RIETI Discussion Paper Series 17-E-093

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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RIETI Discussion Paper Series 17-E-093
June 2017

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 variations in labor cost increases due to Japan's 2003 social insurance premium reform as a natural experiment, we estimate the impacts of the increased social insurance premiums on employment, working hours, and payroll costs. Using the difference in differences (DID) method with establishment fixed effects, we find that firms reduce the number of employees and increase average annual earnings from longer working hours in response to an exogenous increase in labor costs without productivity gains. Firms manage to pay for this increase in the average wage paid to the remaining workers by reducing the number of employees to keep total payroll costs unchanged. In contrast, since social insurance premiums are shared equally between employees and employers, firms pay the remaining half of the premiums with which that they are imposed. Our results imply that an increase in labor costs without productivity gain may reduce employment.

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

[#] We wish to thank Shun-ichiro Bessho, Yuji Genda, Junya Hamaaki, Yoshio Higuchi, Seiichi Inagaki, Tomohiko Inui, Beata Javorcik, Ryo Kambayashi, Takao Kato, Daiji Kawaguchi, Ayako Kondo, Masayuki Morikawa, Akira Nagae, Fumio Ohtake, Hiroyuki Okamuro, Hideo Owan, Kazuyasu Sakamoto, Hitoshi Shigeoka, and Masayuki Takahara, Kotaro Tsuru, Makoto Yano, as well as participants at the Tokyo Labor Economics Workshop, Japanese Economic Association, the microdata seminar at Hitotsubashi University, and RIETI seminars, who provided invaluable advice. This study is conducted as a part of the Project "Change in the Utilization of and Investment in Human Resources" undertaken at the Research Institute of Economy, Trade and Industry (RIETI) and supported by the Joint Usage and Research Center, the Institute of Economic Research, Hitotsubashi University.
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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 negative effect 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 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 2003 reform on various labor-related outcomes such as employment, hours worked, and total payroll costs. In response to the reform, the

[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.

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. 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." 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 labor costs.

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. The 2003 reform increased the insurance premiums for firms with a higher bonus-to-salary ratio more drastically if they paid exactly the same salary and bonuses even after the reform. Even if firms became aware of the reform before 2002, they should not have changed their bonus-to-salary ratio until 2002 because they would have had to pay more in insurance premiums until 2002. In contrast, after the 2003 reform, firms should not have an incentive to change their bonus-to-salary ratio because they could not save the premium burden by changing their bonus-to-salary ratio when 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, Kawaguchi and Mori 2013, Miyazato and Ogura 2010, Sakai 2009, Kawaguchi et al. 2008, and Kim 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. Nunziata (2005) shows that labor market regulations could explain a great part of the labor-cost rise in OECD countries from 1960 to 1994. Related studies including Kugler and Kugler (2009), Autor et al. (2004), and Angrist (2000) 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, Benito and Hernando (2008) find that a 5 percentage point reduction in the payroll tax by the 1997 policy reform in Spain is associated with an 8% increase in permanent labor demand. Eissa and Liebman (1996) examined the impact of the U.S. Tax Reform Act of 1986 (TRA 86), which included an expansion of the earned income tax credit on female labor supply. They used the difference-in-difference estimation to compare the change in labor supply of single women with children with the change for single women without children, and they found that the Tax Reform Act increased labor force participation among single women with children. Furthermore, Blundell et al. (1998) examined the impact of the tax reforms in the United Kingdom on labor supply during the 1980s by comparing the labor supply responses over time for different groups defined by cohort and education level. They found positive and moderately sized wage elasticities and 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 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 (Müller and Neumann 2016, Hamaaki 2012,[3] Kugler and Kugler 2009, Iwamoto and Hamaaki 2009, Tachibanaki and Yokoyama 2008, Hamaaki and Iwamoto 2008, Sakai and Kazekami 2007, Sakai 2006, Komamura and Yamada 2004, Anderson and Meyer 2000, Gruber 1997, Holmlund 1983, and Hamermesh 1979). The results are mixed, as pointed out in the meta-analysis by Melguizo and González-Páramo (2013). Some find that employees pay a substantial amount of the social insurance premiums, and others describe employers financing the premiums.[4]

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

[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 that would have been realized if the 2003 reform had occurred before 2003 as the proxy variable of the actual premium rate before the reform. In contrast, 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.

[4] Iwamoto and Hamaaki (2006) discussed the mechanism of the incidence of social insurance contributions

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 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 labor 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.

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

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theoretically.

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] In 2003, the standard for calculation of the social insurance premium rate changed uniformly, but the effect varied depending on the bonus-to-salary ratio.

As can be confirmed in Table 1, before the reform, the social insurance premium rate for bonuses had been set to 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.

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.

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, 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 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:

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

where y_{it} represents outcomes for establishment i in year t, such as employment, average hours worked, total working hours in the establishment, the average annual earnings, total payroll costs in the establishment,[6] We drop the data in 2003, treating 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. $Treatment_i$ 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] The estimated coefficient of $After_t \times Treatment_i$ is of prime interest, and the negative and significant coefficient indicates that the 2003 reform has negative impacts

6 Average annual earnings and total payroll costs are deflated by CPI of which base year is 2010.
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.

on firms with a heavier burden of social insurance premiums. 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. To control for macro shocks common to all establishments, we also include year fixed effects.

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 working hours 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]

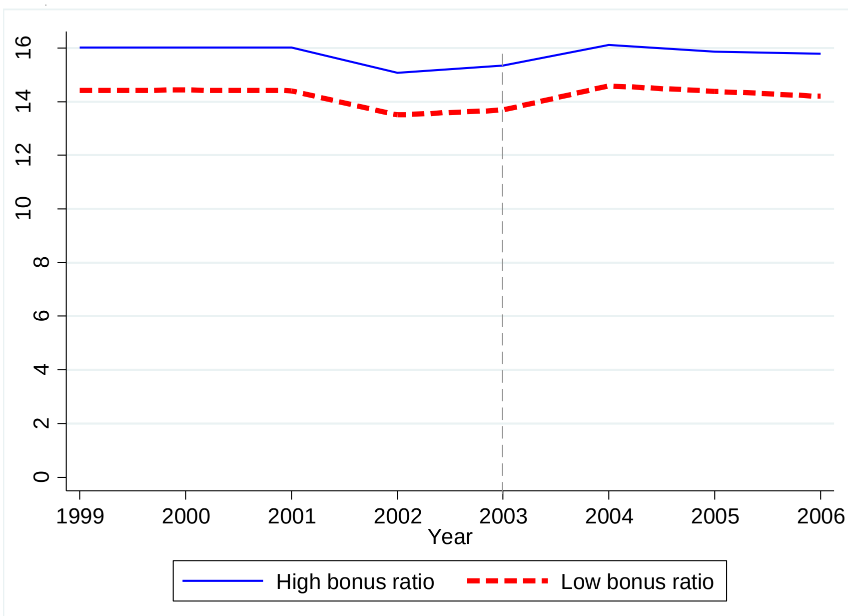
For further discussion, we split the sample firms into four groups: (1) large-sized manufacturing, (2)

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

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 in the latter section.

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.

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:

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

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:

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

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:

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

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:

[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.

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

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 covers almost all industries except 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 covers 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. 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

[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.

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 having one month greater bonus-to-salary ratio in 2002 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.

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 1*a* and 1*b*, 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 these results, indicating that employment decreased in response to the 2003 reform, is consistent with what we confirmed from the descriptive statistics (Table 2).

Table 3. *Impacts of the reform on employment*

	(1)	(2)	(3)
Dependent variable: ln(Number of employees)	Baseline	Control for total labor demand	Control for time-variant establishment effects
$After_t \times Treatment_{i(02)}$	-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 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 one month greater bonus-to-salary ratio before the reform 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 hours)	(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 proxy of the magnitude 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 use an alternative proxy, that is, 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]

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 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 numbers of observations are larger than in the main regressions. In other words, thus far, establishments 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. This means that, 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.

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.

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

	(1)	(2)	(3)	(4)	(5)
	1999–2002 vs. 2004–2006	2002 and 2004		Placebo tests: 1999 and 2001	
	Treatment defined as 1999-2002 Bonus-to-salary ratio	Treatment defined as 2002 Bonus-to-salary ratio	Treatment defined as 1999-2002 Bonus-to-salary ratio	Treatment defined as 2002 Bonus-to-salary ratio	Treatment defined as 1999-2002 Bonus-to-salary ratio
$After_t \times Treatment_{i(02)}$		-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.

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. 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.

Table 6. *Robustness check for employment regression*

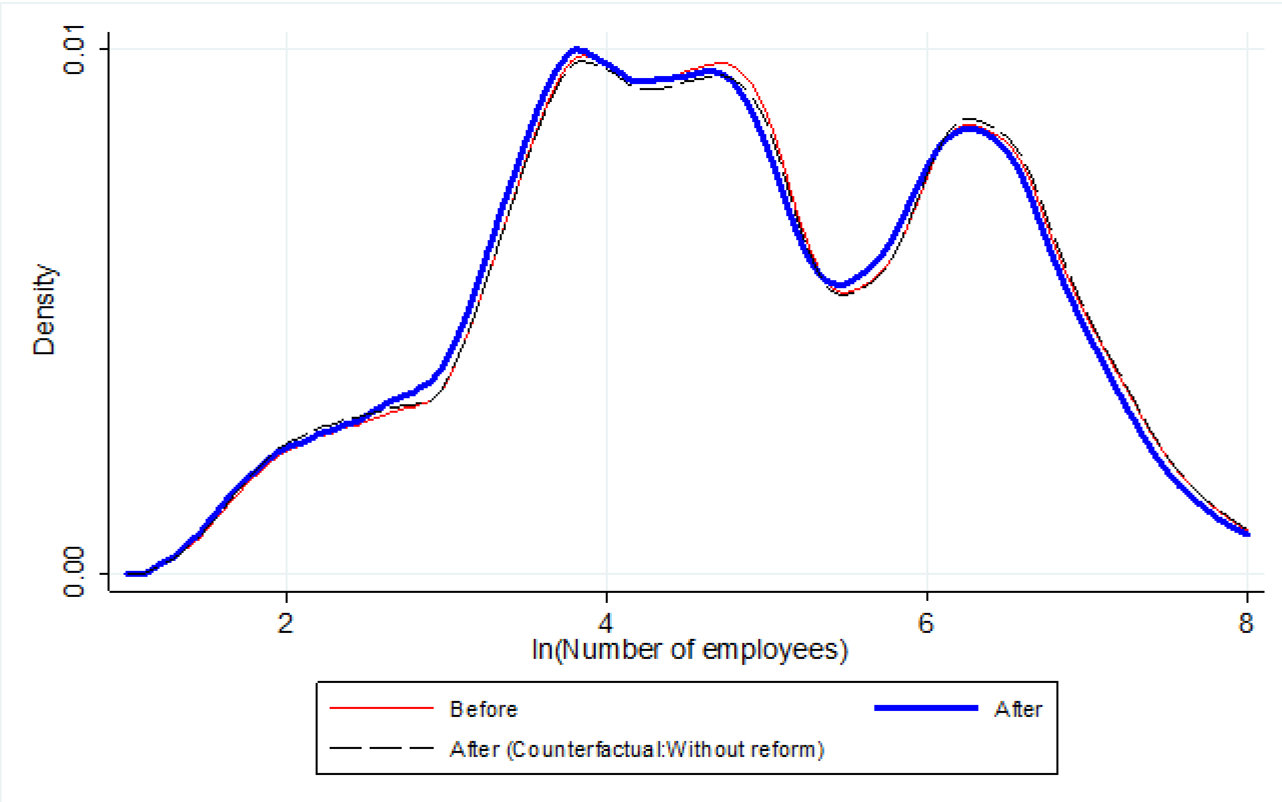
	(1)	(2)	(3)	(4)
	Sub-samples		Allow for Treatment trend	
	10–90 PCT percentiles	25–75 PCT percentiles	Allow for Treatment trend (linear trend)	Allow for Treatment trend (quadratic trend term is included)
$After_t \times Treatment_{i(02)}$	-0.016***	-0.018***	-0.007**	-0.010**
	(0.004)	(0.007)	(0.003)	(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			-0.008***	-0.016*
			(0.002)	(0.010)
$Treatment_{i(02)} \times$Time-trend			-0.0004	-0.001
			(0.001)	(0.001)
Time-trend^2				0.001
				(0.001)
$Treatment_{i(02)} \times$Time-trend^2				0.0001
				(0.0002)
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.

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 not 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.

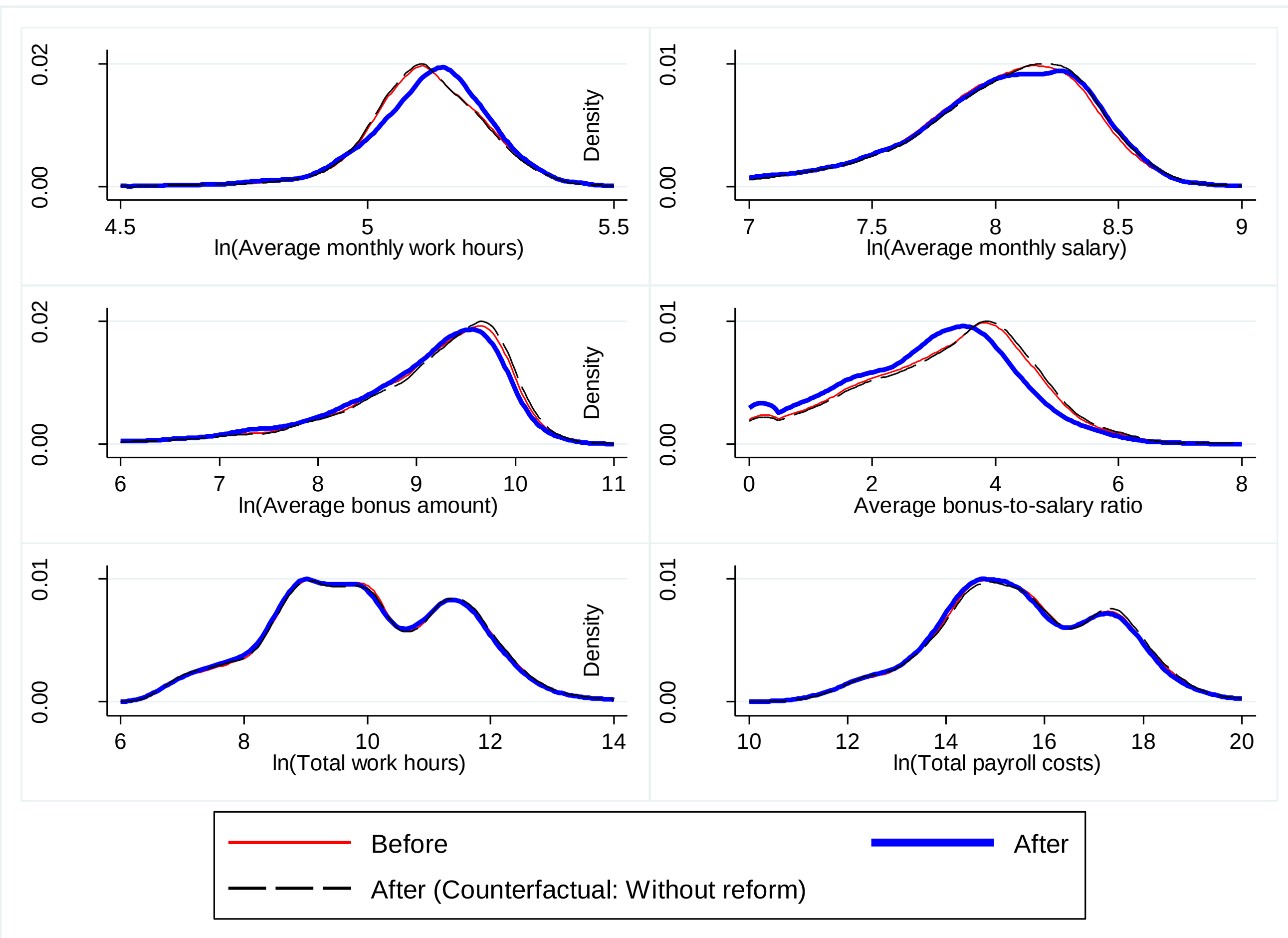
Next, Figure 3 shows what happened to distributions for other variables before and after the reform. According to the two figures on the top row, we can say that, after the reform, the distributions of work hours and hence monthly salary shifted to the right. This reconfirms the increase in monthly salaries 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.

Finally, we check 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.

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).

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 paid wages in this analysis. Thus, assuming 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% (i.e., 0.7%+0.7%=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.

Our estimated coefficient of ln(Average annual earnings), 0.007, shows that the increase in the

[14] Calculation details are in the Appendix.

burden imposed on "surviving" workers is financed by employers. On the other hand, the estimated coefficient of ln(Total 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.

VI. Conclusion

In 2003, the total reward system for insurance premiums was introduced in Japan. The reform produced more social insurance premium burden on firms with a high bonus-to-salary ratio, while less on others. 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[15].

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

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[15] According to Bewley (1999), who conducted a large field survey on nominal downward wage rigidity in the US, the key reason for a firm's reluctance to cut wages is the belief that nominal wage cuts damage worker morale. His survey also reveals that 60% of firms believe that layoffs increase productivity because of selection effects, Furthermore, due to the negative beliefs about the impact of wage cuts on workers' morale, layoffs are likely to be preferred in many cases. Using Japanese panel data from the Keio Household Panel Survey (KHPS), Yokoyama (2014) empirically showed that the evidence revealed by Bewley (1999) also explains recent Japanese labor market well.

allowing for different time trends between the treatment and control groups, where a treatment-specific time trend 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. 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:

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

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

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

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

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

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

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

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

$$Worker's\ Burden_{before} = \mathrm{A} \times \frac{12}{r + 12} \times \frac{0.1735\ +\ 0.085}{2} + \mathrm{A} \times \frac{r}{r + 12} \times \frac{0.01\ +\ 0.01}{2}$$
$$= 0.12925 \frac{12A}{r + 12} + 0.01\mathrm{A} \times \frac{r}{r + 12} \qquad (A5)$$

The numbers 0.1735 and 0.085[16] represent welfare insurance premium rates on salary and health insurance premium rates on salary before the reform, respectively, and 0.01 represents both welfare

16 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:

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

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

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

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

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

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

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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