Do anti-discrimination laws alleviate labor market duality? Quasi-experimental evidence from Korea

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Abstract

This paper examines whether and how labor market duality can be alleviated through legislation that prohibits discrimination based on employment type. In 2007, the Korean government undertook a labor reform banning discriminatory treatment against fixed-term, part-time, and dispatched workers. By exploiting a gradual implementation of the anti-discrimination law by firm size targeting a subset of non-regular workers, I identify the treatment effects of the anti-discrimination law, taking a difference-in-difference-in-differences approach. The results suggest that the anti-discrimination law significantly increases hourly wages and the probabilities of being covered by national pension, health insurance, and employment insurance for targeted non-regular workers in small firms relative to other workers. Anticipatory behaviors of employers and selective transitions of employees in response to the implementation of the anti-discrimination law do not underlie the estimated effects. The presence of labor unions contributes to reducing gaps in labor conditions between regular workers and targeted non-regular workers.

1. Introduction

Labor market segmentation is a growing phenomenon in many countries across different continents (Autor, 2003; OECD, 2013), its causes having been accounted for in a wide array of theories (see, for example, Bulow and Summers, 1986; Reich, Gordon, and Edwards, 1973). An extreme form of segmentation, labor market duality is characterized by sizeable gaps in wage and non-wage benefits between workers in the primary and secondary sectors. As the large differentials in labor conditions produce various detrimental effects, the least of which are income inequality and relative poverty (Cazes and de Laiglesia, 2014), scholars and policy makers have raised concerns about the persistence of labor market duality in the economy.

The primary objective of this paper is to examine whether and how labor market duality can be alleviated through legislation that prohibits discrimination based on employment type. Inequalities in labor market outcomes are not only symptoms of labor market duality, but are also the main causes of the problem as they continuously provide employers with opportunities to profit from labor cost differentials between different types of workers. A regulation concerning the principle of equal pay for work of equal value has been one of the salient policy measures to reduce the gaps; however, the effectiveness of equal pay has not been rigorously studied (Cazes and de Laiglesia, 2014). Therefore, the main contribution of this paper is to provide empirical evidence on causal impacts of equal pay legislation on the gaps in labor conditions between different categories of workers.

The Republic of Korea (hereafter, Korea) provides a useful context to explore the contribution of equal pay legislation in two main respects. First, Korea is among the countries with a highly dualized labor market (Jones and Urasawa, 2013).[1] Non-regular workers who consist of contingent, part-time, and atypical workers, are subject to adverse labor conditions such as low wages, little employment protection, and weak social safety net coverage, while regular workers enjoy high wages, high levels of employment protection, and broad social safety net coverage.[2] Around 34% of wage workers in Korea were non-regular workers in

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[1] Koske, Fournier, and Wanner (2011) point out that Korea is one of few OECD countries where income inequality stems mostly from duality in the labor market.

[2] There is no commonly accepted definition for various non-traditional employment patterns including short-term and temporary work. Non-regular worker is a term that has been widely used in Korea since the 1980s. The Korea Tripartite Commission of Labor, Management, and Government agreed to the classification of non-regular workers according to employment type, and the labor reform in 2007 relied on this classification. Thus, the classification of non-regular workers is used in this paper. Table A1 outlines the definitions of different types of wage workers.

2013 (KNSO, 2013). Looking at the share of temporary workers for purposes of international comparison, Korea had the third highest number among the OECD countries in that year (OECD, 2013).[3]

The Korean context is also informative, because a labor reform in 2007 allows for a quasi-experiment research design to tackle the research question. Since 2007, part of the reform, the so-called "anti-discrimination law" has banned undue discriminatory treatment against fixed-term, part-time, and dispatched workers. By exploiting that the anti-discrimination law targets only a subset of non-regular workers and has been gradually implemented by firm size, I investigate whether and how the anti-discrimination law results in reducing the gaps in labor conditions between regular workers and targeted non-regular workers by applying a difference-in-difference-in-differences (DDD) estimation to the 2007-2010 waves of the Economically Active Population Survey (EAPS). More specifically, the empirical analysis estimates the changes in hourly wages and three major social insurance schemes (national pension, health insurance, and employment insurance) for targeted non-regular workers in firms that are subject to the anti-discrimination law relative to regular workers within the same firms and relative to workers in firms that are not subject to the anti-discrimination law.[4]

The main findings of this study can be summed up as follows. First, the anti-discrimination law leads to significant increases in hourly wages and the probabilities of being covered by national pension, health insurance, and employment insurance for targeted non-regular workers in small firms with 5 to 99 employees, relative to other workers. This suggests that a considerable number of targeted non-regular workers benefit from the reform, as targeted non-regular employment is highly concentrated in small firms. Second, anticipatory behaviors of employers and selective transitions of employees in response to the implementation of the anti-discrimination law do not underlie the estimated effects. Finally, the presence of labor unions contributes to reducing gaps in labor conditions between regular and targeted non-regular workers.

The article proceeds as follows. Section 2 provides an overview of gaps in labor conditions in Korea driven by labor market duality and a description of the anti-discrimination law. The

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[3] To enable better international comparisons, the OECD maintains a database on temporary workers, which are defined as wage workers whose job has a pre-determined termination date. For Korea, temporary workers include contingent workers, dispatched workers, and daily workers. When it comes to the share of temporary workers, Poland ranked highest, followed by Spain and Korea in 2013 (OECD, 2013).

[4] The Korean Employment Insurance System is a combination of a traditional unemployment benefits program and active labor market policy to prevent unemployment; thus, is called employment insurance rather than unemployment insurance (Yoo, 1999).

data are described in Section 3, and the estimation strategy is outlined in Section 4. Section 5 presents the main results, a set of robustness analysis, and a discussion on possible explanations for the heterogeneous treatment effects by firm size. Finally, Section 6 concludes with policy implications.

2. Background

2.1. Labor market duality in Korea

As the 1997 Asian financial crisis led to Korea's rapid integration in a globalized economy, firms began actively employing non-regular staff to reduce labor costs and to increase employment flexibility given the difficulty and cost of dismissing regular workers (Jones and Urasawa, 2013). As a result, the share of non-regular workers rose sharply —from 27.4% in 2002 to 37.0% in 2004—and it remains stable at a high level, around one-third of all wage workers (Ha and Lee, 2013). Like other OECD countries, non-regular employment in the Korean labor market is overrepresented among younger, less-educated, and female workers. In addition, due to early mandatory retirement practices in Korean firms the incidence of non-regular employment increases strikingly for older workers (Grubb, Lee, and Tergeist, 2007).

Non-regular workers in Korea receive significantly less in wage and non-wage benefits compared to their counterparts. To begin with the latter, there are significant differences in access to social insurance between regular and non-regular workers. The legal framework requires that the social insurance system cover nearly all wage workers. In practice, however, there is a sizable gap between the statutory coverage and actual coverage, with many non-regular workers excluded (Korea Labor Review, 2009). According to the EAPS, around 80% of regular workers received at least one social insurance benefit in 2010, while the corresponding figure for non-regular workers was only around 45% (KNSO, 2010).

More importantly, the wage gap between regular and non-regular workers in Korea is substantial. The EAPS indicates that non-regular workers were paid only 64.9% of the hourly wages of regular workers in 2010 (KNSO, 2010). However, unlike the other non-wage benefits, the presence of this "raw" wage gap does not necessarily mean that non-regular workers are discriminated against, since a considerable part of the differential is attributed to their productivity differences. Many studies have measured the "true" wage gap between regular

and non-regular workers in Korea, endeavoring to consider all possible productivity related characteristics, but different results have been found depending on methodology and data used in the analysis (see, for example, Lee, 2009; Nam, 2007; Park and Kim, 2007). There is no conclusive evidence on the size of the "true" wage gap, but most studies point out that the estimated "true" wage gap between regular and non-regular workers is statistically non-zero; it is smaller than the "raw" wage gap, thus discrimination against non-regular workers is likely to exist.[5]

Given that the incidence of non-regular workers in Korea is higher among vulnerable workers, the inferior labor conditions of non-regular workers have played a significant role in worsening income inequality (Jones and Urasawa, 2013). In addition, the persistence of sizable gaps in labor conditions drives Korean youth to make an unproductive effort to become regular workers, engendering inefficiency in the whole economy. The high college entrance rate, reaching almost 80%, and an excessive use of private tutoring to enter a prestigious university is characteristic of the current situation in Korea, where large differentials in labor conditions provide younger people with incentives to adopt extreme strategies to gain an upper hand over their competitors in this fierce job market.[6]

2.2. The 2007 anti-discrimination law

In 2007, the Korean government undertook a labor reform. The primary aim of the labor reform was to prevent the overuse of non-regular employment and to outlaw discrimination against non-regular workers. Although the labor reform faced strong opposition from both labor unions and business organizations, the reform bill was passed in December 2006 and became effective seven months later, beginning 1 July 2007.

One of the main changes introduced by the reform was the anti-discrimination law

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[5] It is very difficult to measure the exact level of discrimination against non-regular workers due mainly to unobserved individual and firm characteristics that affect both wage and employment type (Lee, 2009). Similarly, estimating the effect of the anti-discrimination law on the level of wage discrimination against targeted non-regular workers is complex. Given the limitations of quantifying discrimination, this paper alternatively examines whether targeted non-regular workers experience an increase in wages and other labor market outcomes relative to other workers as a result of the implementation of the anti-discrimination law.

[6] The percentage of high school graduates who begin four-year university courses or two-year technical college studies was 83.8% in 2008 (KEDI, 2009). According to the Survey of Private Education Expenditure (SPEE), 87.4% of elementary school students, 74.3% of middle school students, and 62.8% of general high school students received private tutoring in 2009 with an average monthly private tutoring expenditure per student of 242 thousand Korean won, approximately 220 US dollars (KNSO, 2009b).

prohibiting undue discriminatory treatment against fixed-term, part-time, and dispatched workers. The implementation of the anti-discrimination law has been gradual by firm size. It was first applied in July 2007 to the public sector and firms with 300 employees or more. This application gradually expanded to firms with 100 employees or more in July 2008 and five employees or more in July 2009. Article 8 of Act on the Protection, etc. of Fixed-term and Part-time Employees and Article 21(1) of Act on the Protection, etc. of Dispatched Workers state respectively that an employer shall not give discriminatory treatment against fixed-term, part-time, and dispatched workers on the ground of their employment status in comparison with workers without a fixed-term contract, full-time workers, and workers in the using firms who are engaged in the same or similar jobs in the business or workplace concerned. Workers can file a request for correction of discriminatory treatment in terms of wages and other labor conditions with the Korean Labor Relations Commission. In disputes relating to discriminatory treatment, the burden of proof is placed on employers. Penalties apply for noncompliance with a redress order confirmed by the Labor Relations Commission.[7]

Critics argued that the anti-discrimination law would produce few actual results, because the law lacks objective criteria by which discriminatory treatment could be defined (Cho, 2010). The relevant articles imply that an ideal approach to identifying discriminatory treatment is, for instance, to compare the wages of full-time and part-time workers engaged in the same job and workplace. Even in this ideal case, defining the same job or task is challenging as workers' contribution or productivity is not fully observable. Advocates such as Choi (2011), however, contend that the anti-discrimination law is designed to contribute to improving targeted non-regular workers' labor conditions, as it generates incentives for both employees and employers. On one hand, the anti-discrimination law creates a legal channel through which targeted non-regular workers can ameliorate unfavorable treatment in an active way. On the other hand, employers may also try to reduce discriminatory treatment, being afraid of punishment for noncompliance with the anti-discrimination law.

Empirically, few studies have been conducted on the effectiveness of the anti-discrimination law. To the best of my knowledge, Choi (2011) and Lee (2015) are the only two papers that

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[7] If an employer does not comply with a final redress order confirmed by the Labor Relations Commission without any justifiable reason, he or she is punished by a fine for negligence not exceeding 100 million Korean Won (approximately 85,000 US dollars). Furthermore, if an employer dismisses or gives other unfavorable treatment to a worker on the grounds that he or she made an application for redress to the Labor Relations Commission, the employer is punished by imprisonment of up to two years or a fine not exceeding 10 million Korean Won (approximately 8,500 US dollars).

empirically examine the impacts of the anti-discrimination law. Choi (2011) applies a difference-in-differences (DD) framework to establishment-level data, and finds significant positive effects for wage and training opportunities. Lee (2015) studies the impact of the anti-discrimination law on the wage structure of non-regular workers using the simplest triple difference estimation. He finds that the anti-discrimination law has a negative impact on non-regular workers' wages by lowering the probability that individual incentives will be included in wage structure. However, this research differs in at least two main respects. First, I employ an extended version of DDD estimation. This deals with the issue concerning a violation of the parallel trends assumption in the DD model and enables me to investigate the heterogeneous effects of the anti-discrimination law by firm size. Second, this paper uses individual-level data instead of establishment-level data. This allows me to perform more elaborate analysis on how much each individual worker's wage and non-wage benefits change before and after the reform.

3. Data

This paper employs the Economically Active Population Survey (EAPS), repeated cross-sectional data collected by the Korean National Statistics Office (KNSO). The Ministry of Labor uses official EAPS data to calculate the size of the non-regular employment population. The survey collects information on an individual's labor-related characteristics and other demographic characteristics. It is answered monthly by individuals who are 15 years old and over in 32,000 sample households in Korea.

I use data collected every March from 2007 to 2010. The rationale for this choice is that, since 2007, the KNSO has provided the supplementary survey of the EAPS by employment type every March, which constitutes crucial information for performing the DDD estimation. Specifically, the supplementary survey contains information about wage (average pretax monthly wage received for the last three months) and access to national pension, health insurance, and employment insurance, which are used as outcome variables. It also offers information with which I categorize workers by employment type. This enables me to classify regular workers, targeted non-regular workers, and non-targeted non-regular workers—the main subgroups in the analysis (figure 1).

INSERT FIGURE 1 AROUND HERE

Workers are considered "targeted" non-regular workers if they are categorized as fixed-term, part-time, or dispatched workers, while the remaining non-regular workers are considered "non-targeted" non-regular workers. I drop workers who can, in principle, belong to both targeted and non-targeted non-regular worker categories.[8] Since they can be regarded as both targeted and non-targeted non-regular workers, it is unclear in what way the anti-discrimination law affects their labor conditions. For these reasons, this group of workers, representing about 5% of total wage workers, is excluded from the sample.

The analysis focuses on the treatment effects of the anti-discrimination law for the three subgroups of wage workers. Thus, non-wage workers are dropped from the sample. Among wage workers, those who were temporally not working during the reference week are not included, as their hourly wages cannot be defined due to the zero hours worked for that period. Thus, I work with a sample of 96,246 wage workers from an overall sample of 273,471 individuals. About 18% of the sample is classified as targeted non-regular workers.

Since the dataset used in the analysis is compiled in March every year, no individual in the 2007 EAPS data was affected by the anti-discrimination law, while targeted non-regular workers who worked in the public sector or at a firm with 300 employees or more in the 2008 EAPS data were subject to the reform. In the same way, targeted non-regular workers whose workplace consisted of 100 employees or more in the 2009 EAPS data and those whose workplace consisted of five employees or more in the 2010 EAPS data were affected by the reform. Table 1 summarizes the gradual implementation of the anti-discrimination law by firm size.

INSERT TABLE 1 AROUND HERE

The dataset includes detailed information on the wage and non-wage benefits an individual worker receives. Monthly wage is transformed into hourly wage to make it easier and more informative to compare wages between full-time and part-time workers. Hourly wage is expressed in real terms, adjusted to 2010 prices using a consumer price index. Regarding national pension and health insurance, workers are considered to receive benefits from the

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[8] Typical examples are daily workers (non-targeted non-regular workers) who work in part-time employment (targeted non-regular workers) and temporary help agency workers (non-targeted non-regular workers) on fixed-term contracts (targeted non-regular workers).

National Pension Service (NPS) and National Health Insurance System (NHIS) only if they are workplace-based insured persons. The dataset also contains information on individual demographic characteristics (gender, age, educational attainment, marital status, and head of household) and job-related or firm characteristics (occupation, labor union status, industry). These variables are included in the regression model as individual-specific covariates. Table A2 summarizes the definitions of the variables used in the empirical analysis.

Table 2 reports the means of labor market outcome variables organized by year, firm size, and employment type. The table shows that, on average, in all firm size groups, targeted non-regular worker have lower hourly wages than regular workers and are less likely to be covered by national pension, health insurance, and employment insurance. The gaps in accessibility to social insurance tend to be larger in smaller firms. For instance, the probability that targeted non-regular workers will be covered by employment insurance in the smallest firms is about a half of that for regular workers in the same firm size group, while the corresponding difference between regular and targeted non-regular workers in large firms is marginal. It is also shown that regular workers have experienced a moderate improvement in labor conditions over time. Labor conditions of targeted non-regular workers in large firms have deteriorated during the sample period, while accessibility to social insurance for targeted non-regular workers in small and the smallest firms has drastically improved.

The table also reveals that there are marked differences between targeted non-regular workers and non-targeted non-regular workers. Overall, non-targeted non-regular workers are employed in jobs with poorer labor conditions. This describes a paradoxical situation wherein the anti-discrimination law does not target the workers in greatest need of improved labor conditions. In the case of workers' demographics (table A3), a typical targeted non-regular worker is a woman without a tertiary degree working in a small firm. Targeted non-regular workers are younger than regular workers in large firms, while those in small and the smallest firms are older.

INSERT TABLE 2 AROUND HERE

4. Methodology: Difference-in-Difference-in-Differences estimation

The anti-discrimination law creates three dimensions of variation that I exploit to identify the treatment effects. First, the gradual introduction of the anti-discrimination law by firm size generates variation across firm size groups and over time. This makes it suitable to employ an extended version of the DD estimation with multiple groups and time periods. For the DD estimator to yield a consistent estimate of the treatment effect, the parallel trends assumption needs to be satisfied (Angrist and Pischke, 2008); that is, in this context, in the absence of the treatment, wage (or other outcomes) trends would have been the same in both affected and unaffected firms. However, the fulfillment of the parallel trends assumption is challenging, as different firm size groups are heterogeneous in terms of capability to provide workers with wage and non-wage benefits. Some of the time-invariant heterogeneity could be controlled for by an inclusion of firm size fixed effects in the regression, but a presence of time-variant firm size group-specific shocks is problematic. For instance, negative macroeconomic shocks might affect smaller firms more than larger firms. In this case, the parallel trends assumption does not hold; thus the use of the DD method is inappropriate (Angrist and Pischke, 2008).[9]

I address this problem by focusing on the fact that the anti-discrimination law also creates variation within a firm. The anti-discrimination law was designed to target only fixed-term, part-time, and dispatched workers. This consequently leads the anti-discrimination law to influence workers in affected firms differently, creating "treatment" and "control" groups within the firm. I use as the treatment group targeted non-regular workers whose labor market outcomes may increase either absolutely or relatively to their counterparts, regular workers.

There are two reasons why I use only regular workers as the control group. First, discriminatory treatment against targeted non-regular workers is defined by comparing their labor market outcomes with their counterparts' labor market outcomes. For example, Articles 8(1) and 8(2) of Act on the Protection, etc. of Fixed-term and Part-time Employees indicate that the counterparts of fixed-term and part-time workers are workers under a labor contract without a fixed-term and full-time workers respectively, most of whom are regular workers. Second, as table 2 shows, labor conditions of non-targeted non-regular workers are poorer than

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[9] The parallel trends assumption cannot be graphically tested using years prior to the treatment, because the pretreatment period is not defined in the dataset. The supplementary survey of the EAPS in March started from 2007, and the anti-discrimination law was first applied to large firms in July 2007. Nevertheless, I performed a graphical analysis from 2007 to 2010, as some of the trends are comparable because of the gradual introduction of the anti-discrimination law. For instance, average hourly wages of targeted non-regular workers in medium-sized firms and small firms from 2007 to 2008 are comparable, because they were not subject to the anti-discrimination law in both years. Overall, the parallel trends assumption does not seem to hold (the graphs are available upon request).

those of targeted non-regular workers, and thus they cannot be the group that targeted non-regular workers want to catch up with in terms of labor conditions. For these reasons, non-targeted non-regular workers are excluded from the main analysis. Non-targeted non-regular workers are employed in placebo tests as robustness checks (Subsection 5.3).

The three dimensions of variation (employment type, firm size, and year) enable me to estimate the effect of the anti-discrimination law on the labor conditions of targeted non-regular workers relative to other workers using a DDD method. Take hourly wage, one of the dependent variables, as an example. The DDD method first compares the change in hourly wages of targeted non-regular workers in affected firms to the change in hourly wages of targeted non-regular workers in unaffected firms. This difference in differences is then compared to the difference between the change in hourly wages of regular workers in affected firms and the change in hourly wages of regular workers in unaffected firms. The use of the triple-differences model can difference out trends that may differ for affected and unaffected firms, addressing the concern on the parallel trends assumption in the DD estimator (Zavodny, 2000).[10]

The basic regression used to estimate the effect of the anti-discrimination law on the relative labor conditions of targeted non-regular workers includes fixed effects, interactions of the fixed effects, and individual-specific covariates. The equation at the individual level is

$$Y_{ijkt} = \alpha_j + \alpha_k + \alpha_t + \gamma_{jk} + \lambda_{jt} + \theta_{kt} + \delta D_{jkt} + X'_{ijkt}\,\beta + \varepsilon_{ijkt} \qquad (1)$$

where i denotes individuals, j denotes employment types (regular workers or targeted non-regular workers), k denotes firm size groups (large firms (300 employees or more), medium-sized firms (between 100 and 299 employees), small firms (between 5 and 99 employees), or the smallest firms (fewer than 5 employees)), and t denotes years (2007, 2008, 2009, or 2010). $Y_{ij\,kt}$ is the outcome of interest (logged hourly wage, national pension, health insurance, or employment insurance); α_j is a targeted non-regular worker dummy; α_k is a full set of firm size dummies; and α_t is a full set of year dummies. By including interactions of the fixed effects, this model provides full nonparametric control for the time-invariant firm size group-

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[10] The main advantage of the DDD estimation is that the identifying assumption is weak (Gruber, 1994). For example, even though national business cycle conditions changed over the 2007-2010 period, and this affected workers in affected firms differently than workers in unaffected firms, the DDD method yields an unbiased estimate of the effect of the anti-discrimination law if the relative effects were the same for targeted non-regular workers and regular workers.

specific effects of being a targeted non-regular worker (γ_{jk}), changes over time for targeted non-regular workers common across firm size groups (λ_{jt}), and firm size group-specific time effects common across employment types (θ_{kt}). The variable of interest, D_{jkt}, indicates targeted non-regular workers in firm size groups and years that are subject to the anti-discrimination law. Hence, the DDD estimate δ is interpreted as the effect of the anti-discrimination law on the relative earnings of targeted non-regular workers.

X_{ijkt} controls for observable individual characteristics and includes a dummy for female, age, age squared, dummies for educational attainment (6 categories), dummies for marital status (4 categories), a dummy for head of household, dummies for occupation (9 categories), a dummy for labor union status (4 categories), and dummies for industry (21 categories). Following Bertrand, Duflo, and Mullainathan (2004), I compute heteroscedasticity-robust standard errors to prevent, as much as possible, false rejections of the null hypothesis of no effect.[11]

Given that the anti-discrimination law has been applied to different firm size groups at different points in time, firms' reactions to the policy may not be homogenous. To check for the existence of such heterogeneous effects, I estimate equation (2) where the treatment effect in equation (1) is disentangled by firm size.

$$Y_{ijkt} = \alpha_j + \alpha_k + \alpha_t + \gamma_{jk} + \lambda_{jt} + \theta_{kt} + \delta^l D^l_{jkt} + \delta^m D^m_{jkt} + \delta^s D^s_{jkt} + X'_{ijkt}\beta + \varepsilon_{ijkt} \quad (2)$$

D^l_{jkt} is an indicator variable taking the value 1 if the individual is a targeted non-regular worker in a large firm in 2008, 2009, or 2010. Similarly, D^m_{jkt} takes the value 1 if the individual is a targeted non-regular worker in a medium-sized firm in 2009 or 2010, and D^s_{jkt} takes the value 1 if the individual is a targeted non-regular worker in a small firm in 2010. Thus, the coefficients of interest δ^l, δ^m, and δ^s capture the effect of the anti-discrimination law on the relative wages of targeted non-regular workers in large, medium-sized, and small firms,

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[11] According to Bertrand, Duflo, and Mullainathan (2004), the policy variable in this paper is likely to be serially correlated to some extent, which may lead to the underestimation of the true standard errors. Due to the small number of clusters (four firm size groups), clustering on firm size, employed in many DD papers, cannot be the solution to the problem in this context. However, the serial correlation in the current DDD regression may not be as serious as believed, because the analyzed time period is relatively short (four years) and an inclusion of firm-size group specific trends might already control for much of the correlation over time with the same firm-size group (Bertrand, Duflo, and Mullainathan, 2004). For these reasons, I estimate the regression models with heteroscedasticity- robust standard errors.

respectively.

In principle, equations (1) and (2) are appropriate to estimate wage. However, they are applied to the rest of the dependent variables under the assumption that the controls can also have an impact on the probabilities of being covered by national pension, health insurance, and employment insurance. Workers exempt from the scope of application specified by the National Pension Act, the National Health Insurance Act, and the Employment Insurance Act are excluded from the analysis when running each of the social insurance regressions, since they are ineligible for the social insurance benefit regardless of the imposition of the anti-discrimination law. Table A4 lists workers excluded when running each of the social insurance regressions. The social insurance regressions are estimated using probit models.

5. Results

5.1. Initial DDD estimates with the full sample

Table 3 presents the results of the DDD estimation, the impacts of the anti-discrimination law on hourly wage, national pension, health insurance, and employment insurance for targeted non-regular workers relative to other workers. Panel A shows the estimates of the overall treatment effects of the anti-discrimination law estimated by equation (1), while Panel B shows the estimates of the heterogeneous effects by firm size estimated by equation (2). Targeted non-regular workers appear to be relatively more likely to be covered by national pension, health insurance, and employment insurance after the firms are subject to the anti-discrimination law. However, the anti-discrimination law does not appear to significantly increase the relative hourly wages of targeted non-regular workers. The coefficient of hourly wage is positive, but not large enough to be statistically significant at any conventional level.

INSERT TABLE 3 AROUND HERE

Panel B provides evidence that different firm size groups react to the imposition of the anti-discrimination law in a different manner. Targeted non-regular workers in affected small firms appear to experience significant positive changes in all labor conditions, while there are no statistically significant impacts on labor conditions for targeted non-regular workers in affected

medium-sized and large firms. The results suggest that the positive overall effects on social insurance presented in Panel A are mainly driven by the positive effects in small firms. Targeted non-regular workers in affected small firms also experience an increase in hourly wages relative to other workers.

5.2. Main DDD estimates with the "private sector-dominated industries" sample

However, caution is needed when interpreting the results in table 3. The anti-discrimination law was first applied not only to large firms with 300 employees or more but also to firms in the public sector. The problem is that the EAPS dataset does not distinguish between these firm types, so that incorrect treatment assignment can generate bias, rendering the DDD estimator less convincing. To address this concern, I performed the following test. First, I calculated the share of workers in the public sector in each industry using information from the Census on Establishments conducted in 2009 by the Korean National Statistics Office (KNSO). In this database, each establishment is divided into one of four categories by the form of legal organization: individual proprietorship, incorporated company, non-business corporation, and unincorporated association. By dividing the number of employees that belong to non-business corporations by the number of all employees in each industry, I calculated the percentage of workers in the public sector in each industry (table A5).[12] Second, according to the shares of workers in the public sector computed, I generated a subsample that consisted of "private sector-dominated" industries. Four major industries with very low (around 1%) shares of workers in the public sector formed the subsample, which accounts for about a half of the whole sample. Finally, I estimated equations (1) and (2) using the chosen subsample.

The intuition behind this test is that in this chosen subsample, the incorrect treatment assignment mentioned above is likely to play a relatively minor role. I check how different these results and the initial results are. The results of the test are presented in table 4. In general, the estimates are similar to those obtained with the full sample. Targeted non-regular workers in affected small firms experience a significant improvement in all labor conditions relative to other workers, and the relative increases in the probabilities of being covered by the three social insurance programs in affected small firms are large enough to make the overall effects

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[12] The public sector in Korea consists of central administration organizations, local governments, public institutions, public enterprises, and educational institutions. With few exceptions, those institutions belong to the category "non-business corporation" in the Census of Establishments.

statistically significant. The results that are insensitive to the sample change suggest that the inability to distinguish workers in the public sector does not challenge the robustness of the DDD estimation.

INSERT TABLE 4 AROUND HERE

The DDD estimates reported in table 4 are considered more credible than those in table 3 in that a potential source of bias associated with the public sector issue is relatively minimized. Thus, the results in table 4 are regarded as the main findings of the paper, and the "private sector-dominated industries" sample is used for the rest of the analysis. Panel A in table 4 indicates that the probabilities that targeted non-regular workers in affected firms will be covered by national pension, health insurance, and employment insurance increase respectively by about 4, 5.3, and 3.8 percentage points relative to other workers.

Panel B in table 4 reveals that the positive overall effects on social insurance are mainly attributed to the positive effects in small firms. The probabilities of being covered by national pension, health insurance, and employment insurance for targeted non-regular workers in affected small firms increase by about 6.4, 7.5, and 4.7 percentage points, respectively. Targeted non-regular workers in affected small firms also experience an almost 5% increase in hourly wages relative to other workers, and the estimate is statistically significant at the 10% level. This in turn implies that the anti-discrimination law succeeded in improving all relative labor conditions of targeted non-regular workers only in small firms. Given that about 60% of targeted non-regular employment is concentrated in small firms with 5 to 99 employees (KNSO, 2010), it can be concluded that the anti-discrimination law partly achieved its intended goal.

The insignificant overall effect on hourly wage seems to be attributed to the relatively less significant effect on hourly wage for targeted non-regular workers in small firms. In fact, it is less clear for both employees and employers to define undue discriminatory treatment in terms of wage than the other labor conditions. Wage determination depends on productivity-related factors to a larger extent; some wage discrimination might be justified under the pretext of productivity differences, lessening employers' incentives to increase the relative hourly wages for targeted non-regular workers. In contrast, social insurance eligibility and entitlement are explicitly stated in the relevant legislation. Given that many targeted non-regular workers who are eligible for social insurance programs are not actually covered by them, I suggest that employers were under greater pressure to expand social insurance coverage for targeted non-

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regular workers.

5.3. Placebo tests

The robustness of the main findings is tested by performing placebo tests. For these tests, I excluded targeted non-regular workers from the main sample and included non-targeted non-regular workers. Equations (1) and (2) were estimated to measure the effect of the anti-discrimination law on non-targeted non-regular workers in affected firms relative to regular workers within the same firms and relative to workers in unaffected firms. By construction, non-targeted non-regular workers have nothing to do with the anti-discrimination law, because they are not targeted. However, in these tests, I treated non-targeted non-regular workers as if they were the group of workers that the anti-discrimination law targets. None of the estimates of these placebo policy variables in table 5 are statistically different from zero at conventional levels, reaffirming that the DDD estimator yields an unbiased estimate of the treatment effect.

INSERT TABLE 5 AROUND HERE

5.4. Anticipatory effects

The main findings may still suffer from bias if an anticipatory effect plays a role (Angrist and Pischke, 2008). For instance, the DDD estimates would be biased if firms anticipating the implementation of the anti-discrimination law raised wages for targeted non-regular workers immediately prior to its imposition, because they knew they had to do so in the near future. This would render the treatment effect underestimated. To explore the existence of the anticipatory effect, I repeated the regressions in table 4 adding leads of the anti-discrimination law as in Autor (2003). More specifically, I augmented equation (1) with an indicator for one year before the implementation of the anti-discrimination law. The lead dummy takes the value 1 if the individual is a targeted non-regular worker in a large firm in 2007, in a medium-sized firm in 2008, or in a small firm in 2009. For equation (2), I included the leads for medium-sized and small firms, respectively. The lead for large firms is not included, since there is only one year prior to the implementation of the anti-discrimination law for large firms.

Four years of data might not be enough to carry out this analysis in an elaborate fashion, particularly in estimating equation (2). To double check the heterogeneous effects of the lead

and policy variables by firm size, I also ran specifications where all individuals in large firms are dropped. The estimated coefficients of the policy variables produced by the two types of specifications are similar to the main results in table 4, except that the coefficient of hourly wage in small firms is not statistically significant in the first types of specifications (table 6), while the overall effect on employment insurance is not statistically significant in the second types of specifications (table 7). However, none of the coefficients of the leads in either specification are statistically different from zero, which shows little evidence of an anticipatory response about the anti-discrimination law.

INSERT TABLE 6 AROUND HERE

INSERT TABLE 7 AROUND HERE

Due to a lack of pre-lead years, the heterogeneous anticipatory response of large firms cannot be investigated with the current dataset. However, there is little reason to believe that large firms would preemptively raise wages for targeted non-regular workers before they were subject to the anti-discrimination law since increasing wages is costly. Moreover, given the short time interval (seven months) between the enactment and implementation of the anti-discrimination law, the anticipatory effect might be even harder to identify in the large firms to which the anti-discrimination law was first applied.

5.5. Composition changes

A composition change resulting from treatment needs to be considered as a possible channel to explain the main findings (Angrist and Pischike, 2008). For instance, if workers selectively move to larger (affected) firms or become targeted non-regular workers to benefit from the expected relative improvement in labor conditions arising from the anti-discrimination law, the estimated treatment effects may be confounded. Even in this case, it is still valid that the estimated effects are due to the implementation of the anti-discrimination law. However, it is unclear whether they are direct effects of the policy or effects of the selective transitions of workers across employment types or firm size groups.

Figure 2 shows the compositions of regular workers, targeted non-regular workers, and non-targeted non-regular workers by firm size over time. In general, no dramatic change is observed

in the compositions. In 2008, the share of targeted non-regular workers decreased and the share of non-targeted non-regular workers increased in the smallest firms (figure 2d). However, such changes do not seem to be associated with the implementation of the anti-discrimination law, as the smallest firms have never been affected. Similarly, figure 3 describes the compositions of different firm size groups by employment type over time. The shares of regular and non-targeted non-regular workers in each firm size group have been relatively constant over time (figure 3a and 3c). An increase in the share of targeted non-regular workers in small firms in 2010 might seem to be correlated with the timing of the anti-discrimination law; however, the increase is not attributed to decreases in the share of targeted non-regular workers in the smallest firms.

INSERT FIGURE 2 AROUND HERE

INSERT FIGURE 3 AROUND HERE

An ideal way of testing whether the selective transitions of workers across employment types have taken place is to run the DDD regressions using employment type dummies as dependent variables. However, the problem is that employment type and firm size are two of the three dimensions of variation exploited to identify the treatment effects in the DDD regressions. For example, when using a targeted non-regular worker dummy as a dependent variable, employment type fixed effects and the interactions cannot be included on the right-hand side in the regressions, which is in turn equivalent to running DD regressions. As noted in Section 4, the DD estimation produces less convincing results that are too weak to rule out the potential sources of bias concerning the composition changes.

Furthermore, the data used in this paper are repeated cross-sectionally and do not provide information on individuals' employment history. This makes it hard to directly control for the selective transitions of workers in the main DDD regressions. However, information on when individuals started their current jobs is available. I used this information to at least partially control for the composition changes given the data limitations. I divided the sample into two groups: (potential) "changers" who started their current jobs after the reform, July 2007 and "stayers" who started their current jobs before the reform, July 2007. The main DDD regressions were modified to include interactions of the policy and control variables (except age and age squared) with indicators for "changers" and "stayers", respectively, and were

reestimated. In this practice, I checked for similar treatment effects among the "stayers" who had nothing to do with the selective transitions because they have continued in the jobs they had before the anti-discrimination law came into effect.

Table 8 shows that the "stayers" experience similar treatment effects: targeted non-regular workers' relative labor conditions are improved only in affected small firms. This implies that the implementation of the anti-discrimination law indeed produces a positive outcome for targeted non-regular workers' relative labor conditions in small firms in a direct way rather than through composition changes. Given the data limitations, the results can be seen as evidence supporting that the selective transitions of workers in response to the anti-discrimination law do not underlie the observed changes in targeted non-regular workers' relative labor conditions in small firms.[13]

INSERT TABLE 8 AROUND HERE

5.6. Possible explanations for the heterogeneous treatment effects by firm size

The question at hand is why the relative improvement of labor conditions for targeted non-regular workers is observed only in affected small firms. There are several possible explanations. First, it is probable that insignificant effects in large firms are not because targeted non-regular workers' labor conditions are not improved at all, but because the improvement of targeted non-regular workers' labor conditions is partly or completely offset by the improvement of regular workers' labor conditions. The rationale behind this "spillover from targeted non-regular workers to regular workers" hypothesis is that labor unions are highly concentrated in large firms, and the absolute majority of union members are regular workers.[14] Thus, the observed insignificant effects in large firms might be attributed to the influence of labor unions more interested in maximizing utility of their typical union members than in improving non-members' adverse labor conditions.

Economic theories suggest that there are two directions in which labor unions affect targeted

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[13] One may be concerned about the fact that the "stayers" are not fully comparable with workers in the "private sector-dominated industries" sample. For instance, the "stayers" group consists of fewer targeted non-regular workers, because they tend to have shorter tenure (KNSO, 2010). However, the difference in the shares of targeted non-regular workers in both groups of workers is not outstanding. The shares of targeted non-regular workers in the "stayers" and "private sector-dominated industries" samples are 12.78% and 15.26%, respectively.

[14] According to KNSO (2010), 72% of large firms have labor unions, while 22% of small firms have labor unions in 2010. It also reports that 92% of union members are regular workers.

non-regular workers' labor conditions. On one hand, bargaining theory by Ashenfelter and Johnson (1969) implies that labor unions may want to reduce gaps in labor conditions between regular and targeted non-regular workers. Given that labor unions' primary interest is to increase their bargaining power, an increase in the number of non-members can be a threat. Since the relatively low labor costs of hiring targeted non-regular workers leads to an increase in labor demand for such workers, labor unions are likely to be unhappy about large gaps in labor costs between regular and targeted non-regular workers. On the other hand, according to the theory of insiders and outsiders suggested by Solow (1985), members of labor unions tend to keep the group of insiders small and to curb outsiders' entry into the group in order to guard insiders' interests. They may hope to maintain gaps in labor conditions between regular and targeted non-regular workers, since the relatively low wages of targeted non-regular workers make outsiders attractive to hire, thus preventing the group of regular workers from growing.

The necessary condition required for spillover effects to explain the observed insignificant impacts in large firms is that the latter theory dominates, i.e., labor unions are not in favor of reducing gaps in labor conditions between regular and targeted non-regular workers. To obtain a clearer picture of labor unions' stance on the improvement of targeted non-regular workers' labor conditions, I estimated the DDD models separately for workers whose workplaces have labor unions and for workers whose workplaces do not have labor unions. As table 9 reports, in general, targeted non-regular workers with labor unions experience greater positive impacts in all labor conditions when the anti-discrimination law is introduced. The presence of labor unions seems to contribute to reducing the gaps in labor conditions between regular and targeted non-regular workers. Perhaps labor unions have played an active role in pushing employers to comply with the anti-discrimination law. Hence, the spillover effects hypothesis does not get empirical support.

INSERT TABLE 9 AROUND HERE

Another and quite intuitive possible explanation for why the anti-discrimination law's impacts are concentrated in small firms is that there has been greater discrimination in small firms. Since small firms are relatively less productive, they may have less capacity and will to treat all workers equally without discrimination. If discriminatory treatment against targeted non-regular workers was more prevalent in small firms, employers in small firms would have more room for the improvement of targeted non-regular workers' labor conditions.

The descriptive statistics presented in table 2 indicate that the gaps between regular and targeted non-regular workers in accessibility to social insurance are the largest in small firms, and the wage gap is the second largest in small firms (excluding the smallest firms). Given that, in principle, the social insurance systems cover nearly all wage workers with a few exceptions, the marked gaps in access to social insurance between regular and targeted non-regular workers reflects weak compliance, particularly among small firms (Jones and Urasawa, 2013). It might be that the larger gaps between statutory coverage and actual coverage in small firms were sufficient to be perceived as discrimination by both employers and employees, and therefore the anti-discrimination law has contributed to significantly increasing the social insurance system coverage of targeted non-regular workers, especially in small firms. On the other hand, it is relatively difficult to make a concluding comment on whether there has been greater discrimination against targeted non-regular workers in small firms in terms of wage, as wage gaps do not necessarily indicate the level of discrimination.

Finally, the heterogeneous effects could also be accounted for by the fact that employers in small firms might be more afraid of receiving requests for correction of discriminatory treatment or of punishment. Since fines are not proportional to firm size, the burden of paying fines would be heavier for employers in small firms. Thus, the anti-discrimination law worked as a credible threat, leading them to respond to the policy in a more active manner. In this case, even in the absence of greater discrimination in small firms, the relatively heavier burden could result in the concentration of the treatment effects in small firms. The Central Labor Relations Committee (2013) shows that since small firms became subject to the anti-discrimination law, about 31% of complaints filed have been initiated by workers in small firms, indicating that the possibility of filing a complaint has acted as a credible threat to employers in small firms.

It is not evident through which mechanisms the anti-discrimination law affects the relative labor conditions of targeted non-regular workers in small firms. A small number of cases filed with the Labor Relations Commission and a low relief rate during the sample period suggest that the (partial) success of the anti-discrimination law was achieved by firms' taking action to reduce unreasonable discriminatory treatment against targeted non-regular workers, rather than by the legal process for redressing discriminatory treatment.[15] However, why small firms were the most affected still needs to be explored. While further examining this question is beyond

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[15] From 2007 to 2010, the Korean Labor Relations Commission received around 150 charges every year (Kwon, 2015), and the relief rate was 4.7% from July 2007 to June 2009 (The Central Labor Relations Committee, 2009).

the scope of this paper, it should be highlighted that the implementation of the anti-discrimination law is very likely to have a negative impact on small firms' labor costs. Policies such as subsidizing small firms' contributions to social insurance systems to expand the coverage of their workers will help ease the burden on small firms, thereby enhancing equal treatment for non-regular workers and strengthening the social safety net. Firm-level empirical research—such as the effects of the anti-discrimination law on labor costs, profitability, and employment—should be conducted to fully assess the overall effect of the anti-discrimination law and to further suggest fine-tuned policy recommendations.

6. Conclusion

This paper shows that a labor reform prohibiting discriminatory treatment against fixed-term, part-time, and dispatched workers significantly contributes to improving their relative labor conditions. The anti-discrimination law appears to lead to significant increases in hourly wages and the probabilities of national pension, health insurance, and employment insurance coverage for targeted non-regular workers in small firms with 5 to 99 employees, relative to other workers. Anticipatory behaviors of employers and selective transitions of employees in response to the implementation of the anti-discrimination law do not seem to account for the estimated effects, leaving the anti-discrimination law as a likely cause. Labor unions seem to contribute to reducing gaps in labor conditions between regular and targeted non-regular workers. It can be safely concluded that the policy intervention partly achieved its intended goal, as around 60% of targeted non-regular employment is concentrated in small firms. The findings suggest that policies imposing legal burdens on firms for unjustified discriminatory treatment can make a non-negligible contribution to alleviating labor market duality.

Further research is needed to clearly determine the channels through which the anti-discrimination law causes the positive effects observed in small firms. Panel data on individuals would help to fully control for the potential indirect effects of the anti-discrimination law. It is also important to further study the long-term effects of the anti-discrimination law. The positive impacts revealed in this paper may only take place for a few years after the anti-discrimination law is implemented, perhaps owing to the increased social interest in the early stage of implementation.

The aggregate effect of the anti-discrimination law is indeterminate, as it depends on how

firms react to the increase in the relative price of targeted non-regular workers. If the demand for targeted non-regular workers is elastic, firms are likely to lower the employment level of targeted non-regular workers, especially by means of reduced new employment. In this case, although the existing targeted non-regular workers' welfare increased due to improved labor conditions resulting from the anti-discrimination law, its effect on the total welfare of all targeted non-regular workers in the long run may be ambiguous. The welfare of the people who are willing to take non-regular jobs in the future may decrease due to the reduced chance of entering the labor market.

In addition, the increase in the relative price of targeted non-regular workers may increase the use of their substitutes, generating distributional effects. The anti-discrimination law is unable to protect non-targeted non-regular workers and provides firms with incentives to replace targeted non-regular workers with non-targeted non-regular workers, not with regular workers. Obviously, this is not a scenario the government expected. Therefore, policymakers who seek to curb the proliferation of precarious employment should pay particular attention to removing this loophole from anti-discrimination laws.

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Table 1
Gradual implementation of the anti-discrimination law by firm size

Firm size	Year			
	2007 EAPS	2008 EAPS	2009 EAPS	2010 EAPS
Large firms[a] (300 employees or more)	No	Yes	Yes	Yes
Medium-sized firms (Between 100 and 299)	No	No	Yes	Yes
Small firms (Between 5 and 99)	No	No	No	Yes
The smallest firms (Fewer than 5 employees)	No	No	No	No

NOTE.–EAPS = Economically Active Population Survey. Yes if firms are subject to the anti-discrimination law; No otherwise.

[a] Firms in the public sector belong to the group "large firms".

Table 2
Means of labor market outcomes

	Regular workers				Targeted non-regular workers				Non-targeted non-regular workers			
	2007	2008	2009	2010	2007	2008	2009	2010	2007	2008	2009	2010
A. Large firms (300 employees or more):												
Hourly wage	1.718	1.730	1.717	1.840	1.291	1.303	1.293	1.135	1.411	1.191	.966	1.693
National pension	.988	.985	.990	.993	.849	.816	.798	.720	.870	.745	.548	.775
Health insurance	.990	.984	.994	.993	.867	.842	.822	.764	.878	.765	.524	.775
Employment insurance	.821	.785	.784	.744	.785	.757	.771	.687	.824	.704	.524	.663
Observations	2370	2294	2415	2365	390	354	415	339	131	98	42	89
B. Medium-sized firms (between 100 and 299 employees):												
Hourly wage	1.267	1.286	1.322	1.362	1.093	.951	1.067	.968	.984	.897	.843	1.039
National pension	.958	.967	.968	.969	.808	.800	.798	.819	.628	.577	.414	.503
Health insurance	.965	.971	.973	.975	.830	.811	.846	.819	.644	.562	.434	.542
Employment insurance	.799	.777	.793	.807	.802	.784	.824	.787	.603	.592	.394	.536
Observations	1772	1778	1842	1945	459	435	421	342	239	130	99	153
C. Small firms (between 5 and 99 employees):												
Hourly wage	1.051	1.048	1.074	1.094	.784	.809	.793	.769	.804	.739	.696	.793
National pension	.801	.814	.823	.822	.526	.564	.588	.587	.328	.236	.160	.228
Health insurance	.806	.819	.835	.835	.544	.585	.608	.634	.348	.260	.192	.254
Employment insurance	.671	.675	.696	.701	.520	.554	.592	.622	.328	.243	.198	.269
Observations	9394	9257	9641	9706	2735	2390	2656	2871	2279	2212	1951	1960
D. The smallest firms (fewer than 5 employees):												
Hourly wage	.623	.633	.638	.664	.649	.625	.589	.619	.604	.540	.569	.617
National pension	.315	.314	.318	.327	.127	.164	.155	.171	.109	.077	.068	.085
Health insurance	.324	.334	.333	.335	.139	.183	.167	.191	.130	.105	.092	.109
Employment insurance	.284	.293	.309	.314	.126	.159	.163	.183	.117	.084	.078	.108
Observations	2758	2569	2655	2738	1057	900	885	934	949	1022	863	813

NOTE. – Hourly wage is presented in 10 thousands of Korean Won.

Table 3
Initial DDD estimates with the full sample

Variable	Log Hourly Wage	National Pension (Probit)	Health Insurance (Probit)	Employment Insurance (Probit)
A. Overall effects:				
Policy	.015	.127*	.143*	.207***
	(.016)	(.075)	(.075)	(.065)
		[.022]	[.025]	[.050]
Observations	83,082	75,668	76,291	77,953
B. Heterogeneous effects:				
Policy × Large	-.044	-.001	.113	.201
	(.032)	(.184)	(.185)	(.130)
		[-.001]	[.020]	[.049]
Policy × Medium	-.018	-.053	.012	.019
	(.026)	(.140)	(.141)	(.121)
		[-.009]	[.002]	[.005]
Policy × Small	.049**	.207**	.187**	.283***
	(.020)	(.085)	(.084)	(.081)
		[.036]	[.033]	[.068]
Observations	83,082	75,668	76,291	77,953

NOTE. – Heteroscedasticity-robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The full sample is used.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 4
Main DDD estimates with the "private sector-dominated industries" sample

Variable	Log Hourly Wage	National Pension (Probit)	Health Insurance (Probit)	Employment Insurance (Probit)
A. Overall effects:				
Policy	.018	.208*	.280**	.189*
	(.024)	(.116)	(.124)	(.103)
		[.040]	[.053]	[.038]
Observations	40,511	37,222	36,259	39,607
B. Heterogeneous effects:				
Policy × Large	-.022	.031	.273	.378
	(.046)	(.327)	(.362)	(.291)
		[.006]	[.051]	[.076]
Policy × Medium	-.021	-.117	-.075	-.066
	(.039)	(.215)	(.225)	(.194)
		[-.022]	[-.014]	[-.013]
Policy × Small	.049*	.339***	.400***	.236**
	(.029)	(.130)	(.139)	(.115)
		[.064]	[.075]	[.047]
Observations	40,511	37,222	36,259	39,607

NOTE. – Heteroscedasticity- robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The "private sector-dominated industries (manufacturing, construction, wholesale and retail sale, accommodation and food service activities)" sample is used.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 5
Placebo tests

Variable	Log Hourly Wage	National Pension (Probit)	Health Insurance (Probit)	Employment Insurance (Probit)
A. Overall effects:				
Policy	.045	-.018	-.093	-.031
	(.029)	(.146)	(.157)	(.118)
		[-.003]	[-.017]	[-.006]
Observations	40,106	36,107	34,806	39,458
B. Heterogeneous effects:				
Policy × Large	.083	-.297	-.005	.098
	(.069)	(.476)	(.472)	(.408)
		[-.055]	[-.001]	[.019]
Policy × Medium	.035	.099	-.009	-.013
	(.063)	(.248)	(.255)	(.221)
		[.018]	[-.002]	[-.003]
Policy × Small	.042	-.020	-.123	-.045
	(.032)	(.159)	(.172)	(.127)
		[-.004]	[-.023]	[-.009]
Observations	40,106	36,107	34,806	39,458

NOTE. – Heteroscedasticity- robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The "private sector-dominated industries (manufacturing, construction, wholesale and retail sale, accommodation and food service activities)" sample is used.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 6
DDD estimates with leads

Variable	Log Hourly Wage	National Pension (Probit)	Health Insurance (Probit)	Employment Insurance (Probit)
A. Overall effects:				
Lead	.007	.179	.164	.053
	(.023)	(.112)	(.121)	(.098)
		[.034]	[.031]	[.011]
Policy	.023	.318**	.381***	.222*
	(.030)	(.131)	(.141)	(.117)
		[.060]	[.072]	[.044]
Observations	40,511	37,222	36,259	39,607
B. Heterogeneous effects:				
Lead × Medium	-.070	-.128	-.231	-.176
	(.050)	(.279)	(.285)	(.246)
		[-.024]	[-.044]	[-.035]
Lead × Small	.003	.169	.206	.091
	(.030)	(.136)	(.149)	(.118)
		[.032]	[.039]	[.018]
Policy × Large	-.026	.085	.323	.399
	(.047)	(.330)	(.365)	(.294)
		[.016]	[.061]	[.080]
Policy × Medium	-.053	-.092	-.089	-.102
	(.049)	(.266)	(.283)	(.236)
		[-.018]	[-.017]	[-.020]
Policy × Small	.050	.401***	.476***	.270**
	(.032)	(.139)	(.149)	(.123)
		[.076]	[.090]	[.054]
Observations	40,511	37,222	36,259	39,607

NOTE. – Heteroscedasticity- robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The "private sector-dominated industries" sample is used.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 7
DDD estimates with leads (without workers in large firms)

Variable	Log Hourly Wage	National Pension (Probit)	Health Insurance (Probit)	Employment Insurance (Probit)
A. Overall effects:				
Lead	.011	.156	.151	.097
	(.025)	(.120)	(.129)	(.105)
		[.034]	[.032]	[.022]
Policy	.040	.265**	.306**	.193
	(.032)	(.135)	(.145)	(.119)
		[.057]	[.066]	[.044]
Observations	35,312	32,060	31,091	34,416
B. Heterogeneous effects:				
Lead × Medium	-.074	-.118	-.213	-.173
	(.050)	(.282)	(.288)	(.249)
		[-.026]	[-.046]	[-.039]
Lead × Small	.026	.145	.167	.121
	(.032)	(.140)	(.155)	(.123)
		[.031]	[.036]	[.027]
Policy × Medium	-.031	-.124	-.133	-.085
	(.050)	(.269)	(.287)	(.239)
		[-.027]	[-.029]	[-.019]
Policy × Small	.063*	.345**	.405***	.256**
	(.034)	(.141)	(.152)	(.124)
		[.075]	[.087]	[.058]
Observations	35,312	32,060	31,091	34,416

NOTE. – Heteroscedasticity- robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The "private sector-dominated industries (manufacturing, construction, wholesale and retail sale, accommodation and food service activities)" sample is used. Workers in large firms are dropped.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 8
DDD estimates for "changers" and "stayers"

Variable	Log Hourly Wage	National Pension (Probit)	Health Insurance (Probit)	Employment Insurance (Probit)
A. Overall effects:				
Changer × Policy	.017	.173	.244*	.122
	(.027)	(.120)	(.129)	(.107)
		[.032]	[.045]	[.023]
Stayer × Policy	.040	.501***	.503***	.636***
	(.026)	(.165)	(.165)	(.152)
		[.092]	[.092]	[.123]
Observations	40,511	37,222	36,259	39,607
B. Heterogeneous effects:				
Changer × Policy × Large	.032	-.179	.188	.224
	(.058)	(.363)	(.399)	(.322)
		[-.033]	[.035]	[.043]
Changer × Policy × Medium	.017	-.174	-.186	-.144
	(.044)	(.227)	(.238)	(.207)
		[-.032]	[-.034]	[-.028]
Changer × Policy × Small	.056*	.317**	.378***	.187
	(.030)	(.133)	(.143)	(.119)
		[.058]	[.070]	[.036]
Stayer × Policy × Large	-.019	.264	.399	.459
	(.048)	(.385)	(.408)	(.337)
		[.049]	[.073]	[.089]
Stayer × Policy × Medium[a]	-.010			
	(.047)			
Stayer × Policy × Small	.060*	.501**	.508***	.666***
	(.036)	(.198)	(.195)	(.184)
		[.092]	[.094]	[.129]
Observations	40,511	37,100	36,133	39,481

NOTE. – Heteroscedasticity- robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The "private sector-dominated industries" sample is used. The DDD regressions include interactions of the policy and control variables (except age and age squared) with indicators for "changers" and "stayers".
[a] Targeted non-regular workers in affected medium-sized firm among the "stayers" are dropped when social insurance regressions are estimated since all of them are covered by the social insurance. The policy variables for the "stayers" in medium-sized firms are omitted in the social insurance regressions.
  * $p < .10$.
  ** $p < .05$.
  *** $p < .01$.

Table 9
DDD estimates by presence of labor unions

Variable	Log Hourly Wage		National Pension (Probit)		Health Insurance (Probit)		Employment Insurance (Probit)	
	Union	No Union	Union	No Union	Union	No Union	Union	No Union
A. Overall effects:								
Policy	.089*	.019	.607**	.170	.804***	.179	.523**	.049
	(.052)	(.028)	(.271)	(.126)	(.287)	(.136)	(.257)	(.111)
			[.035]	[.038]	[.046]	[.040]	[.045]	[.011]
Observations	8,044	32,467	7,902	29,320	7,893	28,366	8,399	31,598
B. Heterogeneous effects:								
Policy × Large	.070	-.054	.402	-.165	1.032**	-.489	.827**	-.180
	(.076)	(.070)	(.430)	(.464)	(.469)	(.504)	(.367)	(.400)
			[.028]	[-.037]	[.059]	[-.109]	[.059]	[-.042]
Policy × Medium	.092	-.024	-.179	.137	.193	.177	.343	-.137
	(.068)	(.050)	(.360)	(.257)	(.378)	(.281)	(.341)	(.235)
			[-.012]	[.031]	[.011]	[.040]	[.025]	[-.032]
Policy × Small	.110	.044	1.101***	.205	1.337***	.231	1.024***	.110
	(.067)	(.032)	(.369)	(.137)	(.388)	(.148)	(.362)	(.121)
			[.077]	[.046]	[.076]	[.051]	[.073]	[.025]
Observations	8,044	32,467	8,247	29,320	7,893	28,366	8,009	31,598

NOTE. – Heteroscedasticity- robust standard errors are in parentheses, and marginal effects are in brackets. The national pension, health insurance, and employment insurance regressions are estimated using probit models. The "private sector-dominated industries (manufacturing, construction, wholesale and retail sale, accommodation and food service activities)" sample is used.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Appendix

Table A1
Definitions of wage workers by the EAPS

Employment type	Definition
Regular workers	Wage workers who do not belong to the category of non-regular workers
Non-regular workers	Contingent workers, part-time workers, or atypical workers
Contingent workers	Fixed-term workers or non-fixed-term contingent workers
Fixed-term workers	Workers with prescribed contract period
Non-fixed-term contingent	Workers with open-ended contracts who could be dismissed against their own will
Part-time workers	Workers who work fewer than 36 hours a week
Atypical workers	Dispatched workers, temporary help agency workers, independent contractors, at-home workers, or daily workers
Dispatched workers	Workers who have employment contracts with sending employers but work for and are supervised by using employers
Temporary help agency workers	Workers who have employment contracts with service companies and work for companies that have service work contracts with the service companies (ex. workers at a security service company or a cleaning service company)
Independent contractors	Workers who independently provide goods or service to customers and paid on a freelance basis (ex. insurance planners, caddies)
At-home workers	Workers who work at home in office-type jobs
Daily workers	Workers who work during special events, peak hours, or intermittently when business needs warrant

Table A2
Definitions of variables

Variables	Definition
Hourly wage	Average pretax hourly wage received for the last three months
National pension	1 if individual benefits from the National Pension System; 0 otherwise
Health insurance	1 if individual benefits from the National Health Insurance System; 0 otherwise
Employment insurance	1 if individual benefits from the Employment Insurance system; 0 otherwise
Female	1 if individual is female; 0 otherwise
Age	Age when the survey was conducted
Education	(The reference group is elementary school degree or less)
Middle school	1 if individual has a middle school degree; 0 otherwise
High school	1 if individual has a high school degree; 0 otherwise
Junior college	1 if individual has a junior college degree; 0 otherwise
University	1 if mother has a university degree; 0 otherwise
Graduate school	1 if mother has a graduate degree; 0 otherwise
Marital status	(The reference group is single)
Married	1 if individual is married; 0 otherwise
Bereaved	1 if individual is bereaved; 0 otherwise
Divorced	1 if individual is divorced; 0 otherwise
Head of household	1 if individual is a head of household; 0 otherwise
Labor union status	(The reference group is no labor union)
Cannot join union	1 if individual's workplace has labor unions, but not allowed to join; 0 otherwise
Do not join union	1 if individual's workplace has labor unions, but does not want to join; 0 otherwise
Member of union	1 if individual's workplace has labor unions; 0 otherwise
Size of firm	(The reference group is fewer than five employees)
Large firms	1 if the number of employees is equal to or more than 300; 0 otherwise
Medium-sized firms	1 if the number of employees is between 100 and 299 ; 0 otherwise
Small firms	1 if the number of employees is between 5 and 99 ; 0 otherwise

NOTE. – Monthly wage is first divided by 4.3 to estimate weekly wage, and hourly wage is calculated by dividing the estimated weekly wage by the number of hours worked in the previous week.

Table A3
Means of demographic characteristics

	Regular workers				Targeted non-regular workers				Non-targeted non-regular workers			
	2007	2008	2009	2010	2007	2008	2009	2010	2007	2008	2009	2010
A. Large firms (300 employees or more):												
Age	39.432	39.533	39.731	40.193	35.285	37.949	38.925	40.080	39.458	40.633	37.571	40.247
Female	.208	.216	.207	.223	.454	.424	.448	.552	.298	.367	.476	.416
Without tertiary degrees	.446	.395	.377	.362	.315	.333	.359	.445	.481	.541	.690	.416
Observations	2370	2294	2415	2365	390	354	415	339	131	98	42	89
B. Medium-sized firms (between 100 and 299 employees):												
Age	39.135	39.191	39.629	39.478	36.155	37.382	39.653	40.561	42.335	41.623	44.747	41.699
Female	.299	.280	.275	.278	.423	.480	.430	.494	.464	.492	.545	.477
Without tertiary degrees	.502	.443	.448	.438	.451	.531	.487	.573	.674	.700	.737	.627
Observations	1772	1778	1842	1945	459	435	421	342	239	130	99	153
C. Small firms (between 5 and 99 employees):												
Age	38.943	39.399	39.969	40.159	40.393	40.008	40.966	43.645	44.102	44.338	45.459	45.514
Female	.393	.397	.389	.389	.500	.504	.532	.605	.474	.463	.466	.460
Without tertiary degrees	.544	.515	.504	.500	.656	.626	.619	.674	.759	.798	.807	.764
Observations	9394	9257	9641	9706	2735	2390	2656	2871	2279	2212	1951	1960
D. The smallest firms (fewer than 5 employees):												
Age	39.368	40.109	40.519	40.691	42.515	42.306	43.374	43.344	47.213	46.873	48.254	47.963
Female	.560	.557	.583	.548	.610	.643	.660	.731	.521	.536	.539	.482
Without tertiary degrees	.722	.713	.703	.697	.810	.806	.781	.783	.875	.878	.913	.862
Observations	2758	2569	2655	2738	1057	900	885	934	949	1022	863	813

Table A4
Workers excluded when running the social insurance regressions

Dependent variable	Workers excluded	Related article
National pension	Workers aged under 18	Article 8 of National Pension Act
	Workers aged over 60	
	Daily workers who are employed for a period of less than one month	Article 2 of Enforcement Decree of the National Pension Act
	Part-time workers whose prescribed working hours are fewer than 80 hours in a month	
Health insurance	Daily workers who are employed for a period of less than one month	Article 6 of National Health Insurance Act
	Part-time workers whose prescribed working hours are fewer than 80 hours in a month	Article 9 of Enforcement Decree of the National Health Insurance Act
Employment insurance	Workers aged over 65	Article 10 of Employment Insurance Act
	Part-time workers whose prescribed working hours are fewer than 60 hours in a month	Article 3 of Enforcement Decree of the Employment Insurance Act
	Public officials, private school teachers, workers of a special post office	

Table A5
Share of workers in the public sector in each industry

Industry	Share
A. Agriculture, forestry and fishing	n/a
B. Mining and quarrying	13.68%
C. Manufacturing	0.70%
D. Electricity, gas, steam and water supply	58.06%
E. Sewage & waste treatment, material recovery and restoration activities of environment	15.55%
F. Construction	0.91%
G. Wholesale and retail sale	1.61%
H. Transportation	5.44%
I. Accommodation and food service activities	0.56%
J. Publishing, video, broadcast communications and information services	15.44%
K. Financial service and insurance activities	24.21%
L. Real estate activities and renting and leasing	5.00%
M. Professional, scientific and technical activities	14.18%
N. Business facilities management and business support services	3.65%
O. Public Administration and Defense ; Compulsory Social Security	100%
P. Education	61.95%
Q. Human health and social work activities	39.18%
R. Arts, sports and recreation related services	19.05%
S. Membership organizations, repair and other personal services	8.02%
T. Private households with employed persons	n/a
U. Extra-territorial organizations and bodies	n/a
Mean	15.43%

SOURCE. – Author's calculations using the Census on establishments in 2009 conducted by Korean National Statistics Office (KNSO).