# FIRMS' ADJUSTMENTS TO EMPLOYMENT PROTECTION LEGISLATION: EVIDENCE FROM SOUTH KOREA

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The authors examine the impact of employment protection legislation on firm-level outcomes such as employment and profitability in South Korea. The 2007 Act on the Protection of Temporary Workers restricted the use of specific types of temporary contracts to a period of two years. Exploiting the fact that the impact of the reforms was greater for establishments that intensively used the affected temporary workers, the authors apply a difference-in-differences framework. Their results show that businesses responded to the Act by reducing the use of temporary contracts protected by the reforms and partially substituting them with permanent and other unprotected temporary contracts. As a result, the reform decreased overall employment level of establishments. Furthermore, the authors find that the newly introduced regulations had a limited negative impact on firms' profitability. Evidence suggests that establishments also improved their capital intensity and their labor productivity in response to the labor reform.

The incidence of temporary employment has increased during the past few decades in the United States and the majority of other Organisation for Economic Co-operation and Development (OECD) countries (Autor 2003; Curci, Rani, and Sekerler Richiardi 2012; OECD 2014). Given that temporary workers generally have less job security and poorer working conditions than do regular workers, the increase in temporary employment is considered problematic as it may lead to an increase in labor market segregation among workers. Stronger employment protection for temporary workers is often recommended to discourage the increase in the

numbers of temporary employment contracts (OECD 2014). By contrast, strict employment protection is frequently blamed for a high unemployment rate, especially in European countries (Bertola 1990; Cahuc and Postel-Vinay 2002).

Our goal in this article is to better understand the impact of employment protection for temporary workers, which is among the most controversial labor regulations. We use business/firm-level data to examine its influence on various types of employment. Previous literature on employment protection has focused primarily on its impact on employment (Autor, Kerr, and Kugler 2007). Because the theory does not provide any clear-cut predictions, it is not surprising, perhaps, that the empirical results pertaining to the repercussions of employment protection on the employment level are mixed (Boeri and Garibaldi 2007; Kahn 2010).[1] Thus, our work contributes to the literature by providing additional evidence on whether the businesses respond to the labor regulation by adjusting the size and composition of their workforces. We also attempt to examine other aspects of the impact of employment protection on business-level outcomes such as labor productivity and profitability. These areas are relatively unexplored; the few notable exceptions include Autor et al. (2007) and Bird and Knopf (2009). This relative scarcity of evidence is rather unexpected given that employment protection for workers acts as a restriction on the use of labor from the perspective of business. To the best of our knowledge, this article is the first to examine the influence of regulations pertaining to temporary workers on profitability outside of the banking sector.

We exploit South Korea's labor reforms, which came into effect in 2007, to identify the impact of employment protection at the business level. Two laws regarding the protection of temporary workers were passed in late 2006 and put into effect on July 1, 2007. The major component of these reforms is to increase the employment protection levels of specific types of temporary workers with more than two years of tenure. Because the establishment-level impact of the reforms depends on the pre-policy level of the employment of affected temporary workers at each establishment, we are able to apply a difference-in-differences framework to compare the various business-level outcomes across establishments with different degrees of treatment intensity. Our article is not the first to examine the results of this reform. Most notably, Yoo and Kang (2012) exploited the individual-level data to examine the effect of the reform on employment. Our work is distinguished from Yoo and Kang (2012) by our use of establishment-level data to examine aforementioned firm/business-level outcomes. In particular, our results are driven by a representative sample of firms; thus, the results can be applied to the general population. By contrast, the results of Yoo

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[1]This theoretical ambiguity likely stems from stronger employment protection decreasing both the dismissal and the hiring of workers (Lazear 1990; Hunt 2000; Kugler and Saint-Paul 2004; Kan and Lin 2011; Adhvaryu, Chari, and Sharma 2013).

and Kang (2012) pertained only to people around age 55 because of the research design and the sample construction, as they admit.

Our empirical analysis provides evidence that employment protection for temporary workers has a small but statistically significant negative impact on the total employment level. We find that an establishment with a high proportion of affected temporary workers reduced their employment by a larger amount compared to those that used such temporary workers less intensively. Moreover, our data show that employment protection for specific types of temporary workers has unintended consequences. That is, establishments respond to stronger employment protection for temporary workers with specific types of contracts by increasing their use of other types of temporary workers who are not protected by the labor reforms. In this case, the use of atypical temporary contract workers by establishments increased after the introduction of the protective measures for typical temporary contract workers. Furthermore, we show that this balloon effect—the increase in the incidence of other types of atypical temporary employment contracts—is more prominent among establishments with a labor union. Overall, our results provide evidence that businesses respond to the employment protection legislation by replacing workers who become legally burdensome by the reforms.

In addition to employment-related outcomes, we examine the impact of the labor reforms on the labor cost and profitability of firms. The results generally support that stricter regulations regarding the use of temporary workers result in an increase in the labor cost per worker for businesses that heavily rely on those workers. Moreover, we document that stronger employment protection for temporary workers did not aggravate the profitability of businesses despite its potential repercussion on the per worker labor cost. We find suggestive evidence that firms responded actively by improving management and capital intensity, and thereby labor productivity, to mitigate the impact of labor regulation on their profitability.

## Background

In this section, we provide background information regarding the definition of temporary workers in Korea and the labor market reforms enacted in 2007 that were intended to protect specific types of temporary workers.[2] Temporary employment, as opposed to permanent employment, is usually defined as employment under a contract that is expected to end after a specific period. The detailed definitions of temporary employment, however, vary around the world. Formally, temporary (non-regular) workers in Korea often indicate workers who cannot expect stable employment with their

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[2]The details of the reform can be also found in Grubb, Lee, and Tergeist (2007) and Yoo and Kang (2012).

Figure 1. Composition of Workers in 2005.



(A) Among total workers

(B) Among temporary workers
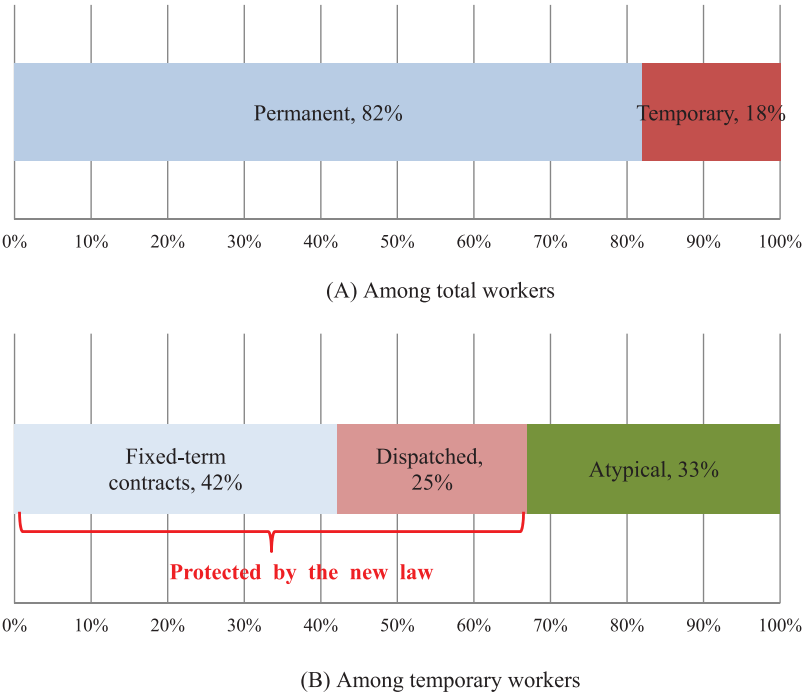
*Source:* Authors' calculation from the Workplace Panel Survey (WPS).

current employer and who have less employment protection than do permanent (regular) workers.[3]

To elaborate on the different types of the temporary workers in Korea, fixed-term contract workers refer to those workers whose labor contracts last for only a specified period. Dispatched workers or temporary agency workers are employed by a temporary work agency but they work under the supervision of the user company. The maximum duration of these two types of employment contract, fixed-term contracts and dispatched contracts, is restricted by the labor reform whereas other types of temporary contracts are not regulated. These other types of temporary workers include contract workers, at-home workers, and on-call workers.[4]

Figure 1 shows the proportion of workers by types of contract in the first wave of our data set, which we describe in the next section. In the 2005 wave, the average firm hired approximately 18% of their total workforce as temporary workers, as shown in Figure 1(A). Figure 1(B) depicts that

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[3]In this article, we use the two terms—temporary workers and non-regular workers—interchangeably.

[4]Contract workers work under the control of their own employer (contractor) who conducts the task requested by the client. On-call employment describes workers who are "called" to work on an as-needed basis, for instance, during peak hours or on occasion when an employer's demand for temporary workers increases.

among those temporary workers, approximately 42% and 25% of them are fixed-term contract and dispatched workers, respectively. Thus, approximately two-thirds of the temporary workers were affected by the labor reform in 2007.

The number of temporary workers in Korea increased rapidly after the Asian Economic Crisis in the late 1990s. And because temporary workers are more likely to experience poor working conditions, the Korean Tripartite Commission (consisting of government representatives, employers, unions, and public interest members for social dialogue) in 2001 began to discuss legislation for protecting these workers. Actual laws intended to protect temporary workers could not be passed until late 2006, however, as the drafted bills submitted by the government in 2004 faced considerable opposition from both unions and businesses. After considerable debate, the Act on the Protection of Temporary Workers was passed by the Korean legislature on November 30, 2006, and came into force on July 1, 2007.

Prior to 2007, there were few restrictions on the use of fixed-term contracts, which were a major form of temporary employment in Korea. The reforms, however, restricted the maximum duration of fixed-term contracts to two years.[5] The law stipulates that a fixed-term contract must either be terminated before two years has passed or be converted into an open-ended contract after the two-year contract period. Moreover, the law obligates a user company to hire a dispatched worker directly as its employee if the user company continues to use the dispatched worker for more than two years. Thus, under the new law, fixed-term contract workers and dispatched workers become potential permanent workers as their employment protection level is expected to increase sharply after two years of service. The other component of the reforms prohibits "discriminatory treatment" against temporary workers. This provision was gradually introduced between 2007 and 2009 according to the size of the establishment. The Prohibition on Discriminatory Treatment was first enforced at establishments with 300 or more workers on July 1, 2007, then to establishments with 100 or more workers on July 1, 2008, and finally to all establishments with five or more workers on July 1, 2009. It is not as binding as the restriction on the length of the contracts, however, as it allows employers to treat temporary workers differently based on justifiable reasons such as ability, types of tasks, and performance.

This empirical setting is contrary to the reduction of employment protection for temporary workers that occurred in the late 1990s in countries such as Germany and the Netherlands where regulations regarding the frequency of fixed-term contracts and the duration of the use of dispatched contracts were relaxed (Kahn 2010). Similar to the effects of easing

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[5]The restriction on the duration of fixed-term contracts applies to all fixed-term contracts with a few exceptions. Most notably, the restriction does not apply to workers older than age 55 at the time of the contract.

employment protection with regard to overall employment levels, the effect of strengthening employment protection for temporary workers is not theoretically clear. The number of temporary workers is likely to decrease and the number of permanent workers to increase given the increased potential cost of hiring temporary workers and the fact that the cost of hiring permanent workers was unaffected by the labor reform. In turn, the effect of employment protection on the total employment level depends on the relative size of the increase in permanent employment and the decrease in temporary employment, both of which are ambiguous. Moreover, the magnitude of the decrease in the numbers of temporary workers depends on whether establishments can readily replace fixed-term contract workers and dispatched workers with other types of temporary workers who are not protected by the reforms.

Thus, the effect of the labor reform laws on overall employment levels and the composition of the workforce warrants an empirical analysis.

## Data

To examine the impact of the 2007 employment protection legislation, we use the Workplace Panel Survey (WPS) data from 2005 to 2011. The data are collected biannually by the Korea Labor Institute. In our main specification, we use four waves of data—2005, 2007, 2009, and 2011. The 2005 wave is pre-policy period, whereas the 2007, 2009, and 2011 waves are post-policy periods. The establishments in the WPS were randomly chosen from a nationally representative sample in the 2005 Census on Establishments collected by Statistics Korea. The WPS covers establishments with more than 30 employees in all sectors apart from agriculture, forestry, fishing, and mining and quarrying. Specifically, our data set covers 65 industries that are classified by the Korean Standard Industrial Classification (KSIC), revision 9, at the 2-digit level.

The data set consists of a panel of approximately 1,730 to 1,900 establishments per wave and includes detailed information on each establishment, such as the industry classification, the age of the establishment, the location, and the existence or absence of a labor union. Of more importance, the WPS shows the total number of workers used by each establishment, including not only employees directly hired by the establishment, such as permanent workers, but also the various types of temporary workers, such as dispatched workers and subcontracted workers.[6]

Therefore, the data allow us to examine the effects of employment protection on the overall employment levels of an establishment and the numbers of temporary workers at the establishment. Moreover, the data have been collected since 2005, which predates the labor reform by two years.

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[6]In this article, we use two terms, total workers and total employment, interchangeably to refer to the sum of permanent and temporary workers used by an establishment, although from a legal perspective some types of temporary workers do not have a direct employment relationship with the establishment.

Thus, we are able to identify establishments that were expected to be severely affected by the employment protection legislation by examining the use of fixed-term contract and dispatched workers in 2005, a year that was not altered by the reforms in 2007.

Table 1 summarizes the data used for our empirical analysis. The results for the full sample are presented in the first column. We also partition the full sample into three groups based on the proportion of fixed-term contract and dispatched workers among all workers in each establishment in 2005. The results for the first, second, and third terciles are reported in columns (2) through (4), respectively. The numbers in the first row of each panel indicate the number of observations in the sample indicated in the column heading. As more than one-third of establishments in the sample hired no fixed-term contract and dispatched workers in 2005, the first tercile contains approximately 38% of the sample, whereas the second tercile contains 29% of the sample. The establishments in the second and third terciles hired at least one fixed-term contract or dispatched worker in 2005. The main entries and the entries in brackets, respectively, report the means and the standard deviations of the variables listed in the left-most column.

The summary statistics of variables related to employment at the establishments are summarized in panel A. In panel B we provide descriptive statistics of other variables pertaining to the characteristics of the establishments. In panel C, the firms' financial information is described. For the analysis using these variables, we drop establishments with missing data on any key financial information, for example, labor cost, capital stock, operating profits, and revenue. We also exclude observations for which the ratio of operating profits to revenue is greater than 1 in terms of the absolute value, following Draca, Machin, and Van Reenen (2011).

A large amount of variation in average observable characteristics of firms exists among the three terciles based on the share of fixed-term contract and dispatched workers in 2005. For instance, the mean number of total workers for establishments in the first tercile, which hired no fixed-term contract or dispatched workers in 2005, is approximately 287, whereas for the second and third terciles, the sizes of establishments are approximately 616 and 694, respectively. Likewise, other characteristics such as establishment age and labor cost per workers differ across the initial shares of fixed-term contract and dispatched workers. To explicitly address the possibility of heterogeneity in the underlying trend of outcome variables by the characteristics of establishments, we control for the size-by-year and sector-by-year effects in all the specifications, as explained below.[7] Furthermore, as a robustness check, we limit the sample to the second and third terciles, for

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[7]The average size of establishments in each sector varies by a large amount across 65 sectors in our data. Specifically, the average number of employees at establishments in each sector ranges from 57 to 2,243. Firms that specialize in the repair of computers, personal, and household goods hire approximately 57 employees on average, whereas the average employment of firms in financial service activities is approximately 2,243.

*Table 1.* Summary Statistics

| Variable | Full sample (1) | 1st tercile (2) | 2nd tercile (3) | 3rd tercile (4) |
|---|---|---|---|---|
| **Panel A: Employment** | | | | |
| | *N* = 5,809 | *N* = 2,203 | *N* = 1,675 | *N* = 1,931 |
| Total workers | 516.9 | 286.9 | 615.7 | 693.5 |
| | [1273.3] | [897.1] | [1310.6] | [1539.9] |
| Permanent workers | 399.5 | 233.2 | 526.7 | 478.9 |
| | [998.4] | [652.6] | [1185.6] | [1114.8] |
| Temporary workers | 117.4 | 53.7 | 89.0 | 214.6 |
| | [457.4] | [428.9] | [297.4] | [572.8] |
| Protected temporary workers | 63.5 | 21.8 | 28.4 | 141.7 |
| | [313.6] | [299.8] | [104.8] | [418.0] |
| Unprotected temporary workers | 53.8 | 31.9 | 60.6 | 72.9 |
| | [296.9] | [306.6] | [269.0] | [307.0] |
| Share of temporary workers | 0.17 | 0.1 | 0.12 | 0.29 |
| | [0.22] | [0.19] | [0.15] | [0.24] |
| Share of protected temporary workers | 0.1 | 0.03 | 0.05 | 0.21 |
| | [0.17] | [0.13] | [0.09] | [0.21] |
| Share of unprotected temporary workers | 0.07 | 0.06 | 0.07 | 0.09 |
| | [0.14] | [0.14] | [0.13] | [0.16] |
| **Panel B: Characteristics of establishments** | | | | |
| | *N* = 5,809 | *N* = 2,203 | *N* = 1,675 | *N* = 1,931 |
| Share of workers with performance pay | 0.43 | 0.35 | 0.48 | 0.50 |
| | [0.49] | [0.47] | [0.49] | [0.49] |
| Organized labor union | 0.42 | 0.27 | 0.50 | 0.53 |
| | [0.49] | [0.44] | [0.5] | [0.5] |
| Establishment age | 22.92 | 19.31 | 26.25 | 24.16 |
| | [17.15] | [14.05] | [17.5] | [19.17] |
| Located in metropolitan cities | 0.57 | 0.52 | 0.54 | 0.64 |
| | [0.5] | [0.5] | [0.5] | [0.48] |
| Business entity is a corporation | 0.97 | 0.95 | 0.98 | 0.99 |
| | [0.17] | [0.23] | [0.14] | [0.08] |
| Single-unit firm | 0.57 | 0.70 | 0.48 | 0.50 |
| | [0.49] | [0.46] | [0.5] | [0.5] |
| **Panel C: Financial information** | | | | |
| | *N* = 3,134 | *N* = 1,103 | *N* = 1,086 | *N* = 945 |
| Labor cost per worker (million KRW) | 48.6 | 41.9 | 51.3 | 53.3 |
| | [19.8] | [17.9] | [18.6] | [21.0] |
| Operating profits to revenue | 0.05 | 0.03 | 0.05 | 0.07 |
| | [0.14] | [0.12] | [0.14] | [0.17] |
| Revenue per worker (billion KRW) | 3.2 | 2.8 | 2.4 | 4.7 |
| | [16.4] | [13.1] | [8.2] | [24.7] |
| Capital per worker (million KRW) | 759.3 | 700.3 | 525.5 | 1096.8 |
| | [4164.9] | [4744.2] | [2738.9] | [4742.5] |

*Source:* Authors' calculation based on the Workplace Panel Survey.
*Notes:* Column (1) reports the statistics of the full sample. Columns (2)–(4) report, respectively, the statistics of the subsamples containing the first, second, and third terciles of the proportion of fixed-term contract and dispatched workers in 2005 in the establishments. The main entries and the entries in brackets report, respectively, the mean and the standard deviation of each variable. The numbers in the first row of each panel indicate the number of observations in the sample indicated in the column heading. KRW, South Korean won.

which observed characteristics of establishments are more balanced, and thus the underlying trend is more likely to coincide.

## Empirical Strategy

We apply a difference-in-differences framework to analyze how businesses adjust in response to changes in employment protection. Specifically, we compare the outcomes for establishments that were heavily affected by the labor reforms with the outcomes for establishments that were not substantially affected.

Applying the standard difference-in-differences method with a control group and a treatment group for the analysis assumes that establishments with different proportions of fixed-term contract workers and dispatched workers among all workers experience the same effect. It is unlikely though that an establishment that hires less than 1% of its employees as fixed-term contract and dispatched workers and an establishment that hires more than 90% of its employees as those types of temporary workers are affected to the same extent. In particular, establishments in the WPS exhibit a nearly all-encompassing range of the positive proportion of fixed-term contract and dispatched workers, from 0.01 to 100%.

To overcome this issue, we use the proportion of fixed-term contract and dispatched workers among all workers in each establishment in 2005, prior to the implementation of the employment protection, as a continuous indicator of the treatment intensity instead of using a control group and a treatment group.[8] We capture the effect of the employment protection legislation by comparing the mean changes of the outcome variables before and after the reform in 2007 across establishments with different degrees of treatment intensity. Thus, the causal effect of the labor reforms on an outcome variable can be summarized by estimates derived from Equation (1):

$$(1) \qquad Y_{ijrt} = \beta After_t \cdot Treat_i + \mathbf{X}'_{ijrt}\Phi + \delta_i + \rho_j + \gamma_r + \tau_t + \lambda_{st} + \mu_{jt} + \epsilon_{ijrt}$$

where $Y_{ijrt}$ indicates the various outcome variables of an establishment $i$, in industry $j$, in region $r$, at year $t$, such as the number of workers, the output per worker, and profitability. $After_t$ is a dummy variable that takes a value equal to 1 after the laws restricting the use of fixed-term contract and dispatched workers were enforced. $Treat_i$ denotes the intensity of the treatment, which is defined as the proportion of fixed-term contract and dispatched workers among all workers at each establishment in 2005. The coefficient of the interaction between $After_t$ and $Treat_i$ is the coefficient of interest; it captures the causal effect of the employment protection on an

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[8]The proportion of fixed-term contract and dispatched workers with more than two years of tenure in each establishment would be a more suitable measure for treatment intensity. Unfortunately, this is not a viable option because the WPS does not contain information about the average tenure of temporary workers.

outcome variable, $Y_{ijnt}$. $\delta_i$ and $\rho_j$ represent the set of dummy variables for an establishment and industry, and $\gamma_r$ and $\tau_t$ indicate region and time-fixed effects, respectively. $\mathbf{X}_{ijnt}$ represents time-varying characteristics of an establishment, such as the type of business entity, the existence of a labor union, and the use of a performance pay scheme for permanent workers.

For the difference-in-differences framework to be valid, the trends in outcome variables across establishments with different degrees of treatment intensity should be similar in the absence of the employment protection legislation. One way to support this common trend assumption is to show that the pre-legislation trends in outcome variables are similar across establishments. Unfortunately, this approach is not possible in this case as the WPS contains only one wave of data prior to the enforcement of the law. Thus, to further account for the possible difference in the trend across establishments with different treatment intensities, we also include size-by-year effects and industry-by-year effects in all of the empirical specifications. Specifically, $\lambda_{st}$ are size-by-year effects, where $s$ represents the categorized size of the establishment,[9] and they control for the possible difference in trends across establishments with different sizes. Similarly, $\mu_{jt}$ are industry-by-year effects. Finally, the error term, $\epsilon_{ijnt}$, is clustered at the establishment level.

## Results

### The Effect of Employment Protection on Employment

Next, we present empirical evidence regarding the causal impact of the Act on the Protection for Temporary Workers. Table 2 summarizes the results of the estimation with Equation (1) using the log of employment according to various types of contracts and the proportion of various types of temporary workers out of all workers as outcome variables.

Column (1) in Table 2 provides the result regarding the impact of employment protection legislation on establishment-level employment. The coefficient is small but negative and statistically significant, which implies that employment protection legislation had a negative impact on the labor use of the businesses. Our interpretation of the coefficient is not identical to the interpretation in a typical difference-in-differences model, as we use continuous treatment intensity. In our case, a 10 percentage point increase in the treatment intensity is associated with a 3.2% decrease in total number of workers. For instance, an establishment that had employed 10% of their workers on fixed-term and dispatched contracts would experience an approximately 3.2% decrease in their total number of workers as a result of

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[9]We categorize the size of establishments into three groups based on their total employment in 2005. The first group consists of establishments with total employment of less than 100 and the second group consists of total employment of more than 100 and less than 300. Establishments hiring 300 or more workers fall into the third group.

*Table 2.* Effect of Employment Protection on Employment

| | In logs | | | | | Proportion | | |
| Dependent variable | Total workers (1) | Permanent workers (2) | Temporary workers (3) | Protected temporary workers (4) | Unprotected temporary workers (5) | Temporary workers (6) | Protected temporary workers (7) | Unprotected temporary workers (8) |
|---|---|---|---|---|---|---|---|---|
| After · Treat | −0.321*** | 1.149*** | −3.387*** | −5.331*** | 1.013*** | −0.572*** | −0.707*** | 0.135*** |
| | (0.100) | (0.152) | (0.291) | (0.329) | (0.329) | (0.038) | (0.032) | (0.026) |
| Adjusted $R^2$ | 0.897 | 0.870 | 0.627 | 0.679 | 0.408 | 0.563 | 0.613 | 0.406 |
| $N$ | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 |

*Source:* Authors' calculation based on the Workplace Panel Survey.

*Notes: After · Treat* is the interaction between *After* and *Treat* where *After* is a dummy variable indicating periods after the Act on the Protection of Temporary Workers was enforced in 2007 and *Treat* is the intensity of treatment defined as the proportion of fixed-term contract and dispatched workers among the total workers at each establishment in 2005. For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, existence of a labor union, and use of performance pay scheme for permanent workers. All regressions include establishment-, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

the labor reforms, compared to an establishment that had employed no such workers.

Moreover, employment protection legislation for temporary workers increased permanent employment and decreased temporary employment at the establishment level. Columns (2) and (3) show a statistically significant effect of the labor reforms on the level of permanent and temporary employment in opposite directions. The magnitude of the coefficient, indicating the increase in permanent employment, 1.149, is smaller than that describing the decrease in the temporary employment, –3.387, an outcome consistent with the negative effect of the labor reforms on employment overall at the establishment level. Therefore, an establishment with a 10 percentage point higher treatment intensity increased their number of permanent workers by 11.5% more and decreased temporary employment by 33.9% more than did those establishments with a lower pre-reform proportion of fixed-term contract and dispatched workers. As a result of the increase in permanent workers and the decrease in temporary workers due to the labor reforms, the proportion of temporary workers among all workers decreased within an establishment, as presented in column (6). For example, an establishment with a 10 percentage point higher proportion of fixed-term contract and dispatched workers prior to the reforms underwent a decrease in the proportion of temporary workers by 5.7 percentage points more than did an establishment with a lower treatment intensity level.

The next set of results shows that the labor reforms had heterogeneous effects on temporary workers across different types of temporary employment contracts. Column (4) shows the effect of the labor reforms on the incidence of fixed-term contract and dispatched workers, and column (5) exhibits the effect on the incidence of other temporary workers with atypical labor contracts. It shows that an establishment reduced its use of fixed-term contracts and dispatched workers but increased its use of other atypical labor contracts. The estimates in columns (4) and (5) demonstrate that, as a result of the labor reforms, an establishment that employed 10% of its workers on fixed-term and dispatched contracts prior to the reforms experienced a 53.3% decrease in the number of such workers and a 10.1% increase in the number of other temporary workers compared to an establishment that used no fixed-term contract or dispatched workers. Consistent with these results, the proportion of fixed-term contract and dispatched workers decreased, whereas the proportion of other forms of temporary employment increased in response to the new regulations. Columns (7) and (8) show that the decrease in the proportion of temporary workers is mainly driven by a decrease in the proportion of fixed-term contract and dispatched workers and that it is partially offset by an increase in the proportion of other types of temporary employment.

This result is similar to the findings in Autor (2003), which documented an increase in temporary employees as a result of the intent to protect the job security of permanent workers. In our case, the intent to protect specific

types of temporary contract workers increased the use of other forms of temporary employment that are legally less burdensome to employers.

Overall, our results show that the employment protection legislation had a negative effect on total labor use of establishments. This decrease in total employment is the result of an insufficient increase in the number of permanent employees, which failed to compensate for the sharp decrease of fixed-term contract and dispatched workers. We also document a response to the new laws that policymakers and legislators perhaps did not intend: establishments' increased use of certain types of temporary workers who are not protected by the law.

## Labor Unions and the Effect of Employment Protection

In this subsection, we show that the pattern of adjustments of labor use by businesses in response to employment protection for temporary workers depends on the employment protection level for permanent workers. Several previous studies, such as Autor (2003) and Chaurey (2015), suggested that employment protection level for regular workers affects demand for non-regular or temporary workers. In a similar vein, we examine whether the flexibility in the use of permanent workers affects firms' response to the employment protection for temporary workers. In particular, we provide evidence that the reduction in the number of fixed-term contract and dispatched workers—potential permanent workers—is more prevalent among establishments with a labor union. Furthermore, we show that the existence of a labor union in an establishment dampens the substitution of permanent workers for temporary workers and amplifies the increase in the use of atypical temporary contracts that are not protected by the labor reform.

The effect of labor reforms in relation to the existence of a labor union is shown in Table 3. Panel A in Table 3 describes the empirical results for the establishments with no labor union in 2005, and panel B summarizes the results for the establishments with a labor union. Panel C shows the results of testing the statistical difference between the coefficients for establishments with and without a labor union in 2005. We include the interaction of $After_t$, $Treat_i$, and an indicator variable for the existence of a labor union in 2005 in Equation (1) while also allowing other coefficients in the equation to vary across establishments with and without a labor union. Thus, the coefficient of the interaction of three variables, $After_t$, $Treat_i$, and the indicator variable for the existence of a labor union in 2005, will capture the difference in the coefficients in panels A and B.

The coefficients in column (1) of Table 3 show that the negative impact of employment protection legislation on total employment is similar regardless of the existence of labor unions, and that the slight difference is not statistically significant. Columns (2) and (3) show that establishments that have stronger protection for permanent workers increased their numbers of permanent workers by a smaller amount and decreased their numbers of

*Table 3.* Effect of Employment Protection Legislation on Employment by the Presence of a Labor Union

| | In logs | | | | | Proportion | | |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Dependent variable | Total workers (1) | Permanent workers (2) | Temporary workers (3) | Protected temporary workers (4) | Unprotected temporary workers (5) | Temporary workers (6) | Protected temporary workers (7) | Unprotected temporary workers (8) |
| **Panel A: Establishments without labor union** | | | | | | | | |
| After · Treat | −0.357*** | 1.263*** | −3.022*** | −4.843*** | 0.689* | −0.576*** | −0.700*** | 0.124*** |
| | (0.119) | (0.191) | (0.345) | (0.382) | (0.360) | (0.048) | (0.042) | (0.031) |
| Adjusted $R^2$ | 0.850 | 0.809 | 0.565 | 0.626 | 0.394 | 0.555 | 0.589 | 0.385 |
| N | 3,403 | 3,403 | 3,403 | 3,403 | 3,403 | 3,403 | 3,403 | 3,403 |
| **Panel B: Establishments with labor union** | | | | | | | | |
| After · Treat | −0.365* | 0.815*** | −4.799*** | −7.032*** | 1.635** | −0.606*** | −0.755*** | 0.149*** |
| | (0.188) | (0.225) | (0.620) | (0.636) | (0.800) | (0.064) | (0.042) | (0.054) |
| Adjusted $R^2$ | 0.904 | 0.886 | 0.635 | 0.698 | 0.404 | 0.587 | 0.671 | 0.455 |
| N | 2,406 | 2,406 | 2,406 | 2,406 | 2,406 | 2,406 | 2,406 | 2,406 |
| **Panel C: Testing** | | | | | | | | |
| After · Treat · LU | −0.00802 | −0.448 | −1.777** | −2.189*** | 0.946 | −0.0303 | −0.0559 | 0.0256 |
| | (0.221) | (0.295) | (0.706) | (0.739) | (0.872) | (0.080) | (0.060) | (0.062) |
| Adjusted $R^2$ | 0.059 | 0.104 | 0.097 | 0.230 | 0.078 | 0.159 | 0.325 | 0.082 |
| N | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 |

*Source:* Authors' calculation based on the Workplace Panel Survey.

*Notes:* Refer to the notes in Table 2 for definition of *After · Treat*. *After · Treat · LU* is the interaction between *After · Treat* and *LU* where *LU* is a dummy variable indicating establishments with a labor union in 2005. For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, and use of performance pay scheme for permanent workers with their interactions with *LU*. All regressions include establishment, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

temporary workers by a larger amount in response to the labor reforms. Columns (4) and (5) show that behavior by businesses to mitigate the impact of the labor reforms by decreasing their use of fixed-term contract and dispatched workers and increasing their use of atypical labor contracts by larger amounts is more prominent for the establishments with labor unions.

The results concerning the proportion of different types of temporary employment among the total workers are consistent with the results using the log of temporary employment. Columns (6) through (8) in Table 3 summarize the impact of the labor reforms on the proportion of temporary workers, fixed-term contract and dispatched workers, and other atypical temporary contract workers among all workers for establishments with and without a labor union in 2005. The increase in the proportion of other types of temporary contract workers and the decrease in the proportion of fixed-term contract and dispatched workers are larger among establishments with a labor union than for those without a labor union. These opposing forces result in a small net difference on the impact of the labor reforms on the proportion of temporary workers between establishments with and without a labor union in 2005. Although the difference in the coefficient is not statistically significant, the overall pattern is consistent with the influence of regulation on temporary workers being affected by the flexibility in the use of permanent workers.

In sum, the unintended consequences of the labor reform increased among establishments with a labor union. In particular, compared to establishments without a labor union, establishments with a labor union were reluctant to increase permanent workers and drastically reduced workers on types of contracts that would be converted to permanent contracts, in response to the labor reforms.

## Robustness Checks

Here, we perform several robustness checks of the main results presented above. To begin with, we limit the sample to the establishments that are observed in all four waves. That is, we use a balanced panel to see whether possibly endogenous attritions from the sample could bias the results.[10] These results are summarized in panel A of Table 4. Overall, the results are not only qualitatively but also quantitatively similar to the main results in Table 2, which uses an unbalanced panel.

We also examine the midterm effect of the employment protection legislation by restricting the sample to the first and fourth waves—as correspondingly observed in 2005 and 2011—of the survey. The results based on the sample excluding the intermediate years are presented in panel B of Table 4. These show that the effect of the employment protection measures

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[10]Because the WPS does not have information regarding the detailed cause of attrition, it is not possible to examine the effect of the survival probability, as attrition from the sample could simply be a result of establishments not responding to the survey.

*Table 4.* Effect of Employment Protection Legislation on Employment for Various Specifications and Various Samples

| | In logs | | | | | Proportion | | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable | Total workers (1) | Permanent workers (2) | Temporary workers (3) | Protected temporary workers (4) | Unprotected temporary workers (5) | Temporary workers (6) | Protected temporary workers (7) | Unprotected temporary workers (8) |
| **Panel A: Balanced panel** | | | | | | | | |
| After · Treat | −0.366*** | 0.925*** | −3.447*** | −5.609*** | 1.187*** | −0.560*** | −0.720*** | 0.160*** |
| | (0.117) | (0.154) | (0.323) | (0.379) | (0.404) | (0.039) | (0.034) | (0.032) |
| Adjusted $R^2$ | 0.899 | 0.880 | 0.628 | 0.672 | 0.427 | 0.554 | 0.588 | 0.411 |
| N | 4,364 | 4,364 | 4,364 | 4,364 | 4,364 | 4,364 | 4,364 | 4,364 |
| **Panel B: Using the first and the fourth waves of WPS** | | | | | | | | |
| After · Treat | −0.308** | 1.118*** | −3.775*** | −5.665*** | 1.356*** | −0.616*** | −0.776*** | 0.160*** |
| | (0.136) | (0.204) | (0.418) | (0.453) | (0.421) | (0.056) | (0.049) | (0.037) |
| Adjusted $R^2$ | 0.872 | 0.851 | 0.563 | 0.619 | 0.299 | 0.537 | 0.646 | 0.275 |
| N | 3,096 | 3,096 | 3,096 | 3,096 | 3,096 | 3,096 | 3,096 | 3,096 |
| **Panel C: Establishments reporting their use of temporary workers were stable** | | | | | | | | |
| After · Treat | −0.567*** | 0.881*** | −1.957*** | −4.855*** | 1.559*** | −0.485*** | −0.662*** | 0.177*** |
| | (0.171) | (0.212) | (0.429) | (0.540) | (0.528) | (0.062) | (0.054) | (0.039) |
| Adjusted $R^2$ | 0.891 | 0.896 | 0.592 | 0.647 | 0.371 | 0.602 | 0.685 | 0.400 |
| N | 2,219 | 2,219 | 2,219 | 2,219 | 2,219 | 2,219 | 2,219 | 2,219 |
| **Panel D: Establishments reporting their use of temporary workers increased** | | | | | | | | |
| After · Treat | −0.316 | 1.230*** | −2.519*** | −3.943*** | 1.581 | −0.560*** | −0.722*** | 0.162* |
| | (0.331) | (0.441) | (0.730) | (0.674) | (1.105) | (0.103) | (0.072) | (0.093) |
| Adjusted $R^2$ | 0.879 | 0.871 | 0.584 | 0.693 | 0.419 | 0.550 | 0.644 | 0.496 |
| N | 725 | 725 | 725 | 725 | 725 | 725 | 725 | 725 |
| **Panel E: Establishments reporting their use of temporary workers decreased** | | | | | | | | |
| After · Treat | −0.251 | 0.891*** | −3.879*** | −6.752*** | 1.905*** | −0.608*** | −0.878*** | 0.270*** |
| | (0.203) | (0.243) | (0.710) | (0.728) | (0.699) | (0.072) | (0.052) | (0.056) |
| Adjusted $R^2$ | 0.900 | 0.853 | 0.546 | 0.640 | 0.388 | 0.521 | 0.581 | 0.407 |
| N | 1,404 | 1,404 | 1,404 | 1,404 | 1,404 | 1,404 | 1,404 | 1,404 |

Table 4. Continued

| Dependent variable | In logs | | | | | Proportion | | |
|---|---|---|---|---|---|---|---|---|
| | Total workers (1) | Permanent workers (2) | Temporary workers (3) | Protected temporary workers (4) | Unprotected temporary workers (5) | Temporary workers (6) | Protected temporary workers (7) | Unprotected temporary workers (8) |
| **Panel F: Using the establishments that hired at least one temporary worker in 2005** | | | | | | | | |
| After · Treat | −0.449*** | 1.094*** | −2.321*** | −3.462*** | 0.879** | −0.573*** | −0.697*** | 0.124*** |
| | (0.112) | (0.198) | (0.297) | (0.327) | (0.392) | (0.042) | (0.037) | (0.029) |
| Adjusted $R^2$ | 0.891 | 0.872 | 0.588 | 0.637 | 0.388 | 0.610 | 0.668 | 0.429 |
| N | 3,606 | 3,606 | 3,606 | 3,606 | 3,606 | 3,606 | 3,606 | 3,606 |
| **Panel G: Control for the year-specific macro-shock** | | | | | | | | |
| After · Treat | −0.307*** | 1.125*** | −3.318*** | −5.246*** | 1.031*** | −0.562*** | −0.697*** | 0.135*** |
| | (0.096) | (0.155) | (0.286) | (0.329) | (0.328) | (0.038) | (0.032) | (0.026) |
| Adjusted $R^2$ | 0.897 | 0.870 | 0.628 | 0.680 | 0.407 | 0.564 | 0.614 | 0.405 |
| N | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 |
| **Panel H: Weighted according to establishment's employment in 2005** | | | | | | | | |
| After · Treat | −0.599*** | 1.124*** | −4.677*** | −7.495*** | 1.248 | −0.620*** | −0.797*** | 0.177*** |
| | (0.209) | (0.240) | (0.664) | (0.653) | (0.793) | (0.061) | (0.043) | (0.045) |
| Adjusted $R^2$ | 0.899 | 0.890 | 0.650 | 0.732 | 0.436 | 0.594 | 0.647 | 0.489 |
| N | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 |

*Source:* Authors' calculation based on the Workplace Panel Survey.

*Notes:* Refer to notes in Table 2 for the definition of *After · Treat*. For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, existence of a labor union, and use of performance pay scheme for permanent workers. All regressions include establishment-, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

is not driven by the initial responses of establishments, as the coefficients are fairly consistent with the main results even after excluding the intermediate years. The results indicate that the behavior of establishments to replace fixed-term contract and dispatched workers is still strongly observed even four years after the introduction of the labor reforms.

Since the data do not allow us to control for linear time trends varying with treatment intensities, we employ various alternative measures to lower the impact of possible heterogeneity in trends that would contaminate our identification strategy. We analyze the impact separately for the groups that are more likely to exhibit similar underlying trends.

We first analyze the effect using establishments that reported stable use of temporary workers prior to the implementation of employment protection legislation. In the 2005 wave, the WPS questionnaire asks respondents about how they tend to use temporary workers, with the replies categorized as follows: never used, declining, stable, increasing, and other. We restrict our analysis to the sample of establishments that reported their use of temporary workers as stable prior to 2005. That is, we provide a robustness check for our main results by examining the effect of the labor reforms only in establishments for which the trend in one of the outcome variables—not necessarily so for the other outcomes—was similar. These results are summarized in panel C of Table 4. In addition, we restrict our analysis to the sample of establishments that reported their use of temporary workers prior to 2005 was either increasing or decreasing. The results based on each of these two samples are presented in panels D and E of Table 4, respectively. Although some estimates are less precise, the overall results are consistent with the main findings.

Furthermore, we limit our sample to those with similar baseline characteristics in 2005 so that establishments in the limited sample will be more likely to follow a similar trend thereafter. Specifically, we drop establishments in the first tercile based on our treatment intensity and restrict our sample to businesses hiring at least one fixed-term contract or dispatched worker in 2005. As shown in Table 1, the establishments in the second and the third terciles exhibit similar observed characteristics. The results based on this limited sample are presented in panel F of Table 4, and they are both qualitatively and quantitatively similar to our main results.

As a further robustness check of our main results, we allow a macro-shock or a business cycle to affect establishments differentially according to the degree of their use of fixed-term contracts and dispatched workers. Our main specification used in Table 2 controls for year-specific macro-shocks, which are common across businesses. It is possible, however, that a macro-shock had heterogeneous effects on establishments with different treatment intensities. If this is the case, the results could have been driven by a macro-shock not causally linked to the labor reforms. This determination is important, as the Global Financial Crisis occurred after the labor reforms in 2007. We explicitly control for this possibility by following the approach of Kugler and Pica (2008) and incorporating the interaction between the GDP growth

*Table 5.* The Effect of Employment Protection Legislation on Labor
Cost and Profitability

| Dependent variable | Total labor cost per worker, in log (1) | Profit/Revenue (2) | Capital per worker, in log (3) | Proportion of workers under performance pay scheme (4) | Revenue per worker, in log (5) |
|---|---|---|---|---|---|
| *After · Treat* | 0.0550 | 0.0342 | 0.576* | 0.106 | 0.730*** |
| | (0.088) | (0.031) | (0.322) | (0.076) | (0.251) |
| Adjusted $R^2$ | 0.734 | 0.507 | 0.875 | 0.381 | 0.879 |
| N | 3,134 | 3,134 | 3,134 | 5,809 | 3,134 |

*Source:* Authors' calculation based on the Workplace Panel Survey.
*Notes:* Profit in column (2) refers to operating profits. Refer to notes in Table 2 for the definition of *After · Treat.* For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, and existence of a labor union. For all dependent variables except proportion of workers under performance pay scheme, we also control for the use of performance pay scheme for permanent workers. All regressions include establishment-, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.
*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

rate and the treatment intensity into Equation (1). The results for this estimation are reported in panel G of Table 4. Thus, this robustness check supports our contention that the main results are not driven by the heterogeneous impacts of the business cycle across establishments with different treatment intensities. Finally, we estimate the coefficients in Equation (1) by weighting observations according to the employment of establishments in 2005, with these results reported in panel H of Table 4. The results are mostly similar to the main results in Table 2.

## The Effect of Employment Protection on Other Firm-Level Outcomes

In this subsection, we discuss the effect of the labor reforms on certain firm-level outcomes, specifically, labor cost and profitability. The regulation on a firm's labor use might have a negative impact on not only employment but also profitability if it causes a large adjustment cost for firms.

First, we examine whether the labor reforms increased the average remuneration to workers as measured by the average labor cost per worker. The labor cost includes wages and other labor-related costs, such as fringe benefits and severance pay. Column (1) in Table 5 summarizes the result from Equation (1) using the log of the labor cost per worker as an outcome variable. The result suggests that the reforms caused an increase—although not statistically significant—in the labor cost per worker.

Furthermore, we examine the effect of the labor reforms on the profitability of establishments, defined in terms of the ratio of operating profits to sales. Following Draca et al. (2011), we confine the sample to those firms for which the absolute value of profitability is less than or equal to 1, to

minimize the impact of extreme values. Column (2) of Table 5 presents the changes in firms' profitability caused by the labor reforms. The small and statistically insignificant estimate suggests that the labor regulation had no discernible effect on firms' profitability. This insignificant change in profitability may have occurred for any of several reasons. First, establishments may have attempted to alleviate the impact by increasing their use of atypical labor contracts that are not protected by the reforms, as we observed in our main results. Next, establishments may have increased their capital per worker, which can help increase labor productivity and thereby mitigate the negative impact on profitability. Last, they could also improve their human resource management and production processes or simply increase the price of their products and services.

It is beyond the scope of this article to identify the mechanisms that affect profitability. Nevertheless, we provide suggestive findings that establishments adjusted not only the composition of their workforces but also other factors that may affect productivity, such as their capital levels and management. In columns (3) and (4) of Table 5, we provide the results estimated by Equation (1) using the log of capital per worker and the proportion of permanent employees rewarded according to their performances. As summarized in column (3) of Table 5, capital per worker increased because of the reforms, and this increase may have increased labor productivity and thereby partially offset the increase in the labor cost. A 10 percentage point increase in the treatment intensity is associated with 5.8% increase in capital per worker. The result shown in column (4) suggests that the use of performance-based pay schemes among permanent workers increased more for establishments with a higher treatment intensity level. This change in the salary scheme could have increased the labor productivity and compensated for the costs stemming from the reforms (Lazear 2000; Paarsch and Shearer 2000; Gielen, Kerkhofs, and Ours 2010).

Finally, we examine the impact of the employment protection measures on labor productivity as measured by the log of per worker revenue. This empirical result is provided in column (5) of Table 5 and supports a positive influence of employment protection on labor productivity. This result can be explained by the change in the composition of workers, the increased capital intensity, the expansion of the use of performance pay schemes, and the increased price of their products and services.

In Table A.1, we provide various robustness checks of the results pertaining to the firm-level outcomes analyzed in this section. We summarize the results using the same set of robustness checks we used to assess the employment outcomes in the preceding subsection. The estimated coefficients presented in Table A.1 are not as robust across specifications as are the results regarding the employment outcomes. Thus, we advise interpreting these results as suggestive evidence only for the response to the labor reform.

## Conclusion

In this article, we empirically examined adjustment efforts by firms in response to employment protection legislation in Korea. We investigated the impact of the Act on the Protection of Temporary Workers enacted in 2007, which restricted the maximum duration of specific types of temporary employment contracts. The impact of the reforms varied across businesses depending on their pre-reform use of fixed-term contract and dispatched workers. This variation allowed us to utilize a quasi-natural experiment research design. We applied a difference-in-differences framework to the establishment-level panel data and compared the mean change in outcome variables of businesses with different degrees of treatment intensity before and after the labor reforms were introduced in 2007.

Our empirical analyses showed that stronger employment protection led to a decrease in total employment at the establishment level. Moreover, the stronger employment protection for temporary workers decreased the relative proportion of temporary workers compared to permanent workers. We showed, too, that the reform had an unintended consequence of increasing the use of temporary employment contracts that are not protected by the reforms. We observed that businesses sharply reduced their use of fixed-term contract and dispatched workers and increased their use of other types of atypical labor contract workers. This pattern—the reduced use of temporary employment contracts that are potentially permanent after two years— is more prevalent among establishments with labor unions. The implications regarding employment are similar to those presented by Acemoglu and Angrist (2001) and Autor, Donohue, and Schwab (2006) in the sense that a policy that was intended to augment the job security of workers actually reduced employment.

We also evaluated whether the employment protection for temporary workers had an impact on the outcomes of firms beyond the employment effects. Overall, we found suggestive evidence that the regulation affecting specific types of labor contracts is a restriction from the firms' perspective, but we found no evidence that the labor regulations had a discernible negative impact on firms' profitability. In addition, we provided suggestive evidence that establishments increased their capital intensity levels, improved their management practices, and increased their labor productivity to actively cope with the new restriction on labor use and to mitigate the possible negative impact.

## Appendix A

In Table A.1, we provide various robustness checks of the results pertaining to the firm-level outcomes analyzed in the main text. We summarize the results using the same set of robustness checks we used to assess the employment outcomes.

*Table A.1.* Effect of Employment Protection Legislation on Labor Cost and Profitability for Various Specifications and Various Samples

| Dependent variable | Total labor cost per worker, in log (1) | Profit/Revenue (2) | Capital per worker, in log (3) | Proportion of workers under performance pay scheme (4) | Revenue per worker, in log (5) |
|---|---|---|---|---|---|
| **Panel A: Balanced panel** | | | | | |
| After · Treat | 0.0616 | 0.0372 | 0.434 | 0.118 | 0.715** |
| | (0.088) | (0.035) | (0.370) | (0.093) | (0.292) |
| Adjusted $R^2$ | 0.725 | 0.457 | 0.874 | 0.382 | 0.877 |
| N | 2,587 | 2,587 | 2,587 | 4,364 | 2,587 |
| **Panel B: Using the first and the fourth waves of WPS** | | | | | |
| After · Treat | 0.146 | 0.0414 | 0.125 | −0.0157 | 0.309 |
| | (0.093) | (0.043) | (0.490) | (0.103) | (0.391) |
| Adjusted $R^2$ | 0.720 | 0.367 | 0.832 | 0.343 | 0.847 |
| N | 1,612 | 1,612 | 1,612 | 3,096 | 1,612 |
| **Panel C: Establishments reporting their use of temporary workers was stable** | | | | | |
| After · Treat | 0.119 | −0.0109 | 0.995* | 0.189 | 1.282*** |
| | (0.176) | (0.054) | (0.564) | (0.129) | (0.457) |
| Adjusted $R^2$ | 0.719 | 0.599 | 0.881 | 0.339 | 0.892 |
| N | 1,274 | 1,274 | 1,274 | 2,219 | 1,274 |
| **Panel D: Establishments reporting their use of temporary workers increased** | | | | | |
| After · Treat | 0.280 | −0.0549 | −0.408 | 0.00184 | 0.358 |
| | (0.195) | (0.100) | (0.713) | (0.235) | (0.557) |
| Adjusted $R^2$ | 0.761 | 0.447 | 0.880 | 0.419 | 0.807 |
| N | 354 | 354 | 354 | 725 | 354 |
| **Panel E: Establishments reporting their use of temporary workers decreased** | | | | | |
| After · Treat | 0.119 | 0.0144 | 0.814 | −0.0526 | 0.676 |
| | (0.190) | (0.091) | (0.639) | (0.149) | (0.596) |
| Adjusted $R^2$ | 0.754 | 0.358 | 0.900 | 0.357 | 0.880 |
| N | 776 | 776 | 776 | 1,404 | 776 |
| **Panel F: Using the establishments that hired at least one temporary worker in 2005** | | | | | |
| After · Treat | 0.0621 | 0.0222 | 0.560 | 0.174* | 0.763*** |
| | (0.098) | (0.042) | (0.363) | (0.096) | (0.272) |
| Adjusted $R^2$ | 0.740 | 0.514 | 0.875 | 0.354 | 0.871 |
| N | 2,031 | 2,031 | 2,031 | 3,606 | 2,031 |
| **Panel G: Control for the year-specific macro-shock** | | | | | |
| After · Treat | 0.0515 | 0.0244 | 0.584* | 0.103 | 0.617*** |
| | (0.088) | (0.037) | (0.315) | (0.076) | (0.230) |
| Adjusted $R^2$ | 0.734 | 0.468 | 0.875 | 0.381 | 0.859 |
| N | 3,134 | 3,134 | 3,134 | 5,809 | 3,134 |
| **Panel H: Weighted according to establishment's employment in 2005** | | | | | |
| After · Treat | 0.0784 | 0.226 | 0.562 | 0.286*** | 0.300 |
| | (0.082) | (0.159) | (0.502) | (0.106) | (0.397) |
| Adjusted $R^2$ | 0.822 | 0.456 | 0.862 | 0.455 | 0.857 |
| N | 3,134 | 3,134 | 3,134 | 5,809 | 3,134 |

*Source:* Authors' calculation based on the Workplace Panel Survey.

*Notes:* Profit in column (2) refers to operating profits. Refer to notes in Table 2 for the definition of *After · Treat*. For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, and existence of a labor union. For all dependent variables except proportion of workers under performance pay scheme, we also control for the use of performance pay scheme for permanent workers. All regressions include establishment-, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

## Appendix B

In addition to our main analysis, we apply a standard difference-in-differences model with three groups partitioned according to treatment intensity terciles. The first tercile is the control group, which consists of establishments that hired no fixed-term contract and dispatched workers in 2005. The two treatment groups, one with a low treatment intensity level and one with a high treatment intensity level, correspond to the second and the third terciles, respectively.[11] The results, summarized in Tables B.1 and B.2, are mostly consistent with our main findings. A notable exception is the effect of the reform on overall employment among establishments with low treatment intensity. We believe that this quantitatively different estimate arose from the assumptions imposed in the difference-in-differences model with the three groups.

Although our interpretation of the estimated coefficients could be less strenuous, the difference-in-differences framework with three groups instead of a continuous level of treatment intensity assumes that establishments with different treatment intensity degrees experienced the same

*Table B.1.* Effect of Employment Protection on Employment using Difference-in-Differences with Three Groups

| Dependent variable | In logs | | | | | Proportion | | |
|---|---|---|---|---|---|---|---|---|
| | Total workers (1) | Permanent workers (2) | Temporary workers (3) | Protected temporary workers (4) | Unprotected temporary workers (5) | Temporary workers (6) | Protected temporary workers (7) | Unprotected temporary workers (8) |
| *After · High* | −0.0131 | 0.312*** | −1.575*** | −2.690*** | 0.704*** | −0.167*** | −0.226*** | 0.0588*** |
| | (0.040) | (0.049) | (0.128) | (0.110) | (0.139) | (0.015) | (0.011) | (0.010) |
| *After · Low* | 0.0882** | 0.140*** | −0.825*** | −1.437*** | 0.0360 | −0.0343*** | −0.0415*** | 0.00717 |
| | (0.035) | (0.040) | (0.118) | (0.092) | (0.126) | (0.011) | (0.008) | (0.009) |
| Adjusted $R^2$ | 0.897 | 0.867 | 0.629 | 0.696 | 0.411 | 0.539 | 0.566 | 0.406 |
| N | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 | 5,809 |

*Source:* Authors' calculation based on the Workplace Panel Survey.
*Notes: After · High* and *After · Low* are the interactions between *After* and *High*, and *After* and *Low* where *After* is a dummy variable indicating periods after the Act on the Protection of Temporary Workers was enforced in 2007; *High* is a dummy variable indicating establishments with a treatment intensity in the third tercile; and *Low* is a dummy variable for establishments with a treatment intensity in the second tercile. For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, existence of a labor union, and use of performance pay scheme for permanent workers. All regressions include establishment-, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.
*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

[11]We define the group with low treatment intensity levels as the second tercile, of which the proportion of fixed-term contract and dispatched workers in 2005 ranged from 0.01 to 10.26%. For the treatment group with high treatment intensity, that is, the third tercile, the proportion of fixed-term contract and dispatched workers in 2005 is 10.27% and greater.

*Table B.2.*  Effect of Employment Protection Legislation on Labor Cost and
Profitability using Difference-in-Differences with Three Groups

| Dependent variable | Total labor cost per worker, in log (1) | Profit/Revenue (2) | Capital per worker, in log (3) | Proportion of workers under performance pay scheme (4) | Revenue per worker, in log (5) |
|---|---|---|---|---|---|
| After · High | 0.00848 | 0.0123 | 0.104 | 0.00149 | 0.133* |
|  | (0.027) | (0.011) | (0.104) | (0.034) | (0.077) |
| After · Low | −0.0238 | 0.0197* | −0.0345 | −0.0150 | −0.0215 |
|  | (0.023) | (0.011) | (0.082) | (0.032) | (0.056) |
| Adjusted $R^2$ | 0.734 | 0.507 | 0.875 | 0.381 | 0.879 |
| N | 3,134 | 3,134 | 3,134 | 5,809 | 3,134 |

*Source:* Authors' calculation based on the Workplace Panel Survey.
*Notes:* Profit in column (2) refers to operating profits. Refer to notes in Table B.1 for the definition of *After · High* and *After · Low*. For all dependent variables, we control for time-varying characteristics of establishments, such as foreign share, type of business entity, and existence of a labor union. For all dependent variables except proportion of workers under performance pay scheme, we also control for the use of performance pay scheme for permanent workers. All regressions include establishment-, industry-, region-, and year-fixed effects, and size-by-year and industry-by-year effects. Standard errors in parentheses are clustered at the establishment level.
*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

effect. This assumption is strict, as the proportion of fixed-term contract and dispatched workers in 2005 among establishments with a low or high treatment intensity ranges from 0.01 to 100%.

## Appendix C

Here we examine whether the use of fixed-term contract and dispatched workers prior to the reforms is related to the subsequent attrition from our sample. The WPS data do not provide detailed causes of the attrition from the sample. Therefore, they do not allow us to examine the effect of employment protection on the survival of businesses, as attrition from the sample could simply be a result of establishments failing to respond to the survey or of the relocation of the firm.[12] Thus, it is not possible to interpret the result in this section of the impact of employment protection legislation on the survival rate of businesses. Nevertheless, we explore whether the probability of responding to the subsequent survey depends on the treatment intensity level by estimating the following equation:

$$(2) \qquad P_i = \beta_0 + \beta_1 Treat_i + \mathbf{X}'_{i,2005}\Xi + \varepsilon_i$$

[12]Although the survey does not provide detailed information pertaining to the attrition at the individual business level, it does tabulate the causes of attrition in the user's guide of WPS. For example, as of the 2011 survey, approximately 127 establishments were closed, whereas 531 businesses did not participate in the survey for various other reasons.

*Table C.1.* Correlation between the Treatment Intensity and the Probability of Responding in the Subsequent Surveys

| Dependent variable | *Responded in 2007* (1) | *Responded in 2009* (2) | *Responded in 2011* (3) | *Responded in all waves* (4) |
|---|---|---|---|---|
| **Panel A: All establishments that answered the survey in 2005** | | | | |
| *Treat* | 0.0112 | −0.0250 | −0.0766 | −0.0679 |
| | (0.061) | (0.064) | (0.068) | (0.069) |
| Adjusted $R^2$ | 0.090 | 0.095 | 0.069 | 0.074 |
| N | 1,905 | 1,905 | 1,905 | 1,905 |
| **Panel B: Establishments with less than 100 employees in 2005** | | | | |
| *Treat* | 0.0984 | −0.0700 | −0.0829 | −0.0759 |
| | (0.111) | (0.117) | (0.123) | (0.126) |
| Adjusted $R^2$ | 0.080 | 0.086 | 0.066 | 0.060 |
| N | 680 | 680 | 680 | 680 |
| **Panel C: Establishments with more than or equal to 100 employees in 2005** | | | | |
| *Treat* | −0.0302 | −0.0308 | −0.0826 | −0.0963 |
| | (0.076) | (0.081) | (0.086) | (0.087) |
| Adjusted $R^2$ | 0.105 | 0.099 | 0.061 | 0.086 |
| N | 1,225 | 1,225 | 1,225 | 1,225 |

*Source:* Authors' calculation based on the Workplace Panel Survey.
*Notes: Treat* is the intensity of treatment defined as the proportion of fixed-term contract and dispatched workers among the total workers at each establishment in 2005. All regressions control for characteristics of establishment in 2005, such as age of establishment, squared term of the age, foreign share, type of business entity, existence of a labor union, and use of performance pay scheme for permanent workers. They also include region dummy variables and industry dummy variables.
*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

where $P_i$ is an indicator variable that takes a value equal to 1 if the business $i$ is in the subsequent data. *Treat$_i$* is the variable of the treatment intensity used in the main equation; that is, the proportion of fixed-term contract and dispatched workers in 2005. We also control for various characteristics of establishments in 2005 that can affect the probability of answering the subsequent survey, $\mathbf{X}_{i,\,2005}$. Thus, if the probability of participating in the survey depends on *Treat$_i$*, the estimated coefficient, $\beta_1$, would be statistically significant.

Table C.1 summarizes the estimation results from Equation (2) using the various outcome variables. Columns (1) through (3) summarize how *Treat$_i$* is related with the probability of answering the survey in 2007, 2009, and 2011, respectively. Column (4) provides evidence on whether the initial use of fixed-term contract and dispatched workers is correlated with the likelihood of the participation of businesses in all the subsequent surveys. Panel A of Table C.1 reports the results using all establishments that participated in 2005, whereas panels B and C summarize the estimation results using relatively small businesses and relatively large businesses, respectively. The results shown in panel B are based on establishments with fewer than 100 employees in 2005, and panel C documents the results for firms with at least 100 employees in 2005.

For all specifications, the estimated relationship between $Treat_i$ and the probability of participating in a subsequent survey is small and insignificant. Results indicate that $Treat_i$ is overall negatively associated with the subsequent probability of participating in the survey. The magnitude is small and statistically insignificant, however. For instance, column (4) indicates that a 10 percentage point increase in the treatment intensity is associated with a less than 1 percentage point decrease in the probability of participating in all subsequent surveys. We find no evidence that the effect of $Treat_i$ is more prevalent for the relatively small establishments. The absolute value of the coefficients reported in panel B is not consistently larger than the estimates summarized in panel C of Table C.1. Overall, we find no evidence that the treatment intensity is negatively associated with the probability of participating in subsequent surveys.

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