

Workers' Bargaining Power and the Fissured Workplace: the Case of Temp Agency Workers*

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Abstract

We study a reform that mandated the wages of temp agency workers be paid according to the collective agreements of user firms. We develop a search-and-matching model with temporary agencies and production firms to understand the implications of pay equalization in fissured workplaces. The empirical results show that the wages of temporary agency employees increased by 10.9%, with wage growth happening for workers well above the minimum wage. Temporary agency employment decreased, but there was no effect on total employment. Job transformations from temporary agency to in-house increased by 33%, and job duration by 14.4%. The analysis for firms shows that temp agencies were the main losers from the reform, with their economic activity declining significantly, while there was no negative effect for user firms.

Keywords: Fissured workplace, temp agencies, workers' bargaining power, policy evaluation

JEL Classification: J31, J38, J42, J64

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1 Introduction

During the last decades, companies have increasingly outsourced certain functions to external firms, leading to a fragmentation of the employment relationship known as the fissured workplace (Weil, 2014). A type of fragmentation is temporary agency employment, in which the worker is employed by the temp agency, that specializes in hiring, and is then, via a commercial contract, hired out to perform work assignments at the user firm. Recent research has shown both positive and negative effects of contracting out. On the one hand, there is a negative wage penalty for affected workers (Drenik et al., 2023; Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Bilal and Lhuillier, 2021). On the other hand, outsourcing can have positive productivity and employment effects (Bilal and Lhuillier, 2021; Felix and Wong, 2023). However, is it possible to mitigate the wage losses while maintaining the productivity and employment gains? Can labor law that protects workers' rights and increases their bargaining power attain these objectives? This paper aims to shed light on these questions.

We study a reform, passed in Spain in 1999, that stated that the wages of temp agency workers had to be set according to the collective agreement of the user firm where they had been deployed to. Similar to practices in outsourcing or in the gig economy, the relationship between temp agencies and user firms is commercial and, hence, it is not governed by the collective agreement between the user firm and its in-house employees, which facilitates paying lower wages to temp agency workers. The new law was passed to limit this procedure. The importance of this debate is exemplified by the many discussions about how to extend employee benefits and protections to individuals in alternative work arrangements, or how to expand workers' rights more generally. This is case of the California Assembly Bill 5, the Protecting the Right to Organize Act in the US, the Hartz I law in Germany or the Temporary Agency Work Directive in the EU, among others.¹

To contribute to these debates, we develop a theoretical framework with temp agencies and user firms, and model the policy as an increase in the bargaining power of temp agency workers. We test the implications of the theory using administrative data of the labor market

¹See Section 2 for more details about these regulations.

and of firm variables. We focus our analysis on worker outcomes such as wages, employment, job transitions and job duration, and on the profits and productivity of both temp agencies and user firms, among other variables. The richness of our data is particularly valuable to evaluate who are the winners and losers of the reform (e.g. temp agencies vs. workers and user firms), which provides further insights of the theoretical mechanisms behind the main effects of the policy.

To guide the empirical analysis, we introduce a theoretical model to think about the potential effects of the policy change. We extend a static version of the (Pissarides, 2000) matching model to represent a labor market with fissured workplaces. There are two types of large firms. Identical temporary work agencies that provide workers to the production firms, and identical production firms that produce a single output good using two types of workers: in-house employees directly hired by them, and temporary agency workers hired by temp agencies. We make four key assumptions. First, temporary agencies have a more efficient matching technology than production firms. This can be thought as temp agencies having a comparative advantage in hiring because of their specialization in this task. Second, temporary agency workers have lower bargaining power, which we see as a consequence of the difficulty that trade unions face in organizing this sector. There are two main reasons for that. It is a sector with high employee rotation, and there are two employers, blurring the lines of who to negotiate with. Third, some temp agency workers become in-house employees, with the transition probability depending negatively on the temp agency wage penalty. Fourth, in-house workers have longer job duration, which increases their productivity. We interpret this assumption as reflecting that the internalization of workers facilitates investment in firm-specific skills, and reduces coordination and communication costs.²

We model the policy as an exogenous increase in the bargaining power of temp agency workers and obtain five predictions about its effect on several variables. First, the wages of temp agency workers increase, while those of in-house employees stay constant. Second, temp agency employment decreases, because the number of vacancies in the sector drops, and there is transformation of temp agency workers into in-house jobs. Third, the effect on in-house employment is ambiguous because two different forces are at play. On the one hand,

²Moreover, the descriptive statistics of our sample confirm that in-house workers have longer contracts.

the transformation of jobs from temp agency to in-house increases in-house employment. We refer to this mechanism as the job transformation channel. On the other hand, the decrease in vacancies in the temp agency sector reduces the job finding rate in this sector, which means there are fewer temp agency workers who could transition to in-house. We call this mechanism the job creation channel. Fourth, the temp agency workers who are converted to in-house have longer job duration and higher productivity after the reform. Fifth, there is also an ambiguous effect on overall employment, as can be deduced from the three previous predictions. It will be more positive, the stronger is the transformation channel.

We then turn to empirically analyzing the effects of the reform on wages, employment and firm outcomes. The empirical strategies use the difference-in-differences framework, which we explain in more detail with each result. We also display the coefficients from event-study specifications that help to visualize the estimates, and we perform robustness checks and placebo exercises. The results are very similar across all these specifications and uncover six interesting facts, which we interpret through the insights of the model, and can be summarized as follows.

First, the wages of temp agency workers increase significantly, in line with the higher bargaining power they obtain with the policy. For a sample of workers who are employed in the temp agency sector before and after the reform, the increase is of 10.9%. For those who become in-house workers after the reform, wage growth is 5.3%. When we do not place any sample restrictions, we estimate wage increases of 12.9%. The empirical strategy is a difference-in-differences with individual and month fixed effects, including many control variables such as age and experience polynomials, firm size, contract type and worker-rank, among others. The control group are in-house workers, whose wages evolved smoothly across the reform. Additionally, we show that the wage growth happens across the wage distribution, with most workers being affected up to percentile 65th.

Second, we estimate that the reform had a negative and significant effect on temp agency employment, as predicted by the model, but that it was compensated by a positive, significant, and larger impact on in-house employment. Overall, we detect a positive and not significant effect on employment. The positive effects on in-house and overall employment are consistent with the idea that the policy encourages conversions from temp agency to

in-house jobs, which are more stable, and that this force is stronger than the negative job creation effect on temp agencies. For the identification of these results, we compare Spanish provinces -which are a close approximation to local labor markets- with a high incidence of temp agency employment the year before the reform, with those with a small incidence. In this case, our main specification uses detrended data, where the outcome is measured as the deviation from its linear pre-treatment trend at the province level.³ Therefore, the estimates capture the deviation from trend in employment to the pre-treatment size of the temp agency sector in each province.

Third, we use worker-level data to understand what mechanisms are at play. For a sample of incumbent temp agency workers at the time of the reform, we detect an increase in transformations of temp agency workers into in-house employees of 33%. Moreover, job duration increases by 14.4%. Additionally, we replicate the results of the province-level analysis: agency employment decreases significantly, in-house employment increases significantly, and the effect on total employment was positive and insignificant. About 42% of the increase in in-house work happens through open-ended contracts, which entitle workers to severance payments in case of dismissal. For identification we compare the employment evolution of individuals employed by temp agencies the month before the policy, with that of employees in a temp agency job 12 months before. This evidence confirms that the workers who become in-house employees have longer contracts, which helps explaining the positive effect on total employment. Furthermore, the results highlight that wages and job quality features such as duration and employment protection are complementary.

Fourth, we work with sector-level data to show that temp agencies were the main losers from the reform. Identification of the impact on the temp agency sector is based on a difference-in-differences and the control group are the sectors with the lowest penetration of agency work. As in the specification for employment at the province level, we work with detrended data and find that, two years after the reform, the sector had become significantly smaller: the number of agencies is 37.8% smaller or its net sales indicate losses of 128%.

In contrast, in the last set of results, we find no evidence of a negative effect on sectors with a high incidence of agency work. In this case, for identification we use the percentage

³See [Dustmann et al. \(2021\)](#) for another application of this empirical strategy.

of temp agency work before the reform as a shock variable in a difference-in-differences framework. The idea behind this setting is that sectors with more temp agency workers receive a larger labor cost shock and should react more to the reform. The results show that they do increase in-house work more, and buy less from other firms (i.e. buy less from temp agencies). In addition, we find increases in productivity and profit per worker of 1.04% and 2.77%, though they are not significant. We interpret this result as showing that the transformation of agency jobs into in-house avoids some productivity losses associated to workplace fragmentation, such as lack of firm-specific investment in human capital, and communication and coordination costs, but that this effect is attenuated by increases in other costs, such as recruitment.

All in all, we use exogenous variation to show that an increase in the bargaining power of temp agency workers can increase their wages with no employment cost. Moreover, wage growth happens all along the wage distribution, and it is complementary to improvements in other dimensions of job quality like job duration and employment protection. The reform shrinks the temp agency sector, while there is no evidence of negative effects on the sectors with a higher incidence of temp agency work. Hence, the reform redistributes away from temp agencies to their workers. These are all novel results and are related to five strands in the literature. First, [Dube and Kaplan \(2010\)](#), [Drenik et al. \(2023\)](#), [Goldschmidt and Schmieder \(2017\)](#) and [Bilal and Lhuillier \(2021\)](#) document the negative effect of workplace fissuring on wages. Relative to these papers, we provide evidence of a policy that alleviates this effect, which is relevant for current debates about the regulation of the labor relationship. Second, previous research has shown that the extension of employment protection to temp agency workers in Chile had negative effects on employment ([Micco and Muñoz, 2024](#)). In comparison, we document no effect on total employment of wage increases, probably due to high substitutability between agency and in-house workers. Instead, the results show a complementarity between wages, job duration and employment protection for agency workers [Dube et al. \(2022\)](#). Third, the employment and wage estimates imply a very small and close to zero own-wage elasticity of 0.003, in the upper range and quite similar to the elasticity associated with the minimum wage ([Cengiz et al., 2019](#); [Dube, 2019](#)). In contrast to that literature, which finds limited spillover effects of minimum wages ([Cengiz et al., 2019](#); [Autor](#)

et al., 2016), the policy we study is useful to increase wages both for low-wage workers and for those further up in the wage distribution, and points to the limitation of remedying the outsourcing wage penalty with minimum wage increases (Bilal and Lhuillier, 2021). Fourth, the lack of negative effects on sectors with a high incidence of agency workers point to the reform causing a reallocation towards jobs with higher value added per worker (Acemoglu, 2001; Dustmann et al., 2021; Daruich et al., 2023), but that this effect is attenuated by increases in other costs, such as recruitment or screening (Autor, 2001; Abraham and Taylor, 1996; Houseman, 2001). Finally, the results suggest that the promotion of non-standard forms of employment might have contributed to the decrease in the labor share (Krueger, 2018; Autor et al., 2020), and that policies that increase workers' bargaining power can revert that (Stansbury and Summers, 2020; Dodini et al., 2023).

The rest of the paper is structured as follows. In Section 2 we present the data we use and the institutional context. Section 3 introduces a search-and-matching labor market model that incorporates a fissured workplace. In Section 4 we describe the empirical strategy and show evidence for wages, employment, and firm outcomes. Section 5 explains the robustness analysis. Finally, in Section 6 we detail the conclusions.

2 Data and the Reform of Temp Agency Employment

In this section we present the datasets that we use for the empirical analysis, the characteristics of temp agency employment in Spain, and the 1999 reform that exogenously increased the power of temp agency employees.

2.1 Data

We combine three different datasets to explore the consequences of the 1999 pay equalization policy. First, we use data from the Continuous Sample of Work Lives (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, the sector, whether the contract was open-ended or short-term, the location of the job, the workers' rank in the firm, and his 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.

In the empirical analysis we perform we include all the workers who were 16-65 years old between 2004-2008 and reconstruct their labor lives since 1997. To put it another way, we follow workers who were 16-58 years old in 1997 and explore what were the effects of the 1999 reform on their labor market outcomes.⁴

The second dataset contains information on non-financial corporations since 1995. 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 both temporary employment agencies and companies in other sectors. We report results for profits, value added, net sales, net purchases, labor costs, and value of production.

The third main dataset is administrative data on the number of temp agency contracts

⁴In Appendix C.1 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.

and workers aggregated at the user firm sector-level. This data is quite unique because, in most cases, administrative datasets do not have information of the sector where temp agency workers are deployed to. This is because temp agencies are the formal employers and, thus, are the ones that pay their social security contributions.⁵ We will use this information to understand how the reform affected the firms in the sectors that used temp agency workers more intensively, relative to firms in sectors with a lower incidence of this type of contracts. The data is from the Ministry of Labor and we use it to measure the exposure of each sector to the policy change.⁶ Additionally, the data also reports the number of temp agencies operating in Spain.

2.2 Temp Agency Employment

Temp agencies specialize in matching job vacancies with unemployed workers. Hence, they can increase labor market efficiency and promote job creation. For instance, [Autor \(2001\)](#) shows that temp agencies screen workers, or [Houseman \(2001\)](#) documents that they introduce numerical flexibility to firms. However, temp agencies might also erode working conditions. This erosion is possible because the user firm and the agency worker do not have an employment contract, and therefore the rules governing their relationship do not have to be in accordance with the collective agreements between the firm and its employees. Thus, employers might find it beneficial to reduce the number of direct workers to limit the wage premia associated to collective agreements. This view is consistent with the outsourcing wage penalty ([Dube and Kaplan, 2010](#); [Goldschmidt and Schmieder, 2017](#); [Drenik et al., 2023](#); [Bilal and Lhuillier, 2021](#)). Similar practices exist in the gig economy, where the use of independent contractors reduces workers' fringe benefits ([Weil, 2014](#); [Mas and Pallais, 2020](#)).

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

⁵In fact, the MCVL does not have information of the user firm sector in which temp agency employees actually work.

⁶The data series are known as *Empresas de trabajo temporal*.

put downward pressure on wages, while preserving the potentially positive effects in terms of job matching efficiency. 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%. In contrast, the wages of the other workers evolved smoothly during the pre- and post-reform periods. 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 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) confirm that the temp agency sector was booming before the reform, and that it declined after it.

The Spanish reform is an interesting case study for at least two reasons. First, at the time of the policy change, collective bargaining was quite centralized. In particular, the collective agreements reached at the industry and regional level between workers and employer groups were legally binding for all employers within the scope of the agreement. Hence, the use of temp agency work as a business strategy to cut labor costs was probably quite appealing, as the increasing trend in Figure 1, Panel (b), suggests.⁷

Second, the Spanish law provided no exemptions. 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

⁷In fact, collective bargaining coverage in Spain has been very high throughout the last decades, being around 90% in the early 2000s and around 80% nowadays (Card and Rica, 2006; Molina, 2023).

that most temp agencies used the exemption to avoid paying wages as those agreed between user firms and their in-house workers.⁸ 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.⁹ 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.¹⁰

2.3 Summary Statistics

Next, we present descriptive statistics for each of the three datasets that we employ in the empirical analysis. Table 1 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: contract 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). Hence, they are more likely to be more vulnerable than individuals employed directly by the companies that hired them. In the next section, we develop a model that takes into account these characteristics of temp agency employment to derive predictions about the effects of the 1999 pay equalization reform.

⁸We find similar exceptions in wage-setting for outsourced workers. See article 42.1 in *Estatuto de los Trabajadores*, the main labor law in Spain.

⁹In 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.

¹⁰The 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. Alternative, the Spanish Riders' law considered that food delivery workers were not independent contractors, but did not cover other workers in the gig economy.

Table 2 shows descriptive statistics for temp agency companies and firms in all other sectors. Columns (1) and (3) are for the year 1999, and columns (2) and (4) for the year 2000. Consistent with Figure 1, Panel d, we observe a decline in the number of temp agencies between the two years. Moreover, the averages of all variables, except purchases to other companies, improve for temp agencies, suggesting that the most productive of them stayed in the market after the reform. In contrast, the number of other companies increased during the same period, and its financial variables stayed quite constant.

Finally, Table 3 shows summary statistics of the “shock” variables that we will use in two of the empirical strategies, as we explain in Sections 4.2 and 4.4.2. They quantify the incidence of temp agency employment by province and by sector. For provinces, which are a close approximation to local labor markets in Spain, 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 52 provinces in Spain, and there is significant variation in temp agency usage across them, ranging from a minimum of 0 to a maximum of 3.1 in 1998. Consistent with Figure 1, we can observe that the mean usage of temp agency work declined between 1998 and 2001, from 1.356% to 1.074%, or -20.8%. These summary statistics suggest that the penetration of temp agencies into the rest of the economy was reversed significantly by the policy, a result we confirm more rigorously in the empirical section.

To calculate the incidence of temp agency employment by sector, we use the data from the Ministry of Labor, which reports the number of temp agency contracts by sector. The ratio is $\%TempAgency_{st} = \frac{TempAgencyContracts_{st} \times 100}{Workers_{st}}$, where $TempAgencyContracts_{st}$ is the number of temp agency contracts issued for workers deployed in sector s at year t . $Workers_{st}$ is the number of employees working in sector s and year t . We can see that the mean usage of temp agency contracts declined between 1998 and 2001, from 2.513% to 1.859%, or -26%. There is significant sector heterogeneity. In 1998, it ranges from a minimum of 0.023 in the housekeeping sector (followed by public administration and education). The sectors closest to the average are hospitality and transport. The sector with the most temp agency contracts is water transportation. In 2001, both the minimum and the maximum are lower than in

1998.

3 The theoretical model

In this section, we present a theoretical model to guide us in the empirical work of Section 4. We model a fissured labor market following a static version of the [Pissarides \(2000\)](#) model with large firms. We consider six distinctive elements. First, there are two types of large firms: identical in-house production firms, N_h , producing a single output good, and identical temporary work agencies, N_t , that provide temporary workers to the production firms. Second, there are two types of employed workers: in-house workers, E_h , directly hired by the production firms, N_h ; and temporary agency workers, E_t , directly hired by the temporary agencies, N_t . Third, there are matching frictions in the labor market and temporary agencies have a more efficient matching technology than production firms. Fourth, the wage of each type of worker is determined through a Nash bargaining process. We assume lower bargaining power for temporary agency workers. Mainly, because it is a sector with high rotation, and because there are two employers, making wage negotiations more difficult. Fifth, the workers' productivity is positively related to the job tenure in the firm, which is exogenous. Finally, some temp agency workers transition to in-house employees, and the probability of such event depends negatively on the temp agency wage penalty.

After presenting the model, we analyze the labor market effects of the 1999 reform introduced in temporary agencies. We model the policy change as an exogenous increase in the bargaining power of temp agency workers and derive the predictions in terms of employment, type of employment, wages and job transformation .

3.1 The matching functions

There is a mass 1 of homogeneous workers, N_h in-house production firms and N_t temporary agencies. Each production firm N_h hires its own in-house workers E_h by opening vacancies V_h , and also hires temporary agency workers E_t directly from N_t . In turn, each temporary agency has E_t workers who are hired by opening vacancies V_t . The aggregate number of employment and vacancies for a given type of worker is equal to $e_i = N_i E_i$ and $v_i = N_i V_i$

where $i = h, t$.

Unemployed workers u look simultaneously for jobs in the two types of job positions. Unemployed workers and vacancies are matched according to the matching functions $m_i = \eta_i m(u, v_i)$, which reflects a matching process that is costly. The parameter η_i captures the matching technology in each type of firm. Similar to [Neugart and Storrie \(2006\)](#), we assume that temporary agencies have a more efficient matching technology than production firms, implying that $\eta_t > \eta_h$. Both the unemployment rate u and the vacancy rates v_i are expressed as a fraction of a normalized labor force. The matching functions $m_i = \eta_i m(u, v_i)$ are non-negative, increasing in both arguments, concave and display constant returns to scale.¹¹

According to the properties of the matching function, unemployed workers find jobs at the rate $p_i(\theta_i, \eta_i) = \eta_i \frac{m(u, v_i)}{u}$, while vacancies are filled at the rate $q_i(\theta_i, \eta_i) = \eta_i \frac{m(u, v_i)}{v_i}$, where $\theta_i = \frac{v_i}{u}$ is the ratio of vacancies to unemployment known as labor market-tightness in each type of job position. According to the properties of the matching function, the larger the number of vacancies relative to unemployment, the more difficult is for a firm to fill a new vacancy $q'_{\theta_i}(\theta_i, \eta_i) < 0$. Conversely, the more vacancies per unemployed worker the more rapidly unemployed workers find jobs, $p'_{\theta_i}(\theta_i, \eta_i) > 0$.

3.2 Employment, unemployment and vacancy rates

Employed workers lose jobs at the exogenous rate s_i . We assume that $s_t > s_h$ since, according to [Table 1](#), the contract length $l_i = \frac{1}{s_i}$ is longer in in-house production firms than in temporary agencies. Thus, $l_h(s_h) > l_t(s_t)$. Moreover, temporary agency workers are transformed to in-house workers at the rate $\iota(\frac{w_h}{w_t})$. We assume that this job transformation rate increases with a reduction in the wage gap between temporary agency and in-house workers. More in detail, and given the wages of in-house workers w_h , an increase in the wages of temporary agency workers w_t pushes up the firms' incentive to convert temp agency workers to job positions with higher labor productivity. Thus, temporary agency contracts become more willing to be converted to more stable and productive jobs because they are not perceived as low paid jobs ([Booth et al., 2002](#)). At the equilibrium, the flows of workers and filled vacancies are

¹¹The assumption of constant returns to scale is consistent with available empirical work. See the survey by [Pissarides and Petrongolo \(2001\)](#).

equal to:

$$\left(s_t + \iota\left(\frac{w_h}{w_t}\right)\right) e_t = p_t(\theta_t, \eta_t)u = q_t(\theta_t, \eta_t)v_t, \quad (1)$$

$$s_h e_h - \iota\left(\frac{w_h}{w_t}\right) e_t = p_h(\theta_h, \eta_h)u = q_h(\theta_h, \eta_h)v_h, \quad (2)$$

Thus, equilibrium employment and the unemployment rates are equal to:

$$e_t = \frac{p_t(\theta_t, \eta_t)u}{\left(s_t + \iota\left(\frac{w_h}{w_t}\right)\right)}, \quad (3)$$

$$e_h = \frac{p_h(\theta_h, \eta_h)u + \iota\left(\frac{w_h}{w_t}\right)e_t}{s_h}, \quad (4)$$

$$u = 1 - e_t - e_f. \quad (5)$$

According to (1)-(2), the market equilibrium number of vacancies are given by:

$$v_t = N_t V_t = \frac{\left(s_t + \iota\left(\frac{w_h}{w_t}\right)\right)e_t}{q_t(\theta_t, \eta_t)}, \quad (6)$$

$$v_h = N_h V_h = \frac{s_h e_h - \iota\left(\frac{w_h}{w_t}\right)e_t}{q_h(\theta_h, \eta_h)}, \quad (7)$$

Since $e_i = N_i E_i$ it turns out that, at equilibrium, each firm opens vacancies according to:

$$V_t = \frac{\left(s_t + \iota\left(\frac{w_h}{w_t}\right)\right)E_t}{q_t(\theta_t, \eta_t)}, \quad (8)$$

$$V_h = \frac{s_h E_h - \iota\left(\frac{w_h}{w_t}\right)E_t \frac{N_t}{N_h}}{q_h(\theta_h, \eta_h)}, \quad (9)$$

3.3 Firms

In line with the human capital theory (Becker (1964)), job tenure in our model reflects higher levels of accumulated human capital through investments specific on-the-job learning, which increases workers' productivity. Thus, we assume that workers generate a productivity $A(l_i)$ that increases with the worker's duration in the firm $l_i(s_i)$. Since $l_h(s_h) > l_t(s_t)$, then we

assume that $A_h(s_h) > A_t(s_t)$.¹² Thus, the quality of each job increases with the job duration in the firm.

The N_h identical price-taking production firms produce a single output good Y_h according to $Y_h = A_h E_h + A_t E_t$. An exogenous proportion s_i of workers E_i break away. To replace its own workers E_h , the production firm needs to open vacancies V_h , incurring in an exogenous cost c per vacancy. Moreover, production firms can avoid incurring in vacancy costs by hiring workers directly from the temporary agency firms. In this case, each production firm pays ξ per worker to a temporary agency. Each temporary agency N_t hires its own workers E_t by opening vacancies V_t at the exogenous cost c .

For a given labor market tightness and wages, each production firms chooses its number of vacancies V_h and the number of temporary agency workers E_t that maximize the profits function Π_h :

$$\max_{V_h, E_t} \Pi_h = A_h E_h + A_t E_t - w_h E_h - \xi E_t - c V_h, \quad (10)$$

In turn, a temporary agency chooses its number of vacancies V_t that maximize the profits function Π_t :

$$\max_{V_t} \Pi_t = \xi E_t - w_t E_t - c V_t. \quad (11)$$

Substituting the values of vacancies (8)-(9) in equations (10) and (11), and solving the profit maximization problem for each firm, the first order conditions of the production firm and the temporary agency are equal to:

$$A_t = \xi, \quad (12)$$

$$A_h = w_h + \frac{c s_h}{q_h(\theta_h, \eta_h)}, \quad (13)$$

$$A_t = w_t + \frac{c(s_t + \iota(\frac{w_h}{w_t}))}{q_t(\theta_t, \eta_t)}. \quad (14)$$

¹²Recent empirical evidence shows that job tenure $\frac{1}{s_i}$ has a positive relationship with labor productivity (See [Gagliardi et al. \(2023\)](#)). Since job separation rates are exogenous in our model, we just assume that $A_h > A_t$.

Condition (12) tells us that production firms will pay to the temporary agencies the productivity of the temp agency workers, A_t . Condition (13) implies that firms N_h hire in-house workers until the marginal productivity is equal to the total labor costs that includes the wage w_h and the expected cost of opening a new vacancy $\frac{cs_h}{q_h(\theta_h, \eta_h)}$. Finally, condition (14) shows that firms N_t hire temporary agency workers until the marginal productivity is equal to the expected cost that includes the wage w_t and the expected cost of opening a new temporary agency vacancy $\frac{c(s_t + \iota(\frac{w_h}{w_t}))}{q_t(\theta_t, \eta_t)}$. Notice that the temporary agency does not only incur in new vacancy costs if the worker moves to unemployment at the rate s_t , but also if there is a job transformation into an in-house positions at the rate $\iota(\frac{w_h}{w_t})$.

3.4 Wages determination

We assume that wages are determined by a Nash bargaining process between workers and firms, where workers have an explicit bargaining power β_i and firms $1 - \beta_i$. We consider $\beta_t < \beta_h$ because the characteristics of the temp agency sector make it more difficult for trade unions to organize. If there is an agreement between workers and firms, they receive w_i and $A_i - w_i$, respectively. If they disagree, workers receive an outside income b (e.g. unemployment income) and firms incurs in a vacancy cost c_i . The wages derived from the Nash bargaining solution maximises the weighted product of the representative worker's and the firm's net return from the job match. Therefore, each wage must satisfy the following condition:

$$w_h = \operatorname{argmax}\{(w_h - b)^{\beta_h}(A_h - w_h + c)^{(1-\beta_h)}\} \quad (15)$$

$$w_t = \operatorname{argmax}\{(w_t - b)^{\beta_t}(A_t - w_t + c)^{(1-\beta_t)}\} \quad (16)$$

Differentiating with respect to w_i , the following wages are obtained:

$$w_h = \beta_h A_h + \beta_h c + (1 - \beta_h)b. \quad (17)$$

$$w_t = \beta_t A_t + \beta_t c + (1 - \beta_t)b. \quad (18)$$

Notice that the wages of agency workers, w_t , will be lower than the wages of in-house workers, w_h , both because $\beta_t < \beta_f$ and $A_h(s_h) > A_t(s_t)$

3.5 Equilibrium

Wages and labor market tightness in production firms are determined according to the job creation condition (13) and wage equation (17). More in detail, the equilibrium wage of the in-house workers w_h and its labor market tightness θ_h are obtained according to:

$$A_h = w_h + \frac{cs_h}{q_h(\theta_h, \eta_h)},$$

$$w_h = \beta_h A_h + \beta_h c + (1 - \beta_h)b.$$

In turn, the wage and labor market tightness in the temporary agency workers are determined according to equations and (14) and (18):

$$A_t = w_t + \frac{c(s_t + \iota(\frac{w_h}{w_t}))}{q_t(\theta_t, \eta_t)},$$

$$w_t = \beta_t A_t + \beta_t c + (1 - \beta_t)b.$$

Finally, since unemployed workers u looks for jobs simulatively in both markets, the equilibrium employment and unemployment rates are obtained simultaneously. Thus, using equations (3)-(5), the equilibrium employment and unemployment rates are equal to:

$$e_t = \frac{p_t(\theta_t, \eta_t)s_h}{(s_t + \iota(\frac{w_h}{w_t}))(s_h + p_h(\theta_h, \eta_h)) + (s_h + \iota(\frac{w_h}{w_t}))p_t(\theta_t, \eta_t)}, \quad (19)$$

$$e_h = \frac{p_h(\theta_h, \eta_h)(s_t + \iota(\frac{w_h}{w_t})) + \iota(\frac{w_h}{w_t})p_t(\theta_t, \eta_t)}{(s_t + \iota(\frac{w_h}{w_t}))(s_h + p_h(\theta_h, \eta_h)) + (s_h + \iota(\frac{w_h}{w_t}))p_t(\theta_t, \eta_t)}, \quad (20)$$

$$u = \frac{(s_t + \iota(\frac{w_h}{w_t}))s_h}{(s_t + \iota(\frac{w_h}{w_t}))(s_h + p_h(\theta_h, \eta_h)) + (s_h + \iota(\frac{w_h}{w_t}))p_t(\theta_t, \eta_t)}. \quad (21)$$

Because of the properties of the matching technology, the job creation curves (13) and (14) can be represented by a downward-sloping curve (JC_i) in the $(w_i; \theta_i)$ space as it is shown in Figure 2. When w_i is higher, firms realized that it is less profitable to open vacancies, reducing the number of vacancies and, therefore, the labor market tightness θ_i falls. In turn, according to the wage equation (17) and (18), wages do not depend on θ_i . This results in a constant wage curve in the $(w_i; \theta_i)$ space as shown in Figure 1. There is only one wage and labor market tightness that satisfy the equilibrium in each market. Once the labor

market tightness and wages in each market are determined, the equilibrium employment and unemployment rates are obtained simultaneously according to equations (19), (20) and (21). Figure 2 shows that e_i increases with θ_i because it rises the job finding rate $p_i(\theta_i, \eta_i)$.

3.6 The effects of the 1999 reform

Next, we analyze the labor market effects of the 1999 reform introduced in temporary agencies. As we already mentioned, the reform implies that agency workers must earn a wage according to the collective agreement of the production firm. We think the reform is well captured by an exogenous increase in the wage bargaining power of temporary agency workers from β_t to the one observed in production firms β_f . Using the framework presented above, we derive the consequences of the policy change, which we summarize in the following proposition:

Proposition 1. *We define the policy change as an increase in the workers bargaining power of temporary agencies from β_t to β_h . This is a policy that:*

- *Proposition 1.1: Increases the wages of temp agency workers.*
- *Proposition 1.2: The wages of in-house workers are not affected.*
- *Proposition 1.3: Increases transformations from temp agency to in-house employment.*
- *Proposition 1.4: Reduces employment in temporary agencies.*
- *Proposition 1.5: There is an ambiguous effect in-house employment and unemployment.*
- *Proposition 1.6: Temp agency workers who transition to in-house have longer job durations and are more productive.*

Proof. See Appendix D.

Graphically, the increase in β_t until it reaches β_h shifts up the wage equation in the temporary agency labor market as shown in Panel (b)-top of figure 3. Hence, the wages of temp agency workers increase. The increase in w_t increases the job transformation rate from temporary agencies to production firms $\iota(\frac{w_h}{w_t})$, increasing the expected hiring costs of

temporary agency workers. Thus, the job creation curve shifts down and, as a consequence, temp agency firms decrease their number of vacancies v_t , reducing the labor market tightness in this sector. A lower θ_t reduces the worker's rate of finding a job in a temporary agency $p_t(\theta_t, \eta_t)$ and, therefore, reduces the temp agency employment rate e_t as shown in panel (b)-bottom of Figure 3. Notice also that the higher job transformation rate generates an additional reduction in e_t , rotating down the temp agency employment curve.

In turn, there is no change in θ_h because the reform does not affect the workers' bargaining power in production firms, β_h . Thus, the wages of in-house workers are not affected by the reform (Figure 3, Panel a-top). The employment effects of the reform on in-house workers are unclear because a higher ι increases e_h while a lower $p_t(\theta_t, \eta_t)$ reduces the number of temporary agency workers that can be converted to in-house positions, reducing e_h . Graphically, the employment curves of in-house contracts can rotate up or down as shown in panel a-bottom of figure 3. Similarly, the unemployment effects of the reform are also ambiguous.

4 Empirical strategy and Results

4.1 Wage Effects

We begin the exploration of the consequences that the policy had on the wages of temp agency workers. We implement a difference-in-differences specification with individual and month fixed effects. Temp agency employees are the treatment group, and in-house workers the control group. Note that using in-house workers is justified theoretically, since the model predicts no wage effect on them, and by the raw data in Panel (a) of Figure 1, which shows a stable and parallel evolution of the wages of in-house workers relative to temp agency employees. The specification is as follows:

$$y_{it} = \alpha + \beta_1 treatment_i + \beta_2 post_t + \beta_3 treatment_i \times post_t + \gamma_t + \rho_p + \delta_i + \psi_c + \beta_4 X_{it} + \mu unemp_{pt} + \epsilon_{it} \quad (22)$$

where y_{it} is the logarithm of the real daily wage of worker i in month t . $treatment_i$ is equal to 1 for temp agency workers, and 0 for in-house workers. $post_t$ is 1 for months after the policy

change, and 0 otherwise. δ_i are individual fixed effects and γ_t are month fixed effects. ψ_c are worker-rank fixed effects and X_{it} are individual controls such as third-order polynomials for age and experience, firm size, contract type, part-time job, gender, etc.¹³ Similarly, we also perform an event-study approach, which allows us to see if there are systematic pre-treatment trends. The regression is:

$$y_{it} = \alpha + \psi post_t + \sum_{j=-5, j \neq -1}^5 \beta_j treatment_i + \gamma_t + \rho_p + \delta_i + \psi_c + \kappa X_{it} + \mu unemp_{pt} + \epsilon_{it} \quad (23)$$

The identification assumption is that the wages of treated and control workers were following parallel trends before the reform. The results are in Figure 4, Panel (a), and show no evidence of pre-treatment trends. Moreover, the coefficient becomes positive and significant after the reform, as predicted by the model. We report the estimates in Panel A of Table 4. The first column reports results with no fixed effects and no control variables other than the unemployment rate. The second column includes time fixed-effects, location fixed-effects and many firm and worker control variables. The third column is our preferred specification and includes individual fixed-effects and worker and firm controls. According to the preferred estimation in Column 3, we measure a significant increase of 12.9%.

A potential concern with the previous analysis is that the estimation might be biased by changes in the composition of temp agency employment caused by the policy (recall Figure 1, Panel (b)). For that reason, Panels (b) and (c) display results for two different samples of treated workers who stay employed throughout the period of analysis. In Panel (b) the treatment group is composed of all temp agency workers who stay employed in the temp agency sector 6 months before and after the reform, and the control group are all non-agency workers who stay employed throughout the same period. In Panel (c), the treatment group are all workers who were continuously employed by temp agencies the 6 months before the reform, became in-house workers after it, and stayed in these jobs at least until 6 months after the policy. The control group is the same as in Panel (b). As can be seen, in both cases, there is no evidence of pre-policy trends, and there is an immediate and positive impact on

¹³The worker-rank variable is based on the 11 contribution groups defined by the social security system to collect payroll taxes. The groups approximate the worker category in the company.

wages once the new law is enacted. The estimates in Table 4 are consistent with the figures. The wage increases are significant in both cases and equal to 10.9% for workers who stay in the temp agency sector, and 5.3% for those who transition to in-house jobs. In Section 5 we provide additional placebo exercises that fail to detect any positive effect on wages.

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 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 (-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 \(2021\)](#)). However, does this come at an employment cost? In the next subsection we answer this question.

4.2 Employment Effects: Province-Level Analysis

Next, we continue by analyzing the impact of the policy on employment. The spatial distribution of temp agency workers across provinces was heterogenous in 1998, the year before the reform, as we showed in Table 2. The idea of the identification strategy is that provinces with a higher share of temp agency workers should experience 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.

Figure 5, Panel (a), is a visual example of the spatial variation. It shows the percentage of temp agency workers for several provinces over time. We can see that, before the reform, the temp 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. Therefore, the specification needs to account for heterogenous linear time trends across provinces. 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. ¹⁴

Panels (c) and (e) plot the percentage of in-house and overall employment, respectively. The evolution of these variables is positive over time, and there is no obvious change in trend that coincides with the policy. Nevertheless, the scale of the graphs makes it difficult to observe, since the values for both variables are much larger than those of temp agency employment. In any case, for consistency we also detrend these outcome variables, but also shows results without detrending them. The dynamic difference-in-differences specification is:

$$\hat{y}_{pt} = \alpha + \delta_p + \delta_t + \sum_{j=-4, j \neq -1}^4 \beta_j 1[j = t] \%TempAgency_{p,98} \times post_t + \sum_{j=-4}^4 \gamma_j 1[j = t] X_p + \epsilon_{pt} \quad (24)$$

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, or continuous treatment, $\%TempAgency_{p,98}$ each year. This variable measures the incidence of temp agency employment in each province in 1998, the year before the reform.¹⁵ Following the theoretical model, we expect that the impact of the policy on temp agency employment is much more negative in the provinces where this sector had expanded more. \hat{y}_{pt} is 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. The hat indicates that we have removed province-specific linear trends using pre-treatment data. The specification also includes interactions of year dummies with controls at baseline, X_p . Specifically, we control for the unemployment rate, a

¹⁴See [Dustmann et al. \(2021\)](#) and [Elias et al. \(Forthcoming\)](#) for recent papers using similar empirical strategies. Appendix D in [Elias et al. \(Forthcoming\)](#) provides a very detailed justification of the empirical strategy.

¹⁵For a more detailed discussion of this variable see Section 2.

dummy for coastal provinces, the share of the construction sector, the share of immigrants, the average firm size and the share of workers with no university degree at baseline.

The evidence from the regression analysis is in the right column in Figure 5 and in Table 5. Panels b, d and f in the figure show the results for temp agency employment, in-house and overall employment. We extract four main conclusions from them. First, there are no significant nonlinear differential pre-treatment trends for any of the three outcome variables. Second, there is a clear negative effect on temp agency employment (Panel b), consistent with model predictions. In light of Panel (a), most of this effect is because the sector stops growing, and this effect is largest in the provinces with a higher incidence of temp agency work before the policy.

Third, in-house employment increases more in the provinces that had a higher share of temp agency work (Panel (d)). In view of the model, this result provides evidence in favor of the job transformation channel, since it suggests that, prior to the reform, user firms were transforming in-house jobs to temp agency jobs, and that the law change reverted this trend. Alternatively, it suggests that the job creation channel, according to which the temp agency sector provides workers to user firms susceptible of becoming in-house employees, is less important.

Fourth, overall employment also increases more in the provinces with a higher percentage of temp agency employment before the policy. This result implies that the positive effect on in-house employment is stronger than the negative one on temp agency employment. We interpret this outcome as showing that the reform not only increases the wages of temp agency workers, but also improves job conditions such as the duration of their contracts. In other words, before the reform, the expansion of temp agencies was shortening employment spells, and the policy change turned this effect around. ¹⁶

Table 5 reports the coefficients from the difference-in-differences analysis. ¹⁷ For each de-

¹⁶In Appendix C, Figure A3 we display the event-studies without detrending the outcome variables. For in-house and overall employment, the coefficients show a U-shape, which reinforces the interpretation that, before the reform, both in-house and overall employment were decreasing faster in the provinces that in 1998 had a higher share of temp agency employment, and that this trend was overturned by the reform.

¹⁷In this case, the specification is $\hat{y}_{pt} = \alpha + \delta_p + \delta_t + \rho_1 \%TempAgency_{p,98} + \rho_2 post_t + \rho_3 \%TempAgency_{p,98} \times post_t + \sum_{j=-4}^4 \gamma_j 1[j = t]X_p + \epsilon_{pt}$.

pendent variable, we show results with and without controls. For the preferred specification with controls, a 1pp increase in the shock variable lowers temp agency employment significantly by 0.32pp, or 23.6% relative to the pre-reform mean. In contrast, a 1pp increase in the percentage of pre-reform temp agency work increases in-house employment significantly by 0.8pp after treatment. The overall effect on employment is positive, not significant, and equal to 0.5pp.

All in all, despite the negative effects that the reform had on the evolution of temp agency employment, the exercises in this section rule out a negative effect on overall employment, and suggest that it actually had positive effects by increasing the employment spells. In the next section, we return to this point and provide evidence that confirms it. Finally, in Section 5 we discuss several robustness checks.

4.3 Employment and Wage Effects for Incumbent Temp Agency Workers

Next, we turn to analyze the impact of the reform on incumbent temp agency workers when the policy is implemented. Relative to the province-level results we just presented, working with individual level data can provide more fine-grained insights into how the policy change affects the labor market.

4.3.1 Employment

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 1999.¹⁸ 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 6, 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:

$$y_{ipmt} = \alpha + \delta_i + \delta_m + \delta_p + \sum_{j=-5, j \neq -2}^5 \beta_j 1[j = m] Treatment_i + \mu unemp_{pt} + \epsilon_{ipmt} \quad (25)$$

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 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 .¹⁹

¹⁸In 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.

¹⁹We omit period -2 because the condition for being on the sample is being in a temp agency employment when $t = -1$. In Figure A5 in the appendix we show the results do not change when we instead omit period -1.

Figure 6 displays the estimates of the event study specification. The coefficients are in Table 6.²⁰ 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 4.2. In particular, the reform decreases employment in the temp agency sector (Figure 6, Panel (b)). The negative effect is a significant -6.1pp on average (Table 6). In contrast, in-house employment increases by a similar amount, or 6.17pp. The effect on overall employment is positive, small, insignificant, and equal to 0.07pp. Consistent with the previous results, the number of transitions from temp agency to in-house jobs increases significantly by 0.98pp (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 continuously after the law change, with the average contract length being 25% higher in the treatment group 5 months after the reform. Part of this increase is explained by the significant positive effect on open-ended employment of 2.6pp (42% of the increase in in-house employment), as these contracts have no predetermined end date, are thus more stable, and have better career prospects. This effect on open-ended employment implies that some workers also gained an entitlement to severance payments of 45 days of salary per year worked in case of wrongful dismissal.²¹

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-employment and public employment. We think these two variables can be interpreted as

²⁰The 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.

²¹In case of fair dismissal, the worker is entitled to 20 days of salary per year worked. However, Elias (2023) 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.

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. We discuss a placebo and additional robustness checks in Section 5.

4.3.2 Elasticity Estimation

In this section, we use the employment and wage estimates to calculate the own-wage employment elasticity (Cengiz et al., 2019). In our case, this elasticity tells us how the employment of the specific group affected by the policy, temp agency workers, responds to the increase in wages caused by linking the wages of temp agency workers to the collective agreements of user firms. This exercise is useful because we can compare the elasticity with those from other wage policies, such as the minimum wage. The formula is:

$$\text{Own-wage employment elasticity} = \frac{\% \Delta \text{Affected Employment}_t}{\% \Delta \text{Affected Wage}_t} \quad (26)$$

We use the employment estimate for incumbent temp agency workers as the numerator. This number has the advantage of being based on a sample of workers targeted by the policy. The $\% \Delta \text{Affected Wage}_t$ is the actual wage increase for affected workers. For that, we use the three employment estimates in Section 4.1.

We report the elasticities in Table 7. The standard errors were calculated using the delta method. The three estimates are very close to zero, positive and insignificant. They range between 0.003 and 0.005. The confidence intervals rule out any own-wage elasticity more negative than -0.1 at the 95% confidence level.

Two considerations are important for a correct interpretation of the elasticity. First, the employment estimate measures the average effect six months after the policy. Hence, it is a short-run elasticity. Given the evidence for larger job durations after the reform (Figure 6, Panel (f)), and the positive employment effects of the province analysis (Figure 5, Panel (c)), we think the reported elasticity is likely to be a lower bound. Second, the employment estimate is conditional on being a temp agency worker in the pre-treatment period. Hence, it does not capture potentially negative effects on hires that might stem from the policy. However, the positive employment effects found in the province analysis suggest the impact on hires was rather small or zero.

Finally, the elasticities estimates in this paper are in the upper bound, but similar, to those reported in (Dube, 2019), who pool across 36 estimates from US minimum wage studies and find a median elasticity of -0.17. Therefore, mandating that temp agency workers have to be paid according to the collective agreement of user firms is a useful tool to increase wages with little or no negative effects on employment. Moreover, the proportionate change in employment relative to the change in wage is similar to that found in the minimum wage literature.

4.3.3 Wage Effects across the Distribution

While in the previous section we have emphasized the similarities between the minimum wage and the policy studied in this paper, we now highlight a key difference between the two. We show that forcing user firms to pay temp agency workers according to the company's collective agreement increases wages across a wide range of the wage distribution, and not only for low-wage workers. Furthermore, we can estimate this effect precisely. This stands in contrast with the evidence that shows that minimum wages have quite limited spillover effects (Cengiz et al., 2019), which are also hard to measure with precision (Autor et al., 2016).

We work with the sample of workers who stay employed by the same temp agency before and after the policy. We do so to keep job characteristics constant, and not confound employer changes with the effects of the policy on wages. Nevertheless, we obtain similar results when we analyze a sample of workers who transition to in-house after the policy. We report these results in Appendix C, Figure A6.²²

We begin by showing raw data of the wage distributions of different workers before and after the policy change in Figure 7. In Panel a we display it for individuals who stay employed in temp agencies 6 months before and after the reform. As is apparent, the wage distributions shift to the right after the policy change. In contrast, Panel b depicts no

²²The difference here is that we cannot be sure that the employees are working for the same firm before and after the policy. Before the policy, we observe them as temp agency workers. After the policy, we know they are not employed by temp agencies, and they appear as employed by another company, but we cannot confirm this is the same firm where they were deployed.

movement in the wage distribution of in-house workers. Panel c shows the first differences of the wage distributions for temp agency workers before and after the policy (red line) and for in-house workers (black line). It can be seen that the workers affected by the policy move up throughout an area already above the minimum wage.

To estimate precisely the wage growth across the distribution we follow the frequency distribution approach used in the minimum wage literature (Cengiz et al., 2019; Giupponi et al., 2022; Gopalan et al., 2021). That is, we put workers into one euro daily wage bins and estimate the increase in wages for each bin. Note that we want to capture wage growth, which is likely to be a one-time event associated to the policy. Therefore, we will use a sample of workers employed the month before and after the policy. The specification is as follows:

$$\begin{aligned}
 y_{ij,t+1} = & \alpha + treatment_i + post_t + \sum_k \gamma_j \mathbb{1}[bin_j = k] + \sum_k \beta_j \mathbb{1}[bin_j = k] \times treatment_i \times post_t \\
 & + \mu_{ij} + \rho_{jt} + \epsilon_{s_{jt}}
 \end{aligned}
 \tag{27}$$

where $treatment_i$ is equal to 1 for workers who were temp agency employees before the reform, and 0 for those who were in-house workers. $post_t$ is equal to 1 for the month after the policy. $y_{ij,t+1}$ is the log wage for worker i in bin j at time $t + 1$. bin_j is the wage in bin j at time t . Hence, we include bin fixed effects. μ_{ij} are wage-bin fixed effects common to either the treatment or control group. ϵ_{ijt} is the error term. The β_j are the coefficients we are interested in and measure the log wage effect for each bin j .

The results are reported in Panel (d) of Figure 7. The evidence shows significant increases for workers earning wages much higher than the minimum wage. Most of the effects happen for salaries up to 65 euros per day, which is 4 times the 16 euros of daily minimum wage in 1999. We still detect positive wage growth for workers earning 92 euros per day. In Panel (e) we show that these effects correspond to the 65th and 92nd percentile of the wage distribution.

4.4 Effects on Firms: Sector-Level Analysis

In Sections 4.1-4.3, we documented that the reform increased the wages of temporary agency workers. Moreover, there were no negative effects on employment. Instead, the results indicate positive impacts on employment in the long-run, as a consequence of improvements in job transformations to in-house positions, implying more job stability and higher contract length. Therefore, the workers win from the policy.

But, who are the losers? In Figure 1, Panel (d), we already showed that the temp agency sector declined after the policy. In this section, we quantify the negative impact that the temp agency sector suffered. The results show that temp agencies were the main losers from the reform. Additionally, we investigate user firms, who adjust to the policy by increasing their number of in-house employees and the value of their production, with positive effects on productivity and profitability, though not significantly different from zero.

4.4.1 Temporary Agencies

To quantify the impact of the policy on temp agencies, we work with two sets of variables. On the one hand, the number of temp agencies operating and their sector-level sums of net sales, value added and labor costs. These variables are useful to measure the aggregate losses suffered by temp agencies following the reform. On the other hand, we use sector-level measures of productivity per worker and profit per worker. For these two variables, the number of workers, or the denominator, includes both in-house and temp agency workers.²³ These variables are useful to understand if the temp agency sector became relatively more productive after the reform. The specification is:

$$\hat{y}_{st} = \alpha + \delta_s + \delta_t + \sum_{j=1995, j \neq 1998}^{2003} \beta_j 1[j = t] Treatment_s + \sum_{j=1995, j \neq 1998}^{2003} \gamma_j 1[j = t] X_s + \epsilon_{st} \quad (28)$$

where \hat{y}_{st} is a sector-level firm variable such as number of firms or profits, and the hat indicates that we have removed pre-reform sector-specific trends. The temp agency sector was booming in the pre-policy years and the law change affected its trend. Hence, to measure

²³We know in which sector the temp agency workers were deployed to thanks to the dataset “Empresas de trabajo temporal”, which we explained in Section 2.

the impact of the reform we need to account for this pre-reform heterogeneous trends.²⁴ δ_s and δ_t are sector and year fixed effects. $Treatment_s$ equals 1 for the temp agency sector, and is 0 otherwise. X_s is a set of pre-reform sector-level mean variables that we interact with year dummies and include as controls. More specifically, we control for firm age, firm size, percentage of public firms, percentage of publicly traded firms, and a sector-level Herfindahl-Hirschman Index of concentration. The control group is composed of the sectors in the first quartile of temp agency penetration. We do that because these are the sectors less affected by the policy, and hence less likely to experience an increase in labor costs.²⁵

We display the results in Figure 8 for several firm variables. We derive two main insights from them. First, economic activity in the sector declined very significantly. Panels (a) - (d) show reductions in the number of temp agencies operating, value added, net sales, and the labor costs of their own, internal, workers. In Table 8 we report the coefficients. The top panel reports them including 2 years after the policy, and the bottom panel taking into account up to 4 years after the reform. Looking at the top panel, we observe that the number of temp agencies is 37.8% smaller. Given that the sector was expanding very fast the years before the reform, we interpret the effect as largely measuring missed sectoral growth. In other words, that absent the policy, there would have been an increase in the number of temp agencies, but it never happened because of the reform. The effect on net sales imply large losses of 128%. Similarly, the sector value added or its labor costs were 63.6% and 67.5% smaller two years after the reform. These results are consistent with the decline in temp agency employment reported in Section 4.2. To be clear, if the main activity of temp agencies is to “sell” workers to user firms, a lower share of temp agency employment necessarily implies that the agencies are selling less and, hence, that the sector size is shrinking.

The second remark is that, despite temp agencies being the main losers from the reform, the average productivity and profitability per worker of the sector was not impacted significantly, though the estimates are negative. The results are in Figure 8, Panels (e) and (f), or

²⁴See the discussion for Equation 24 for a more detailed explanation of this procedure.

²⁵In Section 5 we show that the results are robust to changes in the control group. In particular, we repeat the analysis with sectors below the second decile of temp agency incidence, and with sectors below the third decile.

in Columns 5 and 6 in Table 8. The lack of an impact on productivity and profitability per worker is not surprising because there are offsetting effects. Theoretically, value-added per worker is $\frac{\xi E_t - cV_t}{E_{ht}}$, where E_{ht} are the own internal workers of temp agencies. On the one hand, value-added per worker shrinks because E_t decreases, which we have shown empirically in Section 4.2 and here as a decrease in net sales (Panel (c)). On the other hand, we have shown that temp agencies responded to the negative production shock by decreasing their own internal workers (E_{ht} , or Panel (d)). Additionally, we would also expect the negative effect on productivity to be offset because hiring costs decrease, as a consequence of there being less job postings in the sector.

4.4.2 Companies in Other Sectors

To understand the effects of the reform on user firms, we exploit variation on the incidence of temp agency work per sector. For that, we use the variable $\%TempAgency_{s,1998}$, which measures the percentage of temp agency workers in each sector in 1998.²⁶ Intuitively, sectors with more temp workers in 1998 experience a higher labor cost shock after the reform and, hence, should react more strongly than sectors with less temp agency workers. The specification is the same as Equation 28, but the main explanatory variable is now $\%TempAgency_{s,1998}$:

$$\hat{y}_{st} = \alpha + \delta_s + \delta_t + \sum_{j=1995, j \neq 1998}^{2003} \beta_j 1[j = t] \%TempAgency_{s,1998} + \sum_{j=1995, j \neq 1998}^{2003} \gamma_j 1[j = t] X_s + \epsilon_{st} \quad (29)$$

We begin by confirming that the policy increased the number of in-house workers more in the sectors with a higher penetration of temp agency work, as we expect based on the results in Section 4.2. The evidence is in Figure 9, Panel (a), and shows a positive effect the same year of the reform, that continues growing the following years. We report the coefficients in Table 9, both up to 2 years and 4 years after. We detect increases of .52% or .74%, respectively.²⁷

²⁶We introduced this variable in Section 2.

²⁷Recall that in Section 4.2 this effect was positive and significant. We think the reason we fail to estimate a significant effect now is that the outcome variable is the number of workers, and not the number of months workers are employed.

In Figure 9, Panel (b), we use as an outcome variable the sum of in-house and temp agency workers in each sector. In this case, the evolution of total workers is very similar pre- and post-treatment. Column 3 in Table 9 shows negative effects of -.23 and -.3%.²⁸ Additionally, we also expect that user firms “buy” less workers from temp agencies, and this should be reflected in their purchases from other firms. Panel (c) in Figure 9 displays the coefficients for this variable, which are negative, though not significant (-2.8% and -4.8% in Table 9, Column 4).

Last, we want to know how the productivity and profitability of user firms responded to the policy. Note that the value added of user firms is $A_h E_h + A_t E_t - w_h E_h - \xi E_t - cV_h$, which can be reorganized as $A_h E_h - cV_h + E_t(A_t - \xi)$. The first term is the contribution to value-added of in-house workers, and the second term that of temp agency workers. Since in equilibrium, $A_t = \xi$, it can be expressed as $A_h E_h - cV_h$. Empirically, we have shown that E_h increases because many temp workers become in-house employees. Consequently, we expect that V_h increases too. Therefore, productivity will only increase as long as the temp workers are reallocated to more productive jobs that compensate the higher hiring costs. Similarly, profits will only increase if the reallocation to more productive jobs compensates both higher hiring and wage costs ($E_h(A_h - w_h) - cV_h + E_t(A_t - \xi)$).

The results are in Panels (e) and (f) in Figure 9. Both productivity and profit per worker increase, though the estimates are not significant, suggesting that temp agency workers are reallocated to more productive jobs, but that the effects are attenuated by the increases in hiring and wage costs.

All in all, the evidence for user firms shows that they adjusted to the policy by increasing their number of in-house workers and decreasing their purchases to other firms (potentially, temp agency workers). Moreover, the results indicate weak increases in productivity and profitability, suggesting that labor market regulations that improve job conditions promote the reallocation to more productive jobs (Acemoglu, 2001; Dustmann et al., 2021; Daruich et al., 2023).

²⁸Recall that in Section 4.2 this effect was positive and insignificant. We think the reason we fail to estimate a positive coefficient now is that the outcome variable is the number of workers, and not the number of months workers are employed.

5 Robustness analysis

In this section, we explain a few more empirical exercises that highlight the reliability of the results. First, we perform a placebo exercise in which we estimate the effect of a placebo policy in August 1997. In other words, we see if the policy implemented in 1999 affected outcomes before and after August 1997. We expect to obtain estimates that are not distinguishable from zero. The results for wages are in Table A1. For our preferred specification, including many controls (Column 3), the estimates are one or two orders of magnitude smaller than in the main results (Table 4). For the sample of all temp-agency workers and for those who transition to in-house, we fail to detect significant effects. Only for those that stay as temp agency employees before and after the placebo policy we detect a significant effect, though it is negative. The results for the placebo event studies are in Figure A2 and are consistent with the coefficients in Table A1.

Next, in Table A2, we report the estimates of the placebo exercise for employment outcomes at the province-level. The coefficients for temp agency employment are one or two orders of magnitude smaller than the main estimates in Table 5. Moreover, the estimates for in-house and overall employment are not distinguishable from 0 when we include control variables.

Figure A4 displays placebo estimates for incumbent temp agency workers. None of the placebo event-studies show a consistent effect. We report the coefficients in Table A3, which are consistent with the visual evidence.

Table A4 provides several robustness checks for the effect of the policy on the temp agency sector. Panel A displays the estimates for a placebo policy in August 1997. As can be seen, most estimates are not significant or have a different sign than the main results. Panels B and C shows the results without including controls, up to 2 and 4 years after the reform, respectively. Similarly, Panels D-E and F-G report the results changing the sectors that are included as control group (below 30th or 20th percentile of temp agency penetration, respectively). The events-studies without adding controls and changing the sectors in the control group are in Figures A8-A10.

Table A5 shows robustness checks for the effect of the policy on the sectors that used

temp agency workers. Panel A is for a placebo policy in August 1997, and Panel B and C do not include controls, and measure the impact up to 2 or 4 years after the reform, respectively. The results are also depicted in Figure [A12](#). Overall, they are consistent with those in Table [9](#) and Figure [9](#).

6 Conclusions

Temporary agency work, like other forms of outsourcing, has been linked to lower wages and higher inequality. In this paper, we show that paying agency workers according to the collective agreement of user firms increases their wages without negative employment effects. Moreover, wage growth happens all along the wage distribution, and some workers transition to in-house open-ended contracts, which increases their job duration and offers them higher employment protection. The temp agency sector declines significantly, while we detect no negative effects for sectors with a high incidence of temp agency work. Overall, the evidence highlights that tying the wages of agency workers to those established in the collective agreements of the firms they are deployed to redistributes away from the agencies to the temp agency workers. A relevant question is whether the results hold for other situations in which the labor relationship has been fragmented, be it in firms that outsource certain activities or in the gig economy.

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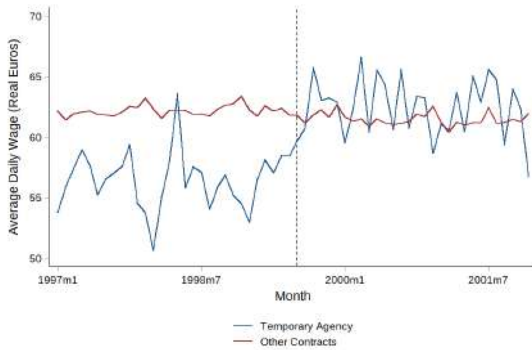
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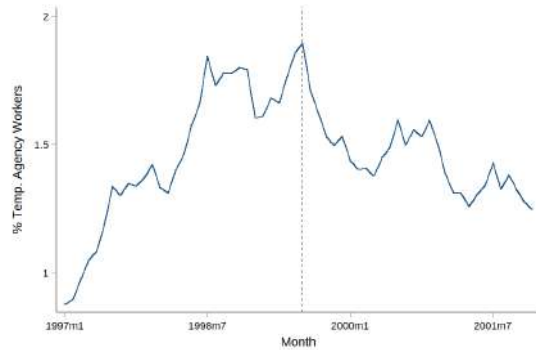
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A Figures

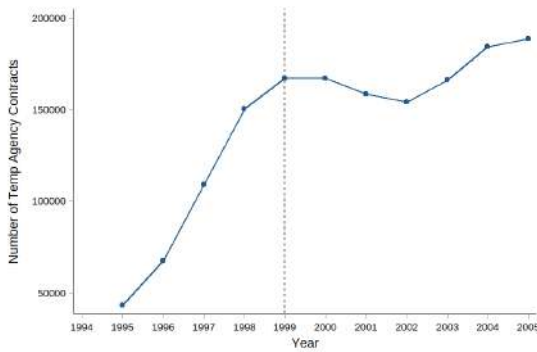
Figure 1:



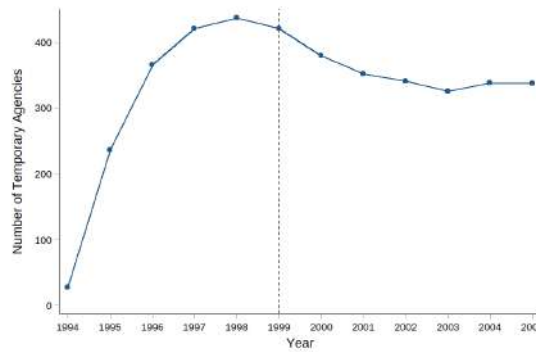
(a) Average Real Wage



(b) Temp Agency Workers



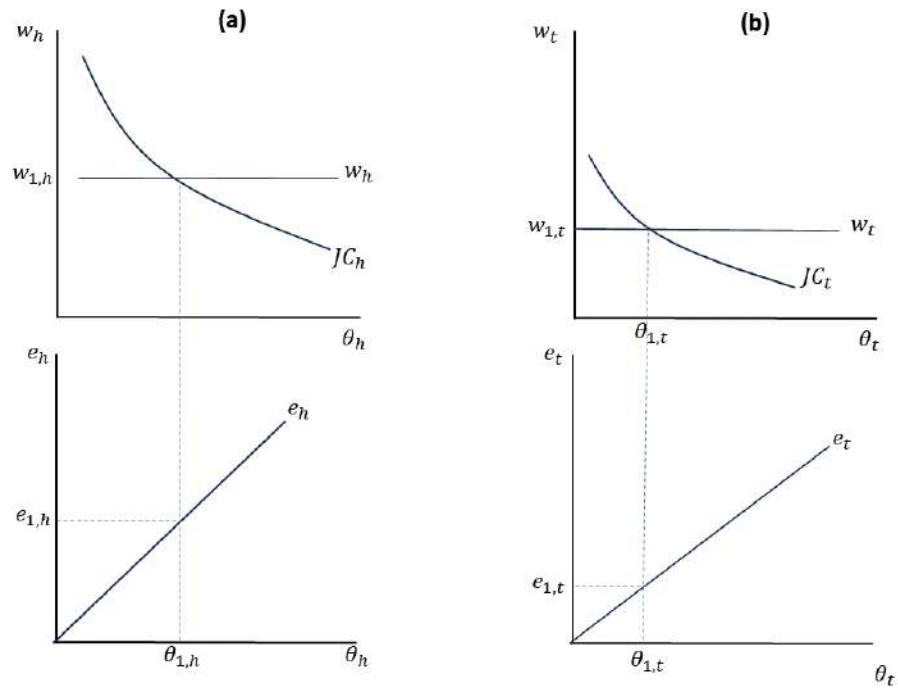
(c) Temp Agency Contracts



(d) Number of Temp Agencies

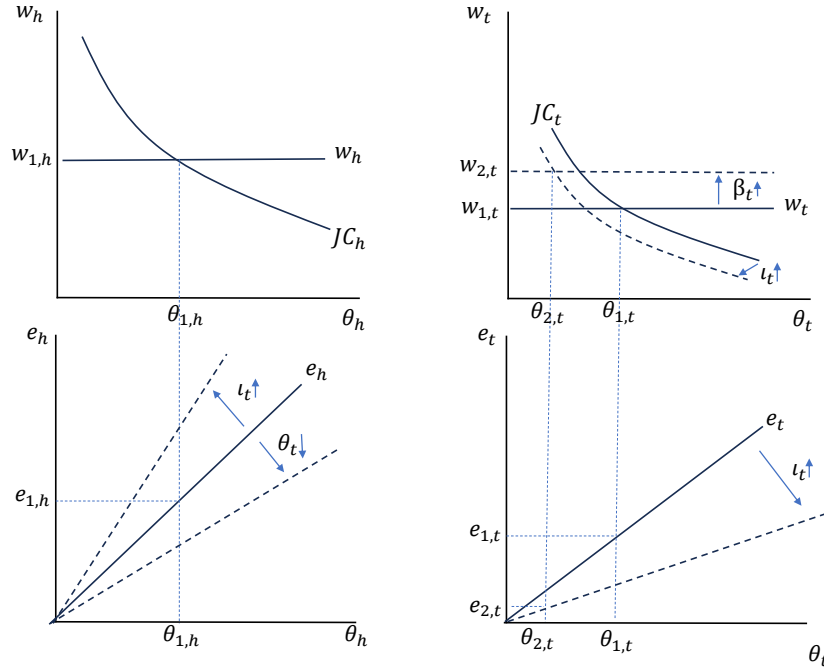
Notes: The panels show the evolution of real daily wages for temporary agency workers and employees in other contracts (a), of temporary agency employment (b), the number of temp agency contracts (c), and of the number of temp agencies (d). Sources: MCVL and the data series of *Empresas de trabajo temporal*.

Figure 2: Labor Market Equilibrium



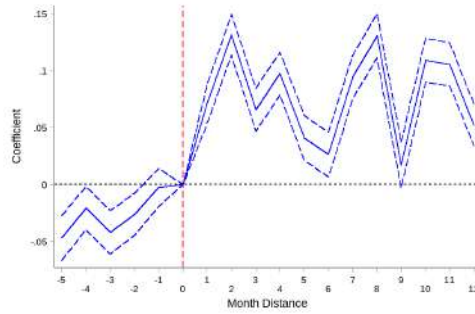
Notes: This figure represents the labor market equilibrium of both in-house production workers (panel a) and temporary agency workers (panel b).

Figure 3: **Effect of a Higher Bargaining Power β_a**

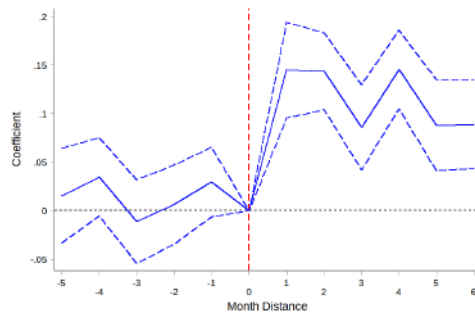


Notes: This figure shows the labor market effects of the 1999 Spanish reform that allows agency workers to be included in the collective pay agreements of the production firms they are deployed to. We model the reform as an exogenous increase in the bargaining power of temporary agency workers. Specifically, it increases from β_t to β_h , becoming equivalent to that of in-house employees.

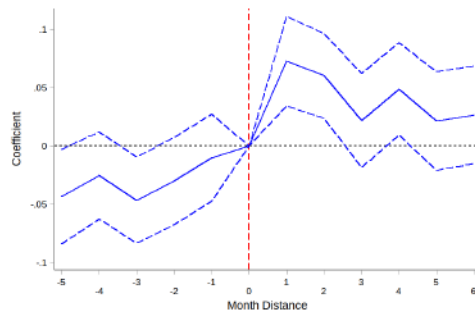
Figure 4: Wage Effects



(a) All temp agency workers



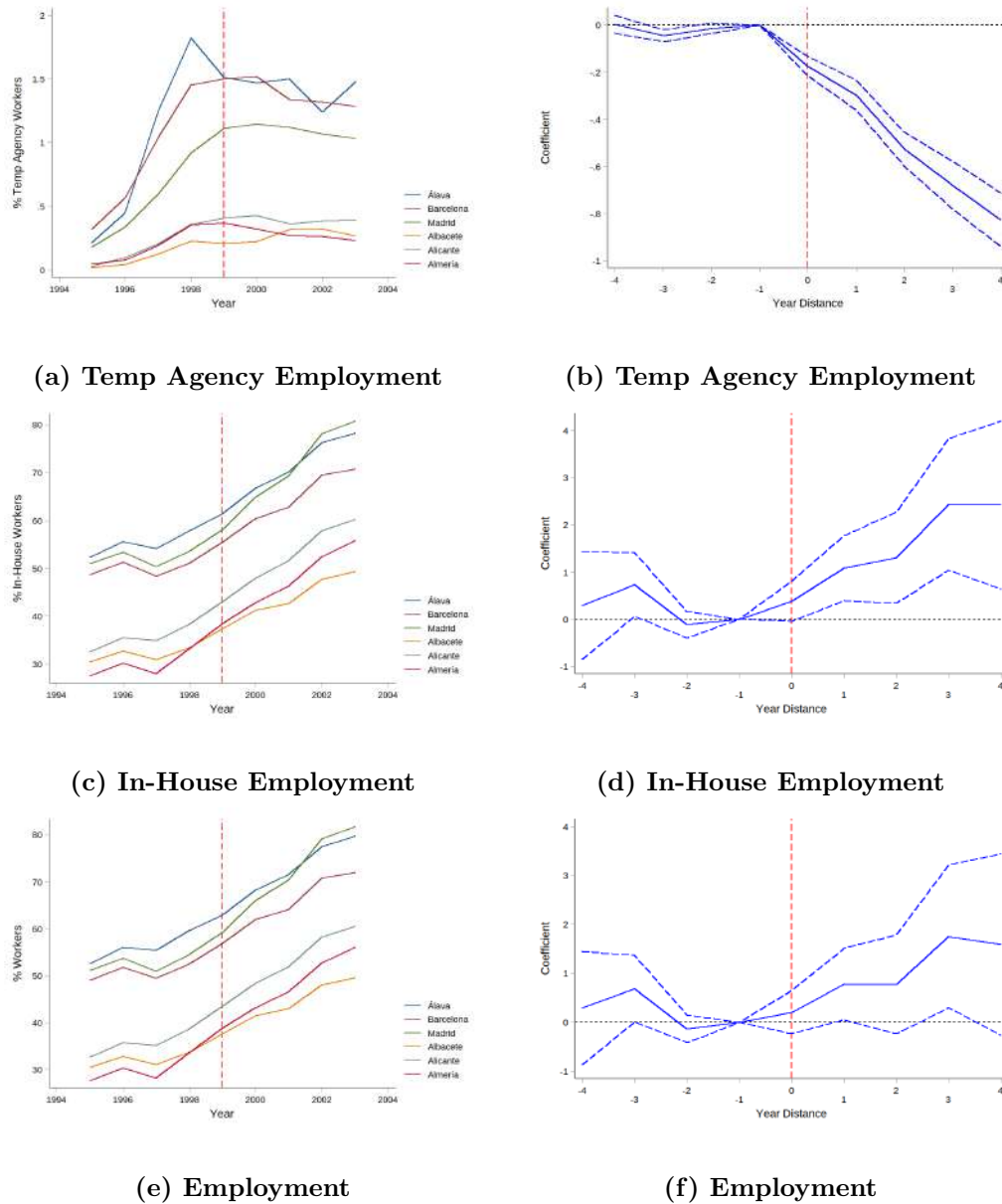
(b) Always temp agency worker



(c) Temp agency workers that become in-house

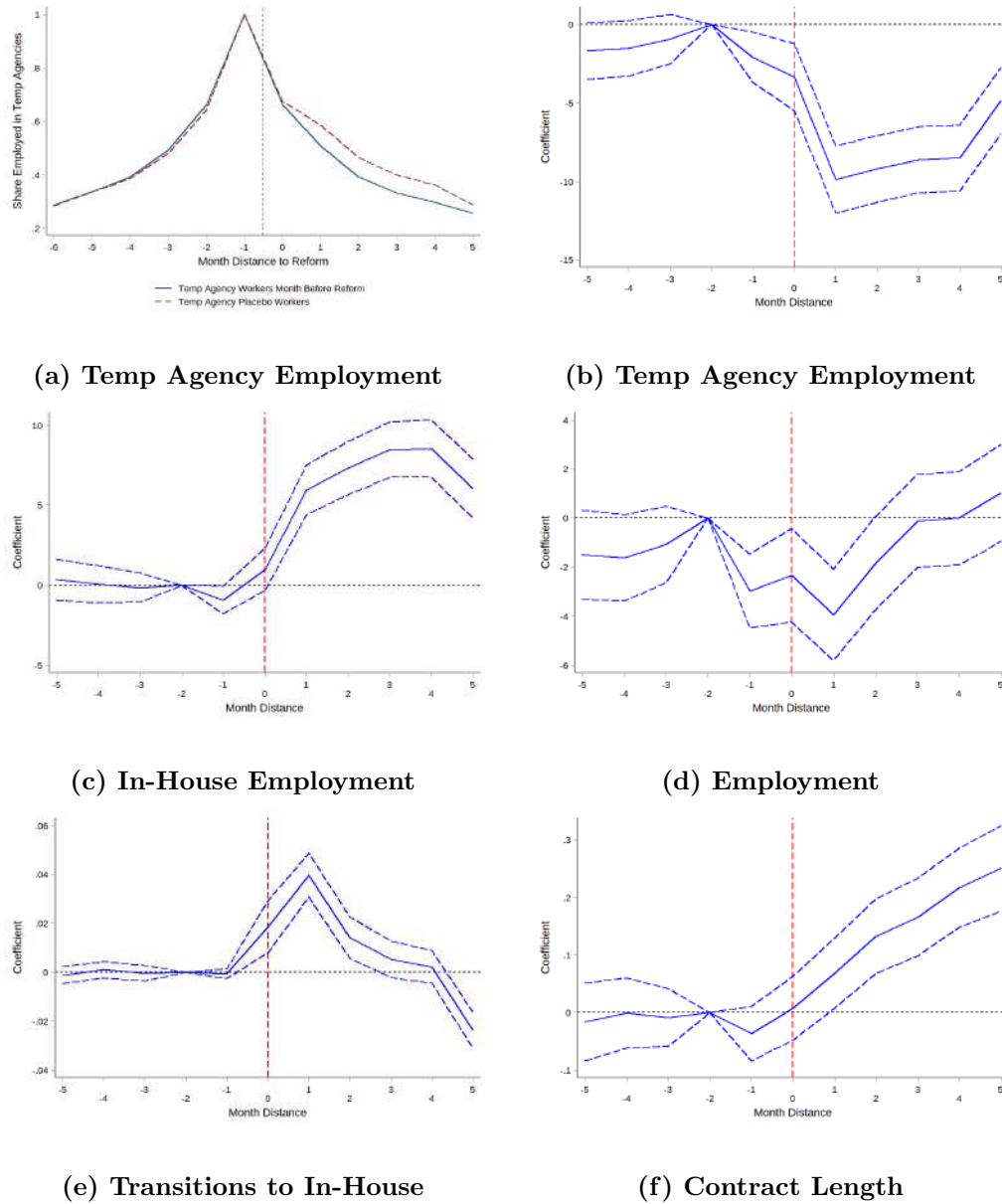
Notes: The figure shows the event-studies that analyze the wage effects of the reform. The specification is a difference-in-differences with individual and month fixed effects, as in Equation 23. The blue solid lines are the coefficient estimates and the blue-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. Panel (a) is for all workers. Panel (b) is for a sample of workers who remain continuously employed in either a temp agency contract (treatment) or a non-temp agency contract (control), for 6 months before and after the policy change. In Panel (c) the treatment sample are workers who transition from a temp agency job to an in-house job during the post-period. The control group are workers who are continuously employed in non-temp agency jobs during the time period analyzed. 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 vertical red-dashed line depicts the month of the reform.

Figure 5: **Employment Effects. Province-Level Analysis**



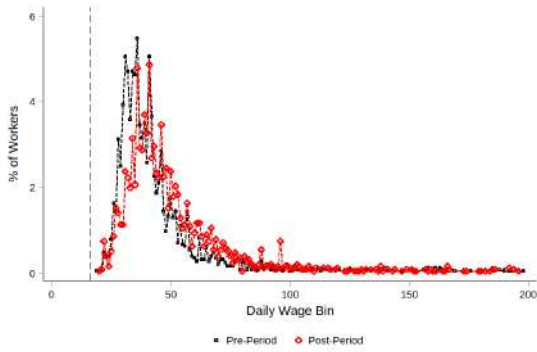
Notes: The figures in the left column show the percentages of temp agency (Panel (a)), in-house (Panel (c)) and overall employment (Panel (e)) relative to province active population at baseline. We display it for three provinces with high temp agency incidence (Álava, Barcelona and Madrid), and three provinces with low temp agency incidence (Albacete, Alicante, Almería). The figures in the right column depict the event-studies of the effects of the reform on temp agency (Panel (b)), in-house (Panel (d)) and overall employment (Panel (f)). The specification is a continuous difference-in-differences (Equation 24), where province-specific pre-change linear trends are removed. Regressions are weighted by population. Controls include coastal dummies, the province unemployment rate, the share of construction, the share of immigrants, the average firm size, and share of workers with no university degree at baseline. The blue solid lines are the coefficient estimates and the blue-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.

Figure 6: **Employment Effects. Sample of Temp Agency Workers a Month Before the Reform**

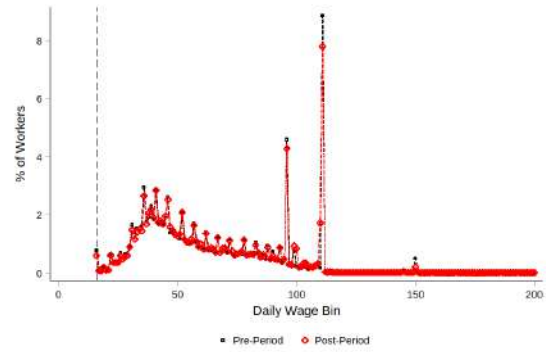


Notes: Panel (a) shows the share of temp agency employment for a sample of workers who got a temp agency contract the month before the reform (blue solid line), and for a placebo sample of workers who got a temp agency contract in July 1998 (13 months before the reform). Panels (b) to (f) show the results of a time-shifted dynamic difference-in-differences with individual, province, calendar-month, and month fixed effects, as explained in Section 4. The specification is equation 25. We also include the province unemployment rate as a control. The blue solid lines are the coefficient estimates and the blue-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. 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.

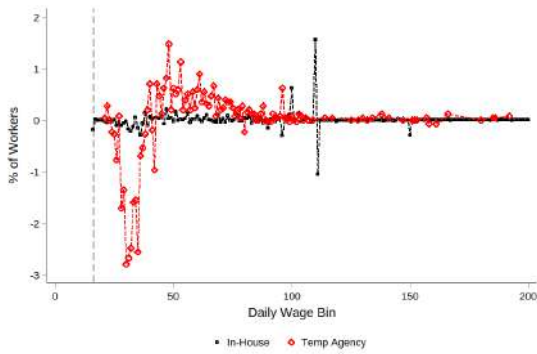
Figure 7: Changes in the Wage Distribution



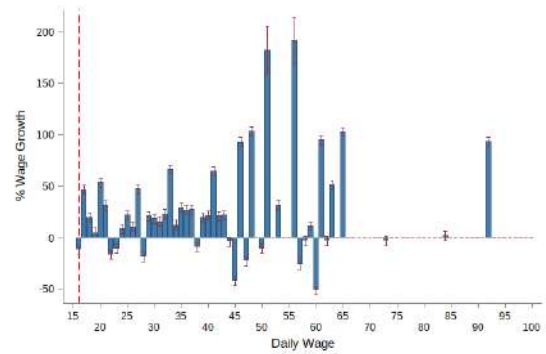
(a) Temp Agency Workers



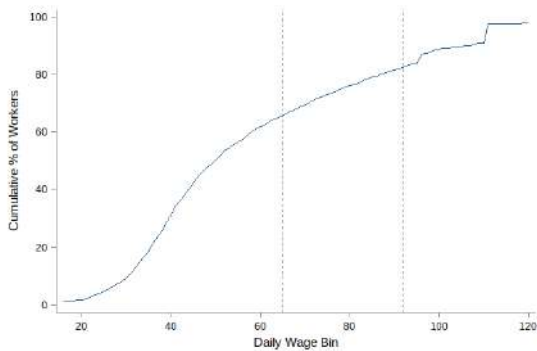
(b) In-House Workers



(c) Before/After Difference



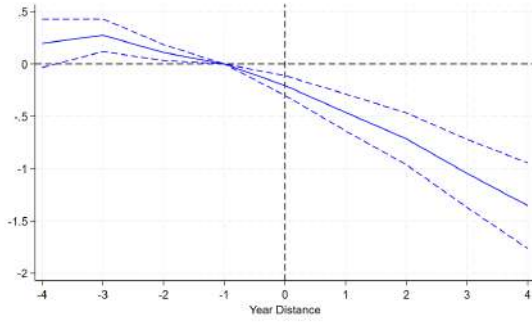
(d) Wage Growth Across the Distribution



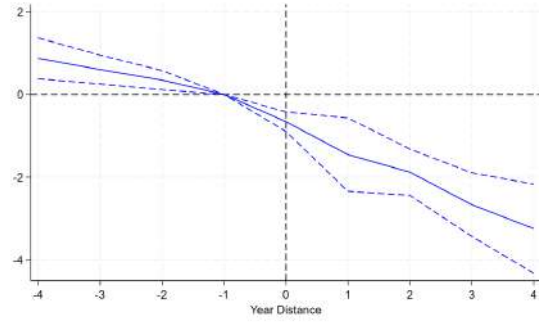
(e) Wage Cumulative Distribution

Notes: Panels (a) and (b) display wage distributions for temporary agency workers and in-house workers, respectively. Panel (c) shows the before/after first difference of the wage distributions for temporary agency workers and in-house workers. Panel (d) shows the estimates (blue bars) of wage growth caused by the reform across the wage distribution. Workers are placed into euro bins of real daily wages. The control group are in-house workers. The specification is a difference-in-differences with bin and month fixed effects, as in Equation 27. The red capped spikes are robust standard errors clustered at the wage-bin level. Panel (e) is the real daily wage cumulative distribution function. The black-dashed vertical lines show the two highest wage bins where we detect significant wage growth for each sample of workers.

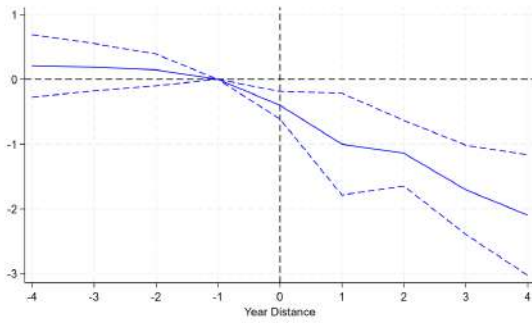
Figure 8: Effects on Temp Agencies



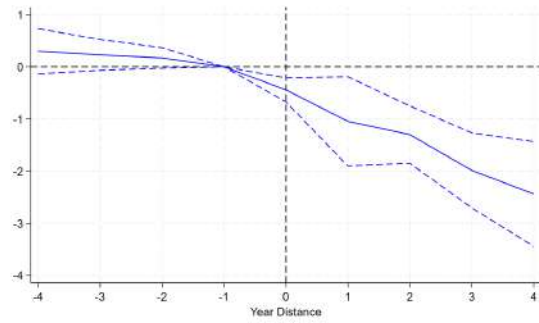
(a) Log Number of Temp Agencies



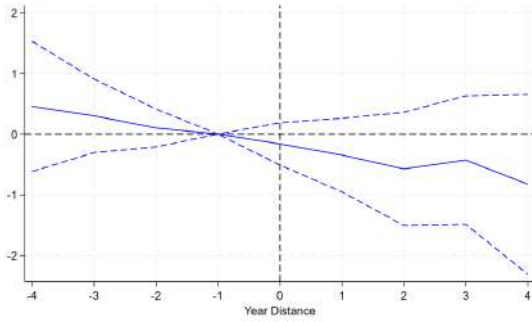
(b) Log Net Sales



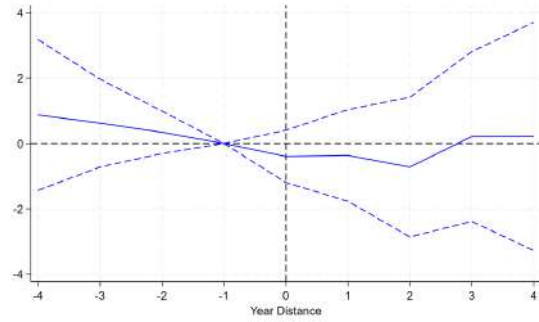
(c) Log Value Added



(d) Log Labor Costs



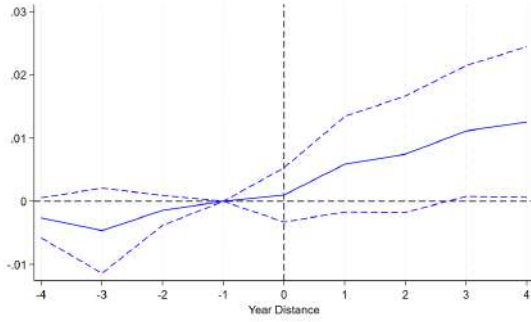
(e) Log Productivity per Worker



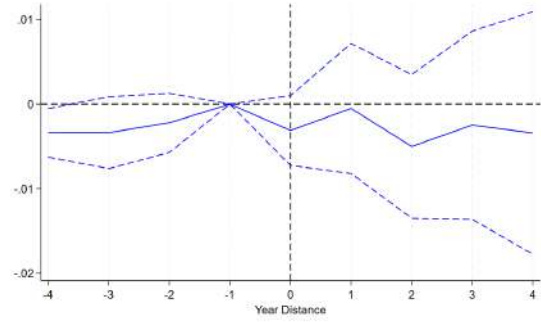
(f) Log Profit per Worker

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 28), 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. Controls include the average number of workers, the average company age, the share of traded companies, and the share of public companies. The vertical black-dashed line depicts the year of the reform. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

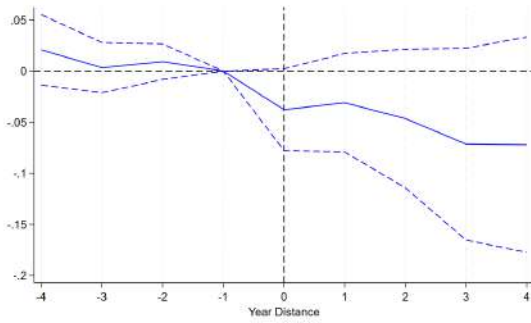
Figure 9: Effects on In-House Companies



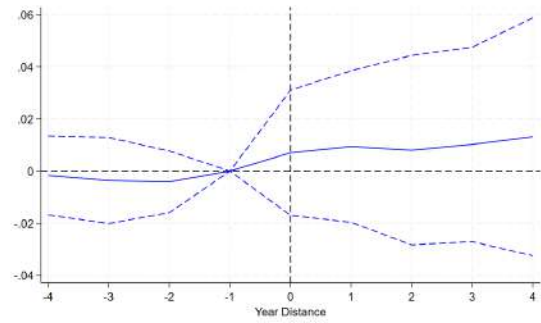
(a) Log Internal Workers



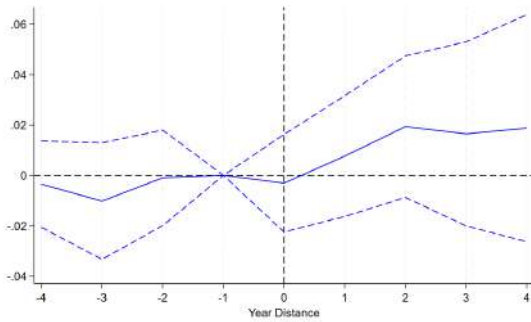
(b) Log Total Workers



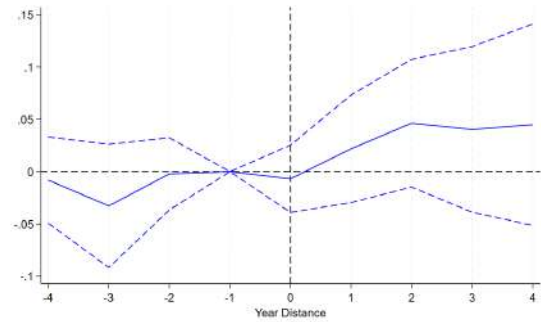
(c) Log Purchases to Other Companies



(d) Log Number of Companies



(e) Log Productivity per Worker



(f) Log Profit per Worker

Notes: The figure shows the effects of the reform on the sectors that use temp agency workers. Estimates are based on a continuous difference-in-differences (Equation 29), where sector-specific pre-change linear trends are removed. Controls include the average number of workers (except when the endogenous variable is in-house or total workers), the average company age, the share of traded companies, the share of public companies, and the average of the Herfindahl-Hirschmann Index. The vertical black-dashed line depicts the year of the reform. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

B Tables

Table 1: Summary Statistics

	Pre-Period		Post-Period		Pre-Period		Post-Period		Pre-Period		Post-Period	
	Temp. Agency Workers		Temp. Agency Workers		Short-Term In-House		Short-Term In-House		Permanent In-House		Permanent In-House	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
% Male	57.255	49.476	55.468	49.705	64.016	47.995	63.173	48.234	66.456	47.214	65.029	47.688
Age	27.475	7.526	27.480	7.419	31.577	9.741	31.931	9.822	39.747	10.451	39.431	10.711
Years of Experience	17.100	19.851	15.810	19.638	28.826	24.533	26.901	24.688	60.803	23.779	56.708	27.200
% Spanish Citizen	99.097	9.462	98.256	13.093	98.424	12.455	97.983	14.057	98.942	10.231	98.682	11.403
Real Daily Wage	49.340	43.346	55.886	42.804	46.414	26.891	46.567	26.244	67.340	32.262	65.520	31.460
Contract Length	105.916	172.372	121.274	192.932	588.103	898.300	586.338	913.689	4289.333	3312.467	4045.533	3209.492
% Part-Time	18.412	38.762	13.524	34.201	17.520	38.014	16.611	37.218	3.727	18.943	5.489	22.777
% Was Unemployed Month Before	15.991	36.656	16.170	36.821	6.101	23.936	5.843	23.456	0.671	8.165	0.740	8.569
% Managerial Layer	1.749	13.110	1.019	10.045	7.635	26.556	7.617	26.527	18.790	39.064	18.846	39.108
% Laborer Layer	98.251	13.110	98.981	10.045	92.365	26.556	92.383	26.527	81.210	39.064	81.154	39.108
Firm Size	333.574	747.633	413.560	828.116	122.725	878.162	140.593	947.396	311.495	1447.844	324.333	1440.551
Observations	5203		5102		110845		115438		191903		216009	

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 a month before the policy (January 1999), and a month after the policy (January 2000).

Table 2: Incidence of Temp Agency Contracts Relative to Employment

	(1)						(2)					
	Year 1998						Year 2001					
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N		
% Temp Agency Employment (Province)	1.625	0.655	0.000	3.112	52	1.323	0.505	0.117	2.234	52		
% Temp Agency Contracts (Sector)	2.513	5.633	0.023	36.110	44	1.859	4.103	0.010	27.057	44		

Notes: The table shows summary statistics of the incidence of temporary agency employment by province and sectors. For each of them, 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).

Table 3: Summary Statistics for Firms

	Pre-Period		Post-Period		Pre-Period		Post-Period	
	Temporary Agencies		Temporary Agencies		User Firms		User Firms	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Profits	188.817	1606.696	293.414	1885.734	268.787	16634.475	253.734	14421.490
Value Added	6590.296	39855.800	8764.627	58863.262	651.649	24255.299	617.396	21286.375
Value of Production	7224.726	43227.320	9468.472	63499.657	2512.641	64108.048	2501.595	69670.157
Net Sales	7163.663	42766.153	9412.802	63187.385	2431.685	62178.354	2416.674	67557.849
In-House Labor Costs	6401.479	38429.980	8471.213	57011.146	382.862	9490.386	363.661	8804.976
Purchases to Other Companies	63.367	208.099	44.994	155.019	1512.204	42408.006	1542.214	49664.506
Observations	221		205		314121		352545	

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).

Table 4: Effects on Wages

	(1)	(2)	(3)
	Log Daily Wage	Log Daily Wage	Log Daily Wage
Panel A: All Temp Agency Workers			
Treatment x Post	0.149*** (0.00376)	0.180*** (0.00383)	0.129*** (0.00357)
Observations	19311041	19311041	19311041
Panel B: Temp that Stay in the Job for 12 Months			
Treatment x Post	0.118*** (0.0126)	0.132*** (0.0125)	0.109*** (0.0124)
Observations	2693016	2693016	2693016
Panel C: Temp that Transition to In-House			
Treatment x Post	0.0666*** (0.0104)	0.0733*** (0.0113)	0.0532*** (0.0104)
Observations	2616468	2616468	2616468
Individual FE	N	N	Y
Month FE	N	N	Y
Location FE	N	Y	Y
Calendar-month 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, as in Equation 22. Panel (a) is for all workers. Panel (b) is for a sample of workers who remain continuously employed in either a temp agency contract (treatment) or a non-temp agency contract (control), for 6 months before and after the policy change. In Panel (c) the treatment sample are workers who transition from a temp agency job to an in-house job during the post-period. The control group are workers who are continuously employed in non-temp agency jobs during the time period analyzed. 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. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Effects on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Temp Agency	Temp Agency	In-House	In-House	Overall	Overall
	Employment	Employment	Employment	Employment	Employment	Employment
$\% TempAgencyWork_{1998} \times Post$	-0.496*** (0.0288)	-0.317*** (0.0696)	2.072*** (0.552)	0.832* (0.456)	1.576*** (0.570)	0.515 (0.472)
Controls	N	Y	N	Y	N	Y
Observations	450	450	450	450	450	450

Notes: The table shows the employment effects of the reform. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. Regressions are weighted by population. Columns 1, 3 and 5 do not include controls, whereas Columns 2, 4 and 6 do. Controls include coastal dummies, the province unemployment rate, the share of construction, the share of immigrants, the average firm size, and share of workers with no university degree at baseline. The specification is equation 24. Robust standard errors, clustered at the province level, are reported. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Effects on Incumbent Temp Agency Workers

	(1)	(2)	(3)	(4)	(5)
	Incumbents: Emp. in Temp Agencies	Incumbents: Emp Open-Ended and Short-Term	Incumbents: Employment	Incumbents: Transition to In-House	Incumbents: Log Contract Length
Treatment x Post	-6.104*** (0.724)	6.173*** (0.647)	0.0690 (0.627)	0.977*** (0.148)	0.144*** (0.0228)
Observations	155796	155796	155796	155796	109300
	Incumbents: Open-Ended Employment	Incumbents: Short-Term Employment	Incumbents: Unemployment Insurance	Incumbents: Self-Employment	Incumbents: Public Employment
Treatment x Post	2.600*** (0.295)	3.573*** (0.619)	-0.0965 (0.312)	0.0639 (0.0979)	-0.0209 (0.0486)
Observations	155796	155796	155796	155796	155796

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, calendar-month, and month fixed effects, as explained in Section 4. The specification is Footnote 20. We also include the province unemployment rate as a control. Except for the contract length 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%

Table 7: Elasticity Estimates

(1)	(2)	(3)
Always Temp Agency Workers	From Temp Agency to In-House Worker	All Temp Agency Workers
0.00342	0.00550	0.00469
(0.0531)	(0.0852)	()

Notes: The table reports the own-wage labor demand elasticities as explained in Section 4.3.2. Standard errors were obtained using the delta method and are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Effects on Temp Agencies

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Number Temp Agencies	Log Net Sales	Log Labor Costs	Log Value Added	Log Productivity per Worker	Log Profits per Worker
Panel A: Up to 2 Years after Reform						
Treatment x Post	-0.378*** (0.122)	-1.284*** (0.306)	-0.675*** (0.186)	-0.636*** (0.167)	-0.114 (0.0942)	-0.164 (0.587)
Observations	945	756	756	756	756	756
Panel B: Up to 4 Years after Reform						
Treatment x Post	-0.700** (0.274)	-1.890*** (0.570)	-0.648* (0.363)	-0.705** (0.346)	-0.235 (0.172)	-0.963 (1.435)
Observations	1215	972	972	972	972	972

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 28. Controls include the average number of workers, the average company age, the share of traded companies, and the share of public companies. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Effects on In-House Sectors

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Number	Log In-House	Log Total	Log Purchases	Log Profits	Log Productivity
	Temp Agencies	Workers	Workers	Other Companies	per Worker	per Worker
Panel A: Up to 2 Years after Reform						
<i>%TempAgency_{s,1998}</i> x Post	0.00624 (0.00538)	0.00521 (0.00442)	-0.00231 (0.00482)	-0.0283 (0.0221)	0.0277 (0.0258)	0.0104 (0.0111)
Observations	301	273	273	273	273	273
Panel B: Up to 4 Years after Reform						
<i>%TempAgency_{s,1998}</i> x Post	0.0110 (0.00903)	0.00742 (0.00504)	-0.00299 (0.00578)	-0.0479 (0.0332)	0.0411 (0.0394)	0.0162 (0.0162)
Observations	387	351	351	351	351	351

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 sector-specific pre-change linear trends are removed. Regressions are weighted by the endogenous variable at baseline. The specification is Equation 29. Controls include the average number of workers (except when the endogenous variable is in-house or total workers), the average company age, the share of traded companies, the share of public companies, and the average of the Herfindahl-Hirschmann Index. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

C Online Appendix (Not for Publication)

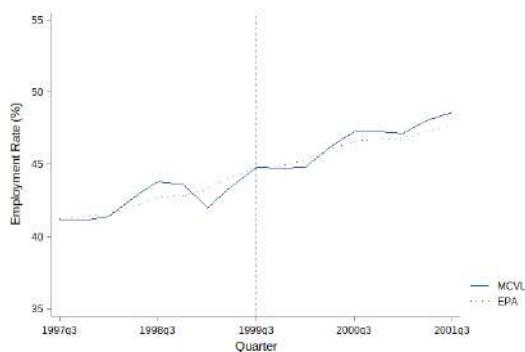
C.1 A Comparison of Survey and Administrative Data

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 before.

In this section, we compare the MCVL with data from the 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 A1. 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 A1: Comparing Survey and Administrative Data

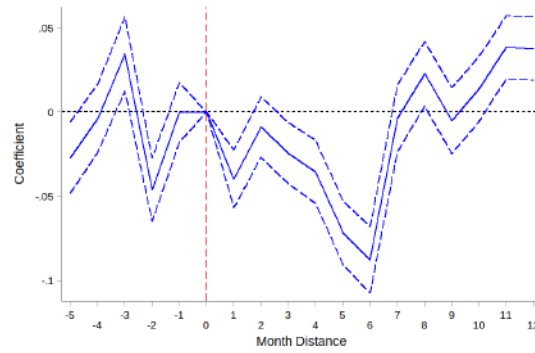


(a)

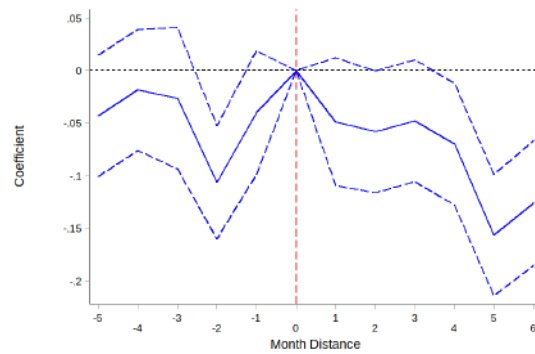
Notes: The figure shows the evolution of the employment rate based on the SLFS (dotted line) and the sample of workers from the MCVL (blue solid line). The vertical dashed line depicts the quarter of the reform.

C.2 Figures

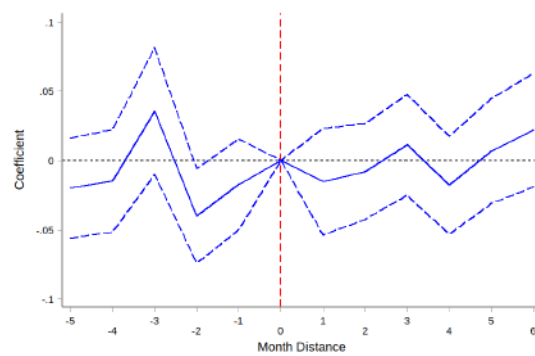
Figure A2: Placebo. Wage Effects



(a)



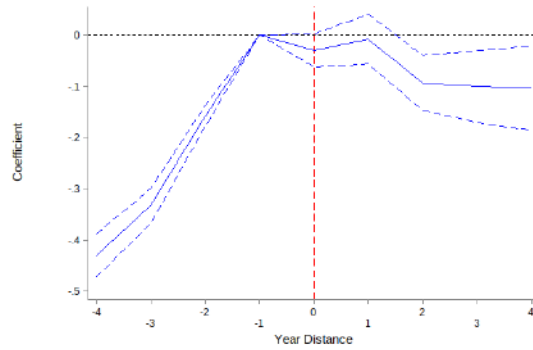
(b)



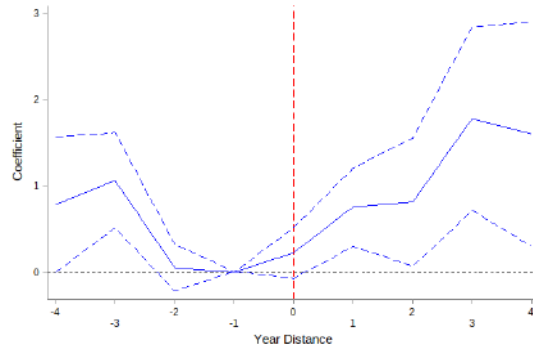
(c)

Notes: The figure shows the event-studies that analyze the wage effects of the placebo reform. The specification is equation 23. Panel (a) is for all workers, panel (b) is for a sample of workers who remain in their same job 6 months before and after the policy change, and panel (c) is for a sample of workers who transition to in-house workers during the post-period. The vertical red-dashed line depicts the month of the placebo reform.

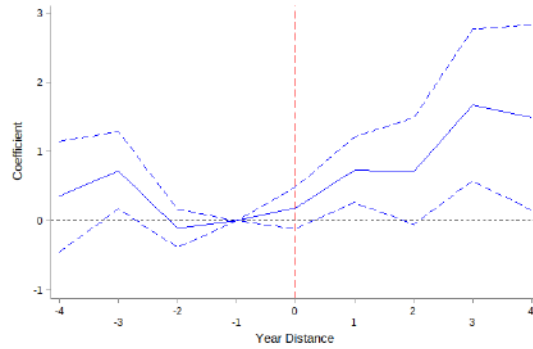
Figure A3: **Employment Effects. No detrend. Province-Level Analysis**



(a) Temp Agency Employment



(b) In-House Employment



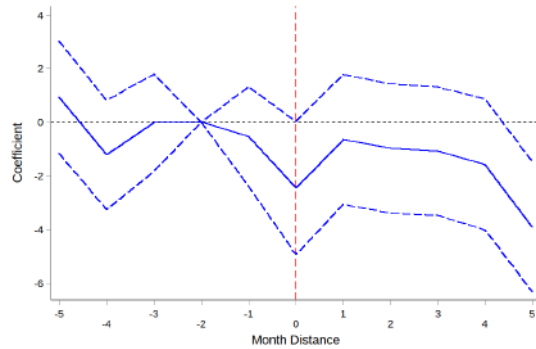
(c) Employment

Notes: The figures depict the event-studies of the effects of the reform on temp agency (Panel (a)), in-house (Panel (b)) and overall employment (Panel (c)). The specification is a continuous difference-in-differences, but we have not removed province-specific pre-change linear trends. Regressions are weighted by population. Controls include coastal dummies, the province unemployment rate, the share of construction, the share of immigrants, the average firm size, and share of workers with no university degree at baseline. The blue solid lines are the coefficient estimates and the blue-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

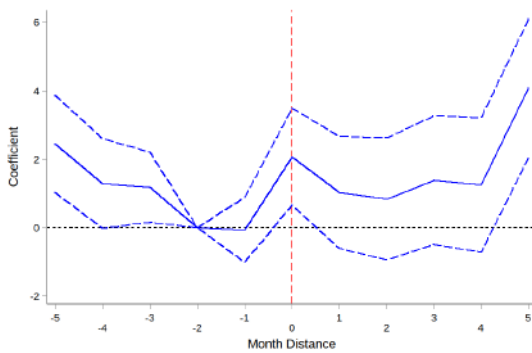
Figure A4: **Placebo. Employment Effects. Sample of Temp Agency Workers a Month Before the Reform**



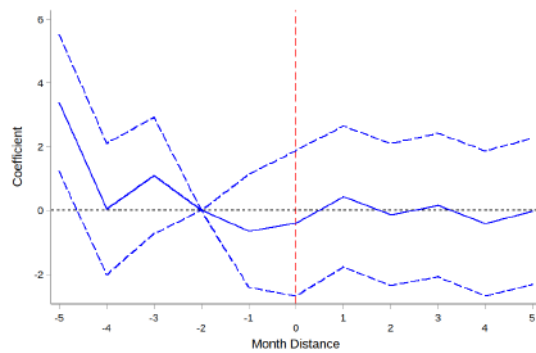
(a) Temp Agency Employment
(Raw Data)



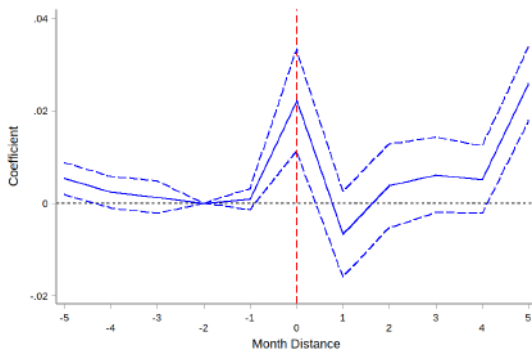
(b) Temp Agency Employment



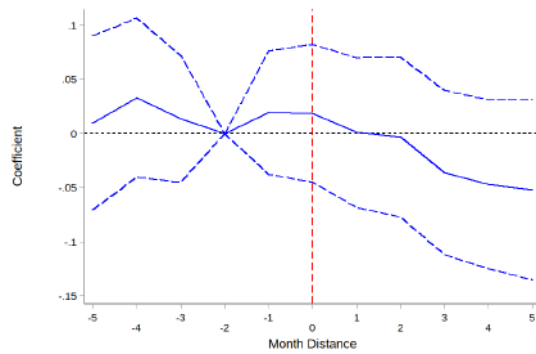
(c) In-House Employment



(d) Employment



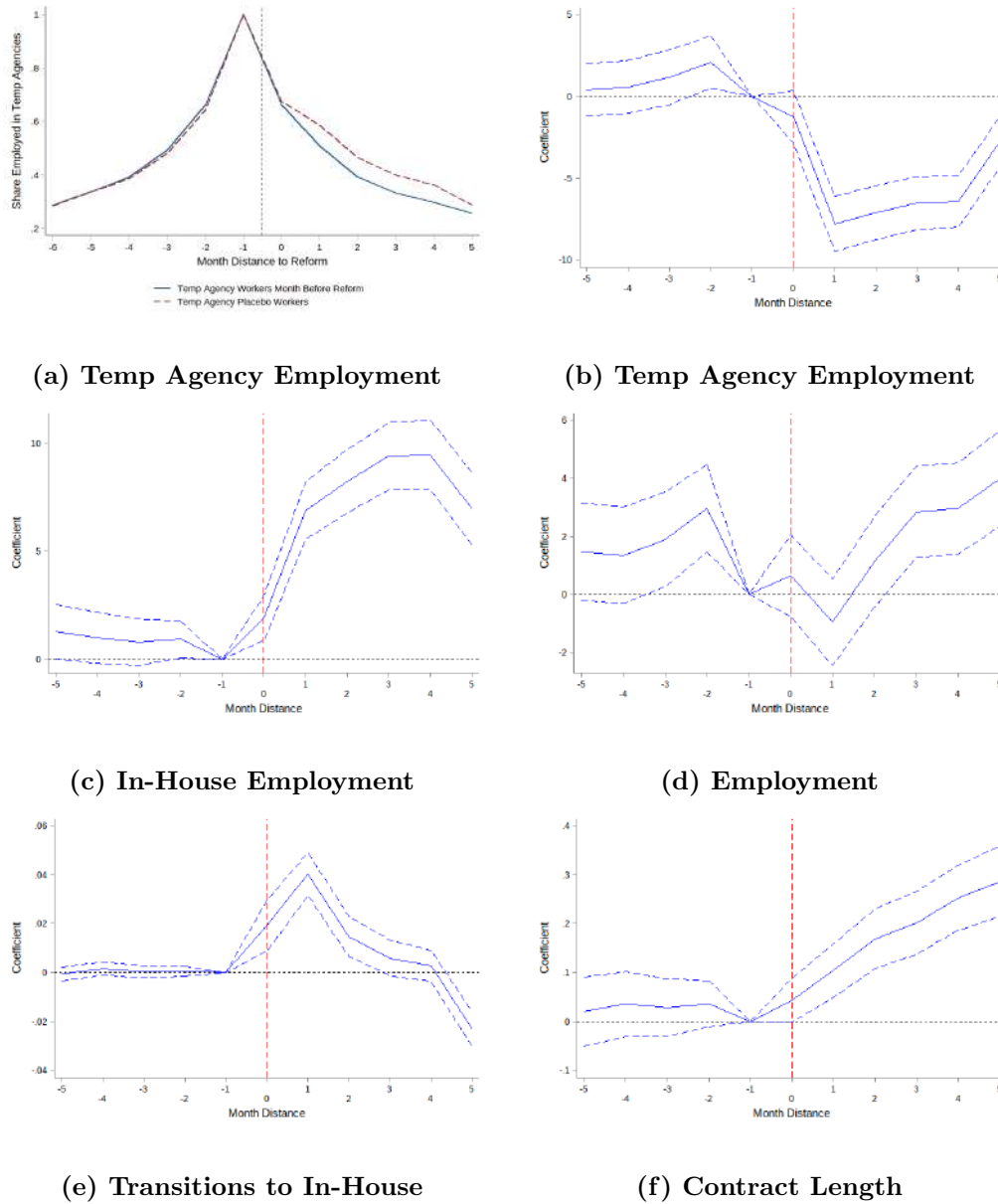
(e) Transitions to In-House



(f) Contract Length

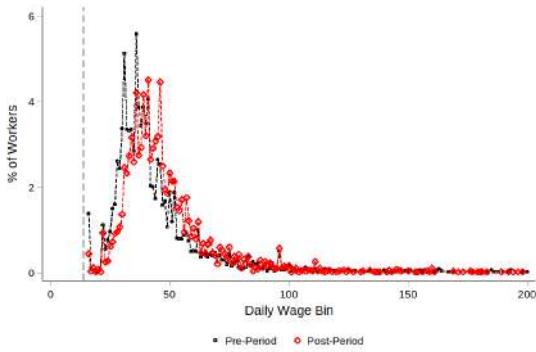
Notes: The figure shows the placebo event-studies that analyze the employment effects of the reform. The specification is a time-shifted dynamic difference-in-differences, as explained in Section 4. The estimates have been multiplied by 100 so that they can be interpreted as percentage points. Panel (a) displays temporary agency employment, panel (b) in-house open-ended employment, panel (c) in-house short-term employment, panel (d) combines the three previous employment measures, and panel (e) job duration in log days worked. The vertical red-dashed line depicts the month of the reform.

Figure A5: **Employment Effects. Sample of Temp Agency Workers a Month Before the Reform**

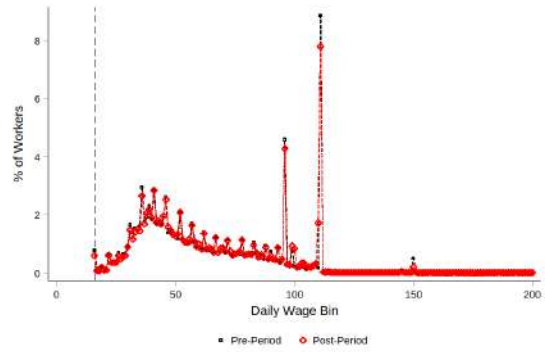


Notes: Panel (a) shows the share of temp agency employment for a sample of workers who got a temp agency contract the month before the reform (blue solid line), and for a placebo sample of workers who got a temp agency contract in July 1998 (13 months before the reform). Panels (b) to (f) show the results of a time-shifted dynamic difference-in-differences with individual, province, calendar-month, and month fixed effects, as explained in Section 4. The specification is equation 25, but omitting period -1 instead of -2. We also include the province unemployment rate as a control. The blue solid lines are the coefficient estimates and the blue-dashed lines 95% confidence intervals of robust standard errors clustered at the individual level. 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.

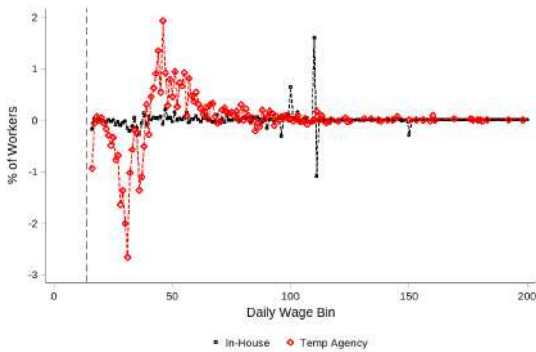
Figure A6: Changes in the Wage Distribution



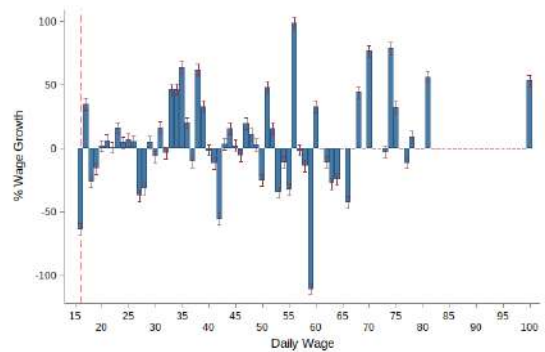
(a) Temp Agency Workers



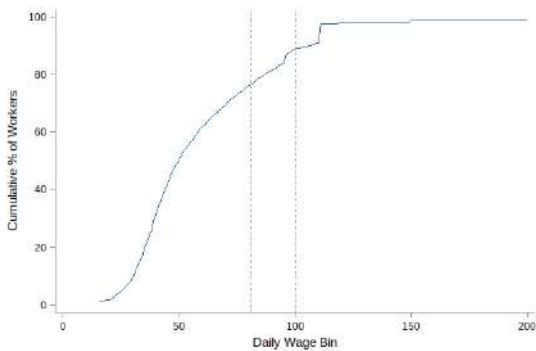
(b) In-House Workers



(c) Before/After Difference



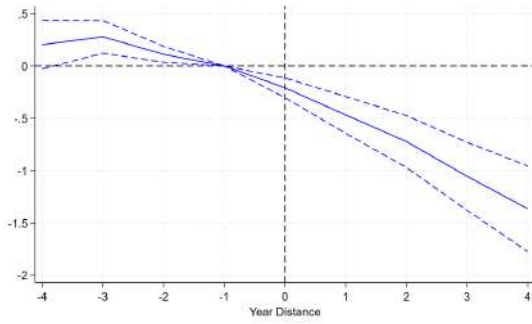
(d) Wage Growth Across the Distribution



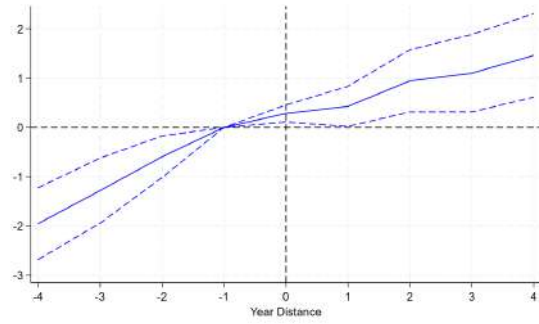
(e) Wage Cumulative Distribution

Notes: Panels (a) and (b) display wage distributions for temporary agency workers and in-house workers, respectively. Panel (c) shows the before/after first difference of the wage distributions for temporary agency workers and in-house workers. Panel (d) shows the wage growth caused by the reform across the wage distribution. Workers are placed into euro bins of real daily wages. The control group are in-house workers. Panel (e) is the real daily wage cumulative distribution function. The black-dashed vertical lines show the highest wage bins where we detect significant wage growth for each sample of workers.

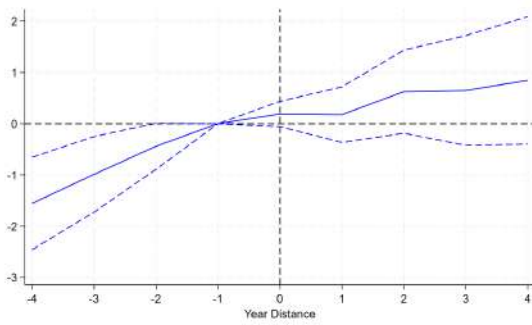
Figure A7: Effects on Temp Agencies, Data Not Detrended



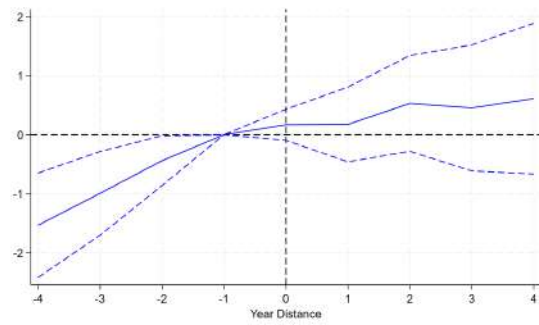
(a) Log Number of Temp Agencies



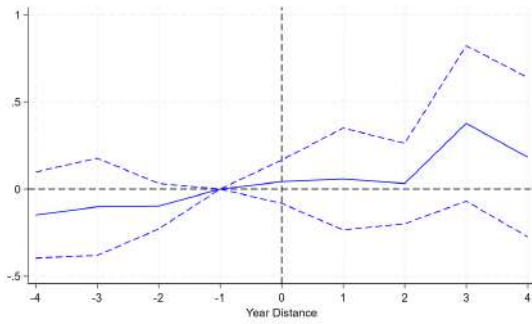
(b) Log Net Sales



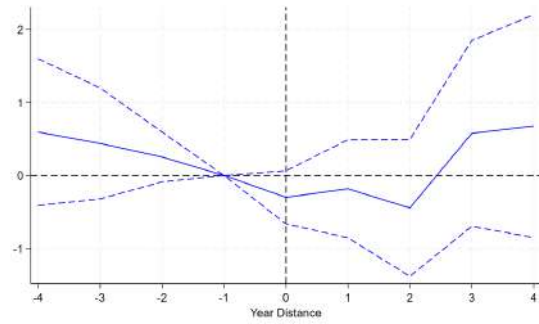
(c) Log Value Added



(d) Log Labor Costs



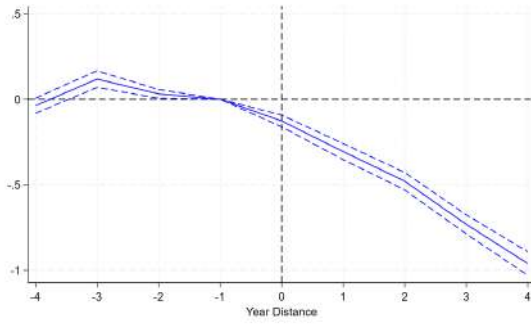
(e) Log Productivity per Worker



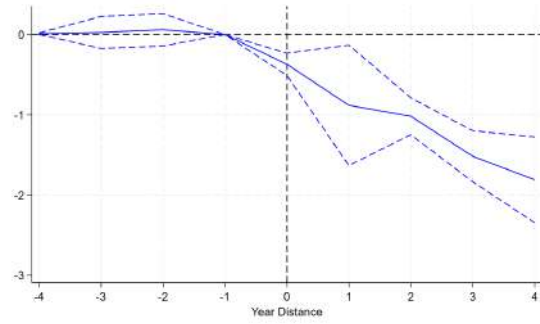
(f) Log Profit per Worker

Notes: The vertical black-dashed line depicts the year of the reform.

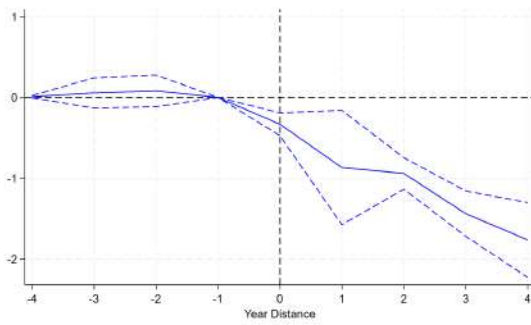
Figure A8: Effects on Temp Agencies, No Controls



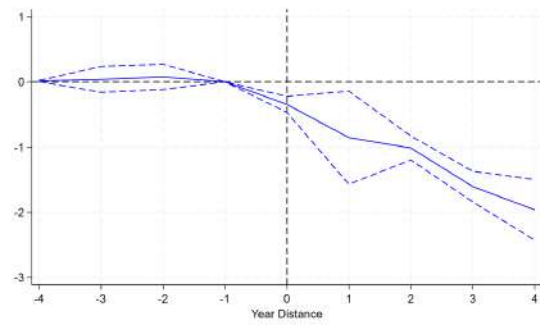
(a) Log Number of Temp Agencies



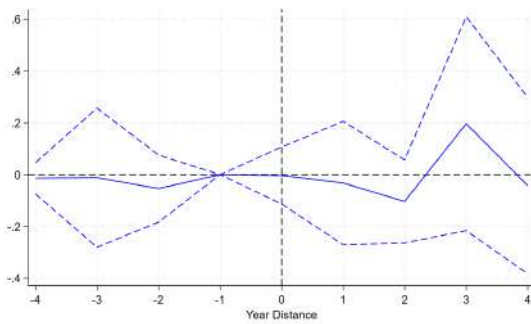
(b) Log Net Sales



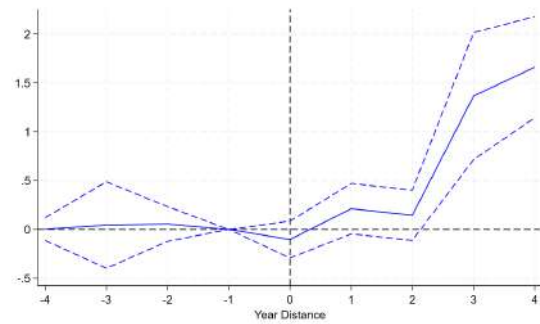
(c) Log Value Added



(d) Log Labor Costs



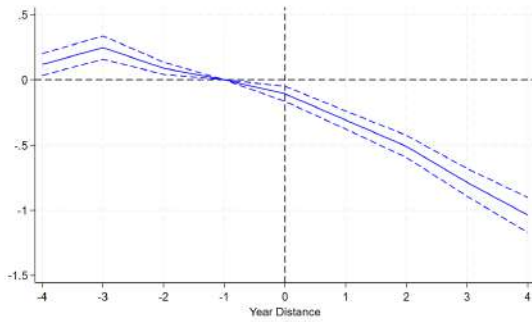
(e) Log Productivity per Worker



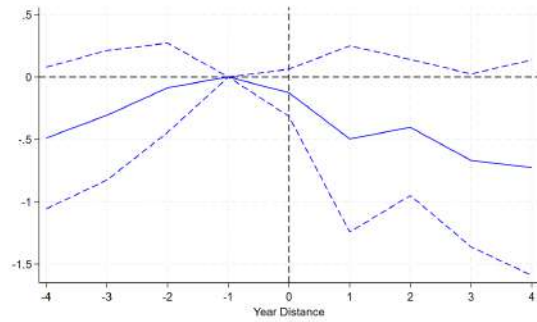
(f) Log Profit per Worker

Notes: The vertical black-dashed line depicts the year of the reform.

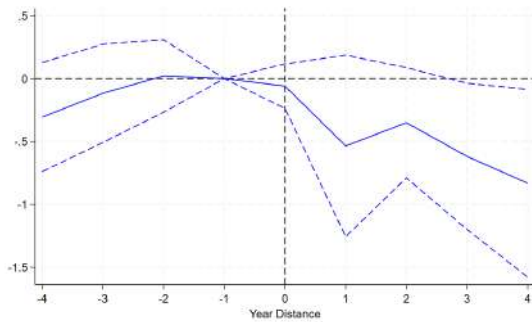
Figure A9: Effects on Temp Agencies, Sectors below 20th Percentile of Temp Agency Incidence



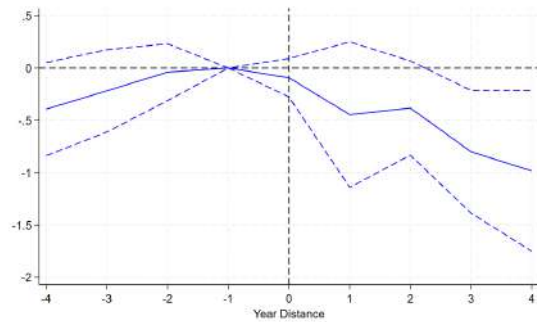
(a) Log Number of Temp Agencies



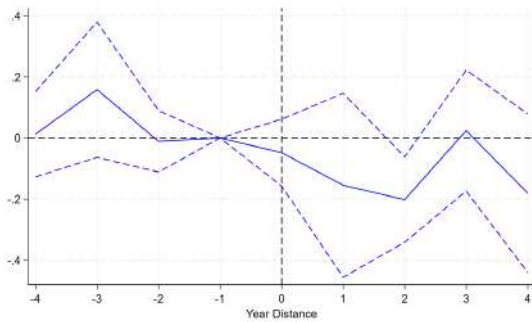
(b) Log Net Sales



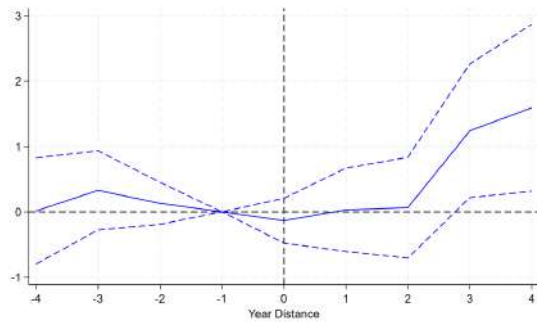
(c) Log Value Added



(d) Log Labor Costs



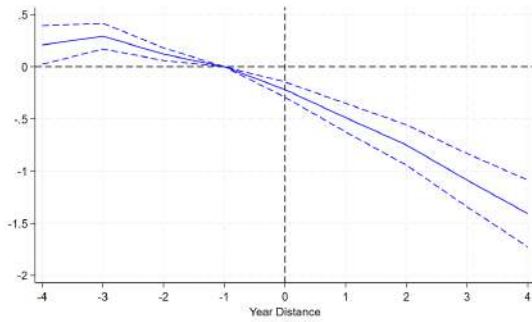
(e) Log Productivity per Worker



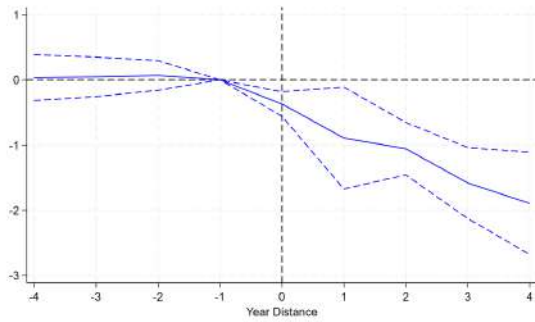
(f) Log Profit per Worker

Notes: The vertical black-dashed line depicts the year of the reform.

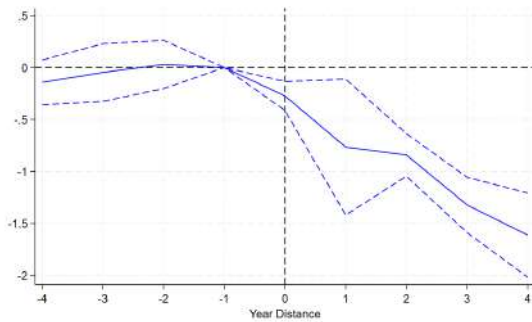
Figure A10: Effects on Temp Agencies, Sectors below 30th Percentile of Temp Agency Incidence



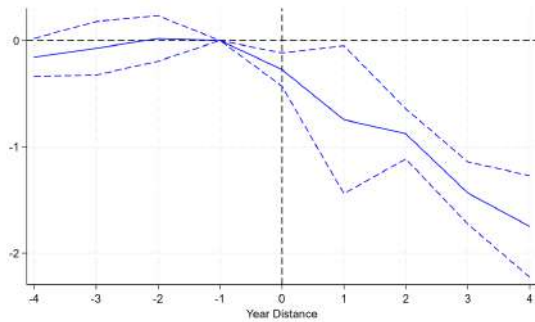
(a) Log Number of Temp Agencies



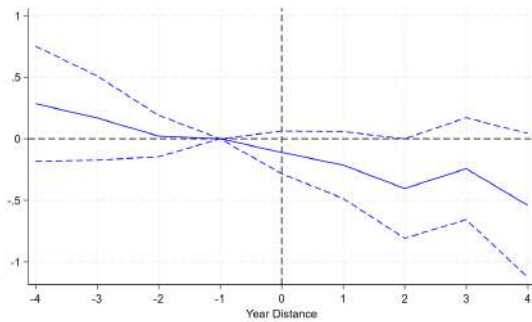
(b) Log Net Sales



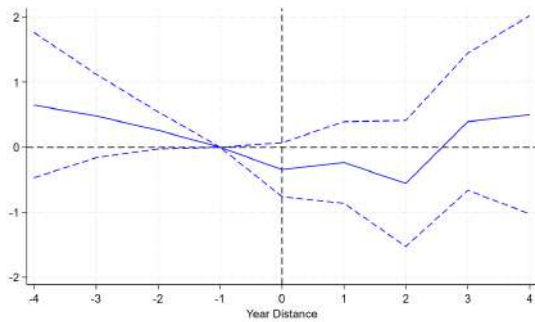
(c) Log Value Added



(d) Log Labor Costs



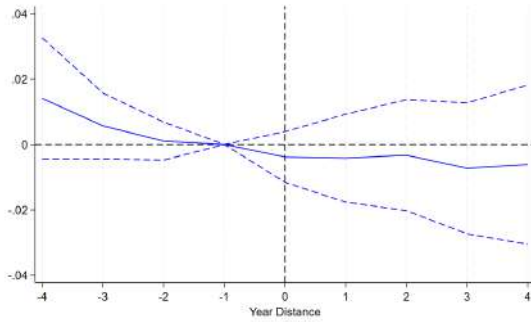
(e) Log Productivity per Worker



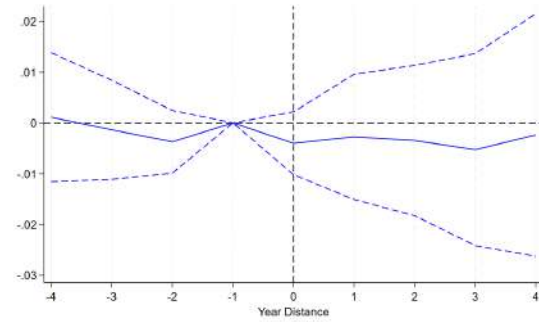
(f) Log Profit per Worker

Notes: The vertical black-dashed line depicts the year of the reform.

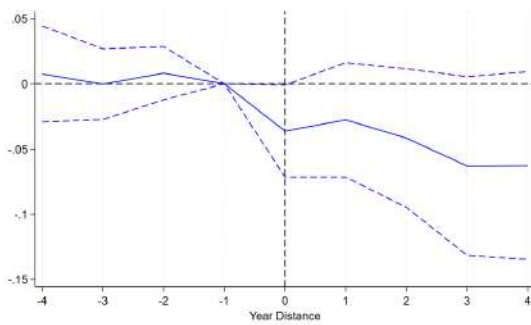
Figure A11: Effects on In-House Companies, Data Not Detrended



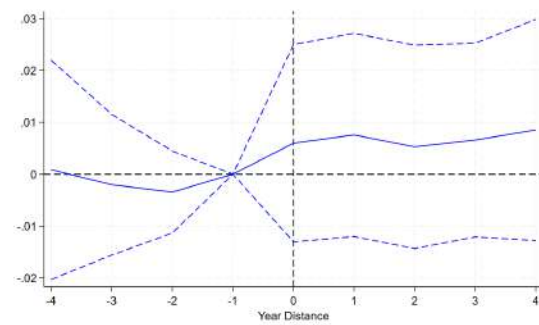
(a) Log Internal Workers



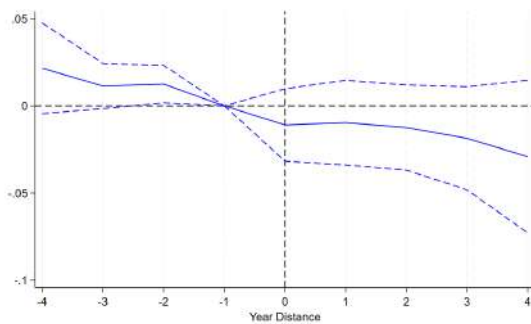
(b) Log Total Workers



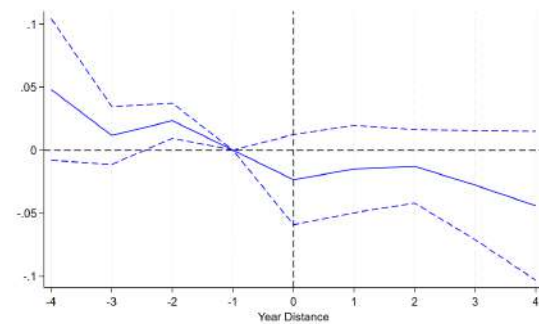
(c) Log Purchases to Other Companies



(d) Log Number of Companies



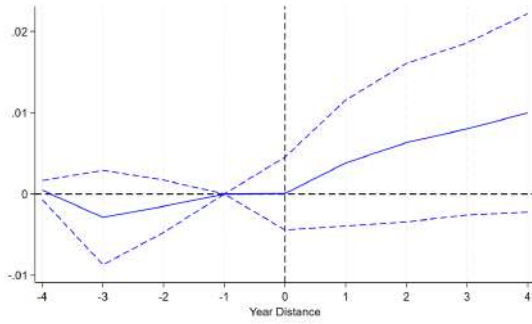
(e) Log Productivity per Worker



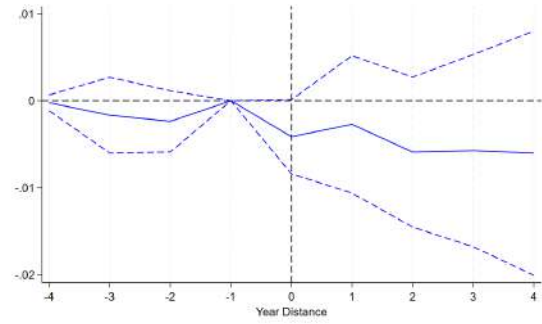
(f) Log Profit per Worker

Notes: The vertical black-dashed line depicts the year of the reform.

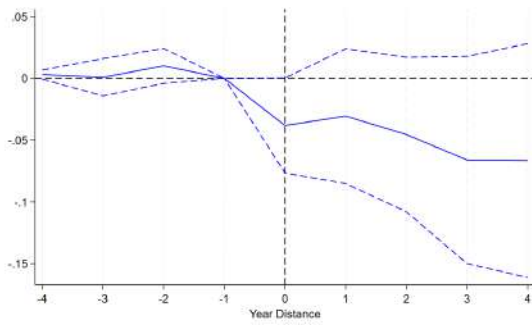
Figure A12: Effects on In-House Companies, No Controls



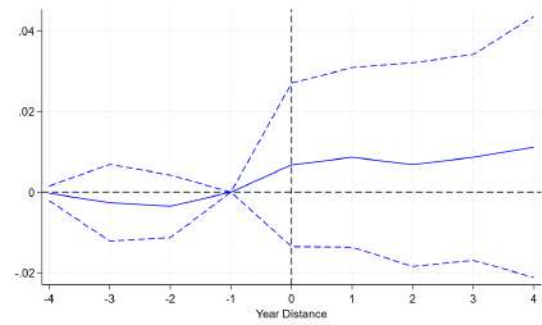
(a) Log Internal Workers



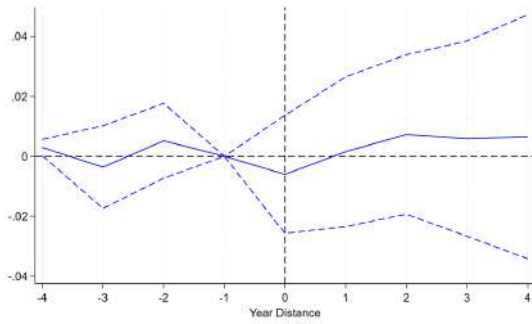
(b) Log Total Workers



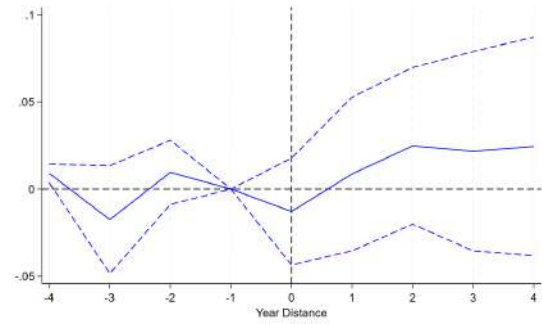
(c) Log Purchases to Other Companies



(d) Log Number of Companies



(e) Log Productivity per Worker



(f) Log Profit per Worker

Notes: The vertical black-dashed line depicts the year of the reform.

C.3 Tables

Table A1: Effects on Wages

	(1)	(2)	(3)
	Log Daily Wage	Log Daily Wage	Log Daily Wage
Panel A: All Temp Agency Workers			
Treatment x Post	0.00514 (0.00456)	0.0457*** (0.00464)	0.00521 (0.00445)
Observations	7142788	7142788	7142788
Panel B: Temp that Stay in the Job for 12 Months			
Treatment x Post	-0.0355*** (0.0134)	-0.0253* (0.0133)	-0.0469*** (0.0133)
Observations	2445187	2445187	2445187
Panel C: Temp that Transition to In-House			
Treatment x Post	0.0157 (0.0107)	0.0185* (0.0111)	0.00759 (0.0104)
Observations	2399647	2399647	2399647
Individual FE	N	N	Y
Month FE	N	N	Y
Location FE	N	Y	Y
Calendar-month FE	N	Y	Y
Other time-varying controls	N	Y	Y
Unemp. Rate	Y	Y	Y

Notes: The table shows the wage effects of a placebo reform in July 1998. The specification is equation 22. Panel (a) is for all workers. Panel (b) is for a sample of workers who remain continuously employed in either a temp agency contract (treatment) or a non-temp agency contract (control), for 6 months before and after the policy change. In Panel (c) the treatment sample are workers who transition from a temp agency job to an in-house job during the post-period. The control group are workers who are continuously employed in non-temp agency jobs during the time period analyzed. 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. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A2: Effects on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Temp Agency	Temp Agency	In-House	In-House	Overall	Overall
	Employment	Employment	Employment	Employment	Employment	Employment
$\% TempAgencyWork_{1998} \times Post$	0.00638*** (0.00213)	0.00546** (0.00210)	-0.0709** (0.0278)	-0.0283 (0.0275)	-0.0498* (0.0263)	-0.0153 (0.0274)
Controls	N	Y	N	Y	N	Y
Observations	200	200	200	200	200	200

Notes: The table shows the employment effects of the reform. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A3: Effects on Incumbent Temp Agency Workers

	(1)	(2)	(3)	(4)	(5)
	Incumbents: Emp. in Temp Agencies	Incumbents: Emp Open-Ended and Short-Term	Incumbents: Employment	Incumbents: Transition to In-House	Incumbents: Log Contract Length
Treatment x Post	-1.794** (0.881)	0.462 (0.740)	-1.332* (0.796)	0.742*** (0.165)	-0.0273 (0.0295)
Observations	110880	110880	110880	110880	73864

	Incumbents: Open-Ended Employment	Incumbents: Short-Term Employment	Incumbents: Unemployment Insurance	Incumbents: Self-Employment	Incumbents: Public Employment
Treatment x Post	0.445 (0.310)	0.0170 (0.704)	0.407 (0.416)	-0.0369 (0.132)	0.0335 (0.0566)
Observations	110880	110880	110880	110880	110880

Notes: The table shows the placebo employment effects of the reform. The specification is a time-shifted difference-in-differences, as explained in Section 4. Except for the contract length variable, the estimates have been multiplied by 100 so that they can be interpreted as percentage points. Standard errors clustered at the worker level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A4: Robustness: Effects on Temp Agencies

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Number Temp Agencies	Log Net Sales	Log Labor Costs	Log Value Added	Log Productivity per Worker	Log Profits per Worker
Panel A: Placebo						
Treatment x Post	-0.238*** (0.0713)	0.196 (0.265)	-0.142 (0.189)	-0.0786 (0.203)	0.727* (0.400)	-0.328 (2.699)
Observations	540	432	432	432	432	432
Panel B: No Controls and Up to 2 Years after Reform						
Treatment x Post	-0.332*** (0.0249)	-0.781*** (0.131)	-0.771*** (0.120)	-0.750*** (0.120)	-0.110** (0.0508)	0.0895 (0.212)
Observations	945	756	756	756	756	756
Panel C: No Controls and Up to 4 Years after Reform						
Treatment x Post	-0.715*** (0.0352)	-1.144*** (0.145)	-1.189*** (0.125)	-1.105*** (0.127)	-0.156** (0.0667)	0.406** (0.167)
Observations	1215	972	972	972	972	972
Panel D: Below 30th Percentile of Temp Agency Incidence and Up to 2 Years after Reform						
Treatment x Post	-0.402*** (0.0995)	-0.639*** (0.192)	-0.472*** (0.104)	-0.429*** (0.0885)	-0.133 (0.0889)	0.0155 (0.301)
Observations	1092	896	896	896	896	896
Panel E: Below 30th Percentile of Temp Agency Incidence and Up to 4 Years after Reform						
Treatment x Post	-0.766*** (0.239)	-1.074*** (0.243)	-0.824*** (0.149)	-0.815*** (0.135)	-0.245* (0.130)	0.0911 (0.406)
Observations	1404	1143	1143	1143	1143	1143
Panel F: Below 20th Percentile of Temp Agency Incidence and Up to 2 Years after Reform						
Treatment x Post	-0.297*** (0.0933)	-0.453*** (0.113)	-0.330*** (0.123)	-0.315** (0.128)	-0.110 (0.0743)	0.0813 (0.477)
Observations	539	427	427	427	427	427
Panel G: Below 20th Percentile of Temp Agency Incidence and Up to 4 Years after Reform						
Treatment x Post	-0.508** (0.211)	-0.403** (0.165)	-0.147 (0.128)	-0.232* (0.120)	-0.230* (0.125)	-1.090 (1.328)
Observations	693	549	549	549	549	549

Notes: The table shows robustness checks of 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 28. In Panels A, and D to G, controls include the average number of workers, the average company age, the share of traded companies, and the share of public companies. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A5: Robustness: Effects on In-House Sectors

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Number	Log In-House	Log Total	Log Purchases	Log Profits	Log Productivity
	Temp Agencies	Workers	Workers	Other Companies	per Worker	per Worker
Panel A: Placebo						
%TempAgency _{s,1998} x Post	0.000706 (0.00806)	0.00701 (0.00749)	0.00243 (0.00581)	0.0162 (0.0170)	0.0377 (0.0457)	-0.00157 (0.0164)
Observations	172	156	156	156	156	156
Panel B: No Controls and Up to 2 Years after Reform						
%TempAgency _{s,1998} x Post	0.00624 (0.00538)	0.00438 (0.00445)	-0.00318 (0.00429)	-0.0416* (0.0246)	0.00658 (0.0175)	-0.000193 (0.0101)
Observations	301	273	273	273	273	273
Panel C: No Controls and Up to 4 Years after Reform						
%TempAgency _{s,1998} x Post	0.0110 (0.00903)	0.00662 (0.00522)	-0.00383 (0.00535)	-0.0529 (0.0314)	0.0131 (0.0224)	0.00191 (0.0128)
Observations	387	351	351	351	351	351

Notes: The table shows the effects of a placebo reform for in-house companies (Panel A), and for the actual reform without controls (Panels B and C). The data is aggregated at the sector level. The specification is a continuous 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 29. In Panel A, controls include the average number of workers (except when the endogenous variable is in-house or total workers), the average company age, the share of traded companies, the share of public companies, and the average of the Herfindahl-Hirschmann Index. Robust standard errors clustered at the sector level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

D Proofs

The propositions state that an increase in the policy parameter β_t :

1. *Proposition 1.1: Increases the wages of temp agency workers.*

Proof: Taking the derivative of the temp agency wage equation (18) with respect to β_t we obtain $\frac{dw_t}{d\beta_t} = A_t + c - b > 0$ since labor productivity A_t is always higher than unemployment benefits b , and vacancy costs $c > 0$.

2. *Proposition 1.2: The wages of in-house workers are not affected.*

Proof: Since β_t has no direct effect in neither the in-house job creation nor in the

wage equation, then the labor market tightness θ_h and wage w_h remain unchanged. Thus, according to equations (13) $\left. \frac{dw_h}{d\beta_t} \right|_{JCC(\theta_h)} = 0$, and (17) $\left. \frac{dw_h}{d\beta_t} \right|_{\omega} = 0$, implying that $\frac{dw_h}{d\beta_t} = 0$

3. *Proposition 1.3: Increases transformations from temp agency to in-house employment.*

Proof: This comes from the assumption that the job transformation rate increases with the ratio of wages $\iota'(\frac{w_h}{w_t}) > 0$. Thus, $\frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} > 0$

4. *Proposition 1.4: Reduces employment in temporary agencies.*

Proof: Since a higher β_t increases w_t and $\iota(\frac{w_h}{w_t})$, then the job filling rate $q_t(\theta_t, \eta_t)$ needs also to increase to restore the equilibrium in (14). This requires a lower θ_t since $q'_{\theta_t}(\theta_t, \eta_t) < 0$. Thus, $\frac{d\theta_t}{d\beta_t} < 0$. Then, applying total differentiation to equation (19):

$$\frac{de_t}{d\beta_t} = \frac{s_h \left\{ D \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} - p_t \left[(s_h + p_h + p_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} + (s_h + \iota) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} \right] \right\}}{D^2},$$

$$\frac{de_t}{d\beta_t} = \frac{s_h \left[(s_t + \iota)(s_h + p_h) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} - p_t (s_h + p_h + p_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} \right]}{D^2} < 0,$$

where D is the denominator in equation (19), $\frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} > 0$ and $\frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} < 0$. Notice e_t goes down because both the job creation channel $(s_t + \iota)(s_h + p_h) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} < 0$ and the job transformation channel $-p_t (s_h + p_h + p_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} < 0$.

5. *Proposition 1.5: There is an ambiguous effect in-house employment and unemployment.*

Proof: Defining the denominator of equation (21) equals D and doing total differentiation with respect to β_t we obtain:

$$\frac{du_t}{d\beta_t} = \frac{s_h \left\{ D \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} - (s_t + \iota) \left[(s_h + p_h + p_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} + (s_h + \iota) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} \right] \right\}}{D^2},$$

$$\frac{du}{d\beta_t} = \frac{s_h \left[(s_h - s_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} - (s_t + \iota)(s_h + \iota) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} \right]}{D^2} \leq 0.$$

In this case, the job creation channel increases unemployment since $-(s_t + \iota)(s_h + \iota) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} > 0$. However, since $s_t > s_h$, the job transformation channel $(s_h - s_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} < 0$ reduces u_t . Now, Given that $e_h = 1 - u - e_t$, then $\frac{de_h}{d\beta_t} = -\left(\frac{de_t}{d\beta_t} + \frac{du_t}{d\beta_t}\right)$ implying that:

$$\begin{aligned} \frac{de_h}{d\beta_t} &= -\frac{s_h \left[(s_t + \iota)(s_h + p_h) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} - p_t(s_h + p_h + p_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} \right]}{D^2} - \\ &\quad \frac{s_h \left[(s_h - s_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} - (s_t + \iota)(s_h + \iota) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} \right]}{D^2}, \\ \frac{de_h}{d\beta_t} &= s_h \left[(s_t + \iota)(\iota - p_h) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} + p_t(p_t + p_h + s_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} \right] \leq 0. \end{aligned}$$

The job transformation channel increases e_h because $p_t(p_t + p_h + s_t) \frac{d\iota}{dw_t} \frac{dw_t}{d\beta_t} > 0$. However, the job creation channel will only increases e_h if $p_h > \iota$, implying that $(s_t + \iota)(\iota - p_h) \frac{dp_t}{d\theta_t} \frac{d\theta_t}{d\beta_t} > 0$.

6. *Proposition 1.6: Temp agency workers who transition to in-house have longer job durations and are more productive.*

Proof: This happens because once e_t is transformed to a e_h , they become more productive $A_h > A_t$ and have lower job separation rate $s_h < s_t$.