Labor Market Impact of Labor Cost Increase without Productivity Gain: A Natural Experiment from the 2003 Social Insurance Premium Reform in Japan[#]

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Abstract

Exploiting heterogeneous variation in labor cost increases due to Japan's 2003 social insurance premium reform as a natural experiment, we find negative effects on employment, positive effects on average annual earnings, and no effect on total payroll costs and total working hours in an establishment that experienced the increase in social insurance contributions. The increase of the burden on surviving workers is financed by an increase in average salaries resulting from longer working hours. Our findings imply that exogenous labor cost increases without productivity gains could trigger job cuts, especially in sectors and countries where dismissals are rigorously regulated.

Keywords: Labor costs, Employment, Fixed-effect difference-in-differences model, DiNardo-Fortin-Lemieux decomposition, Social insurance premiums
JEL Classification: J33, J38, H20

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I. Introduction

For most firms in developed countries, the burden of social insurance premiums for their employees can be substantial costs, along with tax burdens. Although some countries relieve the tax burden, or keep it low, to reinforce international competitiveness, firms in many countries are bearing a greater burden of social welfare and pension expenses, and this trend has been getting stronger as populations age.[1] Japan is not an exception to this, and social insurance premiums in Japan are known to have been high and increasing gradually over the last 20 years with the rapid aging of the population. Since an increase in labor costs could hurt firms' profits and thus can affect labor demand as well, an increase in labor costs without productivity gains can have a bad influence on both firms and workers. In spite of the fact that it is extremely important to understand the impact of the social welfare burden on employment and labor demand, there are few studies discussing the impacts of an increase in social insurance premiums from the labor demand side. This is because of an identification problem: in many cases, we cannot identify the policy impact due to the fact that social policy and law, in general, are uniformly changed and implemented within a country.

However, an exceptional case occurred in Japan in 2003: the standard of calculation of the social insurance premium rate changed across the board, but the degree of influence of the reform depended on the past bonus-to-salary ratio; that is, it was heterogeneous among firms, creating variation in the policy changes. Utilizing this heterogeneity, we estimate the impact of the reform on various labor-related

[1]For example, the percentages for social insurance premiums and of the tax burden to national income, respectively, are 7.4% and 23.3% in the US; 10.7% and 37.0% in the UK; 29.5% and 21.7% in Germany; 36.7% and 25.2% in France; and 24.1% and 17.5% in Japan. http://www.nta.go.jp/osaka/shiraberu/gakushu/kyozai/pdf/04/08.pdf

outcomes such as employment, hours worked, and total payroll costs. More specifically, after the reform, the insurance premiums came to be calculated based on total annual earnings (that is, the total amount of monthly salary and bonuses), whereas before the reform the premiums were calculated based on the monthly salary only. This newly introduced system is called the "total reward system." In response to the reform, the premium rate for bonuses increased from 2.00% to 21.87%, while that for the monthly salary decreased from 25.96% to 21.87% to balance out the overall insurance budget. Theoretically, if firms had not changed any variable at the timing of the reform, firms whose bonus-to-salary ratio was originally relatively high should have experienced an increase in labor costs as a result of this reform, while others should have experienced a decrease. In sum, even though the social insurance premium rates were uniformly changed throughout the nation, the impacts of the reform on firms varied depending on the original bonus-to-salary ratio that had been applied by each firm until that time. Firms with a higher bonus-to-salary ratio would have had to pay more insurance premiums even if they paid exactly the same amount in wages. Assuming that firms became aware of the reform before 2002, they would not have changed their bonus-to-salary ratio before 2002 because they would have had to pay more in insurance premiums until 2002. Firms would not have changed their bonus-to-salary ratio after 2004 because they could not save the premium burden by changing their bonus-to-salary ratio artificially in a situation in which the premium rate for bonuses was same as that for the monthly salary. We believe that our findings may be applicable not only in Japan, but also in many developed countries suffering from an increase in social insurance premium burdens.

Our paper is most closely related to the following three strands of the existing literature. The first

strand of the related literature explores the impacts on employment of labor-related policies that impose higher labor costs, such as minimum wages, and overtime regulation (Hamermesh 2014, Kim 2008, Sakai 2009, Miyazato and Ogura 2010, Kawaguchi and Mori 2013, Kawaguchi et al. 2008). Hamermesh (2014) describes how polices that increase labor costs, such as overtime pay, hiring subsidies, a minimum wage, and payroll taxes, can affect both employment and working hours. Related studies including Angrist (2000), Kugler and Kugler (2009), and Autor et al. (2004) reach a reasonable consensus on the proposition that higher hourly wage costs do lead employers to use fewer workers.

The second strand is the literature on the effects of tax reform on labor market outcomes. For example, Eissa and Liebman (1996) examined the impact of the US Tax Reform Act of 1986 on labor supply. The 1986 reform expanded the earned income tax credit, and the maximum credit varied depending on the number of children. They used a difference-in-difference (DID) estimation to compare the change in the labor supply of single women with and without children, and found that the reform increased relative labor force participation among single women with children. Blundell et al. (1998) examined how the UK tax reforms in the 1980s affected the country's labor supply by comparing the labor supply responses over time for different groups defined by cohort and education level. They found negative income effects for women with children. Many studies have focused on how changes in tax rates affect labor income through working hours or labor participation, as well as job choices, and manner of earning income (such as salary, dividends, or capital gains) from the labor supply side;[2] however, there are few studies from the labor

[2] For example, Aaron (1981), Feldstein (1995), Feldstein and Feenberg (1996), Blundell et al. (1998), Meghir and Phillips (2008), Gelber (2014).

demand side except for Kugler and Kugler 2009, and Gruber 1997. Our paper explores the impacts of policies that result in higher labor costs from the demand side, so it can contribute to the literature by discussing the issue from the demand side.

The final strand of the related literature includes studies focusing on the discussion of who ultimately bears the costs of insurance contributions: employers or employees. These studies yield a wide variety of conclusions on the incidence of insurance premium burdens and who bears them, because the countries, objects, and data sources differ for researchers in different countries (Hamermesh 1979, Holmlund 1983, Komamura and Yamada 2004, Tachibanaki and Yokoyama 2008, Iwamoto and Hamaaki 2006, Sakai 2006, Sakai and Kazekami 2007, Iwamoto and Hamaaki 2009, Gruber 1997, Anderson and Meyer 2000, Kugler and Kugler 2009, Iwamoto and Hamaaki 2006, Hamaaki 2012,[3] Melguizo and González-Páramo 2013, and Müller and Neumann 2016). The results are mixed. Some find that employees pay a substantial amount of the social insurance premiums, and others describe employers financing the premiums. Iwamoto and Hamaaki (2006) discussed the mechanism of the incidence of social insurance contributions theoretically.

Our key contribution is to use the DID method with establishment fixed effects to show that, in response to an exogenous increase of labor costs without any productivity gains, firms reduce the number of employees, and increase average annual earnings stemming from longer working hours. Firms manage

[3] Hamaaki (2012) also uses firm-level panel data. Because the dataset he used does not have information on the amount of bonuses before the reform, he uses a counterfactual premium rate calculated by assuming that the monthly salary is fixed before and after 2003, while we can have the actual bonus-to-salary ratio for both periods: both before and after the reform. In addition, another advantage of using our dataset is that we can utilize more information on time-varying characteristics of establishments such as the composition of workers in each establishment with regard to gender, potential years of experience, tenure, education, and so on. Including these variables makes the estimation more robust to violation of the common trend assumption.

to pay for this increase in the average wage paid to surviving workers by cutting the number of employees to keep total payroll costs unchanged. The continuing workers do not pay for the increase in the burden, and the increase in the burden is financed by dismissed workers. In contrast, firms have to absorb all of the remaining half of the premiums imposed on firms.

In our second exercise, applying the decomposition method of DiNardo et al. (1996), we confirm that the distributions of bonuses and employment shifted down and that the distributions of average working hours and average monthly salaries shifted to the right after the change. Since the increase in costs is cancelled out by the decrease in bonuses and employment, the distribution of the total payroll cost did not change. These results are completely consistent with the results we obtained from the DID method.

To gain insight into why firms with a heavier burden of insurance premiums cut more employment, we split the samples into four groups: (1) large-sized manufacturing, (2) small and medium-sized manufacturing, (3) large-sized non-manufacturing, and (4) small and medium-sized non-manufacturing firms. Firms reduced the number of employees in response to the exogenous increase of labor costs without any productivity gains. This is especially true in large firms in manufacturing sectors, which are likely to be forced to stick with a labor hoarding policy due to social pressure. As Levine and Tyson (1990) argue, Japanese workers, especially in large firms, are known to be guaranteed relatively higher job security through intra-firm transfers, temporary transfers to subsidiaries, and wage compressions.[4] Large manufacturing firms show the highest surplus employment index. Our results indicating that large

[4] Rules on dismissal in Japan are said to be much stricter than those in the US. Article 18-2 of the Labor Contact Law stipulates that a dismissal shall be treated as a misuse of that right and invalid if the dismissal lacks objectively reasonable grounds and is not considered to be appropriate in general societal terms. Large firms tend to operate conservatively to avoid potential lawsuits.

manufacturing firms did fire many workers at the timing of the 2003 reform may imply that the 2003 reform provided a good excuse to cut employment for firms that had been forced to stick with a labor hoarding policy even in a situation of over-employment.

This paper is organized as follows. Section 2 describes the social insurance premium system and its reforms in 2003 in Japan. Section 3 provides an empirical model and Section 4 offers a brief description of the data. Section 5 discusses the results of the empirical analysis. The last section concludes the paper.

II. The 2003 Social Insurance Premium Reforms

In Japan, the premiums for welfare pensions and medical insurance payments are to be shared equally between employees and employers in principle, but employers pay the entire contribution for the child allowance. Specifically, the rates of insurance premiums for employee welfare pensions, medical insurance, and contributions to the child allowance in the private sector are fixed throughout Japan. The respective premium rates before and after the change are shown in Table 1.[5]

Table 1. *Premium rates before and after the policy change*

	Before		After	
	Bonus	Salary	Bonus	Salary
Welfare Insurance Premiums	1.00%	17.35%	13.58%	13.58%
Health Insurance Premiums	1.00%	8.50%	8.20%	8.20%
Child Benefits	0.00%	0.11%	0.09%	0.09%
Total	2.00%	25.96%	21.87%	21.87%

Note: Table 1 shows the social insurance premium before and after April in 2003.

[5] There are ceilings for the insurance expenses for employee welfare pensions and for medical insurance.

In 2003, the standard for calculation of the social insurance premium rate changed across the board, but the effect varied depending on the bonus-to-salary ratio. More specifically, after the reform, the insurance premiums came to be calculated based on total annual earnings, whereas the premiums were calculated based on the monthly salary alone before the reform. This is the reason why the 2003 reform of the social insurance premiums is referred to as the introduction of "the total reward system."

As can be confirmed in Table 1, before the reform, the social insurance premium rate for bonuses had been set to almost zero (25.96% vs. 2.00%), but after April 2003, the premium rates became equal between monthly salary and bonuses, and the total premiums came to depend on the total reward, that is, the total amount of monthly salary and bonuses. The Ministry of Health, Labour and Welfare of the government of Japan explained that the aim of introducing the total reward systems was to rectify unfairness concerning the large difference in premium rates between the monthly salary and bonuses in the old system (25.96% vs. 2.00%), and that they did not aim to support insurance finance by this reform.

As a result, this reform increased the total insurance premiums on bonuses from 2.00% to 21.87%, and decreased the total premiums on the monthly salary from 25.96% to 21.87%. According to this change, if firms paid exactly the same amount in wages, firms with a higher bonus-to-salary ratio had to pay more in premiums for social insurance. Firms with a higher bonus-to-salary ratio would have to pay more in insurance premiums after the reform if they paid exactly the same amount in annual earnings. Assuming that firms became aware of the reform before 2002, they would not have changed their bonus-to-salary ratio before 2002 because they would have had to pay more in insurance premiums until 2002. Firms would not have changed the bonus-to-salary ratio after 2004 because they could not save the premium

burden by changing their bonus-to-salary ratio artificially in a situation in which the premium rate for bonuses was the same as that for the monthly salary. This change could be a good natural experiment, and the effect of this natural experiment would vary depending on the bonus-to-salary ratio in the year preceding the reform.

Before moving on to the empirical section, we will summarize three specific characteristics of Japanese bonuses. First, the bonus-to-salary ratio is relatively high in Japan. The average annual amount paid in bonuses is historically high: about 2.5 times the monthly salary. Second, bonuses are highly sensitive to changes in firm performance (Kato 2016). Bonuses play an important role in a profit-sharing system, since employers tend to distribute extra benefits brought by productivity growth to workers in the form of bonuses. Finally, bonuses have little downward rigidity, unlike the monthly regular salary (Kato 2016). In the Japanese labor market characterized by long-term employment guarantees and a seniority-based wage system, especially for large and old firms, it is not easy to fire employees, and thus firms use overtime hours and bonuses as one of the buffers against negative shocks.

III. Empirical Model

A. Difference-in-differences

To evaluate the effects of the social insurance premium reform on firms' labor-related behavior, we first estimate the following standard fixed-effect DID model:

$$y_{it} = \alpha + \beta After_t \cdot Treatment_i + \gamma X_{it}$$
$$+(year\ \ effects) + (establishments\ \ fixed\ \ effects) + u_{it} \quad (1)$$

where y_{it} represents outcomes for establishment i in year t, such as employment, total payroll costs in the establishment, total working hours in the establishment, average hours worked, and average amount of monthly salary and bonuses at 2010 prices.[6] We treat periods before 2003 as the "before" period and the period after 2003 as the "after" period. Thus, $After_t$ is a dummy variable taking one for years after 2003 and zero for years before 2003. The bonus-to-salary ratio in 2002 (that is, the bonus-to-salary ratio before the reform) is used for the continuous treatment variable by exploiting our establishment-level panel data. Thus, $Treatment_i$ represents the bonus-to-salary ratio in the year preceding the reform for establishment i. As is generally done in fixed-effects DID, we also exclude the treatment dummy from the right-hand-side variable because $Treatment_i$ is time-invariant, and we are now using the fixed effect model. $After_t \times Treatment_i$ is the interaction term with the $After_t$ dummy variable and $Treatment_i$.[7] X_{it} includes time-variant establishment characteristics such as the female employee ratio, average tenure and its square, average experience in years and its square, proportion of graduates from each level of school (junior high school, senior high school, two-year-college, and four-year-university) and industry dummies.

The estimated coefficient of $After_t \times Treatment_i$ is of prime interest, and a significantly negative coefficient indicates that the 2003 reform has negative impacts on firms with a heavier burden of social

[6] CPI is used as a deflator.

[7] For a robustness check, we create an alternative index of the bonus-to-salary ratio using bonus payments of the same establishments in the survey in the next year in order to synchronize the year of the bonus payments with monthly salary.

insurance premiums.

To control for macro shocks common to all establishments, we also include year fixed effects. While year effects control for common shocks, there may be establishment-specific time-variant shocks that can be correlated with $Treatment_i$ and labor outcomes, which leads to biased estimates in the fixed effects model.

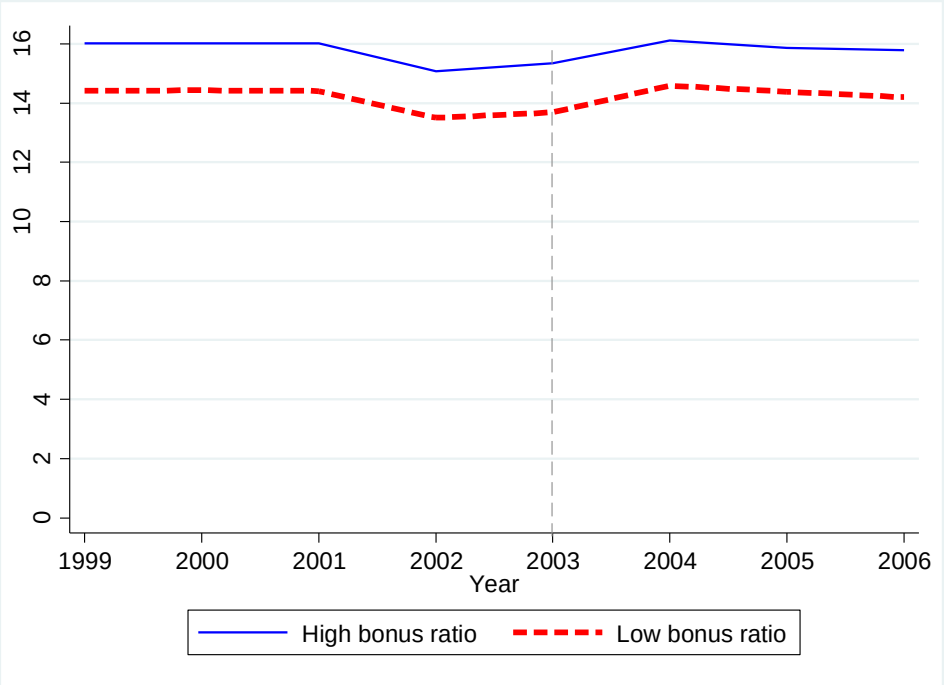
To address the threat to identification that would arise if our treatment and control group had experienced different trends in employment and payroll costs, we check if our treatment and control groups had actually experienced different trends in employment prior to 2003. We show two time series of average employment in Figure 1*a*, and total payroll costs in Figure 1*b*, for the treatment group and the control group. Although we use continuous bonus-to-salary ratio as a treatment variable in the regressions below, here we divide establishments above/below the median of the bonus-to-salary ratio in 2002.

We do not see any obvious difference in the trends between the high bonus ratio group and the low bonus ratio group from Figures 1*a* and 1*b*. Although we can visually confirm the validity of the common trend assumption in Figures 1*a* and 1*b*, there could still be a possibility that the trend is not exactly the same. Moreover, to control for time-variant labor demand shocks, we include total payroll costs and the ratio of new graduates as control variables as a proxy for labor demand shock. In addition, we try another specification assuming different time trends between treatment and control groups in the robustness section.[8]

[8] The same method is applied in Li et al. (2016).

Figure 1*a*. *Changes in ln(Employment): Treatment group vs. control group*

Figure 1*b*. *Changes in ln(Total payroll costs): Treatment group vs. control group*



Notes: In both figures, high and low bonus ratio groups are categorized according to whether they are above the median bonus ratio or not.

For further discussion, we split the sample firms into four groups: (1) large-sized manufacturing, (2) small and medium-sized manufacturing, (3) large-sized non-manufacturing, and (4) small and medium-sized non-manufacturing firms. We apply the DID approach in Equation (1) to the four groups, and focus on heterogenous results.

B. Before and after distribution: DFL decomposition

Lastly, we visually confirm how the behavioral changes affect the overall distribution of employment, average monthly work hours, average monthly salary, average bonus amount, average bonus-to-salary ratio, total work hours, and total payroll costs, applying the DFL decomposition (DiNardo et al. 1996, DiNardo and Lemieux 1997). The advantage of this method is that it can visually decompose the change in the distribution into two parts: structure effects and composition effects.[9]

First, the distribution in 2002 is expressed as:

$$F_{2002} = \int f_{2002}(y|X)h(X|t = 2002)dX \quad (2)$$

where $f_{2002}(y|X)$ is a determination mechanism of "y" (outcomes for establishment i) in 2002 that maps firms' attributes to the distribution of "y." The density $h(X|t = 2002)$ is firms' attributes in the year 2002. Similarly, the distribution during year 2004 is expressed as:

$$F_{2004} = \int f_{2004}(y|X)h(X|t = 2004)dX \quad (3)$$

What the distribution would be after the 2003 reform if the determination mechanism of "y" were identical to its mechanism in 2002 is expressed as:

[9] Because the disadvantage of this method is that it contains the effects of the policy change as well as those of other changes such as business cycle, we use this method only as a check for robustness.

$$F_{2004}^{2002} = \int f_{2002}(y|X)h(X|t = 2004)dX \quad (4)$$

This can be thought of as a counterfactual distribution in the period 2004 without the reform because it consists of the same firms' attributes as the real 2004 distribution of X but of β (coefficients of X) prior to the tax reform. This counterfactual distribution is calculated by DiNardo et al. (1996) method using the reweighting term ω as follows:

$$F_{2004}^{2002} = \int f_{2002}(y|X)h(X|t = 2004)dX = \int \omega f_{2002}(Y|X)h(X|t = 2002)dX \quad (5)$$

The reweighting term ω can be calculated by the DiNardo et al. (1996) method:

$$\omega = \frac{h(X|t = 2004)}{h(X|t = 2002)} = \frac{P(X)P(t = 2004|X)/P(t = 2004)}{P(X)P(t = 2002|X)/P(t = 2002)} = \frac{P(t = 2004|X)P(t = 2002)}{P(t = 2002|X)P(t = 2004)} \quad (6)$$

where the density $h(X|t = T)$ is the p.d.f. of attributes in year T. The second equation is derived from Bayes' rule. In the actual regression of w, $P(t = T|X)$ can be calculated using propensity scores obtained from the probit model in which $P(t = T)$ is regressed on X, and $P(t = T)$ is calculated as the proportion of observations from year T in the pooled data.

IV. Data

We use the Basic Survey on Wage Structures (BSWS), the most comprehensive wage survey in Japan, which is conducted every year by the Ministry of Health, Labour and Welfare. The BSWS excludes agriculture, forestry, fisheries, and public services. It covers private- and public-sector firms with ten or more employees, and private-sector establishments with five to nine employees. The establishments in the sample are randomly chosen in proportion to the size of prefectures, industries, and the number of

employees, using data from the Establishment and Enterprise Census (EEC), which includes all establishments in Japan. The sampling for the survey was implemented in two steps: first, a random sample of establishments was selected; then, the establishments selected in the first step were asked to take a random sample of workers and provide their payroll records.

The data contain information on individual workers' monthly salaries in June, total bonus payments in the previous year, hours worked, gender, age, length of employment, education, job title, and job type.[10] The data include approximately 1.2 million workers for each year, from 70,000 establishments. The reported monthly salary and bonus only include wages paid to workers. In addition to paying these salaries, firms have to pay the other half of the social insurance premiums.

We created the establishment-level panel data using the information from the EEC. We constructed establishment-level data for each variable by using worker-level information within each establishment. The dataset we used in this analysis contains 340,988 establishment observations from 1999 to 2006. We define 1999–2002 as before the reform, and 2004–2006 as after the reform.[11] Since $Treatment_i$, which is the bonus-to-salary ratio in 2002, cannot be obtained from establishments that do not appear in the 2002 survey, observations used in the main analyses are restricted to establishments that have information on their bonus-to-salary ratio in 2002, which amounts to 112,498 establishments.

Columns 1 to 3 in Table 2 summarize descriptive statistics for samples for all years (1999–2006), and before (1999–2002) and after (2004–2006) the reform, which are used in the main analyses.

[10] They report the monthly salary and bonus that firms pay to workers before tax.
[11] We exclude the data for 2003.

Table 2. *Descriptive statistics*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel Structure	Unbalanced (1999–2002 vs 2004–2006)			Balanced (2002 and 2004)		
Sample Period	All	Before	After	All	Before	After
# of employees	227.83	206.65	305.73	355.13	363.01	347.25
	(548.69)	(509.37)	(668.09)	(686.62)	(689.72)	(683.46)
Total Work Hours	38163.25	34516.30	51572.71	59261.52	60214.12	58308.91
	(92779.57)	(85891.70)	(113599.45)	(115210.03)	(114009.61)	(116397.21)
Total Labor Cost	1335.04	1199.46	1833.57	2147.60	2187.42	2107.77
	(3971.01)	(3641.73)	(4966.95)	(5141.83)	(5087.70)	(5195.41)
Bonus Ratio	2.77	2.77	2.79	3.01	3.14	2.87
	(1.55)	(1.57)	(1.49)	(1.47)	(1.46)	(1.46)
Experience	21.44	21.33	21.84	21.12	20.88	21.35
	(7.03)	(7.21)	(6.32)	(6.15)	(6.26)	(6.04)
Mean Tenure	12.15	11.83	13.31	13.36	13.12	13.59
	(6.03)	(6.03)	(5.92)	(5.91)	(5.89)	(5.93)
Junior High School Graduates	0.09	0.10	0.07	0.07	0.08	0.06
	(0.16)	(0.17)	(0.12)	(0.12)	(0.13)	(0.12)
High School Graduates	0.53	0.53	0.52	0.51	0.52	0.51
	(0.28)	(0.28)	(0.29)	(0.28)	(0.28)	(0.29)
Two-year College Graduates	0.13	0.13	0.12	0.13	0.13	0.13
	(0.18)	(0.18)	(0.16)	(0.16)	(0.16)	(0.16)
University Graduates	0.24	0.23	0.29	0.29	0.28	0.30
	(0.25)	(0.25)	(0.27)	(0.26)	(0.26)	(0.27)
Firm Size	1320.29	1267.55	1514.21	1629.97	1636.89	1623.05
	(1829.15)	(1814.53)	(1869.21)	(1910.35)	(1917.88)	(1902.87)
Bonus Amount	9252.26	9109.53	9777.03	10645.10	11033.52	10256.69
	(7080.26)	(7025.24)	(7255.07)	(7343.45)	(7375.42)	(7291.12)
Monthly Salary	2984.01	2941.15	3141.60	3212.26	3192.41	3232.12
	(1058.44)	(1033.70)	(1130.94)	(1121.87)	(1102.72)	(1140.41)
Average Hours	168.41	168.39	168.47	168.23	167.63	168.83
	(24.59)	(24.75)	(24.00)	(23.04)	(22.82)	(23.23)
After	0.21	0.00	1.00	0.50	0.00	1.00
Female	0.33	0.33	0.31	0.31	0.31	0.31
Observations	112498	88444	24054	16466	8233	8233

Note: Columns 1 to 3 summarize descriptive statistics for samples for all years (1999–2006), and before (1999–2002) and after (2004–2006) the reform. Columns 4 to 6 report descriptive statistics for a balanced panel composed of establishments that appear both in the 2002 and 2004 surveys. Standard deviations are in parentheses. All variables related to wages are deflated by CPI.

Comparing the number of observations in Columns 2 and 3, we find that the number of samples used in the analyses are much larger in the "before" period than in the "after" period. This is because establishments in 1999–2001 are more likely to appear in the 2002 survey as well, and thus have information on their bonus-to-salary ratio in 2002, than establishments in 2004–2006, due to the closeness of the year to 2002. As can be expected from the drop in the number of observations from the "before" to the "after" period, the comparison in values between the two periods in the unbalanced panel data can be influenced by changes in the composition of establishments in the sample. We also report descriptive statistics for the balanced panel sample in Columns 4 to 6. These columns consist of establishments that appear both in 2002 and 2004. Column 4 includes samples for 2002 and 2004, Column 5 includes only 2002, and Column 6 includes only 2004. There are only 8,233 establishments in the balanced panel data in 2002 and 2004.

In the latter section, we basically use data from 1999 to 2006 to control for time trends. We also estimate using only 2002 and 2004 balanced panel data for the robustness check.

The BSWS originally consists of repeated cross-sectional data. However, identification of each establishment using EEC codes allows us to construct treatment and control groups. Although the sample size becomes smaller in the process of data construction, there are still tens to hundreds of thousands of samples in our estimates.

Comparing the number of employees between the unbalanced and balanced panel data, we find that the level of the average number of employees is larger for the balanced panel data than the unbalanced panel data. This means that larger establishments are more likely to be tracked in the survey over multiple

years. The number of employees increased after the period in the unbalanced panel but decreased in the balanced panel. The inconsistency in the number of employees can come from selection that occurs in the process of constructing panel data. The balanced panel summary statistics show that the 2003 reform reduced employment, total working hours, and total labor costs.

V. Empirical Results

A. DID results

Table 3 presents the fixed-effects DID estimates for Equation (1), using the data for 1999–2002 (before the reform) and 2004–2006 (after the reform). Our baseline estimates in Column 1 show that the estimated coefficient for $After_t \times Treatment_i$ is negative, and significant at the 1% significance level. The size of the estimated coefficient suggests that establishments having a bonus-to-salary ratio in 2002 that is one month greater leads to a 0.9% decrease in employment after the reforms. This finding implies that the firms subject to the imposition of heavier social insurance premiums exogenously due to this reform reduced their employment to a greater degree. In the previous section, we pointed out a selection issue when using the unbalanced panel data, and the selection issue stated above will be solved by controlling for establishment effects.

Furthermore, we are also careful about time-varying establishment-specific shocks as well. The second column includes total work hours as a proxy for time-varying idiosyncratic demand shocks. Even after controlling for time-varying establishment-specific shocks, the magnitude of the coefficient and its

significance does not change much from that in Column 1.

Table 3. *Impacts of the reform on employment*

	(1)	(2)	(3)
Dependent variable:	Baseline	Control for total labor	Control for time-variant
$After_t \times Treatment_{i(02)}$	-0.009***	-0.007***	-0.005**
	(0.002)	(0.001)	(0.002)
Female	0.463***	0.207***	0.588***
	(0.062)	(0.011)	(0.098)
Experience	0.008***	0.007***	0.009**
	(0.003)	(0.001)	(0.004)
$Experience^2/100$	-0.009*	-0.008***	-0.01
	(0.005)	(0.002)	(0.008)
Tenure	-0.054***	-0.012***	-0.048***
	(0.003)	(0.001)	(0.004)
$Tenure^2/100$	0.116***	0.026***	0.100***
	(0.009)	(0.002)	(0.012)
High School Graduates	-0.077***	-0.011	-0.103***
	(0.022)	(0.007)	(0.032)
Two-year College Graduates	-0.120***	-0.013	-0.129***
	(0.029)	(0.008)	(0.041)
University Graduates	-0.215***	0.011	-0.229***
	(0.039)	(0.009)	(0.051)
ln(Total Work Hours)		0.942***	
		(0.002)	
Ratio of new recruits			0.161**
			(0.073)
Year2000	-0.011***	-0.014***	0.064***
	(0.003)	(0.001)	(0.008)
Year2001	-0.007**	-0.008***	0.061***
	(0.003)	(0.001)	(0.008)
Year2002	-0.065***	0	0.006
	(0.004)	(0.001)	(0.007)
Year2004	-0.058***	0.014***	-0.027***
	(0.009)	(0.003)	(0.004)
Year2005	-0.058***	0.007***	-0.033***
	(0.009)	(0.003)	(0.006)
Year2006	-0.059***	0.001	-0.017***
	(0.009)	(0.003)	(0.006)
R-squared	0.064	0.924	0.073
N	112498	112498	74780

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at establishment level are in parentheses. 1999 is a reference year for the year dummies.

The estimated coefficient on $After_t \times Treatment_i$ is significantly negative at the 1% significance level and the value of the estimated coefficient is 0.7%. This result, in tandem with the evidence of Figures 1a and 1b, implies that the concern regarding an omitted variable biasing the estimated coefficient of our primary interest, $After_t \times Treatment_i$, is not very serious.

The third column provides further evidence for the robustness of the estimated coefficient on $After_t \times Treatment_i$, which changes little even when we use another proxy variable for time-varying firm-specific shocks: the ratio of new recruits to total employees. Note that this result, indicating that employment decreased in response to the 2003 reform, is consistent with what we confirmed from the descriptive statistics (Table 2).

The first column in Table 4 reports the regression results for the logarithm of average monthly working hours, and the second column shows a regression for the logarithm of total monthly working hours in an establishment. The coefficient of the average monthly working hours is positive and significant at the 1% level, but that for the total work hours within an establishment is negative and insignificant. The inconsistency of positive hours worked and negative total work hours within an establishment can be explained by the decrease in employment.

As shown in the third column in Table 4, the coefficient of the logarithm of average annual earnings on $After_t \times Treatment_i$ is significantly positive at the 1% significance level. The size of the estimated coefficient suggests that having a bonus-to-salary ratio before the reform that is one month greater leads to a 0.7% percent increase in average annual earnings.

The fourth column reports the results for the logarithm of total payroll costs in an establishment as

the dependent variable. The estimated coefficient on $After_t \times Treatment_i$ is insignificant. The effects of the increase in the average annual earnings are offset by job cuts.

Table 4. *Impacts of the reform on working hours and wages*

	(1) ln(Average monthly work	(2) ln(Total monthly work hours)	(3) ln(Average annual earnings)	(4) ln(Total payroll costs)
$After_t \times Treatment_{i(02)}$	0.008***	-0.003	0.007***	-0.0037
	(0.001)	(0.002)	(0.001)	(0.0024)
Female	-0.236***	0.272***	-0.652***	-0.143**
	(0.010)	(0.061)	(0.015)	(0.061)
Experience	-0.006***	0.001	0.016***	0.023***
	(0.001)	(0.003)	(0.001)	(0.003)
$Experience^2/100$	0.007***	-0.001	-0.032***	-0.039***
	(0.002)	(0.005)	(0.002)	(0.006)
Tenure	0.008***	-0.044***	0.031***	-0.021***
	(0.001)	(0.003)	(0.001)	(0.003)
$Tenure^2/100$	-0.018***	0.096***	-0.038***	0.076***
	(0.002)	(0.008)	(0.003)	(0.009)
High School Graduates	0.004	-0.070***	0.025***	-0.049**
	(0.007)	(0.023)	(0.009)	(0.024)
Two-year College Graduates	0.005	-0.113***	0.071***	-0.048
	(0.008)	(0.029)	(0.011)	(0.031)
University Graduates	-0.028***	-0.239***	0.140***	-0.071*
	(0.009)	(0.040)	(0.013)	(0.041)
Year2000	0.014***	0.003	-0.003***	-0.014***
	(0.001)	(0.003)	(0.001)	(0.003)
Year2001	0.008***	0.001	-0.005***	-0.012***
	(0.001)	(0.003)	(0.001)	(0.003)
Year2002	-0.004***	-0.069***	-0.017***	-0.082***
	(0.001)	(0.004)	(0.001)	(0.004)
Year2004	-0.025***	-0.077***	-0.064***	-0.116***
	(0.003)	(0.009)	(0.003)	(0.009)
Year2005	-0.026***	-0.069***	-0.077***	-0.120***
	(0.003)	(0.009)	(0.004)	(0.009)
Year2006	-0.022***	-0.063***	-0.075***	-0.116***
	(0.003)	(0.009)	(0.004)	(0.009)
R-squared	0.048	0.052	0.321	0.038
N	112498	112498	112498	112498

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at establishment level are in parentheses. 1999 is a reference year for the year dummies.

B. Robustness check

Thus far, we have treated the bonus-to-salary ratio in 2002 as a variable representing each establishment's bonus-to-salary ratio as a proxy of the impact of the 2003 reform. However, it might be possible that the bonus-to-salary ratio in 2002 is unusual (for example, establishments might have experienced some special shocks in that year), and thus categorizing firms based on the 2002 bonus-to-salary ratio might induce bias. To mitigate against the possibility of an abrupt shock in 2002 that could bias our results, we also use the average bonus-to-salary ratio during 1999–2002 for each establishment as an independent variable, instead of the 2002 bonus-to-salary ratio.[12] Table 5 present the results of the robustness test for the regression of employment (Table 3). In Column 1 in Table 5, we find our key results in Column 1 in Table 3 to be robust to the change in the treatment variable.

Columns 2 and 3 repeat the same analysis, using smaller samples consisting of 2002 and 2004. Using this sample, we also test two specifications with two kinds of treatment variables: the 2002 bonus-to-salary ratio, and the average bonus-to-salary ratio for each establishment during 1999–2002. Reassuringly, our key results – the negative and statistically significant coefficient on $After_t \times Treatment_i$ – changes little even when we restrict years. The placebo test results are shown in Columns 4 and 5 in Table 5, using the data for 1999 and 2001, both of which should not have been affected by the 2003 reforms.[13]

[12] Since observations are now retained in the sample as long as the establishment appears at least once during 1999–2002, the number of observations are larger than in the main regressions. In other words, thus far, for establishments to be in the analysis sample, they have to appear in the 2002 survey to have information regarding the 2002 bonus-to-salary ratio, but now the average bonus-to-salary ratio of each establishment during 1999–2002 is used for the treatment variable. Thus, as long as the establishment appears at least once during the "before" period, the establishment will be included in the unbalanced panel sample.

[13] The reason for using two years in the placebo test as well is to make the results comparable with Columns 2 and 3 in Table 5, where only two years are used for the sample.

Table 5. *Robustness check and placebo tests for employment regression*

	(1)	(2)	(3)	(4)	(5)
	1999–2002	2002 and 2004		Placebo tests: 1999 and 2001	
	Treatment defined over 1999–2002	Treatment defined as 2002 Bonus-to-salary ratio	Treatment defined over 1999–2002	Treatment defined as 2002 Bonus-to-salary ratio	Treatment defined over 1999–2002
$After_t \times Treatment_{i(02)}$		-0.010***		0.001	
		(0.003)		(0.003)	
$After_t \times Treatment_{i(99\text{-}02)}$	-0.010***		-0.010***		0.001
	(0.002)		(0.003)		(0.003)
Female	0.497***	0.597***	0.597***	0.834***	0.834***
	(0.042)	(0.162)	(0.162)	(0.163)	(0.163)
Experience	0.004**	0.018***	0.018***	-0.007	-0.007
	(0.002)	(0.007)	(0.007)	(0.008)	(0.008)
$Experience^2/100$	-0.001	-0.039***	-0.039***	0.022	0.022
	(0.003)	(0.014)	(0.014)	(0.016)	(0.016)
Tenure	-0.053***	-0.059***	-0.059***	-0.030***	-0.030***
	(0.002)	(0.008)	(0.008)	(0.008)	(0.008)
$Tenure^2/100$	0.116***	0.152***	0.152***	0.052**	0.052**
	(0.006)	(0.022)	(0.022)	(0.021)	(0.021)
High School Graduates	-0.045***	-0.058	-0.058	-0.156**	-0.156**
	(0.013)	(0.062)	(0.062)	(0.062)	(0.062)
Two-year College Graduates	-0.077***	-0.125*	-0.124	-0.258***	-0.258***
	(0.019)	(0.076)	(0.076)	(0.076)	(0.076)
University Graduates	-0.175***	-0.234**	-0.233**	-0.429***	-0.429***
	(0.027)	(0.097)	(0.097)	(0.095)	(0.095)
Year2000	-0.013***				
	(0.002)				
Year2001	-0.028***			-0.042***	-0.042***
	(0.002)			(0.011)	(0.011)
Year2002	-0.064***				
	(0.003)				
Year2004	-0.048***	-0.009	-0.009		
	(0.007)	(0.010)	(0.010)		
Year2005	-0.041***				
	(0.007)				
Year2006	-0.041***				
	(0.007)				
R-squared	0.057	0.054	0.054	0.073	0.073
N	247281	59027	67671	64810	64810

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at establishment level are in parentheses.

In Column 4, we use the 2002 bonus-to-salary ratio as $Treatment_i$, and the average bonus-to-salary ratio during 1999–2002 in Column 5. Both of the estimated coefficients on $After_t \times Treatment_i$ are almost zero and insignificant, suggesting that it is unlikely that the estimated coefficient on $After_t \times Treatment_i$ is confounded by a possible permanent structural shift in trends in employment coinciding with the 2003 reform.

Establishments with extremely low or high bonus-to-salary ratios might behave differently from others, and these extreme outliers could contaminate the estimated coefficients. To address this criticism, we implement subsample analyses using establishments within the range of 10 to 90 (and 25 to 75) percentiles of all bonus-to-salary ratios and re-estimate impacts of the 2003 reform on employment. Columns 1 and 2 in Table 6 provide further evidence of the robustness of the estimated coefficients on $After_t \times Treatment_i$. Indeed, the estimated coefficients are larger than those obtained using all samples. The key is that the results are still significantly negative without the contamination caused by outliers.

A threat to identification would arise if our treatment and control groups experienced different time trends in employment over the same period of the 2003 reform. This would cause our regression to erroneously identify a policy effect contaminated by the differences in the time trends between the two groups during the same period of the 2003 reform. We try specifications allowing different time trends between the treatment and control groups following Li et al. (2016). Column 3 in Table 6 includes a linear time trend variable and its interaction term with $Treatment_i$. In this specification, the coefficient for the time trend captures the baseline trend effects with the value of $Treatment_i$ equal to zero, that is, the time trend of control group.

Table 6. *Robustness check for employment regression*

	(1)	(2)	(3)	(4)
	Sub-samples		Allow for Treatment trend	
	10–90	25–75	Allow for Treatment	Allow for Treatment
$After_t \times Treatment_{i(02)}$	-0.016***	-0.018***	-0.007**	-0.010**
	(0.004)	(0.007)	(0.003)	(0.004)
Female	0.532***	0.557***	0.471***	0.463***
	(0.070)	(0.090)	(0.062)	(0.062)
Experience	0.008***	0.009**	0.008***	0.008***
	(0.003)	(0.004)	(0.003)	(0.003)
$Experience^2/100$	-0.011*	-0.015	-0.009	-0.009*
	(0.006)	(0.009)	(0.005)	(0.005)
Tenure	-0.054***	-0.053***	-0.053***	-0.054***
	(0.003)	(0.004)	(0.003)	(0.003)
$Tenure^2/100$	0.115***	0.115***	0.114***	0.116***
	(0.010)	(0.012)	(0.008)	(0.009)
High School Graduates	-0.087***	-0.118***	-0.079***	-0.078***
	(0.026)	(0.035)	(0.022)	(0.022)
Two-year College Graduates	-0.138***	-0.190***	-0.121***	-0.121***
	(0.034)	(0.045)	(0.028)	(0.029)
University Graduates	-0.221***	-0.334***	-0.218***	-0.215***
	(0.046)	(0.058)	(0.039)	(0.039)
Year2000	-0.008***	-0.007**	-0.002	0.005
	(0.003)	(0.003)	(0.003)	(0.006)
Year2001	-0.001	0.003	0.012***	0.023**
	(0.004)	(0.004)	(0.004)	(0.009)
Year2002	-0.056***	-0.048***	-0.037***	-0.025**
	(0.004)	(0.005)	(0.005)	(0.012)
Year2004	-0.029**	-0.012	-0.018***	-0.003
	(0.012)	(0.021)	(0.005)	(0.006)
Year2005	-0.030**	-0.005	-0.009**	
	(0.012)	(0.021)	(0.004)	
Year2006	-0.029**	-0.006		
	(0.012)	(0.021)		
Time-trend				-0.016*
			(0.002)	(0.010)
$Treatment_{i(02)} \times$Time-trend			0.000	-0.001
			(0.001)	(0.001)
$Time\text{-}trend^2$				0.001
				(0.001)
$Treatment_{i(02)} \times Time\text{-}trend^2$				0.000
				(0.000)
R-squared	0.064	0.066	0.067	0.064
N	90000	56253	112498	112498

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at establishment level are in parentheses.

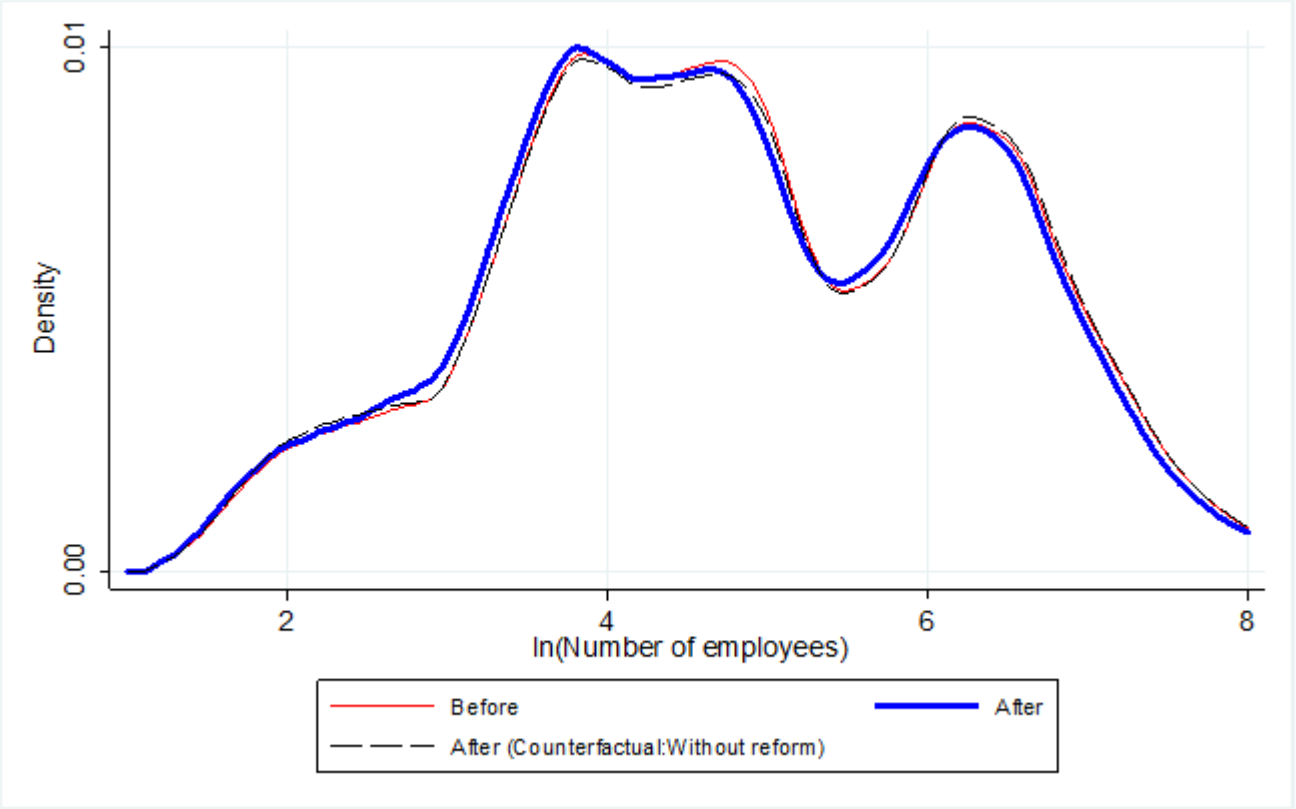
In contrast, the coefficient for the interaction captures how the time-trend effects vary depending on $Treatment_i$.

Column 4 allows the time trend function to be more flexible than linear, that is, quadratic. Fortunately, these assumptions appear to have little effect on our estimates. Columns 3 and 4 in Table 6 show that we consistently find a significantly negative effect of the reform on employment even when we control for the differences in time trends between the treatment and control groups.

C. Before and after distribution: DFL results

Figure 2 present the results of DFL decompositions of the number of employees using balanced panel data in 2002 and 2004. The thinner solid line represents the actual distribution of the number of employees in 2002 (before the reform), the bold line represents the actual distribution of the number of employees after the reform in 2004 (after the reform), and the dashed line represents a counterfactual distribution that would have been realized in 2004 if the 2003 reform had not occurred. After the introduction of the total reward system in 2003, the overall distribution of employment size shifted to the left, which reconfirms the decrease in employment after the 2003 reform as stated previously in Tables 2 and 3. The counterfactual distribution indicates that employment size would have been distributed at a higher level without the reform. Note that the actual "before" line and the counterfactual line almost overlap, which means that the change in attributes of establishments is not the main factor of the leftward shift of the distribution. Instead, the gap between the actual "after" line and the counterfactual line is negligibly small, meaning that the gap represents the impacts of the 2003 reform, which is consistent with the DID results.

Figure 2. *DFL results for the number of employees within an establishment*

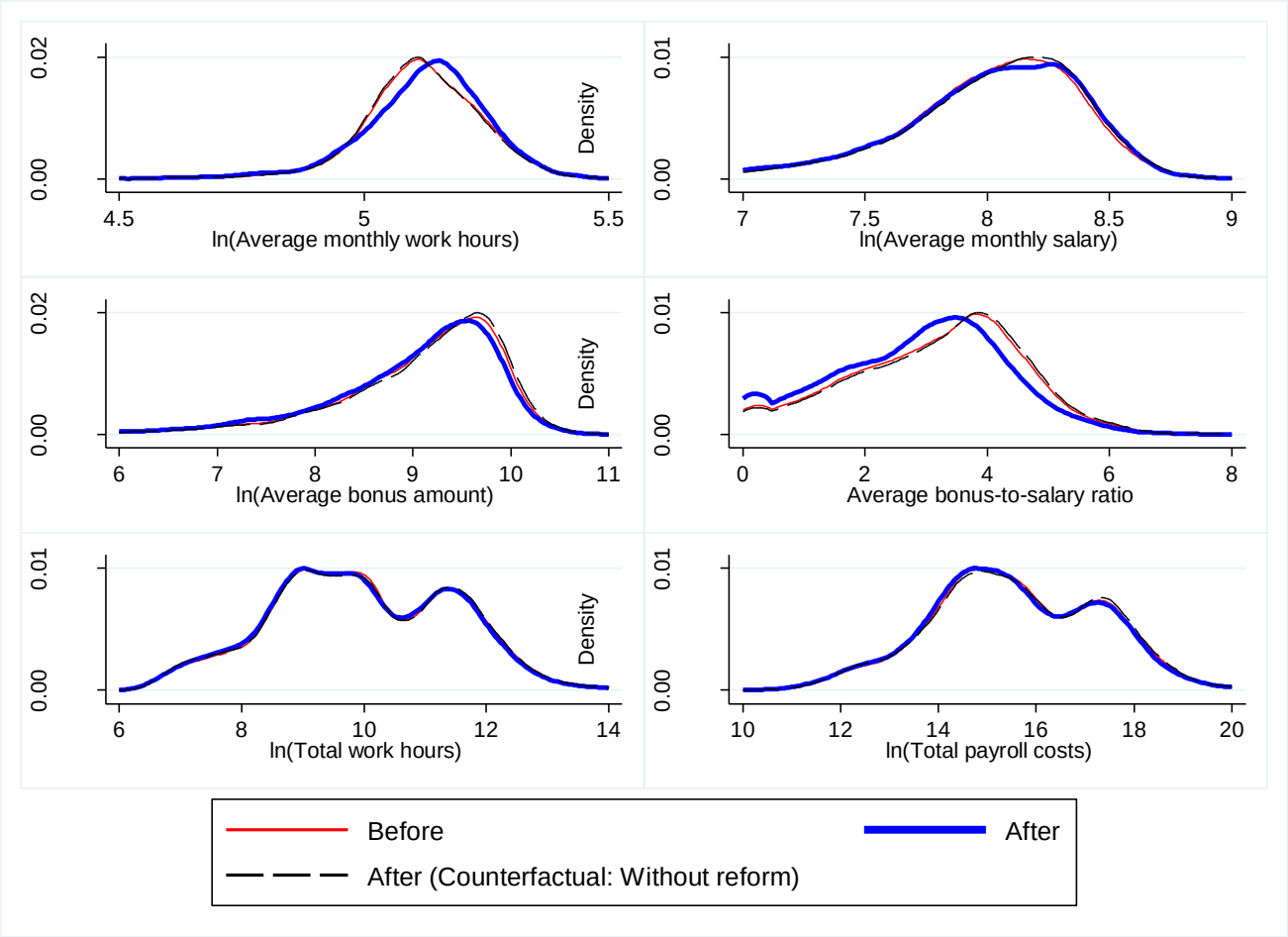


Note: The thinner sold line is the kernel density of ln(Number of employees) in 2002, and the heavier line is that after the reform. The dashed line is the counterfactual distribution that would have been realized in 2004 if the 2003 reform had not occurred. Explanatory variables in the probit regression to calculate ω in Equation (5) include female employee ratio, average tenure and its square, average experience in years and its square, proportion of graduates from each level of school (junior high school, senior high school, two-year-college, and four-year-university), the logarithm of firm size, and industry dummies.

Next, Figure 3 shows what happened to distributions for other variables before and after the reform. According to the two figures on the top row, we can say that, after the reform, the distributions of work hours and hence monthly salary shifted to the right. This reconfirms the increase in monthly salaries along with the increase in work hours after the 2003 reform, which is consistent with DID results. In contrast, when we look at the distributions of ln(Average bonus amount), the distribution shifted to the left after the reform, which is consistent with the balanced panel data in Tables 2. The bonus-to-salary ratio greatly

decreased and its distribution shifted left, perhaps in response to the 2003 reform, which made paying bonuses more costly. What is important and common to the four figures in the first and second rows in Figure 3 is that the gap between the "after" distributions and counterfactual distributions implies an impact of the 2003 reform.

Figure 3. *DFL results for various variables*



Note: The same note applies as in Figure 2, except that Figure 3 includes DFL results for various variables such as ln(Average monthly work hours), ln(Average monthly salary), ln(Average bonus amount), Average bonus-to-salary ratio, ln(Total work hours), and ln(Total payroll costs).

Finally, we check what happened to the total work-hour distribution and the total payroll cost distribution in the bottom row. Compared to the other four figures, it is obvious that the actual two distributions overlap for these two variables. Recall that average work hours increased and employment

within each establishment decreased. The offset of the increase in the average work hours and the decrease in the number of employees leads to keeping the total work hours unchanged. The same thing occurred to total payroll costs as well: average annual earnings increased, but the number of employees decreased.

Though the DFL results are completely consistent with our results obtained from the DID with fixed effects, they should be interpreted with caution, as the DFL decomposition analyses do not strictly estimate the policy effects. However, the results for the direction of movement of each variable are very robust, and the gaps between the two lines are explained by structural effects, which imply the impact of the reform.

In sum, our estimates suggest that the increase in the burden on workers was canceled out by higher monthly salaries resulting from longer work hours. Since the 2003 reform has the characteristic of making paying bonuses more expensive, the bonus amount decreased after the reform. Thus, the bonus-to-salary ratio greatly decreased after the reform. In contrast, firms managed to pay the salary increase by cutting the number of employees, which kept total wages paid to "surviving" workers unchanged. There are two possible pathways for the decrease in bonuses. One explanation is that the increase in monthly salaries through increasing working hours offset the decrease in bonuses within limited resources. The other comes from a drop in performance of companies due to increased labor costs without productivity gains. Unfortunately, at present we cannot settle the matter due to data constraints. The mechanism remains to be discussed as a future issue.

D. Discussion

D.1 Who bears the burden of social insurance contributions: Employers or employees?

According to Table 1, workers pay about 11% (13.58% ÷ 2 + 8.20% ÷ 2) of the annual salary as social insurance premiums after the reform, because in Japan the insurance premiums are split into equal shares borne by employers and employees. In addition, according to theoretical calculations, compared to the period before the 2003 reform, an increase in the bonus-to-salary ratio by one month resulted in 6%[14] more social insurance premiums after the reform.

With the same employment and salary as before, firms with a one-month more bonus-to-salary ratio would experience an increase in the insurance premiums paid by 0.7% (0.11 × 0.06 = 0.007) after the reform. Moreover, there is also another half consisting of a 0.7% increase in the social insurance premium burden imposed on employers, which is not included in wages paid in this analysis. Thus, with the same employment and salary as before, the overall labor costs for firms with a one-month more bonus-to-salary ratio should rise by 1.4%.

We will now interpret our empirical results based on the theoretical calculation above. If the coefficient of the total wage payment was zero, the increase in the burden imposed on workers would be completely borne by the workers, and the increase in the burden imposed on the employers would be completely borne by the employers. In another case, if the coefficient of the total wage payment was -0.007, workers would bear all of the increased burden imposed on both the workers and the employers.

[14] Calculation details are in the Appendix.

Our estimated coefficient of ln(Annual earnings), 0.007, shows that the increase in the burden imposed on "surviving" workers is financed by employers. On the other hand, the estimated coefficient of ln(Payroll costs) is negative and insignificant, suggesting the coincidence of wage raises and job cuts. In summary, the increase in the burden on workers is canceled out by higher monthly salaries resulting from longer working hours. Firms manage to pay this salary increase by cutting employment, which keeps total payroll costs paid to workers unchanged, while firms have to cover all of the remaining half of the increase in the burden imposed on firms by themselves. The 2003 reform triggered more work for surviving workers and job cuts.

D.2 Who cut more employment and why it happened after the 2003 reform

To gain insight on why firms with a heavier burden of social insurance premiums cut more employment, we split the samples into four groups: (1) large-sized manufacturing, (2) small and medium-sized manufacturing, (3) large-sized non-manufacturing, and (4) small and medium-sized non-manufacturing firms.[15] Table 7 shows that the estimated coefficients of employment on $After_t \times Treatment_i$ in all groups is negative and significant. The impacts of the reform are stronger in larger firms than in small and medium-sized firms. Large-sized manufacturing firms are hit hardest by the 2003 reform.

The diffusion index for employment, and specifically the index for excessive employment minus insufficient employment, according to the Bank of Japan's quarterly survey of business sentiment

[15] We define large firms as those with 300 or more employees, and small and medium-sized firms as those with less than 300 employees in our analysis.

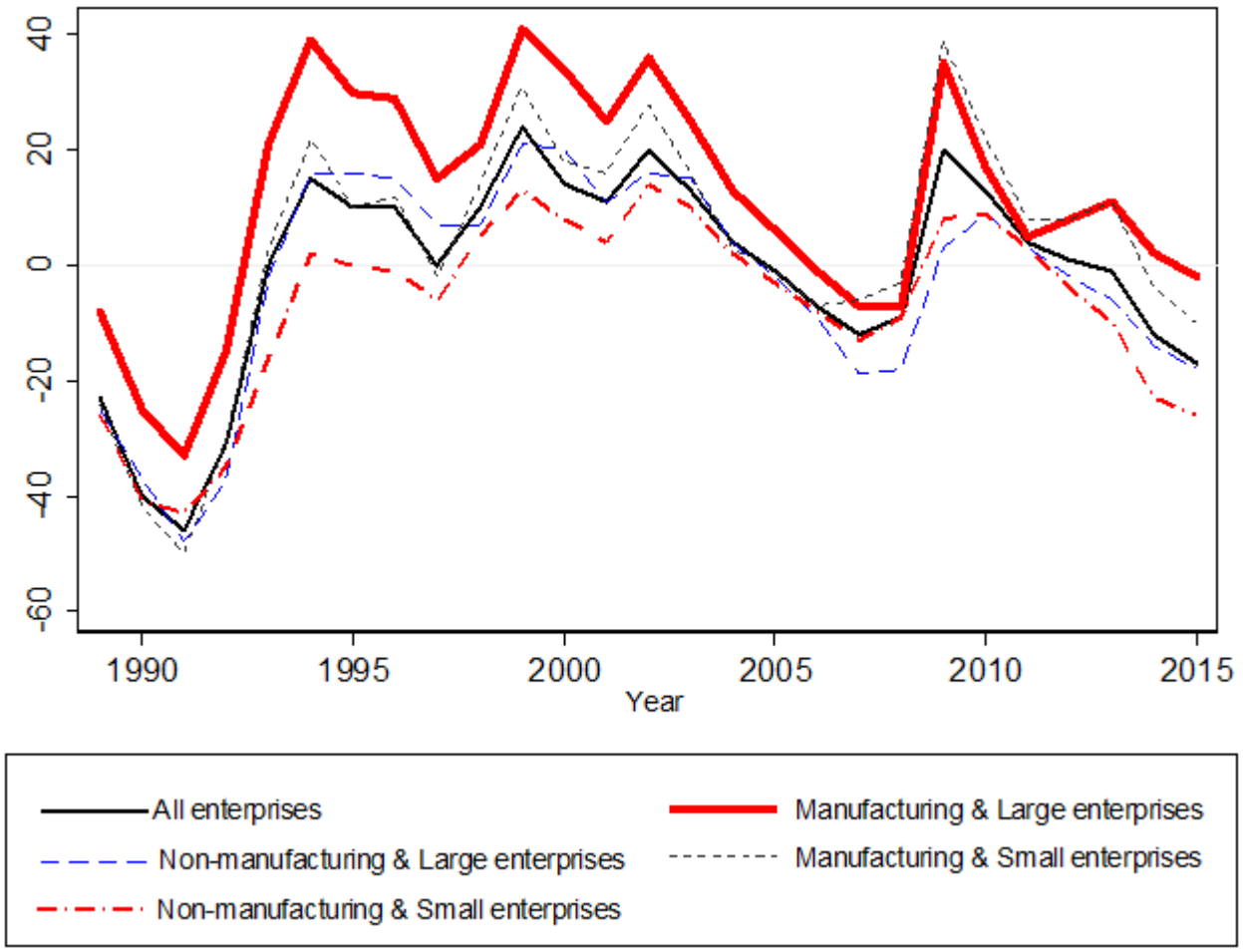
(Tankan) is presented in Figure 9.

Table 7. *Impacts of the reform on employment (sub-sample analysis by industry and size)*

	(1)	(2)	(3)	(4)
	Manufacturing		Non-manufacturing	
	Large-sized enterprises	Small and medium-sized enterprises	Large-sized enterprises	Small and medium-sized enterprises
$After_t \times Treatment_{i(02)}$	-0.030***	-0.004**	-0.016***	-0.003**
	(0.002)	(0.002)	(0.002)	(0.001)
Female	0.268	0.343***	0.726***	0.142**
	(0.187)	(0.052)	(0.118)	(0.058)
Experience	0.018**	-0.007**	0.005	0
	(0.007)	(0.003)	(0.005)	(0.003)
$Experience^2/100$	-0.048**	0.012**	0.005	0.003
	(0.020)	(0.005)	(0.014)	(0.005)
Tenure	-0.046***	-0.035***	-0.063***	-0.042***
	(0.007)	(0.003)	(0.006)	(0.003)
$Tenure^2/100$	0.109***	0.070***	0.129***	0.094***
	(0.019)	(0.007)	(0.018)	(0.009)
High School Graduates	-0.264***	-0.033*	-0.232***	-0.065***
	(0.049)	(0.017)	(0.059)	(0.022)
Two-year College Graduates	-0.303***	-0.044*	-0.307***	-0.065**
	(0.073)	(0.026)	(0.064)	(0.029)
University Graduates	-0.295***	-0.016	-0.425***	-0.142***
	(0.082)	(0.042)	(0.071)	(0.040)
Year2000	-0.011**	0.021***	0.004	0.009**
	(0.004)	(0.004)	(0.005)	(0.004)
Year2001	-0.023***	0.024***	0.016***	0.021***
	(0.005)	(0.005)	(0.006)	(0.005)
Year2002	-0.084***	-0.012***	-0.054***	-0.008**
	(0.007)	(0.004)	(0.006)	(0.003)
Year2004	-0.025***	-0.017***	-0.037***	-0.003
	(0.007)	(0.005)	(0.008)	(0.006)
Year2005	-0.033***	-0.015***	-0.037***	0.007
	(0.008)	(0.006)	(0.009)	(0.007)
Year2006	-0.023**	-0.006	-0.031***	0
	(0.009)	(0.006)	(0.009)	(0.007)
R-squared	0.117	0.285	0.086	0.2
N	19177	30238	42506	49513

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at establishment level are in parentheses.

Figure 9. *Diffusion index for employment*



Note: This figure shows the diffusion index for "excessive employment" minus "insufficient employment," which is calculated from Tankan data provided by the Bank of Japan. The Tankan defines large enterprises as those with 1,000 and more employees, and small enterprises as those with less than 300 employees.

The diffusion index for employment for large-sized manufacturing firms shows the worst results throughout the period. This is because, as Levine and Tyson (1990) suggest, Japanese workers, especially in large manufacturing firms, are guaranteed relatively higher job security through intra-firm transfers, temporary transfers to subsidiaries, and compression of wages. Japanese workers enjoy higher security of employment than those in the US or most EU countries, especially in large firms that have a risk-averse

decision-making system and strong norm consciousness to preserve employment.[16] In this environment in which it is hard to dismiss workers, it is natural for firms to engage in labor hoarding, which leads to a high diffusion index for employment especially for large-sized manufacturing.

The heterogenous results in Table 7 can potentially be explained by labor hoarding practices of Japanese firms. That is, the 2003 social insurance premium reform could have provided a good excuse for job cuts, especially for firms suffering from labor hoarding. Especially for large firms with a surplus workforce, national policy reform might provide a good reason to dismiss workers in a country such as Japan, where dismissals are rigorously regulated.

VI. Conclusion

In 2003, the total reward system for insurance premiums was introduced in Japan. This reform increased the insurance premiums for bonuses from 2.00% to 21.87%, and decreased the premiums for the monthly salary from 25.96% to 21.87%. The social insurance premium burden of firms with a high bonus-to-salary ratio increased as a result of the reform, while that of others decreased. These heterogeneous effects depending on the magnitude of the bonus-to-salary ratio in the year before 2003 function as an exogenous natural experiment, which allows us to estimate the impacts of the increased social insurance premium burden on various labor outcomes, such as employment, working hours, and payroll costs.

[16] Dismissals are rigorously regulated in Japan. Supreme Court rulings permit employers to dismiss employees only if there are objectively reasonable grounds, which are quite stringent (Sugeno and Yamakoshi 2014).

This paper uses unique employer-employee matching data to provide new evidence on the possible effect on employment of an increase in labor costs without productivity gains. Our results indicate that firms suffering from the 2003 social insurance premium reform reduced the number of employees, even after controlling for establishment fixed effects. Our key results change little when we control for time-variant labor demand idiosyncratic shocks. We also try a specification allowing for different time trends between the treatment and control groups, where a treatment-specific time trend term is included, to check for a violation of the common trend assumption.

Our estimates suggest that the increase in the burden on workers is canceled out by higher monthly salaries resulting from longer working hours. Firms manage to finance this increase in paid wages by cutting the number of employees, and keep total wages paid to workers unchanged as a result. On the other hand, firms themselves have to bear all the remaining half of the increase in the burden imposed on firms.

Focusing on dismissals among firms, firms adhering to a labor hoarding policy were likely to fire more workers at the timing of the 2003 reform. National policy reform may provide a good reason to dismiss workers in countries such as Japan where dismissals are rigorously regulated. Thus, our findings imply that an exogenous increase in labor costs without productivity gains could trigger job cuts, especially in sectors and countries where dismissals are rigorously regulated. Our findings could raise important implications for many developed countries plagued by the conflict between increasing social insurance premium burdens and employment stability.

Appendix

Let A be the paid annual earnings of workers, and r the bonus-to-salary ratio. The fraction of the monthly salary among annual earnings can then be written as follows:

$$\text{Annual salary amount} = \text{A}\times\frac{12\times\text{Monthly salary}}{Bonus + 12\times\text{Monthly salary}} \quad \text{(A1)}$$

By dividing both numerator and denominator by monthly salary, we obtain:

$$\text{Annual salary amount} = \text{A}\times\frac{12}{r + 12} \quad \text{(A2)}$$

Similarly, the fraction for the bonus among annual earnings can be written as:

$$\text{Annual bonus amount} = \text{A}\times\frac{Bonus}{Bonus + 12\times\text{Monthly salary}} \quad \text{(A3)}$$

Again, by dividing both numerator and denominator by monthly salary, we obtain:

$$\text{Annual bonus amount} = \text{A}\times\frac{r}{r + 12} \quad \text{(A4)}$$

We write the insurance premium burden placed on the workers' side before the reform as follows:

$$Worker's\ Burden_{before} = \text{A}\times\frac{12}{r + 12}\times\frac{0.1735 + 0.085}{2} + \text{A}\times\frac{r}{r + 12}\times\frac{0.01 + 0.01}{2}$$

$$= 0.12925\frac{12A}{r + 12} + 0.01\text{A}\times\frac{r}{r + 12} \quad (A5)$$

The numbers 0.1735 and 0.085[17] represent welfare insurance premium rates on salary and health insurance premium rates on salary before the reform, respectively, and 0.01 represents both welfare

[17] These numbers are presented in Table 1.

insurance premium rates and health insurance premium rates on bonuses before the reform.

Similarly, the insurance premium burden placed on the workers' side after the reform can be written as:

$$Worker's\ \ Burden_{after} = A\times\frac{0.1358\ \ +\ \ 0.082}{2} = 0.1089\text{A} \qquad (A6)$$

To assess the percentage change from before to after the reform, we calculate as follows:

$$\Delta = \ln\left(Worker's\ \ Burden_{after}\right) - \ln\left(Worker's\ \ Burden_{before}\right) = \ln\left(\frac{Worker's\ \ Burden_{after}}{Worker's\ \ Burden_{before}}\right)$$

$$= \ln(0.1089\text{A}) - \ln\left(\frac{A}{r+12}(0.12925\times 12 + 0.01r)\right)$$

$$= \{\ln(0.1089) + \ln \text{A}\} - \left\{\ln A\ - \ln\left(\frac{0.12925\times 12 + 0.01r}{r+12}\right)\right\}$$

$$= \ln(0.1089) - \ln\left(\frac{0.12925\times 12 + 0.01r}{r+12}\right)$$

$$= \ln\left(\frac{0.1089}{(0.12925\times 12 + 0.01r)/(r+12)}\right)$$

$$= \ln\left(\frac{0.1089(r+12)}{0.12925\times 12 + 0.01r}\right) \qquad (A7)$$

As the DID coefficient on $After_t \times Treatment_i$ represents an increased burden due to increasing the bonus-to-salary ratio by one month, we increase r, the bonus-to-salary ratio, in increments of one. The results in Table A1 show that if the bonus-to-salary ratio increases by one month, the increased burden becomes higher by 6%.

Table A1. *Change in burden according to bonus-to-salary ratio*

r	$\frac{Worker's\ Burden_{after}}{Worker's\ Burden_{before}}$	Difference
0	0.84255319	-
1	0.90691864	0.06
2	0.97046467	0.06
3	1.0332068	0.06
4	1.0951603	0.06
5	1.1563398	0.06
6	1.2167598	0.06

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