The Extension of Collective Agreements in Fissured Workplaces: Wage Premiums, Spillovers and Rent-Sharing*

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Abstract

We study a 1999 Spanish reform that extended collective bargaining coverage by requiring temporary agency workers to be paid according to user-firm agreements. Using administrative data and quasi-experimental variation, we estimate a coverage premium: agency wages rose by about 15%, with additional spillovers to in-house workers. Agency employment declined, but this was more than offset by increases in in-house and permanent jobs, yielding positive total employment effects. Agencies passed all labor-cost increases to user firms. The results suggest that the reform reallocated rents toward workers and reduced monopsonistic distortions, indicating efficiency gains alongside redistribution.

Keywords: Outsourcing, collective bargaining, wage spillovers, rent-sharing
JEL Classification: J31, J38, J42, J52, J64

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1 Introduction

Over recent decades, firms have increasingly fragmented production and employment across legally distinct entities, giving rise to what Weil (2014) termed the fissured workplace. In these arrangements—ranging from outsourcing and subcontracting to platform work—workers perform tasks for a firm without being its direct employees, and thus often fall outside the firm's collective bargaining coverage. While such fragmentation can raise efficiency through specialization and flexibility (Autor, 2001; Bilal and Lhuillier, 2021), extensive evidence of wage penalties for outsourced workers (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2023) suggests that it might stem from increased monopsony power rather than productivity gains. We study how extending collective bargaining coverage to outsourced workers affects this redistribution–efficiency trade-off, focusing on wages, outsourcing, employment, and rent-sharing among workers, agencies, and firms.

We study a 1999 Spanish reform that extended collective bargaining agreements to temporary agency workers. The analysis combines two main administrative sources: a longitudinal dataset covering over one million workers with detailed information on wages and employment spells, and administrative records on sectoral and firm-level collective agreements.[1] The reform created quasi-exogenous variation that identifies the causal impact of making stronger collective bargaining coverage binding for a previously weakly covered segment: the agency workers whose pay had been set in a relatively new and difficult-to-organize, high-turnover sector with limited bargaining capacity.[2] Reflecting this weaker bargaining position and fragmented organization, the data on negotiated wage floors shows that temporary agency workers faced a 16.7% contract-wage penalty prior to the reform.

We begin our analysis by examining the direct wage effects of the reform on temporary agency workers. Using a difference-in-differences framework where in-house workers serve as the control group, we estimate that the reform increased agency wages by about 14.8-15.5%. These estimates indicate that extending collective bargaining coverage—by tying agency pay to the firm- or sector-level agreements in force in user firms—effectively eliminated the wage gap between agency and in-house employees, consistent with the existence of a sizable coverage premium for previously fissured workers. Additionally, many temp agency workers earned wages well above the minimum wage, indicating that the pay increases happened across the wage distribution.

We next examine whether extending collective bargaining coverage to agency workers

[1] We also draw on two complementary administrative sources: firm-level balance sheet data from the Bank of Spain and sector-level aggregates describing the deployment of temporary agency workers across industries. See Section 2 for more details.

[2] Temp agencies were legalized in 1994.

affected the wages of already covered in-house employees. In this context, spillovers capture how changes in bargaining coverage for one group feed back onto others through shifts in wage norms and outside options within the same wage-setting system. Theoretically, the direction of these effects is ambiguous: the reform could strengthen in-house workers' bargaining position by removing the threat of replacement with lower-paid agency labor (Green et al., 2024), or it could weaken it by narrowing the insider–outsider gap and reducing the bargaining advantage of in-house workers (Bentolila and Dolado, 1994).[3]

To test these predictions, we exploit differences across provinces—close approximations to local labor markets—in their pre-reform incidence of agency employment. Our identification strategy relies on the assumption that provinces with higher initial reliance on temporary agency work were more exposed to the extension of coverage. The results show that in-house wages grew significantly faster in these high-incidence provinces: a one–percentage point higher pre-reform share of agency employment is associated with a 0.6% larger post-reform increase in composition-adjusted wages. The pattern indicates that, prior to equal pay, the availability of cheaper agency labor limited in-house workers' bargaining power, while the reform's extension of coverage closed this channel and boosted their wage growth.

Together, the direct and spillover wage effects we estimate capture the causal impact on wages of making stronger collective bargaining coverage binding, a coverage premium rather than a union membership premium. This distinction is central to understanding wage setting in coordinated systems like Spain's, where the reach of collective agreements extends well beyond unionized workers. While the traditional union premium measures how individual union membership affects pay (Barth et al., 2020; Farber et al., 2021), the coverage premium reflects how extending existing agreements to previously uncovered workers, such as agency employees, alters wages across the workforce. In this sense, the estimates are informative for a broad set of OECD countries, where sectoral or national bargaining systems with legal or administrative extensions remain the predominant form of wage setting (Bhuller et al., 2022; Jäger et al., 2024).

We then turn to the effects of the reform on employment. As in the case of wages, economic theory delivers ambiguous predictions. Extending collective bargaining coverage could reduce efficiency by hampering the intermediation role of agencies or by raising labor costs that discourage hiring. Conversely, employment might rise if temporary agency work had been used to circumvent collective agreements that mitigated monopsonistic wage sup-

[3]Note that spillover effects do not bias the direct wage estimates. The reform mandated that temporary agency workers receive the wages specified in the collective agreements of user firms. Therefore, any upward adjustment in in-house wage floors triggered by the reform would mechanically pass through to temporary agency workers. Our estimate of the direct effect thus already incorporates any such contractual spillovers, and the separate spillover analysis isolates additional wage gains among in-house workers.

pression. Consistent with the latter efficiency-enhancing mechanism, we show that before the reform, agency work was more prevalent in sectors with higher labor-market concentration, where firms had greater monopsony power but also higher collectively agreed wages, suggesting that collective bargaining partly offset wage markdowns. The use of temporary agencies thus allowed firms to avoid binding wage floors and restore unexploited monopsony power by channeling employment through intermediaries with lower pay.

Using differences across provinces in their pre-reform incidence of agency employment, we find that the reform led to a sharp decline in agency jobs but an even larger expansion of in-house employment. Overall employment effects appear positive, although the estimates are not statistically precise. These results indicate that the temporary agency sector was inefficiently large before the reform, sustained by monopsonistic wage suppression rather than productive advantages of intermediation. By closing the pay gap, higher wages reduced these distortions and drew additional labor supply into in-house employment. Supporting this interpretation, we find that internal migration toward provinces with a high pre-reform incidence of agency work expanded local employment capacity, allowing in-house jobs to grow beyond the displacement of agency work. Improved job security likely reinforced this adjustment: the reform reduced turnover, lengthened job durations, and raised the share of permanent contracts, increasing the attractiveness of in-house employment and easing recruitment frictions in provinces most exposed to the reform.

The employment response is consistent with a strong pass-through of labor costs from agencies to user firms. To document this mechanism, we reconstruct national time series of gross fees using administrative data on agencies' revenues, and the number of days worked by temp agency employees, since direct price information is unavailable. The results show a sharp post-reform increase in gross fees and a complete pass-through of the higher labor costs to user firms. By raising the effective cost of agency intermediation, the extension of bargaining coverage eroded the cost advantage that had sustained part of fissured employment.

To interpret these price adjustments in terms of rent allocation, we extend the standard two-party rent-sharing framework to a triangular setting in which workers, agencies, and user firms jointly divide the surplus from each match. Using the model's equilibrium conditions, we estimate the total rent generated by agency employment and its division across the three parties. Before the reform, workers captured 59.6% of this rent, with the remaining surplus split between agencies (5.6%) and user firms (34.8%). The equal-pay mandate raised the worker's share to 67.4%, compressing the portion accruing to user firms (27%) and leaving the share of surplus going to surviving temp agency matches unchanged, without changing the overall surplus. This evidence shows how extending bargaining coverage reshapes rent distribution within multi-party employment relationships and links the rent-sharing literature (Card et al., 2018; Kline et al., 2019; Lamadon et al., 2022) to theories of organizational boundaries and de-fissuring (Bilal and Lhuillier, 2021), where narrowing the agency–firm margin induces firms to internalize employment.

The first contribution of the paper is to provide, to our knowledge, the first causal evidence on coverage premia—the wage effects of making stronger collective bargaining coverage binding for workers previously covered by weaker agreements. Our quasi-experimental estimates reveal a coverage premium roughly twice as large as the one reported for Spain by Jäger et al. (2024), whose cross-sectional approach is more exposed to selection on unobservables. This difference underscores the importance of causal identification for assessing the wage impact of coverage extensions. Moreover, the empirical strategy we develop offers a blueprint for analyzing similar reforms in other institutional settings.

The results also speak to the broader literature on how labor market institutions shape wage setting and efficiency (Jäger et al., 2020; Farber et al., 2021; Dustmann et al., 2021). They underscore that collective bargaining coverage can not only redistribute rents but also foster more efficient labor-market equilibria by reducing monopsonistic distortions and coordinating wages across organizational boundaries (Azkarate-Askasua and Zerecero, 2025). In this sense, extending or restoring shared bargaining coverage among legally separate employers can narrow pay gaps, promote the internalization of workers, and preserve the productive flexibility of multi-firm structures. This coordination-based approach contrasts with outright outsourcing bans (Estefan et al., 2024).[4] Importantly, because coverage extensions operate well above the minimum wage, their effects reach workers across the wage distribution rather than primarily those at the bottom, positioning them as a distinct policy lever alongside minimum wages in reshaping labor-market equity and efficiency (Derenoncourt and Montialoux, 2020).[5]

Finally, the paper contributes to the rent-sharing literature (Card et al., 2013, 2018; Kline et al., 2019; Lamadon et al., 2022) by providing new empirical evidence on rent-sharing within a triangular employment structure involving workers, intermediary agencies, and user firms. The analysis extends standard two-party frameworks to settings with labor intermediation, where regulation affects both wage setting and the allocation of surplus across contracting parties. By linking these rent adjustments to changes in organizational boundaries, the

[4] Estefan et al. (2024) analyze Mexico's 2021 reform, which banned domestic outsourcing.

[5] Another related paper is Martins (2021), who uses sector-level administrative aggregates to study the effects of extensions of collective agreements on employment and wage bills. In contrast, our paper exploits worker-level and firm-level microdata, which allows us to measure individual wage adjustments, identify spillovers across the wage distribution, capture employment effects, and quantify the underlying rent-sharing and fee mechanisms that cannot be recovered from aggregate data.

paper connects the rent-sharing literature to research on outsourcing and de-fissuring (Dube and Kaplan, 2010; Bilal and Lhuillier, 2021), offering a novel empirical bridge between wage regulation, surplus division, and firms' make-or-buy decisions.

The rest of the paper is structured as follows. In Section 2 we present the data. Section 3 describes the institutional context and the reform. Section 4 introduces theoretical predictions to guide the empirical analysis. In Sections 5 and 6 we present the empirical strategies and the results. Section 7 explains the additional results and the robustness analyses. Finally, in Section 8 we detail the conclusions.

2 Data

We combine four different datasets to explore the consequences of the 1999 pay equalization policy. First, we use data from the Continuous Sample of Working Histories (Muestra Continua de Vidas Laborales, MCVL). It is an administrative dataset that combines information from social security, the tax administration and the population register. Very importantly for us, it has information on whether workers had been hired by temp agencies or directly by their employers. It also has detailed information on the start and end of each employment and unemployment spell, monthly wages (top-coded), the size of the firm, whether the contract was open-ended or short-term, the location of the job, the workers' skill group or occupation in the firm, and the employees' gender, date of birth and citizenship, among other variables.

The original sample was constructed in the following way: in 2004, 4% of all individuals who were either formally employed, received some kind of unemployment insurance (UI) or unemployment assistance (UA), or perceived a contributory pension that year were selected. In absolute terms, over one million individuals who in 2004 were in any of the three situations mentioned were included in the dataset. Sampling was random, without any kind of stratification. The data contains the labor history of each individual since he started working, including periods when the worker was collecting UI or UA, or after he retired and started receiving pension benefits. New editions of the dataset are published every year and contain all the workers sampled in 2004 unless they stopped having a relationship with social security (the employee is out of employment, does not collect UI or dies). In that case, the worker is replaced by another randomly selected individual that had some relationship with social security that year. Similarly, the whole labor life of that new worker is included in the dataset.

To construct the analysis sample, we take individuals included in the 2004 to 2008 MCVL editions, and reconstruct their labor market histories between 1995 and 2003.[6] For each of these years, we include all workers 16 to 65 years old. In total, the sample comprises around 1 million individuals, which we use to examine the effects of the 1999 reform on their labor market outcomes.

We obtain wage floor data from the Census of Collective Bargaining Agreements -*Registro de Convenios y Acuerdos Colectivos*-. It includes the universe of Collective Bargaining Agreements (CBAs) signed between 1990 and 2001, which had to be registered at the Ministry of Labor using a pre-specified form to obtain legal validity. Wage floors were not a compulsory entry, but they are reported for around 65% of collective contracts. The form contains 10 wage floors, one for each skill group included in the Spanish social security system. Among many other entries, we have information of the type of agreement (sector or firm) and the time period.

The third main source is administrative information on the number of temporary agency contracts and workers, aggregated by the sector of the user firm. This source is quite unique, as administrative datasets rarely include information on the sectors where agency workers are actually deployed.[7] Because temp agencies are the formal employers, such deployment information is typically unavailable. We use the records from the Ministry of Labor to characterize the sectors that relied more heavily on agency work and to measure their exposure to the policy reform.[8] The data also report the number of active temp agencies in Spain.

The fourth dataset contains information on non-financial corporations since 1995.[9] There are around 600,000 firms per year and the data comes from the mandatory deposit of annual accounts in the mercantile register. We use this data to analyze the impact of the policy on the fees that temp agencies charge user firms, and to understand rent-sharing dynamics.[10]

3 The Temporary Agency Sector: Wage Penalties, Market Power, and Reform

A central question in understanding the use of temporary agency work is whether its expansion reflects efficiency gains in job matching or a strategy to circumvent collectively bar-

[6]In Appendix B we show that the retrospective design of the sample we use in the empirical exercises is not a concern for the analysis. We compare the MCVL with data from the Spanish Labor Force Survey (SLFS), which has a representative sample of the Spanish labor market every quarter, and show that both datasets capture analogous labor market dynamics a few years before and after the 1999 reform.

[7]This is also the case for the MCVL dataset.

[8]The data series are known as *Empresas de trabajo temporal*. Link. Last accessed 10 December 2025.

[9]The dataset is the *Central de Balances Integrada* (BELab, 2025).

[10]Also, in additional results we use this data to investigate effects on profits, value added, net sales, net purchases, and labor costs, among other variables. See Section E.4 and E.5 for more details.

gained wage floors in concentrated labor markets. This section describes the institutional background of the temporary agency sector and the 1999 reform, and presents evidence consistent with a cost-saving motive and with greater reliance on agency work in sectors where employers hold market power. The descriptive analysis in this section combines data from collective agreements with sector-level records on the deployment of temporary agency workers.

While temp agencies can improve labor market efficiency by screening workers and increasing firms' flexibility (Autor, 2001; Houseman, 2001), they may also serve as a channel to reduce wages and weaken bargaining outcomes (Autor, 2003; Dube and Kaplan, 2010). Because agency workers are formally employed by intermediaries rather than user firms, their employment conditions need not comply with the collective agreements covering the firms where they work.[11] This institutional separation creates incentives for employers to shift employment away from direct contracts to avoid the wage premia embedded in collective bargaining.

At the time of the reform, the incentives to deviate from collective agreements were strong. Collective bargaining was quite centralized, with two main tiers of negotiation. The first tier, usually at the province-sector or national-sector level, set wage floors for each occupation across a sector. The second tier corresponds to firm or establishment negotiations and follows the favourability principle, which implies that these agreements can only increase wages relative to the first-tier agreements (Jäger et al., 2024). According to OECD data for the year 2000, 84.8% of Spanish workers were covered by a collective agreement.[12]

The Temp Agency Wage Penalty: The temp agency sector was legalized in 1994 and was considered a new industry. Hence, it was not covered by the collective agreements in other industries. Instead, the temp agency sector signed its own collective agreement in 1995, renewed in 1997, with national reach. This agreement could set wages for temp agency workers in any geography and sector. We measure a contract wage penalty of 16.7%, an estimate similar to the outsourcing wage penalty (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2023).

To document this, we use data on wage floors from collective bargaining agreements, both at the sector and firm level. For each occupation, sector, and province, we assign the wage floors set in the national temp agency agreement as if they applied to all sectors where a temp worker could be deployed. Concretely, we duplicate the wage floors in the temp

[11] Similar practices exist in the outsourcing sector. Also in the gig economy, where the use of independent contractors reduces workers' fringe benefits (Weil, 2014; Mas and Pallais, 2020).

[12] Today, coverage in Spain is 80.1%. In other European countries like France, Germany or Sweden, among others, is also high: 98%, 54% and 88%, respectively. In contrast, it is only 26.9% in the UK and 12.1% in the USA.

agency agreement across all other agreements in our data. This allows us to construct a complete set of temp agency wage floors that can be directly compared to the sectoral and firm-specific wage floors.[13] The specification is:

$$y_{ospt} = \alpha + \delta_o + \delta_s + \delta_p + \delta_t + \rho X_{ospt} + \beta AgencyFloor_{ospt} + \epsilon_{ospt} \quad (1)$$

where y_{ospt} is the logarithm of the wage floor for occupation o in sector s, province p, and year t. δ_o, δ_s, δ_p and δ_t are occupation, sector, province and year fixed effects, respectively X_{ospt} are province-sector-occupation-year averages of firm size, short-term contracts, gender, experience, education, age and share of immigrants, obtained from the MCVL. $AgencyFloor_{ospt}$ is a dummy for temp agency floors. β measures the average wage penalty. We calculate it both for firm and sector agreements.

The results are reported in Table 1. We present estimates for all agreements, for sectoral agreements, and for firm-level agreements. Each observation is weighted by the number of workers covered in the corresponding deployment sector or firm. On average, the average penalty is -16.7% (Column 1). The penalty is smaller in sector agreements (-12.3%, Column 2) than in firm agreements (-50.8%, Column 3). Since sectoral agreements cover a substantially larger share of workers, the overall estimate for all agreements is dominated by the sectoral comparison and therefore lies closer to the sector-level penalty.

Table 1: Wage Floor Gap

	(1) All Agreements	(2) Sector Agreements	(3) Firm Agreements
Agency Floor	-0.167*	-0.123	-0.508***
	(0.0912)	(0.100)	(0.0323)
Observations	261902	71030	190872

Notes: The table shows the temp agency wage penalty. The specification is Equation 1. Each observation is weighted by the number of workers covered in the corresponding deployment sector or firm. All columns include fixed effects for province, year, sector, and occupation. All regressions control for province-sector-occupation-year averages of firm size, short-term contracts, gender, experience, education, age and share of immigrants. Robust standard errors clustered at the province level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL and Census of Collective Bargaining Agreements.

Market Power Channel: Now, we analyze the association between temp agencies and labor market concentration. We draw two main remarks. First, the incidence of temp agency workers is higher in more concentrated labor markets. Second, more monopsonistic sectors also had cost-saving incentives to do so. The specification we follow to reach these

[13] Worker-level data such as the MCVL cannot be used for this exercise, since we do not observe the actual firm or sector of deployment for temp agency workers, making it impossible to define the relevant counterfactual wage.

conclusions is:

$$y_{spt} = \alpha + \beta X_{spt} + \beta \log HHI_{spt} + \delta_{FE} + \epsilon_{spt} \tag{2}$$

The main exogenous variable is the log of the Herfindahl-Hirschman Index (HHI) of employment, which we use as a proxy for labor market concentration, calculated at the province and sector level for each year. We use several endogenous variables: the % of temp agency workers in each sector between 1995-1998, wage floors established in collective bargaining agreements in 1994, and the temporary agency wage penalty in 1994.[14] δ_{FE} denotes the collection of fixed effects included in the model. All regressions have province and year fixed effects, and some have sector and occupation fixed effects, which we specify in each column of Table 2.[15] X_{st} is a vector of province-sector averages of worker characteristics (education, age, % female, % short-term contracts, % immigrants and % managers) and firm size.

Table 2 shows that province-sectors with more concentrated labor markets had a higher exposure to temp agency work. Columns 1 and 2 focus on the relationship between concentration and the incidence of temp agency employment. In Column 1, the HHI is measured in 1993, before the legalization of the temp agency sector. The results show that more concentrated industries were more likely to adopt agency work once it became available: a one standard deviation increase in HHI is associated with a 17.89% higher share of temp agency employment.[16] Column 2 uses HHI measured contemporaneously for 1995–1998. Exploiting time variation in the HHI allows us to include sector fixed effects. The positive relationship persists, confirming that industries with greater market power relied more heavily on temporary agency workers.

Columns 3–8 shift the analysis to wage outcomes. Column 3 shows that more concentrated province-sectors are associated with higher negotiated wage floors: a one–standard-deviation increase in HHI is linked to a 5% increase in contract wages, with the effects driven by firm agreements (Column 5). This suggests that collective bargaining was able to counteract part of the downward pressure on wages that would otherwise arise under monopsony power (Azkarate-Askasua and Zerecero, 2025). Columns 6–8 examine the relationship between concentration and the temp-agency wage penalty. The coefficients are negative, as

[14] Sector-level aggregate data on the share of temporary agency workers are available starting in 1995; we therefore use the period 1995–1998, ending in the year immediately preceding the reform. For the other two variables, which are further disaggregated at the occupation level, we use 1994—the year of the liberalization of temporary agencies—to minimize the influence of this sector on wage floors and wage setting. Using time-varying measures over 1994–1998 yields qualitatively similar results, although coefficient magnitudes are smaller; these results are available upon request. For the share of temporary agency workers, we exploit the full pre-reform period to capture the rapid expansion of the sector prior to the reform.

[15] The occupation variable is based on the 11 social security contribution groups, which approximate workers' occupational categories.

[16] The average HHI in 1993 is 3774, with a standard deviation of 3317

expected— a one–standard-deviation increase in HHI is associated with a 22% larger penalty (Column 6). Although the estimates are generally imprecise, this is consistent with sectoral heterogeneity: collective bargaining may offset monopsony power in some concentrated industries but not in others. Taken together, the results align with the market-power channel: more concentrated sectors had stronger cost-saving incentives to adopt agency work and may have been better positioned to reorganize employment in this way because workers' outside options were weaker.

Table 2: Which Sectors Use Temp Agency Work? The Market-Power Channel

	(1) % Temp Agency Employment	(2) % Temp Agency Employment	(3) Log Wage Floors All Agreements	(4) Log Wage Floors Sector Agreements	(5) Log Wage Floors Firm Agreements	(6) Agency Penalty All Agreements	(7) Agency Penalty Sector Agreements	(8) Agency Penalty Firm Agreements
Log HHI	0.284*	0.0608*	0.0772	0.0173	0.145	-0.0834*	-0.0309	-0.161
	(0.158)	(0.0328)	(0.0462)	(0.0356)	(0.114)	(0.0468)	(0.0332)	(0.112)
Observations	6604	6902	8927	3694	5233	8927	3694	5233
Industry FE	N	Y	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Occupation FE	N	N	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y

Notes: The table shows the correlation of the log of the Herfindahl-Hirschman Index of labor market concentration and the % of temp agency workers in each industry between 1995-1998 (Columns 1 and 2), the log of wage floors in 1994 (Column 3 for all agreements, Column 4 for sector agreements and Column 5 for firm agreements) and the wage floor penalty from being employed by an agency, relative to in-house employment in 1994 (Column 6 for all agreements, Column 7 for sector agreements and Column 8 for firm agreements). In Column 1 and 3 to 8, the HHI is calculated for 1993. In column 2, we use the HHI for 1995, 1996, 1997 and 1998. The specification is Equation 2. The controls are province-industry averages of: education level, share of immigrants, share of short-term contracts, firm size, share of women, share of managers and age. The regression in Columns 1 and 2 are weighted by the number of workers in each industry. The weights in Columns 3 to 8 are the number of workers covered by each collective agreement. Robust standard errors clustered at the industry level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL and Census of Collective Bargaining Agreements and the data series *Empresas de trabajo temporal.*

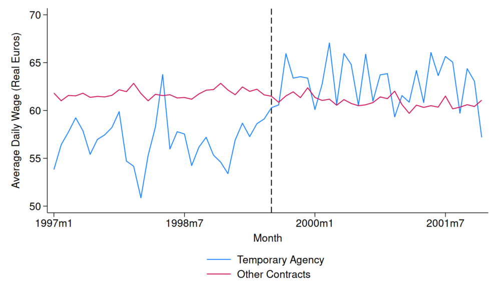
The Reform: The reform we study was passed in 1999 in Spain to prevent the circumvention of collective agreements by firms. It followed the principle of equal treatment between workers and stated that the wages of temp agency workers had to be paid according to the collective agreement of the user firm. Hence, the reform limits the potential use of temp agency employment to put downward pressure on wages, while preserving the potentially positive effects in terms of job matching efficiency.[17]

Figure 1 shows the raw evidence of the effects of the reform. On the one hand, Panel (a) shows the wage evolution of temp agency and in-house workers before and after the reform. As it can be seen, the wages of temp agency employees increased sharply, after the policy change, by approximately 11%. On the other hand, Panel (b) displays the evolution of temp agency employment. It was increasing very fast before the reform. In the 18 months before

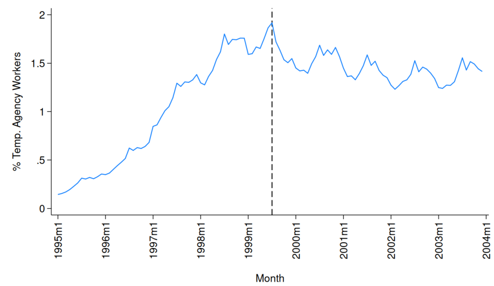
[17]Appendix C highlights that, unlike similar laws regulating temporary agency work, outsourcing, or the gig economy in other contexts, this reform provided no exemptions, thereby ensuring broad coverage of the collective bargaining system. A different but related policy is analyzed by Derenoncourt and Montialoux (2020), who study the 1967 U.S. minimum wage extension to previously uncovered sectors.

the law change, it doubled in size. After it, it started to decrease, suggesting the policy might have had negative employment effects unless some of the temp agency employees became in-house workers. Moreover, Panels (c) and (d) show the evolution of temp agency contracts and the number of temp agencies, respectively, and confirm that the temp agency sector was booming before the reform, and that it declined after it.

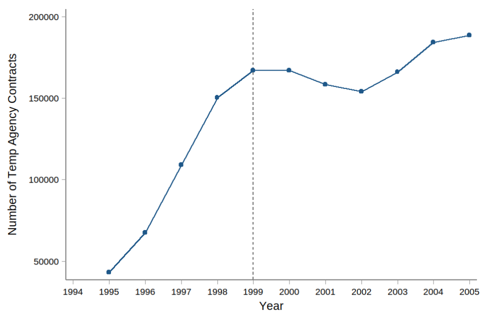
Figure 1: **Raw Evidence of the Reform**



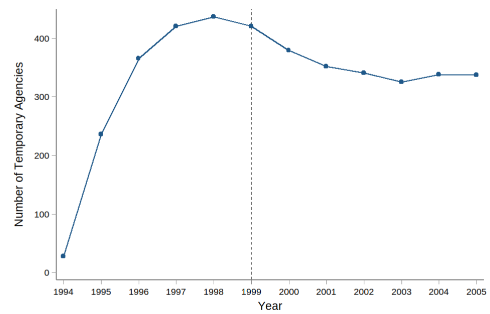
(a) Average Real Wage



(b) Temp Agency Workers



(c) Temp Agency Contracts



(d) Number of Temp Agencies

Notes: The panels show the evolution of real daily wages for temporary agency workers and employees in other contracts (a), of temporary agency employment (b), the number of temp agency contracts (c), and of the number of temp agencies (d). Sources: MCVL and the data series of *Empresas de trabajo temporal*.

Finally, the policy change introduced additional modifications. First, every temp contract had to be shared with worker representatives, which probably facilitated law enforcement. Second, it established that the agency should have at least 12 workers for very 1000 temp contracts, and that it should invest in worker training by an amount equivalent to 1% of the salary mass. Third, temp agencies were responsible for the work safety trainings of their workers. These changes increased operating costs for temp agencies but likely improved the quality of the services they provided. In addition, temporary agency workers hired under short-term contracts became entitled to severance pay equal to 12 days of salary per year worked upon contract termination.[18]

[18]Note that the severance compensation is paid upon contract termination and does not depend on the

Summary Statistics: Next, we present descriptive statistics for the social security dataset that we employ in the empirical analysis. Table 3 displays means and standard deviations for workers in temp agency, in-house short-term, and in-house open-ended contracts. We show both pre- and post-reform descriptive statistics. In addition to the wage increase caused by the reform, the table also shows other interesting facts: job duration is much larger for in-house workers, even for short-term ones. Moreover, temp agency workers are more likely to have been unemployed recently and to have low-rank jobs (laborer layer).[19]

Table 3: Summary Statistics

	Pre-Period Temp. Agency Workers		Post-Period Temp. Agency Workers		Pre-Period Short-Term In-House		Post-Period Short-Term In-House		Pre-Period Permanent In-House		Post-Period Permanent In-House	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
% Male	59.153	49.155	56.766	49.540	63.333	48.190	62.431	48.430	66.698	47.130	64.256	47.925
Age	27.894	7.600	26.673	7.559	31.912	9.955	31.092	10.111	40.683	10.836	38.897	11.011
% Months of Experience	22.373	25.589	15.162	24.201	36.844	31.069	28.858	31.498	78.831	28.584	67.022	36.593
% University Education	48.243	49.970	48.636	49.982	34.339	47.484	34.637	47.581	39.276	48.836	41.900	49.340
% High Secondary Educ.	38.890	48.751	38.292	48.610	42.872	49.489	42.966	49.503	34.948	47.681	35.461	47.839
% Low Secondary Educ.	12.415	32.976	12.606	33.192	21.969	41.404	21.564	41.127	24.572	43.051	21.645	41.182
% Spanish Citizen	99.072	9.588	97.158	16.617	98.568	11.881	97.397	15.924	99.048	9.711	98.549	11.956
Real Daily Wage	52.729	45.584	58.994	44.070	45.806	26.889	46.460	25.756	66.706	32.260	65.040	31.552
Job Duration	117.758	208.099	120.807	189.341	590.865	899.456	562.086	887.546	4392.657	3349.945	3932.469	3173.580
% Part-Time	19.772	39.828	13.234	33.887	19.871	39.903	16.323	36.958	3.202	17.604	6.065	23.869
% Unemployed Before	20.404	40.300	19.022	39.248	8.083	27.257	7.785	26.793	0.578	7.583	0.848	9.169
% Managerial Layer	2.544	15.745	1.016	10.029	7.235	25.906	7.402	26.180	18.455	38.793	18.729	39.014
% Laborer Layer	97.456	15.745	98.984	10.029	92.765	25.906	92.598	26.180	81.545	38.793	81.271	39.014
Firm Size	341.017	745.235	431.074	842.980	107.645	789.959	130.163	812.525	294.467	1416.103	324.127	1428.718
Observations	60465		69387		1320627		1538462		2326196		2754997	

Notes: The table shows summary statistics for temporary agency, in-house short-term and in-house open-ended workers. For each of them, we report sample means, standard deviations and number of observations for 1998 (Pre-Period) and 2000 (Post-Period). Sources: MCVL.

4 The Theoretical Framework

This section examines how extending collective agreements to temporary agency workers can reshape labor-market outcomes—wages, employment, job security, fees, rent-sharing, and productivity—within a tripartite structure linking workers, agencies, and user firms. The reform can affect both efficiency and distribution. Redistribution arises when surplus shifts among workers, agencies, and firms (Dube and Kaplan, 2010). Efficiency gains occur when the reform mitigates monopsony power in wage setting, while potential inefficiencies may arise if it worsens the information frictions that agencies help resolve through screening and rapid matching (Autor, 2001). In this sense, temporary agency work embodies both efficiency-enhancing and distortionary motives: agencies facilitate hiring and reduce adjustment costs, but their use can also allow firms to bypass collectively bargained wage floors

worker being dismissed; it applies to the expiration of the fixed-term contract itself.

[19] Appendix D reports descriptive statistics for the other datasets used in the paper.

and extract monopsonistic rents. The reform directly targets the latter mechanism, while potentially preserving the productive functions of intermediation. Our theoretical predictions build on the literature on collective bargaining and fissured workplaces and are formalized in a directed-search model in Appendix A, which traces how equal-pay mandates propagate through multi-party bargaining.

Agency wages: The reform directly eliminates the wage gap between agency and in-house workers by imposing equal pay. This effect is redistributive because it transfers rents from firms and agencies to workers. Moreover, it also improves allocative efficiency by aligning pay more closely with productivity, in particular if low wages reflected monopsonistic markdowns.

In-house wages. Equal pay reduces firms' ability to replace in-house workers with cheaper agency labor, altering their outside options (Green et al., 2024). As hiring through agencies becomes less attractive, firms' bargaining position weakens, which can raise in-house wages—either through a redistributive effect, as rents shift from firms to workers, or through an efficiency effect, by correcting monopsonistic underpayment. At the same time, the reform may also narrow the insider–outsider gap by reducing firms' ability to substitute in-house employees with cheaper agency labor, thereby weakening insiders' relative bargaining power (Bentolila and Dolado, 1994). The overall effect on in-house wages is therefore ambiguous and depends on which of these forces dominates.

Fees. Higher agency wages raise total employment costs, which agencies may partly pass on to user firms through higher fees. The degree of pass-through depends on competition among agencies and on how easily firms can substitute agency with in-house labor. When pass-through is incomplete, agencies' margins shrink, implying a redistribution of rents from intermediaries to workers without necessarily affecting allocative efficiency.

Temporary agency employment. By raising labor costs, the reform lowers the expected surplus from agency matches. If agencies mainly served as cost-saving intermediaries, this channel reduces inefficiently high agency employment. If instead they provided genuine matching services, it may reduce efficient intermediation. Thus, the overall efficiency effect depends on which motive dominated before the reform.

In-house employment. Two forces interact. Higher fees make agency work less attractive to user firms, encouraging internalization of workers into direct employment—a movement toward more efficient organizational boundaries when outsourcing is driven by monopsony power. Conversely, higher labor costs may reduce vacancy creation. The net employment effect is therefore ambiguous, but in markets where agency use reflected monopsonistic wage suppression, efficiency gains may dominate.

Job security. Agency jobs are typically short-term and high-turnover. By encouraging direct hiring, typically associated with employment relationships that are longer and more stable (see Table 3), the reform reduces inefficient churn and search costs (Bassanini et al., 2024). This adjustment likely raises overall job security and labor-market efficiency.

Rent-sharing. Equal pay shifts surplus toward workers. The distributional effect dominates if total surplus remains constant, but efficiency may also rise if the reform curtails rent extraction by intermediaries or monopsonistic firms, aligning rewards more closely with productivity.

Productivity. The productivity impact depends on whether the reform primarily corrects distortions or removes efficient flexibility. Reduced reliance on agencies might limit organizational adaptability, but if monopsonistic or low-quality matches previously constrained efficiency, better wage alignment and longer matches could enhance productivity.

5 Empirical Results

5.1 Effects on Temp Agency Wages

We begin by examining the impact of the policy on the wages of temporary agency workers. To do so, we estimate a dynamic difference-in-differences specification that allows us to assess whether treatment and control groups followed similar trends before the reform and to trace the evolution of effects afterward. Temporary agency employees are defined as the treatment group, while in-house workers serve as the control group. The specification is given by:

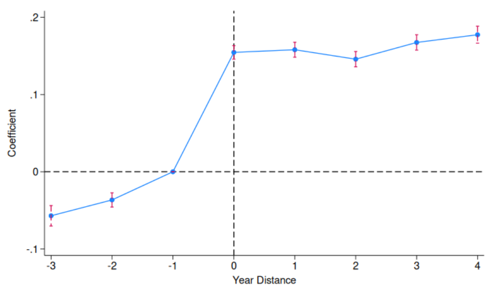
$$y_{it} = \alpha + \sum_{j=-T, j\neq -1}^{T} \beta_j 1[j = t] \times treatment_i + \delta_{FE} + \kappa X_{it} + \mu unemp_{pt} + \epsilon_{it}, \tag{3}$$

where y_{it} is the logarithm of the real daily wage of worker i in period t. The coefficients of interest are the β_j, which capture the interaction of event-time dummies $1[j = t]$ with a treatment indicator, equal to one for temp agency workers and zero for in-house workers. δ_{FE} denotes the set of fixed effects included in the model, while X_{it} are individual controls such as third-order polynomials for age and experience, education level, firm size, contract type, part-time job, and gender.

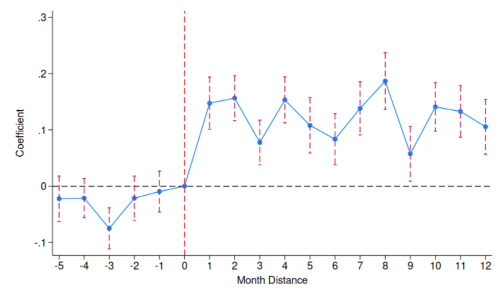
We implement two complementary empirical strategies. The first relies on the full worker-level dataset and includes province, occupation, and year fixed effects. This design maximizes representativeness. A limitation, however, is that we do not observe the sector of deployment for temporary agency workers; thus, even if sector fixed effects were included, they would not absorb the entry of agency workers into new sectors prior to the reform. As a result, mild pre-trends may arise from the rapid expansion of agency employment across industries.

The second strategy addresses these concerns by exploiting a shorter window around the reform and focusing on a balanced sample of workers continuously employed for six months before and twelve months after it (18 months in total). This specification incorporates individual fixed effects, thereby controlling for unobserved, time-invariant worker heterogeneity and partially alleviating the problem of not knowing the sector of deployment, as we expect sectoral composition to be more stable over this short horizon. The cost of this design is lower external validity—since the sample excludes workers with intermittent employment—but it yields more internally valid estimates. The similarity of the results obtained across both strategies, however, suggests that we are capturing an effect representative of most workers.

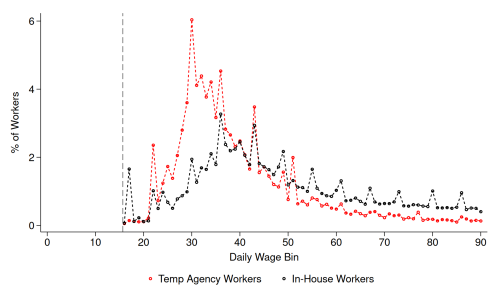
Figure 2: **Wage Effects**



(a) All Workers



(b) Workers Continuously Employed 18 Months



(c) Daily Wage Distributions

Notes: Panels (a) and (b) show the event-studies that analyze the wage effects of the reform. The specification is a difference in- differences as in Equation 3. Panel (a) uses the full worker-level dataset and includes time, occupation and province fixed effects. Panel (b) restricts the sample to workers continuously employed for at least 6 months before and 12 months after the reform, and further includes individual fixed effects. Additional control variables are the province unemployment rate, age and experience third-order polynomials, contract type, education, gender, part-time contract, and company size. The blue solid lines are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. The vertical red-dashed line depicts the month of the reform. Panel (c) plots the wage distributions of temp agency workers and in-house workers in 1998, the year before the reform. The grey-dashed vertical line represents the minimum wage. Sources: MCVL

The results are presented in Figure 2, with Panel (a) showing estimates for the full sample of workers and Panel (b) for the balanced sample of workers continuously employed for 18 months around the reform. In both cases, the coefficients turn positive and statistically significant immediately after the reform, indicating a sharp and persistent rise in the wages of temporary agency workers. Using the full sample, the effects are large—around 15%—and remain stable over time. Importantly, the stability of the post-reform effects suggests that any subsequent wage spillovers onto in-house workers are largely transmitted to agency workers as well, since the reform contractually linked both groups' wages to the same collective agreements.[20]

Although the event study suggests mild pre-trends, these are likely related to the rapid expansion of the temporary agency sector into new industries during the late 1990s rather than to differential wage dynamics between treated and control groups. To verify this interpretation, we turn to the balanced sample with individual fixed effects, which reduces compositional changes across sectors and eliminates any evidence of pre-trends. The estimated effects in this specification are slightly smaller, fluctuating between 10% and 15%, but similar in magnitude to those from the full sample. The close alignment of the two sets of results strengthens confidence that the estimated wage gains reflect the causal impact of the reform rather than confounding sectoral dynamics.

Table 4: Effects on Wages

	(1) Log Daily Wage	(2) Log Daily Wage	(3) Log Daily Wage
	Panel A: All Workers		
Treatment x Post	0.154***	0.181***	0.125***
	(0.00352)	(0.00357)	(0.00344)
Observations	29670206	29670206	29670206
	Panel B: All Workers, First Post Reform Year		
Treatment x Post	-0.0414***	0.155***	0.136***
	(0.00360)	(0.00448)	(0.00432)
Observations	29670206	29670206	29670206
	Panel C: Workers Continuously Employed for 18 Months		
Treatment x Post	0.122***	0.155***	0.148***
	(0.0125)	(0.0127)	(0.0110)
Observations	3311964	3311964	3311964
Individual FE	N	N	Y
Month FE	N	Y	Y
Occupation FE	N	Y	Y
Province FE	N	Y	Y
Other time-varying controls	N	Y	Y

Notes: The table shows the wage effects of the reform. The specification is a difference-in-differences. In Panel A, we use the worker-level dataset for the years 1995 to 2003. Panel B uses the same sample but reports the estimate of the first post period in the dynamic specification (Equation 3). In Panel C the sample is composed of all workers who are continuously employed 6 months before the reform, and for the next 12 months after it. The bottom panel reports the control variables included in each specification-column. The time-varying controls are the province unemployment rate, age and experience third-order polynomials, contract type, education, gender, part-time contract, and company size. Robust standard errors clustered at the worker level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL.

[20] The evidence for wage spillovers is consistent with this interpretation. See Section 5.2.1.

We report the estimates in Table 4. Panel A shows results using the full sample over the entire pre- and post-reform period. Panel B reports estimates from the same sample but restricts attention to the first post-reform year in the dynamic specification, thereby limiting the influence of pre-trends. Panel C uses the balanced sample of workers continuously employed for 18 months around the reform. Our preferred estimate is Column (2) of Panel B, which suggests a wage increase of 15.5%. As a complementary check, Column (3) of Panel C reports a very similar increase of 14.8%, based on the sample of workers continuously employed, and including individual fixed effects. Taken together, the evidence indicates that the reform increased wages for temporary agency workers by about 14.8–15.5%, consistent with the dynamic estimates reported in Figure 2. The magnitude of these effects is remarkably close to the temporary agency wage penalty of –16.7% reported in Table 1, implying that the reform effectively eliminated the pre-existing wage gap between agency and in-house workers.

The results confirm that the policy was successful in increasing the wages of temp agency workers. Hence, linking their wage-setting process to the collective agreements of the sectors where they were actually working is a useful tool to limit the negative wage effects that workplace fissuring has on wages. The wage estimates speak to two literatures. On the one hand, the positive impact on wages that we detect is similar in size, but of opposite sign, to the outsourcing wage penalty documented in the literature for temp agency workers in Argentina (-14%, Drenik et al., 2023), for low-skilled workers in Germany (-15 to -10%, Goldschmidt and Schmieder, 2017), for janitors and security guards in the USA (-24 to -4%, Dube and Kaplan, 2010) or for French outsourced workers (-14%, Bilal and Lhuillier, 2021).

On the other hand, temporary agency employment allowed user firms to apply lower wage floors than those mandated by their sectoral or firm agreements. The reform removed this possibility by extending collective agreement coverage, meaning our estimates capture a coverage premium—the wage gain from making stronger bargaining coverage binding for workers previously covered by weaker agreements. This differs from the union membership premium, which reflects how individual union membership affects wages (Farber et al., 2021; Jäger et al., 2024).[21] In wage-setting systems like Spain's — similar to most OECD countries, where collective agreements cover the majority of workers regardless of union affiliation — distinguishing coverage from membership is essential for understanding how collective

[21] Most existing evidence on union premia relies on Mincer-type regressions that identify wage effects conditional on observables (Farber et al., 2021; Jäger et al., 2024). A smaller literature provides quasi-experimental evidence using union certification elections in the U.S. (DiNardo and Lee, 2004; Lee and Mas, 2012; Sojourner et al., 2015; Frandsen, 2021) or institutional changes in high-unionization settings like Norway (Barth et al., 2020). The coverage premium estimates are similar to most union premium estimates in the literature, that range between 10% and 20%.

bargaining affects pay (Bhuller et al., 2022).

Moreover, the quasi-experimental design provides, to our knowledge, the first causal evidence on coverage premia in a coordinated bargaining system. We estimate that extending binding coverage raised agency-worker wages by 15%. Our estimated effects are about twice the magnitude of those found by Jäger et al. (2024), whose cross-sectional estimates are more susceptible to selection bias. The strategy in this section isolates the direct effect of extending coverage, whereas in Section 5.2.1 we show that spillovers to already-covered in-house workers further amplify this impact.

Finally, Panel (c) of Figure 2 displays the wage distributions of temporary agency and in-house workers in 1998, one year before the reform. The vertical dashed line marks the statutory minimum wage. The figure shows that temporary agency workers were not confined to minimum-wage jobs and that the reform raised pay across a broad segment of the wage distribution, rather than only at the bottom.

5.2 Wage Spillovers, Employment and Job Security: Province-Level Analysis

Next, we continue by analyzing the impact of the policy on in-house wages, employment and job security. The intuition for the identification strategy in this section is that provinces with a higher share of temp agency workers should experience a relatively larger shock in labor costs and, hence, larger changes in outcomes. To the extent that the reform was not differentially implemented across provinces as a function of the outcomes of interest, the estimates can be interpreted as the causal effect of the policy.

In Table 5, we show that the spatial distribution of temp agency workers across provinces was markedly heterogeneous both before and after the reform. For each province, we compute the ratio $\%TempAgency_{pt} = \frac{TempAgencyWorkerMonths_{pt} \times 100}{WorkerMonths_{pt}}$. $TempAgencyWorkerMonths_{pt}$ measures the number of work months in a temp agency contract in province p and year t. Similarly, $WorkerMonths_{pt}$ refers to the number of work months in any contract. There are 50 provinces in Spain, and there is significant variation in temp agency usage across them, ranging from a minimum of 0.46% to a maximum of 3% in 1998.[22] Consistent with Figure 1, we can observe that the mean incidence of temp agency work declined between 1998 and 2001, from 1.59% to 1.43%, or -10.1%.

The empirical strategy is a difference-in-differences, where we use $\%TempAgency_{pt}$ for the year 1998 as the shock or treatment variable that captures the incidence of temp agency

[22] We exclude the autonomous cities of Ceuta and Melilla from the analysis.

employment in each province before the policy. The dynamic specification is:

$$y_{pt} = \alpha + \delta_p + \delta_t + \sum_{j=-4,j\neq-1}^{4} \beta_j 1[j=t]\%TempAgency_{p,98} + \sum_{j=-4}^{4} \gamma_j 1[j=t] X_p + \epsilon_{pt} \tag{4}$$

where δ_p and δ_t are province and year fixed effects. The coefficients of interest are the β_j, which capture the effect of the "shock" variable each year.

The endogenous variables, y_{pt}, are composition-adjusted wages, employment shares and job security outcomes, as we explain with more detail when we discuss the results of each empirical exercise. The specification also includes interactions of year dummies with controls at baseline, X_p. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. In addition, we also estimate a static difference-in-differences model:

$$\begin{aligned} y_{pt} = \alpha + \delta_p + \delta_t + \rho_1\%TempAgency_{p,98} + \rho_2 post_t + \rho_3\%TempAgency_{p,98} \times post_t + \\ + \sum_{j=-4}^{4} \gamma_j 1[j=t] X_p + \epsilon_{pt} \end{aligned} \tag{5}$$

Table 5: Incidence of Temp Agency Contracts Relative to Employment

	(1) Year 1998					(2) Year 2001				
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
% Temp Agency Employment (Province)	1.592	0.643	0.456	3.033	50	1.429	0.556	0.400	2.528	50

Notes: The table shows summary statistics of the incidence of temporary agency employment by province. We report means, standard deviations, minimum, maximum and number of observations, for the year before the policy (1998) and two years after the policy (2001). Sources: MCVL.

5.2.1 Wage Spillovers

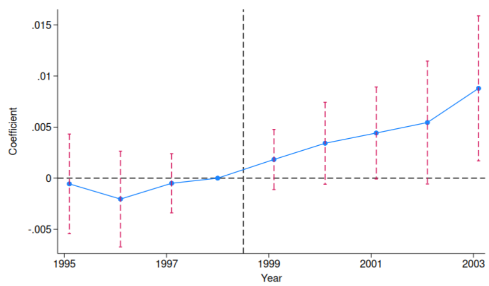
In this section, we measure the spillover effects on the wages of in-house workers, which is crucial for assessing the overall reach of wage-setting institutions (Jäger et al., 2024).[23] To examine these spillovers, we use two complementary measures of in-house wages. The first focuses on actual wages, which capture the overall impact of the reform on in-house workers.

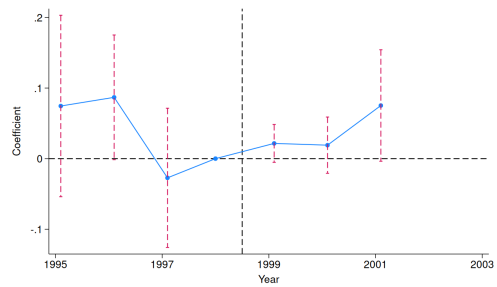
[23]Additionally, Jäger et al. (2024) emphasize that causal estimates of spillovers are crucial to reconcile micro and macro evidence. While micro-level studies often find relatively modest direct wage effects of collective bargaining, cross-country comparisons show large associations between bargaining coverage and wage compression. Credible spillover estimates help bridge this gap by showing how institutions affect workers beyond those directly covered.

The second examines wage floors negotiated in collective bargaining agreements, allowing us to assess whether spillovers operate through collective bargaining.[24]

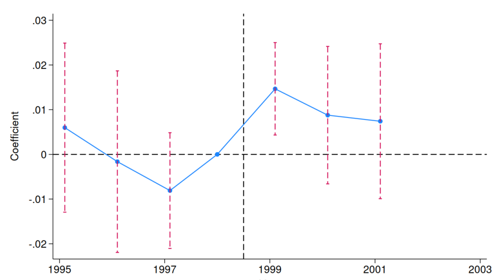
For actual wages, we construct composition-adjusted wages based on a Mincerian regression that allows for specific returns across skills: $\log w_{it} = \alpha + \beta X_{it} + \delta_i + \delta_p + \delta_s + \delta_t + \epsilon_i$, where $\log w_{it}$ is the log of the real daily wage of individual i in month t. The vector X_{it} reflects individual characteristics, including education, a cubic polynomial of experience and tenure, education and experience interactions and contract type. δ_i, δ_p, δ_s and δ_t are individual, province, sector and month fixed effects, respectively. The assumptions that we make with this procedure are that the returns to personal characteristics are equal across provinces and time, but we allow that different periods and different provinces may have different wage levels and wages may be evolving differently across provinces.

Figure 3: **Wage Spillovers**





(a) In-House Wages **(b) In-House Wage Floors**



(c) 20% Wage Cushion

Notes: The figures show the event-studies of the effects of the reform on actual in-house wages (Panel (a)), in-house wage floors (Panel (b)), and the incidence of wage cushions of at most 20% (Panel (c)). The specification is a continuous difference-in-differences (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Panels (b) and (c) display estimates until 2001 because that is the last year in the wage floor data. Sources: MCVL and Census of Collective Bargaining Agreements.

[24]The wage-floor regression uses data with multiple observations for each province–year, reflecting occupation-specific wage floors set in sector- and firm-level collective agreements. Identification comes from differences across provinces in their exposure to the national reform.

For wage floors, we use the logarithm of composition-adjusted contractual wages set in collective bargaining agreements. The specification includes the same explanatory variables as above, and since the data distinguish ten occupational wage groups, we also add occupation fixed effects.[25]

The results, presented in Figure 3 and Table 6, show clear evidence of wage spillovers onto in-house workers. Composition-adjusted wages grew more in provinces with a higher pre-reform incidence of agency work: a one–percentage-point higher incidence is associated with a 0.6% larger post-reform wage increase. These results indicate that extending collective agreements to agency workers also affected the wages of already covered in-house employees, consistent with broader general-equilibrium effects of collective bargaining institutions. This finding aligns with a growing body of evidence showing that labor market institutions generate substantial wage spillovers across segments of the workforce not directly affected by them (Green et al., 2024; Bassier, 2024; Elias et al., 2025).[26]

We next test whether these spillovers operate through collective bargaining by examining the evolution of composition-adjusted wage floors.[27] Provinces with higher pre-reform incidence of agency work had relatively higher negotiated wage floors before the reform, but this differential eroded during the rapid expansion of temporary agency employment and re-emerged afterward, as wage floors grew faster in high-incidence provinces. This pattern suggests that, before the reform, the availability of cheaper agency labor weakened the bargaining position of in-house workers, whereas extending collective agreements helped restore collective bargaining as an effective wage-setting channel. Nevertheless, the pass-through from wage floors to actual wages was modest: in 2001, the estimated coefficient on wage floors is 7.5%, compared with 0.4% for composition-adjusted wages. These figures imply a pass-through rate of about 5.3%, indicating that floors were not binding for many workers and that wage cushions adjusted only partially.[28]

Before the reform, relative decreases in negotiated wage floors in high–temp-agency provinces did not translate into lower actual wages—a pattern reflected in Panel (c), where

[25] The data use the same occupational classification as the social security dataset, except that they do not distinguish a separate category for workers younger than 18 years of age. When merging the two datasets, we assign workers in this missing category to the closest corresponding group, namely unskilled workers aged 18 and over.

[26] Green et al. (2024) document that de-unionization in the United States accounts for roughly 35% of the decline in mean hourly wages between 1980 and 2010, with two-thirds of this effect operating through spillovers onto non-union workers. Bassier (2024) finds that collective bargaining in South Africa transmits wage increases to uncovered firms through worker flows, roughly doubling the aggregate effect of bargaining on the wage distribution. Elias et al. (2025) show that regularization policies equalizing the rights of immigrants and natives can generate positive wage spillovers on natives.

[27] We estimate composition-adjusted wage floors for consistency with the wage spillover analysis and to facilitate estimation of pass-through from floors to actual wages.

[28] See Card and Cardoso (2022) for related evidence of incomplete pass-through in Portugal.

changes in wage cushions were noticeably smaller than changes in floors, consistent with wage floors not being binding. By contrast, after the reform, workers in high-incidence provinces experienced a sharper rise in the share with at most a 20% cushion: for 1999, floors increased by 2.16% while the share of workers close to the floor increased by 1.47%, a pattern in line with the spillover wage gains documented for in-house workers. This indicates that wage floors became more binding in high-incidence provinces. At the same time, the attenuation of the cushion response over subsequent years suggests that collective bargaining is one channel through which spillovers operate, but not the only one: the evidence also points to a role for individual wage bargaining, helping explain why effects on cushions weaken even as overall spillovers persist.[29]

Table 6: Wage Spillovers

	(1) Log Wages	(2) Log Wage Floors	(3) 20% Wage Cushion
$\%\ TempAgencyWork_{1998} \times Post$	0.00556***	-0.0288	0.0112
	(0.00187)	(0.0311)	(0.0104)
Observations	450	6943	350

Notes: The table shows the wage spillover effects of the reform. The endogenous variables are composition-adjusted wages (Column 1), composition adjusted wage floors (Column 2), and the incidence of wage cushions of at most 20% (Column 3). Estimates are obtained from Equation 5. Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. Robust standard errors clustered at the province level are shown in parentheses. The number of observations differs across columns. Column 1 uses province–year data for the period 1995–2003. Column 2 has a larger number of observations because it includes multiple observations per province–year, reflecting occupation-specific wage floors negotiated in sector- and firm-level collective agreements. Column 3 includes only 350 observations because wage-cushion data are available only through 2001. * significant at 10%; ** significant at 5%; *** significant at 1% Sources: MCVL and Census of Collective Bargaining Agreements.

5.2.2 Employment

Now we turn to study employment effects. We begin by showing the raw employment data at the province level. The outcome variables, y_{pt}, are the percentage of temp agency or in-house workers in province p and year t, relative to the population of active workers in province p the year before the reform. Figure 4, Panel (a), shows the percentage of temp agency workers for several provinces over time. We can see that, before the reform, the temp

[29] To compute wage cushions, we merge collective bargaining agreement (CBA) floor data with Social Security records using province, sector, occupation, and year identifiers. We apply a four-step matching procedure: (i) exact province–sector–occupation matches (29.7%); (ii) province–sector matches irrespective of occupation (6.7%); (iii) sector–occupation matches from other provinces (26.6%); and (iv) sector-level matches from other provinces regardless of occupation (0.6%). Overall, this strategy yields matches for 64% of workers, exceeding the approximately 50% matching rates reported by Card and Cardoso (2022) and Adamopoulou et al. (2025).

agency sector was growing very fast in some provinces, and not growing much in others. The graph also shows that the policy affected the trend of the sector, with the fastest-growing provinces being more negatively affected relative to the pre-treatment trend. Panel (b) plots the percentage of in-house employment. The evolution of this variable is positive over time, and there is no obvious change in trend that coincides with the policy.[30]

Panel (a) suggests that the specification needs to account for heterogenous linear time trends across provinces when we analyze temp agency employment. However, since the reform affects the post-treatment trend, including province-specific time-trends, built with both pre- and post-treatment data, would result in biased estimates. Intuitively, when estimating linear time trends in regressions that include post-treatment data, the effect of the treatment on the post-treatment trend affects the estimate of the linear province-specific time trend, biasing the estimate of the effect of the treatment. Thus, to recover the effect of the treatment of interest we fit province-specific time trends before the treatment, and remove them from the outcome of interest. Note that this approach captures the effect of interest in the presence of potentially heterogenous time trends across provinces and is robust to the reform affecting the post-reform trends.[31] For in-house and overall employment, we show results with and without detrending these outcome variables.

The evidence from the regression analysis is presented in Panels (c)-(f) in Figure 4 and in Table 7. Panels (c), (d) and (e) in the figure show the results for temp agency employment, in-house employment, and overall employment, respectively. Three main conclusions emerge. First, we find no significant nonlinear differential pre-treatment trends for any of the three outcome variables. Second, temporary agency employment declines sharply (Panel (c)). This pattern is consistent with the extension of user-firm collective agreements to temporary agency workers altering firms' outsourcing decisions, by eliminating the wage advantage of hiring these workers through intermediaries. Third, in-house and overall employment rise more in high-incidence provinces (Panels (d) and (e)). The increase in in-house jobs suggests that firms internalized workers as outsourcing became relatively more expensive, while the positive effect on overall employment indicates that this internalization more than compensated for the loss of agency jobs.

Taken together, these results imply that temporary agency employment was inefficiently high before the reform—sustained by monopsonistic wage suppression rather than superior matching efficiency.[32] The decline in agency jobs combined with a compensating rise in

[30] Figure E1 in the Appendix plots the percentage of overall employment over time. Its evolution is positive and there is no change in trend after the reform, similar to the case of in-house employment.

[31] See Dustmann et al. (2021) and Elias et al. (2025) for recent papers using similar empirical strategies. Appendix D in Elias et al. (2025) provides a very detailed justification of the empirical strategy.

[32] As shown in Section 3, temporary agency work was disproportionately concentrated in sectors with higher

in-house employment, and presumably no negative effect on total employment, indicates a reallocation that reduced monopsonistic distortions while preserving efficient labor market intermediation by agencies.

The accompanying wage increases likely operated as a pull factor, attracting additional labor supply toward provinces with higher pre-reform agency incidence. Consistent with this mechanism, Panel (f) shows a marked rise in internal migration toward these provinces, expanding local labor supply and allowing in-house employment to grow beyond the displacement of agency work.[33]

Table 7 reports the difference-in-differences estimates. A one–percentage point higher pre-reform share of agency employment reduces agency jobs by 0.46 pp, or about 28.9% relative to the pre-reform mean. Columns 2 and 3 show that in-house employment rises by roughly 1 pp, whether or not we control for pre-treatment trends—suggesting that the decline in agency work was largely compensated by its internalization into direct employment.

Turning to overall employment (Columns 4 and 5), the effect remains positive in both specifications. Focusing on the more conservative detrended estimates, a one–percentage point higher pre-reform share of agency work increases total employment by 0.57 pp, though the 95% confidence interval ranges from –0.62 to 1.75. Hence, the aggregate impact is likely positive but not statistically precise, consistent with heterogeneous responses across sectors: while employment rose in more monopsonistic industries where higher wages attracted labor supply, effects may have been weaker—or even negative—where monopsony power was limited. Finally, Column 6 shows that internal migration increased by about 4.2% for each additional percentage point of pre-reform agency incidence, supporting the view that part of the employment adjustment operated through expanded local labor supply.

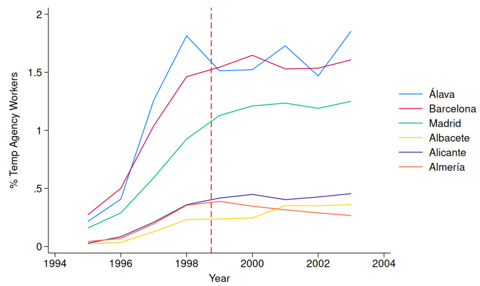
5.2.3 Job Security: Job Rotation, Contract Types and Job Duration

We now turn to the effects of the reform on job security, focusing on job flows, contract types, and job duration. We show that the policy change reduced excessive reliance on temporary agency work, leading to more stable employment relationships characterized by lower turnover, longer job durations, and a higher share of permanent contracts. These dimensions are relevant both as outcomes in themselves—given their implications for worker welfare—and as potential channels through which the reform may have enhanced productiv-

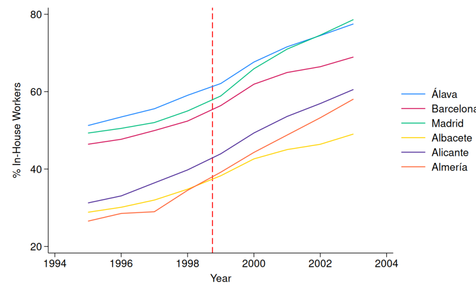
labor-market concentration—where firms had greater monopsony power but where collective bargaining could partly offset wage markdowns.

[33] This adjustment is also consistent with spatial equilibrium dynamics à la Rosen and Roback. In Appendix E.6, we show that the migration response is driven by Spanish-born workers rather than foreign-born workers, ruling out international inmigration as an explanation for the absence of negative employment effects.

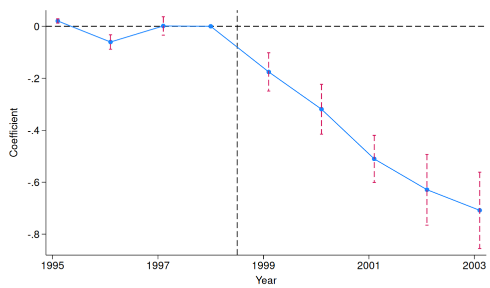
Figure 4: **Employment Effects**



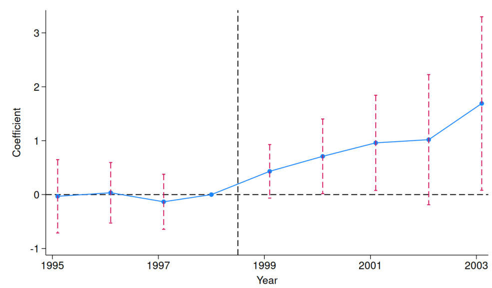
(a) Temp Agency Employment



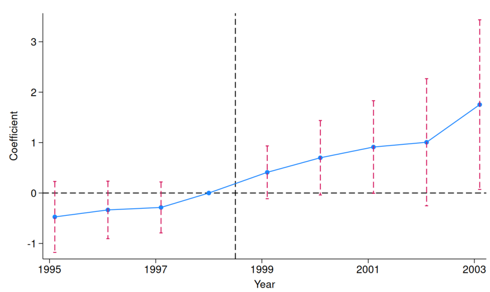
(b) In-House Employment



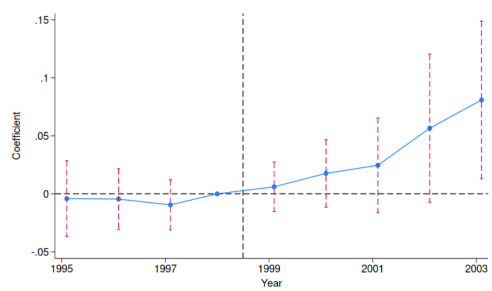
(c) Temp Agency Employment



(d) In-House Employment



(e) Employment



(f) Immigration from Other Provinces

Notes: The figures show the percentages of temp agency (Panel (a)) and in-house (Panel (b)) employment relative to province active population at baseline. We display it for three provinces with high temp agency incidence (Álava, Barcelona and Madrid), and three provinces with low temp agency incidence (Albacete, Alicante, Almería). Panels (c), (d) and (e) depict the event-studies of the effects of the reform on temp agency, in-house and overall employment, respectively. Panel (f) reports the impact on internal migration. The specification is a continuous difference-in-differences (Equation 4). For the temp agency employment variable we remove province-specific pre-reform linear trends. Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Sources: MCVL.

Table 7: Effects on Employment

	(1) Temp Agency Detrended	(2) In-House Detrended	(3) In-House Not Detrended	(4) Both Detrended	(5) Both Not Detrended	(6) Immigration from Other Provinces
% $TempAgencyWork_{1998} \times Post$	-0.459***	1.028*	0.995*	0.569	1.230**	0.0417
	(0.0536)	(0.554)	(0.507)	(0.595)	(0.528)	(0.0273)
Observations	450	450	450	450	450	450

Notes: The table shows the employment effects of the reform. Estimates are obtained from a static difference-in-differences version of Equation 4. Regressions are weighted by population. In columns 1, 2 and 4 we removed province-specific pre-reform linear trends. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. Robust standard errors clustered at the province level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL.

ity.[34]

Job Rotation: Figure 5 presents the results on job flows. The outcome variables are the shares of layoffs (left column) and hires (right column), normalized by the number of separations in each province in 1998. Panels (a) and (b) cover all workers, panels (c) and (d) focus on temp agency workers, and panels (e) and (f) on in-house workers.

The evidence indicates that the reform reduced turnover. Both layoffs and hires declined in the post-reform period, with the entire effect concentrated in the temp agency sector. For in-house workers, the reform does not appear to have generated significant changes in job flows. Table 8, Columns 1–9, reports the corresponding estimates. For each group of workers, we also compute the net effect, defined as the share of hires minus the share of layoffs, $\frac{Hires_t - Layoffs_t}{Separations_{98}}$. The estimates confirm the patterns observed in the figures.

Permanent and Short-Term Contracts: Another key dimension of job quality is contract segmentation. Permanent contracts are generally considered "good" jobs, as they offer higher wages, severance payments, job stability, and promotion opportunities. In contrast, short-term (ST) contracts are typically regarded as "bad" jobs, associated with lower wages, limited severance, and fewer career prospects (García-Pérez et al., 2019; Daruich et al., 2023). Figure 6 shows that the policy increased permanent employment and reduced ST employment, although the decline in ST contracts is not persistent. Table 8, Columns 10 and 11, reports the corresponding estimates. A 1pp higher pre-reform share of temp agency employment increases permanent employment by 1.29pp. Hence, the in-house employment increase reported in Table 7, Column 3, is entirely driven by permanent jobs.[35]

[34] More stable employment relationships can foster on-the-job learning and encourage training (Booth et al., 2002).

[35] Permanent and short-term employment in Columns 10 and 11 are defined to include all contracts of each type, regardless whether they are provided in-house or through temporary work agencies and therefore does not map one-to-one into the in-house employment measure in Table 7, Column 3. As a result, the coefficients

Notably, the rise in open-ended employment implies that many workers also gained access to severance pay rights—45 days of salary per year worked in the case of wrongful dismissal—reflecting a substantial improvement in job security. This highlights that wage gains and job-quality features, such as employment protection, can complement one another rather than trade off (Dube et al., 2022).[36] [37]

Job Duration: Consistent with the increase in permanent employment, we also find that the reform raised job duration for affected workers. To study this outcome more directly, Appendix E.2 presents an alternative empirical strategy based on incumbent temp agency workers, which is better suited to capture contract-level adjustments than the province-level analysis. This approach shows that average job duration increased by about 25% within five months after the reform for incumbent temp agency workers, reinforcing the conclusion that the policy not only expanded in-house and permanent employment but also lengthened employment relationships.

Taken together, these results indicate that the reform reshaped the structure of employment toward more stable and higher-quality jobs. By raising agency wages and fees, the policy reduced the cost advantage of temporary agency work, prompting user firms to internalize these workers and expand direct, permanent employment. The decline in turnover and the rise in job duration are consistent with this internalization process, as well as with a reduction in monopsonistic practices based on high worker rotation. In line with Acemoglu (2001), the reform effectively pushed the labor market toward "good jobs" by shifting adjustment costs from workers to firms—both through higher wages and through tighter regulation of intermediated employment.

5.3 Do Agencies Increase the Fee?

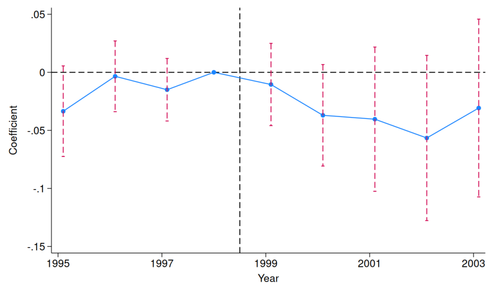
The evidence we have presented has shown that employment in the temp agency sector declined after the reform (Section 5.2.2), with negative effects on the number of temp agencies (Figure 1, Panel (d)). Now, we turn to analyze how the surviving temp work agencies adjusted the fees they charged to user firms in response to the equal-pay reform. Under-

in Columns 10 and 11 need not sum exactly to the in-house employment effect. The endogenous variables are expressed as shares relative to active workers in each province in 1998, consistent with the employment variables used in Section 5.2.2.

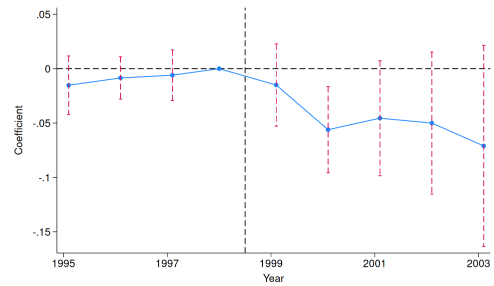
[36] In cases of fair dismissal, workers are entitled to 20 days of salary per year worked. However, Elias (2023) shows that, in 1996–97, 77.9% of separations were wrongful layoffs, 0.47% were fair dismissals, and the remaining 21.63% were voluntary quits.

[37] Recall that the reform also introduced severance pay for temporary agency workers—12 days of salary per year worked for short-term temp agency workers at contract termination (see Section 3). In principle, user firms could have substituted toward direct short-term contracts, which at the time carried no such protection. Yet the results indicate that many instead shifted to permanent hires.

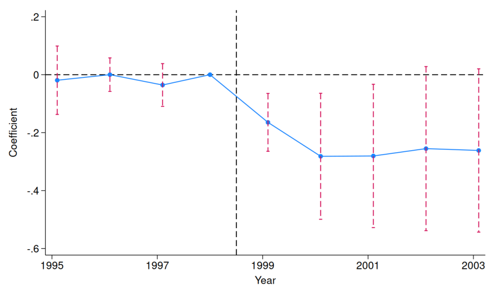
Figure 5: **Effects on Flows**



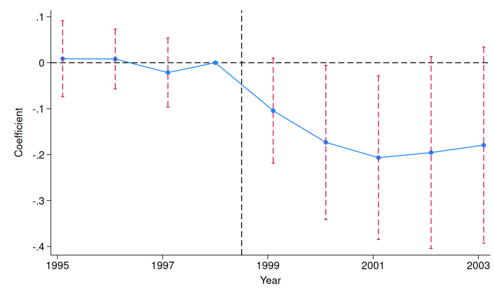
(a) Layoffs (All)



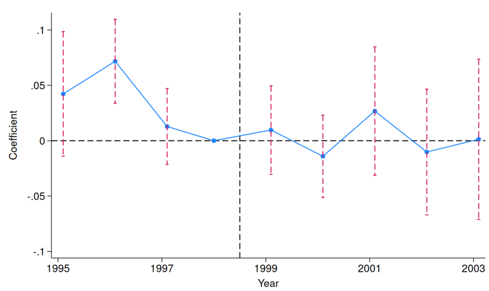
(b) Hires (All)



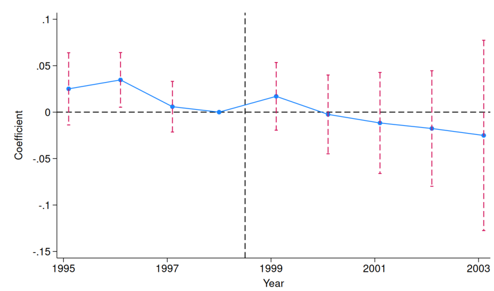
(c) Layoffs (Temp Agency)



(d) Hires (Temp Agency)



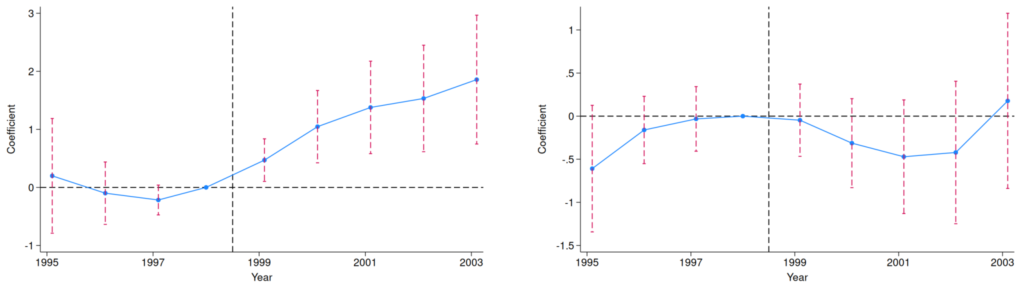
(e) Layoffs (In-House)



(f) Hires (In-House)

Notes: The figures show the event-studies of the effects of the reform on layoffs and hires for all workers (Panels (a) and (b)), temp agency workers (Panels (c) and (d)), and in-house workers (Panels (e) and (f)). The specification is a continuous difference-in-differences (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Sources: MCVL.

Figure 6: **Effects on Permanent and Short-Term Employment**



(a) Permanent Employment **(b) Short-Term Employment**

Notes: The figures show the event-studies of the effects of the reform on permanent and short-term employment. The specification is a continuous difference-in-differences (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Sources: MCVL.

Table 8: Effects on Flows

	(1) Layoffs All	(2) Hires All	(3) Net Hires All	(4) Layoffs Temp Agency	(5) Hires Temp Agency	(6) Net Hires Temp Agency	(7) Layoffs In-House	(8) Hires In-House	(9) Net Hires In-House	(10) Permanent Employment	(11) Short-Term Employment
% $TempAgencyWork_{1998} \times Post$	-0.0221	-0.0401	-0.0180	-0.248**	-0.180**	0.0683*	-0.0290	-0.0244	0.00463	1.286***	-0.0147
	(0.0271)	(0.0276)	(0.0150)	(0.107)	(0.0791)	(0.0343)	(0.0227)	(0.0267)	(0.0190)	(0.431)	(0.384)
Observations	450	450	450	450	450	450	450	450	450	450	450

Notes: The table shows the effects of the reform on flows and contract types. Estimates are obtained from a static difference-in-differences version of Equation 4. Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. Robust standard errors clustered at the province level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL.

standing this channel is crucial for tracing how intermediaries and firms shared the increase in labor costs. The results show that surviving agencies passed all of the cost increase to user firms, indicating that they provided valuable intermediation services for a subset of matches—services for which firms were willing to pay more once wage distortions were removed. At the same time, the complete pass-through helps explain why internalization occurred: where agency services mainly served to exploit wage gaps rather than generate efficiency gains, user firms found it optimal to bring employment back in-house when cost advantages disappeared.

To quantify pass-through, we use the following formula:

$$\text{Pass-through} = \frac{\Delta\text{Gross Fee}}{\Delta\text{Wages} + \Delta\text{Severance Payments} + \Delta\text{Training Costs}} \tag{6}$$

We begin with the numerator. The gross fee measures the total price paid by user firms for each day an agency worker is supplied, including the worker's wage, the agency's operating costs, and its margin. Changes in this fee can therefore arise from two sources: (a) the pass-through of higher labor costs to user firms and (b) shifts in the composition of agency matches—either toward assignments with higher margins due to greater hiring costs or toward more productive (and therefore higher-wage) workers.

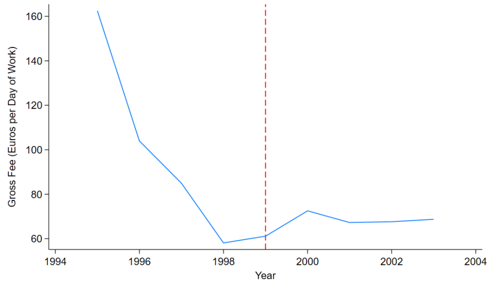
Because detailed information on agency revenues is not available at the local level, and because the revenues of any given agency may be reported in a different province from where its workers are employed, we cannot apply the province-level identification strategy used in previous sections. Instead, we reconstruct national time series of gross agency fees using revenue data from temp agencies in firm balance sheet data, combined with the number of days worked by temp agency workers from social security records.[38] These data allow us to examine how the pricing of agency services responded to the reform and to quantify the extent of cost pass-through.

Figure 7, Panel (a), shows the resulting evolution of the gross fee, calculated as total agency revenues divided by total days worked. The series declined sharply during the expansion years of the sector, from roughly 160 euros per day to 60, reflecting increased competition, scale efficiencies, and a business strategy centered on low-wage placements. Immediately after the reform, however, gross fees rose markedly, from 58.1 euros in 1998 to 72.5 euros in 2000—a 24.8% increase.[39] This sharp rise signals substantial pass-through of higher labor costs to user firms, though it may also reflect compositional changes if agencies exited low-margin segments and concentrated on higher-cost or higher-productivity matches.

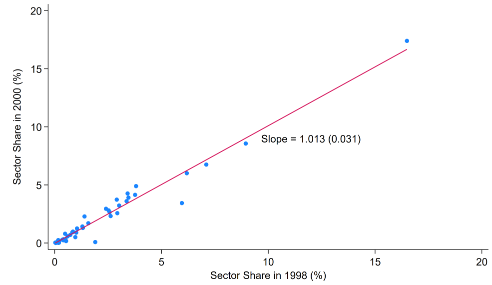
[38]Further details on the calculation are provided in Appendix F.

[39]Extending the window to 1998–2003, the gross-fee increase amounts to 18.3%.

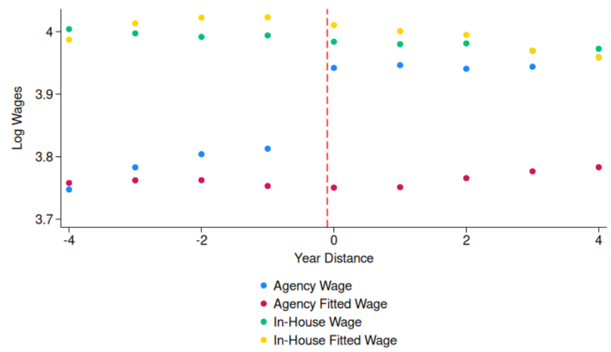
Figure 7: **Did Agencies Increase the Fee?**



(a) Gross Fee



(b) Distribution of Agency Contracts



(c) Average Log Wages

Notes: Panel (a) shows the evolution of the gross fee charged by temp agencies for each day of temp agency worker deployment. Panel (b) displays the sectoral distribution of temporary agency contracts in 1998 and 2000, as a percentage of the total number of temporary agency contracts in each year. Panel (c) shows the evolution of agency and in-house wages. The vertical red-dashed line depicts the time of the reform. See Section F in the Appendix for details about the calculation of the fee. See Section 5.2.1 for details about the estimation of fitted wages. Sources: *Central de Balances Integrada* (BELab, 2025), MCVL and *Empresas de trabajo temporal*.

We now turn to the denominator in Equation 6, which captures the increases in labor costs that can be directly quantified. As explained in Section 3, the reform raised not only wages but also employment protection costs and training costs. On the one hand, severance payments for temp agency workers increased from 0 to 12 days of salary per year worked—an increase of $\frac{12}{365} = 3.28\%$—and all temp agency workers became entitled to them upon contract termination. On the other hand, the reform required temp agencies to invest 1% of the salary mass in worker training. Using the average wage pre- and post-reform (53 and 60 euros, respectively) from Table 3, and the payroll tax rate of 37.2%, the denominator becomes $(60 - 53) \times 1.372 + 60 \times 0.0328 + 60 \times 0.01 = 12.17$ euros. Given these components, pass-through equals $\frac{14.4}{12.17} = 118\%$.

A pass-through rate above 100% can arise naturally if we are not capturing all of the cost increases triggered by the reform. Several mandated changes likely raised the operating costs of temp agencies but cannot be directly measured. For instance, the reform required firms to employ at least 12 staff members for every 1,000 temp contracts, potentially increasing the costs of running an agency. Because we lack information on the number of employees involved in providing intermediation services, these costs cannot be computed. The law also required agencies to share every temp contract with worker representatives, potentially raising transaction costs. These unmeasured components would lead us to understate the denominator in Equation 6, thereby pushing the estimated pass-through upward.

To safely conclude that the measured pass-through reflects these omitted labor and administrative costs, rather than an overestimate, we must rule out two alternative explanations that could also generate a pass-through rate above 100%. First, agencies might have raised their prices by more than the increase in labor costs. This interpretation appears unlikely, however, as the reform did not enhance agencies' market power or expand the profitability of intermediation services. Second, compositional changes could inflate the observed rise in the gross fee if agencies exited low-margin, low-productivity assignments and concentrated on matches with high productivity. In such a case, part of the fee increase would reflect a shift in the composition of contracts rather than genuine pass-through.

Importantly, the data show no evidence of changes in the observable composition of agency workers that could plausibly account for the increase in gross fees. As reported in Table 3, the distributions of education, age, and gender are very stable between the pre- and post-reform periods. Moreover, the few observable differences that do emerge, such as a decline in average experience and a reduction in the share of agency workers in the managerial layer, shift in directions that would, if anything, predict lower rather than higher fees. Moreover, Figure 7, Panel (b), shows that the sectoral distribution of temp agency workers remains very similar before and after the reform, further indicating that compositional changes were minimal.

Therefore, any compositional force capable of raising fee levels would have to operate through unobserved productivity differences rather than through changes in observable worker attributes. To assess this possibility, we compare the evolution of both raw and composition-adjusted wages of temporary agency workers.[40] If gross-fee growth were driven primarily by shifts toward more productive workers, composition-adjusted wages would display a discrete jump at the reform. By contrast, if fee increases mainly reflect pass-through of higher labor costs, composition-adjusted wages should remain smooth. Figure 7, Panel (c), shows that composition-adjusted wages exhibit no such break. Taken together, these patterns indicate that neither increased profitability nor compositional changes can account for the magnitude of the fee increase, supporting the interpretation that the post-reform rise

[40]We apply the same composition adjustment procedure used for the analysis of spillovers on in-house wages in Section 5.2.1.

in agency fees reflects full pass-through of labor costs to user firms.

6 Rent-Sharing

In Section 5.2.2, we showed that the temporary agency sector contracted after the equal-pay reform, indicating that agencies absorbed part of the policy's costs. We now turn to the subset of agency relationships that persisted to examine how the reform reshaped rent-sharing among workers, agencies, and user firms. Building on the rent-sharing literature that studies two-party wage–profit relationships between workers and firms (e.g., Card et al., 2018), we extend the framework to a triangular setting in which an intermediary agency participates in surplus extraction. The formal model, presented in Appendix A, provides a simple structure to quantify how regulation alters the division of surplus across the three actors.

From the Bellman equations in the model, the joint rent from an agency assignment is:

$$R^A \equiv \underbrace{\frac{\varphi - f}{r + s_a}}_{\text{User firm rent } (J_u)} + \underbrace{\frac{f - w_a}{r + s_a}}_{\text{Agency rent } (J_a)} + \underbrace{\frac{w_a}{r + s_a}}_{\text{Worker rent } (W_a)} = \frac{\varphi}{r + s_a}, \tag{7}$$

where φ is the productivity of an agency worker, s_a is the (Poisson) separation rate from an agency assignment, w_a is the agency wage, f is the agency fee, and $\frac{1}{r+s_a}$ is the present-value factor of a unit flow accruing while the match survives.

The decomposition shows that total rent depends only on productivity and the separation rate. The fee f and the wage w_a therefore do not change the size of the rent; they only redistribute it among the three parties. Each agent extracts value from the match: the worker through wages, the agency through the fee charged to the user, and the user through productivity net of payments.

The relevant margin to be split between the agency and the user firm is $(\varphi - w_a)$. Its division is determined by the agency's bargaining weight γ in Nash bargaining. From the Nash rule:

$$f - w_a = \gamma(\varphi - w_a) \quad \Rightarrow \quad \gamma = \frac{f - w_a}{\varphi - w_a}. \tag{8}$$

A higher γ implies that the agency extracts more rent through higher fees, while a lower γ shifts rents toward the user firm.

To quantify these rents, we calibrate the model using Spanish data for 1998, the year preceding the equal-pay reform. Parameter values are computed for a 117-day job duration ($s_a = 1/117$), with an in-house wage of €60/day ($w_h = 60$) and an agency wage of €53/day

($w_a = 53$), as reported in Table 3. The daily interest rate is 0.015% ($r = (1+0.055)^{1/365}-1$), corresponding to an annual interest rate of 5.5% from the Bank of Spain. The calibrated productivity is $\varphi = 89$ €/day, consistent with the computed markdown of user firms ($\varphi/w_h = 1.50$), estimated from BELab firm-level data using the "production function approach" (Loecker and Warzynski, 2012).[41] The observed fee is €58/day ($f = 58$), as shown in Figure 7.

Under this benchmark calibration, the implied bargaining weight is $\gamma = \frac{f-w_a}{\varphi-w_a} \approx 0.14$, indicating that temporary work agencies have considerably less bargaining power than user firms. The total joint rent from an assignment is $R^A = \frac{\varphi_a}{r+s_a} \approx 10{,}233$ euros, which represents the present value of an average agency assignment over its expected duration. Table 9 reports the division of this rent across the three parties.

Table 9: Rent sharing in agency employment

	Pre-reform		Post-reform (equal pay)	
	Value (euros)	Share of R^A	Value (euros)	Share of R^A
Worker (W_a)	6,094	59.6%	6,898	67.4%
Agency (J_a)	575	5.6%	575	5.6%
User firm (J_u)	3,564	34.8%	2,759	27.0%
Total rent (R^A)	10,233	100%	10,233	100%

Notes: The table reports the decomposition of total rents from an average temporary agency assignment before and after the equal-pay reform. Parameter values are computed for a 117-day job duration ($s_a = 1/117$) with an in-house wage of €60/day ($w_h = 60$) and an agency wage of €53/day ($w_a = 53$). The daily discount rate is 0.015% ($r = (1+0.055)^{1/365}-1$), equivalent to an annual rate of 5.5%. Productivity is set to $\varphi = 89$ €/day, implying a markdown of $\varphi/w_h = 1.5$. The observed fee is €58/day ($f = 58$), which according to equation (8) yields a bargaining weight of $\gamma \approx 0.14$. Under the equal-pay rule ($w'_a = w_h = 60$ €/day) and a gross fee of €65/day ($f' = 65$), the implied bargaining weight remains $\gamma' \approx 0.17$. Productivity and job duration are held constant, so the total rent R^A is unchanged; the reform only redistributes rents among workers, agencies, and user firms.

When the equal-pay rule is imposed ($w'_a = w_h = 60$ €/day), the higher wage alters the bargaining outcome between agencies and user firms. Since productivity and job duration of temporary agency workers do not show clear changes according to our empirical results, the total rent R^A is assumed to be constant.[42] Surviving agencies passed 100% of the additional labor cost (7 €/day) to user firms through higher fees ($f' = 65$ €/day), while their bargaining power increases from $\gamma = 0.14$ to $\gamma' = \frac{f'-w'_a}{\varphi-w'_a} = \frac{65-60}{89-60} \approx 0.17$. This outcome indicates that agencies were able to maintain their negotiating position despite tighter regulation. The adjustment thus redistributed existing rents—raising the worker's share from 59.6% to 67.4%, keeping the agency's share at 5.6%, and lowering the user firm's share from 34.8% to 27.0%.

[41] For further details on the markdown estimation, see Appendix OD.

[42] According to the descriptive statistics in Table 3, the average contract length of temporary agency workers remains nearly constant at approximately 120 days. This pattern is further illustrated in Figure E3 in Appendix E.2, which shows that incumbent temporary agency workers do not experience longer job durations unless they transition to in-house positions. In terms of productivity, the empirical analysis in Section E.3 shows weekly positive effects on job quality for in-house firms.

In summary, the model quantifies rent-sharing within a triangular employment structure in which workers, agencies, and user firms jointly divide the surplus from temporary assignments. Agency contracts generate substantial rents, with roughly 40% accruing to intermediaries and user firms jointly before the reform. The equal-pay mandate increases the worker's share and compresses the user firm's portion, while the agency's share remains unchanged due to the full fee pass-through. Unlike standard two-party models of rent-sharing (Card et al., 2013, 2018; Kline et al., 2019; Lamadon et al., 2022), this framework captures how regulation reallocates rents across three actors and links those changes to organizational boundaries: as the regulatory wedge narrows the agency–firm margin, firms find it more profitable to internalize employment, leading to a broader process of de-fissuring.

7 Additional Results, Placebos and Robustness

7.1 Additional Results

Results for incumbent workers: As a robustness check, Appendix E.2 implements an alternative time-shifted difference-in-differences design that follows the employment outcomes of incumbent temp agency workers before and after the reform and compares them with workers employed by temp agencies in the same months one year earlier. This approach focuses on directly affected workers, rather than province-level averages. The evidence goes in the same direction as the employment results discussed in Section 5.2.2. It also shows the increases in job duration discussed in Section 5.2.3.

Worker and Firm Quality: Using pre-reform AKM estimates of worker and firm fixed effects, we find weak evidence of an increase in average worker quality in in-house firms (Appendix E.3). There is no persistent change in average worker quality in temp agencies. Firm effects are small and not persistent.

Effects on User Firms: Using sector-level data from the Central de Balances of the Bank of Spain, we examine how user firms adjusted to higher labor costs following reform. Despite the rise in wages and fees paid to temporary agencies, we find no significant changes in productivity or profitability across sectors with greater initial exposure to agency work. This pattern is consistent with improvements in allocative efficiency offsetting the higher labor costs faced by user firms. Additionally, other firm-level margins such as markups, markdowns, and exit rates remained stable, indicating that the reform primarily redistributed rents without disrupting production efficiency.[43] The results are in Appendix E.4.

[43] The rent-sharing calculations pertain to surplus division within agency matches, whereas the markdown estimates here refer to user firms. These are distinct objects: the rent-sharing results do not imply a change in markdowns for user firms. In fact, we observe a decline in markdowns following the reform—consistent

Effects on Temporary Agencies: In Appendix E.5, we analyze the impact of the 1999 reform on the temporary work agency sector using sector-level data. Consistent with the results on temp agency employment, the reform sharply contracted the sector: the number of agencies, sales, value added, and labor costs fell dramatically.

Internal Migration: In Appendix E.6, we show that the increase in migration toward high–agency-incidence provinces was driven by Spanish citizens, not by workers of immigrant background. This pattern rules out a reallocation of international migrants as the explanation for the absence of negative employment effects.

7.2 Placebos and Robustness

Individual-Level Placebo Tests: We conduct permutation tests to validate the causal interpretation of our individual-level wage estimates from Section 5.1. After excluding temporary agency workers, we randomly reassign treatment status among in-house employees while preserving the original treatment share, repeating this process 300 times on a 25% subsample for computational feasibility. The results, detailed in Appendix OA, show that none of the placebo replications produced coefficients or t-statistics as extreme as our actual estimates, with all empirical p-values effectively zero. This provides strong evidence that the documented wage effects represent genuine causal relationships rather than statistical artifacts.

Province-Level Placebo Tests: For our province-level analyses of wage spillovers, employment, and job security effects, we extend the randomization approach to account for continuous treatment intensity. We re-estimate our main specifications 1,000 times while randomly permuting treatment intensity across all 50 provinces. These results, presented in Appendix OB, show a similar pattern, where most of the actual estimates are extreme outliers compared to the distribution of placebo estimates, reinforcing the causal interpretation of our province-level findings.

Uncensored contributions: Our administrative dataset includes information on the contribution base. As is common in administrative data, earnings are both top- and bottom-coded, with the corresponding caps varying over time and across occupational groups. In a robustness exercise, we replicate our main wage exercises after applying an uncensoring procedure similar to that of Bonhomme and Hospido (2017), which imputes daily earnings for censored observations.[44] Appendix OC describes the uncensoring procedure and shows

with the wage spillovers documented later—although this effect is not persistent.

[44] An alternative approach to uncensoring income data from the MCVL is to use its tax module, as implemented by Roca and Puga (2017). Unfortunately, this module is only available from 2004, which lies outside the temporal scope of our analysis.

that our estimated direct wage and spillover effects remain unchanged.

8 Conclusions

This paper has examined how extending collective bargaining coverage can reshape outcomes in fissured workplaces, using the 1999 Spanish reform that required temporary agency workers to receive the same collectively bargained wages as in-house employees. The reform eliminated a 15% wage gap for agency workers and generated additional wage gains for already-covered in-house employees—together constituting a sizable coverage premium. This premium captures the wage effect of making stronger collective agreements binding for workers previously covered by weaker ones, a parameter central to understanding wage dynamics in coordinated bargaining systems such as those of most OECD countries. Although agency employment fell, in-house and overall employment increased, suggesting that the reform corrected monopsonistic distortions and induced firms to internalize employment relationships. The higher wages were largely passed through to user firms, with agencies absorbing narrower margins, implying a reallocation of rents toward workers without meaningful efficiency losses.

Looking ahead, our findings open several avenues for future research. First, new evidence from other extensions of collective bargaining—across countries and institutional settings—would help assess the external validity of our results. Second, an important question is whether similar extensions can strengthen wage-setting institutions for other fissured workers—such as subcontracted or platform workers—whose coverage under collective bargaining tends to be weaker or more fragmented. Beyond wages, future work could examine the effects of bargaining extensions on non-wage outcomes, including health, safety, and career progression. Finally, it would be valuable to compare the effectiveness of collective bargaining extensions with alternative regulatory approaches to the gig economy—such as minimum earnings rules, algorithmic transparency mandates, or direct employment requirements—to understand which instruments best balance worker protection with flexibility and efficiency.

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Appendix

Contents

A Theoretical model

We develop a directed-search model in which workers actively choose the submarket to which they apply—either the in-house channel (h) or the agency channel (a). Each submarket is characterized by its own Cobb–Douglas matching technology, so that vacancies and applicants meet according to standard matching functions with market-specific tightness. Conditional on a match taking place, wages are determined through Nash bargaining between the worker and the employer in that submarket. In the agency channel, in addition to the wage bargain between the worker and the agency, the fee paid by user firms to agencies is also determined endogenously through a Nash bargaining process between the two parties.

Our theoretical contribution relates to the literature on domestic outsourcing (e.g., Bilal and Lhuillier, 2021; Spitze, 2022; Bergeaud et al., 2024). In these models outsourcing reallocates labor from high-productivity firms to contractor firms. This raises aggregate employment by lowering the marginal cost of labor, but reduces in-house labor demand, limits wage growth at high-productivity firms, and generates a wage penalty for outsourced workers. As a result, outsourcing compresses the wage distribution across firms, shifting employment toward lower-paying employers.

Relative to this literature, our framework departs in two important ways. First, we explicitly model fee negotiation between temporary help agencies and user firms, so that the distribution of rents across intermediaries depends on their relative bargaining power. Second, the possibility of outsourcing through agencies enters as an outside option in the wage bargaining between user firms and their in-house workers. Together, these features allow us to evaluate an equal-pay mandate that eliminates the wage wedge driving lower contractor pay in standard frameworks, while also capturing how the distribution of bargaining power across firms shapes outsourcing outcomes, labor costs, and the effects of regulation.

Environment and technology

Time is continuous and there is a discount rate $r > 0$. There is a mass n_h of identical user firms and a mass n_a of identical temporary help agencies. A user firm can fill jobs either in-house (indexed by h) or by hosting workers supplied by an agency (indexed by a). Per-worker productivity is φ. We normalize the output price to one. Let e_h denote total in-house employment across user firms and e_a total agency-supplied employment currently hosted by user firms; total employment at user firms is $e \equiv e_h + e_a$. The labor force is normalized to one, so $1 = u + e$, where u is unemployment. Aggregate output produced by the user-firm sector is linear in effective employment from both channels:

$$Y = \varphi(e_h + e_a), \qquad e = e_h + e_a, \qquad 1 = u + e, \tag{9}$$

where $y = \frac{Y}{n_h}$ is the output per user firm.

Directed search and matching

Unemployed workers choose a submarket to apply to. Let the share $\sigma_h \in [0,1]$ apply to the in-house market; the agency share is $\sigma_a = 1 - \sigma_h$. Then the mass of applicants is $u_h = \sigma_h u$ and $u_a = (1 - \sigma_h)u$. Each market runs its own Cobb–Douglas matching function:

$$m_i(u_i, v_i) = \mu_i\, u_i^{1-\eta} v_i^{\eta}, \qquad \eta \in (0,1),\ \mu_i > 0, \tag{10}$$

with tightness $\theta_i \equiv v_i/u_i$ and implied job-finding and filling rates

$$p_i(\theta_i, \sigma_i) = \sigma_i \frac{m_i}{u_i} = \sigma_i \mu_i\, \theta_i^{\eta}, \qquad q_i(\theta_i) = \frac{m_i}{v_i} = \mu_i\, \theta_i^{\eta-1}. \tag{11}$$

Similar to Neugart and Storrie (2006), we assume that temporary agencies have a more efficient matching technology than user firms, implying that $\mu_a > \mu_h$.

Stocks, flows, and steady state

Separations occur at rates s_h and s_a. Since temporary agency jobs typically involve shorter contract duration and greater turnover, we assume $s_a > s_h$, so that agency matches dissolve more quickly than in-house matches. Aggregate unemployment evolves as

$$\dot{u} \;=\; s_h e_h + s_a e_a - \big[p_h(\theta_h, \sigma_h) + p_a(\theta_a, \sigma_a)\big] u. \tag{12}$$

In steady state:

$$e_h = \frac{p_h(\theta_h, \sigma_h)}{s_h}\, u, \tag{13a}$$

$$e_a = \frac{p_a(\theta_a, \sigma_a)}{s_a}\, u, \tag{13b}$$

$$u = \frac{1}{1 + \dfrac{p_h(\theta_h, \sigma_h)}{s_h} + \dfrac{p_a(\theta_a, \sigma_a)}{s_a}}. \tag{13c}$$

The job-finding rates $p_h(\theta_h, \sigma_h)$ and $p_a(\theta_a, \sigma_a)$ directly determine the balance between employment and unemployment in steady state. A higher in-house job-finding rate p_h reduces unemployment u by raising the inflow into in-house jobs, which increases e_h while at the same time lowering e_a through the flow balance. Symmetrically, an increase in the agency job-finding rate p_a reduces u and raises e_a, while reducing e_h. In both cases, tighter labor market conditions in either submarket decrease unemployment and expand overall employment, but they also reallocate workers across channels depending on which job-finding rate improves.

Workers value functions

An unemployed worker in submarket i receives the flow benefit b (such as unemployment compensation or home production). With arrival rate $p_i(\theta_i, \sigma_i)$ they find a job in that submarket, which generates the expected capital gain $(W_i - U_i)$. Thus, higher market tightness θ_i or a greater application share σ_i increases the value of unemployment, as they raise the likelihood of leaving unemployment. The unemployed value function is

$$rU_i \;=\; b + p_i(\theta_i, \sigma_i)\big(W_i - U_i\big). \tag{14}$$

An employed worker in submarket i receives the wage w_i per unit of time. However, the job is subject to a Poisson separation hazard s_i, which leads back to unemployment and entails the expected capital loss $(W_i - U_i)$. Hence, shorter job duration (a higher s_i) reduces the value of employment in that submarket. The employed value function is

$$rW_i \;=\; w_i - s_i\,(W_i - U_i). \tag{15}$$

User firm value functions

A user firm can obtain labor either by hiring an in-house worker or by hosting a worker supplied by a temporary help agency. We first describe the value functions associated with in-house hiring.

A filled in-house job yields a flow surplus equal to productivity minus the wage, $\varphi - w_h$. With separation hazard s_h, the match is destroyed and the firm loses J_h but regains the vacancy value V_h. This implies

$$rJ_h \;=\; \varphi - w_h \;-\; s_h(J_h - V_h). \tag{16}$$

An open in-house vacancy costs κ per unit of time while unfilled. With arrival rate $q_h(\theta_h)$, it is matched and becomes a filled job, generating the capital gain $(J_h - V_h)$. Hence

$$rV_h \;=\; -\kappa \;+\; q_h(\theta_h)\,(J_h - V_h). \tag{17}$$

Under free entry, firms post vacancies until their expected value is driven to zero, $V_h = 0$. Combining (16) and (17) gives the job-creation condition

$$\varphi - w_h \;=\; (r + s_h)\,\frac{\kappa}{q_h(\theta_h)}. \tag{18}$$

Equation (18) states that the net surplus from a filled job, $\varphi - w_h$, must equal the expected cost per filled vacancy. Intuitively, firms open vacancies until the profitability of an additional filled job is exactly offset by the expected recruiting cost.

If the user firm hires a worker supplied by an agency, it receives productivity φ but pays the fee f for as long as the match lasts. Importantly, the firm does not incur any vacancy-posting or recruiting costs in this case, since the agency performs the search on its behalf. The value of hosting an agency worker therefore satisfies

$$rJ_u \;=\; \varphi - f - s_a J_u. \tag{19}$$

The firm accepts the agency worker if and only if $f \leq \varphi$.

Temporary agencies value functions

Temporary help agencies operate as intermediaries: they recruit workers on their own account and then supply them to user firms. From the agency's perspective, an active assignment has value J_a, while an open vacancy has value V_a.

An active assignment generates a fee f paid by the user firm, but the agency must pay the worker's wage w_a. With separation hazard s_a, the assignment ends and the agency loses J_a but recovers the vacancy value V_a. This yields

$$rJ_a \;=\; f - w_a - s_a(J_a - V_a). \tag{20}$$

An open agency vacancy costs κ per unit of time. With arrival rate $q_a(\theta_a)$, the vacancy is filled and becomes an active assignment, generating the capital gain $(J_a - V_a)$. Hence

$$rV_a \;=\; -\kappa + q_a(\theta_a)(J_a - V_a). \tag{21}$$

Under free entry, agencies post vacancies until their expected value is driven to zero, so $V_a = 0$. Substituting $V_a = 0$ into (20) and (21) delivers the zero-profit condition

$$f - w_a \;=\; (r + s_a)\,\frac{\kappa}{q_a(\theta_a)}. \tag{22}$$

Equation (22) shows that the agency fee margin, $f - w_a$, must exactly cover the expected recruiting cost per filled vacancy. The term $(r + s_a)$ reflects the effective discounting of future returns, combining the time preference rate r and the risk of separation s_a. The denominator $q_a(\theta_a)$ captures the ease of filling vacancies in the agency submarket: when the market is tighter (higher θ_a), it takes longer to fill a vacancy, which raises the expected cost per assignment. Thus, agencies enter until the fee margin compensates them for both recruiting costs and turnover risk.

Fee determination

The agency fee f is determined by a Nash bargaining process between the user firm and temporary agency, where the agency has an explicit bargaining power γ, and the user firm $1-\gamma$.

Therefore, the agency fee must satisfy the following condition:

$$f = argmax\{[J_u]^{(1-\gamma)}[J_a - V_a]^{\gamma}\}. \tag{23}$$

Differentiating with respect to f and reorganizing, the following agency fee is obtained:

$$f = \gamma\varphi + (1-\gamma)w_a, \tag{24}$$

This equation states that the price the user pays for an agency worker is a weighted average between the worker's productivity (φ) and the worker's wage (w_a). The agency pushes the fee closer to productivity, while the user pushes it closer to the wage. Intuitively, the fee must lie above the wage (so that the agency covers recruitment costs) but below productivity (so that the user gains from hiring). The bargaining power γ governs how the surplus from the match is divided: when the agency has more power, the fee approaches productivity; when the user has more power, it approaches the wage.

Wage determination

Similar to the one-tier wage collective bargaining scenario in Cardullo et al. (2020), we assume that wages are determined by a Nash bargaining process between a representative worker from the sectoral union and the representative firm from the sectoral confederation, where the representative sectoral worker has an explicit bargaining power β_i and the representative sectoral firm $1-\beta_i$. If there is an agreement between the representative worker and firm, workers and firms receive W_i and J_i, respectively. If bargaining fails, the worker receives U, while the firm obtains the vacancy value V_i. Moreover, since the user firm can also obtain an agency worker, the firm's disagreement payoff additionally includes the outside option of hiring through the agency channel. Assuming that such assignments arrive at rate $q_a(\theta_a)$ and deliver value J_u once realized, the firm's continuation value is

$$\Omega \; = \; \frac{q_a(\theta_a)}{r+q_a(\theta_a)}\, J_u, \qquad J_u \; = \; \frac{\varphi - f}{r+s_a}. \tag{25}$$

where Ω is the firm's outside option. So that the relevant surplus for Nash bargaining in the in-house market is given by $(W_h - U,\; J_h - V_h - \Omega)$.[45]

[45]Since vacancies arrive at rate q_a and the future is discounted at rate r, the expression $\frac{q_a(\theta_a)}{r+q_a(\theta_a)}$ captures the likelihood that the firm actually secures an agency worker in present-value terms. Multiplying this by

The wages derived from the Nash bargaining solution maximises the weighted product of the all employed workers and firms net return from the job match in each sector. Therefore, each type of wage must satisfy the following condition:

$$w_h = argmax\{[e_h(W_h - U)]^{\beta_h}[n_h(J_h - V_h - \Omega)]^{(1-\beta_h)}\}, \tag{26}$$

$$w_a = argmax\{[e_a(W_a - U)]^{\beta_a}[n_a(J_a - V_a)]^{(1-\beta_a)}\}. \tag{27}$$

Substituting the firms and workers value functions, differentiating with respect to w_i and reorganizing, the following wages are obtained:

$$w_h = (1 - \beta_h)b + \beta_h(\varphi + \kappa\,\sigma_h\theta_h - (r + s_h)\,\Omega)\,, \tag{28}$$

$$w_a = (1 - \beta_a)b + \beta_a\big(f + \sigma_a\kappa\,\theta_a\big). \tag{29}$$

Equations (28)–(29) show that wages in both submarkets are determined as a weighted average between the worker's fallback value b and the revenue generated in the match, with the weights reflecting bargaining power. In the in-house channel, the relevant revenue is the worker's productivity φ, but the wage also incorporates two adjustments. First, part of the expected recruiting cost $\kappa\sigma_h\theta_h$ is passed on to the worker when markets are tighter or more workers apply to the in-house queue. Second, the wage is reduced by the firm's outside option Ω, which captures the possibility of using an agency worker rather than an in-house employee. A stronger agency option weakens in-house workers' bargaining position and lowers their pay. Importantly, $\Omega = \frac{q_a(\theta_a)}{r+q_a(\theta_a)}\frac{\varphi-f}{r+s_a}$ depends negatively on agency tightness: as θ_a increases, the vacancy fill rate $q_a(\theta_a)$ falls, so it becomes harder for user firms to secure an agency worker. Thus, higher θ_a reduces the value of the outside option and mitigates its depressing effect on w_h. At the same time, tighter agency markets tend to raise the fee f, which further lowers Ω.

In the agency channel, the revenue side is the fee f, and the same logic implies that tighter agency markets (higher $\sigma_a\theta_a$) allow workers to extract part of the recruiting cost in higher wages. Moreover, since the wage w_a is increasing in the fee, a higher f directly raises the worker's share of the match surplus. Intuitively, when agencies negotiate a higher fee with user firms a fraction β_a translates into higher wages.

Notice that wages of agency workers (w_a) will be lower than those of in-house workers (w_h) when the former have weaker bargaining power ($\beta_a < \beta_h$). This difference in bargaining power naturally translates into a wage gap, as agency workers capture a smaller share of the

J_u gives the expected continuation value of the firm's outside option.

match surplus. The assumption is particularly plausible prior to the 1999 reform, when agency workers were not covered by the collective agreements of user firms and thus had limited bargaining leverage.

Search decision

When workers are unemployed, they must decide whether to apply to the in-house submarket or to the agency submarket. Because search is directed, workers allocate themselves across the two queues until their expected gains are equalized. Formally, when both markets are active, the indifference condition is

$$p_h(\theta_h, \sigma_h)\big(W_h - U_h\big) \;=\; p_a(\theta_a, \sigma_a)\big(W_a - U_a\big). \tag{30}$$

Using the Nash surplus splits and free-entry conditions, these worker surpluses can be written as

$$W_h - U_h = \frac{\beta_h}{1-\beta_h}\Big(\frac{\kappa}{q_h(\theta_h)} - \Omega\Big), \tag{31}$$

$$W_a - U_a = \frac{\beta_a}{1-\beta_a}\,\frac{\kappa}{q_a(\theta_a)}. \tag{32}$$

Substituting (31)–(32) into (30) yields the reduced-form search condition

$$\frac{\beta_h}{1-\beta_h}\Big(\kappa\,\sigma_h\,\theta_h \;-\; \mu_h\,\sigma_h\,\theta_h^{\eta}\,\Omega\Big) \;=\; \frac{\beta_a}{1-\beta_a}\,\kappa\,\sigma_a\,\theta_a. \tag{33}$$

Equation (33) states that workers choose application shares (σ_h, σ_a) such that the expected capital gain from joining either queue is the same. If one side delivers strictly higher returns, all search flows there and the other submarket shuts down.

For an interior solution $(0 < \sigma_h < 1)$ to exist in (33), both sides must be strictly positive. This requires (i) positive worker surplus in both markets, in particular $\kappa\sigma_h\theta_h - \mu_h\,\sigma_h\,\theta_h^{\eta}\Omega > 0$ so that the in-house option dominates the temporary agency option, and (ii) positive arrival rates in both queues, i.e. $p_h(\theta_h, \sigma_h) > 0$ and $p_a(\theta_a, \sigma_a) > 0$, which implies $\theta_h, \theta_a > 0$ and $\sigma_h \in (0,1)$. If these conditions fail, the solution collapses to a corner: all workers apply to the agency channel if $\kappa\sigma_h\theta_h - \mu_h\,\sigma_h\,\theta_h^{\eta}\Omega < 0$, or to the in-house channel if $p_a(\theta_a, \sigma_a) = 0$.

Equilibrium

Combining the agency wage equation (29), the fee rule (24), and the zero-profit (22) condition gives

$$(1-\beta_a)(\varphi - b) - \beta_a\,\sigma_a\kappa\theta_a = \frac{1-\beta_a(1-\gamma)}{\gamma}\,(r+s_a)\,\frac{\kappa}{\mu_a}\,\theta_a^{1-\eta}. \tag{34}$$

This condition pins down equilibrium agency tightness θ_a for given search share σ_a.

Combining the in-house job-creation condition (18) with the in-house wage (28) yields

$$(1-\beta_h)(\varphi-b)-\beta_h\,\sigma_h\kappa\,\theta_h+\beta_h(r+s_h)\,\Omega=(r+s_h)\,\frac{\kappa}{\mu_h}\,\theta_h^{1-\eta}. \tag{35}$$

This condition determines in-house tightness θ_h given the agency outside option Ω.

An equilibrium in this economy is characterized by the joint determination of market tightness in both submarkets (θ_h, θ_a) and the allocation of search effort $(\sigma_h, \sigma_a = 1-\sigma_h)$ according to (33) . Three conditions must be simultaneously satisfied. If one of these conditions fails (e.g. the in-house surplus turns negative), the solution collapses to a corner where all search flows to the feasible submarket.

Once $(\theta_h, \theta_a, \sigma_h, \sigma_a)$ are jointly determined, the rest of the variables follow directly from the model's structure. Wages w_h and w_a are obtained from the Nash bargaining rules (28)–(29), and the agency fee f from the bargaining outcome (24). Given these, the firm's outside option Ω is computed from (25). Steady-state employment levels (e_h, e_a) and unemployment u then follow from the flow balance equations (13a)–(13c). Finally, aggregate output Y and output per user firm y are obtained from the production relation in (9). Thus, the full equilibrium allocation can be recovered once tightness and search shares are pinned down.

We use the model to derive the qualitative predictions of an equal-pay mandate. In particular, we ask how imposing $w_a = w_h$ reshapes the equilibrium allocation across the two submarkets and affects the main variables of interest: wages, fees, agency and in-house employment, the user firm's outside option, job quality, productivity, and rent-sharing. The following subsection develops these predictions.

Equal-Pay Policy

We now analyze the equilibrium effects of an *equal-pay mandate* requiring identical wages for in-house and agency workers, $w_a = w_h \equiv w$. The adjustment unfolds sequentially but features endogenous feedbacks across submarkets. Specifically, we trace: (i) the initial rise in w_a; (ii) the response of agency tightness θ_a; (iii) the adjustment of the agency fee f; (iv) the user firm's outside option Ω; (v) the joint determination of in-house tightness and search allocation (θ_h, σ_h); (vi) the in-house wage w_h; and (vii) employment composition and aggregate outcomes.

Proposition 1 (Policy shock)**.** *The equal-pay mandate increases the agency wage up to the pre-reform in-house level, $w_a \uparrow w_h^0$. Defining the agency margin $x \equiv \varphi - w_a$, the reform produces a discrete drop in x, compressing the fee–wage wedge.*

Proof. Before the reform $w_a^0 < w_h^0$, and the policy sets $w_a = w_h^0$. Hence, x falls discontinuously. This mechanical wage increase initiates the sequence of equilibrium adjustments across the two submarkets. $\square$

Proposition 2 (Agency tightness)**.** *Using the agency zero-profit condition* (22) *and the fee rule* (24)*, agency equilibrium satisfies*

$$(r+s_a)\frac{\kappa}{q_a(\theta_a)} = \gamma(\varphi - w_a). \tag{36}$$

Given $q_a(\theta_a) = \mu_a \theta_a^{\eta-1}$*, we obtain*

$$(r+s_a)\frac{\kappa}{\mu_a}\,\theta_a^{1-\eta} = \gamma(\varphi - w_a), \qquad \Rightarrow \qquad \frac{\partial \theta_a}{\partial w_a} < 0.$$

Proof. Differentiating (36) yields $(r+s_a)\frac{\kappa}{\mu_a}(1-\eta)\theta_a^{-\eta}\frac{\partial\theta_a}{\partial w_a} = -\gamma$. Since $1-\eta > 0$, $\frac{\partial\theta_a}{\partial w_a} < 0$. Higher agency wages reduce expected profits from posting vacancies, so agencies cut vacancy creation, lowering market tightness. $\square$

Proposition 3 (Agency fee)**.** *From the Nash bargaining outcome* (24)*,* $f = \gamma\varphi + (1-\gamma)w_a$*, implying*

$$\frac{\partial f}{\partial w_a} = 1-\gamma \in (0,1), \qquad \frac{\partial (f - w_a)}{\partial w_a} = -\gamma < 0.$$

Proof. Differentiating the fee rule with respect to w_a gives the above expressions. Thus, the fee rises less than one-for-one with the agency wage, compressing the fee–wage margin $(f - w_a)$ that finances agency entry. $\square$

Proposition 4 (Outside option)**.** *Combining the definition of the outside option* (25)*, the fee rule* (24)*, and the zero-profit condition* (22) *to eliminate* f *and* q_a*, we obtain*

$$\Omega(x) = \frac{(1-\gamma)\kappa x}{r\gamma x + (r+s_a)\kappa}, \qquad x \equiv \varphi - w_a, \tag{37}$$

which implies $\frac{\partial \Omega}{\partial x} > 0$ *and hence* $\frac{\partial \Omega}{\partial w_a} < 0$*.*

Proof. Substituting f and q_a into (25) yields (37). A higher agency wage ($w_a \uparrow$) compresses the margin x, lowering Ω. Thus, user firms' outside option weakens as outsourcing becomes less profitable. $\square$

The in-house job-creation condition (35) and the worker indifference condition (33) jointly determine (θ_h, σ_h):

$$(1-\beta_h)(\varphi - b) - \beta_h\,\kappa(\sigma_h\theta_h) + \beta_h(r+s_h)\Omega = (r+s_h)\frac{\kappa}{\mu_h}\,\theta_h^{1-\eta}, \tag{38}$$

$$\frac{\beta_h}{1-\beta_h}\,\sigma_h\mu_h\,\theta_h^{\eta}\Big(\frac{\kappa}{\mu_h\theta_h^{\eta}} - \Omega\Big) = \frac{\beta_a}{1-\beta_a}\,(1-\sigma_h)\mu_a\,\theta_a^{\eta}. \tag{39}$$

Proposition 5 (Joint adjustment of θ_h and σ_h)**.** *Equations (38)–(39), with Ω given by (37), jointly determine $(\theta_h(w_a), \sigma_h(w_a))$. Because both conditions shift when w_a changes—through $\Omega(w_a)$ and $\theta_a(w_a)$—the total equilibrium responses satisfy*

$$\text{sign}\left(\frac{\partial\theta_h}{\partial w_a}\right) \text{ and } \text{sign}\left(\frac{\partial\sigma_h}{\partial w_a}\right) \text{ are in general ambiguous.}$$

Proof. An increase in w_a reduces θ_a (by (36)) and lowers Ω (by (37)). On the firm side (38), a lower Ω reduces vacancy profitability and, *holding σ_h fixed*, implies a lower θ_h (the partial effect). On the worker side (39), the same lower Ω raises the in-house surplus, while lower θ_a reduces the agency arrival rate; to restore indifference, the equilibrium may adjust via σ_h and/or θ_h in either direction. Because these adjustments occur simultaneously across both equations, the total derivatives $\frac{\partial\theta_h}{\partial w_a}$ and $\frac{\partial\sigma_h}{\partial w_a}$ cannot be signed without additional restrictions. □

Proposition 6 (In-house wage response)**.** *From the in-house wage equation (28),*

$$w_h = (1-\beta_h)b + \beta_h[\varphi + \kappa(\sigma_h\theta_h) - (r+s_h)\Omega]\,,$$

so that

$$\frac{\partial w_h}{\partial w_a} = \beta_h\left[\kappa\left(\theta_h\frac{\partial\sigma_h}{\partial w_a} + \sigma_h\frac{\partial\theta_h}{\partial w_a}\right) - (r+s_h)\frac{\partial\Omega}{\partial w_a}\right] \gtrless 0.$$

Proof. Differentiating (28) and using $\frac{\partial\Omega}{\partial w_a} < 0$ shows that the outside-option channel raises w_h, while the effect through $(\sigma_h\theta_h)$ is ambiguous. Hence, in-house wages generally increase after equal pay, though the strength of the adjustment depends on how θ_h and σ_h respond jointly. □

Proposition 7 (Employment and output)**.** *Using the steady-state relations (13a)–(13c),*

$$e_i = \frac{p_i(\theta_i,\sigma_i)}{s_i}\,u, \qquad u = \left[1 + \frac{p_h}{s_h} + \frac{p_a}{s_a}\right]^{-1},$$

the response of employment to an equal-pay policy satisfies

$$\text{sign}\left(\frac{\partial e_a}{\partial w_a}\right) \text{ and } \text{sign}\left(\frac{\partial e_h}{\partial w_a}\right) \text{ are generally ambiguous.}$$

In partial equilibrium (holding σ_a and p_h fixed), $\frac{\partial e_a}{\partial w_a} < 0$ because $\theta_a \downarrow$. Aggregate output $Y = \varphi(e_h + e_a)$ inherits the same ambiguity and decreases if the contraction in agency *employment dominates:*

$$\frac{\partial Y}{\partial w_a} = \varphi\left(\frac{\partial e_h}{\partial w_a} + \frac{\partial e_a}{\partial w_a}\right) < 0 \quad \textit{if} \quad \left|\tfrac{\partial e_a}{\partial w_a}\right| > \left|\tfrac{\partial e_h}{\partial w_a}\right|.$$

Proof. From $e_a = \frac{p_a}{s_a}u$ with $p_a = \sigma_a \mu_a \theta_a^\eta$, total differentiation yields

$$d\ln e_a = d\ln\sigma_a + \eta\, d\ln\theta_a + d\ln u.$$

The direct effect $\eta\, d\ln\theta_a < 0$ lowers e_a, but σ_a and u adjust endogenously. An increase in w_a reduces θ_a, which raises unemployment u and may reallocate search effort toward agencies ($\sigma_a \uparrow$) or away from them ($\sigma_a \downarrow$), depending on parameters. Hence, the total derivative $\frac{\partial e_a}{\partial w_a}$ is ambiguous in general equilibrium.

By the same reasoning, e_h depends on the joint response of (θ_h, σ_h, u), so its sign is also ambiguous. Aggregate output $Y = \varphi(e_h + e_a)$ falls when the reduction in e_a more than offsets any increase in e_h. □

In summary, the equal-pay mandate raises w_a, compresses the fee–wage wedge, lowers agency tightness θ_a, and weakens the outside option Ω. In-house tightness θ_h and worker allocation σ_h adjust jointly and ambiguously, while the weaker outside option tends to raise w_h. Although the general-equilibrium effects are theoretically ambiguous, employment is likely to reallocate from agencies toward in-house jobs, reflecting the contraction of the agency margin. The aggregate impact on total employment and output remains, therefore, uncertain.

Although productivity φ is constant in our theoretical framework, the policy may influence effective productivity through compositional and behavioral channels. Consistent with Hirsch and Mueller (2012), a smaller agency share could reduce flexibility and screening capacity, increasing mismatch and lowering efficiency. Conversely, a larger in-house share may enhance morale, reduce turnover, and promote firm-specific human capital accumulation. In this model, such mechanisms correspond to exogenous shifts in φ, which would in turn raise market tightness, job-finding rates, and employment when efficiency improves, and reduce them when it declines.

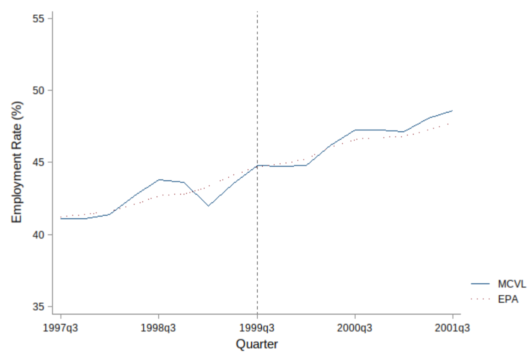
B A Comparison of Survey and Administrative Data

The first MCVL dataset was sampled in 2004 and it contains the entire labor history of the individuals included in each edition. Nevertheless, the policy we study was implemented in 1999. Thus, we use the retrospective information of the 2004-2008 samples for the analysis, which may be problematic if it fails to capture the dynamics of the Spanish labor market a few years before.

In this section, we compare the MCVL with data from the Spanish Labor Force Survey (SLFS) and show that both samples capture very similar labor market patterns. In particular, we plot the evolution, before and after the policy, of the employment rate for the MCVL sample of workers we use in the analysis, and that of the SLFS.

We display the results in Figure B1. As can be seen, the employment rates of the SLFS and the MCVL are almost identical and capture in a similar way the events in the labor market of the time.

Figure B1: **Comparing Survey and Administrative Data**



(a)

Notes: The figure shows the evolution of the employment rate based on the SLFS (dotted line) and the sample of workers from the MCVL (blue solid line). The vertical dashed line depicts the quarter of the reform. Sources: MCVL and the SLFS.

C Comparison with Other Equal-Pay Policies

For a correct interpretation of the reform, it is important to bear in mind that the Spanish law provided no exemptions. In other words, it acted to protect the broad coverage and extension of the collective bargaining system, even in the case of, at the time, a new form of business organization. Therefore, employers had no legal path to avoid the regulation, which probably magnified the positive effects of the reform on wages, but also any negative impact on employment. This is in contrast to similar laws regulating temp agency work, outsourcing or the gig economy, such as the Hartz I law in Germany, the EU Directive 2008/104/EC, or the California Assembly Bill 5, which introduce exceptions that limit the impact of the law.

For example, the Hartz I law granted the right to equal pay between in-house and temp agency workers, *unless* the latter were already covered by a collective labor agreement. The first Hartz law was passed in 2002 and by then the temp agency sector had very few collective agreements. It went into effect in 2004 and, by the end of 2003, 97% of temp agencies paid according to a sectoral collective agreement. Jahn (2008) analyzes the reform and shows that most temp agencies used the exemption to avoid paying wages as those agreed between user firms and their in-house workers.[46] There are also similar exceptions in the European implementation of temp agency work (EU Directive 2008/104/EC), which gave member states leeway in their national application of the principle of equal treatment. For instance, in Germany the principle is applied only after a worker has been employed for more than nine months at the same user company.[47] Additionally, some countries allow collective agreements to set different pay scales for agency workers compared to in-house employees. It is the case of Germany, France, Sweden, Poland, and Hungary.[48]

D Additional Descriptive Evidence

D.1 Summary Statistics for Firms

Table D1: Summary Statistics for Firms

	Pre-Period Temporary Agencies		Post-Period Temporary Agencies		Pre-Period User Firms		Post-Period User Firms	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Net Sales	9546.549	46072.183	12879.548	74659.968	4680.940	89896.720	4212.500	96090.095
Value Added	8751.560	42850.905	12005.718	69548.660	1301.801	39175.286	1076.775	30312.191
Profits	384.275	2788.432	391.598	2228.423	584.704	25797.480	478.166	20472.263
Purchases to Other Companies	92.876	322.477	48.402	156.315	2869.399	57087.500	2709.130	70592.957
In-House Labor Costs	8367.286	40204.493	11614.121	67359.803	717.098	15461.591	598.609	12534.117
Workers	395.973	2091.801	517.791	2804.751	20.857	354.315	18.266	309.293
Markup					1.263	0.379	1.243	0.368
Markdown					1.244	1.029	1.291	1.031
Observations	138		146		127314		173218	

Notes: The table shows summary statistics for temporary agencies and user firms. For each of them, we report sample means, standard deviations and number of observations, for the year before the reform (1998) and the year after (2000). Monetary values are expressed in thousands of 2015 euros. Markups and markdowns are derived from slightly different samples of firms due to stricter methodological requirements for their computation. For temporary agencies, markups and markdowns cannot be computed because of the reduced number of available observations. Sources: *Central de Balances Integrada* (BELab, 2025).

[46] We find similar exceptions in wage-setting for outsourced workers. See article 42.1 in *Estatuto de los Trabajadores*, the main labor law in Spain.

[47] In the Netherlands or the UK (before Brexit), temp agency workers accrue more rights over time, with an initial phase of 78 or 12 weeks, respectively.

[48] The introduction of exemptions to laws that define wage-setting and benefit standards is also common feature of laws that regulate the gig economy. For instance, California Assembly Bill 5 had exemptions on the test used to determine if a worker is an employee or an independent contractor. The California Assembly Bill 2257 amended it and expanded the number of exemptions. Similarly, the Spanish Riders' law considered that food delivery workers were not independent contractors, but did not cover other workers in the gig economy.

D.2 Summary Statistics for Collective Agreements

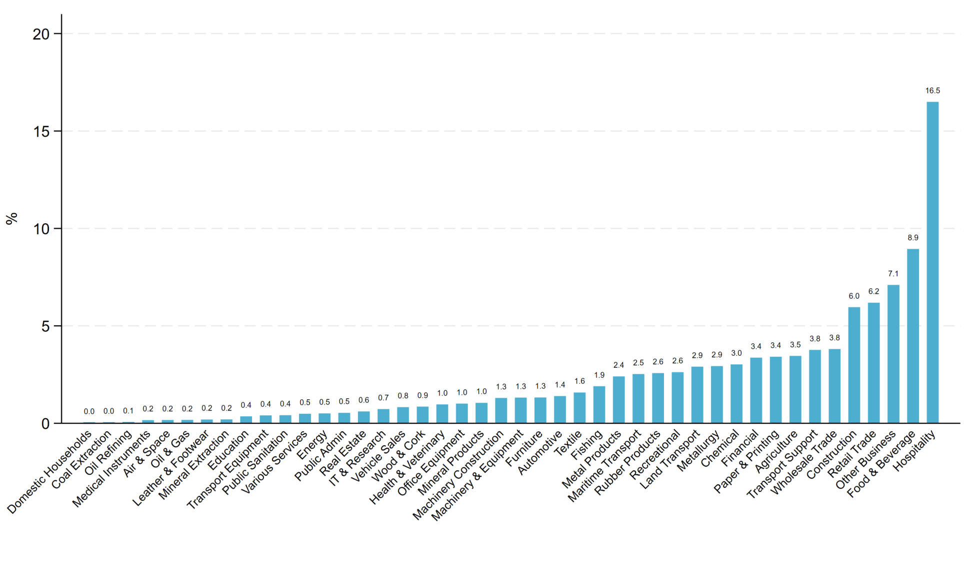
Table D2: Summary Statistics for CBA, 2015 euros

	Pre-Period Temporary Agencies		Post-Period Temporary Agencies		Pre-Period User Firms		Post-Period User Firms	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Engineers and Graduates	16733.785	.	16093.292	.	28317.875	17521.222	27319.932	16848.891
Technical Engineers and Experts	16157.352	.	15538.923	.	23369.862	12787.706	22665.475	12432.171
Administrative and Workshop Managers	14109.722	.	13569.666	.	23021.278	17192.282	22395.683	16535.097
Unqualified Assistants	12457.852	.	11981.021	.	20198.843	11422.060	19572.058	10973.459
Administrative Officers	11287.777	.	10855.732	.	20730.986	21856.831	20247.218	21164.054
Junior Staff	9532.665	.	9167.799	.	15342.404	7273.322	14973.623	7157.634
Administrative Assistants	9532.665	.	9167.799	.	14919.155	7274.188	14509.470	7082.110
First and Second Category Officials	9919.822	.	9540.137	.	20444.877	26335.848	19772.094	24483.641
Third Category Officials and Specialists	9532.665	.	9167.799	.	16015.877	10191.674	15622.556	9690.770
Unskilled Laborers	9532.665	.	9167.799	.	13233.093	6120.478	12989.358	5982.114
Total	11879.697	2855.667	11424.997	2746.365	19386.172	16283.522	18847.866	15513.069
% Sector	100.000		100.000		23.613	42.471	22.382	41.681
Observations	10		10		42260		48239	

Notes: The table reports summary statistics for wage categories in collective bargaining agreements (CBAs) covering temporary agencies and user firms. For each group, we present sample means, standard deviations, and the number of observations for the year before the reform (1998) and the year after (2000). The "% Sector" indicates the proportion of sectoral-level agreements in the sample. Note that for temporary agencies, only one sectoral agreement exists per year; therefore, no standard deviation is reported. Monetary values are expressed in 2015 euros. Sources: Census of Collective Bargaining Agreements.

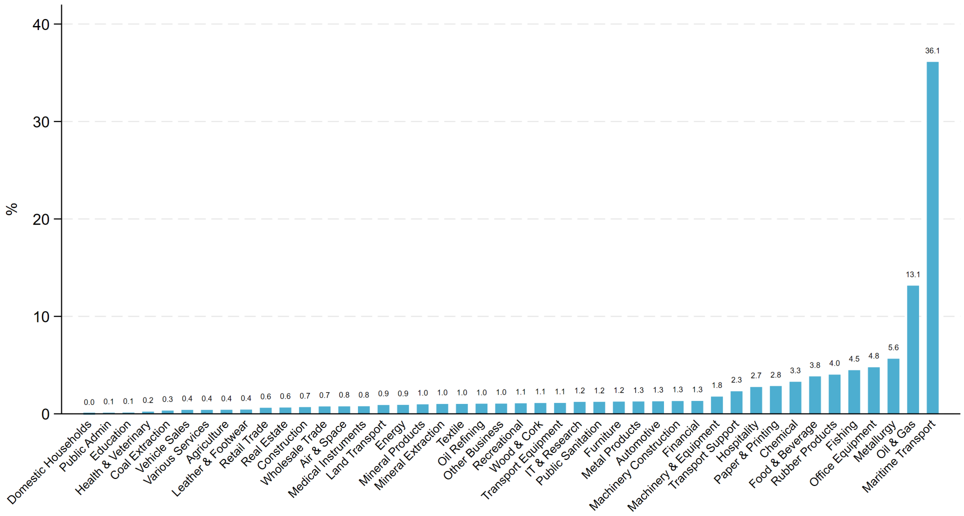
D.3 Distribution of Temporary Agency Contracts Across Sectors

Figure D1: **Sectoral Distribution of Temporary Agency Contracts, Relative to Total Temp Agency Contracts (1998)**



Notes: The figure displays the sectoral distribution of temporary agency contracts in 1998 as a percentage of the total number of temporary agency contracts in 1998. Sources: *Empresas de trabajo temporal*.

Figure D2: **Sectoral Incidence of Temporary Agency Contracts, Relative to Total Contracts in the Sector (1998)**

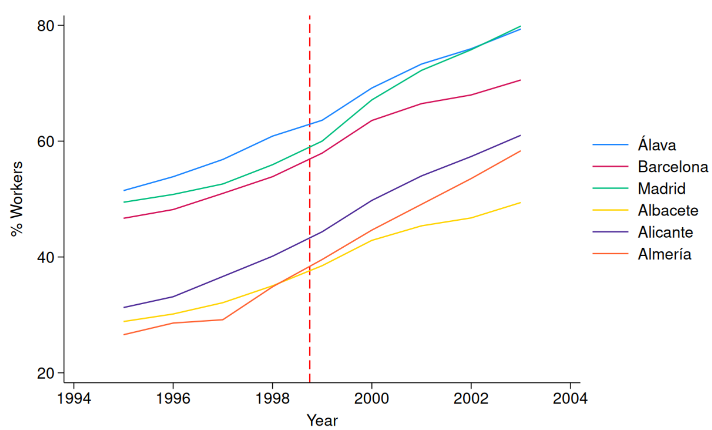


Notes: The figure reports the percentage of temporary agency contracts within each sector in 1998, calculated over sector's employment. This cross-sector variation is used in Sections E.4 and E.5 to estimate the impact of the reforms on firms. Sources: *Empresas de trabajo temporal* and *Instituto Nacional de Estadística*.

E Additional Results

E.1 Evolution of Overall Employment

Figure E1: **Evolution of Overall Employment**



(a) Employment

Notes: The figure shows the percentages of overall employment relative to province active population at baseline. We display it for three provinces with high temp agency incidence (Álava, Barcelona and Madrid), and three provinces with low temp agency incidence (Albacete, Alicante, Almería). Sources: MCVL and *Instituto Nacional de Estadística*.

E.2 Employment Effects for Incumbent Temp Agency Workers

In this section, we present an alternative empirical strategy to the difference-in-differences used in the main text (Equation 4). The empirical challenge we face to understand how the obligation to pay temp agency workers according to the collective agreement of user firms is that the new law was national, and there is no obvious contemporaneous control group.

To circumvent this problem, we resort to a time-shifted difference-in-differences. That is, the treatment group are workers employed by temp agencies the month before the reform (July 1999), and the control group is composed of workers employed by temp agencies 12 months before (July 1998). For each group of workers, we include data for 6 months after July 1999 (or July 1998 for the control), and 5 months before. Therefore, we follow the treatment group between February 1999 and January 2000. Similarly, we follow the control group between February 1998 and January 1999.[49] The identification assumption is that, in the pre-July months, both groups are following parallel trends, and that absent the policy they would have continued to follow parallel trends in the post-July month. Figure E2, Panel (a), provides evidence that supports this assumption. It plots the share of temp agency employees for both the treatment and control workers. In the x axis, -1 refers to July, the month before the reform (or the placebo reform). The blue solid line is for the treatment group (the workers we follow through the year 1999), and the red-dashed line is for the control group (the workers we follow through the year 1998). As can be seen, before August, both lines are almost on top of each other and evolve in parallel, which suggests we can use incumbent temp agency workers in July 1998 as a control group for incumbent temp agency workers in July 1999. We can also see that the lines separate after the month of August, with the treatment group facing a stronger decline in temp agency employment. As long as there is no other change in August 1999 that affects the treatment group, and that did not happen in August 1998, we can interpret the estimates in this section as the causal effects of the policy change. The specification is:

$$\begin{aligned} y_{ipmt} = \alpha + \delta_i + \delta_m + \delta_p + \sum_{j=-5, j\neq -2}^{5} \beta_j 1[j=m] Treatment_i + \\ + \mu unemp_{pt} + \epsilon_{ipmt} \end{aligned} \tag{40}$$

where y_{ipmt} is a dummy variable that reflects whether individual i, m months away from July 1999 (or July 1998 for the control group), is employed in a temp agency or in-house contract. We also include as outcome variables a dummy indicating if the temp agency worker

[49] In a time-shifted difference-in-differences, it can be that the same individual is both in the treatment and the control group. We encode these cases as different persons.

transitions to an in-house job, and the duration of the current employment contract each month. δ_i, δ_p, and δ_m are individual, province and month-distance fixed effects. $Treatment_i$ is equal to 1 for the sample of workers employed in temp agencies in July 1999, and 0 for those employed by a temp agency in July 98. $unemp_{pt}$ is the unemployment rate in province p at time t.[50]

Figure E2 displays the estimates of the event study specification. The coefficients are in Table E1.[51] We extract three remarks from these results. First, there is no sign of pre-treatment trends for any of the outcome variables. Second, the evidence for incumbent workers is consistent with the province-level analysis, and hence provides a robustness check for the results in Section 5.2.2. In particular, the reform decreases employment in the temp agency sector (Figure E2, Panel (b)). The negative effect is a significant -6.1pp on average (Table E1). In contrast, in-house employment increases by the same amount. The effect on overall employment is positive, small, insignificant, and equal to -0.03pp. Consistent with the previous results, the number of transitions from temp agency to in-house jobs increases significantly by 0.94pp (or 33% relative to the pre-policy mean).

Third, the analysis for incumbent workers reveals that the policy also improved other dimensions of job quality, such as job duration and employment protection. Together with the effects on wages, we interpret this evidence as showing a complementarity between wages, job duration, and employment protection (Dube et al., 2022). Panel (f) shows that the duration of current contracts increases steadily after the reform, with average job duration rising by about 25% in the treatment group within five months. As shown in Figure E3, this effect is driven mainly by workers who transition from agency to in-house jobs, which are far more likely to be open-ended. Indeed, the reform raised the share of permanent contracts by 2.5pp (41% of the increase in in-house employment). These open-ended contracts, which have no predetermined end date and carry severance entitlements of 45 days of pay per year worked in the case of wrongful dismissal, offer greater stability and stronger career prospects—helping explain the observed increase in job duration.[52]

Finally, there is no significant impact on unemployment insurance, which is consistent with the policy not having an effect on employment. We also report results for self-

[50] We omit period -2 because the condition for being on the sample is being in a temp agency employment when $t = -1$.

[51] The results shown in the table are based in the following specification: $y_{ipmt} = \alpha + \delta_i + \delta_m + \delta_p + \rho_1 Post_t + \rho_2 Treatment_i + \rho_3 Treatment_i \times Post_t + \mu unemp_{pt} + \epsilon_{ipmt}$, where $Post_t$ is equal to 1 for the months of August, September, October, November, December, and January, and 0 for the months of February, March, April, May, June, and July.

[52] In case of fair dismissal, the worker is entitled to 20 days of salary per year worked. However, Elias et al. (2025) shows that, for the years 1996-97, 77.9% of separations were wrongful layoffs, and fair dismissals accounted for only 0.47%. The remaining 21.63% were quits.

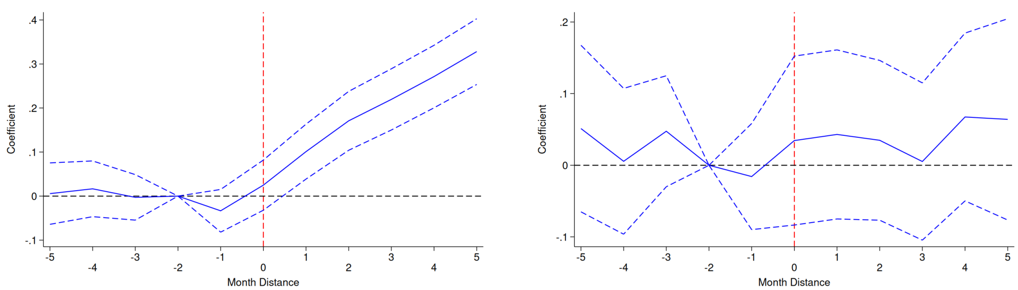
employment and public employment. We think these two variables can be interpreted as a robustness check of the main results, since we do not expect the policy to affect them. Consistent with that, we do not capture a significant effect for them.

Table E1: Effects on Incumbent Temp Agency Workers

	(1) Incumbents: Emp. in Temp Agencies	(2) Incumbents: Emp Open-Ended and Short-Term	(3) Incumbents: Employment	(4) Incumbents: Transition to In-House	(5) Incumbents: Log Job Duration
Treatment x Post	-6.105***	6.075***	-0.0294	0.935***	0.139***
	(0.715)	(0.638)	(0.621)	(0.147)	(0.0227)
Observations	158964	158964	158964	158964	110522
	Incumbents: Open-Ended Employment	Incumbents: Short-Term Employment	Incumbents: Unemployment Insurance	Incumbents: Self-Employment	Incumbents: Public Employment
Treatment x Post	2.477***	3.598***	-0.0913	0.0679	-0.0251
	(0.288)	(0.611)	(0.307)	(0.0964)	(0.0473)
Observations	158964	158964	158964	158964	158964

Notes: The table shows the employment effects of the reform for incumbent temp agency workers. The specification is a time-shifted difference-in-differences with individual, province, and month-distance fixed effects, as explained in Section E.2. The specification is equation 51. We also include the province unemployment rate as a control. Except for the job duration variable, the estimates have been multiplied by 100 so that they can be interpreted as percentage points. Robust standard errors clustered at the worker level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL

Figure E3: **Job Duration Heterogeneity Analysis**



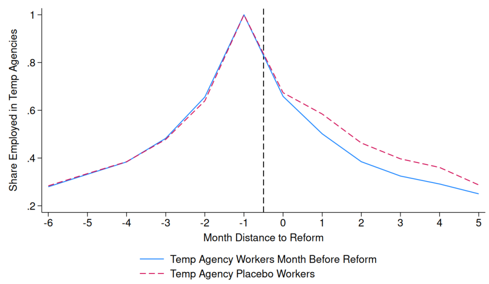
(a) Workers Who Transition **(b) Workers Who Stay**

Notes: Panel (a) shows the effects on job duration for incumbent temp agency workers who transition to in-house jobs. Panel (b) shows it for incumbent temp agency workers who stay working in the agency sector. The specification is a time-shifted dynamic difference-in-differences with individual, province, and month-distance fixed effects, as explained in Section E.2. The specification is equation 40. We also include the province unemployment rate as a control. The blue solid lines are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. Job duration is in log days worked. The vertical dashed line depicts the month of the reform. Sources: MCVL.

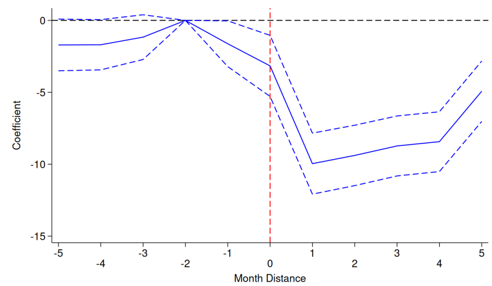
E.3 Productivity: Worker and Firm Quality

The evidence on job stability, contract type, and duration suggests that the reform improved job quality in the in-house sector. These improvements may have altered the composition of employment by attracting and retaining better workers in in-house jobs, while potentially

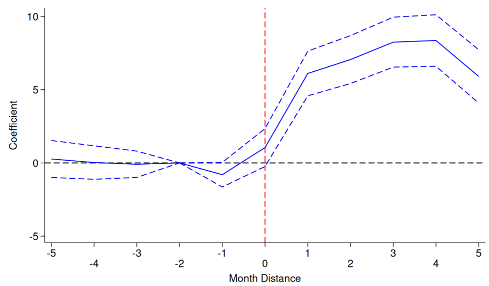
Figure E2: **Employment Effects. Sample of Temp Agency Workers a Month Before the Reform**



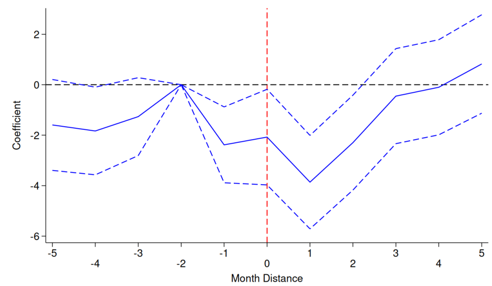
(a) Temp Agency Employment



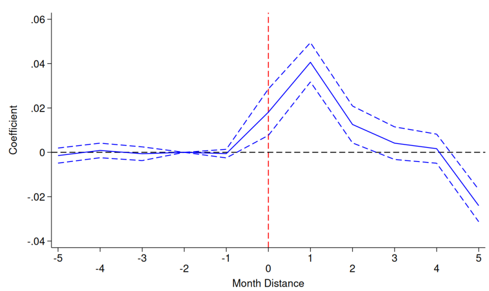
(b) Temp Agency Employment



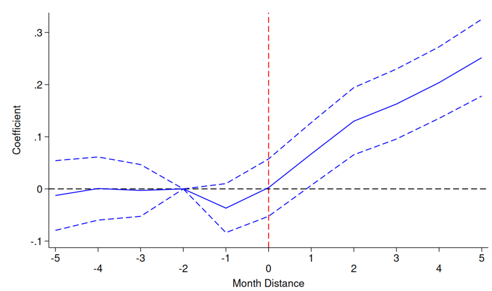
(c) In-House Employment



(d) Employment



(e) Transitions to In-House



(f) Job Duration

Notes: Panel (a) shows the share of temp agency employment for a sample of workers who got a temp agency contract the month before the reform (blue solid line), and for a placebo sample of workers who got a temp agency contract in July 1998 (13 months before the reform). Panels (b) to (f) show the results of a time-shifted dynamic difference-in-differences with individual, province, and month-distance fixed effects, as explained in Section E.2. The specification is equation 40. We also include the province unemployment rate as a control. The blue solid lines are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. Except for the job duration variable, the estimates have been multiplied by 100 so that they can be interpreted as percentage points. Panel (b) displays temporary agency employment, panel (c) in-house employment, panel (d) overall employment, panel (e) transitions from temp agency to in-house contracts, and panel (f) job duration in log days worked. The vertical dashed line depicts the month of the reform. Sources: MCVL.

leaving lower-quality matches concentrated in the temp agency sector. Beyond worker selection, the reform could also have influenced the composition of firms that remained active in the market (Dustmann et al., 2021).

To test these ideas, we estimate worker and firm components of wages using linked employer–employee data. We implement AKM models of wage determination with pre-policy data from 1990–1998, ensuring that the estimated fixed effects are not themselves influenced by the reform. To calculate the fixed effects, we follow the method in Bonhomme et al. (2023), which corrects for the endogeneity of worker mobility and limited mobility bias. We then link these estimated worker and firm effects to our difference-in-differences framework. As long as potential biases in fixed effects estimation are not systematically correlated with the shock variable and the policy change, the resulting estimates can be interpreted as causal.

The results are reported in Figure E4 and in Table E2. Overall, the reform had little impact on worker and firm quality. For in-house firms, the reform appears to have increased the average quality of workers, though the estimates are small in magnitude (Panel (a)). For temp agencies, we also detect an increase in worker quality, but the effect is short-lived and dissipates after the immediate post-reform years (Panel (b)). Turning to firm effects, we find declines in average firm quality both for in-house firms and for temp agencies (Panels (c) and (d)). These effects are, however, statistically insignificant and transitory, suggesting that the reform did not substantially alter firm quality.

Table E2: Effects on Worker and Firm Quality

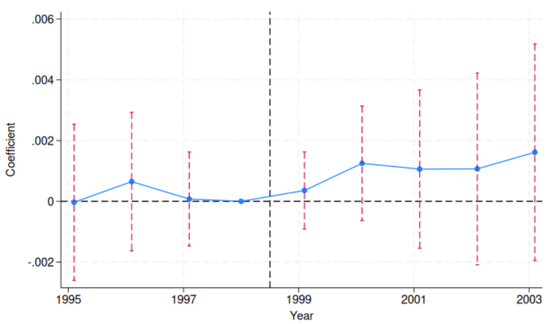
	(1) Worker Fixed Effect (In-House)	(2) Worker Fixed Effect (Temp Agencies)	(3) Firm Fixed Effect (In-House)	(4) Firm Fixed Effect (Temp Agencies)
% $TempAgencyWork_{1998} \times Post$	0.000898	0.0195	-0.00256	-0.00406
	(0.00131)	(0.0188)	(0.00266)	(0.0192)
Observations	450	450	450	450

Notes: The table shows the effects of the reform on worker and firm quality. Estimates are based on a continuous difference-in-difference strategy (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The endogenous variables are estimate with an AKM model with pre-policy data. See Section E.3 for further details. Robust standard errors clustered at the province level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL.

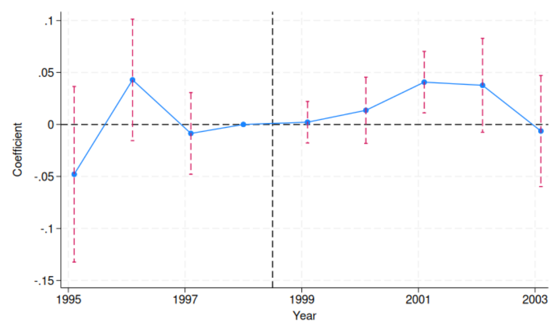
E.4 Effects on User Firms: Sector-Level Analysis

We now investigate the reform's potential effects on user firms across multiple margins. To do so, we shift from the province-level variation used earlier to an analysis leveraging sectoral variation with firm-level data from the *Central de Balances Integrada (CBI)* provided by the

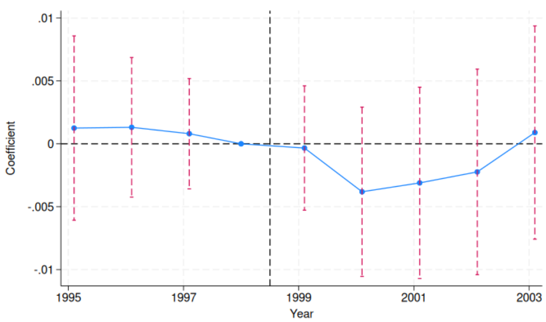
Figure E4: **Effects on Worker and Firm Quality**



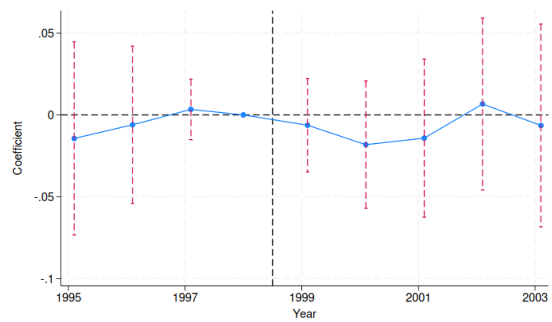
(a) Avg. In-House Worker Fixed Effect



(b) Avg. Agency Worker Fixed Effect



(c) Avg. In-House Firm Fixed Effect



(d) Avg. Agency Firm Fixed Effect

Notes: The figures show the event-studies of the effects of the reform on in-house worker quality (Panel (a)), temp agency worker quality (Panel (b)), in-house firm quality (Panel (c)) and temp agency quality (Panel (d)). The specification is a continuous difference-in-differences (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The endogenous variables are estimate with an AKM model with pre-policy data. See Section E.3 for further details. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the month of the reform. Sources: MCVL.

Bank of Spain.[53]

Similar to the previous approach, our identification strategy relies on cross-sectoral differences in the pre-reform incidence of agency employment, measured by $\%TempAgency_{s,1998}$. We estimate a continuous difference-in-differences model, specified as follows:[54]

$$
\begin{aligned}
y_{st} = \alpha + \delta_s + \delta_t + \sum_{j=1995, j\neq 1998}^{2003} \beta_j 1[j=t]\%TempAgency_{s,1998} + \\
+ \sum_{j=1995, j\neq 1998}^{2003} \gamma_j 1[j=t] X_s + \epsilon_{st}
\end{aligned}
\tag{41}
$$

where δ_s and δ_t are sector and year fixed effects, and β_j capture the reform's yearly impact. X_s is a vector of pre-reform sector-level characteristics (average firm age, size, share of public and listed firms, and a Herfindahl-Hirschman index), interacted with year dummies. y_{st} denotes the log of various sector-level outcomes.

We begin with productivity and profitability per worker.[55] The theoretical effect is ambiguous. Replacing temporary agency workers with in-house employees may boost value added if internal workers are better matched, motivated, or reallocated. Conversely, higher hiring and wage costs could offset these gains. Firms may also raise output prices to pass through costs or reflect improved product quality.[56] Productivity rises only if efficiency or price gains exceed higher costs; profitability rises only if gains cover both wage and hiring costs.

Panels (a) and (b) of Figure E5 show no statistically significant effects on productivity or profitability, suggesting that gains from reallocating temp workers were largely offset by higher labor costs.[57]

[53] The CBI contains financial information for non-financial companies obtained from annual accounts filed in the *Registro Mercantil*. Since these data are consolidated at the company level and do not include establishment-level details, geographical variation cannot be exploited.

[54] We only consider sectors for which we have a balanced panel for all the variables considered in this section. Regressions are weighted by the endogenous variable at baseline. For markups and markdowns, the regressions are weighted by value added.

[55] Numerators are sectoral sums of value added or profits, while denominators are sums of average workers per year.

[56] All dependent variables are in real terms using a national deflator, which removes aggregate price changes but not relative sectoral variation.

[57] Table E3 reports the coefficients for two and four years after the reform. We find a negative but statistically insignificant effect on productivity (-0.5%) and a negative, significant effect on profits (-1.8%) after two years. However, these results are partly driven by a relatively large and positive coefficient in the pre-treatment year 1995, as shown in the event-study plots. Excluding 1995 substantially attenuates the estimates, reducing them to -0.1% for productivity and -0.6% for profits, neither of which is statistically distinguishable from zero. When using detrended variables, the coefficients turn positive but remain insignificant (0.8% and 3%), again reflecting the influence of the 1995 outlier. Once 1995 is excluded, the detrended estimates become -0.8% and -1%, both of which are statistically insignificant.

Panel (c) analyzes purchases from other firms. As expected, given the reduced demand for agency workers, purchases decline in sectors more exposed to the reform. A one percentage point higher initial incidence of agency work reduces purchases by 1.9% after two years and by 3.3% after four, although these estimates are imprecise.

In Panel (d), we test whether the reform led to a decrease in the number of firms, as companies in more affected sectors might have been forced to exit due to higher labor costs. If true, our previous results could be confounded by the exit of less productive firms. However, we find no evidence that the reform influenced firm exit rates.

Finally, Panels (e) and (f) analyze markups and markdowns.[58] The reform shows no significant impact on margin structures.[59]

Overall, user firms adjusted primarily by reducing purchases from other firms. The absence of significant effects on other margins suggests the reform had a limited overall impact on user-firms.

Table E3: Effects on In-House Sectors

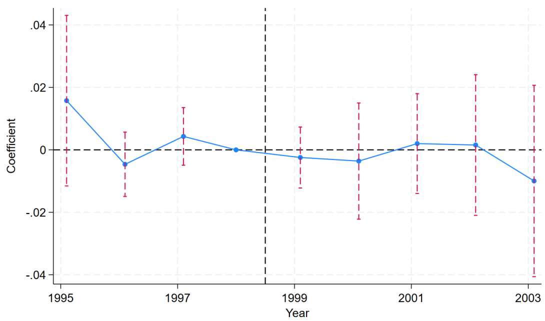
	(1) Log Productivity per Worker	(2) Log Profits per Worker	(3) Log Purchases Other Companies	(4) Log Number Companies	(5) Log Markup	(6) Log Markdown
Panel A: Up to 2 Years after Reform						
$\%TempAgency_{s,1998}$ x Post	−0.005	−0.018*	−0.019	−0.003	−0.004	−0.005
	(0.006)	(0.009)	(0.020)	(0.010)	(0.008)	(0.012)
Observations	287	287	287	287	246	246
Panel B: Up to 4 Years after Reform						
$\%TempAgency_{s,1998}$ x Post	−0.006	−0.019	−0.033	0.000	−0.005	−0.003
	(0.007)	(0.013)	(0.025)	(0.010)	(0.008)	(0.012)
Observations	369	369	369	369	328	328
Sectors	41	41	41	41	41	41

Notes: The table shows the effects of the reform for in-house companies. The data is aggregated at the sector level. The specification is a continuous difference-in-differences where regressions are weighted by the endogenous variable at baseline. The specification is Equation 41. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the average company age, firm size, the share of public firms, the share of listed firms, and a sector-level Herfindahl-Hirschmann concentration index. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: *Central de Balances Integrada* (BELab, 2025) and *Empresas de trabajo temporal*.

[58] Following Dodini et al. (2023) and Yeh et al. (2022), we estimate markups and markdowns using the "production function approach" from Loecker and Warzynski (2012). Due to data availability and methodological requirements, markups and markdowns cannot be computed for 1995, the first year covered in the CBI. For further details on the estimation, see Appendix OD.

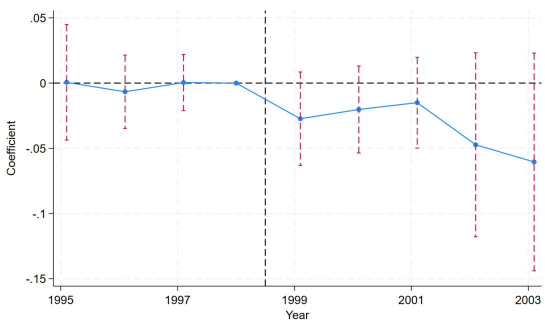
[59] Unfortunately, product-level price data are unavailable for this period, preventing a direct test of price effects.

Figure E5: **Effects on In-House Companies**

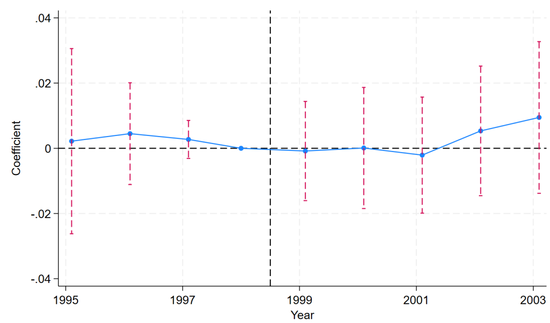


(a) Log Productivity per Worker

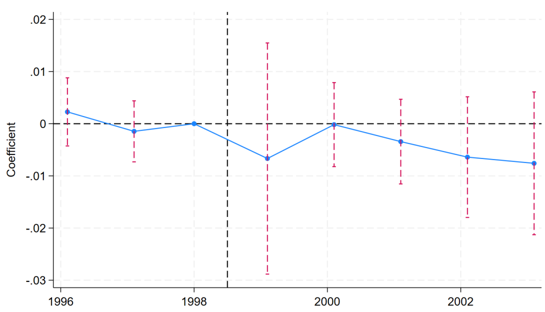
(b) Log Total Profits per Worker



(c) Log Purchases to Other Companies



(d) Log Number of Companies



(e) Log Markup

(f) Log Markdown

Notes: The figure shows the effects of the reform on the sectors that use temp agency workers. The specification is a continuous difference-in-differences where regressions are weighted by the endogenous variable at baseline. The specification is Equation 41. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the average company age, firm size, the share of public firms, the share of listed firms, and a sector-level Herfindahl-Hirschmann concentration index. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Sources: *Central de Balances Integrada* (BELab, 2025) and *Empresas de trabajo temporal*.

E.5 Effects on Temporary Agencies: Sector-Level Analysis

To quantify the impact of the policy on temporary work agencies, we use sector-level aggregates, including the number of active temp agencies, total net sales, value added, labor costs, profits, and the average number of workers. These variables capture the overall economic losses experienced by the sector following the reform.[60]

Our identification strategy compares the temporary work agency sector with other sectors that were less reliant on agency employment prior to the reform. In the absence of a natural control group, the analysis relies on the assumption that sectors with low dependence on temporary agency labor were less affected by the labor cost increases induced by the policy.[61]

Empirically, we estimate:[62]

$$\hat{y}_{st} = \alpha + \delta_s + \delta_t + \sum_{j=1995, j\neq 1998}^{2003} \beta_j 1[j = t] Treatment_s + \sum_{j=1995, j\neq 1998}^{2003} \gamma_j 1[j = t] X_s + \epsilon_{st}, \quad (42)$$

where $\hat{y}_{st}$ is the log of a sector-level outcome, as defined above, and the hat denotes that pre-reform sector-specific trends have been removed. This adjustment is necessary because the temporary agency sector was booming before the policy change, and the reform altered its trajectory (see Figure 1, Panels (b)–(d)).

The specification includes sector (δ_s) and year fixed effects (δ_t). $Treatment_s$ equals 1 for the temp agency sector and 0 otherwise. X_s is a vector of pre-reform sector-level characteristics, interacted with year dummies, to flexibly control for heterogeneity across sectors. Specifically, we account for average firm age and firm size, the share of public firms, the share of listed firms, and a sector-level Herfindahl–Hirschman concentration index.[63] The control group consists of sectors in the bottom quartile of temp agency incidence.[64]

Figure E6 presents the results. Economic activity in the sector declined markedly after the reform. Panels (a)–(f) show sharp contractions in the number of operating agencies, net sales, value added, labor costs, and employment. In contrast, profits declined only moderately and recovered in subsequent years, suggesting that part of the fall in value added was offset by the reduction in labor costs.

Table E4 reports the difference-in-differences estimates. The top panel shows results two

[60] Using means instead of aggregates would primarily reflect compositional changes driven by firm exits, rather than reductions in total economic activity.

[61] The Ministry of Labor provides data on the distribution of temporary agency workers, aggregated into 44 sectors. For consistency with the in-house analysis, we use this aggregation as our benchmark. Our results are robust to employing the more granular NACE 4-digit sectoral breakdown.

[62] Regressions are weighted by the endogenous variable at baseline.

[63] Firm size is proxied by the number of workers at baseline. We exclude this variable from the specification when study the effects on employment.

[64] Results are robust to alternative threshold definitions.

years after the reform, and the bottom panel extends the horizon to four years. Two years post-reform, the number of agencies was 40% lower, net sales fell by 67%, and value added and labor costs declined by 56% and 64%, respectively. These patterns are consistent with the sharp reduction in temporary agency employment documented in Section 5.2.2. Since the core activity of temporary agencies is to "supply" workers to user firms, lower agency employment mechanically translates into fewer transactions and a contracting sector. The relatively smaller decline in profits reflects that labor costs contracted in tandem with value added, cushioning the overall profitability impact.

Given the sector's rapid pre-reform expansion, these results should also be interpreted as reflecting foregone growth. Absent the policy, the number of agencies would likely have continued to rise.[65]

Table E4: Effects on Temp Agencies

	(1) Log Number Temp Agencies	(2) Log Net Sales	(3) Log Value Added	(4) Log Labor Costs	(5) Log Profit	(6) Log Number of Workers
Panel A: Up to 2 Years after Reform						
Treatment x Post	−0.509**	−1.102***	−0.834**	−1.032**	−0.483	−0.720***
	(0.172)	(0.210)	(0.311)	(0.377)	(0.341)	(0.210)
Observations	70	70	70	70	70	70
Panel B: Up to 4 Years after Reform						
Treatment x Post	−0.815***	−1.630***	−1.337***	−1.678***	−0.368	−1.174***
	(0.220)	(0.265)	(0.390)	(0.485)	(0.437)	(0.265)
Observations	90	90	90	90	90	90
Sectors	10	10	10	10	10	10

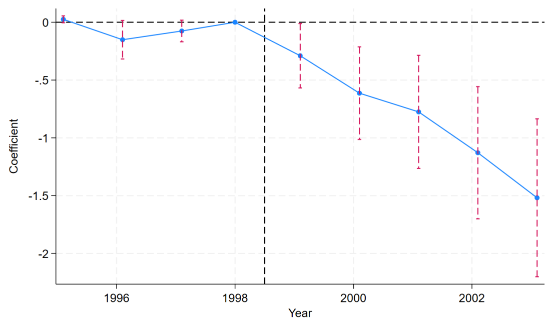
Notes: The table shows the effects of the reform on temp agencies. The specification is a difference-in-differences, where sector-specific pre-change linear trends are removed. Regressions are weighted by the endogenous variable at baseline. The specification is Equation 42. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the average company age, firm size (except when the endogenous variable is the number of workers), the share of public firms, the share of listed firms, and a sector-level Herfindahl Hirschman concentration index. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: *Central de Balances Integrada* (BELab, 2025) and *Empresas de trabajo temporal*.

E.6 Internal Migration

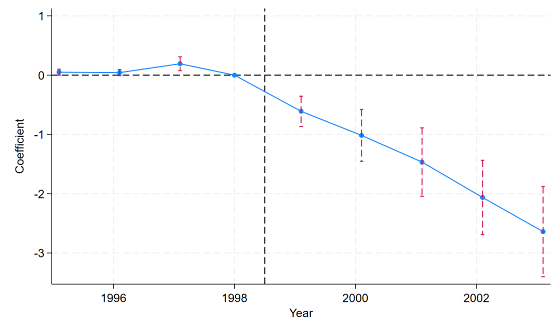
Following the empirical strategy in Equation 4, we examine internal migration responses separately for Spanish citizens and foreign-born workers. The event-study results in Figure E7 and Table E5 show that the increase in migration toward high-incidence provinces is mainly driven by Spanish workers. This rules out inflows of international migrants as the driver of the positive employment effect, reinforcing the interpretation that domestic labor

[65]This counterfactual pattern is evident in the event-study estimates without detrending the dependent variables, which show a continued upward trajectory in value added, net sales, employment, and labor costs prior to the reform. The policy effectively halted this expansion. Results are available upon request.

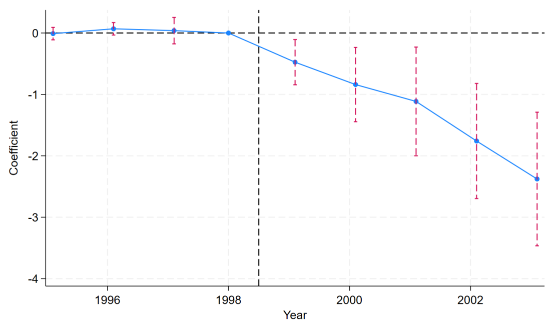
Figure E6: **Effects on Temp Agencies**



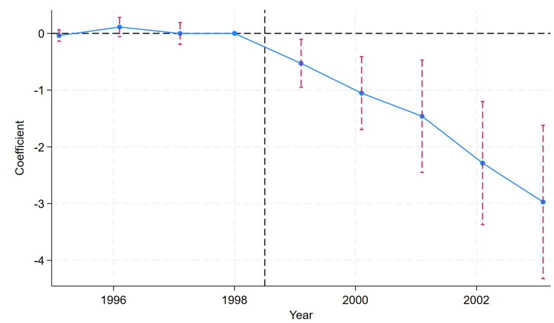
(a) Log Number of Temp Agencies



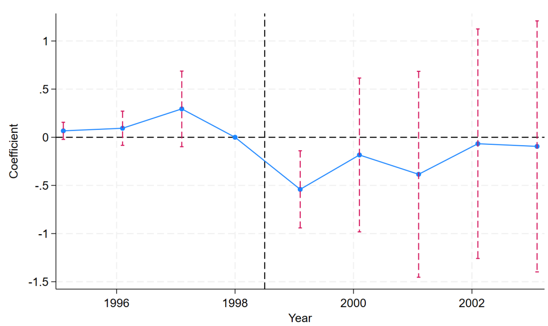
(b) Log Net Sales



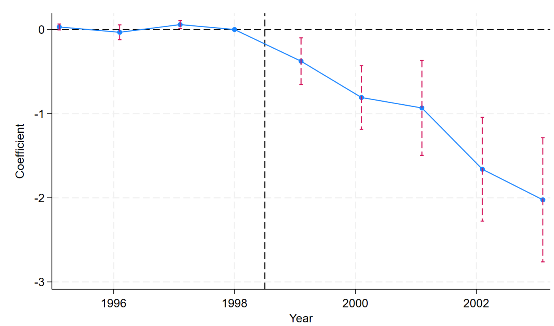
(c) Log Value Added



(d) Log Labor Costs



(e) Log Profit



(f) Log Number of Workers

Notes: The figures show the event-studies for the effects of the reform on the temp agency sector. The specification is a difference-in-differences (Equation 42), where sector-specific pre-change linear trends are removed. The control group is composed of sectors with an incidence of temp agency employment below the 25th percentile. Regressions are weighted by the endogenous variable at baseline. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the average company age, firm size (except when the endogenous variable is the number of workers), the share of public firms, the share of listed firms, and a sector-level Herfindahl-Hirschmann concentration index. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the sector level. The vertical red-dashed line depicts the last pre-reform period. Sources: *Central de Balances Integrada* (BELab, 2025) and *Empresas de trabajo temporal*.

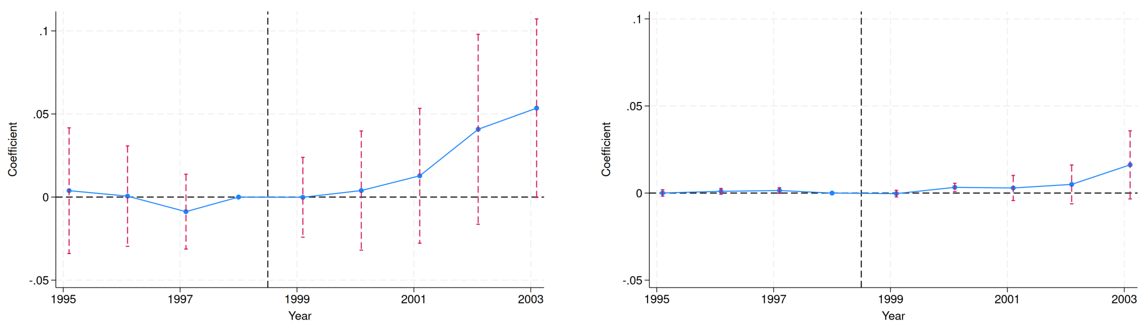
supply responded to the reform rather than masking a negative employment impact through foreign-worker arrivals.

Table E5: Effects Internal Migration: Natives and Foreign-Born Workers

	(1) Natives	(2) Foreign-Born
Treatment x Post	0.0233	0.00479
	(0.0281)	(0.00430)
Observations	450	450

Notes: The table shows the effects on internal migration by migrants' origin (natives versus foreign-born). Estimates are obtained from a static difference-in-differences version of Equation 4. Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. Robust standard errors clustered at the province level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL.

Figure E7: **Effects Internal Migration from Other Provinces: Natives and Foreign-Born Workers**



(a) Natives **(b) Foreign-Born**

Notes: The figures show the event-studies for the effects of the reform on internal migration. Panel (a) is for Spanish citizens and Panel (b) is for foreign-born workers. The specification is a continuous difference-in-differences (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Sources: MCVL.

F Temp agency fee

We compute the yearly gross fee charged by temporary work agencies by combining three complementary data sources. From the *Central de Balances* (CBI) of the Bank of Spain, we obtain annual firm-level information on the revenues of temporary work agencies. The *Continuous Sample of Working Histories* (MCVL) contains social security records of a sample of temporary agency workers, from which we directly observe days worked in user firms.

Finally, the Ministry of Labor and Social Economy provides the total number of temporary work agencies and temporary agency workers at the national level.

We define the gross fee as the average amount billed by agencies per worker-day, computed as:

$$\text{Gross Fee} = \frac{\text{Revenue}}{\text{Days Worked}},$$

To construct consistent national aggregates, we face two main challenges. The first relates to the CBI data. Firms may enter or exit the database over time, so simply summing firm-level data could produce a distorted time series due to compositional effects. To address this, we estimate time trends of revenue using regressions with year and firm fixed effects, which control for firm entry and exit. Regressions are weighted by the average value added to give greater weight to larger firms. The resulting year effects are then normalized to match the observed 1995 levels.

The second challenge is that the numerator (revenues) and the denominator (days worked) come from different sources, which may cover different subsets of the population. To extrapolate to the national level, we apply elevation factors separately to each. For the numerator, we scale using the 1995 ratio of the total number of temporary work agencies reported by the Ministry of Labor and Social Economy to the number observed in the CBI, yielding a factor of 3.1. For the denominator, we scale the total days worked observed in the MCVL by the 1995 ratio of total temporary agency workers reported by the Ministry to those in the MCVL, giving a factor of 31.7. This factor is consistent with the MCVL coverage of approximately 4% of the total workforce.

This approach ensures that both the numerator and the denominator are consistently aggregated at the national level, so that the resulting gross fee accurately reflects the average revenue per worker-day.

Online Appendix (Not for Publication)

Contents

OA Placebo tests for individual-level results

This section conducts placebo tests to validate the causal interpretation of the individual-level wage estimates presented in Section 5.1. To assess whether our findings reflect genuine treatment effects rather than spurious correlations, we implement a randomization inference procedure that repeatedly re-estimates Equation 3 after excluding temporary agency workers and randomly reassigning treatment status among remaining in-house employees.

Our approach proceeds as follows: we first compute the actual proportion of treated observations in the original sample. After removing all temporary agency workers, we randomly assign the treatment indicator to the control group units while preserving the original treatment share.[66] This randomization process is repeated 300 times to construct empirical distributions of placebo coefficients and t-statistics under the null hypothesis of no treatment effect.

We compare our original estimates against these simulated distributions to compute several empirical p-values. Specifically, we report four distinct p-value measures: (i) the proportion of placebo coefficients with absolute values exceeding the observed estimate (*Coef Abs P-value*); (ii) the proportion with coefficients more extreme in the same direction as the observed estimate (*Coef Rel P-value*); (iii) the share of placebo t-statistics larger in absolute magnitude than the observed value (*T Abs P-value*); and (iv) the share more extreme in both magnitude and direction (*T Rel P-value*).

The results, presented in Table OA1 and visualized in Figure OA1, provide compelling evidence against the possibility that our main findings arise from chance. Figure OA1 demonstrates that while the placebo distributions are properly centered around zero, the actual estimated coefficients (indicated by red dashed lines) fall clearly in the extreme tails, well separated from the bulk of the placebo draws across all specifications.

Table OA1 quantitatively confirms this visual pattern. All coefficient- and t-statistic-based p-values are effectively zero (0.000), indicating that none of the 300 placebo replications produced estimates as extreme as those obtained from the actual data.

Collectively, these placebo exercises provide strong evidence that the wage effects documented in our main analysis represent genuine causal relationships rather than statistical artifacts or random noise.

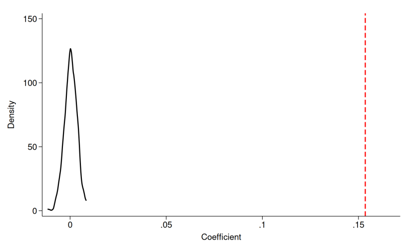
[66]For the sake of computation contraints associated with our large administrative dataset, we implement the placebo tests on a randomly selected 25% subsample of the control group observations.

Table OA1: Effects on Wages: Placebos

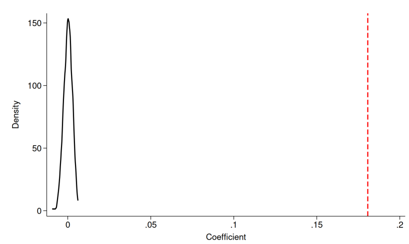
	Table 4, Panel A			Table 4, Panel B			Table 4, Panel C		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Coef Abs P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Coef Rel P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
T Abs P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
T Rel P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: The table reports placebo results obtained by permuting the value of the treatment variable 300 times. For each permutation, the treatment is randomly reassigned across workers in the control group, and the regression is re-estimated to compute the coefficient and the t-statistic of the interaction term. The *Coef Abs P-value* indicates the proportion of placebo estimates with absolute coefficients exceeding that of the original estimate, whereas the *Coef Rel P-value* measures the proportion of placebo coefficients more extreme in the same direction as the original estimate. Similarly, the *T Abs P-value* reports the share of placebo t-statistics greater in magnitude than the observed one, and the *T Rel P-value* shows the proportion more extreme in the same direction. All specifications follow the same structure as the main regressions. Sources: MCVL.

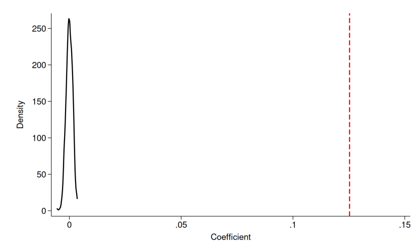
Figure OA1: **Kernel Distributions of Placebo Estimates for Direct Wage Effects**



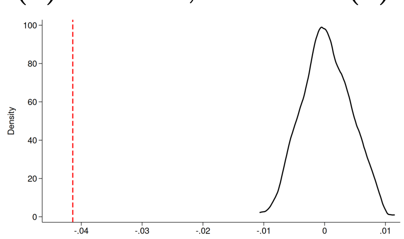
(a) Table 4, Panel A (1)



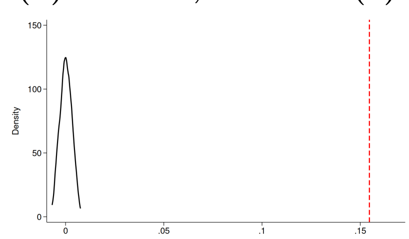
(b) Table 4, Panel A (2)



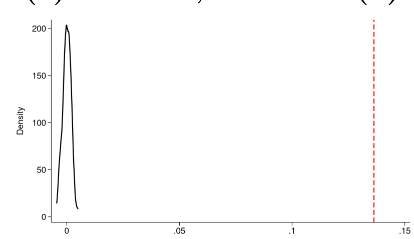
(c) Table 4, Panel A (3)



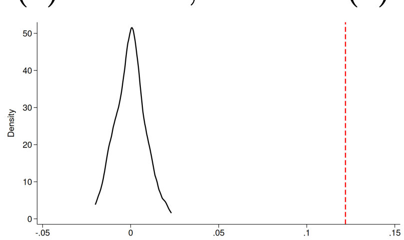
(a) Table 4, Panel B (1)



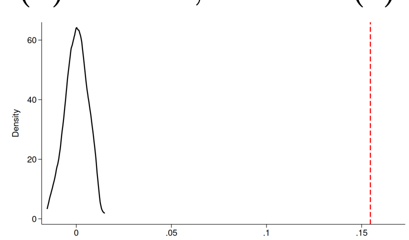
(b) Table 4, Panel B (2)



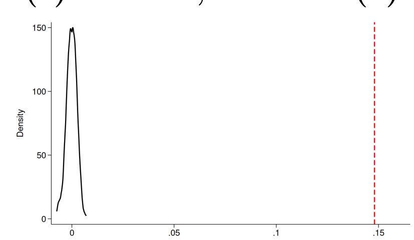
(c) Table 4, Panel B (3)



(a) Table 4, Panel C (1)



(b) Table 4, Panel C (2)



(c) Table 4, Panel C (3)

Notes: Each panel displays the kernel density of interaction coefficients from 300 placebo regressions where treatment status was randomly assigned among control units. The red-dashed vertical line indicates the coefficient estimate from the corresponding original specification. Sources: MCVL.

OB Placebo tests for province-level results

This section presents placebo tests for our province-level analyses of wage spillovers, employment patterns, and job security effects. We extend the randomization inference approach described previously to our continuous difference-in-differences framework, re-estimating the static difference-in-differences version of equation 4 1,000 times while randomly reassigning treatment intensity across all 50 provinces. The key methodological distinction from the individual-level analysis is that these specifications employ continuous treatment measures rather than discrete treatment assignments, requiring permutation across all observational units rather than between treatment and control groups.

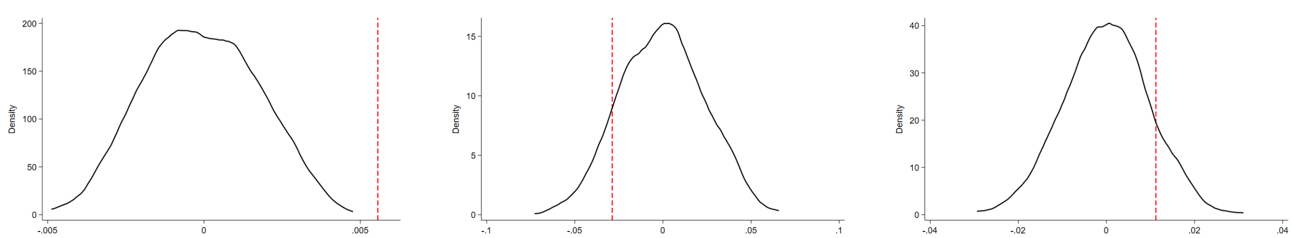
OB.1 Placebo for Wage Spillovers

Table OB1 and Figure OB1 present placebo results for wage spillover effects, examining the three specifications from Table 6: log wages, log wage floors, and the number of workers maintaining at least a 20% cushion above their wage floor.

The results provide compelling causal evidence for spillover effects on in-house worker wages. For log wages, none of the 1,000 placebo estimates exceeded the original coefficient in absolute magnitude, while only 5.3% produced larger absolute t-statistics (2.7% when considering direction).

For wage floors and wage cushion shares, around 25% of placebo coefficients exceed our original estimates in absolute value, while 39-46% of placebo t-statistics are larger in magnitude than the observed statistics. These patterns align with our main results, which showed statistically insignificant effects for these outcomes.

Figure OB1: **Kernel Distributions of Placebo Estimates for Wage Spillovers**



(a) Log Wages **(b) Log Wage Floors** **(c) 20% Wage Cushion**

Notes: Each panel displays the kernel density of interaction coefficients from 1,000 placebo regressions with randomly permuted treatment intensity across provinces. The red-dashed vertical line indicates the original coefficient estimate. Sources: MCVL and Census of Collective Bargaining Agreements.

Table OB1: Wage Spillovers: Placebos

	(1) Log Wages	(2) Log Wage Floors	(3) 20% Wage Cushion
Coef Abs P-value	0.000	0.238	0.246
Coef Rel P-value	0.000	0.116	0.118
T Abs P-value	0.053	0.456	0.386
T Rel P-value	0.027	0.233	0.187

Notes: The table reports placebo results obtained by permuting the value of the treatment variable 1,000 times. For each permutation, the treatment is randomly reassigned across provinces, and the regression is re-estimated to compute the coefficient and the t-statistic of the interaction term. The *Coef Abs P-value* indicates the proportion of placebo estimates with absolute coefficients exceeding that of the original estimate, whereas the *Coef Rel P-value* measures the proportion of placebo coefficients more extreme in the same direction as the original estimate. Similarly, the *T Abs P-value* reports the share of placebo t-statistics greater in magnitude than the observed one, and the *T Rel P-value* shows the proportion more extreme in the same direction. All specifications follow the same structure as the main regressions. Sources: MCVL and Census of Collective Bargaining Agreements.

OB.2 Placebo for Employment Effects

Table OB2 and Figure OB2 present placebo results for employment effects, focusing on the preferred specifications from Table 7. The visual evidence shows that our estimated coefficients fall in the extreme tails of the placebo distributions for temporary agency, with more moderate extremity for in-house, total employment, and interprovincial migration.

The quantitative results strongly support genuine employment effects. For temporary agency employment, all placebo p-values are effectively zero, indicating that none of the 1,000 permutations generated estimates as extreme as those observed. For in-house and total employment, both coefficient- and t-statistic-based p-values remain around or below 10%, suggesting these effects are unlikely to arise from random variation.

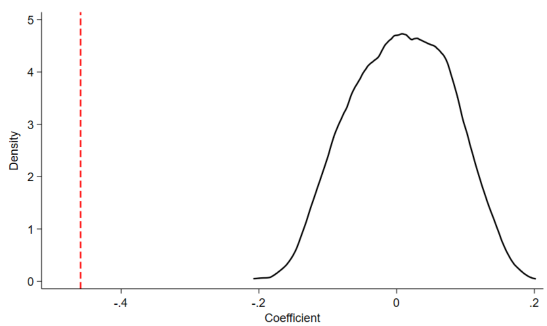
Conversely, placebo results for interprovincial migration yield notably higher p-values for absolute t-statistics (22%) reflecting the greater statistical uncertainty evident in our main employment results.

Table OB2: Effects on Employment: Placebos

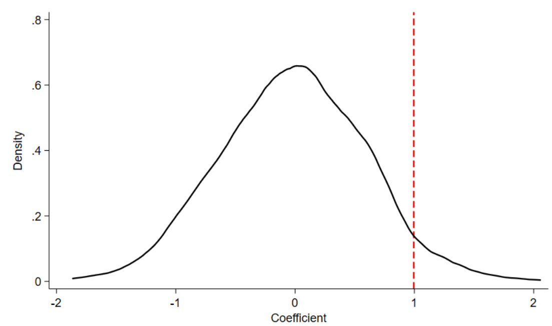
	(1) Temp Agency (Detrended)	(2) In-House (Not Detrended)	(3) Both (Not Detrended)	(4) Immigration from Other Provinces
Coef Abs P-value	0.000	0.099	0.048	0.032
Coef Rel P-value	0.000	0.047	0.022	0.021
T Abs P-value	0.000	0.114	0.066	0.218
T Rel P-value	0.000	0.049	0.031	0.100

Notes: The table reports placebo results obtained by permuting the value of the treatment variable 1,000 times. For each permutation, the treatment is randomly reassigned across provinces, and the regression is re-estimated to compute the coefficient and the t-statistic of the interaction term. The *Coef Abs P-value* indicates the proportion of placebo estimates with absolute coefficients exceeding that of the original estimate, whereas the *Coef Rel P-value* measures the proportion of placebo coefficients more extreme in the same direction as the original estimate. Similarly, the *T Abs P-value* reports the share of placebo t-statistics greater in magnitude than the observed one, and the *T Rel P-value* shows the proportion more extreme in the same direction. All specifications follow the same structure as the main regressions. Sources: MCVL.

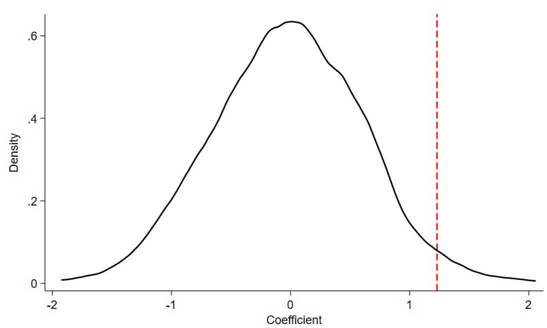
Figure OB2: **Kernel Distributions of Employment Placebo Results**



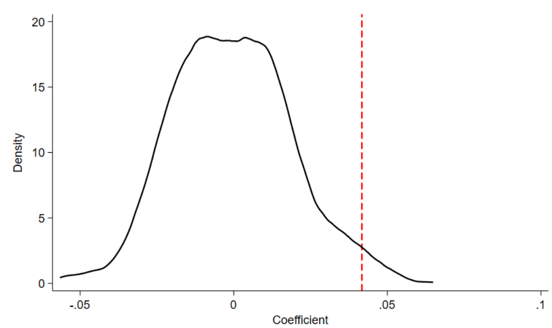
(a) Temp Agency (Detrended)



(b) In-House (Not Detrended)



(c) Both (Not Detrended)



(d) Immigration from Other Provinces

Notes: Each panel displays the kernel density of interaction coefficients from 1,000 placebo regressions with randomly permuted treatment intensity across provinces. The red-dashed vertical line indicates the original coefficient estimate. Sources: MCVL.

OB.3 Placebo for Job Security

Finally, we examine the placebo results for job security outcomes from Table 8, reported in Table OB3 and illustrated in Figure OB3. The findings reveal nuanced patterns across different types of worker flows.

For temporary agency layoffs, hires, and net hires (columns 4–6), the true coefficients lie in the tails of the placebo distribution, with coefficient-based p-values below 0.6% and t-statistic p-values ranging from 5.3% to 7%. A similar pattern emerges for permanent employment, reinforcing the causal interpretation of the main results.

By contrast, aggregate and in-house employment flows exhibit consistently high p-values for both coefficients and t-statistics, consistent with the imprecise estimates reported in the main analysis. Finally, in the case of employment composition, short-term employment shows no meaningful deviation from the null.

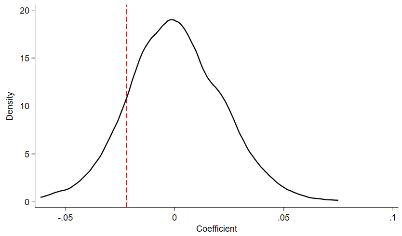
Collectively, these findings suggest the reform had genuine effects on temp agency worker flows and permanent employment.

Table OB3: Effects on Flows: Placebos

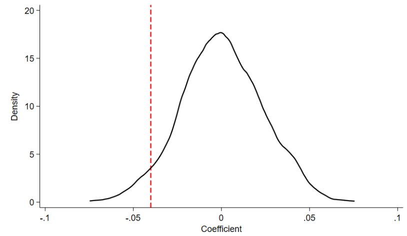
	(1) Layoffs All	(2) Hires All	(3) Net Hires All	(4) Layoffs Temp Agency	(5) Hires Temp Agency	(6) Net Hires Temp Agency	(7) Layoffs In-House	(8) Hires In-House	(9) Net Hires In-House	(10) Permanent Employment	(11) Short-Term Employment
Coef Abs P-value	0.301	0.085	0.177	0.002	0.006	0.003	0.161	0.269	0.757	0.004	0.969
Coef Rel P-value	0.141	0.041	0.091	0.002	0.002	0.001	0.076	0.128	0.382	0.001	0.469
T Abs P-value	0.500	0.243	0.368	0.053	0.069	0.070	0.291	0.466	0.844	0.038	0.978
T Rel P-value	0.249	0.114	0.186	0.023	0.033	0.039	0.149	0.234	0.421	0.015	0.474

Notes: The table reports placebo results obtained by permuting the value of the treatment variable 1,000 times. For each permutation, the treatment is randomly reassigned across provinces, and the regression is re-estimated to compute the coefficient and the t-statistic of the interaction term. The *Coef Abs P-value* indicates the proportion of placebo estimates with absolute coefficients exceeding that of the original estimate, whereas the *Coef Rel P-value* measures the proportion of placebo coefficients more extreme in the same direction as the original estimate. Similarly, the *T Abs P-value* reports the share of placebo t-statistics greater in magnitude than the observed one, and the *T Rel P-value* shows the proportion more extreme in the same direction. All specifications follow the same structure as the main regressions. Sources: MCVL.

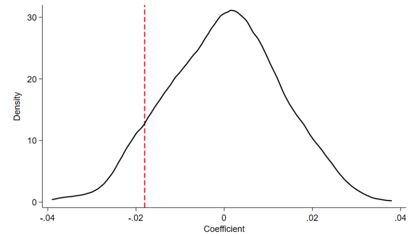
Figure OB3: **Kernel Distributions of Job Security Placebo Results**



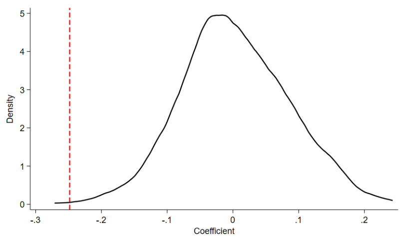
(a) Layoffs (All)



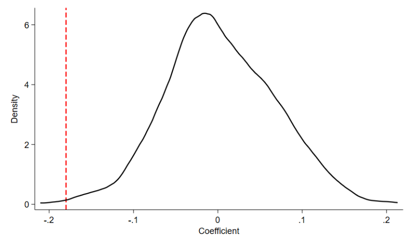
(b) Hires (All)



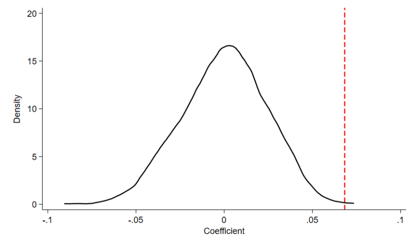
(c) Net Hires (All)



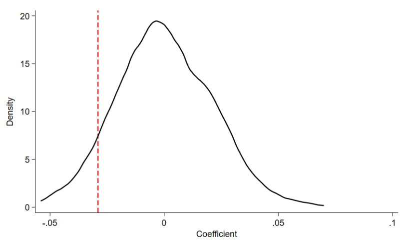
(d) Layoffs (Agency)



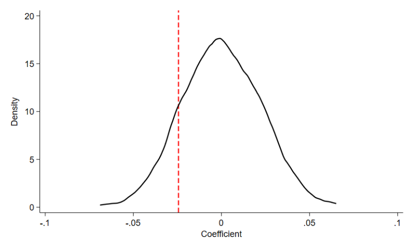
(e) Hires (Agency)



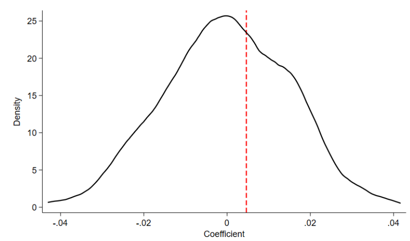
(f) Net Hires (Agency)



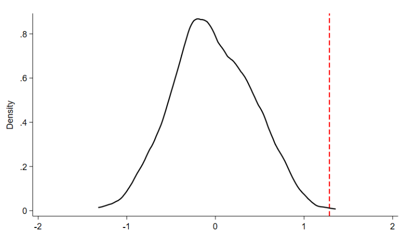
(g) Layoffs (In-House)



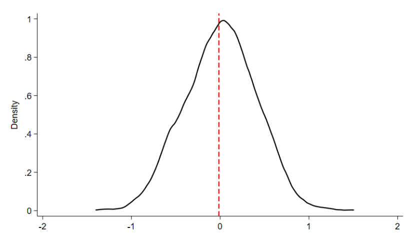
(h) Hires (In-House)



(i) Net Hires (In-House)



(j) Permanent Emp.



(k) Short-Term Emp.

Notes: Each panel displays the kernel density of interaction coefficients from 1,000 placebo regressions with randomly permuted treatment intensity across provinces. The red-dashed vertical line indicates the original coefficient estimate. Sources: MCVL.

OC Uncensored contributions

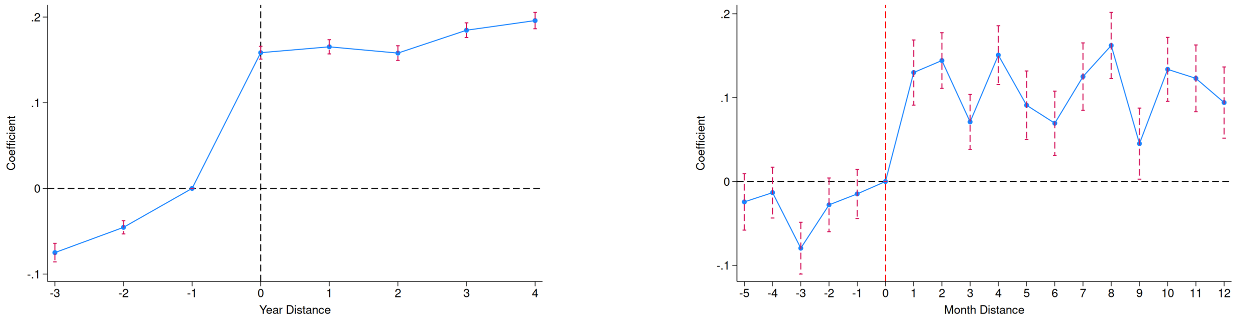
The uncensoring procedure involves three steps. First, we group individuals into cells defined by occupation (a proxy for skills), age group (as a proxy for experience), and time indicators. Specifically, we classify workers into eight age groups (<25, 25–29, 30-34,..., 55–65), three skill categories—low-skilled (occupation groups 8–11), medium-skilled (groups 4–7), and high-skilled (groups 1–3)—and a set of time dummies consisting of nine yearly indicators (1995–2003) and twelve monthly indicators. This results in a total of 2,592 cells.

In the second step, we estimate a cell-specific Tobit model to impute earnings for individuals with censored observations. Within each cell, we parametrically model log earnings, imposing no restrictions across cells. Assuming log-normality, we estimate the cell-specific mean and variance parameters, μ_c and σ_c^2, by maximum likelihood. These parameters are then used to simulate earnings for top- and bottom-coded individuals, generating ten imputations per censored observation.

Figure OC1 and Table OC1 replicate the analysis presented in Figure 2 and Table 4 of the main text, but using the imputed, uncensored measure of wages. As shown, both the patterns and magnitudes of the results remain remarkably similar. The estimated direct wage effect of the reform corresponds to an increase in wages of between 13 and 14%.

Figure OC2 and Table OC2 present the corresponding results to Figure 3 and Table 6 in the main text. In this case, we focus on the effects on wages and on the number of workers earning at most 20% above the wage floor, since the effect on wage floors is not affected by the administrative data on wages. Once again, the patterns and magnitudes are virtually unchanged, with an estimated wage increase of 0.58% for in-house workers and no statistically significant effect on the number of workers earning above the 20% cushion threshold.

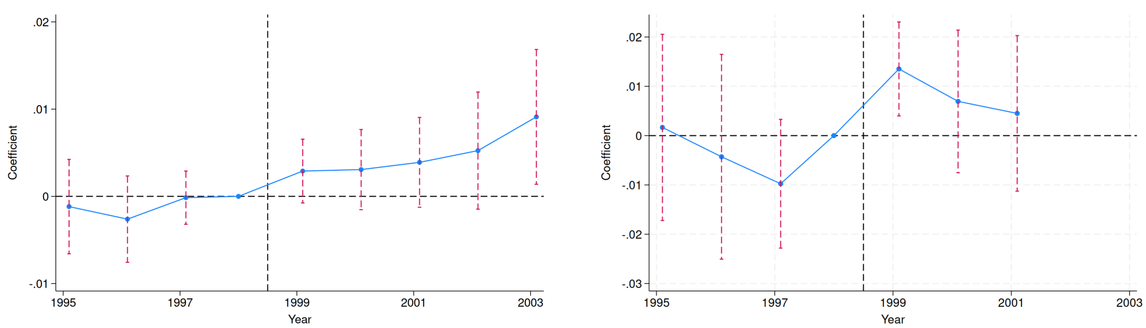
Figure OC1: **Wage Effects: Uncensored Wages**



(a) All Workers **(b) Workers Continuously Employed 18 Months**

Notes: Panels (a) and (b) show the event-studies that analyze the wage effects of the reform. The specification is a difference-in-differences with individual and time fixed effects, as in Equation 3. Panel (a) uses the full worker-level dataset, while Panel (b) restricts the sample to workers continuously employed for at least 6 months before and 12 months after the reform. Additional control variables are the province unemployment rate, age and experience third-order polynomials, contract type, education, part-time contract, company size, location and calendar-month fixed effects and occupation proxies. The blue solid lines are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. The vertical red-dashed line depicts the month of the reform. Panel (c) plots the wage distributions of temp agency workers and in-house workers in 1998, the year before the reform. The grey-dashed vertical line represents the minimum wage. Wage data are uncensored following the procedure described in Appendix OC. Sources: MCVL.

Figure OC2: **Wage Spillovers: Uncensored Wages**



(a) In-House Wages **(b) 20% Wage Cushion**

Notes: The figures show the event-studies of the effects of the reform on actual in-house wages (Panel (a)) and the incidence of wage cushions of at most 20% (Panel (b)). The specification is a continuous difference-in-differences (Equation 4). Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. The dots are the coefficient estimates and the red-dashed lines 95% confidence intervals of robust standard errors clustered at the province level. The vertical red-dashed line depicts the last pre-reform period. Panel (b) displays estimates until 2001 because that is the last year in the wage floor data. Wage data are uncensored following the procedure described in Appendix OC. Sources: MCVL and Census of Collective Bargaining Agreements.

Table OC1: Effects on Wages

	(1) Log Daily Wage	(2) Log Daily Wage	(3) Log Daily Wage
	Panel A: All Workers		
Treatment x Post	0.167***	0.198***	0.128***
	(0.00300)	(0.00306)	(0.00298)
Observations	29339624	29339624	29339624
	Panel B: All Workers, First Post Reform Year		
Treatment x Post	-0.119***	0.158***	0.136***
	(0.00305)	(0.00382)	(0.00369)
Observations	29339624	29339624	29339624
	Panel C: Temp that Stay in the Job for 18 Months		
Treatment x Post	0.113***	0.150***	0.138***
	(0.0117)	(0.0119)	(0.0100)
Observations	3297775	3297775	3297775
Individual FE	N	N	Y
Month FE	N	Y	Y
Location FE	N	Y	Y
Other time-varying controls	N	Y	Y
Unemp. Rate	Y	Y	Y

Notes: The table shows the wage effects of the reform. The specification is a difference-in-differences. In Panel A, we use full sample for the years 1995 to 2003. Panel B uses the same sample but reports the estimate of the first post period in the dynamic specification (Equation 3). In Panel C the sample is composed of all workers who are continuously employed 6 months before the reform, and for the next 12 months after it. The bottom panel reports the control variables included in each specification-column. The time-varying controls are age and experience third-order polynomials, contract type, education, part-time contract, company size, and occupation proxies. Robust standard errors clustered at the worker level are shown in parentheses. Wage data are uncensored following the procedure described in Appendix OC. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL.

Table OC2: Wage Spillovers

	(1) (3) Log Wages	20% Wage Cushion
% $TempAgencyWork_{1998} \times Post$	0.00583**	0.0114
	(0.00254)	(0.00976)
Observations	450	350

Notes: The table shows the wage spillover effects of the reform. The endogenous variables are composition-adjusted wages (Column 1) and the incidence of wage cushions of at most 20% (Column 2). Estimates are obtained from a static difference-in-differences version of Equation 4. Regressions are weighted by population. All regressions include interactions of year dummies with controls at baseline. Specifically, we control for the unemployment rate, a dummy for coastal provinces, the share of the construction sector, the share of immigrants, the share of women, the share of short-term workers, the education level, and the average firm size at baseline. Wage data are uncensored following the procedure described in Appendix OC. Robust standard errors clustered at the province level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Sources: MCVL and Census of Collective Bargaining Agreements.

OD Markup and Markdown Estimation

To estimate firm-level product market markups and labor market markdowns, we follow Yeh et al. (2022), which extends the production-based markup framework of Loecker and Warzynski (2012) to separately identify labor market power.

Yeh et al. (2022) show that, under two key assumptions—(i) firm cost minimization and (ii) the existence of a competitively supplied and flexible variable input—the wedge between output elasticities and revenue shares can be decomposed into product market and labor market components. Let X_{it}^V denote the variable input, which we take to be materials. The product market markup is then

$$\mu_{it} = \frac{\theta_{it}^V}{\alpha_{it}^V},$$

where θ_{it}^V is the output elasticity with respect to materials and α_{it}^V is the materials cost share of revenues. Using materials as the variable input, the markup corresponds to the wedge between the output elasticity with respect to materials and the materials cost share. Because materials are supplied competitively, this wedge reflects product market power rather than input markdowns.

The labor markdown is expressed as:

$$\nu_{it} = \frac{\theta_{it}^L}{\alpha_{it}^L} \cdot \mu_{it}^{-1},$$

where θ_{it}^L is the output elasticity of labor and α_{it}^L is the labor's share. This removes the contribution of the product markup from the wedge between labor's output elasticity and its revenue share, isolating labor market power.

Empirically, cost shares are observed in firm-level accounting data from the *Central de Balances Integrada*. Estimating μ_{it} and ν_{it} therefore requires the output elasticities of materials and labor, which we obtain through production function estimation, following Yeh et al. (2022), and relying on proxy-variable methods (Ackerberg et al., 2015).

Specifically, we estimate:

$$y_{it} = f(x_{it}; \beta) + \omega_{it} + \varepsilon_{it},$$

where y_{it} is log revenue, x_{it} includes flexible polynomial terms in log capital, labor, and materials, ω_{it} is firm-level productivity, and ε_{it} is measurement error. Endogeneity of inputs is addressed by instrumenting current inputs with their lagged values. Estimation proceeds in three steps: (i) a flexible first-stage regression to recover output net of measurement error, (ii) construction of productivity shocks, and (iii) generalized method of moments estimation.

We estimate parameters using data from the *Central de Balances Integrada* for 1995–

2022, assuming time-invariant output elasticities that vary across two-digit NACE sectors. Under a Cobb–Douglas specification, the estimated coefficients β correspond directly to input output elasticities.