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Evidence from reforms to Japanese corporate law

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Abstract

We study how the tradeoff between stock liquidity and stock market scrutiny affects corporate policy. We use panel data that cover public and private companies in Japan. The sample used in our main analysis consists of firms whose ownership is concentrated, in order to mitigate the agency conflicts that come from the separation of ownership and control. We exploit legal reforms as the source of exogenous variation in stock market listing. We find that listing reduces debt financing, especially dependence on long-term debt. In addition, we determine that listing improves profitability. These findings support the stock liquidity hypothesis. However, we also provide evidence that is not consistent with this hypothesis, by reporting that stock market listing does not reduce cash holdings. We discover that it increases capital expenditures and decreases R&D expenses. The two contrasting effects demonstrate the relevance of short-termism pressure from the stock market. We also observe that listing increases dividends and reduces tax aggressiveness, in line with the stock market scrutiny hypothesis. Our findings suggest that the liquidity-scrutiny tradeoff of stock market listing has heterogeneous effects on firm policy, depending on its nature.

JEL classifications: G30

Keywords: stock market listing, stock liquidity, stock market scrutiny, natural experiment

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1. Introduction

Finance theory suggests that stock market listing has the potential to affect corporate policy. Key advantages of listing is better access to capital markets and the liquidity benefit. On the other hand, it has several disadvantages. First, the separation of ownership and control is generally more widespread among public companies than private ones (Jensen and Meckling 1976). Thus, agency conflicts between owners and managers are more severe among public companies. Second, public companies can suffer from short-termism pressure from stock markets (Stein 1989). Such pressure can distort managerial decisions of public companies. These arguments imply that stock market listing can affect corporate policy through different channels, based on stock liquidity, agency conflict, and the stock market scrutiny hypotheses.[1]

Despite theoretical progress, empirical evidence to test these hypotheses is limited. A primary reason is the difficulty in accessing data for private companies. The emerging literature overcomes this issue by taking advantage of unique data in various countries.[2] However, these studies face two issues. First, listing is a firm's choice. Thus, there is a concern about self-selection. Second, the ownership structures of public companies are generally dispersed compared to private ones. Therefore, public companies are more likely to suffer from agency problems. As a result, it is difficult to untangle the impact of stock market listing from the difference in ownership.

We address these issues by exploiting unique panel data and a natural experiment in Japan. Our data cover both public and private companies. Therefore, we can directly investigate the impact of stock market listing. However, the simple comparison between public and private companies does not identify the causal impact of listing because of the difference in ownership structure between them. To control for the ownership effect, our main analysis uses a variation in

[1] See Bharath and Dittmar (2010) and Pagano, Panetta, and Zingales (1998) for more comprehensive discussion on the costs and benefits of stock market listing.

[2] This footnote briefly introduces related papers that study the impact of stock market listing on various aspects of corporate policy. The following three papers study financing: Brav (2009) uses U.K. data to study capital structure; Saunders and Steffen (2011) employ U.K. data to study loan costs; and Schenone (2010) uses U.S. data to study bank lending. The following three papers study other financial decisions: Gao, Harford, and Li (2013) use U.S. data to study cash holdings; Maksimovic, Phillips, and Yang (2013) use U.S. data to study mergers and acquisitions; and Michaely and Roberts (2012) use U.K. data to study dividends. The following five papers study real activities: Asker, Farre-Mensa, and Ljungqvist (2015), Gilje and Taillard (2012), and Phillips and Sertsios (2015) use U.S. data to study capital expenditures; and Bernstein (2015) and Acharya and Xu (2016) use U.S. data to study innovation. The following two papers study various factors in firm policy: Bakke, Jens, and Whited (2012) use U.S. data; and Pagano, Panetta, and Zingales (1998) use Italian data.

stock market listing among firms whose ownership is concentrated. We define a firm's ownership as concentrated if the firm is a subsidiary: a company where a majority of its shares are held by its parent company. In other words, our main analysis makes a comparison between public and private subsidiaries. In this sample construction, we can reduce the agency problems associated with the separation of ownership and control and focus on the liquidity-scrutiny tradeoff of stock market listing.

There remains a concern that OLS estimates can be biased because the decision whether to list stocks is endogenous. We exploit novel legal reforms called a "sea change" in corporate law (Milhaupt 2006) as a natural experiment. These reforms provide a parent company the legal rights necessary to forcibly squeeze out its subsidiary's minority shareholders. As a result, a considerable number of minority shareholders of partly owned subsidiaries were squeezed out, and these subsidiaries became wholly owned. A firm must delist its stock when it becomes wholly owned. There still remains a concern about self-selection, because not all subsidiaries have become subjected to squeezing out. We construct an instrument to explain the cross-sectional variation in squeeze outs using institutional details of corporate law. Thus, we use a difference-in-differences framework with an IV strategy for identification.

Our main data sources are the Financial Statements Statistics by Corporations, collected by the Japanese Ministry of Finance, and the Basic Survey of Japanese Business Structure and Activities, collected by the Ministry of Economy, Trade, and Industry. The final sample consists of 89,943 firm-year observations between 1994 and 2012. 63.3% of them are private. Subsidiaries account for 11.7% of the public observations and 63.9% of the private ones. In the natural experiment, the number of observations is 23,728. The treatment group consists of 2,700 observations. There are 528 private observations in the treatment group after 1999. Thus, we observe a considerable time-series variation in stock delisting among observations in the treatment group.

We use the following comprehensive set of outcome variables in regression: leverage, bank dependence, internal capital market dependence, cash holdings, dividends, capital expenditures, R&D expenses, profitability, and tax aggressiveness. Individual outcome variables reflect different aspects of the liquidity-scrutiny tradeoff of stock market listing. We aim to understand this tradeoff through the comparison of these outcome variables. This approach is in contrast to

that used in most related studies, which focus on one outcome variable.

The first set of outcome variables reflects financing: leverage, bank dependence, and internal capital market dependence. We ascertain that stock market listing reduces leverage. This suggests that there exists a debt-equity substitution, which is consistent with Brav (2009). We also observe that listing reduces bank dependence, but not internal capital market dependence. This implies that there exists a substitution between direct and indirect financing only through external loan markets. Furthermore, we see that listing reduces long-term debt through all three types of debt financing measures. These findings are consistent with the stock liquidity hypothesis.

The second set deals with other financial decisions: cash holdings and dividend payments. First, we find that listing increases cash holdings, but the coefficients are not statistically significant. This at least suggests that listing does not decrease cash holdings. This finding is not consistent with the stock liquidity hypothesis, because public companies should have a weak precautionary motive if stock liquidity affects cash policy. Our finding is in line with Gao, Harford, and Li (2013), who observe that stock market listing significantly increases cash holdings. Second, we ascertain that listing increases dividends. This is consistent with the stock market scrutiny hypothesis, because public companies have an incentive to increase dividends to mitigate information asymmetry with investors. Our findings on these two outcome variables suggest that the stock liquidity hypothesis does not have a dominant effect on financial policy, and we provide evidence that the stock market scrutiny hypothesis can explain corporate financial policy.

The third set involves real activities: capital investment and R&D investment. First, we determine that stock market listing increases capital expenditures. This suggests that the stock liquidity benefit dominates the cost associated with short-termism pressure from stock markets. Our finding is consistent with Bakke, Jens, and Whited (2012) and inconsistent with Asker, Farre-Mensa, and Ljungqvist (2015). Second, we observe that listing reduces R&D expenses. This suggests that the cost from short-termism pressure outweighs the liquidity benefit, perhaps because R&D investment requires longer-term commitment than capital investment. These findings on real activities highlight the assertion that the liquidity-scrutiny tradeoff can yield contrasting effects on firm policy.

The fourth set addresses profitability and tax positions. First, we ascertain that stock market

listing increases profitability. This finding is in contrast to Pagano, Panetta, and Zingales (1998). Our finding suggests that the financing benefit allows public companies to improve profitability once we mitigate self-selection. This is in line with the stock liquidity hypothesis. Second, we find that stock market listing reduces tax aggressiveness. This is consistent with Hanlon, Mills, and Slemrod (2007) and Pagano, Panetta, and Zingales (1998). This suggests that public companies' shareholders suspect managerial diversion when they observe a high level of tax aggressiveness, as suggested by recent theory (Crocker and Slemrod 2005; Desai and Dharmapala 2006), in line with the stock market scrutiny hypothesis. Our findings on these two outcome variables also highlight the idea that the liquidity-scrutiny tradeoff can generate different consequences.

The magnitude of the impact of stock market listing is economically significant as well. For example, it increases the capital expenditures-to-assets ratio by 4.001 percentage points and reduces the R&D expenses-to-assets ratio by 1.213 percentage points. This comparison implies that stock market listing induces a considerable substitution of real activities, based on the liquidity-scrutiny tradeoff. In addition, given that the former coefficient is over three times as large as the absolute value of the latter, the liquidity-scrutiny tradeoff yields a positive effect on investment behavior measured by the total expenditures for the two investments. In other words, our evidence suggests that stock market listing encourages investment behavior.

Our paper is related to the literature that studies the relationship between stock market listing and corporate policy. Our paper has three unique characteristics. First, we exploit legal reforms as the source of variation in listing. Given that a change in public policy is an exogenous event for firms and we use an IV strategy to further mitigate the endogeneity, our findings are relatively immune to the criticism that unobservable, time-varying factors affect our estimates. Thus, our paper makes a contribution in terms of the methodology that aims to identify the causal impact of stock market listing.[3]

[3] Several recent papers exploit various features to mitigate the self-selection concern of the decision to list stocks. Asker, Farre-Mensa, and Ljungqvist (2015) use differences in the VC supply to construct the instrument. Acharya and Xu (2016) and Bakke, Jens, and Whited (2012) use a regression discontinuity design, with the listing requirements of NASDAQ as the threshold of the discontinuity. Bernstein (2015) compares completed and withdrawn IPO filings using NASDAQ fluctuations as the instrument. Gao, Harford, and Li (2013) employ industry-level underwriter concentration as the instrument. Phillips and Sertsios (2015) use a change in Medicare coverage as the exogenous variation in investment opportunities. Saunders and Steffen (2011) use the distance to London's capital markets to construct the instrument.

Second, we make a public-private comparison among subsidiaries. This research design allows us to study the liquidity-scrutiny tradeoff after mitigating the impact from the difference in ownership structure. This aspect is worth emphasizing because it is uncommon in the literature that ownership structure data are available for both public and private companies.[4] Our framework is particularly useful for mitigating the ownership effect because the parent company, as the top shareholder, does not change after listing or delisting stocks. Therefore, the cost of equity financing from losing control through a change in ownership structure (Amihud, Lev, and Travlos 1990; Stulz 1988) is relatively less of a concern in this framework. Thus, we can concentrate on the liquidity-scrutiny tradeoff.

Third, we examine the tradeoffs of stock market listing using various outcome variables in a unified research design. We find that the consequence of the liquidity-scrutiny tradeoff depends on the type of corporate policy. In other words, our findings imply that neither stock liquidity nor stock market scrutiny is a dominant factor in corporate decision-making. Our paper is complementary to related studies focusing on specific outcome variables: we provide first evidence in bank dependence, internal capital market dependence, and R&D expenses; we confirm the robustness of the existing evidence in leverage (Brav 2009), cash holdings (Gao, Harford, and Li 2013), dividends (Michaely and Roberts 2012), capital expenditures (Bakke, Jens, and Whited 2012), and tax aggressiveness (Hanlon, Mills, and Slemrod 2007; Pagano, Panetta, and Zingales 1998); and we find evidence that is contrary to the existing evidence in capital expenditures (Asker, Farre-Mensa, and Ljungqvist 2015) and profitability (Pagano, Panetta, and Zingales 1998).

The rest of this paper is organized as follows. Section 2 presents our hypothesis and reviews the literature. Sections 3 explains the data and the research design. Section 4 shows our estimation results. Section 5 provides our conclusions.

2. Hypothesis and literature

This section explains our hypotheses about the impact of stock market listing on individual outcome variables. The outcome variables are: leverage, bank dependence, internal capital

[4] The data used in Brav (2009) and Michaely and Roberts (2012) cover ownership information basically only among private companies, and the data are only for the cross section of the last available data period. The data used in Gao, Harford, and Li (2013) cover ownership data only among public companies. The data used in Asker, Farre-Mensa, and Ljungqvist (2015) do not cover ownership information, and they use each firm's legal form as the substitute.

market dependence, cash holdings, dividends, capital expenditures, R&D expenses, profitability, and tax aggressiveness. Individual outcome variables aim to capture various aspects of the tradeoff of listing among the following three factors: stock liquidity, agency conflicts between managers and shareholders, and stock market scrutiny. We also review the literature in this section.

2.1. Financial policy

2.1.1. Financing (leverage, bank dependence, internal capital market dependence)

Brav (2009) argues that the availability of a public stock market makes public equity less costly than private equity. As a result, the relative cost of equity to debt is lower for public companies than private ones. Thus, stock market listing should reduce leverage. Brav (2009) provides evidence that is consistent with this hypothesis. Another reason behind this debt-equity tradeoff is that concentrated ownership among private companies makes corporate control more valuable for shareholders, and thus they are less likely to sell stock to avoid losing control (Amihud, Lev, and Travlos 1990; Stulz 1988). Our research design, using variations in stock market listing among subsidiaries, helps mitigate the effect through such a change in ownership, because the top shareholder remains the same after the listing or delisting of a subsidiary's stock. Thus, the main purpose of using leverage as one of the outcome variables in our paper is to test the stock liquidity hypothesis. We can also examine the robustness of the finding by Brav (2009), who relies on a non-experimental research design, about the endogeneity of stock market listing.

If there exists a substitution within capital market financing instruments, we should observe that the difference in leverage between public and private companies is especially large for long-term debt. In contrast, Brav (2009) provides evidence that private companies maintain liquidity using short-term debt by showing that the ratio of short-term to long-term debt is larger among private companies. We examine how stock market listing affects the maturity of debts in our experimental setting.

Furthermore, we predict a substitution between direct financing through stock markets and indirect financing. Testing this relationship is important to examine the implication of Pagano, Panetta, and Zingales (1998) and Saunders and Steffen (2011). They determine that loan costs in external markets are higher among private companies than public ones. This cost disadvantage itself should discourage private companies from relying on indirect financing. However, they do

not directly test whether the loan cost disadvantage is large enough for private companies to abandon indirect financing measures. We test whether the direct-indirect substitution or the loan cost disadvantage has greater weight.

Our research design and data further allow us to examine the impact of stock market listing on borrowing from external and internal capital markets. Borrowing from internal capital markets can work as a counterfactual of that from external loan markets. Given that information asymmetry should be less severe for internal markets than external markets, we predict that private companies rely more on internal capital markets than external loan markets. In addition, we have data of different maturities (i.e., long and short) for these two indirect financing measures. Thus, we can examine the potential heterogeneity of the impact of stock market listing, which comes from the difference in maturity and in the structure of loan markets (i.e., internal or external).

2.1.2. Cash holdings

Gao, Harford, and Li (2013) explain the tradeoff on cash holdings. Better access to capital markets provides less incentive for public companies to hold cash based on a precautionary motive (Keynes 1936). This argument is in line with the stock liquidity hypothesis. On the other hand, dispersed ownership among public companies can lead their managers to hold cash to avoid monitoring from external markets (Jensen 1986). This argument is in line with the agency conflict hypothesis. Thus, we do not have an a priori clear prediction on cash holdings because of the competing effects of stock market listing on cash holdings.

Gao, Harford, and Li (2013) observe that public companies hold more cash. Thus, the agency conflicts have the dominant effect on cash holdings according to their estimation results. We may not find the same result as Gao, Harford, and Li (2013) because the sample of our main analysis consists of subsidiaries whose ownership is concentrated, and therefore the agency problems are relatively less serious in our research design. In other words, the stock liquidity benefit can play a more important role in our study than in theirs. Therefore, it is an empirical question whether the stock liquidity benefit outweighs the costs from agency conflicts, and as a result, whether listing reduces cash holdings. If we find a negative relation between listing and cash holdings, we can interpret this result as evidence for the stock liquidity hypothesis.

2.1.3. Payout policy

Michaely and Roberts (2012) compare dividend policy between public and private companies. Their main purpose is to test the dividend smoothing hypothesis (Lintner 1956). In other words, their focus is on the time-series volatility of dividend streams, not the level of dividend payments. They provide evidence that the tendency of dividend smoothing is stronger among public companies than private ones.[5]

Michaely and Roberts (2012, Table 1) present summary statistics that public companies pay higher dividends than private ones. This finding can be explained by the asymmetric response of the stock price to a change in dividends (Grullon, Michaely, and Swaminathan 2002); the negative response to the stock price when a firm cuts dividends is larger relative to the positive response when a firm increases dividends. Given that stock prices are publicly available only for public companies, stock market listing provides public companies with an incentive to avoid cutting dividends, which in turn leads them to pay higher dividends than private companies. We test this prediction, which is in line with the stock market scrutiny hypothesis. A characteristic of our paper is that we examine this causality among firms with similarly concentrated ownership structures. This framework allows us to isolate the effect of stock market listing that comes from the observability of stock prices on dividend payments.[6] Thus, our framework is suitable for testing whether the stock market scrutiny hypothesis can explain how a firm determines its level of dividends.

2.2. Real decision

2.2.1. Capital expenditures

Bakke, Jens, and Whited (2012) find that stock market listing increases investment. The authors argue that the liquidity benefit for public companies can induce them to invest more than private companies. This interpretation is in line with the theory by Levine (1991) and is supported by the empirical finding by Becker-Blease and Paul (2006) that stock liquidity increases capital expenditures, using data of public companies. In contrast, Asker, Farre-Mensa, and Ljungqvist (2015) find that listing reduces investment. The authors attribute their finding to short-termism pressure from the stock market on public companies (Stein 1989). Thus, the empirical findings

[5] This empirical finding by Michaely and Roberts (2012) is consistent with the survey evidence by Brav, Graham, Harvey, and Michaely (2005), who ascertain that private companies are more likely than public ones to change dividends when earnings change temporarily.

[6] Michaely and Roberts (2012) compare public companies with dispersed private companies. However, they do not make a public-private comparison among firms whose ownership is concentrated.

from the previous studies are mixed because of the relative importance of the tradeoff between stock liquidity and stock market scrutiny. We test which factor carries the greatest weight in our research design.

A caveat of these two papers is that they do not control for the ownership structure.[7] Thus, the agency problems that come from the separation of ownership and control can affect their findings.[8] In contrast, our research design allows us to isolate the liquidity-scrutiny tradeoff of stock market listing on capital expenditures. Thus, our framework may be able to reconcile the mixed findings in the literature.

2.2.2. R&D expenses

Acharya and Xu (2016) emphasize that there are differences between capital investment and R&D investment. For example, the authors cite evidence that the existence of short-term institutional investors impedes innovation (Fang, Tian, and Tice 2014). In other words, short-termism pressure is especially problematic for R&D because the time span of R&D investment is longer than that of capital investment. As in capital expenditures, the decision regarding R&D expenses involves the tradeoff between stock liquidity and stock market scrutiny, especially short-termism pressure. Therefore, we do not have an a priori clear prediction on how stock market listing affects R&D. However, the argument by Acharya and Xu (2016) implies that the costs incurred by short-termism pressure can outweigh the financing advantages when we use R&D as the outcome variable. In this case, we predict that listing reduces R&D expenses.

Acharya and Xu (2016) find that public companies in industries that are dependent on external financing have more R&D expenses than their private counterparts. However, Acharya and Xu (2016) do not examine the average impact of stock market listing on R&D. We test this average impact to examine the liquidity-scrutiny tradeoff of listing.[9] We also compare our finding in R&D with that in capital investment to explore whether the relative importance of stock liquidity and stock market scrutiny affects firms' incentive for real activities.

2.3. Other outcome variables

[7] Asker, Farre-Mensa, and Ljungqvist (2015) use a data set that does not provide ownership information. They use the information of legal forms of companies to show that investment sensitivity among private companies does not depend on the legal forms. However, they do not directly test whether the ownership structure affects investment.

[8] For example, Cho (1998) empirically investigates the relationship between ownership and investment.

[9] Another related paper is Bernstein (2015). The author finds that stock market listing reduces the quality of innovation. Bernstein (2015) uses patent data as the measure of innovation quality. The author does not use R&D expenses as the outcome variable.

2.3.1. Profitability

Pagano, Panetta, and Zingales (1998) detect a negative correlation between stock market listing and profitability by focusing on IPOs. The authors argue that their finding is consistent with the window of opportunity hypothesis; firms go public when they view other firms in the same industry as overvalued. This argument implies that stock market listing is an endogenous decision for firms. Our experimental research design allows us to examine the causal impact of listing on profitability.

The stock liquidity hypothesis suggests that listing increases profitability because of better access to capital. In contrast, if agency conflicts exert the dominant effect, listing should reduce profitability. Given that our research design mitigates the agency conflicts, we predict that it is likely that it increases profitability. In other words, we predict that the liquidity benefit of listing has the dominant impact on profitability.

2.3.2. Tax aggressiveness

The literature since Crocker and Slemrod (2005) and Desai and Dharmapala (2006) implies that stock market scrutiny reduces tax aggressiveness. Tax avoidance is inevitably obscured from the tax authority, which in turn creates information asymmetry between managers and outside investors. Consequently, managers may seek opportunities for rent seeking when they establish tax sheltering schemes. Predicting the possibility of managerial diversion, outside investors can discount the firm value when managers are tax aggressive.[10] Negative investor response to tax sheltering provides public company managers with an incentive to be less tax aggressive. Thus, the stock market scrutiny hypothesis predicts that listing reduces tax aggressiveness.

Our prediction is in line with the findings by Hanlon, Mills, and Slemrod (2007) and Pagano, Panetta, and Zingales (1998).[11] In contrast, some recent papers pose a question on the complementarity between managerial rent extraction and tax aggressiveness (for example, Armstrong et al. 2015)[12]. Our paper contributes to this debate, using variations in stock market

[10] Hanlon and Slemrod (2009) present event study evidence that stock prices react negatively to news that firms are involved in tax sheltering.

[11] The tax aggressiveness measures of these two papers are different from ours. Hanlon, Mills, and Slemrod (2007) employ the level of proposed tax deficiencies, and Pagano, Panetta, and Zingales (1998) use tax divided by operating income.

[12] Armstrong et al. (2015) use data that cover public companies. They find no evidence for a relationship between corporate governance quality and tax aggressiveness on average. They report heterogeneity of the relationship, depending on the level of tax aggressiveness.

listing for identification.

3. Data and research design

3.1. Data description

The main data source is the Financial Statements Statistics by Corporations (FSSC data). The data are collected annually by the Japanese Ministry of Finance. They cover standard items in unconsolidated financial statements of non-financial corporations in Japan.[13] We use the data between 1994 and 2012.[14] A feature of the FSSC data is that the data source is not publicly available financial statements. The Ministry requests that corporations submit their financial information under the Statistics Act.[15] This data collection process ensures that the FSSC data can cover both public and private companies.[16]

The FSSC data do not include information about the ownership structure of individual firms. We obtain this information and some other data, such as R&D expenses, from the Basic Survey of Japanese Business Structure and Activities (JBSA data), collected by the Ministry of Economy, Trade, and Industry.[17] The JBSA data provide information about parent company ownership ratios. We classify a firm's ownership structure as concentrated when its parent's ownership ratio is 50% or higher. We call such a firm a subsidiary. We merge the FSSC data with the JBSA data based on the accounting information.

We merge these data sets and keep necessary observations as follows. First, we retain observations whose lagged assets are one billion yen or higher. Private companies are smaller than public ones on average, and therefore we keep comparable observations in terms of firm

[13] The FSSC data provide information on borrowing from non-financial institutions. We interpret this item as borrowing from internal capital markets.

[14] The fiscal year in Japan starts in April and ends the following March. For example, the 2000 fiscal year runs from 2000 April to 2001 March.

[15] The Ministry of Finance conducts a population survey for large corporations and a sample survey for small corporations. The target of the population survey is all large corporations whose legal capital, which is one component of net worth, is 500 million yen or higher. The threshold of legal capital is 600 million yen in some years.

[16] The FSSC data do not classify whether individual firms are public or private. We use other data - Nikkei NEEDS FinancialQUEST collected by Nikkei Inc. - as a secondary source of information. FinancialQUEST includes all firms that are currently listed and those listed in the past. We match observations in the FSSC data with those in FinancialQUEST based on the corporate name and accounting data. We classify matched observations as public and unmatched ones as private.

[17] The target of the JBSA data is corporations whose legal capital is 30 million yen or larger and which have 50 or more employees. Therefore, the JBSA data impose a restriction on the target companies in terms of the number of employees, while the FSSC data do not. Firms used in the analysis are relatively large, and therefore this restriction is not relevant when merging these two datasets.

size. Second, we keep observations whose legal capital is over 100 million yen. This is because various tax breaks are available to small corporations, which are defined as firms whose legal capital is below this threshold of 100 million yen.[18] All variables are winsorized at 1% and 99% levels.

The final sample consists of 89,943 firm-year observations. There are 22,959 public observations and 56,929 private ones. Thus, 63.3% are private. During the data periods, 26,758 observations stayed public and 54,500 stayed private. The remaining 8,685 experienced a change, either from private to public or public to private. Therefore, we observe considerable within-firm variation in stock market listing or delisting. This within-firm variation plays a key role in identification because we use firm-fixed effect models.

3.2. Identification strategy

A simple comparison of outcome variables between public and private companies does not necessarily identify how stock market listing affects corporate policy. One reason is that ownership is more highly concentrated in private companies than public ones in general. Owners with large stakes have the incentive and ability to monitor managers. Thus, private company shareholders may be able to detect managerial rent-seeking more frequently than their public company counterparts.

To mitigate this ownership effect, we include ownership ratio as a control variable. In our experimental framework, we restrict observations to companies whose ownership is concentrated. We use business group structures as a source of variation in ownership structures.[19] Ownership of subsidiaries is concentrated by definition. Corporate law in Japan allows subsidiaries to list their stocks on stock exchanges.[20] Therefore, we can examine the impact of stock market listing on subsidiaries. An advantage of this research design is that intensive monitoring under concentrated ownership can mitigate agency conflicts in corporations. Another advantage is that

[18] When the regressand is tax aggressiveness measures, we keep observations where both the current before-tax profits and corporate tax liabilities are positive. This data restriction is necessary to define book tax differences, which will be discussed later.

[19] A business group consists of a parent company and its subsidiaries. Subsidiaries refer to companies where the majority of their shares are held by the parent company. The business group is a common corporate structure in Japan. For example, Toyota Motor Corporation is the parent company of Toyota Group, which consists of Toyota Motor Corporation and its subsidiaries. Business group structures have been used in some papers that take advantage of the Japanese economic environment for identification (for example, Hoshi, Kashyap, and Scharfstein 1991).

[20] For example, both Daihatsu Motor and Hino Motors are subsidiaries of Toyota Motor Corporation. They are listed on the Tokyo Stock Exchange.

the top shareholders, parent companies, do not change after listing or delisting stocks. Thus, concerns about the difficulty of untangling the effect of the change in listing on the change in control is less severe in our context.

The challenge is the endogeneity of a firm's decision to list its stock. We use a natural experimental research design with an IV strategy. The treatment in this experiment is two legal reforms that encourage delisting among public subsidiaries, which will be explained in detail below. The treatment group consists of all subsidiaries that were public before the legal reforms, because they have the potential to delist their stocks. The control group consists of subsidiaries that are private during all of the data periods, because these companies are not affected by the treatment that encourages delisting. A concern in this research design is that not all public subsidiaries delisted their stocks, which leads to a selection problem. We use an IV approach to deal with this endogeneity.

Our identification strategy takes advantage of a series of corporate law reforms instituted around 2000. These reforms involve fundamental changes in the legal environment which Milhaupt (2006) calls a "sea change". Two of the legal reforms allow parent companies to forcibly eliminate (or squeeze out) their subsidiaries' minority shareholders. These reforms play a key role in our paper since they provide an exogenous variation in stock market listing. The first reform is the share exchange system introduced in 1999. This system provides parent companies the necessary legal rights to eliminate their subsidiaries' minority shareholders by granting the parent companies' stocks to their subsidiaries' minority shareholders. In other words, squeeze out is implemented by exchanging parents' stocks for their subsidiaries'. The second reform is the introduction of a class shares subject to wholly call system introduced in 2006. This legal system has a similar economic function to share exchanges for the purpose of squeeze outs.[21]

If these reforms are effective, we expect to see a considerable change in the ownership structures of Japanese business groups after 1999. More specifically, a large number of partly

[21] The outline of the legal procedure for a squeeze out through the class shares subject to wholly call system is as follows. Companies first alter their articles of incorporation at their shareholders meetings, and all the common stocks are changed into class shares subject to wholly call. Next, the companies acquire all the stocks from the shareholders. The companies redeem the shares in such a way that their minority shareholders receive shares amounting to less than one unit. This procedure leaves the minority shareholders with no alternative but to receive cash in exchange for their stocks. As a result, the minority shareholders are squeezed out.

owned subsidiaries should have changed into wholly owned ones as a result of the reforms that have made squeeze out less costly. When companies change from partly owned to wholly owned subsidiaries, these subsidiaries must delist their stocks from stock exchanges because of the limited supply of stocks available to the markets.[22] Therefore, this change in ownership will increase the number of subsidiaries that go from public to private. This is the variation in stock delisting we use in the natural experiment.

It is questionable, however, whether this delisting can be treated as an exogenous variation because not all partly owned subsidiaries are squeezed out. In other words, there is a concern about selection bias. Institutional details of the legal systems provide an exogenous source of variation in the likelihood concerning which subsidiaries are to be squeezed out. In principle, these two legal systems require extraordinary resolutions that must be passed by 2/3 of the votes cast at shareholders meetings of both acquiring and acquired companies. Therefore, it is sufficient for a parent to own 2/3 of their subsidiary's stocks to eliminate the subsidiary's minority shareholders. If the parent owns less than 2/3, they need to issue tender offers before using these legal systems. This two-stage squeeze out strategy is common in practice. For example, Panasonic squeezed out Sanyo in 2011 following this procedure. Sanyo was one of Panasonic's partly owned, public subsidiaries. Panasonic's ownership of Sanyo was 50.2% in March 2010. Panasonic issued a tender offer in 2010, enabling it to raise its ownership to 80.1% by December 2010. Panasonic then exploited the share exchanges, and Sanyo became a wholly owned subsidiary of Panasonic in April 2011. Sanyo delisted its stock as a result.

The possibility of using this two-stage strategy allows us to construct an instrument for a private observation dummy variable. This dummy variable has a value of one when the observation is private. The literature demonstrates that tender offers involve a considerable amount of takeover premium.[23] Therefore, the total takeover premium that parents are required to pay in the first stage of the strategy is a decreasing function of their ownership. Our argument implies that the likelihood that the subsidiaries are squeezed out flatters out beyond the threshold of 2/3 ownership, because 2/3 ownership is sufficient to pass a proposal for squeeze out at a shareholders meeting.

We construct an instrument based on this idea. The instrument has a value of zero for all

[22] For example, at least 5% of stocks must be traded for firms to stay listed on the Tokyo Stock Exchange.
[23] Rossi and Volpin (2004) report that the average takeover premium is 40%.

observations before the legal reforms regarding squeeze outs. Therefore, a variation in the instrument is observed only after the reforms took effect. The instrument can take non-zero values only when the observations were public subsidiaries before the reforms, that is, when the firms are in the treatment group. This is because the purpose of the instrument is to explain the cross-sectional variation in stock delisting among firms in the treatment group.

The instrument cannot be time-dependent after the subsidiaries in the treatment group delist their stocks. For example, if we choose the subsidiaries' lagged ownership ratio as an instrument, this instrument is increased to 100 if the subsidiaries become private as a result of squeezing out. Therefore, this instrument and the private dummy variable can exhibit a mechanical, positive correlation after the squeezing out. In addition, using this instrument can create another problem because parent companies can adjust their subsidiaries' ownership stakes some years before using the legal systems. This ownership adjustment creates a positive correlation between this instrument and the private observation dummy before the squeezing out.

We construct an instrument to avoid these issues. We evaluate the ownership ratio of the subsidiaries one year before the introduction of the reforms. This instrument captures the likelihood that public subsidiaries are subjected to squeezing out when the legal systems are not available. This instrument takes the same value in 1999 and afterwards to avoid a mechanical correlation between the instrument and the private observation dummy. For example, suppose that a parent's ownership ratio of its public subsidiary is 60% in 1998. The instrument is zero up to 1998, and 60 from 1999 on.

As long as the legislation of these legal systems is not anticipated, parent companies do not have an incentive to adjust the ownership of their subsidiaries before the legal reforms. According to the following newspaper articles, official discussions on the introduction of the legal systems started in July 1998. On July 9, 1998, The Nikkei reported that the commercial law committee of the legislative council of the Ministry of Justice released an interim report suggesting an introduction of the share exchange system. On November 26, 1998, The Nikkei reported that the government decided to introduce the share exchanges in 1999. Therefore, the legal reform might have been anticipated one year before the actual introduction of the legal system. Although the main IV regression uses 1998 as the year to evaluate the ownership ratio, we alternatively use 1997 or 1996 to construct an instrument to check the robustness of our

findings.[24]

A concern about this research design is that the instrument may reflect information about past ownership structures before the reforms. As a result, the exclusion restriction can be violated because we argue that ownership is a factor that can affect corporate policy. In addition, there is a difference between subsidiaries in the treatment group and those in the control group because the former are listed, and thus their ownership structures are likely to be more dispersed than those in the latter. We include the contemporaneous ownership ratio as a control variable to mitigate these concerns. There may remain a possibility that past ownership has a direct impact on current corporate policy. We conduct falsification tests by regressing the past ownership ratio on the outcome variables using the data periods before the introduction of the reforms. Insignificant estimates from this test support the assertion that the exclusion restriction is not violated.

3.3. Estimation model and variable definition

We explain the estimation procedure. The main regressor is a private company dummy variable. Since stock market listing is a firm's choice, we use the IV strategy as we have discussed in the previous subsection. All the regressands are normalized by assets. Most of the regressands are straightforward to define, other than the tax aggressiveness measure. Thus, we explain the tax aggressiveness measure at length below.

The literature on tax aggressiveness uses a book tax difference to evaluate corporate tax aggressiveness. A standard book tax difference is defined as income before taxes minus estimated taxable income, which is tax liabilities divided by corporate tax rates, normalized by lagged assets (Manzon and Plesko 2002).[25] A larger Manzon-Plesko book tax difference implies

[24] Because of the unbalanced panel structure, we may not know the ownership ratios in 1998. In this case, we use the ownership ratios in 1997. We repeat this procedure when ownership in the year of interest is not observed. Another issue raised from the unbalanced panel structure is that we cannot classify subsidiaries that existed only in 1999 and afterwards as either belonging in the treatment group or in the control group, because the group classification is based on information before the legal reforms. We therefore removed these subsidiaries from the sample used for our experimental research design.

[25] The FSSC data cover information on tax liabilities of individual corporations. Available information on tax liabilities varies across years as follows: only corporate income taxes paid are available between 1994 and 1998; only corporate income tax expenses, which take into account deductible temporary differences, are available between 1999 and 2003; and both corporate income taxes paid and corporate income tax expenses are available after 2004. We use corporate income taxes paid as the measure of tax liabilities, except for between 1999 and 2003. During these five years, we use corporate income tax expenses instead. Another issue worth mentioning is that the Japanese tax system introduced a consolidated taxation system in 2002. This system provides business groups the option to file a consolidated tax return. In other words, this system allows a business group to offset losses with gains elsewhere in the group. Our tax aggressiveness measures are constructed based on individual tax returns, even when the firms use the

that a firm pays less corporate taxes relative to its accounting profitability. Thus, firms take an aggressive tax position when the Manzon-Plesko book tax difference is large.

Recent studies do not necessarily accept the Manzon-Plesko book tax difference as an adequate measure of corporate tax aggressiveness because public company managers have an incentive to manage earnings to exceed analysts' forecasts. Therefore, earnings management can increase the Manzon-Plesko book tax difference in the absence of tax avoidance. Isolating the earnings management effect from the Manzon-Plesko book tax difference is especially important for our purpose because analysts' forecasts are more likely to affect the incentive of public company managers, who pay attention to stock prices, which are publicly observable. As a result, earnings management can affect a public company's book tax difference more than that of a private company, if we follow Manzon and Plesko (2002).

To eliminate the earnings management effect from the Manzon-Plesko book tax difference, we use another type of book tax difference introduced by Dharmapala and Desai (2006). The Desai-Dharmapala book tax difference removes the earnings management effect from the Manzon-Plesko book tax difference to isolate the tax avoidance effect. More specifically, Dharmapala and Desai (2006) first regress the Manzon-Plesko book tax difference on total accruals with firm-fixed effects, where total accruals measure the degree to which firms can manage earnings.[26] The residual from this regression is the Desai-Dharmapala book tax difference. Formally, Dharmapala and Desai (2006) use the regression represented by

$$\mathrm{MPBTD}_{it} = \beta \mathrm{TotalAccrual}_{it} + \mu_i + \varepsilon_{it}$$

where index i represents the company, index t represents the year, MPBTD is the Manzon-Plesko book tax difference, TotalAccrual is total accruals, μ is firm-fixed effects, and ε is error terms. A larger Desai-Dharmapala book tax difference implies that the observations are more tax aggressive. We use the Desai-Dharmapala book tax difference as the measure of corporate tax aggressiveness.

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consolidated taxation system. Thus, our results are not affected by the introduction of the consolidated tax filing system.

[26] This paper defines total accruals based on the balance sheet approach suggested by Hribar and Collins (2002). This variable is defined by [(change in current assets) - (change in cash) - (change in current liabilities) + (change in short term debt) – depreciation] divided by lagged assets.

We turn to the explanation of estimation techniques for all the outcome variables. First, we use cross-sectional regression. We observe a considerable cross-sectional variation in stock market listing. Thus, starting with cross-sectional regression is useful for understanding our topic of interest. In addition, since some of the related papers use only cross-sectional regression, this analysis allows us to compare our results with those in these previous studies. We include an industry-year dummy to control for industry-year level economic shocks. We also include the log of assets, the firm age, and the squared firm age as control variables to capture firm-year level characteristics.[27] Furthermore, we include parents' ownership ratios, which measure the parents' monitoring intensity.

Second, we use the firm-fixed effect model. This framework mitigates concerns that time-invariant, firm-level heterogeneity causes a correlation between stock market listing and corporate policy. Some of the related studies also use the firm-fixed effect model. This framework also allows us to examine the robustness of the findings of the previous studies that use only the cross-sectional model. The cross-sectional model and the firm-fixed effect model are represented by

$$\text{Outcome}_{it} = \beta_1 \text{Private}_{it} + \gamma X_{it} + \mu_i + \eta_{jt} + \epsilon_{it} \quad (1)$$

where index i represents the company, index t represents the year, index j represents the industry, Outcome is various outcome variables, Private is the private company dummy variable, X is the matrix that includes control variables (parent's ownership ratio, log of assets, firm age, and squared firm age), μ represents firm-fixed effects and does not depend on i when we use cross-sectional models, η is industry-year fixed effects, and ε is error terms. We use robust standard errors clustered at the firm level.

The third methodology takes advantage of the squeeze-out legal reforms. This methodology seeks to eliminate a potential bias in OLS estimates associated with the endogeneity of stock market listing by using a difference-in-differences framework with the IV strategy. A difference

[27] Controlling for firm size is common in the literature because public firms are larger than private companies on average, and firm size can affect corporate policy, which creates endogeneity. For example, large firms may have a weaker incentive to hold cash based on a precautionary motive. We also include firm age and its square to capture the life cycle effect. For example, DeAngelo, DeAngelo, and Stulz (2006) find evidence that supports the life cycle theory of dividends, using retained earnings as their proxy for the firm's life cycle.

in the data composition between the previous two methodologies and this third one is that the former include all firms as the sample while the latter includes only subsidiaries because the legal reforms used in the third methodology are relevant only for subsidiaries. We use firm-fixed effect models in this third methodology. The first-stage and second-stage regressions are respectively represented by

$$\text{Private}_{it} = \delta \text{Past ownership IV}_{it} + \gamma X_{it} + \mu_i + \eta_{jt} + \epsilon_{it} \quad (2)$$

$$\text{Outcome}_{it} = \beta \widehat{\text{Private}}_{it} + \gamma X_{it} + \mu_i + \eta_{jt} + \epsilon_{it} \quad (3)$$

where equation (2) is the first-stage regression, equation (3) is the second-stage one, Past ownership IV is the ownership ratio evaluated principally in 1998, which is capped at 66.67, and $\widehat{\text{Private}}$ is the predicted value of Private from the first-stage regression.[28] We use robust standard errors clustered at the firm level.

Panel A of Table 1 reports the mean and standard deviations of individual variables. We separately report the statistics of public and private observations. The second row shows that 11.7% of the public observations are subsidiaries. These observations play key roles in the natural experiment. This table supports the assertion that private observations are more concentrated than public ones, and the mean parent ownership ratio is 57.7% among private observations and 7.3% among public observations.

Panel B of Table 1 reports the number of observations used in the natural experiment. This panel shows that the treatment and control groups consist of 2,700 and 21,028 observations, respectively. There are 528 private observations in the treatment group after 1999, which accounts for about 30% of the observations in the treatment group after 1999. This observation suggests that a considerable number of public subsidiaries delisted their stocks as a result of the legal reforms. Thus, this panel suggests that we can use the difference-in-differences framework in which the legal reforms are the treatment effect.

4. Result

Table 2 presents the first-stage estimation results to examine the strength of the instrument.

[28] Note that the treatment dummy variable, which takes a value of one when the subsidiary is in the treatment group, is absorbed by the firm-fixed effects.

Columns (1) – (2) display the estimation results when we include all the observations used in the experimental framework, and columns (3) – (4) display the estimation results when we include the subsample used when the regressand is the tax aggressiveness measure. The F-statistic is over 60 in columns (1) – (2) and over 30 in columns (3) – (4). Therefore, the instrument is sufficiently strong. The past ownership instrument coefficients are positive and significant at the 1% level in all columns. Therefore, a subsidiary is more likely to be subjected to squeezing out when its parent's past ownership is higher. Our findings do not change when we include the parent's current ownership ratio as a control variable (columns (2) and (4)). This result provides evidence that past ownership itself explains a variation in stock delisting.

Each table in the following subsections reports the estimation results in order to examine the impact of stock market listing on various outcome variables. In each table, columns (1) – (3) use all the observations, and columns (4) – (8) use the subsample for the experimental framework. Columns (1) – (4) use OLS, and columns (5) – (8) use the IV strategy for estimation. Columns (2), (3), and (6) – (8) include the control variables in addition to the industry-year dummy. Columns (5) and (6) use 1998 as the year to evaluate the instrument. Columns (7) and (8) use 1997 and 1996, respectively, to evaluate the instrument to examine the robustness of the year to evaluate the instrument.

We treat column (6) as the main model. In other words, our evaluation of the estimation results is basically based on the estimate in column (6). This is because the model in column (6) uses the IV strategy, where the instrument is evaluated in 1998, and this column includes the full set of control variables. When necessary, we discuss our estimation results from other columns. This discussion is useful for comparing our findings with those from related studies that use non-experimental frameworks. In addition, we can examine whether our research design mitigates the endogeneity by comparing our results from the IV strategy with those from OLS. This comparison helps us assess the consequence of self-selection in the decision to list stocks.

4.1. Financial policy

4.1.1. Financing (leverage, bank dependence, internal capital market dependence)

Table 3 reports the estimation results when the regressand is leverage. The private dummy coefficients are positive and significant across all columns. Thus, stock market listing reduces leverage. This result implies a substitution between equity financing and debt financing. In

addition, this finding is consistent with the stock liquidity hypothesis and is in line with and quantitatively similar to Brav (2009). Brav (2009, Table IV) reports that the difference in leverage is 11.9 with the firm fixed effect model. Our estimate from column (6), 12.5, is close to this figure. Given that Brav (2009) treats the listing status as exogenous, we provide support for the assertion that the findings of Brav (2009) are robust with respect to self-selection.

Table 4 reports the estimation results when the regressand is short-term debt. Columns (1) – (4) of this table report that stock market listing reduces short-term debt. However, the coefficients are not significant under the IV strategy in columns (5) – (8). This is a consequence of large standard errors under the IV strategy. For example, the coefficients are almost identical between columns (4) using OLS and (5) using IV, 2.26 and 2.43, respectively. The coefficients are not significant in column (5) because the standard error is over three times as large as that in column (4). Thus, our evidence that stock market listing decreases short-term debt is weak in the statistical sense.

Table 5 reports the estimation results when the regressand is long-term debt. The coefficients are positive and significant across all columns. Thus, we find clear evidence that stock market listing reduces long-term debt. This finding is consistent with the prediction that the substitution of financing instruments is clear among long-term measures. It is also consistent with the descriptive evidence of Saunders and Steffen (2011, Table 3). In contrast, our findings from Tables 4 and 5 may seem to be inconsistent with Brav (2009), who finds that stock market listing increases the ratio of short-term debt to long-term debt, because we find that the negative impact of stock market listing on long-term debt is clear, but the impact is not clear on short-term debt. However, when we use the ratio of short-term debt to long-term debt as the regressand, we do not obtain significant coefficients. Therefore, our finding about the difference in the impact of stock market listing on short-term and long-term debt is not necessarily inconsistent with Brav (2009).

Table 6 reports the estimation results when the regressand is bank dependence. The private dummy coefficients are positive and significant at the 1% level across all columns. Thus, stock market listing reduces bank dependence. Tables 7 and 8 report the estimation results when the regressands are short-term and long-term bank borrowing, respectively. The private dummy coefficients are positive and significant at the 1% level across all columns in these two tables as

well. Thus, we find clear evidence for the substitution between equity financing and bank dependence, independent of maturity.[29] This finding is consistent with the stock liquidity hypothesis. This finding also suggests that banks play especially important roles among private companies. Our finding can be inconsistent with Pagano, Panetta, and Zingales (1998) and Saunders and Steffen (2011), who find that loan costs are higher among private companies. One interpretation that reconciles our finding with theirs is that the financing needs from banks outweigh the disadvantages of loan costs among private companies.[30]

Table 9 reports the estimation results when the regressand is internal capital market dependence. Our main model in column (6) shows that stock market listing increases dependence on internal capital markets. This result is not consistent with our prediction that private companies use internal capital markets as a substitute for equity. An interpretation of this finding, coupled with our findings from Tables 6 – 8, is that private subsidiaries rely more on banks than internal capital markets as indirect financing measures. In other words, this interpretation implies a substitution between banks and internal capital markets.[31]

Tables 10 and 11 report the estimation results when the regressands are short-term and long-term borrowing from internal capital markets, respectively. These two tables provide a contrasting finding; stock market listing increases short-term borrowing and decreases long-term borrowing from internal capital markets. The negative private dummy coefficient in Table 10 implies that private companies do not maintain liquidity from internal capital markets. Our finding in Table 7 suggests that private companies rely on banks, rather than internal capital markets, as sources of short-term funding. The positive private dummy coefficient in Table 11 suggests that long-term financing from internal capital markets is a substitute for equity

[29] The magnitude of the coefficients is considerably different between OLS and IV. For example, the coefficient is 4.991 in column (4) under OLS and 34.584 in column (5) under IV. The latter is larger than the mean of the bank dependence of 20.209 in Table 1. This large coefficient is at least partly a consequence of large standard errors; the standard error in column (4) is 1.084 and that in column (5) is 5.423. The confidence interval of column (6) suggests that stock market listing increases bank dependence between 28.925 and 55.386.

[30] Another finding from Tables 6 – 8 is that parent ownership coefficients are negative and significant across all columns. This finding suggests that there is a substitution between banks and internal capital markets in the indirect financing measures. We discuss this substitution in Tables 9 – 11 as well.

[31] Table 9 also reports that parent ownership is positively associated with internal capital market dependence across all columns. This finding suggests that a closer relationship with the parent enables firms to have better access to internal capital markets. Another finding is the positive coefficients on the private dummy in columns (1) and (2) that do not use the IV strategy. These estimates highlight the importance of considering the endogeneity of stock market listing. These positive estimates might be observed because subsidiaries that need to rely on internal capital markets are more likely to be private. Thus, the OLS estimates can exhibit an upward bias.

financing. This finding, together with that in Table 8, implies that equity-debt substitution is strong for long-term debt instruments.

4.1.2. Cash holdings

Table 12 reports the estimation results when the regressand is cash holdings. Columns (1) – (3) report significant negative coefficients on cash holdings. Columns (5) – (8) also report negative coefficients, but they are not statistically significant. This is a consequence of large standard errors under the IV strategy, as in Table 4. For example, the coefficients and standard errors are -1.195 and 0.390 in column (3) under OLS and -3.502 and 3.722 in column (6) under IV, respectively. Thus, we provide weak evidence in a statistical sense that stock market listing increases cash holdings. Our evidence is not consistent with the stock liquidity hypothesis, and it suggests that this hypothesis is not the dominant theory behind the effect of listing. This conclusion is in contrast to our finding in the previous subsection, where stock liquidity works as a critical factor in listing.

In terms of economic significance, our estimate from column (6) of -3.502 is close to that from Gao, Harford, and Li (2013, Table 4) of -3.86.[32] A difference with their paper is the research design. Given that the sample of our main analysis consists of subsidiaries, the agency conflicts among public companies are less severe in our paper because of the high ownership concentration. As a result, agency problems that can induce public companies to increase cash holdings may be relatively weak. We may be able to attribute our insignificant result at least partly to our research design.

4.1.3. Payout policy

Table 13 reports the estimation results when the regressand is dividends. Our main model in column (6) shows that stock market listing increases dividend payments. This finding is consistent with the descriptive evidence found by Michaely and Roberts (2012, Table 1) and with the prediction that the asymmetric response of a change in dividend to stock price is more problematic for public companies. Thus, our finding is in line with the stock market scrutiny theory.[33]

[32] Gao, Harford, and Li (2013) use the natural log of the cash-to-assets ratio as the regressand. Thus, the value of -3.86 is not directly from Table 4 of their paper.

[33] Another finding is that a parent's ownership ratio is positively correlated with dividend payments. This finding suggests that parents use their subsidiaries' profits as a source of income. Our finding is not consistent with the life cycle theory of dividends because firm age is not significantly related to dividend payments.

Our finding is similar to that of Michaely and Roberts (2012) in terms of economic significance as well. The magnitude of the private dummy coefficient is 0.632 standard deviations of the dividend payments in our main estimates. Michaely and Roberts (2012, Table 1) report that the difference in dividend payments between public and private companies is 0.45 standard deviations of their dividend payment measures, which is dividends divided by profits. Thus, we find evidence that is close to the literature using our experimental research design.

4.2. Real decision

4.2.1. Capital expenditures

Table 14 reports the estimation results when the regressand is capital expenditures. Our main model shows that stock market listing increases investment. This finding is consistent with Bakke, Jens, and Whited (2012), but not with Asker, Farre-Mensa, and Ljungqvist (2015).[34] Our finding suggests that the liquidity benefits outweigh the costs incurred by short-termism pressure from the stock market regarding capital expenditures.

Our estimate of -4.001 from column (6) is in the middle of the range of estimates in Bakke, Jens, and Whited (2012, Table 3); their estimates range from -1.83 to -5.98. Table 14 also highlights the importance of endogeneity associated with stock market listing. Column (1) shows that stock market listing decreases investment. Once we control for parent's ownership and other factors in column (2) or firm-level heterogeneity as well in column (3), we obtain insignificant coefficients. In contrast, when we use the IV strategy, we always obtain significant negative coefficients.

4.2.2. R&D expenses

Table 15 reports the estimation results when the regressand is R&D expenses. Our main model shows that stock market listing reduces R&D. This finding is consistent with our prediction that short-termism pressure on public companies is especially problematic for R&D, and the cost can outweigh the financing benefit.

This table also highlights the importance of the endogeneity of stock market listing, as in Table 14. Columns (1) – (3) show that stock market listing increases R&D expenses. This result

[34] One explanation that can reconcile our finding with Asker, Farre-Mensa, and Ljungqvist (2015) is the positive coefficient on the parent's ownership ratio in columns (6) - (8) of Table 14. Given that Asker, Farre-Mensa, and Ljungqvist (2015) do not control for the degree of ownership concentration, the difference in ownership structure may lead to a negative bias on the coefficient for the decision for stock market listing because public companies are generally less concentrated than private ones.

holds even when we control for parent's ownership or time-invariant firm characteristics in column (2) or (3). We obtain positive and significant coefficients only when we use the IV strategy in columns (5) – (8).

Our contrasting findings regarding capital expenditures and R&D expenses suggest that the tradeoff between stock liquidity and stock market scrutiny, especially short-termism pressure, has qualitatively different effects on real activity, depending on its nature. In other words, our findings imply that neither of the factors in stock market listing has the dominant impact on the investment decision. In addition, our findings imply that stock market listing increases the capital expenditures-to-assets ratio by 4.001 percentage points and reduces the R&D expenses-to-assets ratio by 1.213 percentage points (from column (6) of Tables 14 and 15). Because the former effect is over three times as large as the absolute value of the latter, stock market listing increases the level of total investments. This finding implies that stock market listing yields quantitatively positive consequences with respect to real activities.

4.3. Other outcome variables

4.3.1. Profitability

Table 16 reports the estimation results when the regressand is profitability. The private dummy coefficients are negative and significant across all columns. Therefore, stock market listing increases profitability. This finding suggests that the stock liquidity benefit has the dominant effect on profitability and is consistent with the stock liquidity hypothesis.

Our finding is the opposite of that of Pagano, Panetta, and Zingales (1998). The difference in the findings can come from the characteristic that their paper focuses on IPOs. Therefore, their results can be a consequence of window dressing at the time of the IPO. However, this interpretation may not be particularly convincing because the private dummy coefficients are negative even under OLS in columns (1) – (4), which do not consider self-selection in stock market listing. Another feature of our paper is that we reduce agency problems through the construction of our sample. Thus, the financing benefit can be more pronounced than in their paper, and as a result, we may observe the positive impact of stock market listing on profitability.

4.3.2. Tax aggressiveness

Table 17 reports the estimation results when the regressand is tax aggressiveness. Our main model shows that stock market listing reduces tax aggressiveness. This finding is consistent with

the empirical findings of Hanlon, Mills, and Slemrod (2007) and Pagano, Panetta, and Zingales (1998)[35] and also with the recent theory that emphasizes stock market scrutiny as a determinant of tax aggressiveness through the complementarity between managerial rent extraction and tax aggressiveness (Crocker and Slemrod 2005; Desai and Dharmapala 2006). In contrast, our finding is not consistent with recent findings that pose a question on such complementarity (e.g., Armstrong et al. 2015).

The magnitude of our estimates is larger than those reported in the literature. We find that stock market listing increases our tax aggressiveness measure by 1.246 standard deviations. The magnitude reported in Hanlon, Mills, and Slemrod (2007, Table 4 - 5) and Pagano, Panetta, and Zingales (1998, Table IV) are 0.2 and 0.12 standard deviations of their tax measures, respectively. The magnitudes of 0.12 – 0.2 standard deviations are close to the estimate from column (5) of our paper, which uses OLS. Thus, our finding suggests that addressing endogeneity is an important consideration in terms of the economic significance.

4.4. Exclusion restriction

In this subsection, we provide evidence to address concerns about our IV estimates: weak instruments and exclusion restrictions. We have provided evidence that the instrument is strong by showing that the F-statistic of the excluded instrument is over 30 in Table 2. We have also used years other than 1998 to evaluate the instrument in Tables 3 - 17. To determine if the exclusion restriction is not violated, we conduct a falsification test to show past ownership does not predict future corporate policy.

We use the data periods before the introduction of the legal systems for squeeze out. Before this introduction, the indirect effect of a parent's past ownership on future corporate policy through a change in the costs for squeeze out does not exist. Therefore, insignificant coefficients on the past ownership instrument support the hypothesis that the exclusion restriction is not violated. Specifically, we restrict the data periods from 1994 to 1998. We estimate the equation represented by

$$\text{Outcome}_{it} = \delta \text{Past ownership IV}_{it-n} + \gamma X_{it} + \mu_i + \eta_{jt} + \epsilon_{it}$$

[35] The tax aggressiveness measures of these two papers are different from ours. Hanlon, Mills, and Slemrod (2007) use the level of proposed tax deficiencies. Pagano, Panetta, and Zingales (1998) employ taxes paid divided by operating income.

where index i represents the company, index t represents the year, index j represents the industry, index n represents the number of lags of the past ownership IV (i.e., the ownership ratio, capped at 66.67), Outcome is an outcome variable, X is a matrix that includes the control variables (parent's ownership ratio, log of assets, firm age, and firm age squared), μ represents firm-fixed effects, η is industry-year fixed effects, and ε is error terms.

Table 18 reports the coefficients on the past ownership instrument. We do not report the estimates of other regressors for simplicity. Note that each regression includes only one of the past ownership instruments. For example, the second column of this table does not include three past ownership instruments at the same time. Table 18 reports that none of the coefficients are statistically significant. Thus, this table supports the claim that the exclusion restriction is not violated.

5. Conclusion

We examine the impact of stock market listing on various aspects of corporate policy. The empirical literature has faced two challenges: limited access to data that cover private companies and endogeneity associated with stock market listing. We address these issues by taking advantage of unique data of Japanese firms and legal reforms that provide exogenous variation in stock market listing. In particular, our main analysis compares public and private subsidiaries. This comparison helps us examine the relative importance of the tradeoff between stock liquidity and stock market scrutiny in corporate decisions. We use various outcome variables that can capture different aspects of the liquidity-scrutiny tradeoff, in order to understand the causal effect of stock market listing from a comprehensive perspective.

Our study provides evidence that the tradeoff of stock market listing can yield a heterogeneous effect on corporate decision making. Some evidence is consistent with the stock liquidity hypothesis. For example, we find that listing reduces leverage, especially long-term debt. In contrast, we also find some other evidence that is more likely to be explained by the theory that emphasizes the role of stock market scrutiny. For example, we find that listing increases dividends and reduces tax aggressiveness.

We find that the tradeoff of stock market listing has the opposite effect on real activities, depending on the time horizon of the investment. More specifically, we find that listing increases

capital expenditures and reduces R&D expenses. Our findings also suggest that listing increases total expenditures for the two investments.

Our study demonstrates that using various outcome variables in a unified research design is important for examining the consequences of the liquidity-scrutiny tradeoff of stock market listing. Thus, our paper is complementary to the emerging literature that studies the impact of listing on individual outcome variables.

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Table 1
Summary statistics and the number of observations

Panel A of Table 1 separately reports summary statistics for public, private, and total observations. The data periods are between 1994 and 2012. Private dummy is a variable that has a value of one when the observation is a private company. Subsidiary dummy is a variable whose value is one when the majority of a firm's shares are held by another company (the parent company). Parent's ownership is the ownership ratio of the parent company when the observation has a parent. This value is zero when the observation does not have a parent. Ln(assets) is the natural log of assets. Age is the firm's age. Leverage is liabilities divided by lagged assets, expressed as a percentage. Borrowing from internal capital markets is borrowing from other firms divided by lagged assets as a percentage. Profitability is after-tax profit divided by lagged assets as a percentage. Tax aggressiveness is the Desai-Dharmapala book tax difference divided by lagged assets as a percentage. This variable is the residual from the regression of the Manzon-Plesko book tax difference on total accruals and firm-fixed effects. Manzon-Plesko book tax difference is before-tax profits minus estimated taxable income, which is corporate tax liabilities divided by corporate income tax rates. All other variables are normalized by lagged assets as a percentage, and their definition is self-explanatory.

Panel A: Summary statistics

	Private observations			Public observations			Total observations		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Private dummy	1	0	56929	0	0	33014	0.633	0.482	89943
Subsidiary dummy	0.639	0.480	56929	0.117	0.321	33014	0.447	0.497	89943
Parent's ownership	57.704	44.789	56929	7.339	19.356	33014	39.218	44.683	89943
Ln(assets)	9.286	1.185	56929	10.527	1.366	33014	9.741	1.390	89943
Age	37.198	19.640	56929	52.727	18.376	33014	42.898	20.594	89943
Squared age	1769	1624	56929	3118	2011	33014	2264	1891	89943
Leverage	70.951	27.798	56929	51.051	22.036	33014	63.647	27.555	89943
Short-term debt	52.264	25.936	56929	35.654	17.608	33014	46.167	24.569	89943
Long-term debt	18.455	18.437	56929	15.390	12.525	33014	17.330	16.580	89943
Bank borrowing	22.215	23.587	56929	16.750	16.690	33014	20.209	21.478	89943
Short-term bank borrowing	13.223	15.953	56929	10.255	11.540	33014	12.134	14.561	89943
Long-term bank borrowing	8.895	13.755	56929	6.486	8.666	33014	8.011	12.193	89943
Borrowing from internal capital markets	6.542	16.068	56929	0.682	3.274	33014	4.391	13.241	89943
Short-term borrowing from internal capital markets	4.219	11.662	56929	0.535	2.539	33014	2.867	9.571	89943
Long-term borrowing from internal capital markets	2.102	8.374	56929	0.142	1.614	33014	1.382	6.799	89943
Cash holdings	9.592	11.063	56929	12.308	11.009	33014	10.589	11.121	89943
Dividends	0.666	1.677	56929	0.843	0.911	33014	0.731	1.447	89943
Capital expenditures	4.137	5.616	56929	3.717	4.472	33014	3.983	5.229	89943
R&D expenses	0.853	1.853	56929	1.609	2.149	33014	1.130	2.000	89943
Profitability	1.345	5.774	56929	1.583	5.003	33014	1.433	5.505	89943
Tax aggressiveness	0.023	2.451	29569	0.027	1.949	25694	0.025	2.232	55263

Table 1 - Continued

Panel B of Table 1 reports the number of observations used in the natural experiment. The sample consists of subsidiaries. We divide the year periods between 1994 - 1998 (before the legal reforms) and 1999- 2012 (after the reforms). We also divide the observations into the treatment and control group.

Panel B: The number of observations in the natural experiment

	1994-1998	1999-2012	Total
Treatment group	895	528 (private) 1277 (public)	2700
Control group	7689	13339	21028

Table 2
First-stage result

This table presents the first stage estimation results of the IV strategy to examine whether the instrument can explain the decision to delist a stock. The data periods are between 1994 and 2012. The sample includes only subsidiaries. The sample used in columns (3) – (4) include only observations with positive after-tax profits. The dependent variable is private dummy, which is a variable that takes on a value of one when the observation is a private company. The past ownership instrument is constructed from the parent's ownership ratio evaluated principally in 1998, and is capped at 66.67. The past ownership instrument takes this ownership ratio when the subsidiaries are in the treatment group and the year periods are from 1999 on. Otherwise, the past ownership instrument has a value of zero. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Private dummy			
	(1)	(2)	(3)	(4)
Past ownership instrument	0.004***	0.003***	0.004***	0.003***
	(0.001)	(0.000)	(0.001)	(0.000)
Parent's ownership		0.007***		0.009***
		(0.001)		(0.001)
Ln(assets)		-0.004		0.005
		(0.006)		(0.009)
Age		-0.001*		-0.001
		(0.001)		(0.001)
Squared age		0.000		0.000
		(0.000)		(0.000)
Firm-fixed effect	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes
F-statistic of excluded instrument	64.13	61.89	39.78	35.61
Observations	23728	23728	13531	13531

Table 3
Stock market listing and leverage

This table presents the estimation results to examine whether stock market listing affects leverage. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is leverage that is liabilities divided by lagged assets in percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Leverage							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	18.874***	15.718***	5.815***	3.594***	9.620**	12.508**	11.276*	9.957*
	(0.472)	(0.587)	(0.732)	(1.311)	(4.654)	(6.266)	(6.060)	(5.154)
Parent's ownership		0.055***	-0.064***			-0.086*	-0.073	-0.068
		(0.006)	(0.019)			(0.052)	(0.048)	(0.042)
Ln(assets)		-0.046	-13.306***			-14.170***	-14.191***	-14.174***
		(0.193)	(0.564)			(0.949)	(0.954)	(0.955)
Age		-0.464***	-0.212***			-0.105	-0.102	-0.107
		(0.034)	(0.052)			(0.092)	(0.093)	(0.094)
Squared age		0.005***	0.002***			0.001	0.001	0.001
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 4
Stock market listing and short-term debt

This table presents the estimation results to examine whether stock market listing affects short-term debt. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is short-term debt divided by lagged assets expressed as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Short-term debt							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	15.933***	8.376***	2.849***	2.263**	2.431	3.563	3.022	3.013
	(0.384)	(0.466)	(0.619)	(1.139)	(3.868)	(5.198)	(5.089)	(4.317)
Parent's ownership		0.129***	-0.010			-0.019	-0.015	-0.021
		(0.005)	(0.017)			(0.042)	(0.040)	(0.034)
Ln(assets)		-0.352**	-11.131***			-12.643***	-12.632***	-12.623***
		(0.150)	(0.464)			(0.791)	(0.795)	(0.797)
Age		-0.389***	-0.070			-0.106	-0.103	-0.104
		(0.030)	(0.045)			(0.078)	(0.079)	(0.079)
Squared age		0.004***	0.000			0.001	0.001	0.001
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 5
Stock market listing and long-term debt

This table presents the estimation results to examine whether stock market listing affects long-term debt. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is long-term debt divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Long-term debt							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	2.732***	7.308***	3.074***	1.465*	7.412***	8.571**	8.156**	6.851**
	(0.260)	(0.332)	(0.484)	(0.820)	(2.758)	(3.780)	(3.705)	(3.103)
Parent's ownership		-0.075***	-0.057***			-0.064**	-0.058*	-0.047*
		(0.004)	(0.013)			(0.032)	(0.031)	(0.026)
Ln(assets)		0.347***	-1.600***			-0.565	-0.595	-0.585
		(0.117)	(0.377)			(0.576)	(0.579)	(0.580)
Age		-0.062***	-0.149***			-0.024	-0.023	-0.027
		(0.021)	(0.035)			(0.062)	(0.063)	(0.063)
Squared age		0.001***	0.002***			0.001	0.001	0.001
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 6
Stock market listing and bank borrowing

This table presents the estimation results to examine whether stock market listing affects borrowing from banks. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is borrowing from banks divided by lagged assets in percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Bank borrowing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	5.087***	10.949***	6.141***	4.991***	34.584***	42.155***	40.133***	34.985***
	(0.379)	(0.491)	(0.585)	(1.084)	(5.423)	(6.750)	(6.471)	(5.079)
Parent's ownership		-0.126***	-0.153***			-0.336***	-0.305***	-0.255***
		(0.005)	(0.017)			(0.058)	(0.053)	(0.043)
Ln(assets)		-0.727***	1.651***			3.925***	3.806***	3.812***
		(0.154)	(0.447)			(0.767)	(0.765)	(0.759)
Age		-0.036	-0.371***			-0.090	-0.089	-0.094
		(0.028)	(0.049)			(0.088)	(0.089)	(0.088)
Squared age		0.001**	0.004***			0.002*	0.002*	0.002*
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 7
Stock market listing and short-term bank borrowing

This table presents the estimation results to examine whether stock market listing affects short-term borrowing from banks. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is short-term borrowing from banks divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Short-term bank borrowing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	2.756***	4.202***	2.786***	3.237***	19.270***	23.798***	22.454***	19.947***
	(0.253)	(0.324)	(0.452)	(0.848)	(3.662)	(4.656)	(4.485)	(3.601)
Parent's ownership		-0.034***	-0.081***			-0.185***	-0.167***	-0.142***
		(0.003)	(0.013)			(0.040)	(0.037)	(0.031)
Ln(assets)		-0.730***	0.852***			1.846***	1.784***	1.794***
		(0.096)	(0.324)			(0.582)	(0.583)	(0.581)
Age		0.003	-0.133***			-0.019	-0.019	-0.020
		(0.019)	(0.036)			(0.065)	(0.066)	(0.065)
Squared age		0.000**	0.002***			0.001	0.001	0.001
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 8
Stock market listing and long-term bank borrowing

This table presents the estimation results to examine whether stock market listing affects long-term borrowing from banks. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is long-term borrowing from banks divided by lagged assets in percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Long-term bank borrowing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	2.249***	6.693***	3.372***	1.869***	15.501***	18.384***	17.836***	15.167***
	(0.192)	(0.267)	(0.372)	(0.470)	(2.465)	(3.192)	(3.121)	(2.437)
Parent's ownership		-0.092***	-0.071***			-0.149***	-0.138***	-0.113***
		(0.003)	(0.010)			(0.029)	(0.027)	(0.022)
Ln(assets)		0.012	0.885***			2.156***	2.100***	2.097***
		(0.087)	(0.257)			(0.376)	(0.376)	(0.374)
Age		-0.035**	-0.243***			-0.080	-0.079	-0.083*
		(0.015)	(0.026)			(0.050)	(0.050)	(0.050)
Squared age		0.000	0.003***			0.001*	0.001*	0.001**
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 9

Stock market listing and borrowing from internal capital markets

This table presents the estimation results to examine whether stock market listing affects borrowing from internal capital markets. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is borrowing from internal capital markets divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Borrowing from internal capital markets							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	5.920***	1.293***	0.373	0.891	-6.504***	-11.475***	-11.436***	-9.992***
	(0.165)	(0.159)	(0.332)	(0.872)	(2.506)	(3.199)	(3.154)	(2.579)
Parent's ownership		0.072***	0.077***			0.137***	0.132***	0.113***
		(0.003)	(0.013)			(0.029)	(0.028)	(0.024)
Ln(assets)		-0.159*	-2.134***			-3.864***	-3.788***	-3.797***
		(0.082)	(0.355)			(0.719)	(0.721)	(0.722)
Age		-0.188***	0.037			-0.072	-0.073	-0.074
		(0.019)	(0.034)			(0.064)	(0.065)	(0.065)
Squared age		0.002***	-0.000*			0.001	0.001	0.001
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 10

Stock market listing and short-term borrowing from internal capital markets

This table presents the estimation results to examine whether stock market listing affects short-term borrowing from internal capital markets. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is short-term borrowing from internal capital markets divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Short-term borrowing from internal capital markets							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	3.743***	0.712***	0.165	-0.654	-10.633***	-15.399***	-15.279***	-13.095***
	(0.118)	(0.105)	(0.257)	(0.657)	(2.244)	(2.965)	(2.912)	(2.292)
Parent's ownership		0.051***	0.067***			0.142***	0.135***	0.111***
		(0.002)	(0.011)			(0.027)	(0.026)	(0.022)
Ln(assets)		0.041	-1.660***			-3.298***	-3.230***	-3.242***
		(0.057)	(0.245)			(0.511)	(0.511)	(0.511)
Age		-0.111***	0.035			-0.085*	-0.089*	-0.088*
		(0.014)	(0.026)			(0.050)	(0.051)	(0.051)
Squared age		0.001***	-0.001***			0.001	0.001	0.001
		(0.000)	(0.000)			(0.001)	(0.001)	(0.001)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 11

Stock market listing and long-term borrowing from internal capital markets

This table presents the estimation results to examine whether stock market listing affects long-term borrowing from internal capital markets. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is long-term borrowing from internal capital markets divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Long-term borrowing from internal capital markets							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	1.964***	0.567***	0.231	1.564***	4.263***	3.454**	3.395**	2.753**
	(0.082)	(0.085)	(0.169)	(0.515)	(1.241)	(1.570)	(1.557)	(1.284)
Parent's ownership		0.018***	0.008			-0.002	-0.001	0.004
		(0.001)	(0.007)			(0.014)	(0.013)	(0.012)
Ln(assets)		-0.163***	-0.087			0.086	0.100	0.103
		(0.042)	(0.188)			(0.361)	(0.362)	(0.363)
Age		-0.071***	-0.021			-0.026	-0.024	-0.025
		(0.010)	(0.019)			(0.034)	(0.034)	(0.035)
Squared age		0.001***	0.000***			0.001	0.001	0.001
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 12
Stock market listing and cash holdings

This table presents the estimation results to examine whether stock market listing affects cash holdings. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is cash holdings divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Cash holdings							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	-2.800***	-1.691***	-1.195***	0.140	-2.722	-3.502	-3.112	-2.805
	(0.201)	(0.254)	(0.390)	(1.085)	(2.877)	(3.722)	(3.664)	(3.145)
Parent's ownership		-0.072***	-0.024***			0.014	0.010	0.006
		(0.002)	(0.008)			(0.027)	(0.025)	(0.021)
Ln(assets)		-1.619***	-2.704***			-1.999***	-1.986***	-1.991***
		(0.080)	(0.238)			(0.308)	(0.309)	(0.310)
Age		0.021	0.016			-0.017	-0.020	-0.019
		(0.013)	(0.020)			(0.033)	(0.034)	(0.034)
Squared age		-0.001***	-0.000			0.000	0.000	0.000
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 13
Stock market listing and dividend payments

This table presents the estimation results to examine whether stock market listing affects dividends. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is dividends divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dividend payments							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	-0.150***	-0.341***	-0.048	0.167	-0.525*	-0.915**	-0.923**	-0.807**
	(0.019)	(0.021)	(0.047)	(0.143)	(0.309)	(0.429)	(0.420)	(0.351)
Parent's ownership		0.004***	0.005***			0.009***	0.008***	0.007***
		(0.000)	(0.001)			(0.003)	(0.003)	(0.003)
Ln(assets)		0.072***	0.151***			0.148***	0.147***	0.148***
		(0.009)	(0.029)			(0.053)	(0.054)	(0.054)
Age		0.001	0.015***			0.007	0.007	0.007
		(0.002)	(0.003)			(0.006)	(0.006)	(0.006)
Squared age		-0.000***	-0.000***			0.000	0.000	0.000
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 14
Stock market listing and capital expenditures

This table presents the estimation results to examine whether stock market listing affects capital expenditures. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is capital expenditures divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Capital expenditures							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	0.534***	0.071	-0.307*	-0.378	-3.208***	-4.001**	-4.104**	-2.623**
	(0.066)	(0.081)	(0.183)	(0.308)	(1.218)	(1.622)	(1.616)	(1.247)
Parent's ownership		0.007***	-0.001			0.025*	0.025*	0.014
		(0.001)	(0.004)			(0.014)	(0.013)	(0.010)
Ln(assets)		0.169***	-1.428***			-1.279***	-1.279***	-1.276***
		(0.031)	(0.109)			(0.178)	(0.179)	(0.178)
Age		-0.033***	-0.022*			0.002	0.002	0.006
		(0.006)	(0.011)			(0.020)	(0.020)	(0.020)
Squared age		0.000**	0.000***			0.000	0.000	-0.000
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 15
Stock market listing and R&D expenses

This table presents the estimation results to examine whether stock market listing affects R&D expenses. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is R&D expenses divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	R&D expenses							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	-0.560***	-0.386***	-0.162***	0.037	0.749*	1.213**	1.259**	1.063**
	(0.036)	(0.040)	(0.056)	(0.163)	(0.433)	(0.560)	(0.554)	(0.468)
Parent's ownership		0.000	-0.000			-0.009**	-0.008**	-0.006*
		(0.000)	(0.001)			(0.004)	(0.004)	(0.003)
Ln(assets)		0.183***	-0.242***			-0.212***	-0.218***	-0.218***
		(0.016)	(0.033)			(0.059)	(0.059)	(0.059)
Age		-0.011***	-0.005			0.004	0.004	0.004
		(0.003)	(0.003)			(0.006)	(0.006)	(0.006)
Squared age		0.000***	0.000			-0.000	-0.000	-0.000
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 16
Stock market listing and profitability

This table presents the estimation results to examine whether stock market listing affects profitability. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is after-tax profit divided by lagged assets as a percentage. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Profitability							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	-0.222***	-0.345***	-0.666***	-1.088**	-5.594***	-7.744***	-7.331***	-6.269***
	(0.066)	(0.081)	(0.191)	(0.442)	(1.169)	(1.638)	(1.586)	(1.274)
Parent's ownership		0.001	0.007			0.051***	0.044***	0.037***
		(0.001)	(0.005)			(0.014)	(0.013)	(0.011)
Ln(assets)		0.152***	-1.472***			-0.853***	-0.845***	-0.837***
		(0.030)	(0.142)			(0.215)	(0.215)	(0.214)
Age		0.002	-0.010			-0.033	-0.031	-0.028
		(0.006)	(0.012)			(0.020)	(0.021)	(0.020)
Squared age		-0.000***	0.000			0.000*	0.000*	0.000
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	89943	89943	89943	23728	23728	23728	23556	23365

Table 17
Stock market listing and tax aggressiveness

This table presents the estimation results to examine whether stock market listing affects tax aggressiveness. The data periods are between 1994 and 2012. We use OLS or IV for estimation. When we use IV, the sample includes only subsidiaries. The dependent variable is the Desai-Dharmapala book tax difference divided by lagged assets as a percentage. The Desai-Dharmapala book tax difference is the residual from the regression of Manzon-Plesko book tax difference on total accruals and firm-fixed effects. Manzon-Plesko book tax difference is before-tax profits minus estimated taxable income, which is corporate tax liabilities divided by corporate income tax rates. When we use IV, the private dummy is predicted from the first stage estimation. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Tax aggressiveness							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private dummy	0.017	-0.064*	0.066	0.487**	2.493***	2.780***	2.766***	2.100***
	(0.028)	(0.034)	(0.098)	(0.209)	(0.705)	(0.962)	(0.967)	(0.681)
Parent's ownership		0.003***	0.001			-0.021**	-0.019**	-0.012*
		(0.000)	(0.003)			(0.009)	(0.009)	(0.006)
Ln(assets)		0.023*	-0.616***			-0.672***	-0.670***	-0.659***
		(0.013)	(0.070)			(0.109)	(0.110)	(0.109)
Age		-0.012***	-0.029***			-0.034***	-0.032***	-0.032***
		(0.003)	(0.007)			(0.010)	(0.010)	(0.010)
Squared age		0.000***	0.000***			0.000***	0.000***	0.000***
		(0.000)	(0.000)			(0.000)	(0.000)	(0.000)
Firm-fixed effect	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Year to evaluate instrument	-	-	-	-	1998	1998	1997	1996
Observations	55263	55263	55263	13531	13531	13531	13403	13249

Table 18
Falsification test

This table presents estimation results to examine whether past ownership predicts future corporate policy. We restrict the data periods to 1994 - 1998. The sample includes only subsidiaries. We use OLS for estimation. The dependent variables are represented in the first row. The past ownership instrument is constructed from the parent's ownership ratio, which is capped at 66.67. The past ownership instrument takes this ownership ratio when the subsidiaries are in the treatment group and the year periods are from 1999 on. Otherwise, the past ownership instrument has the value zero. The number in the parenthesis added after the past ownership instrument in the table refers to the number of lags of the past ownership instrument (for example, (t-1) refers to one year lag). We include parent's ownership, natural log of assets, firm age, square of firm age as other covariates, but we only report the estimates of the past ownership instrument coefficients. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Leverage	Bank borrowing	Internal capital market	Cash holdings	Dividends	Capital expenditures	R&D expenses	Profitability	Tax aggressiveness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Past ownership IV (t-1)	0.131	-0.087	0.044	0.032	-0.009	0.008	-0.043	-0.079	-0.032
	(0.278)	(0.164)	(0.066)	(0.180)	(0.009)	(0.096)	(0.036)	(0.130)	(0.033)
Past ownership IV (t-2)	0.041	-0.025	0.073	0.165	0.002	-0.112	-0.010	-0.079	0.015
	(0.259)	(0.200)	(0.088)	(0.150)	(0.008)	(0.132)	(0.012)	(0.067)	(0.023)
Past ownership IV (t-3)	-0.496	-0.477	0.159	0.161	0.012	-0.091	-0.003	-0.025	0.062
	(0.450)	(0.304)	(0.164)	(0.502)	(0.023)	(0.147)	(0.018)	(0.096)	(0.077)
Other variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes