The Impact of Corporate Governance Reforms on Distribution Inequality: Micro Evidence from Japan

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First version: March 19, 2024
Current version: December 25, 2024

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Abstract

The distribution inequality between increasing shareholder payouts and stagnant wages has recently attracted attention. Only a few studies, however, have explored the factors driving this phenomenon. This study examines whether national-level corporate governance reforms worsen this inequality using unique firm-level wage data and an exogenous institutional change in Japan. Employing a difference-in-differences methodology, I find that Japanese corporate governance reform increases distribution inequality by boosting payouts, particularly share repurchases, while having little impact on wages. The findings indicate that corporate governance reforms, which are often claimed to support sustainable growth, might have created economic inequality.

Keywords: Corporate governance, Governance reforms, Payout policy, Distribution inequality

JEL Classifications: G34, G35, G38, L51

1. Introduction

Rising distribution inequality between shareholders and workers, which is defined here as a significant discrepancy between increasing corporate payouts (dividends and share repurchases) and stagnant wages, has garnered attention from both researchers and practitioners (see, e.g., Financial Times, 2014; Lazonick, 2014; The Nation, 2018; Bivens and Mishel, 2015; IN THESE TIMES, 2022). For example, a recent Oxfam report (Mariotti, 2019) shows that between 2011 and 2017, dividends in G7 countries grew ten times faster than wages. In the US, shareholder returns also increased significantly (The Washington Post, 2019), promoting Senators Chuck Schumer of New York and Bernie Sanders of Vermont to assert that "…[w]hat's clear to the vast majority of Americans is that companies should devote resources to workers and communities before buying back stock" (The New York Times, 2019).

This disparity would be less concerning if all people owned an equal proportion of stocks and workers received the same proportion of shareholder returns in addition to their wages (Piketty, 2014). In reality, however, top earners own the majority of financial assets, including stocks (CNBC, 2021; Cabinet Office, 2022), and thus receive the largest share of payouts.[1] Therefore, distribution inequality between shareholders and workers is closely tied to income inequality. Despite its importance, empirical evidence exploring the factors driving this phenomenon remains limited.

[1] For example, the wealthiest 10% of American households own 89% of all U.S. stocks (CNBC, 2021), and the wealthiest 10% of Japanese households own 60% of dividend and interest income (Cabinet Office, 2022).

Since the late 1990s, a number of countries have launched corporate governance reforms (Fauver et al., 2017), defined as deliberate interventions in a country's corporate governance tradition by the state, security and exchange commission, or stock exchanges (Kim and Lu, 2013).[2] Although the precise nature and strength of these reforms might vary across countries, they all share the common objective of strengthening shareholder rights (Bae et al., 2021). This study examines the impact of these national-level corporate governance reforms on distribution inequality between shareholders and workers.

Corporate governance reforms often aim to protect shareholder rights to enhance shareholder value (Kim and Lu, 2013; Bae et al., 2021). In doing so, these reforms are likely to increase shareholder influence over firms (La Porta et al., 2000; Maffett et al., 2022).[3] This strengthened influence might lead firms to prioritize maximizing shareholder value and disregard claims from other stakeholders that might restrain shareholder wealth (Friedman, 2007). Previous studies have claimed that when the management is exclusively responsible for shareholder value maximization, the conflicts of interest between shareholders and stakeholders can cause the emergence of excessive payouts (Holder et al., 1998; Lie, 2005; Vermaelen, 2005; Hribar et al., 2006; Chu, 2018).[4] Thus, corporate governance reforms are likely to increase payouts by reallocating cash that might otherwise be used to address the interests of other stakeholders (Maxwell and Stephens, 2003; Vermaelen, 2005). Furthermore,

[2] For example, these reforms include promoting increased board independence and enhanced dialogue with shareholders.
[3] Maffett et al. (2022), analyzing nearly 7,000 shareholder activist campaigns across 56 countries, show that worldwide governance reforms induce shareholder activism.
[4] Acemoglu et al. (2022) find that shareholder-oriented managers are more likely to increase payouts. Ni et al. (2020) and Chronopoulos et al. (2023) show that firms pay out less when they consider the interests of other stakeholders.

if prioritizing shareholder value maximization depletes a firm's cash reserves, raising wages for workers becomes more challenging (Lazonick, 2014). In this case, corporate governance reforms could exacerbate distribution inequality between shareholders and workers through increased payouts.

On the other hand, empowered shareholders might also urge firms to increase both payouts and worker wages. Recent literature suggests that protecting stakeholders' interests can also enhance shareholder value (Jiao, 2010). For example, Edmans (2011) demonstrated a positive relationship between worker satisfaction and long-term shareholder value. Moreover, some institutional investors have called on firms to raise wages (Governance Intelligence, 2024; LGIM, 2024). In this scenario, governance reforms might not significantly affect distribution inequality and could even reduce it. In summary, the effect of governance reforms on distribution inequality between shareholders and workers is an empirical question.

However, conducting the empirical analysis requires addressing at least two major challenges. First, researchers often lack access to publicly available data on non-shareholders, including wages (Dittmann et al., 2023). Consequently, existing studies have primarily relied on data from firms that voluntarily disclose labor costs, questionnaire data, or industry average wage data (Faleye et al., 2013; Mueller et al., 2017; Uygur, 2019). Second, researchers need an exogenous institutional change in corporate governance to employ a shock-based research design for causal inference (Atanasov and Black, 2016).

The present study overcomes these challenges by leveraging a unique dataset and institutional setting in Japan. First, all listed firms in Japan are required to disclose the

average worker wage at the non-consolidated level in the "Status of Workers" section of their annual reports (Minister of Finance, 1973). This regulatory requirement provides access to credible firm-level wage data. Second, Japan offers a unique setting where the national-level corporate governance reform, the "Corporate Governance Code," was implemented differently across markets. Specifically, firms listed on the 1st and 2nd sections of the Tokyo Stock Exchange (treatment firms) were required to adhere to 5 general principles, 30 principles, and 38 supplementary principles, while firms listed on Mothers and JASDAQ of the Tokyo Stock Exchange (comparison firms) were subject to only the 5 general principles. This differential exposure to the governance reform allows this study to examine the effects of the national-level governance reform on distribution inequality between shareholders and workers.

Using a difference-in-differences (DID) research design, this paper finds that Japan's corporate governance reform increased the "shareholder-worker ratio" (the ratio of the sum of cash dividend payments and cash share repurchases to total wages) by approximately 100%. To make the parallel trends assumption credible, I first plot the annual trends of the outcome variable for treatment and comparison firms. I next conduct the event-study analysis and placebo tests. These tests find no significant effects in pre-treatment periods. The study next confirms that the reform increases distribution inequality by boosting payouts, particularly share repurchases, while having minimal impact on wages. Overall, the main findings support the prediction that corporate governance reforms raise distribution inequality between shareholders and workers.

To provide further evidence that governance reforms increase distribution inequality due to heightened shareholder influence rather than other factors, I conduct a mechanism analysis. If the reform exacerbates distribution inequality by strengthening shareholder influence over firms, its effects should be more pronounced in firms where shareholders are more likely to actively exercise their rights when empowered. I find that the treatment effects are indeed stronger in firms with active shareholders, thereby supporting the hypothesized mechanism. Additionally, I confirm that alternative mechanisms cannot fully account for the findings. Specifically, I show that the increase in distribution inequality is not attributable to improved corporate investment decisions, decreased cash flow uncertainty, increased corporate cash generating ability, or firm size expansion.

Additionally, I perform several robustness tests to strengthen the validity of the main results. First, I re-estimate the main analysis using alternative DID specifications such as doubly-robust DID and synthetic DID designs. Second, I re-estimate the baseline analysis while controlling for confounding events. Third, I conduct other robustness tests using a winsorized sample, additional treatment and comparison firms, firms with at least 50 workers, and alternative outcome measures. All results remained consistent with the original findings.

Further, I conduct supplementary tests to assess the validity of several possible arguments. First, the reform might push firms to hire more workers, positively impacting workers through new job creation. Second, the governance reform might facilitate capital reallocation, thereby contributing to innovation and economic growth. Third, the findings might primarily reflect firm-specific characteristics in Japan, limiting the external validity of this research.

However, supplementary tests provide no compelling evidence to support any of these arguments.

This study makes at least three key contributions. First, it contributes to the literature on the effects of corporate governance reforms. While prior research has provided valuable insights into the impact of these reforms on firm-level outcomes, including firm value (Fauver et al., 2017), cash holdings (Chen et al., 2020), payout policies (Bae et al., 2021), and audit fees (Kim et al., 2023), research on their impact on inequality among stakeholders remains scarce. This study expands our understanding of the societal impact of governance reforms by providing empirical evidence that they can increase distribution inequality between shareholders and workers.

Second, this paper is among the few that empirically explore distribution inequality between shareholders and workers. While this inequality has recently attracted attention in developed countries, most inequality research in corporate governance, finance, and accounting has focused on inequalities between workers (Mueller et al., 2017), between CEOs and workers (Dittmann et al., 2023), or between shareholders and bondholders (Maxwell and Stephens, 2003). Unlike these studies, this paper examines inequality between shareholders and workers and provides novel insights into the broader literature on inequality surrounding firms.

Third, this paper contributes to policy debates concerning the usefulness of corporate governance reforms. Many countries are implementing these reforms as public policy tools to achieve sustainable development that benefit all stakeholders (OECD, 2004; Financial Reporting Council, 2018). For instance, the code of corporate governance in Singapore states

that "[G]ood corporate governance is good for the company, with a well-governed company better placed to perform over the longer-term. … A sustainably successful company is good for myriad stakeholders: workers, suppliers, customers, shareholders, as well as society at large" (Monetary Authority of Singapore, 2018). However, this paper's findings suggest that these reforms might not promote sustainable growth and could instead lead to a significant wealth transfer from stakeholders to shareholders. Therefore, this study calls for a reassessment of the ongoing social debate on the true benefits of corporate governance reforms.

The remainder of the paper is organized as follows. Section 2 describes the institutional background. In Section 3, I present the data, summary statistics, and empirical strategy. I report the empirical findings in Section 4. The study addresses several possible arguments in Section 5. Finally, Section 6 concludes the study.

2. Institutional Background

Since World War II, the majority of Japanese firms' stocks were held by main banks and keiretsu firms.[5] Consequently, there has been limited external pressure from shareholders on firms to improve capital efficiency and profitability (Chattopadhyay et al., 2020). As a result, Japanese firms traditionally prioritized the interests of other stakeholders over those of shareholders (Yoshimori, 1995; Aoki et al., 2007).

[5] The term 'keiretsu' refers to a group of firms with long-term business relationships led by a core firm. These firms mutually held shares within the group, resulting in infrequent trading of their stocks on the open market (Johnston, 1998).

However, the collapse of the banking system after the bursting of the economic bubble and subsequent financial market reforms in the late 1990s significantly altered the shareholder composition of Japanese firms. Cross-shareholdings were reduced, and external shareholders began owning larger shares (Miyajima and Nitta, 2011). These changes gradually attracted attention to the concept of corporate governance, and improving firm profitability through strong corporate governance was seen as a strategy to revive Japan's stagnant economy.[6]

In response to this trend, former Japanese Prime Minister Shinzo Abe initiated government-led corporate governance reforms as part of a growth strategy.[7] One of the most important reforms is the Corporate Governance Code (hereinafter "CGC"), introduced in June 2015. The CGC is a set of "comply-or-explain" principles that serve as guidelines for corporate governance by listed firms. It comprises 5 general principles, 30 principles, and 38 supplementary principles. General principles outline universal ideals and goals to be realized through enhanced governance.[8] Principles and supplementary principles outline specific

[6] For example, the "Japan Revitalization Strategy 2014 - Japan's Challenge for the Future" (Prime Minister of Japan and His Cabinet, 2014) states: "To increase corporate profits through the improvement of productivity and ensure that the increased profits lead to an increase in wages, reinvestment, and the return to shareholders, it is important – primarily for global companies – to achieve sustainable increases in corporate value by considering the cost of capital and enhancing corporate governance."

[7] To achieve sustainable economic growth, he implemented the economic policy with three components ("three arrows"), which attracted attention from the media as "Abenomics". The first arrow is a bold monetary policy to combat deflation. The second arrow is a flexible fiscal policy to stimulate demand. The third arrow is a growth strategy to encourage private investment. Japanese corporate governance reforms were implemented as a part of the third arrow, with the objective to expand corporate profitability, promote industrial renewal, and ultimately foster the sustainable development of the Japanese economy.

[8] The five general principles include (1) Securing the Rights and Equal Treatment of Shareholders, (2) Appropriate Cooperation with Stakeholders Other Than Shareholders, (3) Ensuring Appropriate Information Disclosure and Transparency, (4) Responsibilities of the Board, and (5) Dialogue with Shareholders.

practices for firms to implement to achieve these goals. For example, these principles recommend increased information disclosure (Principle 3.1), the appointment of more outside directors (Principle 4.8), and dialogue with shareholders (Principle 5.1). Although most principles focus on strengthening shareholder influence over firms, the CGC also asserts that their implementation contributes to the sustainable development of the economy as a whole (Tokyo Stock Exchange, 2015). In practice, all principles were applied to firms listed on the first and second sections of the Tokyo Stock Exchange (hereinafter "TSE"), whereas only the general principles were applied to firms listed on Mothers and JASDAQ of the TSE. A detailed description of CGC can be found in Appendix B.

This paper examines the impact of Japanese corporate governance reform on distribution inequality by using the differences in exposure to CGC. Specifically, the study treats the general principles as a common shock and additional principles (principles and supplementary principles) as an exogenous shock in corporate governance. As shown in Figure 1, firms listed on the 1st and 2nd sections of the TSE are designated as treatment firms, while those listed on Mothers and JASDAQ are designated as comparison firms. This institutional setting allows the study to establish treatment and comparison groups within a single country, enabling the analysis while controlling for national-level institutional and historical factors.[9]

[9] There is often a tradeoff between internal and external validity. For example, many prior studies have examined the effects of corporate governance reforms using multinational comparisons (Fauver et al., 2017; Chen et al., 2020; Bae et al., 2021; Maffett et al., 2022; Kim et al., 2023). Their sample, comprising a wide range of countries, supports the universality and external validity of their results. However, multinational comparisons rely on the strong assumption that no country-specific events occur during the sample period except for the governance reform (Zhou et al., 2016). Given the frequent annual implementation of regulatory and corporate law changes in the securities markets, this assumption is

[Insert Figure 1 here]

3. Research Design

This section provides an overview of the data, summary statistics, and empirical strategy.

3.1 Data

The sample is constructed from several databases. I collect firms' financial data from Nikkei NEEDS Financial Quest. I also gather data on firms' listed markets and market capitalizations from the Japan Company Handbook CD-ROM. Finally, I obtain the data on workers' wages from Nikkei NEEDS Labor Situation.[10] I define the financial year t as the period from April of year t to March of year t + 1. The sample period is from 2010 to 2020, which is five years before and after the implementation of CGC in 2015. I exclude firm data in the financial year 2015 from the sample. This is because CGC started in June 2015 and it is complicated to identify pre-treatment and post-treatment within that period. A visual explanation of this process is provided in Figure 2.

difficult to validate (Jaeger et al., 2020). In contrast, this research can draw causal inference while controlling for country-specific shocks and events, as both treatment and comparison firms are drawn from a single country. However, since the results are country-specific conclusions, the external validity of this research is not guaranteed compared to that of multinational comparisons. The external validity of this study is discussed in Section 5.3.

[10] The wage data from 2010 to 2019 was collected from Nikkei NEEDS Labor Situation. The wage data for 2020 was manually collected from the annual reports of each firm.

[Insert Figure 2 here]

I manually collect data on each firm's ticker symbol and listed market from the 2011 to 2021 editions of the Japan Company Handbook CD-ROM.[11] The sample is narrowed down to firms that had been in existence for the entire sample period. I exclude firms that were listed on Sapporo, Nagoya, Fukuoka, and Osaka Stock Exchanges.[12]

The dataset is created by combining the data obtained from the above databases based on firms' ticker symbols. Firms in the financial, securities, and insurance industries are excluded from the sample. In addition, firms missing basic accounting information (net income, total assets, operating cash flow, liabilities, net assets, and sales) are excluded.[13] Finally, balanced panel data restricted to firms continuously observed during the sample period is generated and used for analysis. The final sample consists of 21,570 firm-year observations between 2010 and 2020 (observations in 2015 are excluded).

The main outcome variable is the "shareholder-worker ratio" (*Ratio*), which represents the ratio of the sum of cash dividend payments and cash share repurchases to total wages.[14] This variable indicates how many times the amount distributed to workers is distributed to shareholders. Therefore, if the reform increases distribution inequality between shareholders

[11] This study utilizes the summer issues of the Japan Company Handbook CD-ROM.
[12] I exclude firms listed on the Sapporo, Nagoya, and Fukuoka exchanges due to significant differences in market characteristics. However, these firms are included in the robustness checks. Additionally, I exclude firms listed on the Osaka Stock Exchange, as it was abolished following its merger with the TSE in 2013.
[13] I also exclude firms with missing wage data, zero wages, or positive payments for dividends or share repurchases.
[14] Total wages are calculated as the product of the average wage and the number of workers, based on non-consolidated data.

and workers, this ratio will increase significantly in treatment firms in the post-treatment period. Also, additional analyses use sum of cash dividend payments and cash share repurchases divided by total assets (*Payout*), natural logarithm of the average worker pay (*Log Wage*), dividends divided by total assets (*Div*), and share repurchases divided by total assets (*Rep*) as outcome variables. Control variables include natural logarithm of total assets (*Log TA*), natural logarithm of firm age (*Log Age*), return on assets (*ROA*), debt divided by total assets (*Lev*), and operating cash flow divided by total assets (*OCF*). Detailed definitions of all variables are provided in Appendix A.

3.2 Summary Statistics

Panel A of Table 1 reports pre-period summary statistics for the sample of firms utilized for the primary analyses. The six columns report the observations and distributional statistics (quartiles, mean, and standard deviation) of variables for the full pre-period sample. For better interpretability, I show the variables without log-transformation even if log-transformed forms are used in the analysis. The mean of shareholder-worker ratio is 0.86. This shows that, on average, firms distribute to shareholders 0.86 times the total amount distributed to workers. On average, corporate payout per 1 yen of total assets is 0.011 yen, and the average worker wage is 6 million yen. Also, an average firm has total assets of 321 billion yen, an age of 56 years old, a return on assets of 2.3%, a leverage of 49%, and an operating cash flow of 0.06 yen per total assets.

Panel B of Table 1 shows the mean, standard deviation, and median of each variable in the pre-treatment period by treatment and comparison firms. On average, shareholder-worker

ratio of treatment firm is more than six times higher than that of comparison firms. The difference is expected because comparison firms are still young, and thus are not active in shareholder returns. Additionally, it is worth noting that treatment and comparison firms differ on several important variables such as *TA* and *ROA*. These differences are expected since treatment firms are listed in the leading securities market and thus are subject to stricter monitoring by investors. Also, I report summary statistics for the variables used in mechanism analysis, robustness checks, and additional tests addressing possible arguments in Appendix Table E1.

[Insert Table 1 here]

3.3 Empirical Strategy

This section describes an empirical strategy for examining the impact of the governance reform on distribution inequality between shareholders and workers. Since each firm's exposure to the reform is not randomly assigned, constructing an appropriate empirical strategy is challenging.[15]

To overcome the issue, this study applies a difference-in-differences (DID) design. This design attempts to identify the effects of the reform by comparing the change in the average outcome of treatment firms with that of comparison firms. The main regression model is the

[15] Pre- and post-comparisons of outcome variables across treatment firms would be misleading since the difference includes the effects of year-specific events, such as a recession. A naive comparison of outcomes between treatment and comparison firms might also be misleading because those firms are likely to be different from the beginning.

following:

$$Y_{it} = f_i + \theta_{jt} + \mu_{kt} + \delta POST_{it} + \Omega X_{it} + \epsilon_{it} \quad (1)$$

Here Y_{it} is the outcome variable (*Ratio*) of firm i at year t. I control for unobserved time-invariant firm characteristics by including firm fixed effects (f_i). Instead of just including year fixed effects, I include industry-year fixed effects (θ_{jt}) and prefecture-year fixed effects (μ_{kt}) so that I can control for any events that occur at certain times in certain industries or prefectures.[16] $POST_{it}$ is an indicator variable which equals one for treatment firms during the post-CGC period (2016-2020). Hence, δ is the coefficient of interest. X_{it} is a set of control variables measured at the firm-year level.[17] Also, standard errors are clustered at the firm level to address serial correlation.

4. Empirical Findings

This section presents the results of primary analysis, mechanism analysis, and robustness checks.

4.1 Primary Results

[16] In terms of industry-year fixed effects and prefecture-year fixed effects, the study uses TSE 33 industry classification and Japan's 47 prefectures, respectively.

[17] Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt divided by total assets, and operating cash flow divided by total assets. Detailed definitions of all variables are provided in Appendix A.

4.1.1 The impact of the governance reform on distribution inequality

Table 2 reports the main results. Columns 1 and 2 show the results of the simple DID regressions including firm and year fixed effects, without and with control variables. Column 3 re-estimates the DID regression in Column 2, replacing year fixed effects with industry-year fixed effects. Column 4 re-estimates the DID regression in Column 2, replacing year fixed effects with prefecture-year fixed effects. Finally, Column 5 re-estimates the DID regression in Column 2, including both industry-year and prefecture-year fixed effects. All DID estimates in Table 2 are statistically significant at the 1% level, suggesting that the governance reform increased shareholder-worker ratio by 0.86 to 1.06. Given that the mean *Ratio* of pre-treatment treatment firms is 1.039, these results represent an increase of approximately 83% to 102% in the ratio. Overall, the main results support the view that corporate governance reforms worsen distribution inequality between shareholders and workers.

[Insert Table 2 here]

To interpret δ as the treatment effect, the parallel trends assumption (hereinafter PTA) indicating that in the absence of additional principles (30 principles and 38 supplementary principles), treatment firms would have followed parallel paths to comparison firms should

be satisfied (Angrist and Pischke, 2009; Clarke et al., 2023).[18] It is impossible to test PTA directly since it relates to a counterfactual situation. However, researchers often attempt to confirm the validity of PTA based on whether there are any differences in pre-treatment trends (pre-trends). In this study, therefore, I conduct three tests on pre-trends to check the validity of PTA indirectly.

First, I simply plot the annual trends in the outcome variable for treatment and comparison firms. Figure 3 presents the annual average of *Ratio* separately for treatment and comparison firms. It can be seen that the outcome variable is significantly greater for treatment firms. Importantly, shareholder-worker ratio of treatment and comparison firms seem to move in a relatively parallel manner before the implementation of CGC. However, the gap becomes wider after CGC (i.e., 2016). This trends plot supports PTA in this DID setting and the empirical prediction that governance reforms expand distribution inequality between shareholders and workers.

[Insert Figure 3 here]

Second, I conduct event-study analysis, which estimates the following regression:

$$Y_{it} = f_i + \theta_{jt} + \mu_{kt} + \sum_{\tau=-5}^{4} \delta_\tau \, POST_{i\tau} + \Omega X_{it} + \epsilon_{it} \qquad (2)$$

[18] More precisely, DID identifies a causal effect under the stable unit treatment value assumption (SUTVA), parallel trends assumption, and no anticipation assumption (Roth et al., 2023).

$POST_{i\tau}$ is an indicator variable which equals one for treatment firms in the period τ years after the CGC implementation in the sample.[19] I include the same fixed effects and control variables as in the baseline model. By performing this process, I can obtain DID estimate for each time point from 5 years before to 5 years after the introduction of CGC rather than a single point estimate.[20] If the trends of the outcome variable between treatment and comparison firms are parallel in the pre-treatment period, no pre-treatment effects should be statistically significant.[21]

Table 3 reports the results of the event study analysis. All pre-treatment estimates are statistically insignificant, while all post-treatment estimates are significant. These results indicate that the pre-treatment trends of the outcome variable (*Ratio*) in treatment and comparison firms are roughly parallel and significant effects occur only in the post-treatment period. Figure 4 illustrates the estimation results in Column 5 of Table 3 and Appendix Figure E1 shows event study graphs of other specifications. Overall, these results indirectly support the PTA and validity of treatment effects.

[Insert Table 3 here]

[Insert Figure 4 here]

[19] τ is -5 to -1 for the period of 2010 to 2014, and 0 to 4 for the period of 2016 to 2020.
[20] To deal with multicollinearity, δ_{-1} is used as the reference point ($\delta_{-1} = 0$).
[21] In the event study design, treatment effects in the pre-treatment period (δ_{-5}, δ_{-4}, δ_{-3}, δ_{-2}) and the post-treatment period (δ_0, δ_1, δ_2, δ_3, δ_4) are available. Diagnosing the significance of the coefficients in the pre-treatment period can indirectly test the credibility of the PTA.

Finally, I perform placebo tests to provide additional evidence that the main results are not driven by pre-trends. Specifically, I exclude all post-treatment observations and assign each pre-treatment year (2011, 2012, 2013, and 2014) as a pseudo-reform implementation year. Next, I define a new indicator variable, *PLACEBO*, which equals one for treatment firms in the period following a pseudo-reform year. I then run the baseline regressions, replacing *POST* with *PLACEBO*. Table 4 presents the results of these placebo tests. Regardless of the year selected as the pseudo-reform year, I find no significant effects. This evidence further supports the validity of the PTA.

[Insert Table 4 here]

4.1.2 Driver of distribution inequality

The predicted view is that the governance reform increases distribution inequality by boosting payouts while having little impact on wages. To verify the validity of this view, this study next examines whether the expanded distribution inequality is driven by increased payouts. I re-estimate the baseline DID regression, using the sum of cash dividend payments and cash share repurchases divided by total assets (*Payout*) and the natural logarithm of the average worker pay (*Log Wage*) as outcome variables. Estimates in Table 5 show that the reform increased corporate payouts significantly while having little impact on the average wage. These findings align with the predicted view.

[Insert Table 5 here]

To further investigate the drivers of the increase in total payouts, I examine whether it was primarily driven by dividends or share repurchases. Prior research suggests that share repurchases are more flexible than dividends in terms of adjustments (Skinner, 2008; Chu, 2018) and are more influenced by external regulations (Ni et al., 2020; Chronopoulos et al., 2023). Based on this, I hypothesize that firms rely more heavily on share repurchases than dividends to boost total payouts.

To analyze this, I re-estimate the DID regression model specified in Eq. (1), using dividends divided by total assets (*Div*) and share repurchases divided by total assets (*Rep*) as outcome variables. Table 6 presents the estimation results. Approximately two-thirds of the increase in total payouts is attributable to share repurchases. Furthermore, based on the pre-treatment mean of treatment firms, the estimates indicate that the governance reform increased dividends by 15% (0.00146/0.00949) and share repurchases by 140% (0.00302/0.00216). These findings support the hypothesis that firms rely more on share repurchases than dividends to increase payouts in response to regulatory changes.

[Insert Table 6 here]

4.1.3 Discussion on policy effects

The study finds that Japanese corporate governance reform expanded distribution inequality through the increase of corporate payouts. The estimate of 0.0045 in Table 5 means

an annual increase in corporate payout of 1.9 billion yen for the average treatment firm, considering that the pre-treatment mean of total assets for treatment firms is 426 billion yen. It also means an annual increase in corporate payouts of 3 trillion yen ($ 19 billion) for all treatment firms (1610*1.9 billion). Given that there are five post-treatment periods in the sample, the total amount of corporate payouts increased by the reform from 2016 to 2020 is as much as 15 trillion yen ($ 96 billion). This total increase is comparable to 13% of Japan's national budget.

In addition, the current wage increase target set by RENGO (Japan's largest labor union) is 5% (The Nikkei, 2023). Based on this, firms could have achieved a policy-desirable wage increase if they had allocated approximately 33% of the increased payouts from the reform to wages.[22] These discussions demonstrate that the treatment effects are not only statistically significant, but also economically significant.

4.2 Mechanism Analysis

To provide further evidence that the governance reform increases distribution inequality due to increased shareholder influence rather than other factors, I conduct the mechanism analysis. First, I test whether the treatment effects are more pronounced in firms with active shareholders, who are more likely to exercise their rights when empowered. Second, I confirm that the treatment effects are not caused by other mechanisms such as an

[22] The main results show that the governance reform increased annual payouts per firm by 1.9 billion yen. Therefore, one-third of the increase is 633 million yen. Given that an average treatment firm in the pre-treatment period has 1760 workers, one worker would receive 360,000 yen if the firm distributes one-third of the increased payouts. Considering that the mean wage in the pre-treatment period is 6.23 million yen, a 360,000 yen wage increase is about a 5% wage increase.

improvement of firms' investment decisions, a decrease in cash flow uncertainty, an increase in firms' ability to generate cash flow, or an expansion of firm size.

4.2.1 Shareholder influence

If the reform increases shareholders' influence and leads to distribution inequality, the main effects should be stronger in firms with more active shareholders because the reform builds the environment where these shareholders can easily put pressure on firms. To test whether treatment effects depend on the existence of active shareholders, I estimate the baseline regression including the interaction term between *POST* and *HIGHFO*. *HIGHFO* is an indicator variable equal to one if a treatment firm's pre-treatment mean of foreign shareholder ownership (*FO*) is above its median. I use *FO* as a proxy variable for active shareholders since foreign shareholders are more likely to be activists and involved with corporate management compared to domestic shareholders in the Japanese context (Suzuki, 2021).

Table 7 reports the estimation results by including the interaction between *POST* and *HIGHFO*. I find that all estimates on the interaction term are significant and positive, suggesting that the effects are pronounced for firms with more active shareholders. The results support the prediction that the impact of the governance reform on distribution inequality is caused by strengthened shareholder influence.

[Insert Table 7 here]

4.2.2 Other mechanisms

The above results support the predicted mechanism that governance reforms increase distribution inequality through strengthening shareholder influence. Next, this study confirms that alternative mechanisms do not explain the main results.

First, the CGC might improve the quality of investment decisions, as it recommends that firms reconstruct their management strategies by employing individuals who contribute to long-term firm value. In this case, such improvements might encourage firms with limited investment opportunities to return their cash to shareholders, thereby increasing distribution inequality. If the governance reform influences distribution inequality by improving firms' investment decisions, the treatment effects are likely to be more pronounced in firms with limited investment opportunities.

To examine this, I use *Tobin's Q* as a proxy variable for investment opportunities, defined as the sum of market capitalization and the book value of debt divided by total assets.[23] Next, I construct *HIGHTOBIN*, an indicator variable that equals one if a treatment firm's pre-treatment mean of *Tobin's Q* is above the median. Table 8 presents the estimation results, including the interaction between *POST* and *HIGHTOBIN*. In Table 8, the coefficients on the interaction term are statistically insignificant across all specifications, suggesting that the treatment effects do not depend on firms' investment opportunities.

[Insert Table 8 here]

[23] Since Tobin's Q roughly measures the average return on a firm's capital as anticipated by the market (Gugler et al., 2004), a higher Tobin's Q is associated with a higher likelihood of investment expansion.

Second, the CGC might reduce firms' cash flow uncertainty by improving management strategies. A reduction in cash flow uncertainty could result in increased payouts and heightened distribution inequality (Chay and Suh, 2009).

To examine whether the reform reduces cash flow uncertainty, I estimate the baseline model using standard deviation of *OCF* over the five-year pre-treatment and post-treatment periods (*SDOCF*) as the outcome variable. As *SDOCF* is the standard deviation over a five-year period, the sample is aggregated into two periods in the analysis: pre-treatment and post-treatment. For covariates, I use the averages for five years. Estimates in Table 9 suggest that the governance reform has little effect on cash flow uncertainty.

[Insert Table 9 here]

Third, the CGC might increase corporate cash generating ability since one of its goals is to improve firms' profitability and capital efficiency. If the amount of cash flows flowing into firms increases, the increase would cause firms to raise payouts and distribution inequality (Jensen, 1986).

To examine whether the reform increases corporate cash flow, I estimate the baseline regression model, using operating cash flow divided by sales (*CFRATIO*) as the outcome variable. Estimates in Table 10 imply that the reform has little effect on corporate cash generating ability.

[Insert Table 10 here]

Finally, the CGC might increase corporate sales size because the improved corporate governance can enhance a firm's credibility, potentially leading to an increase in transactions with consumers and client firms. If firm size expands, it enables firms to have greater asset diversification and a lower probability of bankruptcy, which has a positive effect on payouts (Rajan and Zingales, 1995). In this case, it can be argued that CGC raises distribution inequality by expanding firm size.

To examine whether the reform increases corporate sales size, I estimate the baseline regression model, using natural logarithm of corporate sales (*Log Sales*) as the outcome variable. Estimates in Table 11 suggest that the governance reform does not expand corporate sales size.

[Insert Table 11 here]

In summary, I obtain evidence supporting the claim that the reform increases distribution inequality due to heightened shareholder influence. Also, I confirm that alternative mechanisms do not fully explain the findings.

4.3 Robustness Checks

Additionally, I perform several types of robustness tests to increase the validity of the main results.

4.3.1 Doubly robust difference-in-differences

In the main analysis, I include time-varying covariates in the DID regressions. However, this design cannot identify causal effects when covariates themselves are affected by the treatment (Angrist and Pischke, 2009; Sant'Anna and Zhao, 2020). Thus, I confirm that the main results remain unchanged after conditioning on pre-treatment covariates. To conduct the above, this study uses doubly-robust difference-in-differences (DRDID) estimator developed by Callaway and Sant'Anna (2021). Appendix C briefly explains this method.

I show the event study plot created from DRDID estimation in Appendix Figure E2. Also, I report the estimates from DRDID in Appendix Table E2. These results are consistent with the main DID results.

4.3.2 Synthetic difference-in-differences

Given the inherent difference between treatment and comparison firms in the sample, it is still possible that the parallel trends assumption might be violated even after including covariates. To perform DID estimation with emphasis on firms and years for which the PTA seems most likely to hold, this research adopts synthetic difference-in-differences (SDID) estimation, which is developed in Arkhangelsky et al. (2021). Appendix D concisely describes how I obtain the SDID estimate.

The study plots dynamic effects from SDID in Appendix Figure E3. I report the SDID estimate in Appendix Table E3. These results indicate that the main results remain the same when I conduct SDID estimation.

4.3.3 Controlling for confounding events

In Japan, various corporate governance policies were introduced between 2014 and 2015, which could potentially confound the estimated treatment effects. Specifically, I focus on the Stewardship Code (Financial Services Agency, 2014), which requires institutional investors to engage in dialogue with firms to enhance corporate governance, and the Ito Review (Ito, 2014), which establishes an 8% target return on equity (ROE) for Japanese firms.

To assess whether the main results remain unchanged after considering these events, I conduct the estimation controlling for institutional investor ratio (*INST)* and return on equity (*ROE*). I employ two estimation approaches to adjust for these variables. First, as shown in Panel A of Appendix Table E4, I include these two variables as control variables in the baseline regressions. Second, as shown in Panel B of Appendix Table E4, I control for the pre-treatment status of these variables using DRDID estimator. Appendix Figure E4 provides event study plots after controlling for these events. Overall, I find robust evidence that the primary results remain consistent even when these events are taken into account.

4.4.4 Additional robustness checks

Additionally, I estimate the baseline regressions using a winsorized sample, where each control variable is winsorized at the 0.5% level on both tails of the distribution, additional treatment and comparison firms from other markets, a sample restricted to firms with 50 or more workers, and alternative outcome variables.

I plot event study graphs of these specifications in Appendix Figure E5, E6, E7, and E8. I report the estimates in Appendix Table E5, E6, E7, and E8. I find that the main results remain robust to these different conditions.

5. Possible Arguments

This section performs several additional tests to check the validity of possible arguments.

5.1 Corporate Employment

The main results show that the reform increases distribution inequality by boosting payouts while having little impact on wages. However, the reform might encourage firms to hire more workers, having positive consequences for workers in terms of job creation. To examine whether the reform makes firms proactive about hiring more workers, I estimate the baseline model, using natural logarithm of the number of workers at the end of fiscal year (*Log EMP*) as the outcome variable.

Estimates in Table 12 report that the governance reform does not increase corporate employment, suggesting that there is no positive effect of the governance reform on workers even in terms of job creation.

[Insert Table 12 here]

5.2 Capital Reallocation

Also, some might argue that the reform promotes capital reallocation from firms with low growth potential to firms with high growth potential. In this case, workers indirectly benefit as more efficient use of capital and innovation is promoted, leading to economic growth. To examine the validity of the above argument, the study examines whether more capital flowed into firms with higher growth opportunities in the post-reform period. Specifically, I treat firms whose pre-treatment mean of *Tobin's Q* is above the median as quasi-treatment firms. Also, I treat others as quasi-comparison firms. Using quasi-treatment and quasi-comparison firms, I estimate the baseline model, using cash provided by shareholders divided by total assets (*Capital*) as the outcome variable. If the reform promotes capital reallocation from low-growth firms to high-growth firms, the DID estimates should be positive and significant.

Table 13 reports that all the estimates are insignificant. It implies that the governance reform does not promote capital reallocation from low-growth firms to high-growth firms.

[Insert Table 13 here]

5.3 External Validity

This study finds that Japanese corporate governance reform raised distribution inequality between shareholders and workers. However, whether the findings can be applied to non-Japanese settings remains an open question. One possible explanation is that specific features of Japanese firms affect the results and thus reduce external validity. In this section, I conduct several additional tests to examine whether treatment effects depend on these Japanese firms' characteristics.

It is widely observed that Japanese firms exhibit lower profitability, maintain lower payout ratios, and hold higher cash reserves compared to their non-Japanese counterparts (Ito, 2014; Chattopadhyay et al., 2020; ETF trends, 2023; Kim et al., 2023). If the main results are derived from these characteristics, the impact of the reform should be more pronounced in firms with low profitability, low payout ratios, and high cash holdings. To examine this, I estimate the baseline model, including the interactions between *POST* and *HIGHROE, HIGHPAYOUT,* and *HIGHCASH*. *HIGHROE*, *HIGHPAYOUT*, and *HIGHCASH* are indicator variables equal to one if a treatment firm's pre-treatment means for return on equity (*ROE*), the sum of cash dividend payments and cash share repurchases divided by total assets (*Payout*), and cash holdings divided by total assets (*Cash*), respectively, exceeds their median values.

In Panel A of Table 14, I find that all the coefficients on the interaction term are statistically insignificant, indicating that the treatment effects are not dependent on firms' pre-treatment profitability. Panel B of Table 14 shows that the treatment effects in primary results are not more pronounced in firms with lower pre-treatment payout ratios. Panel C of Table 14 indicates that the impact of the corporate governance reform is not influenced by firms' pre-treatment cash holdings.

[Insert Table 14 here]

While unobservable characteristics unique to Japanese firms may still undermine the external validity of the results, this paper finds no strong evidence that the results are

influenced by at least the main observable characteristics. Overall, these additional tests provide no strong evidence in support of the possible concerns.

6. Conclusion

The distribution inequality between shareholders and workers has recently attracted attention in developed countries. However, few empirical studies have explored the underlying causes of this phenomenon. This paper uses firm-level wage data and institutional settings in Japan to examine whether national-level corporate governance reforms exacerbate this inequality. Employing a difference-in-differences methodology, I find that the Japanese governance reform significantly increases distribution inequality between shareholders and workers. This effect is driven by increased payouts, particularly through share repurchases, with minimal impact on wages. Next, the study confirms that the increase in distribution inequality is driven by heightened shareholder influence rather than other factors. Finally, I find no compelling evidence to support several possible counterarguments.

Since the late 1990s, a number of countries have launched corporate governance reforms (Fauver et al., 2017). Although the primary goal of these reforms is to protect shareholder rights (Kim and Lu, 2013; Bae et al., 2021), they are also used as public policy tools to promote sustainable development benefiting all stakeholders (OECD, 2004; Financial Reporting Council, 2018; Monetary Authority of Singapore, 2018). However, this paper's findings indicate that these reforms actually increase distribution inequality, leading to a significant wealth transfer from stakeholders to shareholders. Given the recent emphasis on firms considering the interests of a broader range of stakeholders (Business Roundtable,

2019), this paper's findings offer valuable insights for policymakers to reassess the actual effects of corporate governance reforms.

This study has several limitations that need to be acknowledged. First, while I confirm that certain characteristics of Japanese firms do not influence the main results, it remains possible that unobservable characteristics specific to Japan may have influenced the results, thereby limiting their generalizability. Second, although corporate governance reforms generally aim to strengthen investor protection (Kim and Lu, 2013), their nature and strength vary across countries. Therefore, the unique characteristics of the Japanese corporate governance reform may have contributed to the primary results. Third, the long-term effects of the reform on distribution inequality remain uncertain. On the one hand, the reform may continue to widen distribution inequality, ultimately contributing to greater income inequality in society. On the other hand, the surge in shareholder payouts could attract foreign capital inflows, which might, in the long run, foster economic growth and increase wages.

Notwithstanding the limitations, this study contributes to the literature by demonstrating that corporate governance reforms can significantly impact distribution inequality between shareholders and workers, a critical issue in recent years. Although the findings are limited to a single country and a specific timeframe, they underscore the importance of investigating how corporate governance reforms influence distribution inequality across diverse contexts using broader samples and longer time periods (Angrist and Pischke, 2010). I look forward to further research in this area.

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Figure 1
Description of treatment and comparison firms

This figure visually explains how I define treatment and comparison firms. From 2015, full CGC principles were applied to firms on 1st and 2nd sections of TSE, and only general principles were applied to firms on Mothers and JASDAQ of TSE. Therefore, I regard the former as treatment firms, and the latter as comparison firms.

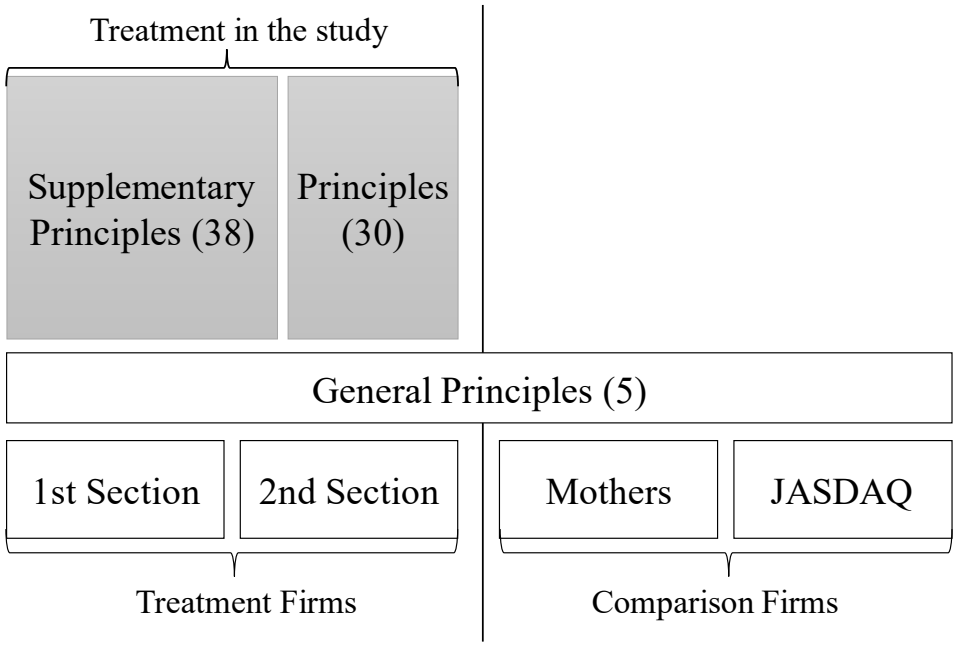


Figure 2
Description of sample construction

This figure visually explains which period is included in the sample. I treat the period from April of year t to March of year t + 1 as the year t. Firms in 2015 are excluded because of the difficulty of identifying pre-treatment and post-treatment within the period. The numbers in parentheses indicate the number of years since the reform in the sample.

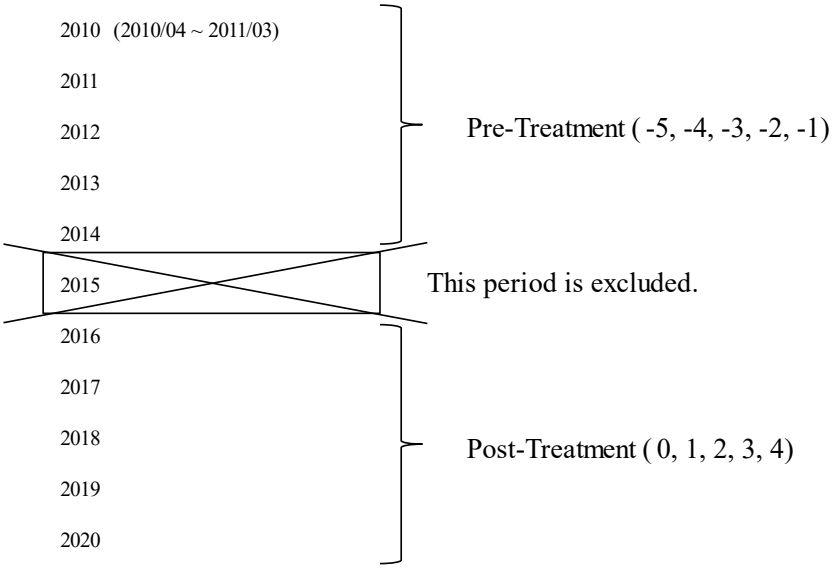


Figure 3
Trends plot of shareholder-worker ratio

Figure 3 displays the annual trends of the *Ratio* between treatment and comparison firms. The black vertical dotted line in the middle indicates the timing of CGC introduction. In the pre-treatment period, shareholder-worker ratio moves roughly parallel between the two groups. However, in the post-treatment period, the ratio in treatment firms increases significantly.

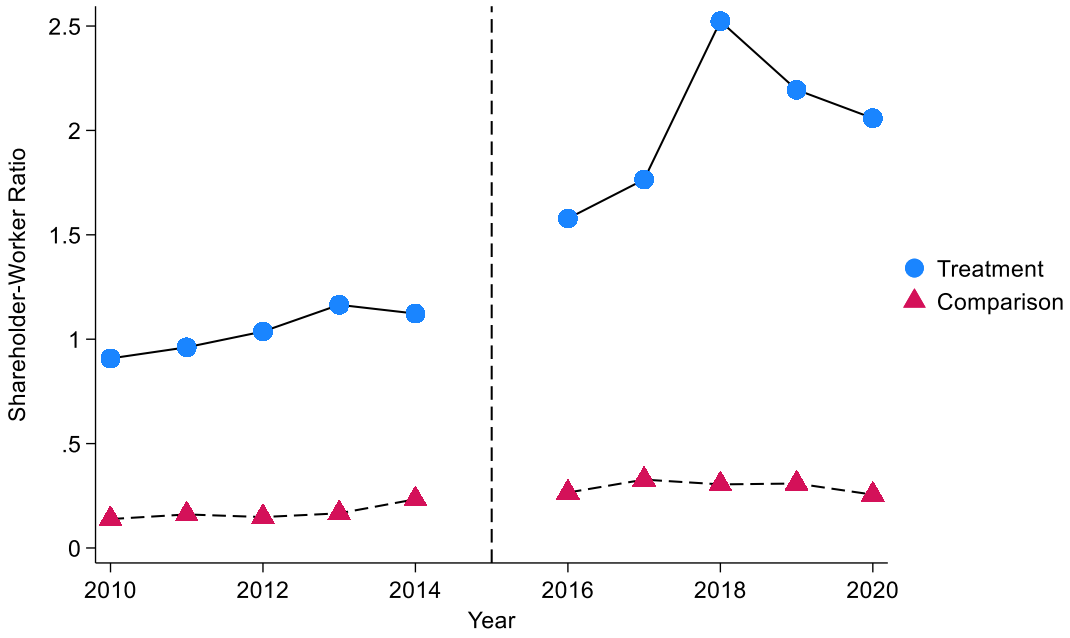


Figure 4
Event study plot

This figure reports the event study plot estimated from the model in Column 5 of Table 3. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Firm fixed effects, industry- year fixed effects, and prefecture-year fixed effects are included. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Standard errors are clustered at the firm level.

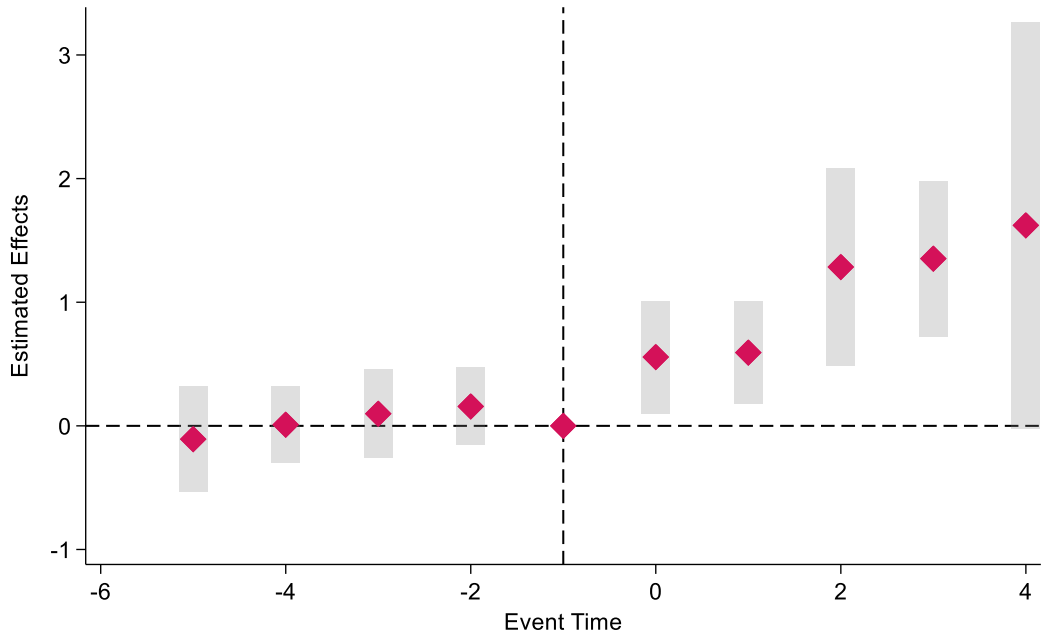


Table 1
Summary statistics

Panel A reports summary statistics on the pre-treatment period (2010-2014) for the full sample used in the main DID regressions. The six columns report the observations and distributional statistics (quartiles, mean, and standard deviation) of covariates for the full pre-period sample. Panel B shows the mean, standard deviation, and median of each variable in the pre-treatment period for treatment and comparison firms. A detailed definition of each variable is provided in Appendix A.

Panel A

	Full pre-period sample					
	N	p25	Mean	p50	p75	SD
Ratio	10,785	0.0411	0.8183	0.1088	0.2744	5.08
Payout	10,785	0.00362	0.0113	0.00715	0.0130	0.0197
Wage	10,785	4.98	5.958	5.81	6.71	1.493
Div	10,785	0.00357	0.00902	0.00682	0.01127	0.01064
Rep	10,785	0	0.00228	0	0.00004	0.01483
TA	10,785	13,961	321,449	41,461	135,596	1,421,179
Age	10,785	39	56.07	60	70	24.60
ROA	10,785	0.0107	0.0229	0.0256	0.0459	0.0961
LEV	10,785	0.326	0.486	0.484	0.644	0.206
OCF	10,785	0.0290	0.0573	0.0576	0.0888	0.0858

Panel B

	Pre-CGC 2010-2014					
	Treatment Firms			**Comparison Firms**		
	Mean	SD	p50	Mean	SD	p50
Ratio	1.03878	5.84741	0.13514	0.16937	0.74883	0.05292
Payout	0.0117	0.0191	0.00749	0.0103	0.0213	0.00590
Wage	6.234	1.52	6.06	5.145	1.052	5.04
Div	0.00949	0.01087	0.00719	0.00764	0.00979	0.00556
Rep	0.00216	0.01404	0	0.00265	0.01695	0
TA	425,750	1,631,873	70,212	14,459	19,972	8,670
Age	60.90	24.28	64	41.85	19.51	42
ROA	0.0284	0.0477	0.0267	0.00688	0.171	0.0223
LEV	0.491	0.198	0.488	0.470	0.226	0.467
OCF	0.0604	0.0566	0.0589	0.0483	0.140	0.0526
Firm-Year		8,050			2,735	

Table 2
Treatment effects in distribution inequality

This table estimates the effects of the reform on distribution inequality based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *Ratio*, which is the sum value of cash dividend and share repurchases scaled by total wages. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	0.862***	0.988***	1.059***	1.003***	1.048***
	(0.218)	(0.252)	(0.293)	(0.265)	(0.303)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.394	0.396	0.402	0.399	0.405
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 3
Event study analysis

This table reports the results of the event study analysis. Instead of obtaining a single estimate, I estimate treatment effects at each timing of the sample period. To address multicollinearity, *POST-1* is used as the reference point. The same set of control variables and fixed effects as in the baseline models are included. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST-5	-0.120	-0.156	-0.0621	-0.178	-0.107
	(0.164)	(0.219)	(0.209)	(0.229)	(0.217)
POST-4	-0.0891	-0.104	0.00475	-0.116	0.00965
	(0.120)	(0.157)	(0.154)	(0.166)	(0.158)
POST-3	-0.000875	-0.0323	0.0839	-0.0307	0.0982
	(0.146)	(0.166)	(0.174)	(0.183)	(0.183)
POST-2	0.109	0.111	0.161	0.101	0.157
	(0.128)	(0.136)	(0.156)	(0.144)	(0.160)
POST0	0.423**	0.494***	0.569**	0.501***	0.557**
	(0.169)	(0.176)	(0.226)	(0.185)	(0.233)
POST1	0.548***	0.627***	0.625***	0.627***	0.593***
	(0.185)	(0.197)	(0.207)	(0.205)	(0.211)
POST2	1.329***	1.453***	1.390***	1.430***	1.285***
	(0.463)	(0.474)	(0.431)	(0.479)	(0.407)
POST3	0.996***	1.137***	1.336***	1.179***	1.352***
	(0.221)	(0.255)	(0.302)	(0.285)	(0.321)
POST4	0.914**	1.062**	1.571*	1.072**	1.622*
	(0.457)	(0.476)	(0.803)	(0.496)	(0.838)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.394	0.396	0.402	0.400	0.406
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No

Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 4
Placebo tests

This table reports the results of the placebo tests by each year. Specifically, I drop all observations in the post-treatment period and treat any given year (2011, 2012, 2013, and 2014) as a pseudo-reform implementation year. I next create *PLACEBO*, which is an indicator variable that equals one for treatment firms after a pseudo-reform implementation year. I then estimate the baseline regressions, replacing *POST* with *PLACEBO*. The same set of control variables and fixed effects as in the baseline models are included. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
A: Placebo Year = 2011	0.125	0.153	0.161	0.171	0.210
	(0.149)	(0.211)	(0.199)	(0.226)	(0.212)
B: Placebo Year = 2012	0.141	0.166	0.152	0.181	0.175
	(0.105)	(0.165)	(0.146)	(0.177)	(0.158)
C: Placebo Year = 2013	0.125	0.151	0.0991	0.156	0.104
	(0.0988)	(0.152)	(0.128)	(0.159)	(0.137)
D: Placebo Year = 2014	0.025	0.051	-0.009	0.062	-0.005
	(0.113)	(0.148)	(0.138)	(0.157)	(0.145)
Observations	10,785	10,785	10,785	10,785	10,785
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes

Year ×Prefecture FE	No	No	No	Yes	Yes

Table 5
Driver of distribution inequality

This table estimates the baseline regressions in Table 2, using alternative outcome variables to determine the driver of expanded distribution inequality. *Payout* is the sum of cash dividends and cash repurchases scaled by total assets. *Log Wage* is the natural logarithm of average worker pay. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: *Payout*

	(1) *Payout*	(2) *Payout*	(3) *Payout*	(4) *Payout*	(5) *Payout*
POST	0.00402***	0.00390***	0.00435***	0.00396***	0.00447***
	(0.000682)	(0.000687)	(0.000735)	(0.000719)	(0.000768)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.427	0.430	0.439	0.438	0.447
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Panel B: *Log Wage*

	(1) *Log Wage*	(2) *Log Wage*	(3) *Log Wage*	(4) *Log Wage*	(5) *Log Wage*
POST	0.00776	0.00409	0.00538	0.00370	0.00531
	(0.00496)	(0.00489)	(0.00498)	(0.00518)	(0.00530)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.911	0.915	0.918	0.916	0.919
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 6
Driver of corporate payouts

This table estimates the baseline regressions in Table 2, using alternative outcome variables to determine the driver of increased payouts. *Div* is cash dividends scaled by total assets. *Rep* is cash repurchases scaled by total assets. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: *Div*					
	(1)	(2)	(3)	(4)	(5)
	Div	*Div*	*Div*	*Div*	*Div*
POST	0.00129***	0.00108***	0.00138***	0.00116***	0.00146***
	(0.000351)	(0.000361)	(0.000394)	(0.000378)	(0.000414)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.736	0.744	0.750	0.747	0.752
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes
Panel B: *Rep*					
	(1)	(2)	(3)	(4)	(5)
	Rep	*Rep*	*Rep*	*Rep*	*Rep*
POST	0.00273***	0.00282***	0.00297***	0.00280***	0.00302***
	(0.000531)	(0.000542)	(0.000574)	(0.000566)	(0.000597)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.160	0.165	0.176	0.177	0.189
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 7
Treatment effects and shareholder influence

This table estimates the baseline regressions in Table 2, including the interaction term to test whether the impact of the reform on distribution inequality depends on the existence of influential shareholders. The estimation is based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *Ratio*, which is the sum value of cash dividend and share repurchases scaled by total wages. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*×*HIGHFO*. *POST* is an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. *HIGHFO* is an indicator variable equals to one for treatment firms whose pre-treatment mean of foreign shareholder ratio is above its median and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ratio*	(2) *Ratio*	(3) *Ratio*	(4) *Ratio*	(5) *Ratio*
POST	0.208**	0.409**	0.428**	0.418**	0.430**
	(0.104)	(0.160)	(0.186)	(0.172)	(0.202)
POST×HIGHFO	1.309***	1.149***	1.298***	1.165***	1.290***
	(0.429)	(0.387)	(0.458)	(0.389)	(0.456)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.395	0.397	0.403	0.400	0.406
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 8
Treatment effects and investment decisions

This table estimates the baseline regressions in Table 2, including the interaction term to test whether the impact of the reform on distribution inequality depends on firms' investment opportunities. The estimation is based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *Ratio*, which is the sum value of cash dividend and share repurchases scaled by total wages. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*×*HIGHTOBIN*. *POST* is an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. *HIGHTOBIN* is an indicator variable equal to one for treatment firms whose pre-treatment mean of Tobin's Q is above its median and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ratio*	(2) *Ratio*	(3) *Ratio*	(4) *Ratio*	(5) *Ratio*
POST	0.549***	0.810***	0.911***	0.856***	0.941***
	(0.144)	(0.207)	(0.254)	(0.227)	(0.274)
POST×HIGHTOBIN	0.626	0.356	0.283	0.292	0.206
	(0.430)	(0.365)	(0.352)	(0.365)	(0.347)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.394	0.396	0.402	0.400	0.405
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 9
Treatment effects in cash flow uncertainty

This table estimates the baseline regressions in Table 2, using alternative outcome variables to test the impact of the reform on cash flow uncertainty based on a sample of 4,314 firm-year observations (2,157 firms in pre-treatment period and 2,157 firms in post-treatment period). The dependent variable is *SDOCF*, which is the standard deviation of *OCF* over the five-year pre-treatment and post-treatment periods. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, and debt scaled by total assets (I use the averages for five years). Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	SDOCF	*SDOCF*	*SDOCF*	*SDOCF*	*SDOCF*
POST	0.00152	0.00240	0.00261	0.00227	0.00248
	(0.00373)	(0.00301)	(0.00305)	(0.00316)	(0.00321)
Observations	4,314	4,314	4,314	4,314	4,314
R-squared	0.734	0.789	0.798	0.790	0.800
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 10
Treatment effects in corporate cash generating ability

This table estimates the baseline regressions in Table 2, using the alternative outcome variable to test the impact of the reform on corporate cash generating ability based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *CFRATIO*, which is operating cash flow divided by sales. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, and debt scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	CFRATIO	*CFRATIO*	*CFRATIO*	*CFRATIO*	*CFRATIO*
POST	0.0461	0.0443	0.0497	0.0454	0.0482
	(0.0379)	(0.0364)	(0.0435)	(0.0380)	(0.0443)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.316	0.318	0.340	0.319	0.341
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 11
Treatment effects in firm size

This table estimates the baseline regressions in Table 2, using the alternative outcome variable to test the impact of the reform on firm sales size based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *Log Sales*, which is the natural logarithm of corporate sales. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Log Sales	*Log Sales*	*Log Sales*	*Log Sales*	*Log Sales*
POST	0.0129	-0.00706	0.00259	-0.00774	0.00246
	(0.0204)	(0.0123)	(0.0129)	(0.0128)	(0.0134)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.985	0.993	0.993	0.993	0.993
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 12
Treatment effects in corporate employment

This table estimates the baseline regressions in Table 2, using the alternative outcome variable to test the impact of the reform on corporate employment based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *Log EMP*, which is the natural logarithm of the number of workers at the end of fiscal year. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Log EMP	*Log EMP*	*Log EMP*	*Log EMP*	*Log EMP*
POST	0.0177	0.0223	0.0298	0.0204	0.0356
	(0.0253)	(0.0259)	(0.0271)	(0.0272)	(0.0283)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.949	0.950	0.952	0.952	0.953
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 13
The governance reform and capital reallocation

This table estimates the baseline regressions in Table 2, using the alternative outcome variable and treatment/comparison categorization to test whether the reform promotes capital reallocation. The estimation is based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). I treat firms whose pre-treatment mean of *Tobin's Q* is above its median as quasi-treatment firms and regard others as quasi-comparison firms. The dependent variable is *Capital*, which is cash provided by shareholders divided by total assets. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*, an indicator variable equal to one for quasi-treatment firms in the post-treatment period and zero otherwise. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Capital	*Capital*	*Capital*	*Capital*	*Capital*
POST	-0.000150	0.000442	0.000257	0.000511	0.000357
	(0.00136)	(0.00122)	(0.00118)	(0.00122)	(0.00119)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.422	0.448	0.459	0.455	0.466
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table 14
External validity

This table estimates the baseline regressions in Table 2, including the interaction terms to test whether the impact of the reform on distribution inequality depends on characteristics unique to Japanese firms. The estimation is based on a sample of 21,570 firm-year observations from 2010 to 2020 (observations in 2015 are excluded). The dependent variable is *Ratio*, which is the sum value of cash dividend and share repurchases scaled by total wages. In column (1), I estimate the DID regression model with firm and year fixed effects. In column (2), I estimate the regression model in column (1), including time-varying control variables. In column (3), I estimate the regression model in column (2), replacing year fixed effects with industry-year fixed effects. In column (4), I estimate the regression model in column (2), replacing year fixed effects with prefecture-year fixed effects. In column (5), I estimate the regression model in column (2), replacing year fixed effects with both industry-year and prefecture-year fixed effects. The main independent variable is *POST*×*HIGHROE* in panel A, *POST*×*HIGHPAYOUT* in panel B, and *POST*×*HIGHCASH* in panel C. *POST* is an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. *HIGHROE*, *HIGHPAYOUT*, and *HIGHCASH* are indicator variables equal to one if a treatment firm's pre-treatment means for *ROE* (return on equity), *Payout* (the sum of cash dividend payments and cash share repurchases divided by the firms' total assets), and *Cash* (cash holdings divided by total assets), respectively, exceeds their median values. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: *HIGHROE*

	(1) *Ratio*	(2) *Ratio*	(3) *Ratio*	(4) *Ratio*	(5) *Ratio*
POST	0.656***	0.950***	1.009***	0.967***	1.004***
	(0.227)	(0.264)	(0.285)	(0.275)	(0.294)
POST×*HIGHROE*	0.413	0.0769	0.0994	0.0738	0.0877
	(0.43)	(0.315)	(0.307)	(0.31)	(0.303)

Panel B: *HIGHPAYOUT*

	(1) *Ratio*	(2) *Ratio*	(3) *Ratio*	(4) *Ratio*	(5) *Ratio*
POST	0.771***	0.970***	1.129**	1.002***	1.140**
	(0.289)	(0.339)	(0.483)	(0.353)	(0.507)
POST×*HIGHPAYOUT*	0.182	0.0346	-0.131	0.00131	-0.173

	(0.430)	(0.490)	(0.580)	(0.500)	(0.591)
Panel C: *HIGHCASH*					
	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	0.829***	1.024***	1.079***	1.014***	1.047***
	(0.254)	(0.295)	(0.301)	(0.308)	(0.309)
POST×HIGHCASH	0.0663	-0.0720	-0.0365	-0.0204	0.00251
	(0.430)	(0.389)	(0.390)	(0.395)	(0.391)
Observations	21,570	21,570	21,570	21,570	21,570
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year×Industry FE	No	No	Yes	No	Yes
Year×Prefecture FE	No	No	No	Yes	Yes

Online Appendix

The Impact of Corporate Governance Reforms on Distribution Inequality: Micro Evidence from Japan

Appendix A

Description of Variables

This table defines variables used in the analysis.

Variable	Description
Ratio	(Cash dividend paid + Cash repurchases paid - Proceeds from sale of treasury stock) / (The average worker wage*The number of workers at the end of fiscal year)
POST	An indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise
Payout	(Cash dividend paid + Cash repurchases paid - Proceeds from sale of treasury stock) / Total assets
Log Wage	Natural logarithm of the average worker wage
Wage	The average worker wage (million yen)
Div	Cash dividend paid / Total assets
Rep	(Cash repurchases paid - Proceeds from sale of treasury stock) / Total assets
Log TA	Natural logarithm of total assets
TA	Total assets (million yen)
Log Age	Natural logarithm of firm age
Age	Firm age
ROA	Net income / Total assets
LEV	Debt / Total assets
OCF	Operating cash flow / Total assets
FO	Foreign shareholder ownership
HIGHFO	An indicator variable which equals to one if a treatment firm's mean of *FO* in the pre-treatment period is above its median
Tobin's Q	(Market capitalization + Debt) / Total assets
HIGHTOBIN	An indicator variable which equals to one if a treatment firm's mean of *Tobin's Q* in the pre-treatment period is above its median
SDOCF	Standard deviation of *OCF* over the five-year pre-treatment and post-treatment periods
CFRATIO	Operating cash flow / Sales
Log Sales	Natural logarithm of sales
Sales	Sales
Inst	Institutional shareholder ownership (foreign shareholder ownership + pension fund ownership + mutual fund ownership)
ROE	Net income / Net assets
Ratio2	(Cash dividend paid [unconsolidated based] + Cash repurchases paid [unconsolidated based] - Proceeds from sale of treasury stock [unconsolidated based]) / (The average worker wage*The number of workers at end of fiscal year)

Ratio3	(Cash dividend paid + Cash repurchases paid - Proceeds from sale of treasury stock – Cash provided by shareholders) / (The average worker wage*The number of workers at end of fiscal year)
Ratio4	Net income / (The average worker wage*The number of workers at end of fiscal year)
Log EMP	Natural logarithm of the number of workers at the end of fiscal year
EMP	The number of workers at end of fiscal year
Capital	Cash provided by shareholders / Total assets
Cash	Cash holdings / Total assets
HIGHROE	An indicator variable which equals to one if a treatment firm's mean of *ROE* in the pre-treatment period is above its median
HIGHPAYOUT	An indicator variable which equals to one if a treatment firm's mean of *Payout* in the pre-treatment period is above its median
HIGHCASH	An indicator variable which equals to one if a treatment firm's mean of *Cash* in the pre-treatment period is above its median

Appendix B

Description of CGC

Primary Objective

The CGC (Corporate Governance Code) is a set of guidelines for listed firms in Japan, jointly compiled by the Financial Services Agency and the Tokyo Stock Exchange in June 2015. It outlines the principles necessary to achieve a desirable level of corporate governance and was introduced as part of a growth strategy to revitalize the stagnant Japanese economy. The policy aims to increase firm profitability and promote sustainable economic development, creating a virtuous circle of increased employment opportunities, higher wages, and dividends.

Approaches

The CGC is characterized by its adoption of two methods: a principles-based approach and a comply-or-explain approach. The principles-based approach involves establishing broad principles and leaving the specifics to each firm. This approach is based on the logic that each firm would strive to understand the purpose and spirit of each principle and apply them in a way that best suits their circumstances.

The comply-or-explain approach does not uniformly enforce compliance with the principles of the CGC. Instead, it requires firms to explain their rationale for not implementing the principles. Therefore, in the Japanese context, firms must either apply each CGC principle or explain their reasons for non-implementation in their corporate governance

report. The Japan Exchange Group (JPX) states, “Since the Code adopts a comply-or-explain approach, no penalties will be immediately applied, even if a firm does not implement each principle. If a firm does not implement a principle of the Code, it is required by the listing rules to explain the reasons in the report on corporate governance. If a firm fails to explain the reasons for non-implementation, the fact might be publicly disclosed.”

General Principles / Principles

The CGC consists of 5 general principles, 30 principles, and 38 supplementary principles. This section summarizes the content of the general principles and principles of CGC. A more detailed explanation can be found in the link provided below.

https://www.ecgi.global/sites/default/files/codes/documents/japan_cg_code_1jun15_en.pdf

General Principle 1

Securing the Rights and Equal Treatment of Shareholders

Principle 1.1 Securing the Rights of Shareholders

Principle 1.2 Exercise of Shareholder Rights at General Shareholder Meetings

Principle 1.3 Basic Strategy for Capital Policy

Principle 1.4 Cross-Shareholdings

Principle 1.5 Anti-Takeover Measures

Principle 1.6 Capital Policy that May Harm Shareholder Interests

Principle 1.7 Related Party Transactions

General Principle 2

Appropriate Cooperation with Stakeholders Other Than Shareholders

Principle 2.1 Business Principles as the Foundation of Corporate Value Creation Over the Mid- to Long-Term

Principle 2.2 Code of Conduct

Principle 2.3 Sustainability Issues, Including Social and Environmental Matters

Principle 2.4 Ensuring Diversity, Including Active Participation of Women

Principle 2.5 Whistleblowing

General Principle 3

Ensuring Appropriate Information Disclosure and Transparency

Principle 3.1 Full Disclosure

Principle 3.2 External Auditors

General Principle 4

Responsibilities of the Board

Principle 4.1 Roles and Responsibilities of the Board (1)

Principle 4.2 Roles and Responsibilities of the Board (2)

Principle 4.3 Roles and Responsibilities of the Board (3)

Principle 4.4 Roles and Responsibilities of Kansayaku and the Kansayaku Board

Principle 4.5 Fiduciary Responsibilities of Directors and Kansayaku

Principle 4.6 Business Execution and Oversight of the Management

Principle 4.7 Roles and Responsibilities of Independent Directors

Principle 4.8 Effective Use of Independent Directors

Principle 4.9 Independence Standards and Qualification for Independent Directors

Principle 4.10 Use of Optional Approach

Principle 4.11 Preconditions for Board and Kansayaku Board Effectiveness

Principle 4.12 Active Board Deliberations

Principle 4.13 Information Gathering and Support Structure

Principle 4.14 Director and Kansayaku Training

General Principle 5

Dialogue with Shareholders

Principle 5.1 Policy for Constructive Dialogue with Shareholders

Principle 5.2 Establishing and Disclosing Business Strategy and Business Plan

Appendix C

Description of Doubly Robust DID Estimator

In DRDID (Doubly-Robust Difference-in-Differences), DID estimation is performed conditioning on covariates in the reference pre-treatment period by regression adjustment and inverse probability weighting approaches. DRDID estimator is consistent if either of the models is correctly specified (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). In the analysis, I set the reference period to the year before treatment. For each relative year, the doubly-robust estimation is conducted as follows:

$$\delta_t^{DR} = \mathrm{E}\left[\left(\frac{TREAT}{\mathrm{E}[TREAT]} - \frac{\frac{g(X)COMPARISON}{1-g(X)}}{\mathrm{E}\left[\frac{g(X)COMPARISON}{1-g(X)}\right]}\right)\left(\Delta Y - \hat{E}[\Delta Y^0|X]\right)\right] \quad (1)$$

Here $\frac{TREAT}{\mathrm{E}[TREAT]}$ is the treatment indicator divided by its expected value. $COMPARISON$ is an indicator variable which equals to one for comparison firms and $g(X)$ is the estimate of the propensity score. Thus, $\frac{\frac{g(X)COMPARISON}{1-g(X)}}{\mathrm{E}\left[\frac{g(X)COMPARISON}{1-g(X)}\right]}$ are inverse probability weights which should be applied to comparison firms. $\Delta Y = Y_t - Y_{2014}$ is the difference between the outcome variable in year t and the outcome variable in the reference period, 2014. $\hat{E}[\Delta Y^0|X]$ is the prediction for the value of ΔY using an outcome regression model fit over

the comparison firms. Both $g(X)$ and $\hat{E}[\Delta Y^0|X]$ are estimated based on the covariates in the reference period.

Appendix D

Description of Synthetic DID Estimator

Synthetic Difference-in-Differences (SDID) is the identification strategy which borrows strengths from both the difference-in-differences and synthetic control methods (Arkhangelsky et al., 2021). Like synthetic control methods, SDID uses pre-treatment data of comparison units to create a synthetic control for the average outcome of treated units and does not rely on a strong parallel-trends assumption for identification. Similar to difference-in-differences, SDID allows for time invariant difference between treatment and comparison units. Under SDID, the estimation proceeds as follows:

$$\left(\hat{\delta}^{SDID}, \hat{\mu}, \hat{f}, \hat{\beta}\right) = \underset{\delta,\mu,f,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - f_i - \beta_t - POST_{it}\delta)^2 \hat{\omega}_i^{SDID} \hat{\lambda}_t^{SDID} \right\} \quad (2)$$

Here, N is the set of firms and T is the set of years in the balanced panel. Y_{it} is the outcome variable and μ is a constant term. Also, firm fixed effects (f_i) and year fixed effects (β_t) are included. $POST_{it}$ is the binary variable which equals to one for treatment firms in the post-treatment period.

$\hat{\omega}_i^{SDID}$ is calculated so that the pre-treatment comparisons firms weighted by $\hat{\omega}_i^{SDID}$ could be the most accurate approximation of the pre-treatment treatment firms allowing for the time-invariant constant term. Also, $\hat{\lambda}_t^{SDID}$ will be obtained from the sample of the

comparison firms in the pre-treatment period so that pre-treatment comparison firms weighted by $\hat{\lambda}_t^{SDID}$ could most accurately predict the comparison firms in the post-treatment period allowing for the constant term. Therefore, in SDID design, the causal effect of the governance reform is estimated by minimizing the weighted squared error using the new weights (Clarke et al., 2023).

Appendix E

Additional Figures and Tables

Figure E1
Event study plots with multiple specifications

This figure displays coefficient plots estimated from event study design in Eq. (2). The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Panel A shows the estimation results of the event study without covariates. Panel B illustrates the results of the event study with time-varying covariates. Panel C illustrates the results of the event study with time-varying covariates and industry-year fixed effects. Panel D demonstrates the results of the event study with time-varying covariates and prefecture-year fixed effects. Standard errors are clustered at the firm level.

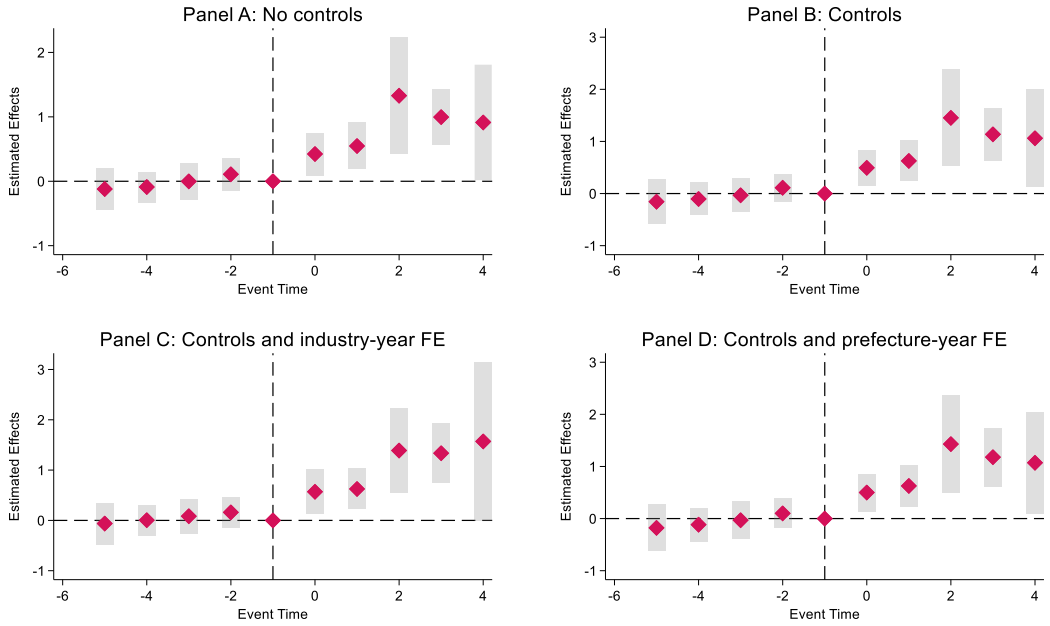


Figure E2
Event study plot from doubly-robust DID estimator

This figure displays the event study plot created from doubly robust DID estimation. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. As with the standard event study plot, the period one year prior to the reform is used as the reference point. All time-varying covariates and time-invariant covariates are included. Standard errors are clustered at the firm level.

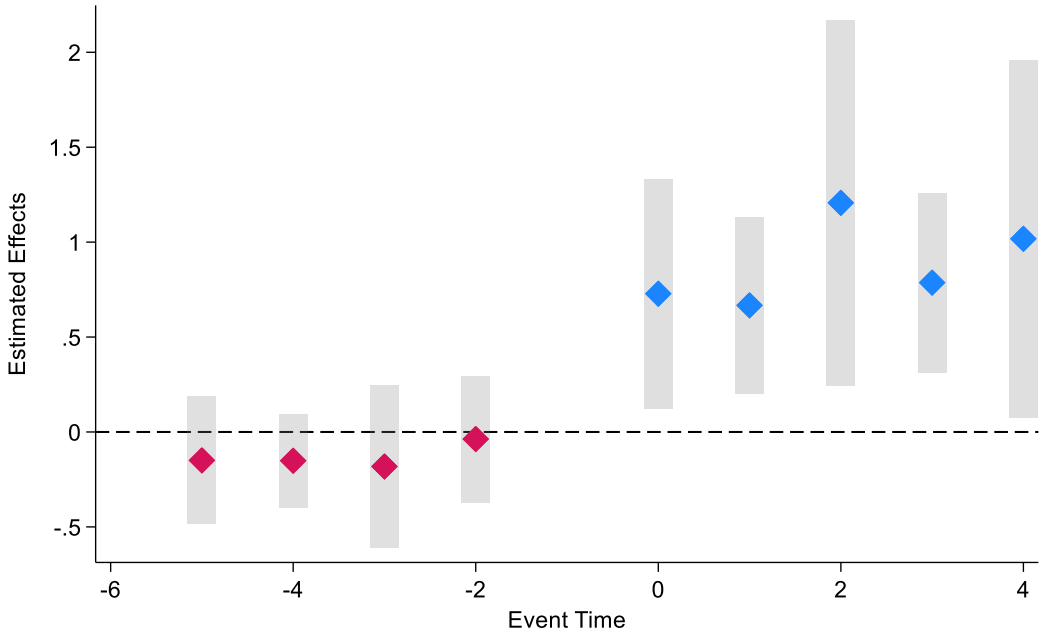


Figure E3
Dynamic effects from synthetic DID estimator

This figure displays dynamic treatment effects from SDID estimation. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. As with the event study plot, the period one year prior to the reform is used as the reference point.

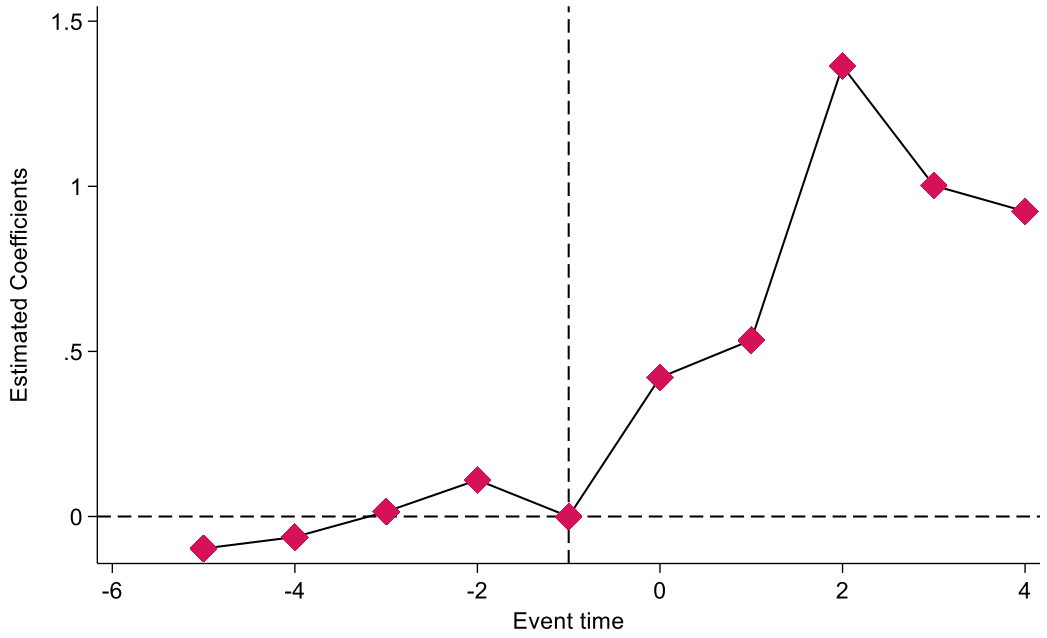
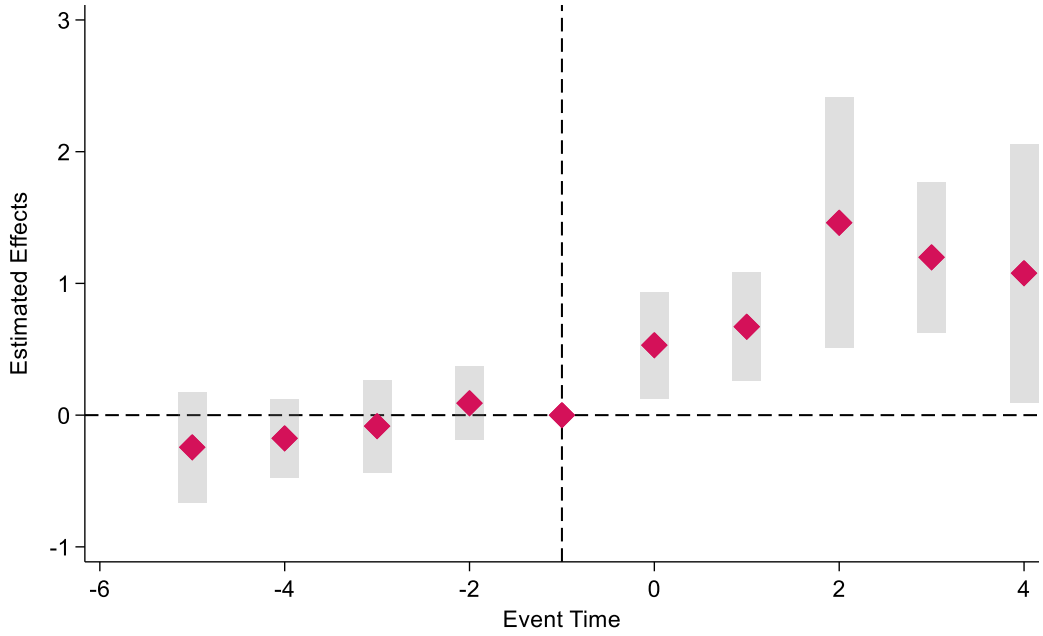


Figure E4
Event study plots controlling for confounding events

This figure displays the event study plots controlling for confounding events. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Firm fixed effects, industry- year fixed effects, and prefecture-year fixed effects are included. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, operating cash flow scaled by total assets, institutional shareholder ratio, and return on equity. Standard errors are clustered at the firm level.

Panel A: Standard DID estimation



Panel B: Doubly-robust DID estimation

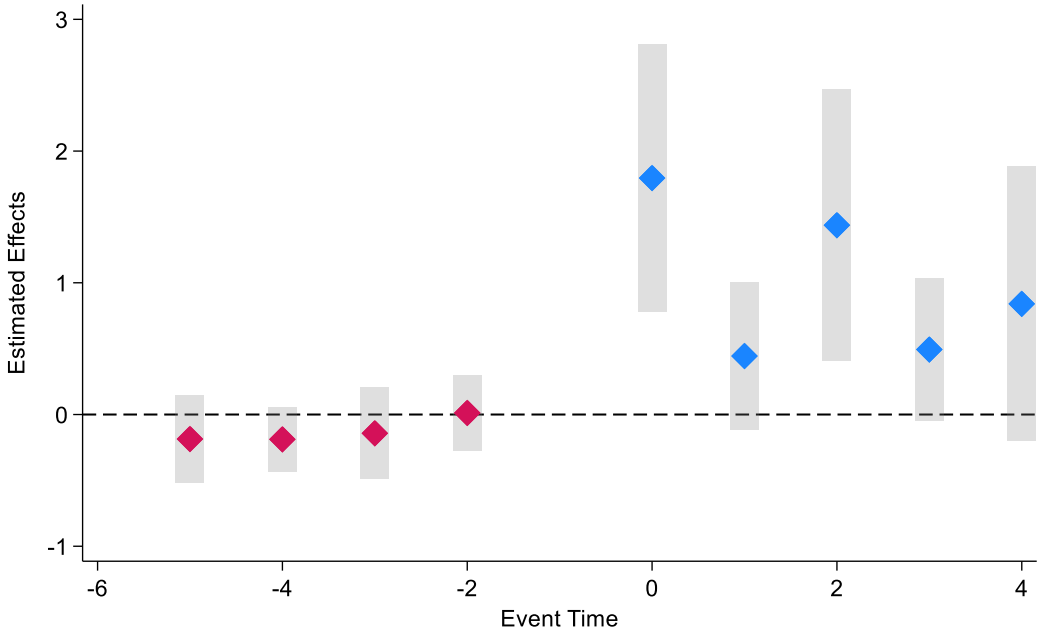


Figure E5
Event study plot using the winsorized sample

This figure reports the event study plot using the winsorized sample where each control variable is winsorized at the 0.5% level on both sides of the distribution. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Firm fixed effects, industry- year fixed effects, and prefecture-year fixed effects are included. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Standard errors are clustered at the firm level.

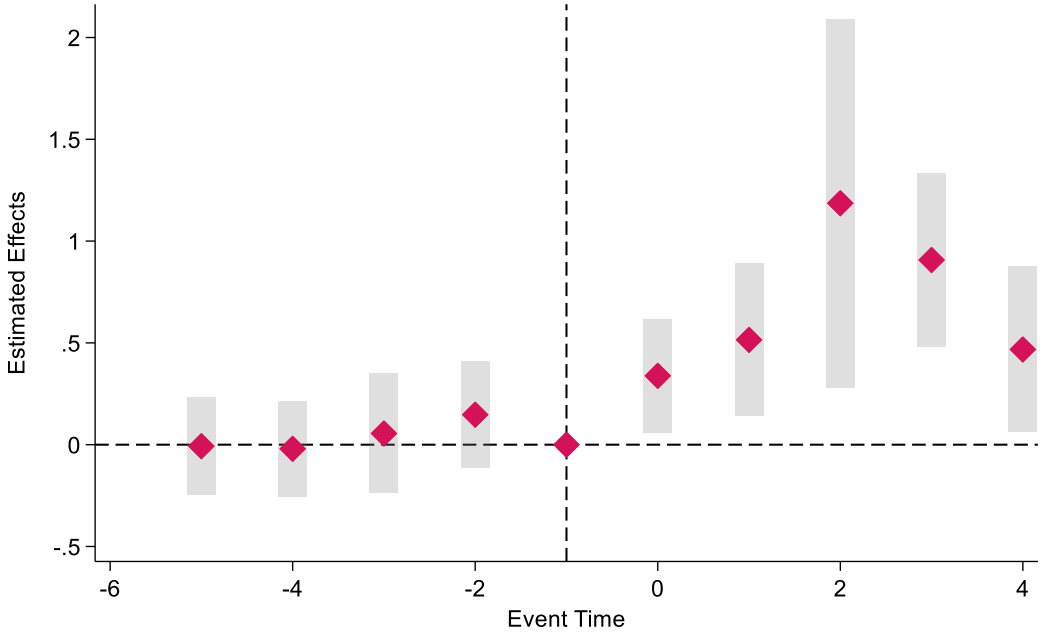


Figure E6
Event study plot using additional treatment and comparison firms

This figure reports the event study plot using additional treatment and comparison firms from other market. Specifically, I include firms on Nagoya stock exchange as treatment firms, and include firms on Sapporo and Fukuoka stock exchanges as additional comparison firms. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Firm fixed effects, industry- year fixed effects, and prefecture-year fixed effects are included. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Standard errors are clustered at the firm level.

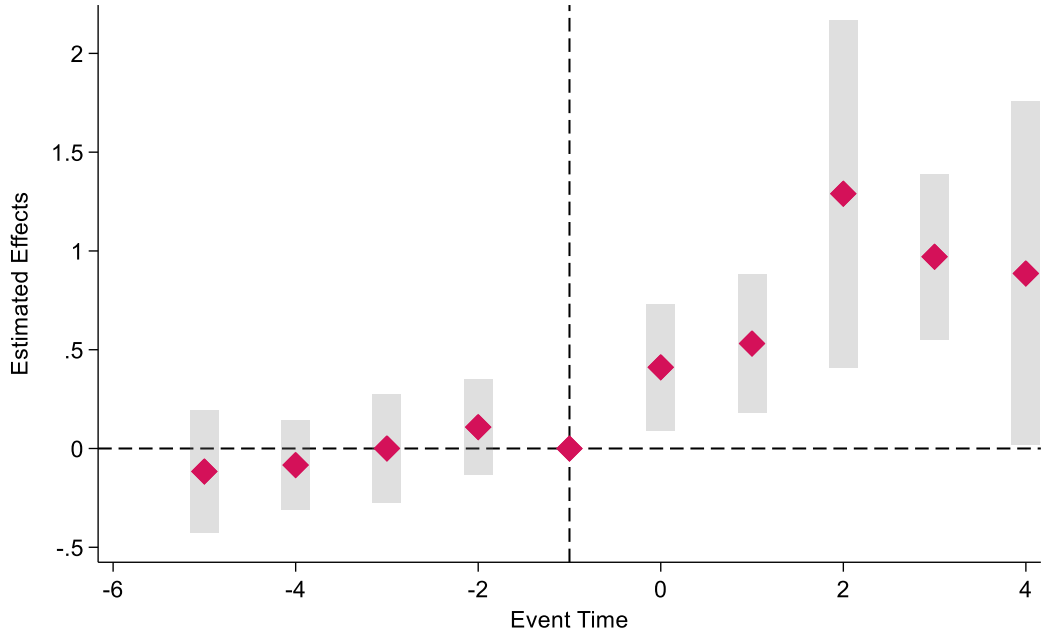


Figure E7
Event study plot using firms with at least 50 workers

This figure reports the event study plot using firms with at least 50 workers. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variable, *Ratio*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Firm fixed effects, industry- year fixed effects, and prefecture-year fixed effects are included. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Standard errors are clustered at the firm level.

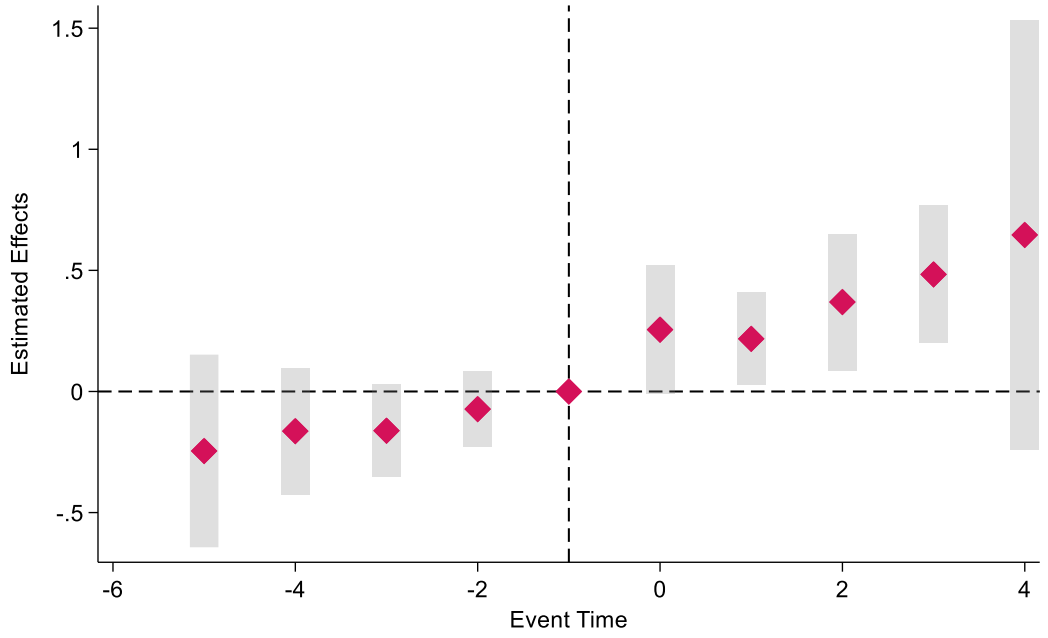
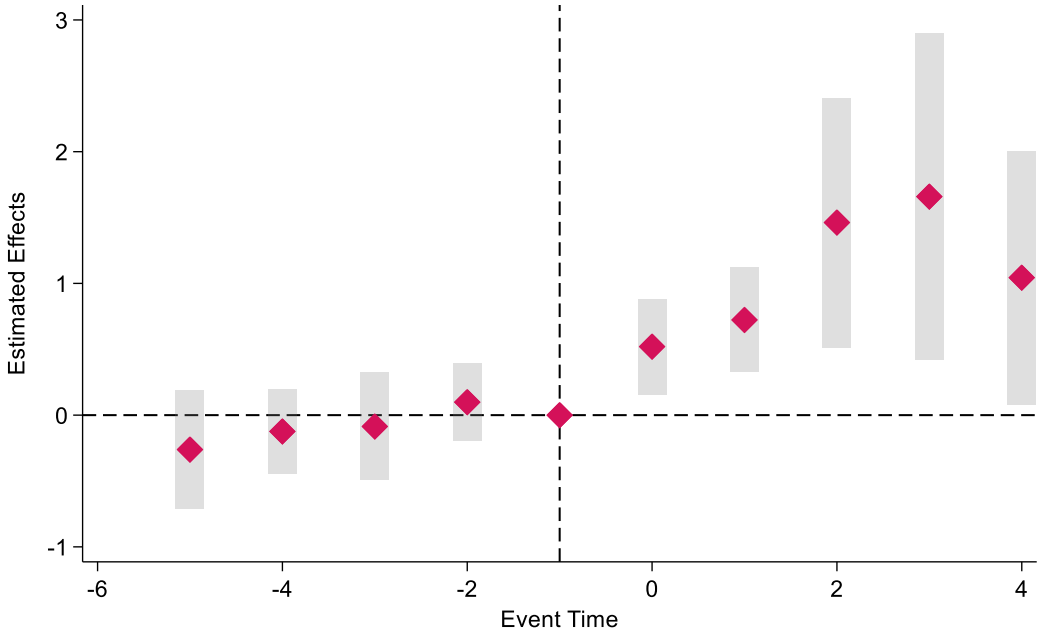


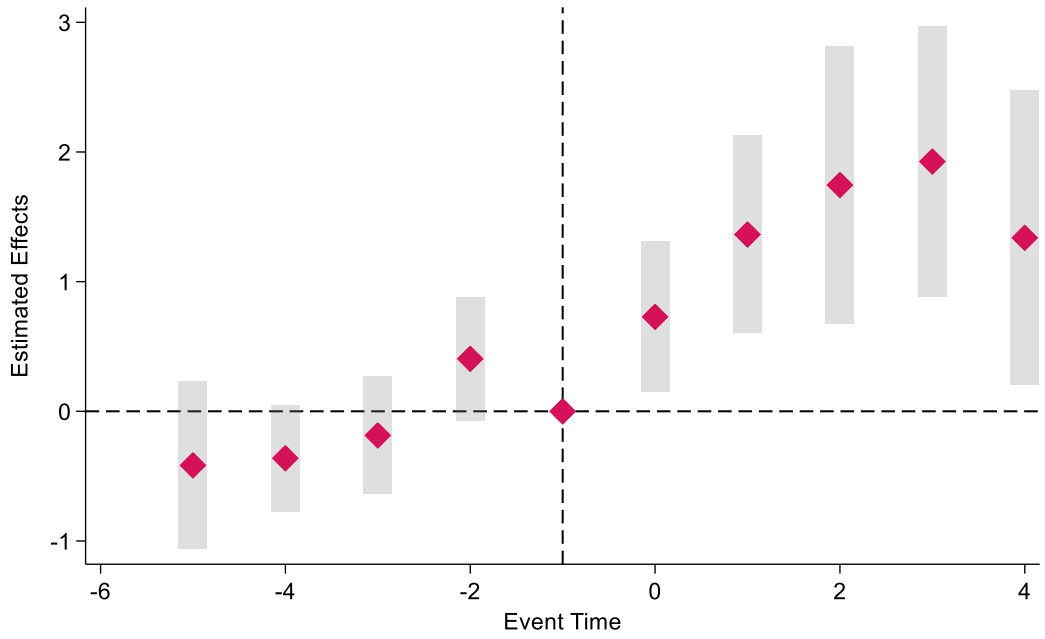
Figure E8
Event study plots using alternative outcome measures

This figure reports the event study plots using alternative measures of the outcome variables. The x-axis reports the year relative to CGC implementation in the sample, and the y-axis reports the level of estimated effect for the outcome variables, *Ratio2*, *Ratio3*, and *Ratio4*. The estimates and reported 95% confidence intervals represent unexplained trend differences in the outcome variable between treatment and comparison firms. Firm fixed effects, industry- year fixed effects, and prefecture-year fixed effects are included. Control variables include natural logarithm of total assets, natural logarithm of firm age, return on assets, debt scaled by total assets, and operating cash flow scaled by total assets. Standard errors are clustered at the firm level.

Panel A: *Ratio2*



Panel B: ***Ratio3***



Panel C: ***Ratio4***

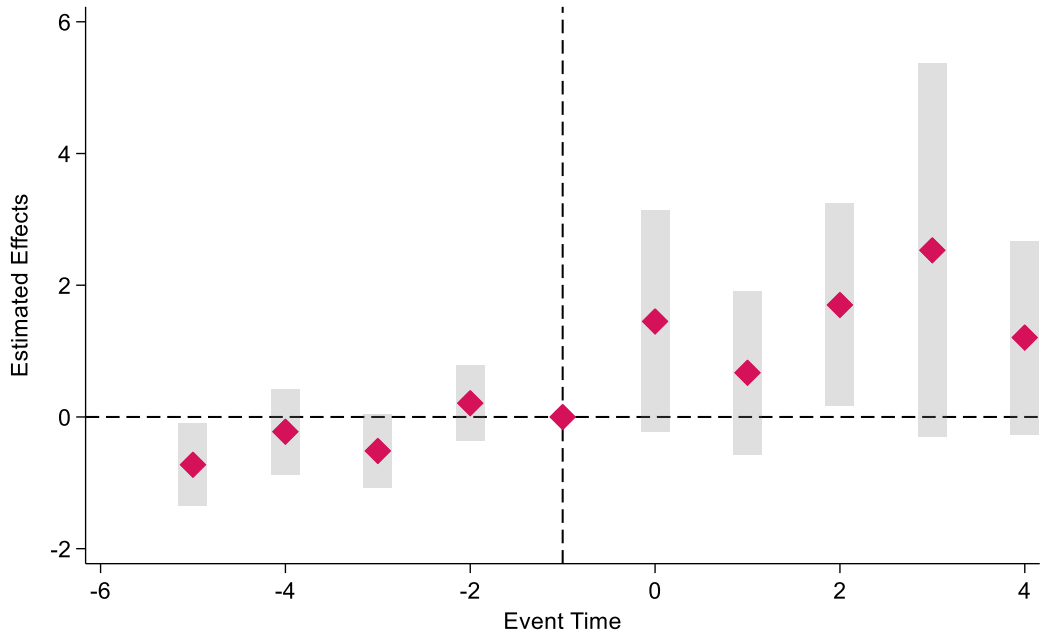


Table E1
Summary statistics for other variables

This table reports summary statistics for variables used in mechanism analysis, robustness checks, and additional tests for possible arguments. The six columns report the observations and distributional statistics (quartiles, mean, and standard deviation) of variables for the full pre-period (2010-2014) sample. A detailed definition of each variable is shown in Appendix A.

	Full pre-period sample					
	N	p25	Mean	p50	p75	SD
FO	10,785	0.0101	0.105	0.0543	0.164	0.123
Tobin's Q	10,785	0.803	1.088	0.939	1.131	0.777
SDOCF	2,157	0.0183	0.0410	0.0289	0.0479	0.0533
CFRATIO	10,785	0.0258	0.0532	0.0579	0.0977	0.336
Sales	10,785	13,819	251,499	40,705	143,970	934,707
INST	10,785	0.0176	0.139	0.0866	0.224	0.147
ROE	10,785	0.0247	0.0255	0.0536	0.0902	0.752
Ratio2	10,785	0.0408	0.805	0.108	0.271	5.247
Ratio3	10,785	0.0377	0.620	0.106	0.268	5.902
Ratio4	10,785	0.126	2.211	0.391	0.929	17.99
EMP	10,785	178	1,377	448	1,137	3,687
Capital	10,875	0	0.004	0	0	0.044
Cash	10,785	0.0834	0.181	0.147	0.239	0.136

Table E2
Doubly-robust difference-in-differences estimation

This table estimates the baseline analysis, using DRDID (doubly-robust difference-in-differences) estimator from Callaway and Sant'Anna (2021). In column (1), I conduct DRDID estimation without covariates. In column (2), I conduct the same estimation as in column (1), including pre-treatment time-varying covariates *(Log TA*, *Log Age*, *ROA*, *LEV*, and *OCF*). In column (3), I conduct the same estimation as in column (2), including industry dummy variables. In column (4), I conduct the same estimation as in column (2), including prefecture dummy variables. In column (5), I conduct DRDID estimation including all time-varying covariates and time-invariant covariates. The main independent variable is *POST*, an indicator variable equal to one for treatment firms in the post-treatment period and zero otherwise. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ratio*	(2) *Ratio*	(3) *Ratio*	(4) *Ratio*	(5) *Ratio*
POST	0.842*** (0.216)	0.850*** (0.213)	0.874*** (0.222)	0.854*** (0.216)	0.896*** (0.228)
Observations	21,570	21,570	21,570	21,570	21,570
Time-Varying Covariates	No	Yes	Yes	Yes	Yes
Industry	No	No	Yes	No	Yes
Prefecture	No	No	No	Yes	Yes
Reference Year	2014	2014	2014	2014	2014

Table E3
Synthetic difference-in-differences estimation

This table estimates the baseline regressions, using synthetic difference-in-differences estimator from Arkhangelsky et al. (2021). I do not include control variables to avoid bias from covariates affected by the governance reform. To estimate standard errors, I adopt the block bootstrap approach and iterate resampling process 1000 times. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Ratio
	(1)
POST	0.7873***
	(0.2333)
Firm FE	Yes
Year FE	Yes
Observations	21,570

Table E4
Controlling for confounding events

This table presents the results of DID (difference-in-differences) and DRDID (doubly-robust difference-in-differences) estimations while controlling for confounding events. The study specifically focuses on the Stewardship Code (2014), which mandates that institutional investors engage in dialogue with firms, and the Ito Review (2014), which sets an 8% target return on equity (ROE) for Japanese firms. To assess whether the main results remain unchanged after considering these events, I conduct the estimation while additionally controlling for institutional investor ratio (*Inst*) and return on equity (*ROE*). I use two estimation approaches to adjust for these variables. First, as shown in Panel A, I include these two variables as control variables in the baseline regression model. Second, as shown in Panel B, I control for the pre-treatment status of these variables using DRDID estimator (Callaway and Sant'Anna, 2021). Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: DID					
	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	0.914***	1.056***	1.119***	1.067***	1.105***
	(0.259)	(0.285)	(0.341)	(0.296)	(0.348)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.394	0.396	0.402	0.400	0.406
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year×Industry FE	No	No	Yes	No	Yes
Year×Prefecture FE	No	No	No	Yes	Yes
Inst	Yes	Yes	Yes	Yes	Yes
ROE	Yes	Yes	Yes	Yes	Yes
Panel B: DRDID					
	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	1.062***	0.960***	1.030***	0.943***	1.013***

	(0.286)	(0.236)	(0.249)	(0.237)	(0.247)
Observations	21,570	21,570	21,570	21,570	21,571
Time-Varying Covariates	No	Yes	Yes	Yes	Yes
Industry	No	No	Yes	No	Yes
Prefecture	No	No	No	Yes	Yes
Inst	Yes	Yes	Yes	Yes	Yes
ROE	Yes	Yes	Yes	Yes	Yes

Table E5
Using the winsorized sample

This table estimates the baseline regressions, using the winsorized sample. Each control variable is winsorized at the 0.5% level on both sides of the distribution. The same set of control variables and fixed effects as in the baseline models are included. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	0.650***	0.769***	0.767***	0.766***	0.730***
	(0.177)	(0.269)	(0.253)	(0.286)	(0.259)
Observations	20,957	20,957	20,957	20,957	20,957
R-squared	0.451	0.452	0.458	0.457	0.464
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table E6
Using additional treatment and comparison firms

This table estimates the baseline regressions, including additional treatment and comparison firms. Specifically, I include firms on Nagoya stock exchange as additional treatment firms, and include firms on Sapporo and Fukuoka stock exchanges as additional comparison firms. The same set of control variables and fixed effects as in the baseline models are included. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	0.870***	0.992***	1.060***	1.045***	1.098***
	(0.217)	(0.249)	(0.288)	(0.281)	(0.315)
Observations	22,370	22,370	22,370	22,370	22,370
R-squared	0.395	0.396	0.402	0.399	0.405
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table E7
Using firms with at least 50 workers

This table estimates the baseline regressions, using the sample limited to firms with at least 50 workers. The same set of control variables and fixed effects as in the baseline models are included. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ratio	*Ratio*	*Ratio*	*Ratio*	*Ratio*
POST	0.443***	0.510***	0.527***	0.518***	0.559***
	(0.153)	(0.153)	(0.189)	(0.159)	(0.201)
Observations	19,639	19,639	19,639	19,639	19,639
R-squared	0.357	0.361	0.370	0.362	0.370
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Table E8
Using alternative outcome measures

This table estimates the baseline regressions, using alternative measures of the outcome variable. *Ratio2* is shareholder-worker ratio calculated from dividends and share repurchases on non-consolidated financial statement. *Ratio3* is shareholder-worker ratio adjusted for corporate financing. *Ratio4* is shareholder-worker ratio where payouts are replaced by corporate profits. The same set of control variables and fixed effects as in the baseline models are included. Robust standard errors are clustered at the firm level and are reported in parentheses. Definitions of all variables are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively

Panel A: *Ratio2*					
	(1)	(2)	(3)	(4)	(5)
	Ratio2	*Ratio2*	*Ratio2*	*Ratio2*	*Ratio2*
POST	0.984***	1.135***	1.241***	1.152***	1.232***
	(0.239)	(0.285)	(0.341)	(0.299)	(0.354)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.311	0.313	0.318	0.316	0.321
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes
Panel B: *Ratio3*					
	(1)	(2)	(3)	(4)	(5)
	Ratio3	*Ratio3*	*Ratio3*	*Ratio3*	*Ratio3*
POST	1.299***	1.477***	1.545***	1.527***	1.574***
	(0.305)	(0.332)	(0.35)	(0.362)	(0.373)
Observations	21,570	21,570	21,570	21,570	21,570

R-squared	0.339	0.34	0.359	0.346	0.366
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Panel C: *Ratio4*

	(1)	(2)	(3)	(4)	(5)
	Ratio4	*Ratio4*	*Ratio4*	*Ratio4*	*Ratio4*
POST	1.821***	1.770***	1.851***	1.757***	1.751***
	(0.524)	(0.518)	(0.594)	(0.561)	(0.627)
Observations	21,570	21,570	21,570	21,570	21,570
R-squared	0.338	0.345	0.351	0.348	0.353
Controls	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Year ×Industry FE	No	No	Yes	No	Yes
Year ×Prefecture FE	No	No	No	Yes	Yes

Appendix References

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