composition and firm size pre-reform distribution observed in INPS-INVIND. Standard errors are displayed in parentheses and are clustered at the LLM level.

Table 3: Heterogeneity in the Firm-Level Analysis Based on Pre-Determined Characteristics

	Share of Temp Contract Employees	Log Value Added per Worker	Log Labor Costs per Worker	Profit Margin
Baseline Effects on Full Sample				
Medium-Run Effect of the Reform	0.0491***	-0.0203**	-0.0281***	0.0098**
	(0.0067)	(0.0086)	(0.0065)	(0.0042)
Pre-reform Mean	.15	3.78	3.29	.12
Observations	1,199,410	1,199,410	1,199,410	1,199,410
Panel (a): High Labor Costs				
Medium-Run Effect of the Reform	0.0589***	0.00136	-0.0494***	0.0300***
	(0.0102)	(0.0159)	(0.0105)	(0.00806)
Pre-reform Mean	.13	4.31	3.81	.14
Observations	300,379	300,379	300,379	300,379
Panel (b): High Turnover Costs				
Medium Run Effect of the Reform	0.0145	0.0151	0.00208	0.00579
	(0.0141)	(0.0182)	(0.00916)	(0.00972)
Pre-reform Mean	.14	3.85	3.37	.12
Observations	93,877	93,877	93,877	93,877
Panel (c): 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 (d): High-AKM Firm Effects				
Medium-Run Effect of the Reform	0.0606***	-0.00479	-0.0354***	0.0103
	(0.0123)	(0.0164)	(0.0113)	(0.00749)
Pre-reform Mean	.14	4.04	3.53	.13
Observations	267,094	267,094	267,094	267,094
Panel (e): Low Expected Survival Rate				
Medium-Run Effect of the Reform	0.0530***	-0.0684***	-0.0642***	0.00890
	(0.0160)	(0.0236)	(0.0192)	(0.0124)
Pre-reform Mean	.14	3.54	3.07	.02
Observations	206,012	206,012	206,012	206,012

Note: This table reports the coefficient θ_3—defined as the ``Medium-Run Effect of the Reform"—from equation (2) that refers to the effect four-plus years after the reform's implementation, estimated separately for each group described in Panels (a)–(e). 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 before the reform was implemented. We use the dominant CCNL in the pre-reform period to define our event study indicators. See Section 6.2 for details. We start by printing the main effects displayed in Table . Then, in Panel (a), we estimate our firm-level event study specification only among firms belonging to the highest quartile of the log labor costs per worker distribution observed before the reform. In Panel (b), 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 (c), we estimate the event study conditional on firms that, in the pre-reform period, have 25% or more of their employees aged 50 years or older. In Panel (d), we condition on firms where their estimated AKM effect—computed using only data from 1990 to 2001—belongs to the fourth quartile of the corresponding AKM firm effects distribution. In Panel (e), we first estimate a model that predicts the probability of a firm dying in 2001 based on various firm characteristics (e.g., log value added per worker, log labor per cost per worker, profit margin, average earnings) while controlling for CCNL and province fixed effects. We then take quartiles of this measure and estimate our firm-level event study specification just within those firms belonging to the fourth quartile of these predicted values, that is, firms with a lower expected probability to survive. All underlying regressions control for firm fixed effects and province-by-year fixed effects. The results are weighted using inverse propensity score weights designed to match the sector composition and firm size pre-reform distribution observed in INPS-INVIND. Standard errors are displayed in parentheses and are clustered at the LLM level.

Table 4: The Effects of the Reform on Incumbent Temporary Workers

	Employed	Earnings	Conditional on Employment: Earnings	Log Daily Wages	Same Employer	Same Employer & Permanent Contract	Value Added per Worker
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel (a): Incumbent Temporary Workers							
Short Run Effect of the Reform	-0.00191	-266.2***	-348.0***	-0.0121***	-0.0311***	-0.0397***	-3989.8***
	(0.00334)	(83.39)	(86.99)	(0.00311)	(0.00754)	(0.00610)	(879.9)
Medium Run Effect of the Reform	-0.0344**	-778.7**	-633.6**	-0.0648***	-0.0312**	-0.0873***	-5173.6***
	(0.0167)	(327.4)	(251.4)	(0.00867)	(0.0144)	(0.0164)	(1822.2)
Pre-reform Mean Outcome	.97	11,053.6	11,433.53	4.14	.6	.27	41,817.13
# of Person-Year Observations	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	-0.00699*	-427.3***	-462.2***	-0.00845**	-0.0324***	-0.0397***	-3760.1***
	(0.00376)	(109.2)	(107.5)	(0.00334)	(0.00741)	(0.00630)	(984.4)
Medium Run Effect of the Reform	-0.0458***	-1205.5***	-923.4***	-0.0476***	-0.0472***	-0.117***	-5970.2***
	(0.0132)	(384.1)	(320.5)	(0.00910)	(0.0147)	(0.0176)	(2260.1)
Pre-reform Mean Outcome	.97	9,893.41	10,224.84	4.11	.59	.20	39,287.97
# of Person-Year Observations	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 (3). The former refers to the reform's effect at enactment, while the latter refers to the effect four-plus years after the reform's implementation. Panel (a) estimates equation (3) on the sample of incumbent temporary worker defined as individuals who were always employed with a temporary contract over the pre-reform period 1998--2001 and that were 40 or less years of age in 2001. Outcomes from columns 1 and 2 do not condition on employment, and we assign zero earnings to non-employment spells. The results from columns 3–7 condition on employment. Value added per worker is the average value added of a given firm averaged over the sample period; this measure is only available for firms belonging to the CERVED sample. See text for details. Log daily wage is the log daily wage associated with the primary job of an individual in a given year. To account for a linear unobserved trend in this particular outcome, we estimate a linear trend using pre-reform data only and residualize log daily wages with respect to this fitted trend, see Figure G.7 and Table G.5 for details and robustness. Same employer is a dummy equal to one if the worker did not switch employer in that year. Same employer & permanent contract is a dummy equal to one 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 (3) using the sample of Panel (a) but further restricts it to individuals aged 25 or younger in 2001. We report the average in the pre-reform period for a given panel and outcome for all columns. The pre-reform mean outcome reported in column 6 corresponds to the unconditional share of individuals who, before the reform, transitioned from a temporary to a permanent job with the same employer in our overall sample (and restricted to workers 25 years of age or younger for Panel (b)). Standard errors are displayed in parentheses and are clustered at the LLM level.

Table 5: 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 (4) 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 were 30 years old or less upon entry. See Section 6.3.2 for details. Regression estimates are based on cell data at the level of entry year 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 the current year minus the 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 the year since 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. 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 who 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 one for all micro-observations in the micro-data in the in the first year of entry). Standard errors are displayed in parentheses and are clustered at the LLM level.

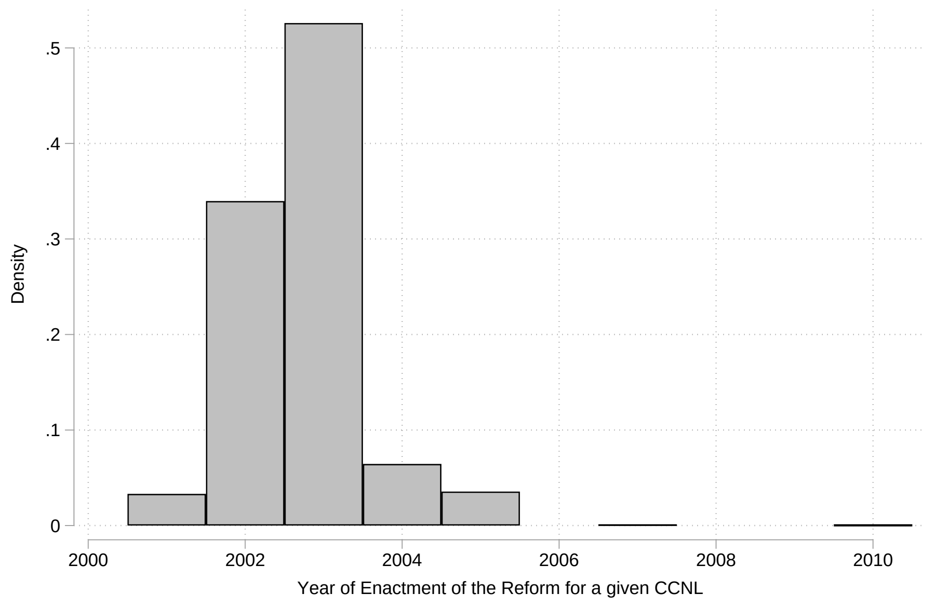
Table 6: Within-Employer, Within-Person Evidence of Differential Rent Sharing

	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 a Oaxaca decomposition of the wage change experienced by workers when moved from temporary to permanent employment by their employer. Column 1 lists outcomes corresponding to the last year under a temporary contract for a worker, while column 2 refers to the first year under a permanent contract. Column 3 presents differences between column 2 and column 1. Row 1 reports average log daily wage, row 2 reports the average log value added per worker, and row 3 reports the rent-sharing coefficients from equation (6), as plotted in Panel (b) of Figure 5. Rows 4–5 report a Oaxaca decomposition where the covariate-adjusted wage change at the time of the event is decomposed in two terms. The first term, the Surplus Component , captures how much of the change in average surplus per worker (weighted by the rent-sharing elasticity in the last year before the conversion into a permanent contract) explains the reported wage change. The second term, the Bargaining Component , captures how much the changes in the rent-sharing elasticities (weighted by average surplus observed in the first year under a permanent contract) explains the reported wage change. The terms in square brackets in column 3 represent the percentage of the raw wage gap reported in row 1, that is explained by either the surplus or the bargaining component.
Estimates are based on workers who start with a temporary contract and eventually transition to a permanent contract with the same employer and we have data on the value added per worker of their employer from CERVED. The 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 rounded parentheses.

Figure 1: The Reform

(a) Year of Renewal

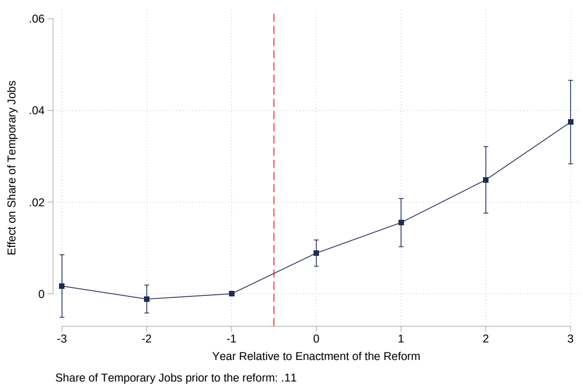


(b) Staggered Implementation

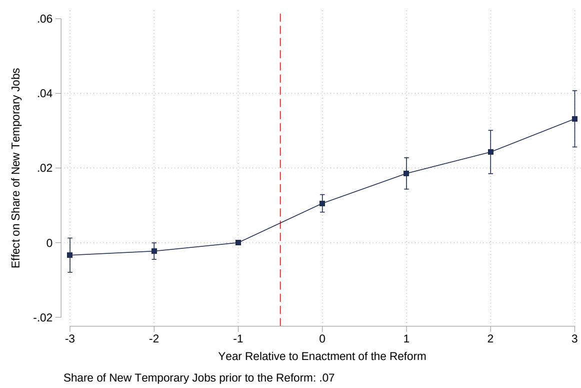
Note: Panel (a) shows the person-year weighted distribution of enactment years of the reform across CCNLs. See Section 5 for more details. Panel (b) shows the evolution of the share of temporary jobs in two CCNLs. The yellow line plots this share for CCNLs in the food sector, which implemented the reform in 2002. The light blue line corresponds to the metal handicrafts sector, which implemented the reform in 2005.

Figure 2: Effects on Share, Creation and Destruction of Temporary Jobs and Employment

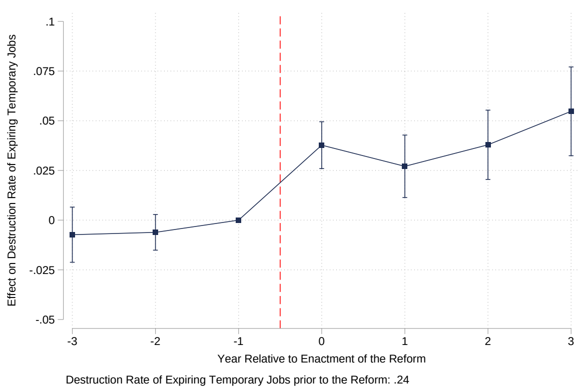
(a) Share of Temporary Jobs



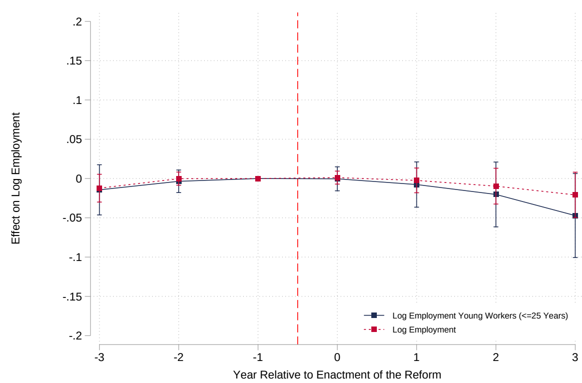
(b) Share of New Temporary Jobs



(c) Temporary Jobs Destruction Rate



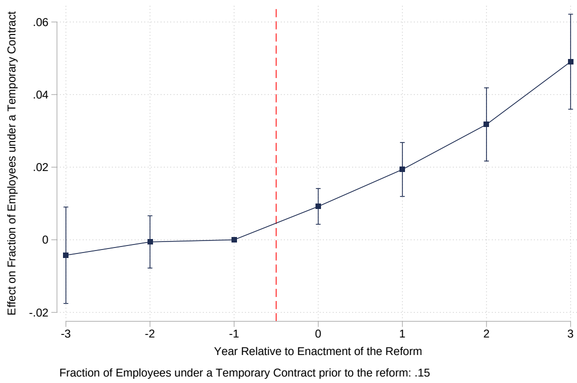
(d) Log Employment



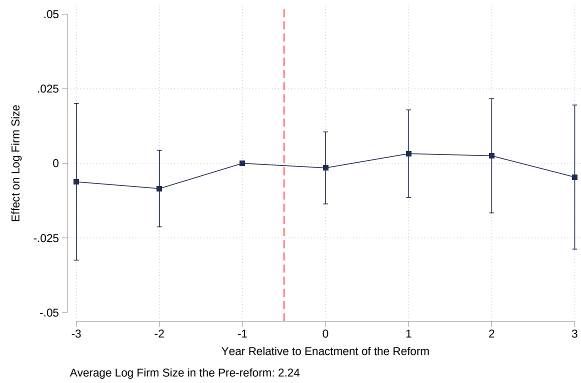
Note: This figure reports estimates from equation (1). In Panel (a), the outcome variable is the share of temporary jobs. In Panel (b), the outcome variable is the share of new temporary jobs defined as total number of new temporary jobs divided by total number of observed jobs. In Panel (c), the outcome is represented by the destruction rate of expiring temporary jobs. Panel (d) reports the effects on log employment in a given LLM × year cell as well as log employment of young workers (workers aged 25 years or less). Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 3: Impact of the Reform on Firms

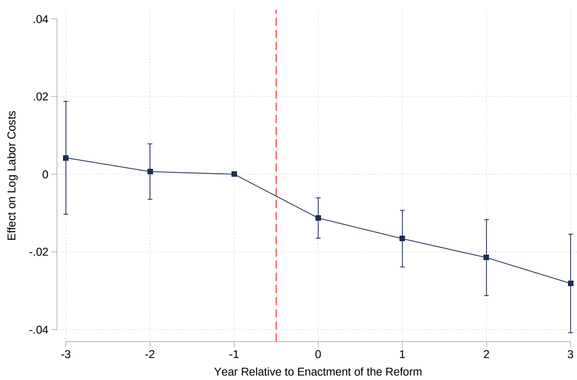
(a) Fraction of Employees under a Temp Contract



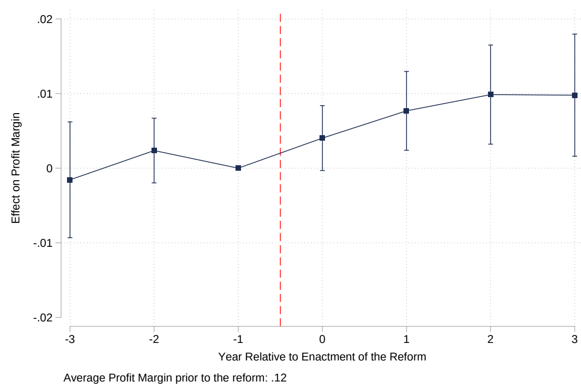
(b) Log Firm Size



(c) Log Labor Costs per Worker



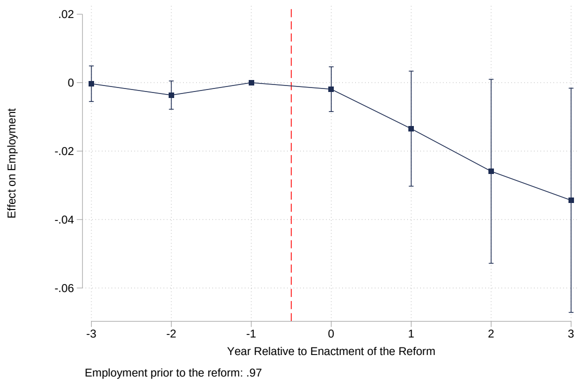
(d) Profit Margin



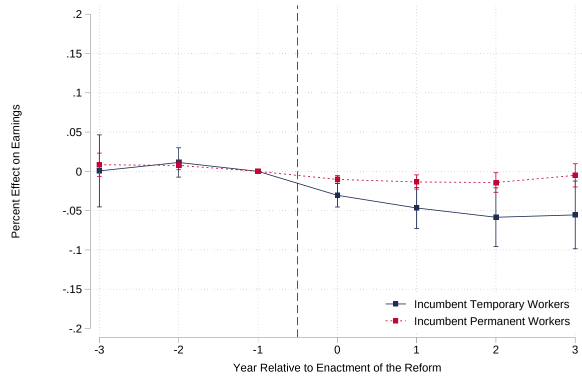
Note: This figure presents the event study coefficients from the firm-level specification described in equation (2). Panel (a) shows estimates on the fraction of employees under a temporary contract. Panel (b) reports effects on log firm size. Panel (c) shows the estimates on the logarithm of labor costs per worker. Panel (d) shows the effects on the profit margin, defined as profits divided by value added. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 4: Impact of the Reform on Incumbent Workers

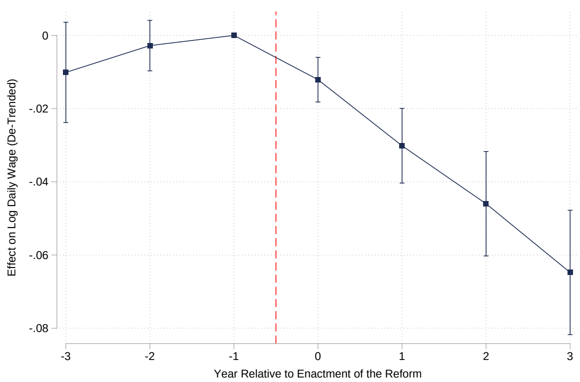
(a) Employment—Incumbent Temporary Workers



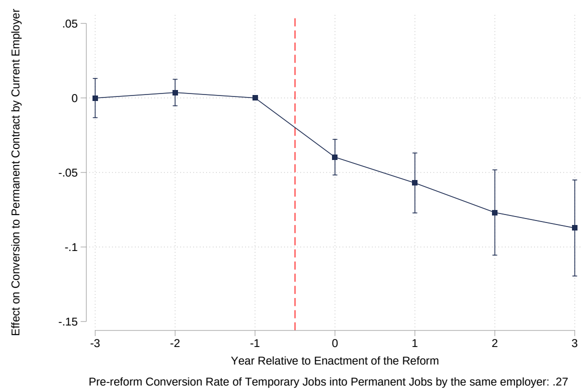
(b) Earnings—Temporary vs. Permanent



(c) Log Daily Wages of Temporary Workers



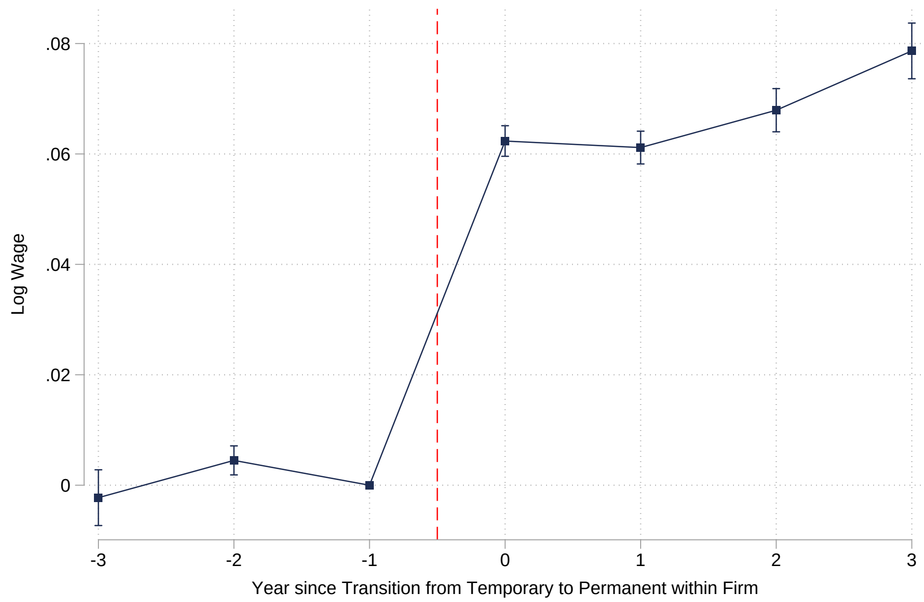
(d) Conversion from Temporary to Permanent



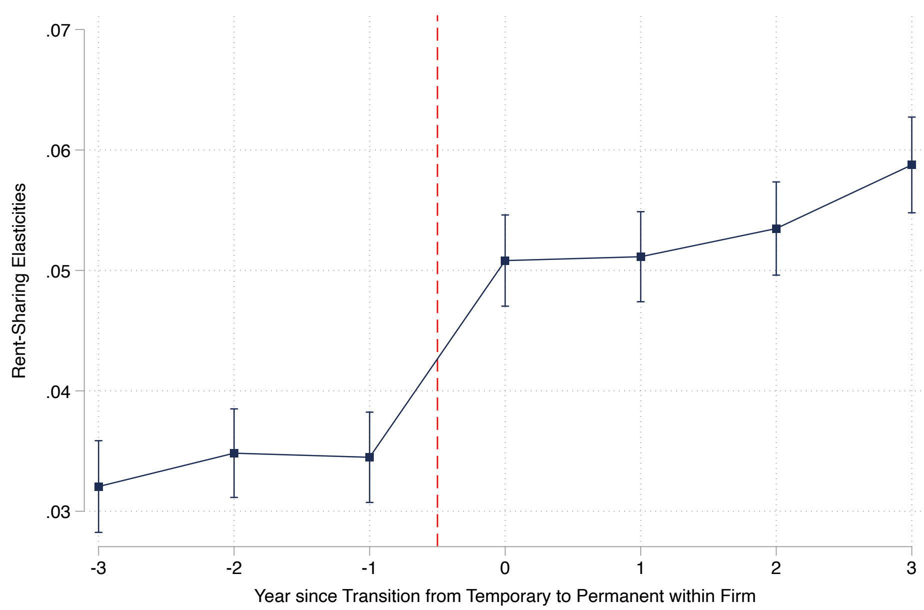
Note: Panel (a) displays event study coefficients from equation (3) estimated on the sample of incumbent temporary workers using as an outcome an indicator equal to one if the incumbent temporary worker is employed in year t. Panel (b) displays the results on the annual earnings of incumbent temporary and permanent workers (conditional on employment). The coefficients in this panel have been rescaled by the corresponding pre-reform average earnings level. Panel (c) displays the effects of the reform on the log daily wage of incumbent temporary workers after de-trending the latter from a linear trend; see text for more details and Figure G7 for the results without de-trending. Panel (d) estimates equation (3) using as an 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 pre-reform annual conversion rate of all temporary jobs into permanent ones. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

Figure 5: Within-Person, Within-Employer Transition from Temporary to Permanent Contract

(a) Event Study



(b) Rent-Sharing Elasticities



Note: Panel (a) reports the event study coefficients from equation (5) 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 more details. Panel (b) reports these event study coefficients when interacted with current log value added per worker; see equation (6) and text for more details. All regression models control for worker fixed effects, a quadratic term in potential experience, and year effects interacted with one-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 before this transition. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

Appendix

A Data Appendix

In this section, 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.[52]

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, and part-time versus full-time status. These data are also combined with some information on the worker such as age, gender, and nationality (Italian versus 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 additional restriction by (i) excluding public-sector employees (around 10% of the existing spell-year observations), (ii) focusing only on spells in which the worker is 16–64 years of age (dropping 0.12% of observations), and (iii) excluding 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 versus 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 CCNL renewals collected by the CNEL as discussed in Section 4.2. 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

[52] The INVIND survey's coverage has improved over time. It started out as being representative only of manufacturing firms with 50+ employees. More industrial sectors were included in the survey in 1999, as were firms with 20–49 employees in 2001 and likewise retail and services firms in 2002 (Bank of Italy, 2008). Given that the survey includes each worker's complete employment history (between 1990 and 2013), regardless of their particular sector each year, we end up with a sample that represents a sizable part of the Italian economy. Di Addario, Kline, Saggio, and Sølvsten (2021) show that firms in INPS-INVIND are slightly larger than the universe of firms with a similar sector decomposition. They also note that an Abowd et al. (1999) based wage decomposition from INPS-INVIND are similar to the ones from the universe of INPS data (e.g., Casarico and Lattanzio, 2019).

99%.

A.2 Firms

In the complementary database labeled "Anagrafica" in Section 4.3, 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 mean 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 includes wages and salaries, social security contributions, severance packages, and retirement contributions as well as other smaller costs.

- Value added: the value that the firm was able to create from inputs during the production process. It is computed as the following: 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 fiscal year profits before taxes. It is computed as the following: 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%.

Table A1 describes our sample construction for the firm-level analysis. The statistics refer to the pre-reform era. Column 1 reports the industry composition as well as average firm size of all the firms observed in INPS-INVIND that existed before the reform. Column 2 reports 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 reweigh the statistics of the INPS-INVIND-Cerved sample using a propensity score reweighing 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, one-digit sector CSC codes[53] 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. Our firm-level event study results based on equation (2) are thus weighted using the inverse propensity score weights just described.

Table A.1: Size and Sector Composition in the Pre-Reform Period

	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 one-digit sector composition in the pre-reform period. 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 can match balance sheet information from CERVED. In column 3, we reweigh these statistics using propensity score reweighting designed to match the sector and firm size pre-reform composition as reported in hte INPS-INVIND sample of column 1. Number of observations refer to the number of firm-year observations during the pre-reform period. We use the CSC (Codice Statistico Contributivo) codes provided by INPS to assign one-digit sector dummies. See text for details.

A.3 Local Labor Market Analysis

The LLM results on the share of temporary jobs, temporary job creation and destruction, and employment 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,

[53]These industry codes ("Codice Statistico Retributivo") are provided directly by the INPS and correspond to four main industries: manufacturing, handicraft ("artigiani"), banking/insurance, and retail and services.

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. We proceed in a similar way to construct the share of new temporary jobs and the destruction rate of expiring temporary jobs. Using the information contained in "Anagrafica," we then calculate the total number of employees in an LLM. Finally, we winsorize log employment at 5% and 95%.

B A Case Study of Three Italian CCNLs

To better understand how the legislative framework regarding temporary work changed after the approval of the reform, this appendix examines in detail the text of the retail, food, and metal manufacturing sector CCNLs, which are some of the most common contracts registered in the INPS-INVIND administrative data.[54]

B.1 Retail

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).[55] This CCNL states that the national legislation 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).

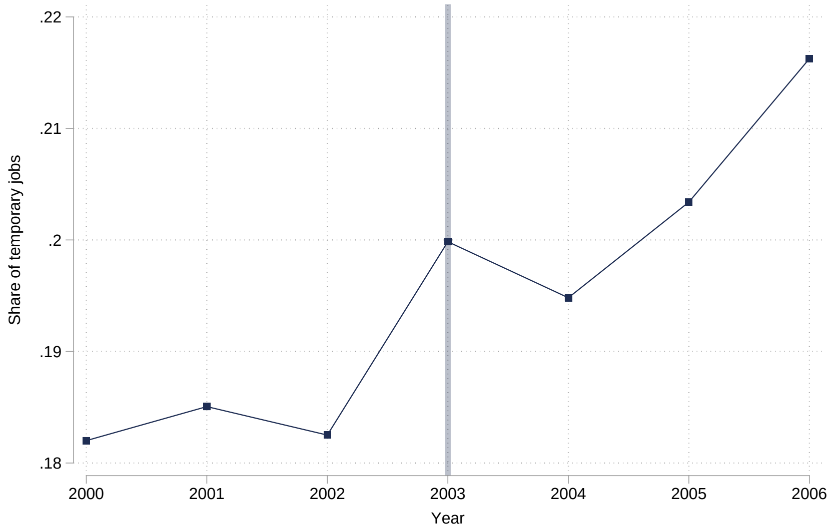
What is the evolution of the share of temporary workers in the retail sector? While the share of temporary workers seems to remain relatively constant between 1999 and 2002, the time series exhibits a discontinuity in 2003, which coincides with the year in which the CCNL in this sector is renewed and thus with the year in which the first CCNL adopting the reform is signed. In particular, we observe a 4 percentage point increase in temporary contracts in the year the new legislation was adopted—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 frame-

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

[55] We refer, in particular, to the retail and services CCNL of 07/18/2003.

Figure B1: Evolution of the Share of Temporary Workers in the Retail CCNL



Note: This figure shows the share of temporary workers in the retail CCNL.

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

B.2 Food

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 reform was approved, the CCNLs associated with the food sector stated clearly that an employment contract should typically be on a permanent basis.[56] Temporary work arrangements could only be allowed under specific circumstances, in line with the provisions of law 56/1987.[57] 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 reform was approved, the structure of the CCNLs changed in a similar fashion to what we observed in the retail sector. Importantly, unlike in the pre-reform CCNLs, the

[56]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 industry CCNL, and therefore this is the one we analyzed in more detail.

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

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 special circumstances under which it was possible to hire on a temporary basis is no longer specified.[58] 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 the reform.

Figure B2 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 the reform 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 B2: 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 small and medium food enterprises ("alimentari PMI").

B.3 Metal Manufacturing

The metal manufacturing CCNL ("industrie metalmeccaniche") is one of the most important CCNLs in Italy.[59] This agreement exhibits a very peculiar legal framework with respect to temporary contracts. While the CCNL signed before the approval of the reform lists a series of specific circumstances allowing temporary worker hiring (citing law 56/1987), the following CCNL (signed on 05/07/2003) does not mention temporary work at all.[60] In contrast to the food

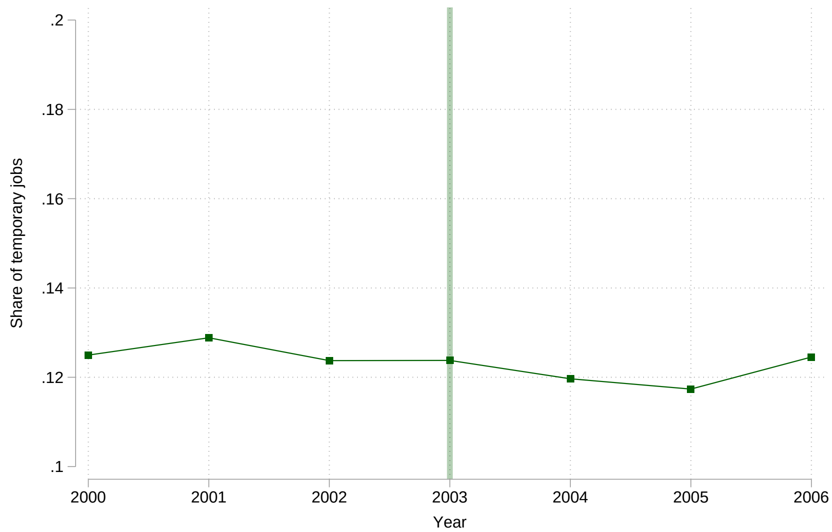
[58]The only exception is the food SME CCNL. While this CCNL states that temporary contracts can be signed in accordance with the reform, it still provides a list (which appears to be unchanged relative the pre-reform one) of circumstances under which it was possible to hire temporary workers.

[59]This CCNL should not be confused with the metal *handicraft* CCNL that we analyzed in Section 5.

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

and retail sector's CCNLs analyzed above, this agreement does not cite the reform but instead defers the regulation on temporary contracts to an agreement planned for 2004, at which point, however, is unclear whether it was fully implemented.[61]

Figure B3: 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 CCNL ("industrie metalmeccaniche").

Figure B3 shows that 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. The narrative presented above combined with this result led us to perform a robustness analysis in which we consider the metal manufacturing CCNL as a control group in all the periods of our event study specification. As discussed in more detail in Section C.1, this analysis is useful to assess the sensitivity of our event study specification to the fact that in our context (i) all units are going to be eventually treated and (ii) they become treated in a relatively short-window of time. However, our preferred empirical strategy remains one where also the metal manufacturing CCNL remains treated. As stated by the law, the new policy on temporary contracts is implemented once the corresponding CCNL has been renewed. Even if this sector decided to either have a partial or null take up of the reform, this means that we are estimating intention-to-treat effects that capture the overall (reduced-form) effect of the reform.

[61] 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 Robustness

C.1 Metal Manufacturing CCNL as a Control Group

As previously mentioned, 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 reform's passage—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 reform to be implemented on temporary contracts. It is unclear, however, whether the legislation on temporary employment agreements was fully implemented in the metal manufacturing sector (Santucci et al., 2008); see Appendix B.3 for details.

As a robustness check, we thus evaluate whether our results change if we assume that the metal manufacturing CCNL continued to operate in the pre-reform regime when it comes to temporary employment contracts.[62] Specifically, we re-estimate equation (1) using the metal manufacturing CCNL as a control group. This also provides us with a useful specification from which we can assess if our baseline estimates of equation (1) are sensitive to the fact that all CCNLs are eventually going to receive treatment and, additionally, that they do so within a relatively short period, issues that we also analyze in more detail in Section C.2.

Figure C1 shows our baseline results on the share of temporary contracts and total employment using the metal manufacturing CCNL as a control group. Reassuringly, both patterns and magnitudes of the event study coefficients are very close to the estimates reported in Figure 2, which are reproduced in Figure C1 for convenience.

C.2 Two-Way Fixed Effects Estimates and Treatment Effects Heterogeneity

The baseline results of this paper are based on variations from the two-way fixed effects event study specification highlighted in equation (1). However, several recent papers (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021) have warned against the identification of average treatment effects from two-way models in the presence of treatment effects heterogeneity. These concerns extend to dynamic difference-in-differences or event study models (Sun and Abraham, 2020; Borusyak et al., 2021).

The origin of the problem is the fact that models such as equation (1) impose an assumption of treatment effects homogeneity. Under this assumption, identification of the event study coefficients $\{\theta_k\}$ is going to come from comparisons of units that implemented the reform late versus units that implemented it earlier. However, in the presence of treatment effects heterogeneity, such comparisons between late and early takers might be spurious and distort the weights that the OLS estimator of $\{\theta_k\}$ from equation (1) is going to place on the various treat-

[62] This was also the choice of Cappellari et al. (2012).

ment effects, even making them negative in some instances. These issues can be particularly pressing in contexts—like ours—where all the units are going to eventually receive the treatment. As pointed out by Sun and Abraham (2020), treatment effects heterogeneity can also invalidate tests based on pre-trends. To address these concerns, we re-estimate our baseline results using methods designed to deal with issues of treatment effects heterogeneity inspired by the recent contribution Sun and Abraham (2020) and Borusyak et al. (2021).

Sun and Abraham (2020). We begin by implementing a version of the interaction-weighted estimator of Sun and Abraham (2020). That is, we fit equation (1) separately for each cohort t^*, where cohorts correspond here to the year in which a given CCNL has implemented the reform. In performing this exercise, we use the metal manufacturing CCNL as a control group, following the discussion presented in Section C.1. Thus, our event-study coefficients are now obtained by simple 2×2 comparisons where we confront the evolution of a given outcome for a cohort t^* relative to the control metal manufacturing CCNL. This avoids comparisons between early and late enactors which can be problematic in the presence of treatment effects heterogeneity.[63] Figure C2 displays the results using the share of temporary employment contracts in a given LLM as the outcome for the most important cohorts (2002, 2003, 2004, 2005).

The pattern for each cohort resembles the overall pattern displayed when estimating the aggregate specification in equation (1). Pre-trends in particular appear relatively flat within each cohort. This is important because a key result of Sun and Abraham (2020) is that assessing the parallel trend assumption by looking at the event study coefficients θ_k for $k < 0$ from the two-way model in equation (1) might be invalid in the presence of treatment effects heterogeneity. The cohort-specific evidence presented in Figure C2 thus provides an additional test in favor of the parallel trend assumption. The fact that the later cohorts, such as the 2005 cohort, exhibit a relatively flat profile pre-event also suggests that anticipation effects do not represent a particular concern here and that these late adopters CCNLs are *not* implementing the reform in a systematically different way and that this, in-turn, is affecting our results.[64]

As a last step, we then aggregate each estimated cohort-specific study coefficient $\hat{\theta}_{k,t^*}$ as suggested Sun and Abraham (2020); i.e., we compute

$$\hat{\theta}_k^{SA} = \sum_{t^*} \omega_{t^*,k} \hat{\theta}_{k,t^*}, \tag{7}$$

where $\omega_{t^*,k}$ represents the share of micro-observations observed for cohort t^* at event time k. This aggregation of cohort-specific treatment effects using their corresponding empirical

[63] The approach pursued here is similar in spirit to the "stacked" approach presented by Cengiz et al. (2019).

[64] There are a total of 18 CCNLs that implemented the reform in 2005 or later. Most of these CCNLs tend to be relatively small, as they employ less than 1% of workers present in our data. The exemption is the Metal Handycraft CCNL which is a relatively large CCNL broadly similar in scope and size to other important CCNLs present in Italy such as the Metal Manufacturing CCNL or the Retail CCNL.

share avoids the issues of negative weights described in Sun and Abraham (2020). The resulting estimates $\{\hat{\theta}_k^{SA}\}$ are plotted in Figure C1 when evaluating the reform's effect on the share of temporary contracts and log employment. Reassuringly, the estimates obtained overlap well with our baseline two-way estimates from equation (1).

Borusyak, Jaravel, and Spiess (2021). To further validate our baseline results, we also use the recently proposed "imputation" estimator of Borusyak et al. (2021) that is robust to the presence of unrestricted heterogeneity in treatment effects. We proceed as follows.

1. We first fit the model
$$y_{cpt} = \eta_{cp} + \lambda_{pt} + X_{cpt}^{\top}\beta + e_{cpt} \tag{8}$$
to all LLM × year observations in the pre-reform regime.

2. We then impute a counterfactual outcome $\hat{y}_{cpt}$ by extrapolating the predictions obtained from equation (8) to observations corresponding to the post-reform regime. Using $\hat{y}_{cpt}$, we construct LLM × year specific treatment effects as defined by
$$\hat{\theta}_{cpt} = y_{cpt} - \hat{y}_{cpt}. \tag{9}$$

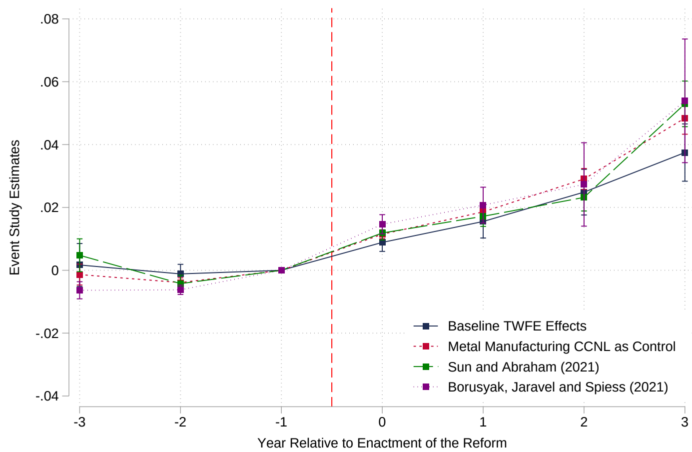
3. The average effect of the reform k years after its implementation is then computed as the weighted average of $\hat{\theta}_{cpt}$:
$$\hat{\theta}_k^{BJS} = \sum_{c,p} \hat{\theta}_{cpt}\omega_{cpt}\mathbf{1}\{t = t_c^* + k\}, \tag{10}$$
where ω_{cpt} represents the share of micro-observations in a given LLM × year cell.

4. As described by equation (11) in Borusyak et al. (2021), the "pre-event" coefficients, θ_k for $k < 0$, are constructed by augmenting equation (8) with coefficients for the pre-event event dummies:
$$y_{cpt} = \eta_{cp} + \lambda_{pt} + X_{cpt}^{\top}\beta + \theta_{-3}R_{ct}^{-3} + \theta_{-2}R_{ct}^{-2} + e_{cpt}, \tag{11}$$
where recall that R_{ct}^k is defined as $R_{ct}^k = \mathbf{1}\{t = t_c^* + k\}$.

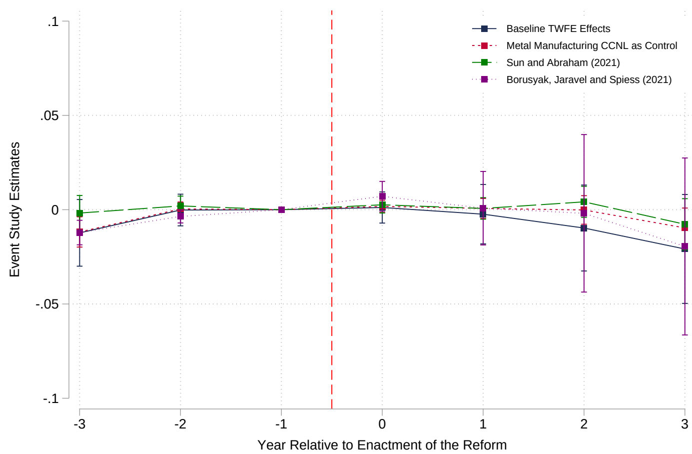
Figure C1 displays the resulting event study estimates. These estimates also appear to closely track our baseline estimates and deliver the same message of Section 7: the reform significantly increased the share of temporary contracts but did not significantly increase total employment.

Figure C1: Robustness of the Baseline Estimates

(a) Share of Temporary Jobs



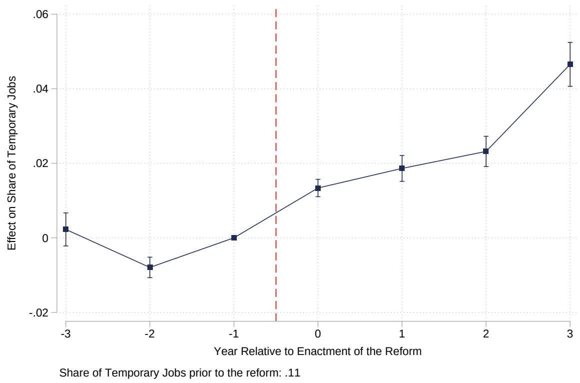
(b) Log Employment



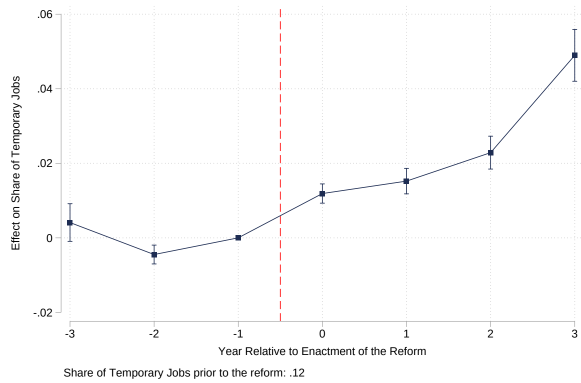
Note: In this figure, we confront our baseline event study estimates from the two-way fixed effects specification of equation (1) displayed in Figure 2—and reprinted here under the "Baseline TWFE Effects" label—with alternative approaches designed to deal with the issue of negative weights of two-way models described, for instance, in Borusyak et al. (2021) and Sun and Abraham (2020). We begin by reporting the estimates that we obtain under the alternative assumption that the metal manufacturing sector represents a never-enacting sector; see Section C.1 for details. We then display the estimates described in Section C.2 inspired by the interaction-weighted estimator proposed by Sun and Abraham (2020). We also report estimates obtained after applying the estimator recently proposed by Borusyak et al. (2021), whose implementation is also described in Section C.2. To compute both the Sun and Abraham (2020) and Borusyak et al. (2021) estimator, we maintain our binning strategy at $k = -3$ and $k = 3$. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level. The standard errors for the Sun and Abraham (2020) and Borusyak et al. (2021) point-estimates are computed via a block-bootstrap procedure.

Figure C2: Effects of the Reform across Different Cohorts of Implementation

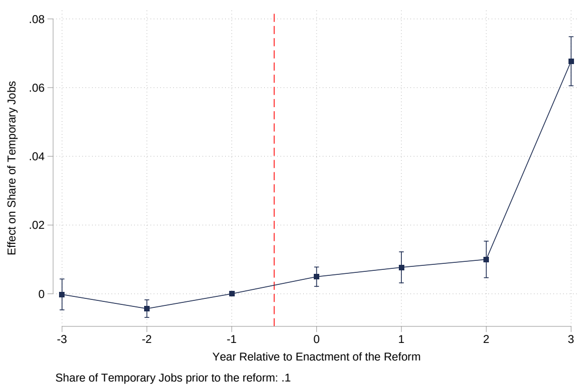
(a) CCNLs That Implemented Reform in 2002



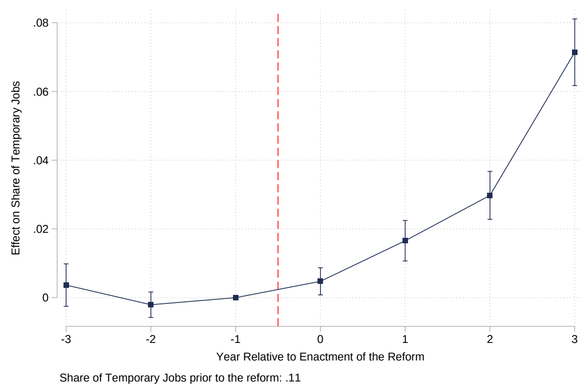
(b) 2003



(c) 2004



(d) 2005



Note: This figure reports estimates from equation (1) fitted separately for cohorts of CCNLs that implemented the reform in $t^* \in \{2002, 2003, 2004, 2005\}$ using as an outcome the share of temporary jobs observed in a given LLM × year cell. To compute these coefficients, we maintain the assumption described in Section C.1 that the metal manufacturing CCNL represents a never-enacting sector. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

C.3 Additional Robustness Checks

Here we describe a set of additional robustness checks and analysis for our baseline results described in Section 7.

Alternative Definition of Implementation Year. In our baseline analysis, we assign the first year in which the new agreement post-2001 is signed by a given CCNL, as explicitly expressed in the reform, as the year of the reform's implementation. However, the decision of when to sign the contract could be endogenous. To account for this concern, Figure C3 displays the results from an alternative specification of equation (1) in which we define the first year of the reform's implementation as the expiration year of the pre-reform CCNL. We find basically the same effects displayed in Figure 2, with a large positive effect on the share of temporary jobs and no evidence of positive effects on log employment. More specifically, the point estimates on log employment remain negative and are not significant in the short run. In the medium run, they are also negative (as in the baseline), but, differently from the baseline, they are now marginally significant.

Temporary Jobs Signed Directly by the Firm. In the Italian labor market, as discussed in Section 4.1, a temporary job might be signed directly by the user firm or via a temp agency. We can then evaluate whether the impact on the share of temporary jobs is robust to excluding temporary agency firms. Figure C3 displays the results from equation (1) using as an outcome the share of temporary jobs signed directly by the firm. Interestingly, the effect on the share of temporary jobs and on the share of temporary jobs signed directly by the firm tend to coincide almost perfectly. For instance, the medium-run effect of the reform on the share of temporary jobs signed directly by the firm is 0.036, around 96% of the effect on the overall share obtained when summing both types of temporary jobs. Therefore, we conclude that the reform has almost exclusively impacted the propensity of firms to directly sign temporary jobs with workers.

Excluding Sampling of New Sectors. As discussed in Section 4.1, the INVIND sample has been updated over the years, with, for instance, the inclusion of the retail sector in 2002. We thus verify that our results are not driven by the entry of new sectors in the underlying data. Specifically, we re-estimate equation (1), focusing only on workers whom at some point worked for a sector already existing in the pre-2001 data, i.e., as if INVIND had not expanded the sample of sectors included. The results displayed in Figure C3 show that we obtain virtually the same effects using this alternative sampling methodology. This is most likely because the original micro-data INPS-INVIND itself already pertains a balanced structure. That is, it provides the entire employment histories of individuals (no matter the sector), provided that they were employed by a firm covered by the INVIND survey in a given year.

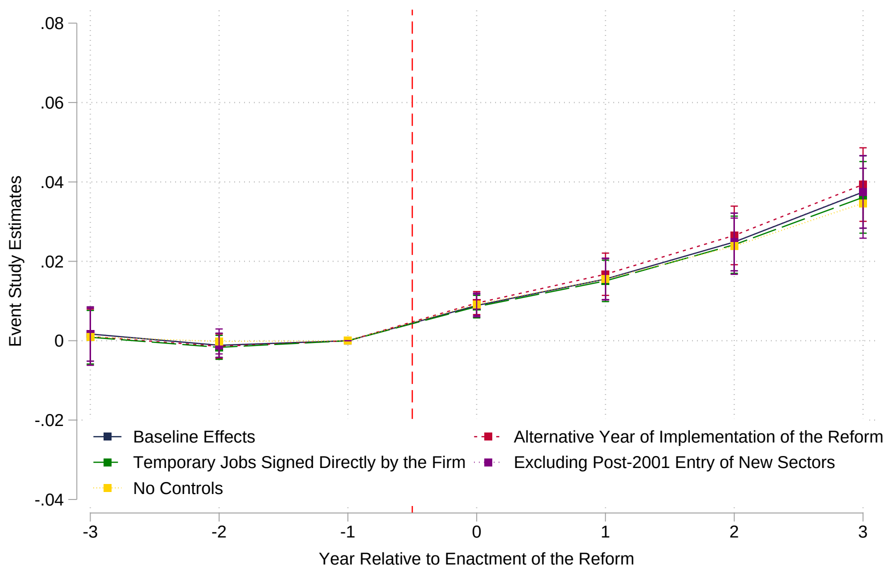
No Controls. Finally, Figure C3 also shows the effects obtained when estimating equation (1) without controls. Again, the point estimates associated with the reform's effects are very similar to those from our baseline specification.

Effect of the Reform versus Renewal of the CCNL. In our setting, the year of the reform's implementation coincides with the renewal of the associated CCNL. This could potentially conflate the reform's effect with the effects of the CCNL renewal and most prominently the update of the associated wage floors. Several comments are in order.

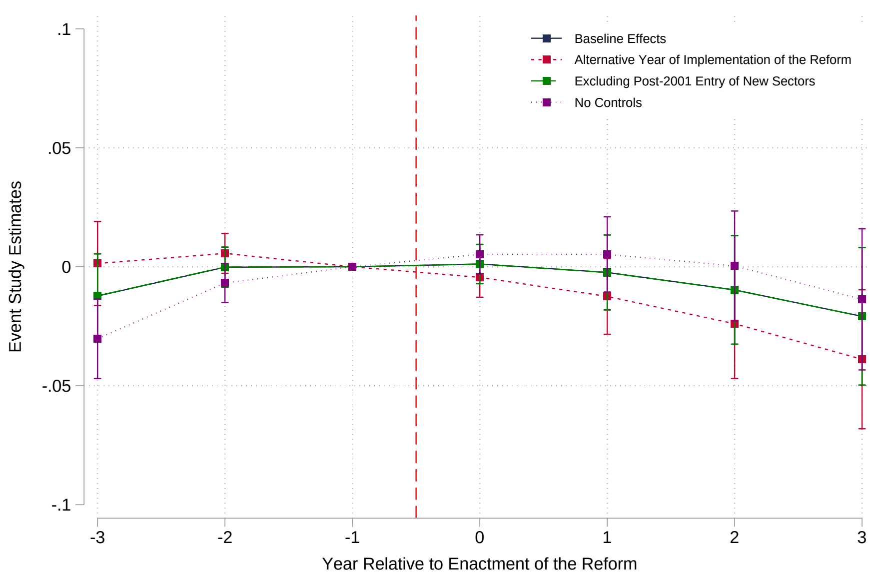
First, if this concern is valid, we should expect effects on, say, firms that do not increase the share of temporary employees (the key first stage induced by the reform) but yet experience changes in other outcomes as a result of the CCNL renewal. Similarly, permanent workers are

Figure C3: Additional Robustness Checks

(a) Share of Temporary Jobs



(b) Log Employment



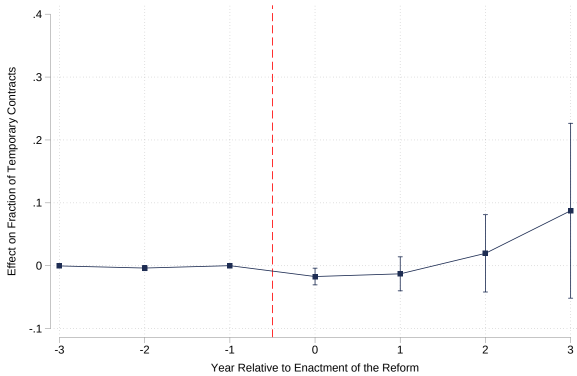
Note: In this figure, we confront our baseline event study estimates from equation (1) displayed in Figure 2—and reprinted here under the "Baseline Effects" label—with alternative estimates designed to address various concerns. "Alternative Year of Implementation of the Reform" shows estimates obtained after letting the first year of the reform's implementation coincide with the pre-2001 CCNL's year of expiration (as opposed to the year in which the new agreement is signed). "Temporary Jobs Signed Directly by the Firm" shows the effects of the reform on the share of temporary jobs signed directly by the firm (therefore excluding those temporary jobs signed via a temporary work agency). "Excluding Post-2001 Entry of New Sectors" estimates equation (1) only using sectors already existing before the national reform was signed. "No Controls" estimates equation (1) without the inclusion of controls. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

not directly exposed to the reform but are still bounded by the CCNL renewals and thus might experience significant changes in key labor outcomes. In the firm-level analysis displayed in Table 3, we showed that firms with high-turnover costs cannot respond to the reform by increasing their share of temporary employees. This zero first stage maps into a null effect on key outcomes, consistent with the key idea underlying our research design that the renewal of CCNLs after the reform's passage is in fact capturing the reform's effects. Similarly, Figure 4 shows that permanent worker earnings are virtually unaffected by the reform's passage, and thus all earnings losses are concentrated among temporary workers.

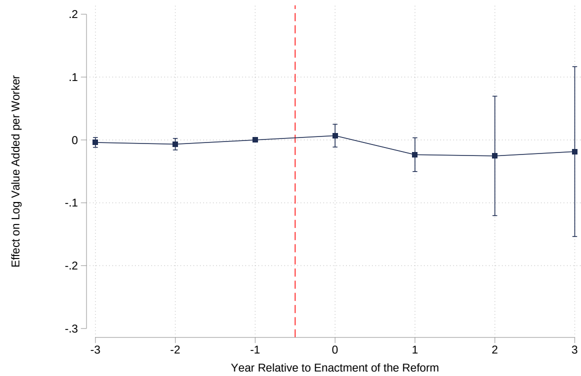
Second, we run a simple placebo experiment where we fit a fictitious year of a reform (2006) and run a placebo analysis at the firm level. In this placebo event study analysis, firms are treated according to the year of renewal following the fake reform year 2006. As we would expect if our main results were not affected by the CCNL renewals themselves, Figure C4 shows there are no significant effects on key outcomes such as the share of temporary workers, labor costs, value added, and profit margins. Thus, we find no evidence suggesting our results may conflate the reform's effect with the effects of the renewal of CCNLs.

Figure C4: Placebo Analysis

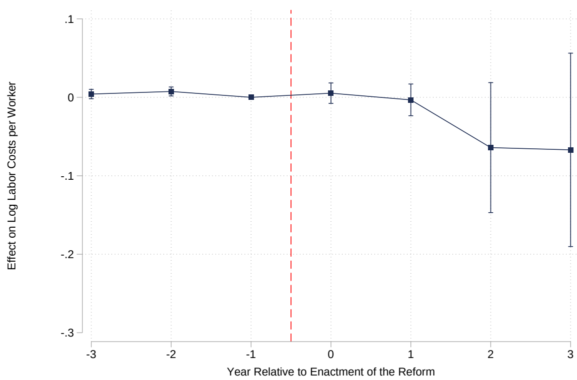
(a) Fraction of Employees under a Temp Contract



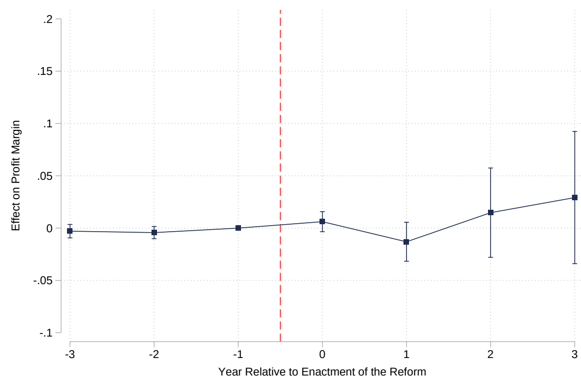
(b) Log Value Added Per Worker



(c) Log Labor Costs



(d) Profit Margin



Note: This figure presents results from a placebo event study analysis at the firm level. We start by setting a fake year of the reform (2006). We then consider firms as treated based on the year where their dominant CCNL was renewed following the passage of this fake reform using the event study equation (2). Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

D Additional Analysis

D.1 The Reform's Effects on Job Transitions

Section 7 shows that the reform significantly increased the destruction rate of expiring temporary jobs. Since these are *expiring* temporary jobs, we expect this increase in their destruction rate to be entirely explained by a decrease in the within-firm conversion rate of temporary jobs into permanent positions. Panel (a) of Figure D1 confirms this prediction. It shows that the increase in the medium-run job destruction rate of expiring temporary jobs is largely explained by a reduction in the probability to observe within-employer transitions from temporary to permanent jobs.

But how is the post-reform labor market absorbing the increase in the destruction rate of expiring temporary jobs? 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 in $t+1$, (ii) be hired by a new employer under a permanent contract in $t+1$, or (iii) remain non-employed in $t+1$. Panel (b) of Figure D1 shows that the vast majority (87%) 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 $t+1$, suggesting the reform significantly increased job-to-job mobility (across temporary employment contracts), consistent with the job creation evidence shown in Section 7. 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.

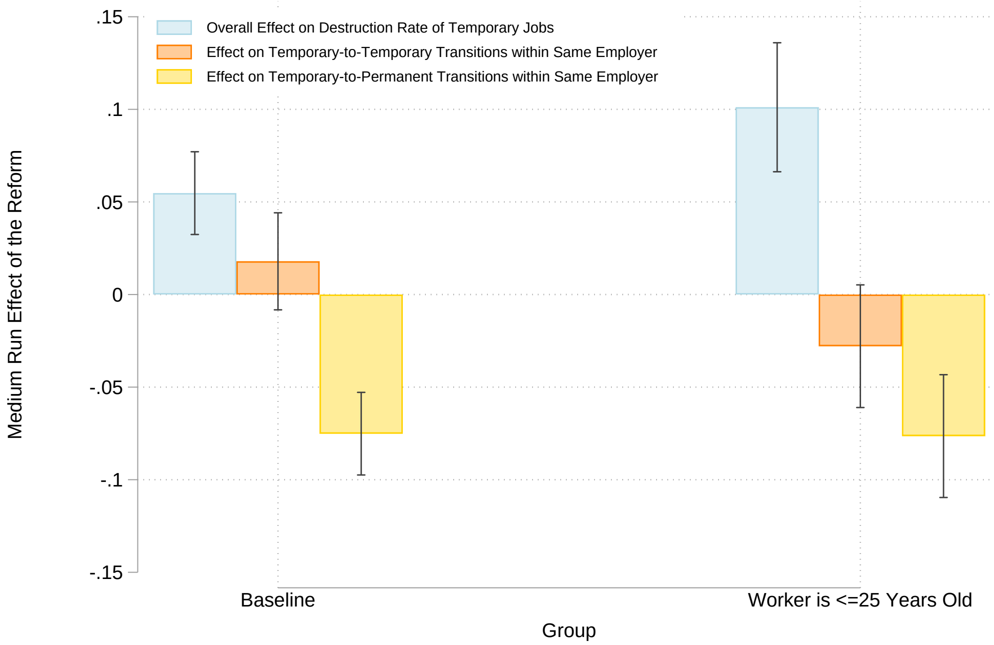
Table D1 prints the results displayed in Figure D1 and provides some additional results. Panel (a) reports the coefficient θ_3 from equation (1) for all observed temporary jobs (i.e., without restricting to expiring temporary jobs). The general patterns that we found when restricting to expiring temporary 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 destruction rate) for all temporary jobs, but it increases by roughly 5.4 percentage points ($\approx$ 23%) once we condition on expiring temporary jobs. This is consistent with a key prediction of the model presented in Appendix E: once a temporary contract's "expiration date" arrives, firms in the post-reform period will be disproportionately more likely to avoid converting the expired temporary job contracts to permanent ones.

Panel (c) further restricts to expiring temporary jobs of young workers (up to 25 years old). We find an extremely large and significant ($\approx$ 40%) increase in the associated destruction rate. A decomposition of this increase in the destruction rate suggests that after the reform, younger workers were particularly less likely to experience a within-employer conversion to a permanent job: 74% found another temporary job with a new employer within the next year, while 19% ended up being non-employed for a full year. By contrast, Panel (d) shows that the reform had essentially no economically meaningful effect on the transition rates of individuals with a permanent job in year t.

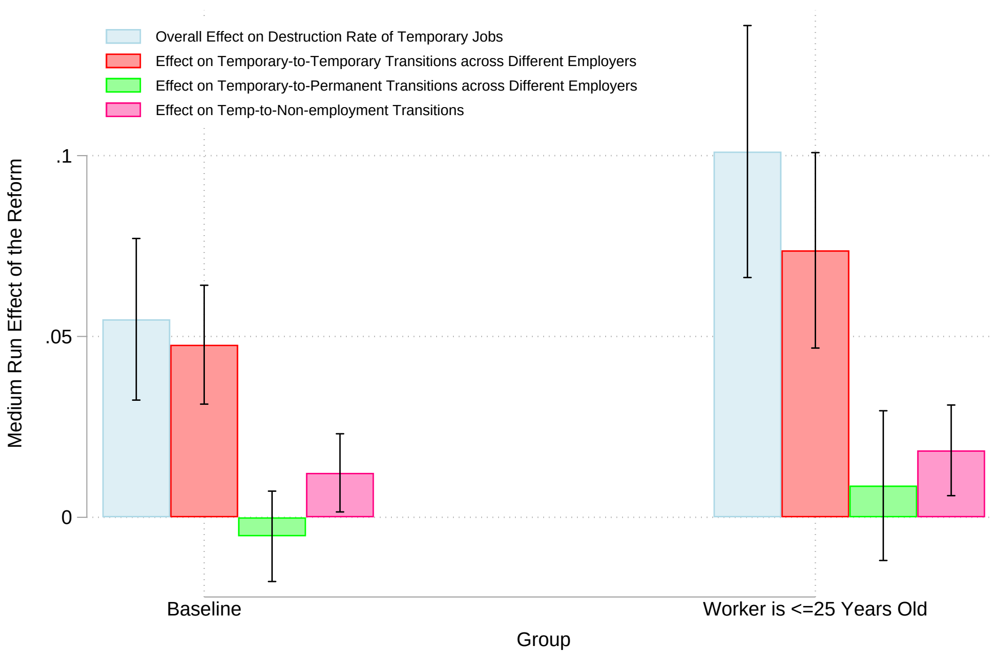
In conclusion, the reform affected two key transitions. It significantly reduced the probability of within-employer temporary-to-permanent job transitions while significantly increasing the probability of between-employer temporary to temporary job transitions, with these two patterns being particularly evident for young workers. Remarkably, the decrease in the within-employer temporary-to-permanent conversion followed by an increase in the between-employer temporary-to-temporary job transitions can also be seen when looking at the aggregate time-series data. Figure D2 shows that in 1998 around 30% of temporary contracts were converted into permanent positions by employers. By 2010, this share has dropped to a little over 10%.

Figure D1: Decomposition of the Increase in the Temporary Jobs Destruction Rate

(a) Decomposition #1



(b) Decomposition #2



Note: This figure presents estimates from equation (1) 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 (e.g., share of expiring temporary jobs that are converted into a permanent position in $t+1$ by the corresponding employer) as described in the figure legends; see text for more details. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

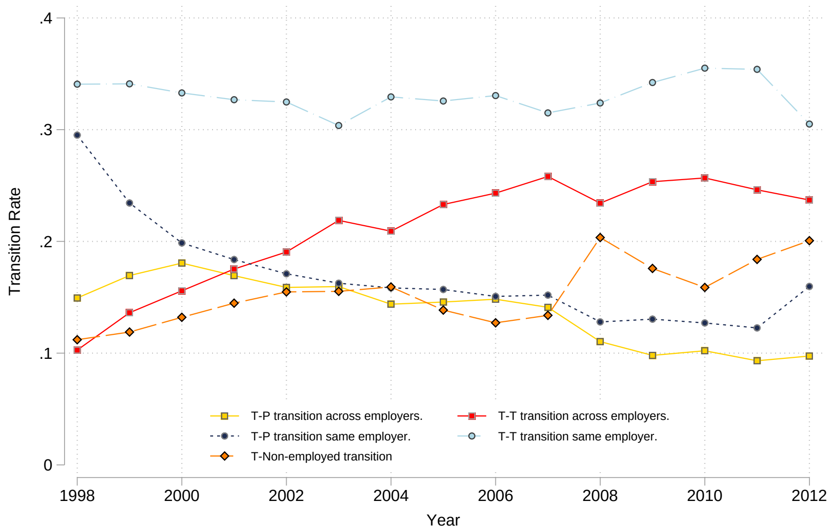
Table D1: The Impact of the Reform on Job Flows

	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	0.0323***	-0.0039	-0.0299***	0.0292***	0.0037	-0.0005
	(0.0064)	(0.0064)	(0.0046)	(0.0050)	(0.0035)	(0.0029)
Pre-Reform Mean	.37	.36	.27	.12	.16	.09
# of LLM-Year Observations	158,841	158,841	158,841	158,841	158,841	158,841
Panel (b): Expiring Temp Jobs						
Medium-Run Effect of the Reform	0.0547***	0.0179	-0.0751***	0.0477***	-0.0053	0.0123**
	(0.0114)	(0.0134)	(0.0114)	(0.0084)	(0.0064)	(0.0055)
Pre-Reform Mean	.24	.27	.49	.07	.12	.04
# of LLM-Year Observations	103,598	103,598	103,598	103,598	103,598	103,598
Panel (c): Expiring Temp Jobs, Young Workers						
Medium-Run Effect of the Reform	0.1011***	-0.0279*	-0.0764***	0.0738***	0.0088	0.0185***
	(0.0178)	(0.0169)	(0.0169)	(0.0138)	(0.0106)	(0.0064)
Pre-Reform Mean	.25	.28	.46	.09	.12	.04
# of LLM-Year Observations	47,885	47,885	47,885	47,885	47,885	47,885
Panel (d): Permanent Jobs						
Medium-Run Effect of the Reform	0.0027	-0.0006	-0.0043	0.0084**	-0.0102	0.0045
	(0.0089)	(0.0012)	(0.0090)	(0.0040)	(0.0068)	(0.0051)
Pre-Reform Mean	.26	.01	.72	.04	.13	.09
# of LLM-Year Observations	216,293	216,293	216,293	216,293	216,293	216,293

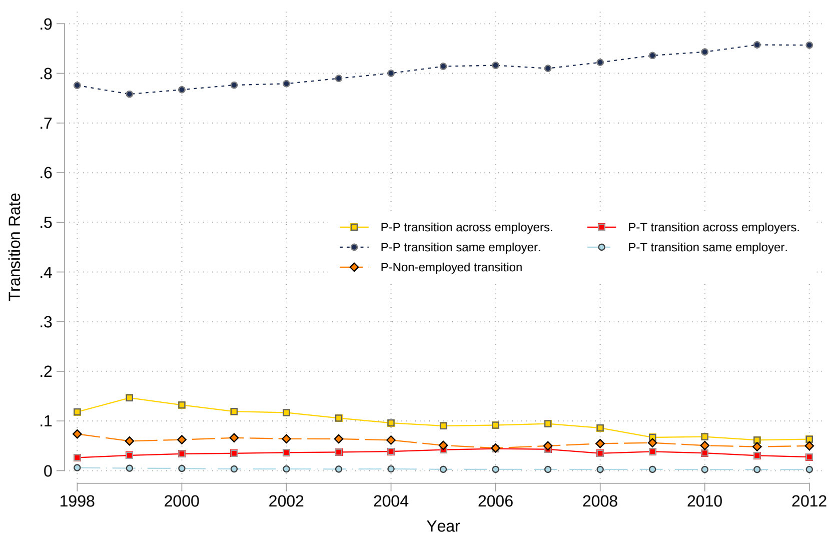
Note: This table reports the coefficient θ_3, defined as ``Medium-Run Effect of the Reform," which corresponds to the effect of the reform measured four-plus years after the reform's enactment; see equation (1). Panel (a) estimates equation (1) using the destruction rate of temporary jobs as an outcome. Panel (b) estimates equation (1) only for temporary jobs that in year t already existed for two or more years ("expiring temporary jobs"). 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 in year t . Panel (d) instead focuses on permanent job turnover rates as well as transitions. In Column 1 the dependent variable is the share of jobs that are observed in year t and end up being destroyed in year t+1 . We also report how the reform impacted year-to-year transition rates within/across employers interacted with different contract types (temporary/permanent) for the sample 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. All results are weighted by the number of workers observed in a given LLM-by-year cell. Standard errors are displayed in parentheses and are clustered at the LLM level.

Figure D2: Transitions of Temporary and Permanent Jobs

(a) Transitions of Temporary Jobs



(b) Transitions of Permanent Jobs



Note: Panel (a) focuses on individuals who had a temporary job in year t and reports the corresponding shares of individuals who in year $t+1$ are either (i) under a temporary contract with a different employer relative to year t (T-T transition across employers), (ii) under a permanent contract with a different employer relative to year t (T-P transition across employers), (iii) under a temporary contract with the same employer of year t (T-T transition same employer), (iv) under a permanent contract with the same employer of year t (T-P transition same employer), or (v) no longer employed (T-Non-employed transition). Panel (b) is similar but for is conducted on individuals with a permanent contract job in year t.

D.2 Bounding of Labor Market Entrants

When studying new entrants, an important restriction of the empirical design of equation (4) is that we must condition on individuals finding a first job. The reform, however, might increase the likelihood of obtaining that 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 to compensate for the net present value of earnings losses reported in column 1 of Table 5.

Let (E^R_{it}, E^{NR}_{it}) 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 $E_{it} = E^R_{it}R_i + E^{NR}_{it}(1 - R_i)$, where R_i denotes reform status. Similarly, realized earnings can be written as $Y_{it} = Y^R_{it}R_i + Y^{NR}_{it}(1 - R_i)$. 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) $(Y^R_{ie+s}, Y^{NR}_{ie+s}, E^R_{ie}, E^{NR}_{ie}) \perp R_i$, where $s \in \{0, 1, \ldots, S\}$; (ii) $E^R_{ie} \geq E^{NR}_{ie}$; (iii) $Y^R_{ie+s} - Y^{NR}_{ie+s} = \Delta_s$; and (iv) the reform affects the probability of finding a job only in the first year of the search.[65] Assumption (i) states that reform status is as good as randomly assigned.[66] 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 present discounted value (PDV) effect of the reform on earnings for individuals entering the labor market in year e at a given interest rate r is

$$PDV_{e,S}(r) = \underbrace{\frac{\Pr(E^R_{ie} > E^{NR}_{ie})}{\Pr(E_{ie} = 1|R_i = 1)} \mathrm{E}\left[Y^R_{ie}|E^R_{ie} > E^{NR}_{ie}\right]}_{\text{Extensive margin effect at entry}} + \\ + \underbrace{\frac{\Pr(E^R_{ie} = E^{NR}_{ie})}{\Pr(E_{ie} = 1|R_i = 1)}\Delta_0}_{\text{Intensive margin effect at entry}} + \underbrace{\sum_{s=1}^{S} \frac{\Delta_s}{(1+r)^s}}_{\text{Post-entry treatment effects}} . \tag{12}$$

The first group in (12) includes the "compliers" (i.e., individuals who can find a first job in the first year of the search in the reform counterfactual but not in the non-reform one).[67] This

[65] Assumption (iv) implies that people always find a job by the second year of their 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 D3), we believe that our results are robust to alternative assumptions.

[66] 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 (4).

[67] These are individuals such that $E^R_{ie} > E^{NR}_{ie}$ who populate the red area in Figure E1 in our model.

creates an extensive margin effect at entry, the magnitude of which 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 must sum the discounted income differences Δ_s for $s \geq 1$ following entry, as reflected by the third term in (12).

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 would not 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.[68] 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:

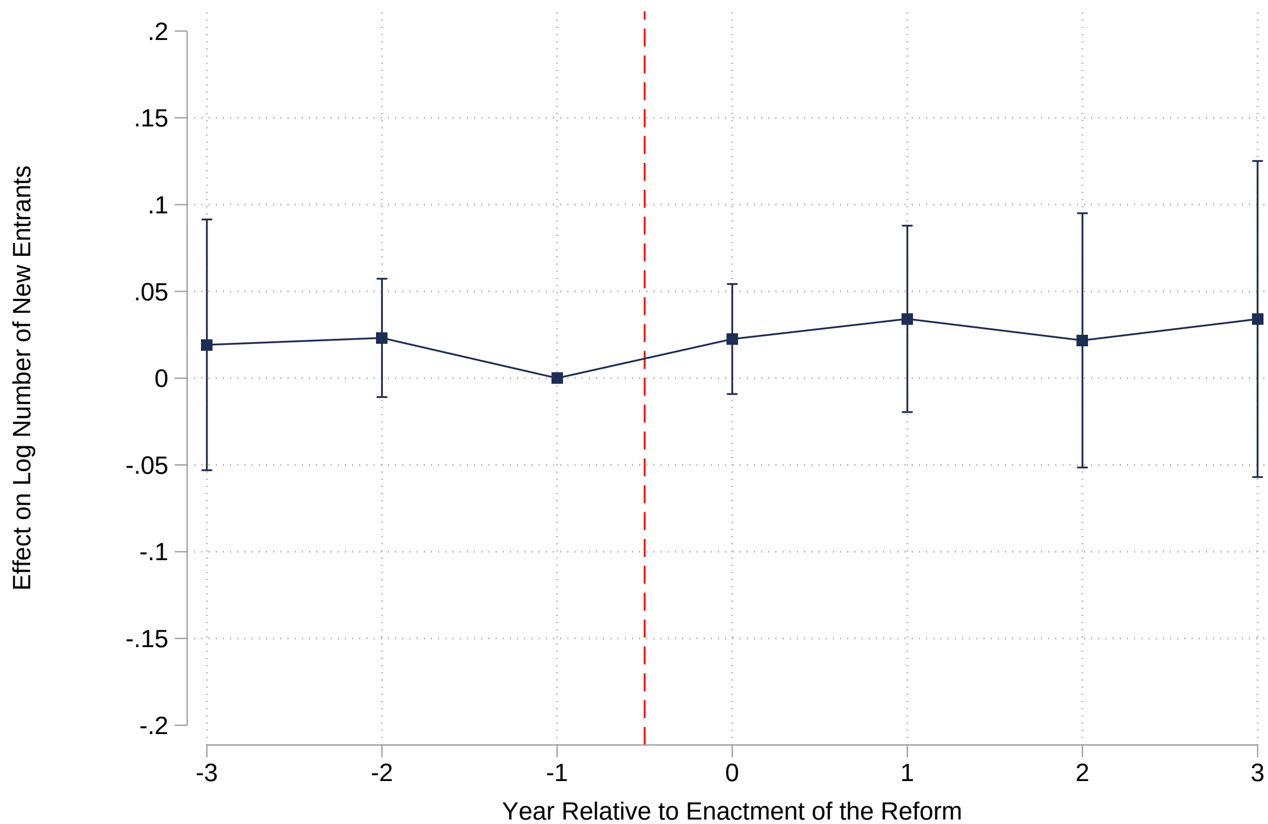
$$0 = \underbrace{\pi_e \left\{ \mathrm{E}[Y_{ie}|R_i = 0, E_{ie} = 1] - \Delta_0 \right\}}_{\substack{\text{Extensive margin}\\ \text{effect at entry}}} + \underbrace{(1 - \pi_e)\,\Delta_0}_{\substack{\text{Intensive margin}\\ \text{effect at entry}}} + \underbrace{\sum_{t=1}^{S} \frac{\Delta_s}{(1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

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% to equalize the net present value of earnings before and after the reform. Figure D3 shows that the logarithm of the number of new entrants appears flat in event time, implying that the share

[68] 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.

of compliers is relatively small. More importantly, this suggests our main results on earnings losses for new entrants are robust to a potential, empirically reasonable increase in the entry rate.

Figure D3: Number of Entrants in Event Time



Note: This figure displays the event study coefficients from a version of equation (1) where the dependent variable corresponds to the logarithm of the number of new entrants in a given (LLM × year) cell. The model controls for the share of female workers and non-Italian workers in a given cell. Ninety-five percent confidence intervals are obtained after clustering the standard errors at the LLM level.

D.3 Bargaining vs. Other Channels in Explaining the Wage Returns Following a Conversion into a Permanent Contract

Section 9 highlights the importance of relative differences in bargaining power in driving the wage return following a within-employer conversion from a temporary to a permanent contract. However, other forces such as changes in hours or in the nature of job tasks might be driving the wage return following a $T \rightarrow P$ transition.

While we cannot observe hours in our data, we try to address the importance of the hours channel by focusing exclusively on transitions of full-time workers and found very similar results (see Table G.8). However, other things beside hours might change following the conversion into a permanent position such as the nature of the job tasks. Unfortunately, the INPS-INVIND does not have detailed data on occupations that would have been helpful in this context.

Due to these limitations, we test here for an additional testable implication of the bargaining mechanism highlighted in what is now Section 9. The latter builds upon the idea that when the worker is moved into a permanent position by her employer, her rent-sharing coefficient changes from γ_T to γ_P. A conversion from temporary to permanent therefore maps in a wage increase given by $(\gamma_P - \gamma_T)S_j$, where S_j is the "surplus" of the employer. It follows that a testable implication of this theory is that when we observe conversion in firms that offer no surplus above an employee's outside option, the return of transitioning from temporary to permanent within these "zero-surplus" firms should be equal to zero. If, on the other hand, other aspects such as change of the job task or working hours are the leading factors behind the wage return of being converted into a permanent job, we should expect a positive wage response even among zero-surplus firms.

To test for this idea, we use the sample of all workers who started their career with a temporary job and eventually experience a within-employer conversion to a permanent position, as analyzed in Section 9. We then estimate the following differential rent-sharing model:

$$w_{it} = \alpha_i + \theta Perm_{it} + NS_{j(i,t),t}\gamma_T + (NS_{j(i,t),t} \times Perm_{it})\gamma_P + X_{it}'\phi + r_{it}, \tag{13}$$

where w_{it} is the log daily wage of individual i at time t. $Perm_{it}$ is a dummy equal to 1 if the job of worker i at time t is under a permanent contract; α_i is the worker fixed effect; and X_{it} controls for various observable characteristics as described in Section 9. Finally, $NS_{j(i,t),t}$ captures the net surplus offered by firms. Following Card et al. (2016), we define $NS_{j(i,t),t}$ as the net surplus offered by the employer of worker i in period t; that is,

$$NS_{j(i,t),t} = \max\{0, S_{j(i,t),t} - \tau\} \tag{14}$$

so that τ represents the threshold above which firms start sharing rents with their workers and $S_{j(i,t),t}$ is proxied by value added per worker. Therefore, θ from equation (13) captures the effect of being on a permanent contract among "zero-surplus firms", i.e., firms with value added per worker below τ.

We follow Di Addario et al. (2021) and choose τ so that all firms in the bottom vingtile of the person-year distribution of value added per worker represents zero-surplus firms. Estimates from (13) are displayed in Table D.2. Among these zero-surplus firms, it does not appear that there is a positive, statistically significant return to being on a permanent contract as the point estimate of θ is actually *negative* and around −1%. To evaluate the sensitivity of our results to different choices of τ, we show results where τ is chosen so that now all firms in the bottom decile of the distribution of value added per worker represent zero-surplus firms.

After doubling the set of zero-surplus firms, we still obtain economically and statistically insignificant returns of being on a permanent contract among these firms. Remarkably, even after increasing τ so that zero-surplus firms represent firms in the bottom *quartile* of the log value added per worker distribution, we continue to see that being converted into a permanent contract within these low-rents firms returns a small economic increase of less than 1%, on average. Evidently, most of the wage increase associated with this conversion occurs only among firms with consistently larger surpluses, as shown by our estimates of γ_P displayed in Table D.2.[69] The fact that differences in wages paid to temporary workers and permanent workers are close to zero within lower value added per worker firms can be appreciated also from the raw data displayed in Figure G.9.

An interpretation of these results is that firms with low value added per worker simply pay to all its workers a wage close to the minimum wage established by the corresponding CCNL. As mentioned at the beginning of Section 9, the law requires that these minima are the same for temporary and permanent workers. Hence, when a temporary worker is moved by the employer into a permanent position, we should not expect any change in the wage within these firms. However, firms with higher value added per worker may provide "top-ups" above these wage floors that are directly linked with their underlying productivity and are bargained directly by the firm with unions (Card et al., 2014; Card and Cardoso, 2021). Temporary workers, however, might be excluded from these premia as detailed in Section 9. But once these temporary workers are converted into a permanent position by employers that do provide top-ups, this will result into a wage increase.

[69] We also tried an alternative approach to choose τ that sets it equal to the average value added per worker observed in the hairdressing and fishing CCNL, two particularly low value-added sectors, following an approach also presented in Card et al. (2016). In this case, we also obtained economically and statistically insignificant returns to a conversion into a permanent contract among the resulting zero-surplus firms (see Column 4 of Table D.2).

Table D.2: Return to a Permanent Contract at ``Zero-Surplus" Firms

Outcome: Log Daily Wage	(1)	(2)	(3)	(4)
Permanent Contract	-0.0113***	-0.0010	0.0070***	0.0011
	(0.0033)	(0.0028)	(0.0026)	(0.0028)
Log VA/L	0.0204***	0.0199***	0.0204***	0.0197***
	(0.0021)	(0.0022)	(0.0023)	(0.0022)
Permanent Contract x Log VA/L	0.0435***	0.0458***	0.0469***	0.0463***
	(0.0025)	(0.0026)	(0.0027)	(0.0026)
Zero-Surplus Firms	Bottom 5%	Bottom 10%	Bottom 25%	Fishing/Hair Dressing
# of Person-Year Observations	5,668,325	5,668,325	5,668,325	5,668,325

Note: This table reports the coefficients from equation (13). This equation is estimated on the sample used in Section 9, i.e. the set of workers in INPS-INVIND that start their career 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. Different columns correspond to different choices of τ from equation (13) and thus different definitions of firms that correspond to ``zero-surplus firms", i.e. firms that offer no surplus above an employee's outside option. In Column (1), we define these firms as those with value added per worker belonging to the bottom vingtile of the corresponding person-year distribution of value-added per worker observed in INPS-INVIND. Similarly, in Columns (2) and (3), where we consider zero surplus firms as those belonging to the bottom decile or quartile of the distribution of log value-added per worker, respectively. From equation (13), the coefficient on the permanent contract dummy reported in the first row captures the returns of being converted to a permanent contract among zero-surplus firms. Log VA/L stands for Log value added per worker. The reported estimates control for worker fixed effects, a quadratic term in potential experience, year effects interacted with one-digit sector codes, gender, age at entry, and Italian nationality. Standard errors clustered at the LLM level.

D.4 The Role of Bargaining Power Differences

In Section 9.2 we quantify the potential role of the estimated differential bargaining power between temporary and permanent workers in explaining the reform effects on firms' labor costs and incumbent temporary workers' earnings. To do this, we perform some back-of-the-envelope calculations that can be motivated by the following simple model. Suppose all workers have identical productivity, denoted by surplus (or value added per worker in our data) S. As is standard in labor models with Nash bargaining, we assume that wages are given by a constant, b, and a rent-sharing coefficient, α_c, which may be interpreted as the bargaining power of workers with contract $c \in \{T, P\}$. Thus, worker earnings (and labor costs) for each worker with contract c are

$$y_c = b + \alpha_c S. \tag{15}$$

Labor Costs. Letting $\delta \in [0, 1]$ denote the share of temporary workers at a firm, the corresponding total costs per worker is given by

$$C = b + S(\alpha_P + \delta(\alpha_T - \alpha_P)). \tag{16}$$

Assuming that the reform facilitated the hiring of temporary workers without affecting the surplus or the relative bargaining power of temporary and permanent workers, the percent change in total labor costs per worker is thus given by

$$\begin{aligned} \frac{\Delta C}{C} &= \Delta\delta\,(\alpha_T - \alpha_P)\,\frac{S}{C} \\ &= \Delta\delta\,(\pi - 1)\,\alpha_P \frac{S}{C}, \end{aligned} \tag{17}$$

where π is the relative bargaining power of temporary workers (α_T/α_P). From Table 2, we know that the reform increased the temporary share of workers by approximately 4.9 percentage points in the medium run. The differential rent-sharing analysis of Section 9 suggests $\pi = 0.67$. From Table 2, we find that the labor share $\frac{C_{PRE}}{S}$ in the pre-reform period is around 0.56. Assuming that permanent workers have a bargaining power of around 0.5, as is standard in the literature (e.g., Pissarides, 2009; Gertler and Trigari, 2009), we obtain that differences in bargaining power alone can account for 52% of the overall reduction in labor costs reported in Table 2.

Worker Earnings. Regarding the effect on incumbent temporary workers, next period earnings y' can be written as

$$\begin{aligned} y' &= P^{same}_{T\to P} y_P + \left(P^{same} - P^{same}_{T\to P}\right) y_T + \left(1 - P^{same}\right) \underline{y} \\ &= P^{same}_{T\to P} \left(\alpha_P - \alpha_T\right) S + P^{same} \left(y_T - \underline{y}\right) + \underline{y}, \end{aligned}$$

where P^{same} represents the probability that the worker remains with the same employer, $P^{same}_{T\to P}$ represents the probability that her position is converted into a permanent position by her current employer, and $\underline{y}$ represents the next period's expected pay if she separates from the current employer, an event that occurs with probability $1 - P^{same}$.[70]

Assuming that the reform only affects the transition probabilities, the percent change in expected earnings can be decomposed as follows:

$$\frac{\Delta y}{y_T} = \underbrace{\Delta P^{same}_{T \to P} \left(1 - \pi\right) \alpha_P \frac{S}{y_T}}_{\text{Bargaining Effect}} + \underbrace{\Delta P^{same} \frac{y_T - \underline{y}}{y_T}}_{\text{Reallocation Effect}}, \tag{18}$$

where $\Delta P^{same}_{T \to P}$ represents how the reform impacted the probability to observe temporary to permanent transitions. Similarly, $1 - \Delta P^{same}$ represents how the reform impacted the separation probability. The bargaining effect captures changes in pay due to the reform preventing the worker from obtaining a permanent job with her current employer and thus preventing her from enjoying a higher share of the surplus. The last term captures the reallocation effects induced by the reform.

From Table 4, we know that incumbent temporary workers' pay decreases by around 7.0% in the medium run after the reform is passed. The latter also decreased the probability to observe a within-firm temporary to permanent transition by 8.7 percentage points. From Section 9, we estimate a relative bargaining power $\pi = 0.67$. Once again, we assume $\alpha_P = 0.5$ (e.g., Pissarides, 2009; Gertler and Trigari, 2009). Using the information in Table 4, we estimate the ratio of surplus to labor cost for an incumbent temporary worker to be 3.78. Combining this information, we thus obtain that differences in bargaining power between temporary and permanent workers explain around 77% of the loss in pay of incumbent temporary workers.[71] The remaining 23% is thus explained by negative reallocation effects.

[70] Thus, the term $\underline{y}$ depends on the probability to find a new job and the associated generated surplus generated by such hypothetical new job.

[71] Another interpretation of y_T in this simplified model is that it is equal to the labor cost of a temporary worker. Using evidence from all the firms associated with incumbent temporary workers, we estimate that the median firm has labor costs per worker that are 44% higher than earnings per worker. Under this alternative interpretation, differences in bargaining power would explain around 54% of the reform-induced earnings reductions of incumbent temporary workers.