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The Impact of Employment Protection on the Quality of Job Match : Evidence from Job Duration Data in South Korea

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Highlights

- This study analyzes the impact of regulations limiting the use of temporary employment.
- Following the implementation of the regulations, the probability of job separation in the first five months of tenure decreased, implying better job match quality.
- Based on worker overtime, there is no evidence that the regulation caused temporary employees to provide greater effort in their jobs.

Abstract

This study analyzes how firms and workers respond to regulations limiting the use of temporary employment. In 2007, the Korean government introduced a labor market reform that required employers to convert a worker's contract from a temporary to permanent one in order to continue to employ a worker for more than two years. From the perspective of employers, the new regulation can be thought of as a potential increase in firing costs for temporary workers after two years. Thus, employers have an incentive to improve the screening process to establish better matches and weed out bad matches prior to the increase in firing costs. From the perspective of workers, temporary workers have an incentive to provide greater effort after the policy change because the reform offers a potential path to permanent employment. My result shows economically and statistically significant decreases in the probability of job separation in the first five months of tenure after the policy change, which implies that firms respond to the increased protection for temporary workers by improving their recruitment practices. However, based on observed overtime, I find no evidence supporting the view that temporary employees provide greater effort after the reform.

JEL classification: J68; J63; J24; C41; C24; C25

Keywords: Labor market policy; Fixed-term contract; Job separation; Duration model; Employee effort; Overtime

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I. Introduction

An extensive literature has studied the effect of employment protection legislation[1] (EPL) on the labor market. Prior studies focused on whether strict protection for workers had an impact on the level of unemployment and employment in European countries in 1970s-90s, but their findings were inconclusive.

In the 1980s, several European countries introduced labor market reforms that allowed new forms of employment such as temporary contracts, fixed-term contracts or hiring through temporary help agencies, which relaxed existing labor market protection for specified classes of employment. Through the introduction of such alternative employment structure, policymakers hoped to make the labor markets more flexible and to lower unemployment. These reforms are usually called "two-tier" labor market policies or "partial" reforms because they tried to improve the flexibility "at the margin" of labor markets by easing restrictions on the use of temporary or fixed-term contracts while keeping strong protection for permanent workers (Bentolila and Dolado, 1994; Blanchard and Landier, 2002). Although such policies may have reduced rigidity in labor markets, they also encouraged firms to substitute temporary jobs for permanent ones. In fact, these reforms were associated with a surge of temporary jobs.[2] Two-tier labor market policies can also be evaluated in terms of human capital accumulation. On one hand, strong protection and job security may help both employers and permanent workers to invest on firm-specific human capital by reducing concerns about job termination (Kahn, 2012). On the other hand, the extensive protections for permanent workers may induce employers to fill existing permanent jobs with temporary workers, impeding the accumulation of specific human capital (Kahn, 2007).

The extensive use of temporary jobs provoked a debate on whether they are stepping stones to better jobs, which ensure job security and higher wages, or just dead-ends. In countries with fewer employment protection regulations such as the United States and the United Kingdom, temporary jobs seem to play a role as stepping stones to permanent jobs (Booth *et al.*, 2002). In contrast, temporary jobs are less likely to function as stepping stones in the countries such

[1] Employment protection legislation (EPL) includes labor market policies and institutions that regulate or constrain a firm's hiring and firing behaviors. Barone (2001) refers to EPL as the multidimensional regulations that influence a firm's behavior in terms of human resource management. EPL may not be in the form of law, but could result from court rulings or collective bargaining between management and worker groups.

[2] As pointed out in Lee's (1996) study, the surge in temporary employment could be attributed to not only changes in labor market policy that protect permanent workers from the market adjustment but also changes in economic environment (e.g., technological progress and the rapid integration of trade markets).

as Spain where segmented labor markets result from rules providing strict protection for permanent jobs but few restrictions on temporary employment. The segmented labor market consists of core and peripheral sectors: the core sector is filled with permanent jobs that provide high job security and good compensation while jobs in a peripheral sector are characterized by bad working conditions, low job security, and low wages. Amuedo-Dorantes (2000) suggested that temporary work in Spain is more likely to be a dead-end rather than a stepping stone to a permanent job, and argued that Spain's experience could be generalized to other segmented labor markets.

Employment protection regulation in South Korea (below denoted simply as Korea) seems to follow the model of Spain and several other European countries. Since the Asian financial crisis in the late 1990s, the Korean government has tried to increase flexibility in the labor market by allowing the extensive use of temporary jobs while keeping strict protection for permanent workers. As a result, the dual labor market structure solidified in the early 2000s, and the share of temporary employment in wage and salary workers almost doubled from 16.6% in 2001 to 29.4% in 2005 (Grubb *et al.*, 2007). In addition, similar to Spain's case, temporary employment in Korea seems not to function as a stepping stone since the transition rate from temporary to permanent employment over a one-year period was only 11.1% in Korea, while the transition in most European countries was above 50% (OECD, 2013).

The drastic increase in temporary jobs has been pointed out as a main source of social inequality in Korea. Temporary jobs are usually characterized as inferior, as most temporary workers are paid less, are offered less training, and are less satisfied with their jobs (Booth *et al.*, 2002). Thus, a steady increase in the proportion of temporary jobs could lower the welfare for workers and be a source of wage inequality. From workers' perspectives, temporary workers hope to advance to permanent employment through temporary jobs, but they must endure poor labor conditions in the temporary jobs in terms of wage, working time and job security (D'Addio and Rosholm, 2005).

Accordingly, the Korean government proposed a labor market reform in 2007 to lower the incidence of temporary jobs and to encourage employers to convert temporary contracts into permanent ones. The main policy change was to restrict the maximum duration of employment to two years in a job with a fixed-term contract. After the reform, an employer who employed a worker in a fixed-term contract for two years would need to convert the worker's contract from a temporary to permanent one. According to Yoo and Kang (2012) who examined the impact of 2007 Korean reform on the incidence of employment and temporary jobs, the intensity of the reform was weak, and the effect fades

away by two years after the reform. While their analysis focused on the process of job creation – the changes in the probability of being employed and having a temporary job after the reform – I investigate employment dynamics after the policy change. This study relates the change in protection for temporary employment to job sorting mechanisms, and hence it is in line with the research by Boockmann and Hegen (2008) and Marinescu (2009) who focused on a firm's screening process using duration analysis.

In this study, I describe in detail the policy change in the Korean labor market in 2007 first and then consider how firms and workers are expected to react to the policy change. The policy change is expected to induce behavioral changes of both firms and workers on fixed-term contracts. From the perspective of employers, the reform can be thought of as a potential increase in firing costs. Thus, the employers may try to improve the screening process to establish better matches and weed out bad matches prior to the increase in firing costs, which results in better job match quality. From the perspective of workers, workers on fixed-term contracts have an incentive to provide greater effort after the reform because the reform offers a potential path to permanent employment that ensures higher job security and compensation.

According to the results of this study, the probability of job separation decreased in the first five months of tenure after the introduction of the new regulation, which suggests that firms reacted to the policy change by improving their recruitment process, Firms' better recruitment practice can result in well-matched jobs, which can lower the separation probability. However, I cannot find any evidence supporting the view that temporary workers provide greater effort in their jobs after the reform.

This study contributes to the understanding of the consequence of employment protection regulations in several ways. First, it provides evidence on a developing country in Asia that is characterized by a segmented labor market like that in Spain. Thus, we can verify whether Spain's experience can be generalized to another country that has a similar labor market structure. Second, the Korean 2007 reform offers an unusual policy change that increases protection for temporary workers, while previous empirical studies have focused on policy changes for permanent employment or the policies that made it easier for firms to create temporary jobs. Thus, the Korean case can give policymakers insight into the consequences of alternative policy options. Lastly, this study approaches the consequence of employment protection from the perspective of job separations, while previous studies analyze the

effect of employment protection on the net job creation.

II. 2007 labor market reform in Korea and its possible effects on job separation

1. Institutional background of 2007 reform in Korea

Between 1960 and the mid-1990s, Korea experienced rapid economic growth, and benefits from it seemed to be shared with workers through high job security and increased compensation (Sakong, 1993). According to Kang and Yun (2008), the Korean economy experienced not only one of the highest growth rates in the world but also persistent declines in wage inequality between 1980s and the mid-1990s. In addition, most Korean firms adopted seniority-based pay systems at that time, which ensured steady increases in wages given the high job security.

However, as Korea's economic growth slowed following the Asian financial crisis in the late 1990s, the high employment and income security for the workers was pointed out as a source of inefficiency, and firms demanded a more flexible labor market environment. The Korean government responded to the demand by instituting a "two-tier policy" that introduced new forms of employment[3], while keeping strong employment protection for permanent workers. Since then, workers on fixed-term contract – a type of employment contract that terminates at a future date when a specific term expires or when a particular task is completed – has accounted for a majority of the new forms of employment. However, the government did not place any restrictions on the use of fixed-term contracts as there was no restriction on either the duration of contracts or on repeated renewal of fixed-term contracts.

As Lazear (1990) pointed out, employers have an incentive to evade the strict employment protection laws by hiring uncovered (temporary) workers. In most cases, Korean firms set the period of fixed-term contract to less than one year to avoid offering severance pay, which is required by Korean labor law to be given to a worker who has been employed for one year or more.[4] Moreover, firms could renew the fixed-term contract many times with agreement of the worker,

[3] The new forms of employment include workers on fixed-term contract, temporary agency workers, dispatched workers, and atypical workers who are classified as self-employment by labor law but still have many characteristics of employees. The workers in these types of employment are called "non-regular" or "non-standard" employees in Korea.

[4] In Korea, severance pay is based on years of service with a company on a specific contract, and at least one month's wages are provided to the worker for each full year of employment. Firms must offer the severance pay to any salary and wage workers who has worked for one year or more

which allows them to not only continue employing the worker for several years but also terminate the employment effectively without severance pay. Accordingly, many permanent jobs had been replaced with temporary ones in an attempt to reduce labor costs and increase employment flexibility: a Korean government survey showed 32.1% of firms cited reducing labor costs and 30.3% cited increasing employment flexibility as the most important reason for hiring temporary (or *non-regular*) workers (Jones and Urasawa, 2012).

As a result, the share of temporary employment in wage and salary workers almost doubled from 16.6% in 2001 to 29.4% in 2005 (Grubb *et al.*, 2007), which solidified the dual labor market structure in Korea.[5] Recently, the high level of labor market dualism has been pointed out as one of the major factors responsible for rising income inequality because temporary workers are paid about 60% as much as permanent workers (Jones and Urasawa, 2012, 2014). Furthermore, wage of temporary workers are reduced because of their relatively short tenure under the prevalent seniority-based systems in Korea (OECD, 2016).

After five years of discussion with social partners, legislators proposed bills on temporary employment in November 2004 with the goal of lowering the incidence of temporary jobs and preventing firms from using fixed-term contracts as a long-term substitute for permanent workers. Legislation was passed two years later and implemented in 2007. A brief timeline for the legislation and application of the reform is provided in Fig. 1.

Although the legislation enacted several changes to labor market regulations, the primary one is to restrict the length of fixed-term employment with one employer – even employment on successive fixed term contracts – to a maximum of two years.[6] More specifically, after the reform, if an employer chose to continue to employ a worker for more than two years, then the employer had to convert the worker's contract from a fixed-term to permanent one.[7] Of course,

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under a specific employment contract.

[5] The proportion of temporary jobs increased substantially in Korea after the financial crisis in 1997 since not only did the government allow firms to use more flexible employment contracts but also people became desperate for jobs during the severe recession. Holmlund and Storrie (2002) show the incidence of temporary jobs is greatly influenced by macroeconomic conditions and, more importantly, severe recession can cause a surge of temporary jobs by both making not only firms more liable to offer temporary contract but workers more willing to accept them.

[6] Another regulation in the bills was to prohibit discrimination against temporary workers who perform tasks similar to permanent workers in the same firm. According to the new regulation, temporary workers – workers on fixed-term contracts, part-time workers, and temporary agency workers – can submit complaints of discriminatory treatment relating to wages and working conditions to the Korean Labor Relations Commission (Grubb *et al.*, 2007). However, only 2,443 cases affecting 5,262 workers were filed between July 2007 and February 2012 (Jones and Urasawa, 2013), and hence the number of correction orders by the Korean Labor Relations Commission had to be small. Thus, the regulation is considered to have little effect on the labor market.

[7] There are some exceptions in the new regulation, and the following cases are excluded from the application of two-year maximum duration of

employers still had the option to dismiss a worker employed for less than two years on a fixed-term contract with no costs by not renewing the contract. However, if the worker was still on the job at the end of the two-year period, the fixed-term contract would be regarded as a permanent contract. Fig. 2 shows an example of a fixed-term contract after the reform. The reform took effect in July 2007, and hence any fixed-term contracts signed from July 2007 onward are subject to the new regulation.

Fig. 1. A brief timeline for the legislation

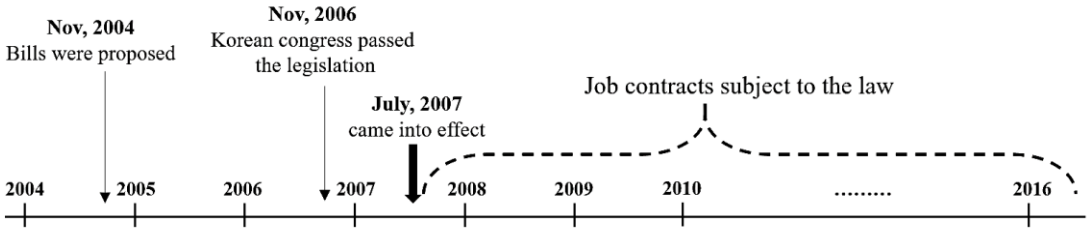
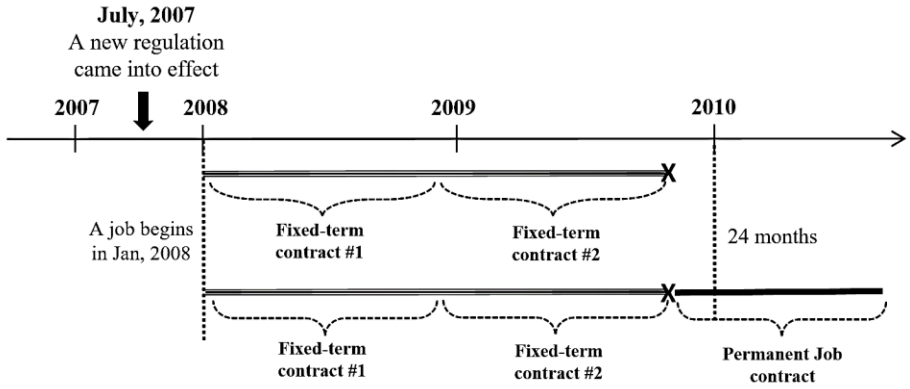


Fig. 2. An example of fixed-term contracts after the reform



Note) ***X*** *denotes a separation from a job.*

fixed-term employment: (i) firms in the private sector with fewer than five employees (ii) workers aged 55 or older at the time of signing a fixed-term contract, (iii) Workers who work less than 15 hours per week regularly (iv) workers holding doctoral degrees or other highly technical and professional qualifications, (v) part-time instructors in tertiary education institutions, (vi) workers subject to other contract duration specified by other laws (Yoo and Kang, 2012).

2. The possible effects of the 2007 reform on the termination of employment

1) Firms' behavioral change

Before the policy change, firms that hired workers on fixed-term contracts had three choices twenty-four months after first hiring a worker on a fixed-term contract: (i) Continue to employ the worker by converting the contract from fixed-term to permanent, (ii) Continue to employ the worker by offering another fixed-term contract, (iii) Dismiss the worker and possibly replace him or her with a new employee. However, since the firms' second option – continue to employ the worker under a fixed-term contract – is no longer available after the reform, the firms must consider alternatives among the other options – (i) and (iii) – depending on the type of the job that has been filled by the fixed-term employment (Fig. 3 summarizes the change in a firm's choice before and after the reform). Here, jobs can be classified according to whether they involve accumulation of firm-specific human capital.

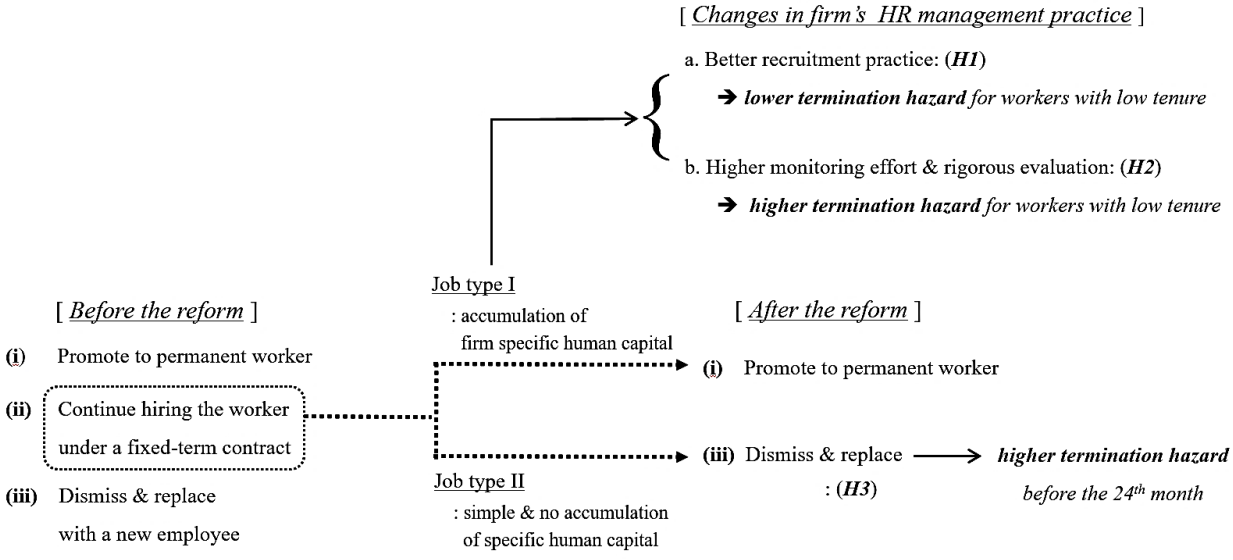
If firm-specific human capital can be accumulated through working on a job, and the employer values it (Job type I), then the firm that would have chosen option (ii) may choose the option (i) after the reform. In this case, firms valuing the accumulation of specific human capital face a discontinuous increase in firing costs after 24 months of employment. Thus, they have an incentive to improve their screening process to establish better job matches and weed out bad matches. For this, firms can change their human resource management practice in two ways.[8] First, at the various stages of the recruitment process, firms can exert greater effort to establish better job matches. For example, they may require strict qualification for a job, review job applications more thoroughly, or filter candidates through in-depth interviews and so improve the quality of job matches. Even though the quality of job matches is difficult to quantify, the result of the change in the quality can be captured by the change in the probability of job separation. Thus, better recruitment practices and higher match quality could cause a decrease in the probability of employment termination – *a recruitment channel (H1)*. The decrease in the probability is expected to be more prominent at very low tenures because most separations from jobs happen in the early stages of working (Marinescu, 2009). Second, firms can exert greater effort in their monitoring and evaluation process to weed out bad matches before the increase

[8] Marinescu (2009) found that increased job security for workers with 1−2 years of tenure can lower the firing hazards for workers with 0−1 year of tenure, which is prior to the period of the increased security. She explained this finding as due to firms' behavioral changes in recruitment and monitoring practices. The reasoning of this study about firms' reaction to the reform is in line with her arguments.

in firing costs. Through higher monitoring effort and rigorous evaluation, the match quality of the remaining jobs may be improved. The result, however, can be represented statistically as an increase in the probability of job separation before 24 months of tenure. Furthermore, since it is better, in the view of the human capital accumulation, to identify bad matches as early as possible and replace unproductive workers with new ones, the increase in the probability is more likely to be observed in the early stages of employment – *a monitoring channel (H2)*.

On the other hand, if a job is simple, and working on it accumulates little specific human capital (Job type II), then firms that have filled the simple job with a temporary worker can replace the worker with another temporary one easily. Then, the firm's best choice after the reform is to initially hire a worker on a fixed-term contract for a period of less than a year, renew that contract, and then dismiss the workers right before his/her tenure reaches 24 months, after which firing costs increase discontinuously. In this case, even though firms may not experience the new employment regulation as an increase in firing costs, their reaction to the regulation could change the probability of job separation. More specifically, job separation hazards may increase right before 24 months of workers' tenure after the reform – *a replacement channel (H3)*.

Fig. 3. A change in firms' options after the reform and its possible effect



Based on the reasoning so far, I suggest three hypotheses on how the reform influences firm behavior and what

changes in the hazards of employment termination are expected after the reform. A summary of the possible effects of the reform is suggested in Fig. 3. In the first part of the empirical analysis, this study investigates evidence that supports each hypothesis and tests the hypotheses using statistical models. In the recruitment *(H1)* and monitoring *(H2)* channels, the direction of reform changes in termination hazards is opposite. Thus, if we can observe either an increase or a decrease in the termination hazards at low tenures, we can tell which hypothesis dominates the other. In addition, the replacement effect *(H3)* can be easily verified by looking at the change in the hazards around 24 months.

2) Workers' behavioral change

Workers' effort in their jobs could be considered an important factor when their employers decide which worker should be kept or weeded out (Booth *et al*., 2002). Because the new regulation requires employers to convert a fixed-term contract to a permanent one when tenure with the employer exceeds two years, workers on fixed-term employment have an incentive to exert greater effort in their job to achieve an advancement to permanent employment, which usually offers high job security and compensation. Putting greater effort into a job may include enduring harsh working conditions, complying with excessive requests from their employer or boss without reasonable compensation, or working overtime voluntarily. If many workers on fixed-term contracts prefer permanent employment and expect the chance of getting converted to permanent employment to be relatively high after the reform, then their higher effort in their jobs could be expressed statistically as a decrease in the termination hazards throughout the duration of a fixed-term employment – *a worker's effort channel (H4)*. Thus, when we investigate the consequence of a new employment regulation, the response of employers as well as employees should be considered. Accordingly, the last hypothesis *(H4)* is added to the possible effects of the reform from the perspective of workers.

In the second part of the analysis, this study seeks to find empirical evidence that supports the view that workers on fixed-term contracts exert greater effort after the reform. Although it is difficult to measure workers' effort, changes in the level of their efforts could be investigated by using a proxy variable. To date, few studies on workers' effort have been conducted in economics, but two studies used similar variables to proxy the level of workers' effort. First, Booth *et al.* (2002) used the number of weekly unpaid overtime hours to proxy the effort and showed high effort increases the probability of exiting from temporary employment only for women. Second, Engellandt and Riphahn

(2005) used a binary variable – whether a worker provides unpaid overtime hours or not – as a proxy for workers' effort levels and confirmed workers on temporary contracts exert significantly greater effort than permanent workers in Switzerland. Following these studies, I use the information on worker's overtime work to proxy the level of effort.

III. Analysis I: Firm's behavioral change after the reform

In the first empirical analysis, this study investigates the effect of the 2007 Korean reform on the probability (or hazard) of employment termination. Through the analysis, I seek to examine how firms' reactions to the reform are reflected in the changes in employment termination hazards.

This study uses the duration (r) in months reaching to the termination of employment as a dependent variable and examines various factors that influence on the duration. The termination of employment includes both voluntary and involuntary separations from jobs. There are some reasons why both kinds of separations are included in the sample. First, all separations may result from the interaction between employers and employees, which is the main concern of this study. Second, an interviewee may choose "voluntary separation" as a reason for his/her job termination, even though he or she was dismissed. Third, although the questionnaire on which our data based has a question asking about a specific reason for job separation, the question response rate is only 56.8%.

1. Data set and sample

To investigate the impact of the 2007 Korean reform on the labor market, this study uses the Korean Labor & Income Panel Study (KLIPS).[9] The KLIPS consists of three data sets, for households, individuals, and job histories. The job history data are composed of observations of jobs (rather than individuals) and contain information on the jobs held by individuals who were surveyed between Jan, 1998 and Aug, 2016. The data offer information on the jobs such as the date at which a job began or terminated (if the job ends before an interview), interview date, type of

[9] KLIPS is a longitudinal survey of the labor market/income activities of households and individuals residing in urban areas. Being the first domestic panel survey on labor-related issues, it has served as a valuable data source for microeconomic analysis concerning labor market activities and transitions. This data set is publicly available on the Korea Labor Institute's website (https://www.kli.re.kr/klips_eng/index.do).

employment (*regular*, *temporary-contract* or *casual*), and other job characteristics (occupation, industry, firm size, sector, average hours of regular or overtime work, wage, etc.). Although KLIPS is a yearly survey and began in 1998, it also asks every interviewee about his/her job history since entering the labor market. Thus, it contains the full records of job history for all respondents. Furthermore, an in-depth analysis is possible when the job history data are combined with the data set of KLIPS for individuals that contains workers' characteristics such as gender, age, education level, marital status, and the area of residence.

Fixed-term employees regulated by the 2007 Korean reform are defined as the workers whose contracts end on a specified date or when a specific task is completed; *temporary-contract* employees according to the classification of KLIPS[10] cover workers whose employment contracts are at least one month and less than one year. Thus, *temporary-contract* employees are a subset of fixed-term employees. In the Korean labor market, however, most fixed-term employees are on *temporary-contracts* because firms set the period of the fixed-term contract to less than one year to avoid offering severance pay, which is required by Korean labor law for contracts of one year or more. Thus, only observations classified as jobs that began with *temporary-contracts* in KLIPS data are selected for the sample to analyze the effect of the reform; other types of employment status – *regular* workers, *casual* workers (or *day laborers*) – are excluded.

The dependent variable (r), which is the tenure of a job in months,[11] is measured by the duration in months from job start date to job end date if a job terminated before the interview. Where a job is still in progress at the last interview date, the dependent variable is measured by the time from job start date to the last interview date and the case is coded as right-censored.

The sample includes only jobs that began from January 2001 and onward. Since the Korean economy had experienced the Asian financial crisis in 1998, the effect of which persisted for several years, the early years of the

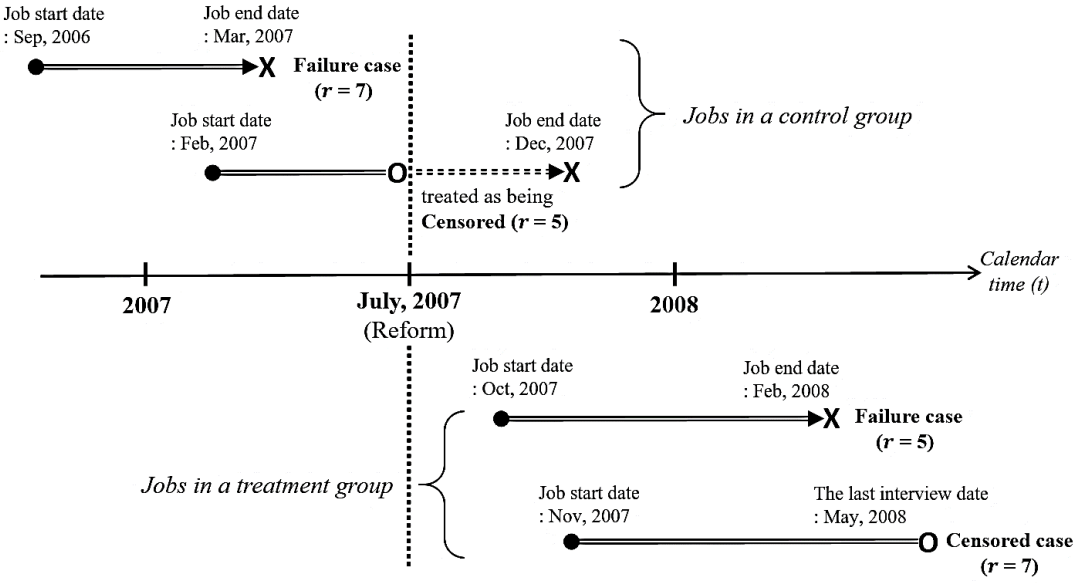
[10] KLIPS classifies salary and wage workers into three groups: 1) *Regular worker*: workers whose employment contract period is at least one year, or workers who can be kept employed as long as he/she wants if their employment contracts are not pre-specified. 2) *Temporary-contract worker*: workers whose employment contracts are at least one month and less than one year, or the workers who expect their job to be terminated within a year if the period of their contracts is not specified in advance. 3) *Casual worker (or day laborer)*: workers whose contract period is less than one month, or the workers who are hired and paid on a daily basis.

[11] The survey asks the exact year, month, and day for the date of job start, job end, and interview. However, about 40% of the respondents didn't answer the exact day of the date. For the purpose of using as many observations as possible, the tenure of a job is measured in months.

survey were still affected by the crisis. To prevent the experience of the crisis from influencing the results of this study, the jobs that began before January 2001 are excluded from the analysis. Thus, the final sample includes only the jobs beginning under *temporary-contracts* that span the period between January 2001 and August 2016.

Lastly, the sample is divided into two parts, a control group – jobs that began before the reform – and a treatment group – jobs that began after the reform. In addition, jobs in the control group that continue beyond the effective date of the regulation (July, 2007) are treated as being censored at the reform's effective date in order to exclude the possibility that the jobs in the control group could also be affected by the introduction of the new regulation and to estimate precisely the change in the termination hazard caused by the reform. Thus, the analysis examines whether the termination hazards of *temporary-contract* employment differ significantly for the control and treatment group. Fig. 4 shows an example of jobs in the control and treatment groups.

Fig. 4. An example of jobs in control and treatment groups



Note) 1. ***X*** *denotes a separation from a job; O means that the observation is right-censored.*

2. ***r*** *stands for the duration of a job in months.*

2. Empirical strategy

As a first step, I estimate the hazard function *h(r)* for the control and treatment groups using the nonparametric

method suggested by Kaplan and Meier. The hazard function for a job is the limiting probability that employment termination occurs right after the tenure of *r* conditional on the job having lasted until *r*:

$$h(r) = \lim_{\Delta r \to 0} \frac{\Pr(r < R < r + \Delta r \mid R > r)}{\Delta r} \qquad \text{(Eq. 1)}$$

The Kaplan-Meier estimate of the hazard function can be represented by (Eq. 2) where n_r is the number of jobs at risk at *r*, and f_r is the number of jobs terminating at *r*. [12]

$$\hat{h}(r) = \frac{f_r}{n_r} \qquad \text{(Eq. 2)}$$

First, jobs under *temporary-contracts* are divided into the control and treatment groups, and the basic statistics for each group are presented in Table 1. Although jobs in the control group began earlier than those in the treatment group, the median value of job tenure is lower for the jobs in the control group since the jobs are treated as being censored at July, 2007. In addition, the treatment group has more jobs since the post-reform period is longer than pre-reform period. However, the proportion of failure and censored cases are almost the same in the control and treatment groups.

Table 1. Basic statistics for the control and treatment group

Jobs under *temporary-contracts*	The number of jobs	The number of failures	The number of censored cases	Median value of job tenure	
				Full sample	Sub-sample : including only uncensored cases
Control group (2001.01~2007.06)	2,236	1,596 (71.4%)	640 (28.6%)	9 months	9 months
Treatment group (2007.07~2016.08)	3,454	2,282 (66.1%)	1,172 (33.9%)	13 months	12 months
Total	5,690	3,878 (68.2%)	1,812 (31.8%)	11 months	10 months

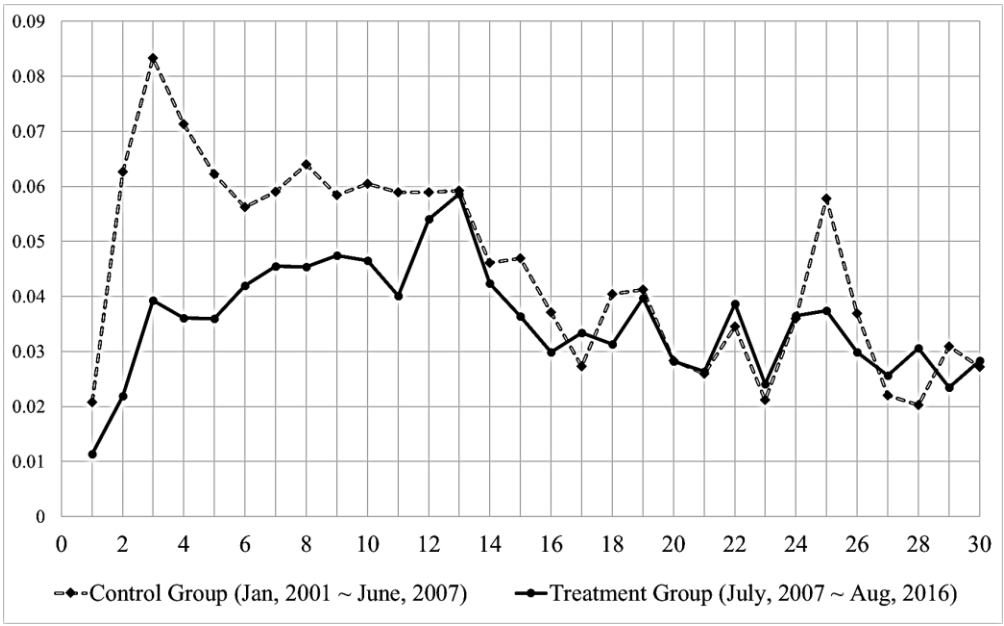
Note) Full sample consists of the total 5,690 jobs; sub-sample contains only failure cases(3,878) in which specific date of job separation is known.

Second, the hazard functions for each group are nonparametrically estimated using (Eq. 2). The detailed hazard

[12] Another way of describing the changes in analysis time *r* is a survivor function, which is the probability that there is no termination of employment prior to the analysis time *r*. The survivor function is simply the reverse cumulative distribution function: *S(r) = 1 – F(r) = Prob(R>r).*
The survivor function can be estimated nonparametrically using Kaplan-Meier's nonparametric version of the survivor function *S(t)* :
$\hat{S}(r) = \prod_{j|r_j \le r}(\frac{n_j - f_j}{n_j}) = \prod_{j|r_j \le r}(1 - \hat{h}(r_j))$ where n_r is the number of jobs at risk at *r*, and f_r is the number of jobs terminated at r.

table is provided in the Appendix (Table A1). Fig. 5 shows the nonparametrically estimated hazard functions for the control and treatment groups visually using the estimates (*h(r)*) in the Table A1. For jobs that started before the reform (the control group), the hazards of employment termination increase drastically at first having a peak at three months and then declines overall,[13] although it shows some fluctuations across tenure, and it has another peak around twenty-five months. However, the shape of the hazard function for the treatment group is different from that for the control group. The termination hazard for the treatment group increases at first, peaks at thirteen months, and declines gradually with some fluctuations. The main difference in the hazard functions can be found in the first eleven months of tenure. The hazard function of the treatment group is much lower than that of the control group in the early portion of the job spells implying that the probability of employment termination decreases substantially for that period.

Fig. 5. Kaplan-Meier hazard estimates for the workers on *temporary-contracts*



[13] The overall shape of the hazard functions for both the control and treatment groups is consistent with the prediction of Jovanovic's (1979a) model. He regards the quality of a job match as an "experience good", which is revealed as firms and workers experience it. The hazard of employment termination is low at the very early stage and then increases as quality is revealed and bad matches are weeded out. However, the hazard declines afterward, since the remaining matches are progressively better. This main prediction from the theoretical model was also confirmed empirically by Faber (1994). Using monthly data, he showed the hazard of a job ending increases to a maximum at 3 months and declines thereafter.

Fig. 6. The difference in hazard estimates between the periods of before and after the reform

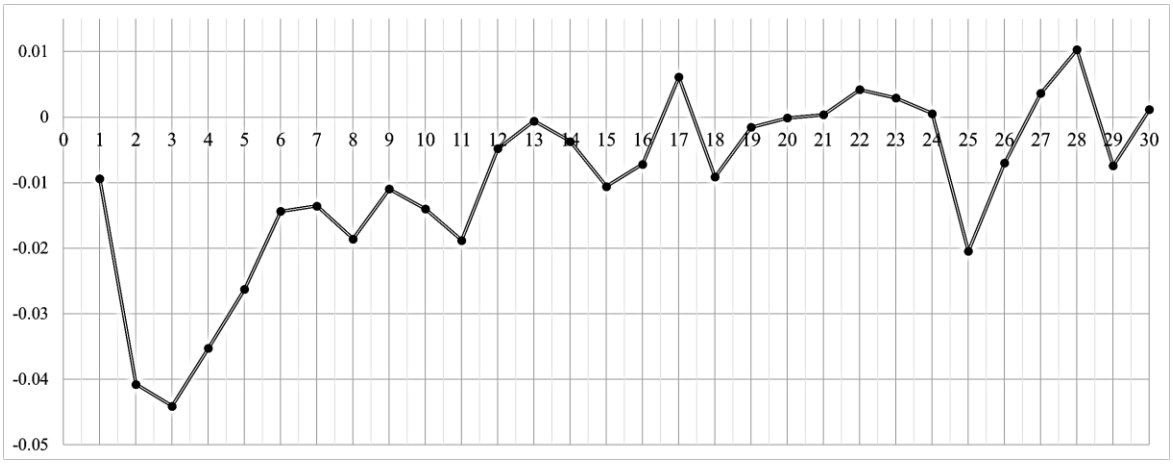


Fig. 6 provides the difference of the hazard rates between the control and treatment group at each month of tenure. It shows large decreases in the hazards in the first eleven months and at twenty-five months after the reform; decreases are relatively large between two to five months. However, the difference is about as likely to be positive as negative after thirteen months. Thus, the main concern of this study is whether the difference in the two hazard functions remains significantly different even after controlling for other variables relevant to the employment. To verify this, I estimate the following Probit model:

- Probit Model

Posit an unobserved latent variable, Y_{it}^{*}, for individual *i* in a job lasting at least *t*, as

$$Y_{it}^{*} = X_{it}\Pi + \sum_{r=2}^{30} \beta_r D_{rit} + \sum_{r=1}^{30} \delta_r \left(D_{rit} \cdot Post\text{-}job_i \right) + \varepsilon_{it} \quad \text{(Eq. 3)}$$

The observed variable, $Y_{it} = 1\{Y_{it}^{*} \geq 0\}$

Here, Y_{it} is a dummy variable indicating whether a job *i* terminated at *t*. $Post\text{-}job_i$ represents a treatment effect that has a value of one when a job began after the reform (July 2007), and D_{rit} is a dummy identifying month of tenure (*r*) for a job. The coefficient, δ_r, of the interaction term captures the effect of the reform on hazards of employment termination at each month of tenure (*r*). In the model, the error term, ε_{it}, is assumed to follow a normal distribution.

The variable X_{it} is a set of controls including worker characteristics (gender, marital status, education level, and age), job characteristics (firm size, occupation, industry, and union membership), and a constant. In addition, the number of previous jobs for a worker is also included in the model to control for worker heterogeneity. In the presence of worker's unobservable heterogeneity, the duration dependence in the probability of job separation cannot be estimated consistently without controlling for the heterogeneity. Farber (1994) proposed to use the information on worker's previous jobs as one way of controlling worker heterogeneity. He showed the frequency of job change prior to the start of the current job has a positive impact on the hazards of job separation. I found a similar result that the hazard is positively related to the number of previous jobs since a worker entered the labor market. The detailed results can be found in the Appendix (Table A2).

In order to control for the macroeconomic conditions upon job separation, I tested various unemployment rates of the previous months, and the average unemployment rate over the last three months gives the most statistically significant result. Moreover, all the coefficients of the previous unemployment rates that I tested show negative signs, and this can be interpreted in terms of workers' incentives. Higher unemployment rates imply that temporary workers have fewer outside opportunities to find better jobs and, accordingly, so higher unemployment increases their interest in remaining in their jobs (Güell and Petrongolo, 2007).

The control variables used in the model are briefly summarized in Table 2, and a detailed explanation can be found in the Appendix (Table A2).

Table 2. Definition of control variables

Variables	Description of variables
· $Female_i$	Female indicator
· $Married_i$	Marital status at the beginning of a job
· $Edu\ level\ j_i$	Indicators for education level *j* where *j*=1, … , 7
· $Age\ k_i$	Indicators for an age category *k* at the beginning of a job where *k*=1, … , 11
· $Previous\ \ job_i$	The number of previous jobs since a worker entered the labor market
· $Unemp\_rate_{it}$	The average unemployment rate over the last three months
· $Firm\ size\ l_i$	Indicators for a firm size *l* where *l*=1, … , 7
· $Occupation\ m_i$	Indicators for an occupation m where *m* = 1, … ,10
· $Industry\ n_i$	Indicators for an industry *n* where *n* = 1, … ,17
· $Union_i$	Indicator for the existence of a labor union in the workplace
· In_union_i	Indicator for labor union membership

3. Results

In the analysis, the duration of a job (r) is restricted to thirty months because there are just a few observations after thirty months of tenure. The full results from the Probit analysis are provided in the Appendix (Table A2), and Table 3 collects only the estimates for the coefficient, δ_r , of the interaction term, which captures the effect of the reform on the hazard of employment termination at each month of tenure (r).

The first section (*No Control*) of Table 3 provides the result from Probit analysis without controlling for the covariates except for D_{rit}, $Post\text{-}job_i$, and the interaction terms. The result is similar to the difference in the hazard functions suggested in Fig. 6. After the reform, the hazards decrease significantly in the first five months, and at eight months. After eleven months, the coefficient has either negative or positive sign, but it is not statistically significant except at twenty-eight months of tenure. The result from the analysis controlling for the covariates is suggested in the second section (*Control*).

The three right columns contain the results estimated by using subsamples, considering the exceptions of the regulation. The legislation allows exceptions in the application of two-year maximum duration of fixed-term employment, and the three subsamples in Table 3 exclude exceptional cases that can be identifiable in the data set: (i) firms in the private sector with fewer than five employees, (ii) workers aged 55 or older at the beginning of a job, (iii) workers who work less than 15 hours per week regularly. [14] Although the sample size decreases by 45% after taking the exceptions (i)-(iii) into account, the results do not change much except that statistical significance of coefficients increase at twelve and twenty-seven months of tenure.

[14] Since job characteristics such as firm size and hours of working could change over time in a job spell, it is not obvious in some jobs whether a worker is excluded from the regulation throughout a job spell. To identify the exceptional cases in terms of firm size and working week, I used information on jobs (firm size and hours of working) at the date of the last interview. As for the age of a worker, a worker who was aged 55 or older at the beginning of a job is considered the exceptional case.

Table 3. Probit analysis for workers on *temporary-contracts*

		Full Sample			Subsamples *excluding exceptions in the regulation*		
		No Control	*Control*		*Control*		
				Average Marginal Effects	*Firm size ≥ 5*	*Firm size ≥ 5 & Age < 55*	*Firm size ≥ 5 & Age < 55 & Working hours ≥ 15*
δ_r : The coefficient of $[D_{rit} \cdot Post\text{-}job_i]$	D_1 * *Post-job*	-0.266***	-0.247***	-0.008	-0.272***	-0.252**	-0.218**
	D_2 * *Post-job*	-0.498***	-0.474***	-0.036	-0.460***	-0.415***	-0.392***
	D_3 * *Post-job*	-0.380***	-0.363***	-0.039	-0.329***	-0.308***	-0.332***
	D_4 * *Post-job*	-0.341***	-0.319***	-0.032	-0.284***	-0.268***	-0.266***
	D_5 * *Post-job*	-0.253***	-0.240***	-0.022	-0.215***	-0.190**	-0.172**
	D_6 * *Post-job*	-0.133*	-0.104	-0.010	-0.046	-0.130	-0.108
	D_7 * *Post-job*	-0.113*	-0.103	-0.010	0.022	-0.116	-0.098
	D_8 * *Post-job*	-0.140**	-0.122*	-0.012	-0.096	-0.139	-0.087
	D_9 * *Post-job*	-0.087	-0.0619	-0.006	-0.040	-0.069	-0.051
	D_{10} * *Post-job*	-0.099	-0.078	-0.008	-0.046	-0.095	-0.043
	D_{11} * *Post-job*	-0.134*	-0.119	-0.011	-0.094	-0.072	-0.064
	D_{12} * *Post-job*	0.005	0.039	0.004	0.162*	0.189*	0.204**
	D_{13} * *Post-job*	0.053	0.079	0.009	0.059	0.147	0.149
	D_{14} * *Post-job*	-0.011	0.006	0.001	-0.051	-0.032	-0.063
	D_{15} * *Post-job*	-0.038	-0.008	-0.001	0.091	0.056	0.069
	D_{16} * *Post-job*	-0.049	-0.012	-0.001	0.084	0.028	0.122
	D_{17} * *Post-job*	0.171	0.200*	0.013	0.237*	0.262	0.253
	D_{18} * *Post-job*	-0.018	0.012	0.001	0.034	0.007	-0.008
	D_{19} * *Post-job*	0.061	0.086	0.007	0.141	0.184	0.164
	D_{20} * *Post-job*	0.148	0.195	0.011	0.212	0.082	0.121
	D_{21} * *Post-job*	0.168	0.196	0.010	0.202	0.253	0.272
	D_{22} * *Post-job*	0.122	0.152	0.012	0.089	0.055	0.026
	D_{23} * *Post-job*	0.099	0.115	0.006	0.003	-0.162	-0.198
	D_{24} * *Post-job*	0.097	0.132	0.010	0.098	0.053	0.087
	D_{25} * *Post-job*	-0.084	-0.080	-0.008	-0.071	-0.122	-0.150
	D_{26} * *Post-job*	0.040	0.053	0.004	0.116	0.270	0.239
	D_{27} * *Post-job*	0.175	0.199	0.010	0.405**	0.629**	0.653***
	D_{28} * *Post-job*	0.278*	0.307*	0.018	0.260	0.411*	0.401*
	D_{29} * *Post-job*	0.080	0.114	0.006	0.011	0.106	0.126
	D_{30} * *Post-job*	0.172	0.194	0.011	0.253	0.240	0.155
Sample size			85,530		64,486	50,413	46,402

Note) 1. *** p<0.01, ** p<0.05, * p<0.1

2. The results without controlling for the covariate are presented under No Control, and the results controlling for the covariate are under Control.

3. The average marginal effect (AME) is the average of conditional marginal effects computed at all sample values. The AME of *Post-job* at r is computed as follows: $AME(Post\text{-}job|D_r = 1) = \frac{1}{n}\sum_{i=1}^{n} ME_i\,(Post\text{-}job|D_r = 1)$ where $ME_i(\cdot) = \hat{P}(y_i = 1 \mid Post\text{-}job = 1,\ D_r = 1,\ D_s = 0,\ \boldsymbol{X_i};\ \hat{\boldsymbol{\Pi}},\ \widehat{\beta_r},\ \widehat{\delta_r}) - \hat{P}(y_i = 1 \mid Post\text{-}job = 0,\ D_r = 1,\ D_s = 0,\ \boldsymbol{X_i};\ \hat{\boldsymbol{\Pi}},\ \widehat{\beta_r},\ \widehat{\delta_r})\ for\ s \neq r.$

4. The exceptions of the regulation that can be identifiable in the data set are as follows: (i) Firms in the private sector with fewer than five employees, (ii) Workers aged 55 or older at the time of signing an employment contract, (iii) Workers who work less than 15 hours per week regularly.

The results can be interpreted as follows: First, even after controlling for covariates, the effect of the reform is still statistically significant in the first five months of tenure. The decrease in the hazards at tenure of less than six months could be evidence supporting the first hypothesis, *(H1) recruitment channel,* or could be interpreted as the recruitment effect of *(H1)* dominates the monitoring effect of *(H2)*. Second, the estimates of the coefficients have positive signs after seventeen months of tenure except at twenty-five months, but they are not statistically significant at the 5% significance level. Thus, the third hypothesis, *(H3) replacement effect*, cannot be accepted, and the reform seems not to influence the termination hazards through replacement channel– the replacement of a worker on fixed-term contract with another one. In conclusion, the results support the effect of the reform on the hazards of job termination only through *(H1) Recruitment channel*.

Table 4. The comparison of hazard estimates and marginal effects at the sample mean

			Duration in month											
			1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*
Difference in K-M hazard estimates			-0.009	-0.041	-0.044	-0.035	-0.026	-0.014	-0.014	-0.019	-0.011	-0.014	-0.019	-0.005
Average Marginal Effects (AMEs)	No Control		-0.010	-0.042	-0.044	-0.036	-0.025	-0.013	-0.012	-0.015	-0.009	-0.010	-0.013	0.001
			(0.004)	*(0.007)*	*(0.008)*	*(0.007)*	*(0.007)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.009)*	*(0.008)*	*(0.009)*
	Control		-0.008	-0.036	-0.039	-0.032	-0.022	-0.010	-0.010	-0.012	-0.006	-0.008	-0.011	0.004
			(0.003)	*(0.005)*	*(0.007)*	*(0.007)*	*(0.006)*	*(0.007)*	*(0.007)*	*(0.007)*	*(0.007)*	*(0.008)*	*(0.008)*	*(0.009)*
		Firm size ≥ 5	-0.009	-0.035	-0.035	-0.027	-0.019	-0.004	0.002	-0.010	-0.004	-0.005	-0.009	0.017
			(0.004)	*(0.006)*	*(0.008)*	*(0.007)*	*(0.007)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.009)*	*(0.009)*	*(0.01)*
		Firm size ≥ 5 Age < 55	-0.009	-0.034	-0.034	-0.027	-0.018	-0.012	-0.011	-0.014	-0.006	-0.010	-0.007	0.022
			(0.004)	*(0.007)*	*(0.009)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.009)*	*(0.009)*	*(0.009)*	*(0.011)*	*(0.01)*	*(0.011)*
		Firm size ≥ 5 Age < 55 Working hr≥15	-0.008	-0.033	-0.037	-0.026	-0.017	-0.010	-0.009	-0.009	-0.005	-0.005	-0.007	0.024
			(0.004)	*(0.008)*	*(0.009)*	*(0.008)*	*(0.009)*	*(0.009)*	*(0.009)*	*(0.01)*	*(0.009)*	*(0.011)*	*(0.011)*	*(0.012)*

Note) 1. Robust standard errors are in parenthesis, which are clustered at the individual worker level and calculated by using the delta-method.

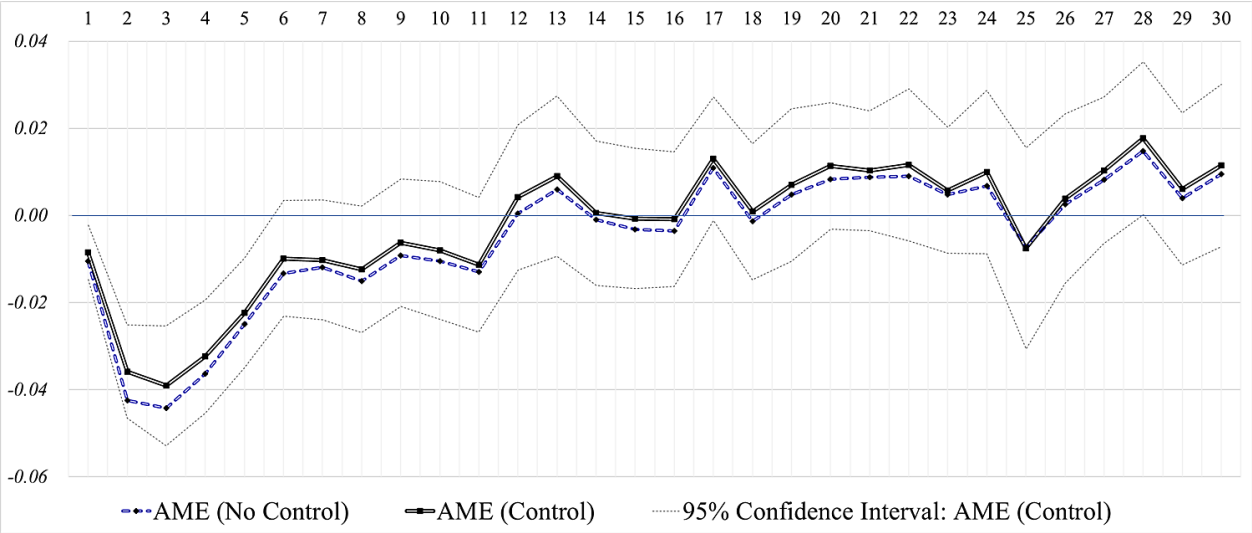
2. The average marginal effect (AME) is the average of conditional marginal effects computed at all sample values.

Table 4 provides the average marginal effects (AMEs) for the first twelve months of tenure to compare those with the results from Kaplan-Meier nonparametric estimation. In addition, Fig. 7 shows visually the AMEs for the interaction term ($D_{rit} \cdot Post\text{-}job_i$) across tenure. Through the comparison of the estimates, it can be confirmed that the

AME from the Probit analysis without controlling for the covariates (No Control) is similar to the difference in Kaplan-Meier hazard estimates in Fig. 6 in terms of the shape of the hazard function. Moreover, although the effect of the reform becomes slightly smaller after controlling for the covariates, it is still statistically significant in the first five months of tenure.

In summary, the probability of job separation decreases in the first five months of tenure after the reform, which suggests that firms react to the policy change by improving their recruitment process. Firms' better recruitment practice can result in well-matched jobs, which can lower the separation probability in several ways: workers with well-matched jobs have less incentive to search for alternative jobs, and they are also less likely to accept outside job offers (Jovanovic, 1979b); we also expect that better matched workers are less likely to be terminated.

Fig. 7. The comparison of average marginal effects (AMEs)



The results can also be interpreted in terms of the firms' purpose of hiring fixed-term employees. Alba-Ramirez (1998) suggested the reasons why firms use temporary employment contracts: first, they use temporary workers to perform temporary work or to avoid the employment rigidity of a permanent contract; second, a temporary contract can also be used as a screening device. In the Korean labor market, temporary jobs have been used mainly as a long-term substitute for permanent ones to reduce labor costs and increase employment flexibility. However, they seem to function as a screening tool as well after the reform improving job matches.

4. Sensitivity and placebo tests

I perform a sensitivity test to examine how estimates of the effect change if a shorter sample period is used for the same Probit model. I use shorter periods, 2001-2013 and 2004-2010, instead of full sample period, 2001-2016, and the results are provided in the Appendix (Table A3). When the shorter sample periods are used, the effect of the reform is still statistically significant in the first five months of tenure, although a point estimate for month seventeen is statistically significant, and estimates for seven later months are significant for the shortest period.

In addition, placebo tests are performed to see how the results look with false reforms, and the results are provided in the Appendix (Table A4). In the first placebo test, I code the data as if the reform occurred in Jan. 2004 or Jan. 2013 (instead of the actual reform in Jul. 2007) and estimate the effects of the two false reforms with the same Probit model. The results show no statistically significant effect of the false reforms in the first five months of tenure, except for a point estimate at five months of tenure when the false reform is set in Jan. 2013.

In the second placebo test, I code the data as if there was a reform effecting permanent workers instead of temporary workers and estimate the effect of the false reform with the same Probit model. To identify permanent workers in the data set, I select *regular* workers who were provided with social insurance by their employers, since they can be thought of as the most protected workers in the Korean labor market. Social insurance in Korea include unemployment insurance, national pension coverage, national health insurance, and industrial accident compensation insurance. The results show no statistically significant effect of the false reform on permanent workers in the first five months of tenure, although positive effects of the false reform are found in month twenty and five later months of tenure.

As neither placebo test finds effect of the false reforms in the first five months of tenure, these results support the view that the actual reform in July 2007 has a casual effect on termination hazards in the early stages of temporary employment.

IV. Analysis II: Workers' behavioral change after the reform

The goal of the second analysis is to test the last hypothesis, (H4). The analysis seeks to find empirical evidence which supports the view that workers on *temporary-contracts* provide greater effort after the reform to obtain advancement to permanent employment. If evidence supports this view, it can be argued that the reform also influences the hazards of employment termination through the channel of workers' effort, which results in decreases in the exit hazard. However, it is difficult to measure the level of workers' effort quantitively. Thus, this study follows previous studies, Booth *et al.* (2002) and Engellandt and Riphahn (2005), which use information on workers' overtime as a proxy for workers' effort.

1. Data set and sample

As above, we use the KLIPS data for individuals to investigate the impact of the 2007 Korean reform on workers' effort. The data set consists of nineteen annual waves, but, as in the previous analyses, the first three waves (1998-2000) of the survey are excluded from the analysis to prevent the experience of the 1998 Asian financial crisis from influencing the results of this study. Thus, the final sample that has a panel structure consists of sixteen waves of the data surveyed between 2001 and 2016. In addition, we exclude casual workers (day laborers), so the sample includes only *regular* workers and *temporary-contract* workers.

This study compares the effort levels of *temporary-contract* workers – those subject to the regulation – and permanent workers – those not subject to the regulation. To identify permanent workers among *regular* workers, we use a criterion whether a worker is provided with social insurance programs from his/her employer in a job since workers covered by the insurance programs can be thought of as the most protected workers in the Korean labor market. Social insurance programs in Korea include unemployment insurance, national pension, national health insurance, and industrial accident compensation insurance. Thus, the final sample consists of selected *regular* workers and *temporary-contract* workers.

The survey offers various information on working hours of wage and salary workers – for example, regular working

hours per week, whether a worker provides overtime hours, the average of weekly (or monthly) overtime hours, and whether the overtime hours is paid or unpaid. Using this information, four dependent variables – two binary variables and two continuous variables – are derived to proxy workers' effort (see Table 5). The binary variables – OT_{it} and UOT_{it} – indicate whether a worker provides overtime hours or not (OT_{it} includes both paid and unpaid overtime; UOT_{it} denotes unpaid overtime). Second two continuous variables – HR_{it} and UHR_{it} – stand for how many hours of overtime a worker provides on average per week (HR_{it} includes both paid and unpaid overtime hours; UHR_{it} denotes the hours of unpaid overtime). The continuous variables – HR_{it} and UHR_{it} – are censored at zero because they have positive values only when workers provide overtime hours and have the value of zero for workers who didn't provide overtime hours.

Table 5. Dependent variables as a proxy for workers' effort

Binary variables	(1)	OT_{it} = 1, if a worker provides overtime hours *(including both paid and unpaid OT)*
	(2)	UOT_{it} = 1, if a worker provides unpaid overtime hours
Continuous variables *(censored at zero)*	(3)	HR_{it} = Average weekly overtime hours *(including both paid and unpaid OT)*
	(4)	UHR_{it} = Average weekly unpaid overtime hours

Descriptive statistics for each dependent variable are provided in the Appendix (Table A5 and A6) in detail. Table 6 provides the proportion of the *regular* or *temporary-contract* workers who provide overtime hours. To see the changes in the proportions visually across the survey years, Fig. 8 plots the proportions at each year.

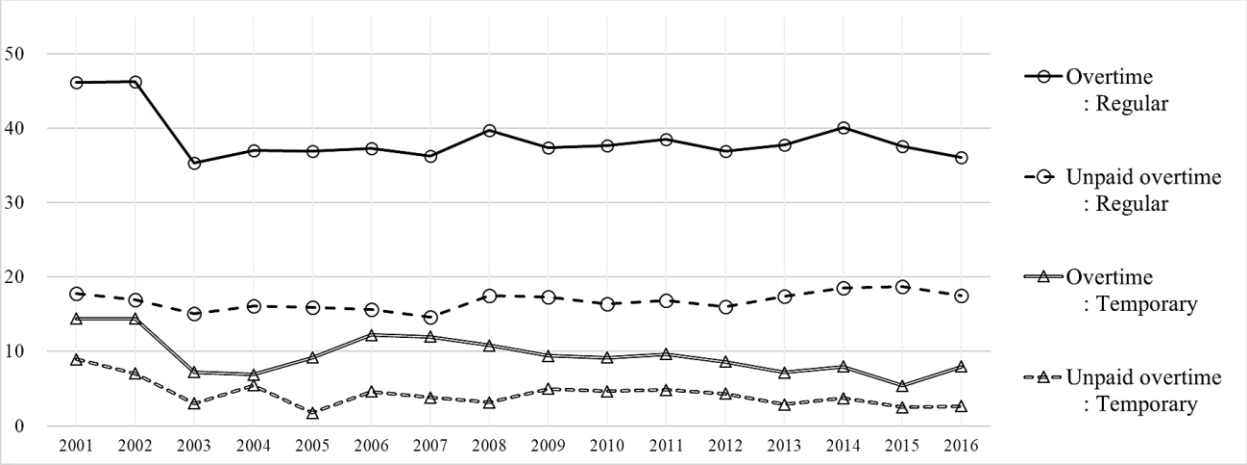
First, the proportion of *regular* workers who work overtime ($OT_{it} = 1$) is much higher than that of *temporary-contract* workers. The proportion for *regular* workers fluctuated around 38%, but that for *temporary-contract* workers seemed to decrease over time. In addition, the difference in the proportions (ΔOT_t) decreased slightly until 2007, but it showed small increases thereafter.

Second, similar patterns are found in the proportion of employees who work unpaid overtime ($UOT_{it} = 1$). The proportion of workers who work overtime without financial compensation was nearly three times higher for *regular* workers in the early survey years. However, the differences in the proportions (ΔUOT_t) became larger in later years due to the decrease in unpaid overtime for *temporary-contract* employees.

Table 6. The proportion of workers providing overtime hours (%)

		2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
OT_{it}= 1, *if providing overtime hours*	*Regular*	46.2	46.2	35.3	37.0	36.9	37.3	36.2	39.7	37.4	37.7	38.5	36.9	37.8	40.1	37.6	36.1
	Temporary	14.4	14.4	7.2	6.9	9.2	12.2	12.0	10.8	9.4	9.2	9.6	8.6	7.2	8.0	5.4	7.9
	Difference (ΔOT_t)	31.8	31.8	28.1	30.1	27.8	25.0	24.3	28.9	28.0	28.5	28.9	28.3	30.6	32.1	32.2	28.1
UOT_{it}= 1, *if providing unpaid overtime hours*	*Regular*	17.8	16.9	15.0	16.1	15.9	15.6	14.6	17.5	17.3	16.4	16.9	16.0	17.4	18.5	18.7	17.5
	Temporary	8.9	7.0	3.0	5.4	1.8	4.6	3.8	3.1	5.0	4.7	4.8	4.3	2.9	3.7	2.5	2.6
	Difference (ΔUOT_t)	8.9	9.9	12.0	10.7	14.1	11.0	10.9	14.4	12.3	11.7	12.0	11.7	14.5	14.8	16.2	14.9

Fig. 8. The proportion of workers providing overtime hours



Third, the two differences – for both overtime (ΔOT_{it}) and unpaid overtime (ΔUOT_{it}) – in the proportions between *regular* and *temporary-contract* workers fluctuated relatively less during the pre-reform period (2001~2006), and the gaps seemed to become larger after the reform. This suggests that the proportions for the *temporary-contract* workers evolved in a similar way as the proportions for the *regular* workers during the pre-reform period. Based on the finding, this study applies a difference-in-differences approach[15] to the analysis for workers' effort. In the setting, the control

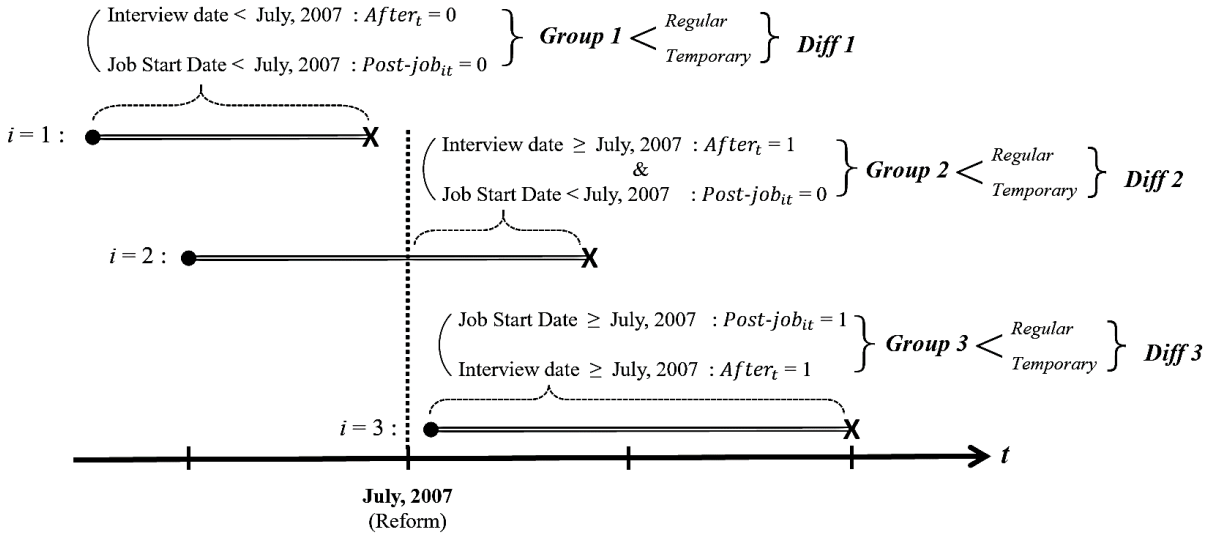
[15] Blundell and Costas Dias (2000) suggest the common trends condition that is crucial for difference-in-differences estimator to be consistent, which means that treatment and control groups respond to macroeconomic shocks in the similar way.

and treatment groups are composed of *regular* workers and *temporary-contract* workers respectively because only the *temporary-contracts* – which are signed from July 2007 and onward – are subject to the new regulation, and there was no significant change in regulations on permanent contracts during the analysis period that would directly influence working hours.

2. Empirical strategy

In order to examine whether workers on *temporary-contracts* ($Temp_{it} = 1$) show greater effort after the reform, observations in the sample are divided into three groups. *Group 1* includes workers who were surveyed before the reform ($After_t = 0$); hence, their jobs had to begin before the reform ($Post\text{-}job_{it} = 0$). *Group 2* consists of workers who started their jobs before the reform ($Post\text{-}job_{it} = 0$) but were surveyed after the reform ($After_t = 1$). *Group 3* has workers whose jobs began after the reform ($Post\text{-}job_{it} = 1$); hence, they had to be surveyed after the reform ($After_t = 1$). Fig. 9 describes the three groups visually.

Fig. 9. Three groups of sample observation and two types of treatment effects



Although only *temporary-contract* workers in Group 3 are subject to the new regulation initially, we test whether

the reform influences the effort levels of *temporary-contract* workers in Group 2 as well as Group 3. For this, two econometric models – (Eq.4) and (Eq.5) – are employed based on a difference-in-differences approach.

- Probit Model

  An unobserved latent variable, Y_{it}^{*}, is assumed to be predicted as follows:

$$Y_{it}^{*} = X_{it}\Pi + \beta_1\, Temp_{it} + \beta_2\, After_t + \beta_3\, Post\text{-}job_{it} + \beta_4\,[Temp_{it} \cdot After_t] + \beta_5\,[Temp_{it} \cdot Post\text{-}job_{it}] + \varepsilon_{it}$$

$$where\ Y_{it}^{*} = \ OT_{it}^{*} \ \text{or} \ UOT_{it}^{*} \qquad \text{(Eq. 4)}$$

  The observed variable, $Y_{it} = 1\{Y_{it}^{*} \geq 0\}$, $\quad where\ Y_{it} = \ OT_{it} \ \text{or} \ UOT_{it}$

In the specification of the models, two interaction terms – '$Temp_{it} \cdot After_t$' and '$Temp_{it} \cdot Post\text{-}job_{it}$' – are included to examine the two types of treatment effects. The coefficient, β_4, of the first interaction term captures the first treatment effect – whether the *temporary-contract* employees in the *Group 2* provide greater effort after the reform relative to the *regular* workers ($\beta_4 = Diff_2 - Diff_1$). The coefficient, β_5, of the second interaction term compares the effort levels of the *temporary-contract* workers in *Groups 2* and *3*. Thus, β_5 captures the additional treatment effect for the observations that were surveyed after the reform, where a job contract was also made after the reform ($\beta_5 = Diff_3 - Diff_2$).

This model implies a Probit model for the binary dependent variables (*OTit* and *UOTit*); for the continuous dependent variables (*HRit* and *UHRit*), a Tobit model is used to deal with the censoring issue. In both models, the error term, ε_{it}, is assumed to follow a normal distribution.

- Tobit Model

  An unobserved latent variable, Y_{it}^{*} :

$$Y_{it}^{*} = X_{it}\Pi + \beta_1\, Temp_{it} + \beta_2\, After_t + \beta_3\, Post\text{-}job_{it} + \beta_4\left[Temp_{it} \cdot After_t\right] + \beta_5\left[Temp_{it} \cdot Post\text{-}job_{it}\right] + \varepsilon_{it}$$

$$where\ Y_{it}^{*} = \ HR_{it}^{*} \ \text{or} \ UHR_{it}^{*} \qquad \text{(Eq. 5)}$$

  The observed variable, $Y_{it} = \max\{0,\ Y_{it}^{*}\}$, $\quad where\ Y_{it} = \ HR_{it} \ \text{or} \ UHR_{it}$

In the models, the variable X_{it} is a set of controls including worker characteristics (gender, marital status, education level, age, and tenure) and job characteristics (sector, union membership, firm size, occupation, and industry). Moreover, in order to capture the time effect and the macro economic conditions in the survey year, a linear time trend and its square term, employment rates, and unemployment rates are controlled. The control variables used in the model are briefly summarized in Table 7, and a detailed explanation can be found in the Appendix (Table A7).

Table 7. Definition of control variables

· $Female_i$	Female indicator
· $Married_{it}$	Marital status
· $Female_i$ x $Married_{it}$	An interaction term of Female and Married dummies
· $Edu\ level\ j_{it}$	Indicators for education level j where j=1, … , 7
· $Age\ k_{it}$	Indicators for an age category k where k=1, … , 11
· $Tenure\ l_{it}$	Indicators for an tenure category l where l=1, … , 8
· $Private\ sector_{it}$	Indicator for a private sector
· $Union_{it}$	Indicator for the existence of a labor union in the workplace
· In_union_{it}	Indicator for labor union membership
· $Firm\ size\ m_{it}$	Indicators for a firm size m where m=1, … , 8
· $Occupation\ n_{it}$	Indicators for an occupation n where n = 1, … ,11
· $Industry\ \ s_{it}$	Indicators for an industry s where s = 1, … ,18
· $Year_t$	Calendar time effect with a linear trend and its square term
· Emp_rate_t	The employment rate in each year
· $Unemp\_rate_t$	The unemployment rate for the people of age 15 to 64 in each year

3. Results

Full results estimated by the Probit and Tobit models are provided in the Appendix (Table A7). Table 8 contains only the four coefficients of interest and the conditional average marginal effects. The two sections, (1) and (2), show the results for the binary dependent variables, OT_{it} and UOT_{it}. First, *temporary-contract* workers ($Temp$) are less likely to work overtime and unpaid overtime compared to *regular* workers.[16] The estimated average marginal effect

[16] This result is contrary to the main finding of Engellandt and Riphahn (2005). They showed empirically the likelihood of working unpaid overtime

implies a 11 percentage point difference in overtime work and 3.9 percentage point difference in unpaid overtime work.[17] Second, the likelihood that *regular* workers work overtime increased by 3.3 percentage points in the period of post-reform ($After$), but there is no statistically significant increase in the likelihood of unpaid overtime. Third, whether a job contract was made before or after the reform ($Post\text{-}job$) has a statistically significant impact on neither overtime or unpaid overtime work for *regular* workers. Fourth, *temporary-contract* workers in *Group 2* seem not to work more overtime or unpaid overtime after the reform ($Temp \cdot After$) compared to *regular* workers. Fifth, there is also no statistically significant diffrence in the likelihood of providing overtime or unpaid overtime work between the *temporary-contract* workers in *Groups 2* and *3* ($Temp \cdot Post\text{-}job$). In sum, I cannot find any evidence that *temporary-contract* workers are more likely to work overtime or unpaid overtime after the reform compared to *regular* workers.

The last two sections, (3) and (4), provide the results for workers' average weekly overtime and unpaid overtime hours, HR_{it} and UHR_{it}, which were estimated with a Tobit model. The results are similar to those in the first two columns. First, *temporary-contract* workers are likely to provide fewer overtime and unpaid overtime hours than *regular* workers: *temporary-contract* employees work about 4.7 fewer hours overtime and about 3.1 fewer unpaid hours overtime in a week than *regular* workers. Second, *regular* workers work overtime about 1.4 hours more in a week after the reform, but there is not statistically significant increase in the hours of unpaid overtime. Third, for *regular* workers, the average weekly hours of overtime and unpaid overtime seem not to depend on whether a job contract was entered into before or after the introduction of the new regulation. Fourth, for the *temporary-contract* workers in *Group 2* and *Group 3*, the reform has no significant impact on the average weekly hours of overtime and unpaid overtime.

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is much higher for temporary workers than permanent ones in Switzerland. They argued that temporary workers have more incentive to exert greater effort because temporary contracts function as a screening tool and provide stepping stones into permanent employment in Switzerland. On the other hand, Landers *et al*. (1996) and Booth *et al*. (2003) suggested permanent workers have incentives to prove they are hardworking. In Korea, it seems that both higher promotion incentives for permanent workers and limited advancement from temporary to permanent employment result in longer (unpaid) overtime hours and higher chance of working (unpaid) overtime for permanent workers.

[17] The conditional average marginal effect (AME) is computed by averaging conditional marginal effect (ME) for each observation *i* over all sample values. For example, the conditional AME of *temporary-contract* (*Temp*) is computed as follows: $AME(Temp|After = 0,\ Post\text{-job} = 0) = \frac{1}{n}\sum_{i=1}^{n} ME_i\,(Temp|After = 0,\ Post\text{-job} = 0)$ where $ME_i(\cdot) = \hat{P}(y_i = 1 \mid Temp = 1,\ After = 0,\ \text{Post-job} = 0,\ \boldsymbol{X_i};\ \widehat{\boldsymbol{\beta}},\ \widehat{\boldsymbol{\pi}}) - \hat{P}(y_i = 1 \mid Temp = 0,\ After = 0,\ Post\text{-}job = 0,\ \boldsymbol{X_i};\ \widehat{\boldsymbol{\beta}},\ \widehat{\boldsymbol{\pi}})$. Similarly, the conditional AME of *After* and *Post-job* stand for $AME(After|Temp = 0,\ Post\text{-job} = 0) = \frac{1}{n}\sum_{i=1}^{n} ME_i\,(After|Temp = 0,\ Post\text{-job} = 0)$ and $AME(Post\text{-job}|Temp = 0,\ After = 1) = \frac{1}{n}\sum_{i=1}^{n} ME_i\,(Post\text{-job}|\text{Temp} = 0,\ After = 1)$ respectively.

Table 8. The effect of the reform on workers' overtime work

	Dependent variables					
	(1) *OTi* = 1, if providing overtime work *(paid & unpaid)*		(2) *UOTi* = 1, if providing unpaid overtime work		(3) *HRi* = average weekly overtime hours *(paid & unpaid)*	(4) *UHRi* = average weekly hours of unpaid overtime
Variables	Probit	*Average Marginal Effect*	Probit	*Average Marginal Effect*	Tobit	Tobit
Temp : β_1 *(Temporary-contract)*	–0.396*** (0.053)	–0.110	–0.215*** (0.065)	–0.039	–4.768*** (0.620)	–3.152*** (0.934)
After : β_2 *(After = 1, if surveyed after July, 2007)*	0.106*** (0.036)	0.033	0.044 (0.044)	0.009	1.484*** (0.389)	0.755 (0.596)
Post-job : β_3 *(Post-job = 1, if a job began after July, 2007)*	0.058 (0.037)	0.018	0.031 (0.041)	0.006	0.451 (0.403)	0.544 (0.573)
Temp x After : β_4	0.031 (0.093)	0.002	–0.017 (0.111)	–0.005	0.201 (1.107)	–0.186 (1.602)
Temp x Post-job : β_5	–0.128 (0.094)	–0.037	–0.142 (0.109)	–0.024	–1.097 (1.105)	–1.570 (1.553)
The Number of Observations	37,835					

Note) 1. Robust standard errors are in parenthesis, which are clustered at the individual level (the number of individuals: 8,497).

2. The average marginal effect is the average of conditional marginal effects computed at all sample values.

3. *** p<0.01, ** p<0.05, * p<0.1

In summary, the results do not support the view that *temporary-contract* workers work overtime more after the reform relative to permanent workers (or selected *regular* workers). If workers' effort levels in their jobs are captured by overtime work (or the hours of overtime work), then the results do not suggest any evidence that supports the last hypothesis *(H4)* – workers on *temporary-contract* provide greater effort after the reform. Hence, there is no evidence that greater worker effort produces the decrease in the hazards of employment termination that we observe in the first five months on the job. Thus, the hypothesis *(H4)* is not supported, and I conclude that there is no change in the hazards of employment termination through the channel of workers' effort.

V. Conclusion

This study analyzes how firms and workers respond to increased protection for temporary employment. In 2007, the Korean government introduced a new regulation that restricts the length of fixed-term employment with an employer to a maximum of two years. After the policy change, an employer who employed a worker in a fixed-term contract for two years would need to convert the worker's contract from a fixed-term to a permanent one.

First, from the perspective of employers, the new regulation can be thought of as a potential increase in firing costs for temporary workers. Thus, the employers may try to improve the screening process to establish better matches and weed out bad matches prior to the increase in firing costs, which results in better job match quality. In order to test hypotheses on firms' and workers' behavioral change, this study employs survival analysis that investigates the change in the probability of employment termination after the reform. The results show statistically significant decreases in the probability of job separation in the first five months of tenure after the policy change, which implies that firms respond to the strict protection for temporary workers by improving their recruitment practices.

Second, temporary workers have an incentive to provide greater effort after the policy change because the reform offers a potential path to permanent employment that ensures higher job security and compensation. Moreover, the greater effort in their jobs can result in a decrease in the probability of employment termination. This study uses information on workers' overtime as a proxy for worker's effort since it is difficult to measure the level of workers' effort. However, the results provide no evidence supporting the view that temporary employees are more likely to work overtime after the policy change. Thus, if workers' effort levels in their jobs are captured by overtime work, our results do not confirm the hypothesis that strict protection for temporary workers can decrease the probability of employment termination through the increase in the level of workers' effort.

In conclusion, the increased protection for temporary workers through the reform induces employers to improve recruitment process, which results in better-matched jobs. Temporary jobs in the Korean labor market have been used mainly as a long-term substitute for permanent ones to reduce labor costs and increase employment flexibility. However, they seem to function as a screening tool as well after the reform, improving job matches.

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Appendix

Table A1. Kaplan-Meier nonparametric estimates for the hazard function *h(r)*

Control Group (Before the reform : Jan, 2001 ~ June, 2007)							**Treatment Group** (After the reform : July, 2007 ~ Aug, 2016)							**Difference in *h(r)***	
r	***n(r)***	***f(r)***	***c(r)***	***h(r)***	***1-h(r)***	***S(r)***	***r***	***n(r)***	***f(r)***	***c(r)***	***h(r)***	***1-h(r)***	***S(r)***	***r***	***Δh(r)***
1	2314	48	31	0.021	0.979	0.979	1	3438	39	19	0.011	0.989	0.989	1	-0.009
2	2235	140	31	0.063	0.937	0.918	2	3380	74	46	0.022	0.978	0.967	2	-0.041
3	2064	172	27	0.083	0.917	0.841	3	3260	128	55	0.039	0.961	0.929	3	-0.044
4	1865	133	60	0.071	0.929	0.781	4	3077	111	44	0.036	0.964	0.896	4	-0.035
5	1672	104	22	0.062	0.938	0.733	5	2922	105	51	0.036	0.964	0.863	5	-0.026
6	1546	87	20	0.056	0.944	0.692	6	2766	116	36	0.042	0.958	0.827	6	-0.014
7	1439	85	10	0.059	0.941	0.651	7	2614	119	29	0.046	0.954	0.789	7	-0.014
8	1344	86	7	0.064	0.936	0.609	8	2466	112	33	0.045	0.955	0.754	8	-0.019
9	1251	73	21	0.058	0.942	0.574	9	2321	110	17	0.047	0.953	0.718	9	-0.011
10	1157	70	18	0.061	0.939	0.539	10	2194	102	25	0.046	0.954	0.685	10	-0.014
11	1069	63	4	0.059	0.941	0.507	11	2067	83	25	0.040	0.960	0.657	11	-0.019
12	1002	59	14	0.059	0.941	0.477	12	1959	106	28	0.054	0.946	0.621	12	-0.005
13	929	55	7	0.059	0.941	0.449	13	1825	107	21	0.059	0.941	0.585	13	-0.001
14	867	40	17	0.046	0.954	0.428	14	1697	72	30	0.042	0.958	0.560	14	-0.004
15	810	38	17	0.047	0.953	0.408	15	1595	58	33	0.036	0.964	0.540	15	-0.011
16	755	28	31	0.037	0.963	0.393	16	1504	45	23	0.030	0.970	0.524	16	-0.007
17	696	19	9	0.027	0.973	0.382	17	1436	48	14	0.033	0.967	0.506	17	0.006
18	668	27	11	0.040	0.960	0.367	18	1374	43	22	0.031	0.969	0.490	18	-0.009
19	630	26	4	0.041	0.959	0.352	19	1309	52	17	0.040	0.960	0.471	19	-0.002
20	600	17	5	0.028	0.972	0.342	20	1240	35	27	0.028	0.972	0.458	20	0.000
21	578	15	12	0.026	0.974	0.333	21	1178	31	10	0.026	0.974	0.446	21	0.000
22	551	19	12	0.034	0.966	0.321	22	1137	44	13	0.039	0.961	0.428	22	0.004
23	520	11	8	0.021	0.979	0.315	23	1080	26	13	0.024	0.976	0.418	23	0.003
24	501	18	16	0.036	0.964	0.303	24	1041	38	13	0.037	0.963	0.403	24	0.001
25	467	27	6	0.058	0.942	0.286	25	990	37	16	0.037	0.963	0.388	25	-0.020
26	434	16	9	0.037	0.963	0.275	26	937	28	12	0.030	0.970	0.376	26	-0.007
27	409	9	6	0.022	0.978	0.269	27	897	23	24	0.026	0.974	0.366	27	0.004
28	394	8	30	0.020	0.980	0.264	28	850	26	15	0.031	0.969	0.355	28	0.010
29	356	11	14	0.031	0.969	0.256	29	809	19	14	0.023	0.977	0.347	29	-0.007
30	331	9	10	0.027	0.973	0.249	30	776	22	14	0.028	0.972	0.337	30	0.001

Note) 1. ***r*** *stands for the duration of a job in months.*

2. n(r) is the number of jobs at risk at r; f(r) is the number of jobs terminated at r; c(r) is the number of jobs censored at r.

3. h(r) and S(r) is Kaplan-Meier estimates of the hazard and survivor functions respectively. Δh(r) denotes the difference in the hazard estimates between the control and treatment groups.

Table A2. The effect of the reform on the hazards of job separation: Probit analysis

VARIABLES	Full sample	Sub samples (*excluding exceptions in the regulation*)		
		Firm size≥5	Firm size≥5 Age < 55	Firm size≥5 Age < 55 Working hours≥15
dt dummies (t = 2 … 30)	*Yes*	*Yes*	*Yes*	*Yes*
*dt * Post-job* dummies (t = 1 … 30)	*Yes*	*Yes*	*Yes*	*Yes*
Female	-0.025	-0.035	-0.018	-0.025
Marriage status (1 = Married)	-0.088***	-0.109***	-0.161***	-0.137***
Female * Married	0.074**	0.078*	0.097**	0.076
Education Level (base*: High school*)				
edu level 1: Less than elementary school	-0.005	0.020	0.004	0.006
edu level 2: Middle school	-0.016	-0.002	0.042	0.045
edu level 4: College (2 years)	0.096***	0.084***	0.093***	0.095***
edu level 5: University (4 years)	0.080**	0.085**	0.101***	0.112***
edu level 6: Master degree	0.054	0.049	0.062	0.107
edu level 7: Ph.D	0.065	0.019	-0.020	0.349*
Age Cohort (base: *age 25-29*)				
age cohort1: 15-19	0.416***	0.448***	0.432***	0.442***
age cohort2: 20-24	0.196***	0.188***	0.184***	0.180***
age cohort4: 30-34	-0.028	-0.051	-0.044	-0.046
age cohort5: 35-39	-0.064*	-0.054	-0.043	-0.046
age cohort6: 40-44	-0.122***	-0.163***	-0.155***	-0.135***
age cohort7: 45-49	-0.125***	-0.160***	-0.156***	-0.125**
age cohort8: 50-54	-0.105**	-0.097*	-0.097*	-0.091*
age cohort9: 55-59	-0.123**	-0.118**	–	–
age cohort10: 60-64	-0.104**	-0.141**	–	–
age cohort11: 65-85	-0.116**	-0.127**	–	–
The number of previous jobs	0.021***	0.023***	0.024***	0.024***
The average unemployment rate over the last three months	-0.192***	-0.189***	-0.153***	-0.143***
Private Sector	0.013	-0.003	0.016	0.012
Part-time job	-0.001	-0.002	0.009	0.040
Firm size (base: 1-9 people)				
10-29 people	-0.0139	-0.006	0.005	0.001
30-99 people	0.016	0.021	0.050	0.047
100-299 people	-0.080*	-0.076	-0.078	-0.086
300-999 people	-0.030	-0.028	-0.037	-0.008
1,000 or more than 1,000 people	-0.009	-0.008	0.001	-0.002
Unknown	0.063**	0.062*	0.106***	0.124***

Table A2. The effect of the reform on the hazards of job separation: Probit analysis *(Continued)*

VARIABLES	Full sample	Sub samples (*excluding exceptions in the regulation*)		
		Firm size≥5	Firm size≥5 Age < 55	Firm size≥5 Age < 55 Working hours≥15
(Continued)				
Union in the workplace	-0.099**	-0.100**	-0.119**	-0.121**
Union membership	-0.120	-0.133	-0.187*	-0.187*
Occupation (base: *Office clerk*)				
Legislative officers, executives, and high managers	-0.036	0.026	0.045	0.054
Professional	-0.103**	-0.098**	-0.102**	-0.095*
Engineers and semi-professional	-0.080**	-0.097**	-0.090*	-0.073
Service workers	0.010	-0.004	0.020	0.021
Sales force	0.030	0.073	0.074	0.076
Agriculture and fishery workers	-0.060	-0.132	-0.381**	-0.400**
Technicians	0.029	0.035	0.021	0.024
Equipment, machine, and assembly workers	0.002	0.008	0.043	0.049
Unskilled workers	0.081**	0.084**	0.092**	0.096**
unknown	0.251	0.278	0.293	0.292
Industry (base: *Manufacturing*)				
Agriculture, hunting, forestry, and fishery	-0.055	0.014	0.181	0.184
Mining	-0.071	-0.330	–	–
Electricity, gas, and water	0.180	0.187	0.278*	0.296**
Construction	-0.106**	-0.073	-0.067	-0.070
Wholesale and retail	-0.079*	-0.093**	-0.097*	-0.097*
Lodge and food	-0.009	-0.012	-0.0254	-0.025
Transportation and communication service	-0.117**	-0.118**	-0.081	-0.080
Banking and insurance	-0.130**	-0.115*	-0.101	-0.103
Real estate and leasing service	-0.142**	-0.207***	0.048	0.041
Business service	-0.181***	-0.173***	-0.104*	-0.100*
Public, social security, and defense administration	0.125**	0.103*	0.063	0.066
Education service	-0.144***	-0.143***	-0.140**	-0.119**
Health and social welfare service	-0.080*	-0.098*	-0.103*	-0.105*
Entertainment, broadcasting, and performance	-0.025	-0.033	-0.076	-0.067
Other individual service	-0.056	-0.052	-0.001	-0.004
Other association, organization, and international agency	-0.188**	-0.106	-0.129	-0.114
Unknown	-0.326**	-0.378**	-0.357**	-0.358**
Constant	-1.507***	-1.466***	-1.625***	-1.691***
Observations	85,530	64,486	50,413	46,402

Note) 1. Standard errors are clustered at a worker level.

*2. *** p<0.01, ** p<0.05, * p<0.1*

Table A3. Sensitivity test: Probit analysis with various sample periods

Sample period		2001 - 2016 *(Full sample)*	2001 - 2013	2004 - 2010	2001 - 2016	2001 - 2013	2004 - 2010
					Excluding Jan. – Dec. 2007		
	Before	*Jan. 2001 – Jun. 2007*	*Jan. 2001 – Jun. 2007*	*Jan. 2004 – Jun. 2007*	*Jan. 2001 – Dec. 2006*	*Jan. 2001 – Dec. 2006*	*Jan. 2004 – Dec. 2006*
	After	*July. 2007 – Dec. 2016*	*July. 2007 – Dec. 2013*	*July. 2007 – Dec. 2010*	*Jan. 2008 – Dec. 2016*	*Jan. 2008 – Dec. 2013*	*Jan. 2008 – Dec. 2010*
δ_r : The coefficient of $[D_{rit} \cdot Post\text{-}job_i]$	D_1 * *Post-job*	-0.247***	-0.211**	-0.232*	-0.273***	-0.242**	-0.292**
	D_2 * *Post-job*	-0.474***	-0.449***	-0.374***	-0.512***	-0.496***	-0.444***
	D_3 * *Post-job*	-0.363***	-0.311***	-0.285***	-0.422***	-0.373***	-0.388***
	D_4 * *Post-job*	-0.319***	-0.296***	-0.222**	-0.357***	-0.338***	-0.280***
	D_5 * *Post-job*	-0.240***	-0.222***	-0.215**	-0.298***	-0.284***	-0.319***
	D_6 * *Post-job*	-0.104	-0.059	-0.087	-0.166**	-0.122	-0.188*
	D_7 * *Post-job*	-0.103	-0.049	0.021	-0.167**	-0.110	-0.073
	D_8 * *Post-job*	-0.122*	-0.081	0.066	-0.197***	-0.155**	-0.044
	D_9 * *Post-job*	-0.0619	-0.087	0.102	-0.132*	-0.156**	0.007
	D_{10} * *Post-job*	-0.078	-0.135	-0.106	-0.148*	-0.206**	-0.210*
	D_{11} * *Post-job*	-0.119	-0.117	-0.081	-0.194**	-0.192**	-0.192
	D_{12} * *Post-job*	0.039	0.013	0.143	-0.039	-0.069	0.024
	D_{13} * *Post-job*	0.079	0.075	0.241**	-0.014	-0.025	0.095
	D_{14} * *Post-job*	0.006	0.028	0.215	-0.069	-0.049	0.105
	D_{15} * *Post-job*	-0.008	0.026	0.175	-0.104	-0.074	0.027
	D_{16} * *Post-job*	-0.012	0.005	0.134	-0.075	-0.058	0.056
	D_{17} * *Post-job*	0.200*	0.241**	0.464***	0.139	0.181	0.396**
	D_{18} * *Post-job*	0.012	0.008	0.100	-0.068	-0.075	-0.012
	D_{19} * *Post-job*	0.086	0.084	0.182	-0.001	-0.005	0.058
	D_{20} * *Post-job*	0.195	0.243*	0.539***	0.136	0.187	0.482**
	D_{21} * *Post-job*	0.196	0.193	0.327*	0.129	0.127	0.252
	D_{22} * *Post-job*	0.152	0.129	0.184	0.086	0.062	0.103
	D_{23} * *Post-job*	0.115	0.146	0.253	0.068	0.101	0.210
	D_{24} * *Post-job*	0.132	0.158	0.359*	0.070	0.095	0.290
	D_{25} * *Post-job*	-0.080	-0.045	0.387**	-0.138	-0.103	0.333*
	D_{26} * *Post-job*	0.053	0.040	0.593**	-0.006	-0.023	0.527**
	D_{27} * *Post-job*	0.199	0.190	0.091	0.103	0.086	-0.111
	D_{28} * *Post-job*	0.307*	0.309*	0.510**	0.204	0.202	0.350
	D_{29} * *Post-job*	0.114	0.127	0.273	0.052	0.064	0.169
	D_{30} * *Post-job*	0.194	0.204	0.809**	0.131	0.140	0.724*
Sample size		85,530	74,874	40,933	80,050	69,394	35,453

Note) 1. Standard errors are clustered at a worker level.

*2. *** p<0.01, ** p<0.05, * p<0.1*

Table A4. Placebo test: Probit analysis with false reforms

		Placebo test I: False reforms on *temporary-contract* workers		**Placebo test II:** A false reform on selected *regular* worker groups	
		A false reform in Jan. 2004	*A false reform in Jan. 2013*	*Regular workers covered by social insurance*[1]	*excluding exceptions of the regulation*[2]
Sample period		2001 - 2006	2010 - 2015	2001 - 2013	
	Before	*Jan. 2001 – Dec. 2003*	*Jan. 2010 – Dec. 2012*	*Jan. 2001 – Jun. 2007*	
	Reform	*A reform in Jan. 2004*	*A reform in Jan. 2013*	*A reform in Jul. 2007*	
	After	*Jan. 2004 – Dec. 2006*	*Jan. 2013 – Dec. 2015*	*Jul. 2007 – Dec. 2013*	
δ_r : The coefficient of $[D_{rit} \cdot Post\text{-}job_i]$	D_1 * *Post-job*	0.095	-0.028	-0.105	-0.010
	D_2 * *Post-job*	0.092	0.120	0.032	-0.021
	D_3 * *Post-job*	0.143	-0.139	-0.255*	-0.205
	D_4 * *Post-job*	-0.027	-0.035	0.013	0.042
	D_5 * *Post-job*	0.039	-0.231**	-0.181	-0.219
	D_6 * *Post-job*	0.162	-0.192*	-0.039	-0.054
	D_7 * *Post-job*	-0.019	-0.069	-0.147	-0.137
	D_8 * *Post-job*	-0.163	-0.208*	-0.061	-0.126
	D_9 * *Post-job*	-0.245**	0.201*	0.056	0.039
	D_{10} * *Post-job*	0.093	0.243**	-0.056	-0.058
	D_{11} * *Post-job*	-0.041	0.166	0.006	-0.043
	D_{12} * *Post-job*	-0.080	0.124	0.001	0.016
	D_{13} * *Post-job*	-0.039	0.065	0.141	0.172*
	D_{14} * *Post-job*	-0.345**	-0.039	0.139	0.139
	D_{15} * *Post-job*	-0.095	-0.068	0.114	0.084
	D_{16} * *Post-job*	-0.119	0.124	0.066	0.068
	D_{17} * *Post-job*	-0.261	-0.292	0.128	0.104
	D_{18} * *Post-job*	0.088	0.136	0.134	0.090
	D_{19} * *Post-job*	-0.049	0.209	0.141	0.127
	D_{20} * *Post-job*	-0.165	-0.153	0.270***	0.294***
	D_{21} * *Post-job*	-0.073	-0.051	0.266***	0.244**
	D_{22} * *Post-job*	0.048	-0.011	0.169*	0.211**
	D_{23} * *Post-job*	-0.165	0.142	0.162	0.092
	D_{24} * *Post-job*	-0.250	-0.195	0.226**	0.200*
	D_{25} * *Post-job*	-0.592***	0.229	0.211**	0.209*
	D_{26} * *Post-job*	-0.745***	0.161	0.243**	0.236**
	D_{27} * *Post-job*	0.358	0.284	0.212**	0.166
	D_{28} * *Post-job*	-0.115	0.288	0.254**	0.215*
	D_{29} * *Post-job*	-0.409	0.460*	0.044	0.034
	D_{30} * *Post-job*	-0.994***	0.570**	0.133	0.182
Sample size		27,813	37,701	145,713	131,429

Note) 1. Social insurance programs in Korea include Unemployment insurance, National pension, National health insurance, and Industrial accident compensation insurance.

2. The exceptions of the regulation that can be identifiable in the data set are as follows: (i) Firms in the private sector with fewer than five employees, (ii) Workers aged 55 or older at the time of signing an employment contract, (iii) Workers who work less than 15 hours per week regularly.

Table A5. The descriptive statistics for the workers providing overtime hours

	OTi = 1, if a worker provides overtime work and *HRi = Weekly overtime hours*																	
	Year	*2001*	*2002*	*2003*	*2004*	*2005*	*2006*	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*	*2013*	*2014*	*2015*	*2016*
Regular workers	The number of workers	1089	1147	1417	1552	1559	1726	1865	1840	2101	2100	2129	2250	2286	2274	2426	2537
	The number of OTi = 1 (%)	503 (46.2)	530 (46.2)	500 (35.3)	574 (37.0)	576 (36.9)	643 (37.3)	676 (36.2)	731 (39.7)	786 (37.4)	791 (37.7)	820 (38.5)	831 (36.9)	864 (37.8)	911 (40.1)	912 (37.6)	915 (36.1)
	E[HR*i*|*HRi>0*]	9.0	8.9	8.8	8.8	8.6	8.5	8.9	9.4	8.9	9.7	9.7	9.4	9.4	9.6	9.2	9.1
Temporary-contract workers	The number of workers	257	271	264	276	284	303	317	351	564	600	643	650	699	704	687	755
	The number of OTi = 1 (%)	37 (14.4)	39 (14.4)	19 (7.2)	19 (6.9)	26 (9.2)	37 (12.2)	38 (12.0)	38 (10.8)	53 (9.4)	55 (9.2)	62 (9.6)	56 (8.6)	50 (7.2)	56 (8.0)	37 (5.4)	60 (7.9)
	E[HR*i*|*HRi>0*]	6.6	6.9	6.8	6.2	5.9	6.1	9.4	9.7	7.0	6.3	7.4	7.3	8.7	12.7	8.7	7.9

Table A6. The descriptive statistics for the workers providing unpaid overtime hours

	UOTi = 1, if a worker provides unpaid overtime work and *UHRi = Weekly unpaid overtime hours*																	
	Year	*2001*	*2002*	*2003*	*2004*	*2005*	*2006*	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*	*2013*	*2014*	*2015*	*2016*
Regular workers	The number of workers	1089	1147	1417	1552	1559	1726	1865	1840	2101	2100	2129	2250	2286	2274	2426	2537
	The number of UOTi = 1 (%)	194 (17.8)	194 (16.9)	213 (15.0)	250 (16.1)	248 (15.9)	270 (15.6)	273 (14.6)	322 (17.5)	363 (17.3)	344 (16.4)	359 (16.9)	360 (16.0)	397 (17.4)	421 (18.5)	453 (18.7)	444 (17.5)
	E[UHR*i*|*UHRi>0*]	9.0	8.4	7.9	8.1	8.4	7.7	7.9	8.0	8.6	8.6	9.3	8.8	8.5	8.3	8.3	8.4
Temporary-contract workers	The number of workers	257	271	264	276	284	303	317	351	564	600	643	650	699	704	687	755
	The number of UOTi = 1 (%)	23 (8.9)	19 (7.0)	8 (3.0)	15 (5.4)	5 (1.8)	14 (4.6)	12 (3.8)	11 (3.1)	28 (5.0)	28 (4.7)	31 (4.8)	28 (4.3)	20 (2.9)	26 (3.7)	17 (2.5)	20 (2.6)
	E[UHR*i*|*UHRi>0*]	7.0	6.0	6.5	6.1	3.6	5.4	10.2	7.0	6.0	5.4	5.8	6.8	8.9	15.1	9.9	6.6

Table A7. The effect of the reform on workers' overtime

		I. *OTit* = 1, if providing overtime work (paid & unpaid)	II. *UOTit* = 1, if providing unpaid overtime work	III. *HRit* = average Weekly overtime hours (paid & unpaid)	IV. *UHRit* = average weekly hours of unpaid overtime
Variables	Variable explanation	Probit	Probit	Tobit	Tobit
Temp	*Temporary-contract*	-0.396***	-0.215***	-4.768***	-3.152***
After	Post-reform period (1=after July, 2007)	0.106***	0.044	1.484***	0.755
Post-job	Post-job=1, if a job began after the reform	0.058	0.031	0.451	0.544
Temp * After	*Temporary-contract* * After	0.031	-0.017	0.201	-0.186
Temp * Post-job	*Temporary-contract* * Post-job	-0.128	-0.142	-1.097	-1.570
	Age Cohort (base: *age 35-39*)				
age_c1519	*age cohort1: 15-19*	-0.026	-0.629***	-0.650	-8.456***
age_c2024	*age cohort2: 20-24*	-0.029	-0.0672	-0.516	-0.912
age_c2529	*age cohort3: 25-29*	0.012	-0.007	0.011	-0.098
age_c3034	*age cohort4: 30-34*	0.057**	0.028	0.451	0.210
age_c4044	*age cohort6: 40-44*	-0.063**	-0.049	-0.449	-0.651
age_c4549	*age cohort7: 45-49*	-0.147***	-0.100**	-1.508***	-1.560***
age_c5054	*age cohort8: 50-54*	-0.304***	-0.276***	-3.376***	-4.245***
age_c5559	*age cohort9: 55-59*	-0.425***	-0.285***	-4.508***	-3.974***
age_c6064	*age cohort10: 60-64*	-0.713***	-0.437***	-8.215***	-6.414***
age_c65up	*age cohort11: 65 or older*	-1.009***	-0.627***	-12.890***	-9.581***
Female	Female	-0.176***	-0.271***	-2.234***	-4.106***
Married	Marriage status: 1=married	0.151***	0.0926***	1.677***	1.709***
Female * Married	Female * Married	-0.135***	-0.200***	-1.554***	-3.033***
	Education Level (base: *High school*)				
edu_1	*Elementary school or less*	0.121*	0.148*	1.712**	2.442*
edu_2	*Middle school*	0.028	0.015	0.616	0.527
edu_4	*College (2 years)*	-0.001	0.063*	-0.178	0.937*
edu_5	*University (4years)*	0.080**	0.244***	0.855**	3.677***
edu_6	*Master*	0.174***	0.325***	1.602**	4.682***
edu_7	*Ph.D.*	0.080	0.324**	0.649	4.453**
	Tenure in month (base: *13-24 months*)				
tenure_1	*1 ~ 3 months*	-0.309***	-0.147***	-3.023***	-1.748**
tenure_2	*4 ~ 6 months*	-0.129***	-0.093**	-1.349***	-1.304**
tenure_3	*7 ~ 12 months*	-0.080***	-0.034	-0.828**	-0.637
tenure_5	*25 ~ 36 months*	0.027	0.038	0.249	0.534
tenure_6	*37 ~ 60 months*	0.004	0.022	-0.025	0.360
tenure_7	*61 ~ 120 months*	0.003	-0.006	-0.188	-0.081
tenure_8	*121 months or more*	0.070*	0.082*	0.264	0.908
Private_sector	Private sector	0.106***	0.205***	1.598***	2.935***
Union	Union in the workplace	0.076**	0.081**	0.646*	1.122**
Union * In-union	Union membership	0.145***	-0.205***	1.179***	-2.866***
Parttime	Part-time job	-0.202***	-0.268***	-2.372***	-3.401***

Table A7. The effect of the reform on workers' overtime *(continued)*

		I. *OTit* = 1, if providing overtime work (paid & unpaid)	II. *UOTit* = 1, if providing unpaid overtime work	III. *HRit* = average Weekly overtime hours (paid & unpaid)	IV. *UHRit* = average weekly hours of unpaid overtime
Variables	Variable explanation	Probit	Probit	Tobit	Tobit
(continued)					
	Firm size (base: *1-9 people*)				
fsize_2	*10-29 people*	0.215***	0.0387	2.837***	0.706
fsize_3	*30-99 people*	0.385***	-0.007	4.916***	0.207
fsize_4	*100-299 people*	0.439***	-0.0247	5.747***	-0.018
fsize_5	*300-999 people*	0.498***	-0.0519	6.208***	-0.334
fsize_6	*1,000 or more than 1,000 people*	0.579***	-0.0620	6.896***	-0.307
fsize_7	*unknown*	0.234***	-0.231***	3.291***	-2.991***
	Occupation (base: *Office clerk*)				
occupation_1	*Legislative officers, executives, and high managers*	-0.110	-0.026	-0.469	0.106
occupation_2	*Professional*	0.117***	0.029	1.410***	0.561
occupation_3	*Engineers and semi-professional*	0.075*	0.022	0.939**	0.501
occupation_5	*Service workers*	-0.154**	-0.486***	-1.224	-6.034***
occupation_6	*Sales force*	-0.186***	-0.266***	-1.982***	-3.447***
occupation_7	*Agriculture and fishery workers*	0.046	-0.330	0.379	-3.652
occupation_8	*Technicians*	0.204***	-0.327***	2.342***	-4.359***
occupation_9	*Equipment, machine, and assembly workers*	0.184***	-0.634***	2.865***	-8.437***
occupation_10	*Unskilled workers*	-0.024	-0.451***	0.129	-6.120***
occupation_11	*unknown*	0.374	0.136	4.170*	2.456
	Industry (base: *Manufacturing*)				
ind_1	*Agriculture, hunting, forestry, and fishery*	-0.538**	-0.196	-6.232**	-3.336
ind_2	*Mining*	0.050	-0.309	0.945	-4.526
ind_4	*Electricity, gas, and water*	-0.501***	-0.242	-6.756***	-4.105*
ind_5	*Construction*	-0.302***	0.044	-3.221***	0.616
ind_6	*Wholesale and retail*	-0.464***	-0.121**	-5.290***	-1.978***
ind_7	*Lodge and food*	-0.451***	-0.110	-5.022***	-1.478
ind_8	*Transportation and communication service*	-0.610***	-0.171***	-6.936***	-2.440***
ind_9	*Banking and insurance*	-0.133**	0.256***	-1.581***	3.343***
ind_10	*Real estate and leasing service*	-0.871***	-0.386***	-10.20***	-5.606***
ind_11	*Business service*	-0.240***	0.075*	-2.654***	0.817
ind_12	*Public, social security, and defense administration*	-0.303***	-0.103	-3.810***	-2.290
ind_13	*Education service*	-0.621***	-0.215***	-6.916***	-3.313***
ind_14	*Health and social welfare service*	-0.534***	-0.236***	-6.059***	-3.309***
ind_15	*Entertainment, broadcasting, and performance*	-0.626***	-0.280***	-6.877***	-3.622**
ind_16	*Other individual service*	-0.494***	-0.034	-5.453***	-0.928
ind_17	*Other association, organization, and international agency*	-0.605***	-0.111	-6.890***	-2.061
ind_18	*unknown*	-0.490*	-0.154	-4.887*	-2.470
year	Linear time trend (in year)	-0.039***	-0.039***	-0.373***	-0.567***
year_sq	Square of linear time trend	0.002**	0.002***	0.016**	0.030***
emprate	Employment rate in each year	0.021	-0.0302	0.185	-0.534
unemprate	Unemployment rate in each year	-0.030	0.006	-0.398	-0.008
Constant		-1.637	0.920	-16.78	18.66
Sigma (σ)		-	-	11.91***	14.39***
Observations	Total number of observations (*left-censored observations at zero*)	37,835	37,835	37,835 (*25,663*)	37,835 (*32,452*)

Note) 1. Standard errors are clustered at an individual level (the number of individuals: 8,497).

*2. *** p<0.01, ** p<0.05, * p<0.1*