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Stock market listing and corporate tax aggressiveness: Evidence from legal reforms in squeeze out in Japan

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Abstract

Recent literature argues that agency conflicts between shareholders and managers reduce corporate tax aggressiveness. Although stock market listing is a fundamental source of the agency costs, a dearth of widely available data prevents researchers from investigating how monitoring from stock markets affects tax aggressiveness. We use unique panel data that cover both publicly-traded (listed) companies and privately-held (unlisted) companies in Japan. To mitigate endogeneity concerns about the choice to list stocks on public equity markets, we use legal reforms in squeeze out as a quasi-natural experiment. We provide evidence that stock market listing decreases tax aggressiveness among companies whose ownership is concentrated. This result suggests that minority shareholders' option to sell stocks in public markets reduces managers' incentives to be tax aggressive. Our findings link a function of capital markets with public finance by demonstrating that financial developments can contribute to the effective collection of tax revenues.

JEL classifications: G30, G38, H25, H26

Keywords: tax aggressiveness, stock market listing, ownership structure, squeeze out

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1. Introduction

All firms appear to have an incentive to be tax aggressive to reduce tax liabilities.[1] However, empirical studies provide evidence that there exists a considerable variation in individual firms' tax aggressiveness.[2] This observation raises a question on what tradeoff firms face when they choose their optimal level of tax aggressiveness. A clear cost of tax avoidance for firms is resources spent to establish tax avoidance schemes. Another cost is potential tax penalties imposed on firms. The literature points out that these costs are smaller than the benefits from tax sheltering in the U.S. (Desai and Dharmapala (2009a)). This argument is likely to be applied in Japan as well where the corporate tax rates are around 40% after 1990s, which are relatively high compared to other countries. More important, these costs do not explain the existing variation of tax aggressiveness given that all firms in the same countries are subject to the same tax systems.

Recent literature since Crocker and Slemrod (2005) and Desai and Dharmapala (2006) links corporate tax aggressiveness with agency costs. They emphasize that managers, who are not necessarily owners of the firms, choose the level of tax aggressiveness. Since tax sheltering is obscured from the tax authority by its nature, it is obscured from outside investors as well. Investors can be skeptical whether the tax sheltering aims to enhance shareholder value or it is motivated by managers' self-interests. If investors believe that the latter factor outweighs the former factor, tax sheltering reduces firm value among publicly-traded (listed) companies through a drop in stock prices. Public companies' managers, predicting the negative responses from stock markets, choose to be less tax aggressive. In contrast, this tradeoff does not affect privately-held (unlisted) companies' managerial incentives because their stocks are not traded in

[1] We use "tax aggressiveness", "tax sheltering", "tax evasion" and "tax avoidance" interchangeably in this paper.
[2] The literature in tax aggressiveness is surveyed in Slemrod and Yitzhaki (2002) and Hanlon and Heitzman (2010) among others.

public markets.

This argument does not conclude that a negative association between stock market listing and tax aggressiveness is causal. This is because the ownership of private companies is more highly concentrated than that of public companies on average. To eliminate this potential ownership effect, our main analysis uses a variation in stock listing among firms whose ownership is concentrated. Specifically, we compare tax aggressiveness between public subsidiaries and private subsidiaries. Subsidiaries refer to companies whose majority of shares are held by their parent companies.

There remains a concern that OLS estimates are biased because the choice whether to list stocks is endogenous. We use a “sea change” in corporate law (Milhaupt (2006)) around 2000 as a quasi-natural experiment. These reforms provide parent companies legal rights to forcibly eliminate (or squeeze out) their subsidiaries’ minority shareholders. As a result of these legal reforms, a considerable number of partly owned subsidiaries’ minority shareholders were squeezed out, and these subsidiaries were changed into wholly owned subsidiaries. Firms must delist stocks when they become wholly owned. This is the variation in stock listing we use for identification.

The exogeneity of this variation is questionable, however, since not all subsidiaries have become a target of squeeze out. We instrument the parents’ decision to implement squeeze out transactions. The corporate law requires parents to hold 2/3 of their subsidiaries’ stocks to squeeze out the subsidiaries’ minority shareholders. We argue that the costs for squeeze out decrease in the parents’ ownership, and that the costs are constant above 2/3 ownership. Based on this idea, we use past information of the parent’s ownership ratio to construct an instrument. We include the contemporaneous ownership ratio in regression to control for the direct effects of

ownership on tax aggressiveness. We also conduct falsification tests to support that the exclusion restriction is not violated, using the data periods before the legal systems were introduced. In brief, we use a difference-in-differences framework with the IV strategy for identification.

We combine three datasets: Financial Statements Statistics by Corporations collected by the Ministry of Finance; Basic Survey of Japanese Business Structure and Activities collected by the Ministry of Economy, Trade, and Industry; Nikkei NEEDS FinancialQUEST collected by Nikkei Inc. The first two datasets cover both public companies and private companies. This feature allows us to use a variation in stock listing. This variation is not available in other widely used financial datasets such as Compustat because they mostly cover only public companies. The final sample consists of 39976 firm-year observations between 1994 and 2012. 17017 observations are private. 3148 observations have either listed their stocks or delisted their stocks during the data periods. Thus, we observe both cross sectional variations and within-firm variations in stock listing.

Estimation results are consistent with our hypothesis. We find evidence that stock market listing reduces tax aggressiveness among subsidiaries. We obtain robust evidence under the quasi-natural experiment with the IV strategy. The F-statistic of the excluded instrument at the first stage regression is over 30. Thus, the instrument is strong. The falsification test shows that the past ownership does not predict current tax aggressiveness before the legal systems were introduced. This evidence provides support that the instrument does not violate the exclusion restriction. The IV estimates are economically significant as well. The estimates show that stock market listing reduces tax aggressiveness by 0.58 – 1.09 standard deviations of the tax aggressiveness measure.

Our findings have implications for tax policy. The vast literature investigating how taxes

affect corporate behavior surveyed in Graham (2003) generally assumes that firms treat tax rates as given. We show that individual firms' effective tax rates are endogenously determined by the choices to list stocks. This finding can be interpreted that stock markets monitor tax avoidance. In other words, our results suggest that financial developments help governments collect tax revenues effectively. This is a testable implication in the literature that studies various determinants of corporate tax revenues (Auerbach and Poterba (1987); Auerbach (2007); and Clausing (2007)). Furthermore, our finding sheds new light on the intense policy debate on tax avoidance.[3] Our findings suggest that more attention can be needed on private companies than on public companies because private companies have stronger incentives to be tax aggressive.

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

2. Hypothesis and literature

The literature in tax aggressiveness is classified into two groups. One group of research motivated by Allingham and Sandmo (1972) interprets tax sheltering as an attempt to shift income from governments to individuals. These studies treat tax avoidance as one of various activities that increases after-tax profits. Thus, individuals engage in tax avoidance as long as tax costs, which depend on the frequency of tax audits and the magnitude of tax penalties, are lower than tax benefits. A characteristic of this line of literature is that they presume that those who evade taxes are individuals. Crocker and Slemrod (2005) argue that this framework is not suitable to analyze corporate tax avoidance. This is because agency conflicts resulting from the separation of ownership and control in corporations are not considered in this framework.

[3] For example, OECD's webpage "Fighting tax evasion" describes their policy measures against tax evasion <http://www.oecd.org/ctp/fightingtaxevasion.htm>.

The second group of relatively new literature relates this agency dimension to corporate tax aggressiveness. Tax avoidance inevitably needs to be obscured from the tax authority, which in turn creates information asymmetry between managers and outside investors. Consequently, managers may seek opportunities for rent diversion when they establish tax sheltering schemes. Predicting the possibility of managerial rent seeking, outside investors can discount firm value when the managers are tax aggressive. This argument implies that investors can make a distinction between income from regular business activities and that from tax avoidance, in contrast to the view discussed in the previous paragraph. If agency costs associated with a high level of tax aggressiveness are sufficiently large, shareholders do not prefer tax avoidance.

There is ample support for the hypothesis that tax sheltering reduces market value of firms. Hanlon and Slemrod (2009) present event study evidence that stock prices react negatively to news that firms involve in tax sheltering. Kim, Li, and Zhang (2011) show that tax avoidance increases stock price crash risks. Anecdotal evidence reveals that complicated tax sheltering schemes contribute to managerial rent seeking, which has resulted in high-profile accounting scandals such as those by Enron or Tyco (Desai, 2005). These pieces of evidence suggest that the negative consequence of tax sheltering outweighs the positive aspect of tax sheltering.

Investors' negative responses to tax sheltering provide contrasting incentives to public companies' managers and to private companies' managers. Recent studies, which will be discussed later, provide evidence that public companies' managers pay considerable attention to the potential impacts of their decisions on stock prices. Therefore, public companies' managers have incentives to be less tax aggressive to maintain stock prices. On the other hand, private companies' managers do not face this incentive problem because stocks of private companies are not traded on public markets. Thus, we expect that public companies are less tax aggressive than

private companies through this stock market listing effect.

A simple comparison in tax aggressiveness between public companies and private companies does not necessarily identify how the scrutiny from stock markets affects tax aggressiveness. This is because the ownership is more highly concentrated among private companies than among public companies in general. Owners with large stakes have incentives and abilities to monitor managers.[4] Thus, private companies' shareholders may be able to detect managerial rent-seeking masked by tax avoidance more frequently than public companies' shareholders. Such intensive monitoring in private companies makes their managers less tax aggressive. Another possibility is that private companies' shareholders may be in a better position to ensure that their managers engage in tax sheltering for shareholder value than public companies' shareholders. This higher degree of control in private companies can make their managers more tax aggressive. Although it remains inconclusive whether concentrated ownership increases or decreases tax aggressiveness, this argument suggests that ownership can have direct impacts on tax aggressiveness.

Our estimation therefore treats the impacts of stock market listing on tax aggressiveness as a function of ownership structures. Alternatively, we restrict observations to companies whose ownership is concentrated. We use business group structures as a variation in ownership structures.[5] The ownership of subsidiaries is concentrated by definition. Corporate law in Japan

[4] Desai and Dharmapala (2006) provide evidence that this argument is applied in the context of tax aggressiveness. They show that high-powered incentives reduce tax aggressiveness among companies whose ownership is dispersed. This evidence suggests that incentive contracts and concentrated ownership are complementary for the purpose of monitoring managers.

[5] A business group is a collection of a parent company and its subsidiaries. Subsidiaries refer to companies whose majority of 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 that consists of Toyota Motor Corporation and its over 500 subsidiaries. Business group structures have been used in some papers that take advantages of Japanese economic environments for identification (for example, Hoshi, Kashyap, and Scharfstein (1991)).

allows subsidiaries to list their stocks on stock exchanges.[6] Therefore, we observe a variation in stock market listing among subsidiaries. The goal of this paper is to identify the causal impacts of stock market listing on tax aggressiveness among subsidiaries, in order to show that managers' optimal choice of tax aggressiveness is affected by the observability of stock prices.

The challenge for this paper is endogeneity of firms' decisions to list stocks. Before turning to a discussion on our research design, we review the literature in the rest of this section. Building on the agency view of tax aggressiveness, several papers examine the relationship between various factors that affect agency costs and corporate tax aggressiveness. Chen, Chen, Cheng, and Shevlin (2010) show that family owned firms are less tax aggressive than non-family owned firms. Their argument is that minority shareholders of family firms are concerned that managers engage in tax avoidance not for shareholder value but for their own private benefits. Thus, managers of family firms forgo tax benefits to prevent a drop in stock prices. Chyz, Leung, Li, and Rui (2013) show that labor unionization decreases tax aggressiveness. The authors attribute this result to unions' ability in monitoring managers and to unions' aversion to risks.

A caveat of these two papers is that they use data that mostly cover public companies. The reason is that these papers, or more broadly many papers in corporate finance, use Compustat North America that are collected by Standard and Poor's as a data source. They exploit publicly available financial statements to compile Compustat data files. As a result, most firms covered in Compustat are publicly-traded (listed) companies.[7] This limitation in data availability implies that most studies cannot address agency costs created by the observability of stock prices.[8] We

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

[7] Compustat covers a limited number of private companies. This is because companies issuing public debts disclose financial statements.

[8] There is a small but growing literature in corporate finance that compares public companies with private companies in various aspects. Asker, Farre-Mensa, and Ljungqvist (2015) show that mangers of public companies in the U.S. are

need to have access to confidential data such as those collected by governments or accounting firms to study the impacts of stock market listing on corporate behavior or on financial policy.

Hanlon, Mills, and Slemrod (2007) are an exception that makes a comparison in tax aggressiveness between public companies and private companies. Their tax aggressiveness measure is the level of proposed tax deficiencies. Using tax return data in the U.S., they show that private companies have higher proposed tax deficiencies than public companies. There are three differences between Hanlon, Mills, and Slemrod (2007) and our paper. First, they use a cross sectional variation, while our paper uses both a cross sectional variation and a within-firm variation. Our paper shows that estimation results from cross sectional regression are not necessarily robust when including firm-fixed effects. Second, Hanlon, Mills, and Slemrod (2007) use a non-experimental framework, while our paper uses a quasi-natural experiment as well. Our research design mitigates endogeneity concerns of stock listing. Third, Hanlon, Mills, and Slemrod (2007) do not consider the possibility that the relationship between stock market listing and tax aggressiveness depends on ownership structures. Our paper provides evidence that the impacts of stock market listing are substantially different between among concentrated companies and among unconcentrated companies.

3. Data and research design

3.1. Data description

The main data source is Financial Statements Statistics by Corporations (FS data). This dataset is collected annually by the Ministry of Finance Japan. It covers unconsolidated financial

overly sensitive to stock prices when choosing the level of capital expenditures. This over-sensitivity causes underinvestment among public companies compared to private companies' counterparts. Michaely and Roberts (2012) show that public companies in the U.K. pay dividends more smoothly than private companies. The authors argue that the scrutiny of public capital markets explains this difference in dividend policy. Brav (2009) shows that private companies are more highly leveraged than public companies in the U.K. The author argues that this is because issuing private equity is more costly than issuing public equity.

statements of non-financial corporations in Japan. We use the data between 1994 and 2012. A distinctive feature of the FS data is that the data sources are not publicly available financial statements. The Ministry requests corporations to submit their financial information under Statistics Act.[9] This data collection process ensures that the FS data can cover both public companies and private companies. The FS data do not classify whether individual firms are public or private. We use another 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.[10] We match observations in the FS data with those in FinancialQUEST based on the corporate name and accounting data. We classify those matched observations as public, and those unmatched observations as private.[11][12]

[9] 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 certain years. The response rate is generally over 90% among large corporations. Thus, the data exhibit unbalanced panel structures.

[10] We evaluate whether the firms are public or private at the end of the fiscal year. We assume that fiscal year starts in April and it ends in next calendar year's March, which is common in Japan. For example, we evaluate that the company is public in 2005 if the company's stock is listed at the end of 2006 March.

[11] There exist discrepancies in the reported corporate name or financial information between the FS data and FinancialQUEST. We can match 90.9% of the observations that are classified as public companies in our final sample based on the company name, legal capital, and assets almost exactly, where we allow a plus or minus one difference in financial information. The naming of corporations is not always consistent between these two datasets. Thus, we match the remaining public observations in FinancialQUEST with those in the FS data based on legal capital, assets, sales, and ordinary income. We match 2.5% of the final public observations after this matching process. We then match the company identification number of the FS data with that in FinancialQUEST. Although this process helps us connect the unmatched observations in the two datasets, this process causes a problem. For example, suppose that a company in the FS data is matched with a company in FinancialQUEST before 2005. If this company established a holding company in 2006, the company recorded in the FS data can be a subsidiary of this holding company in 2006, and the company recorded in FinancialQUEST can be the holding company itself in 2006. We judge the accuracy of this data matching process by hand. We further match the remaining public observations in FiancialQUEST with observations in the FS data with weaker conditions such as allowing 10% differences in financial information between the two datasets. We also check the accuracy of this procedure by hand. Although this classification is conducted carefully, there might remain concerns in the accuracy regarding the 6.6% (100% - 90.9% - 2.5%) of the observations that are classified as public companies in the sample. We obtain almost identical results when we remove these observations.

[12] Note that not all public companies in FinancialQUEST are matched with some observations in the FS data. This is because financial corporations are not included in the FS data while they are included in FinancialQUEST, for example.

The FS data cover information on tax liabilities of individual corporations.[13] 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 account of 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. The FS data do not include information on ownership structures of individual firms. We obtain this information from Basic Survey of Japanese Business Structure and Activities (BS data) collected by the Ministry of Economy, Trade, and Industry.[14] The BS data also exhibit unbalanced panel structures like the FS data. The BS data tell information of parent companies' ownership ratio. We define the firms' ownership as being concentrated when their parents' ownership is 50% or higher. We define these firms as subsidiaries. We merge the FS data with the BS data based on accounting information.[15]

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

[13] The Japanese tax system introduced a consolidated taxation system in 2002. This system provides business groups an option to file a consolidated tax return. In other words, this system allows business groups to offset losses with gains elsewhere in the business groups. We expect that firms that use the consolidated taxation system are more tax aggressive. Our tax aggressiveness measures are constructed based on individual tax returns even when the firms use the consolidated taxation system. Thus, our results are not affected by the introduction of the consolidated tax filing.

[14] The target of the BS data is corporations whose legal capital is 30 million yen or larger and whose number of employees is 50 or more. The BS data impose a restriction on the target companies in terms of the number of employees while the FS data do not. Firms used in the analysis are relatively large, and therefore this restriction on the BS data is not relevant when merging these two datasets. The BS data cover companies in almost all non-financial industries, with some exceptions such as those in the construction industry, while the FS data cover companies in all non-financial industries.

[15] We match the FS data with the BS data based on legal capital, assets, and sales. We adopt a similar data matching procedure with what we have used when merging the FS data with FinancialQUEST.

firm size. Second, we keep observations whose legal capital is over 100 million yen because various tax breaks are available for small corporations, which are firms whose legal capital is 100 million yen or less. Third, we keep observations whose before-tax profits are recorded for the past five consecutive years to approximate past accumulated losses.[16] We keep observations both of whose 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. We keep observations whose lagged assets are non-missing since this variable is used as a denominator of some variables in regression such as leverage. We then replace missing values with zero.[17] All variables used in regression are winsorized at 1% levels and at 99% levels.

The final sample consists of 39976 firm-year observations. The number of public observations is 22959, and that of private observations is 17017. Thus, 42.6% of the observations are private. This percentage is smaller than that reported in Hanlon, Mills, and Slemrod (2007), which is 58.5%. One reason behind this difference in the data composition is that we keep observations with positive before tax income, while Hanlon, Mills, and Slemrod (2007) do not impose this restriction. In our data, non-positive profits are less common among public observations than private observations. As a result, private companies are less prevalent in our data than in their data. This observation suggests that public companies' managers engage in earnings management to avoid reporting non-positive profits, in line with our discussion below.

[16] The Japanese tax system allows firms to deduct past tax losses from their current or future corporate taxable income like in many other countries. We include a past loss dummy variable in the regression as a control variable. The time span for loss carryforwards changed over time as follows: five years up to 2003; seven years between 2004 and 2010; and nine years in 2011 and afterwards.

[17] We interpolate missing values or zero for parent companies' ownership ratio in the BS data. We first replace missing values with zero when we observe only either missing values or zeros during the entire data periods for the firm. We then replace missing values or zeros with the average value of previous period's value and the next period's value when these two values are exactly the same or the difference of these two values is 0.1 percentage point. We treat the remaining zeros as missing values, and we drop these observations. Main findings are not affected by this interpolation.

During the entire data periods, 20849 observations stay public, and 15979 observations stay private. The remaining 3148 observations experienced a change either from private to public or from public to private during the data periods. Therefore, we observe a considerable within-firm variation in stock listing or stock delisting. This within-firm variation plays key roles for identification. First, we use the firm-fixed effect models. Thus, we need a within-firm variation. Second, we use some firms that have changed from public to private as the treatment group in a quasi-natural experiment.

3.2. Identification strategy

We use a quasi-natural experimental research design with an IV strategy. The treatment in this experiment is two legal reforms that cause 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 the entire data periods because these companies are not affected by the treatment that causes delisting. A concern in this research design is that not all public subsidiaries delisted stocks, which leads to a selection problem. We use an IV approach to deal with this endogeneity.

Our identification strategy takes advantages of a series of corporate law reforms around 2000. These reforms involve fundamental changes in legal environments 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 are important to our paper since they provide an exogenous variation in stock listing. The first legal reform is the share exchange system introduced in 1999. This system provides parent companies 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 with their subsidiaries' stocks. The second reform is an introduction of a class shares subject to wholly call system introduced in 2006. This legal system has similar economic function to the share exchanges for the purpose of squeeze out.[18]

If these reforms are effective, we expect to see a considerable change in ownership structures of Japanese business groups after 1999. More specifically, a large number of partly owned subsidiaries should have changed into wholly owned subsidiaries as a result of the legal reforms that have made squeeze out less costly. When companies change from partly owned subsidiaries to wholly owned subsidiaries, these subsidiaries must delist their stocks from stock exchanges because of the limited supply of their stocks on markets.[19] 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 quasi-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 a 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. Using this information, we can construct an instrument to explain the cross sectional variation in squeeze out after the legal reforms. In principal, these two legal systems require extraordinary resolution that must be passed by a 2/3 of the vote casts at shareholders meetings of both acquirers and

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

[19] At least 5% of stocks must be traded for firms to stay listed at Tokyo Stock Exchange, which is the largest stock exchanges in Japan. Tokyo Stock Exchange has additional regulations that restrict subsidiaries whose parents' ownership is high to list their stocks.

acquired companies.[20] Thus, it is sufficient for parents to own 2/3 of their subsidiaries' stocks to eliminate the subsidiaries' minority shareholders. If the parents' ownership is lower than 2/3, they need to issue tender offers before using the legal systems. This two-stage strategy in squeeze out is common in practice. For example, Panasonic squeezed out Sanyo in 2011 following this two-stage procedure. Sanyo was one of Panasonic's partly owned, public subsidiaries. Panasonic's ownership of Sanyo was 50.2% in 2010 March. Panasonic issued a tender offer during 2010, and its ownership became 80.1% in 2010 December. Panasonic then exploited the share exchanges, and Sanyo became a wholly owned subsidiary of Panasonic in 2011 April. Sanyo delisted their stock as a result of this process.

The possibility of using this two-stage strategy allows us to construct an instrument of a private observation dummy variable. The private observation dummy takes one when the observation is private. The literature demonstrates that tender offers involve a considerable amount of takeover premium. For example, Rossi and Volpin (2004) report that the average takeover premium is 40%. Therefore, total takeover premium that parents are required to pay at the first stage of the two-stage strategy is a decreasing function in their ownership. Our argument implies that the likelihood that the subsidiaries are squeezed out is kinked at 2/3 ownership, and this function is flat beyond this threshold because 2/3 ownership is sufficient to pass proposals for squeeze out at the shareholders meetings.

We construct an instrument based on this idea.[21] The instrument takes zero for all

[20] There is an exception to this rule. When the target subsidiaries are considerably smaller than the parents, the extraordinary resolution at acquirers' shareholders meetings can be bypassed when using the share exchanges. Specifically, this simplified share exchange system is available when parent companies' net worth is at least five times as large as these parents' payments to the subsidiaries' minority shareholders.

[21] We can also exploit the condition for which parent companies can follow the simplified share exchanges explained in the previous footnote to construct an instrument. These parents can save costs associated with the regular share exchange procedures such as legal fees. Thus, we expect that subsidiaries of these parents are more likely to be squeezed out. Using this information as an instrument generates similar estimation results.

observations before the legal reforms in squeeze out. Thus, a variation in the instrument is observed only after the legal reforms. The instrument can take non-zero values only when the observations were public subsidiaries before the legal reforms, that is, only when the firms are in the treatment group. This is because the purpose of using the instrument is to explain the variation in stock delisting among firms in the treatment group. Thus, the instrument can take non-zero value only after the legal reforms and only when the firms are 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 being squeezed out. Therefore, this instrument and the private dummy variable can exhibit a mechanical, positive correlation after being squeezed out. Using this instrument brings 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 being squeezed out.

We construct an instrument to avoid these issues. We evaluate the ownership ratio of the subsidiaries in one year before the introduction of the legal reforms. This instrument captures the likelihood that the public subsidiaries are squeezed out when the legal systems are not available. As long as the legislation of these systems is not anticipated, parent companies do not have incentives to adjust ownership of their subsidiaries before the reforms. According to the following newspaper articles, official discussions on the introduction of the legal systems started in 1998 July. The Nikkei on July 9, 1998 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. The Nikkei on November 26, 1998 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 the information of two or more years before the legal reforms to construct an instrument to check the robustness of our findings.

Because of the unbalanced panel structure, which is relatively salient in the context of tax aggressiveness since we remove observations with non-positive profits, we may not know the ownership ratio in 1998. In this case, we use the ownership ratio in 1997. We repeat this procedure when the ownership of the year of interest is not observed. Another issue raised from the unbalanced panel structure is that we cannot classify subsidiaries that exist only in 1999 and afterwards either into the treatment group or into the control group because the group classification is based on information before the legal reforms. We remove these subsidiaries from the sample used for the quasi-natural experiment.

Another concern about this research design is that the instrument reflects information of past ownership structures before the legal reforms. This IV strategy might violate the exclusion restriction because we argue that ownership is a factor that can affect tax aggressiveness. In addition, there exists a qualitative difference between subsidiaries included in the treatment group and those in the control group. This is because subsidiaries in the treatment group are listed and thus their ownership structure is likely to be more dispersed than that in the control group. We include the contemporaneous ownership ratio as a control variable to mitigate these concerns. There remains a possibility that past ownership has direct impacts on the current level of tax aggressiveness even after controlling for the contemporaneous effects. We conduct falsification tests by regressing past ownership on current tax aggressiveness using the data

periods before the introduction of the legal reforms. Insignificant estimates from this test provide support that the exclusion restriction is not violated.

3.3. Estimation model and variable definition

We turn to the explanation of estimation models. The main independent variable is a private company dummy variable, which takes one when the observation is private. Since stock market listing is firms' choice, we instrument this variable as we have discussed in the previous subsection. The literature uses a book tax difference to evaluate corporate tax aggressiveness. A standard book tax difference is defined by income before taxes minus estimated taxable income, which is tax liabilities divided by corporate tax rates, normalized by lagged assets (Manzon and Plesko (2002)). Larger Manzon and Plesko book tax difference (MP book tax difference) implies that the firms pay less corporate taxes relative to their accounting profitability. Thus, firms take an aggressive tax position when the MP book tax difference is large.

Recent studies do not necessarily accept the MP book tax difference as an adequate measure of corporate tax aggressiveness. This is because public companies' managers have incentives to manage earnings to exceed analysts' forecasts. Therefore, earnings management can increase the MP book tax difference in the absence of tax avoidance. Isolating the earnings management effects from the MP book tax difference is especially important for our purpose because analysts do not generally make forecasts of private companies' performance. Even when forecasts of private companies are available, a lack of publicly observable stock prices provides private companies' managers weaker incentives to manage earnings than public companies' managers. As a result, earnings management affects public companies' MP book tax difference more than private companies' MP book tax difference.

To eliminate the earnings management effects from the MP book tax difference, we use

another type of book tax difference that was first introduced by Desai and Dharmapala (2006). Desai and Dharmapala book tax difference (DD book tax difference) removes the earnings management effects from the MP book tax difference to isolate the tax avoidance effects. More specifically, Desai and Dharmapala (2006) first regress the MP book tax difference on total accruals with firm-fixed effects, where total accruals measure the degree to which firms can manage earnings.[22] The residuals from this regression are the DD book tax difference. Formally, Desai and Dharmapala (2006) use the regression represented by

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

where index i represents company, index t represents year, MPBTD is the MP book tax difference, TotalAccrual is total accruals, μ is firm-fixed effects, and ε is error terms. The DD book tax difference is the residuals from this regression. Larger DD book tax difference implies that the observations are more tax aggressive. Our argument implies that the DD book tax difference is the most appropriate measure of corporate tax aggressiveness especially in our context, and thus we use this variable as the dependent variable in regression.

This paper uses three estimation techniques. First, we use cross sectional regression. We observe a considerable cross sectional variation in stock market listing. Thus, starting with cross sectional regression is a useful step to understand our topic of interest. In addition, since three of the closely related papers (Hanlon, Mills, and Slemrod (2007); Chen, Chen, Cheng, and Shevlin (2010); and Chyz, Leung, Li, and Rui (2013)) use only cross sectional regression, this analysis

[22] 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.

allows us to compare our results with those in previous studies. Second, we use firm-fixed effect models. This framework mitigates concerns that an observed correlation between stock market listing and tax aggressiveness is caused by time-invariant, firm-level heterogeneity. Some of the related studies in tax aggressiveness use firm-fixed effect models (Desai and Dharmapala (2006, 2009b)). This framework also allows us to examine the robustness of the findings by Hanlon, Mills, and Slemrod (2007) that use only cross sectional regressions. The cross sectional model and the firm-fixed effect model are represented by

$$TA_{it} = \beta_1 Priate_{it} \times Sub_{it} + \beta_2 Private_{it} + \beta_3 Sub_{it} + \gamma X_{it} + \mu_i + \eta_{jt} + \epsilon_{it} \quad (1)$$

where index i represents company, index t represents year, index j represents industry, TA is DD book tax difference, Sub is a subsidiary dummy variable, Private is a private company dummy variable, X is a matrix that includes various control variables that are explained below, μ represents firm-fixed effects and μ does not depend on i when we use cross sectional models, η is industry-year fixed effects, and ε is error terms.

β_1 evaluates whether private observations are more tax aggressive than public observations given that the firms' ownership is concentrated. We expect a positive sign on this estimate. β_2 tests whether tax aggressiveness depends on stock market listing when the ownership is not concentrated. To compare our results with those by Hanlon, Mills, and Slemrod (2007), we run regression (1) without Private×Sub or Sub. This model estimates the average differences in tax aggressiveness between public observations and private observations. We use robust standard errors clustered at the firm level.

The third methodology takes advantages of the legal reforms in squeeze out that provide an

exogenous variation in stock market delisting. This methodology seeks to eliminate a potential bias in OLS estimates associated with endogeneity of stock market listing. For example, corporations that are planning to go from private to public typically sign a multi-year contract with accounting firms to prepare for listing their stocks. These private companies are likely to have various opportunities to learn tax avoidance schemes from the accounting firms. As a result, newly listed companies can be more likely to be tax aggressive. OLS estimates on private company dummy exhibits an upward bias in this case. The third methodology uses a difference-in-differences framework with the IV method explained above to mitigate endogeneity concerns.

A difference in the data composition between the previous two methodologies and this third methodology is that the former two models include all firms while the latter includes only subsidiaries because the legal reforms used in the third methodology are relevant only for subsidiaries. We use both cross sectional models and firm-fixed effect models in this third methodology to compare our results with those obtained from the non-experimental framework. The first stage regression and the second stage regression are respectively represented by

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

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

where equation (2) is the first stage regression, equation (3) is the second stage regression, index i represents company, index t represents year, index j represents industry, Private is a private company dummy variable, Past ownership IV is ownership ratio evaluated principally in 1998 that is capped at 66.67, X is a matrix that includes various control variables that are explained

below, μ represents firm-fixed effects and μ does not depend on i when we use cross sectional models, η is industry-year fixed effects, ε is error terms, TA is DD book tax difference, and $\widehat{\text{Private}}$ is imputed value of Private from the first stage regression.

The X matrix in the third methodology includes a treatment dummy variable, which takes one when the subsidiaries are in the treatment group, in addition to other variables. This group dummy is absorbed by the firm-fixed effects when including them in regression, while the group dummy can be estimated in the cross sectional models. Note that Private×Treat is redundant as an independent variable because a variation in stock market listing is observed only among firms in the treatment group and we include the treatment dummy in regression. In other words, a variation in Private×Treat is identical to that in Private in the regression models. We use robust standard errors clustered at the firm level.

Control variables included in the X matrix are taken from related studies such as Chen, Chen, Cheng, and Shevlin (2010). These variables are as follows: leverage that is liabilities divided by lagged assets; profitability that is operating profit divided by lagged assets; PPE that is fixed tangible assets divided by lagged assets; intangibility that is intangible assets divided by lagged assets; log of lagged assets; and accumulated loss dummy that takes one when firms' before tax income aggregated across the past five years is negative. We also include contemporaneous ownership ratio. Furthermore, we include an industry-year dummy to absorb industry-year level economic shocks.

We include these control variables for the following reasons. Leverage captures the degree of available debt tax shields. This aspect of leverage makes firms with a higher level of leverage more tax aggressive. However, in our context, leverage is also likely to capture differences in financing environments because only public companies' stocks are traded at stock exchanges.

Thus, signs of leverage coefficients are not theoretically clear. Profitability is expected to be positively associated with tax aggressiveness because profitable firms have larger before tax income, and therefore they have a stronger incentive to avoid taxes. PPE captures capital intensity, which reflects the importance of the different treatments of depreciation between in tax statements and in financial statements. Highly capital intensive firms have an incentive to take tax avoidance measures, and therefore we expect a positive association between PPE and tax aggressiveness. Intangibility is expected to be positively associated with tax aggressiveness because firms with high intangibility can engage in income shifting for tax benefits more easily. The accumulated loss dummy is expected to be positively associated with tax aggressiveness because firms with past losses can deduct them from their current taxable income. Parents' ownership captures monitoring intensity as we have discussed.

Table 1 reports mean and standard deviation of individual variables used in regression. We separately report the statistics among public companies and among private companies. The second row shows that 11% of the public observations are subsidiaries. These observations play key roles in identification given that the treatment group in the quasi-natural experiment consists of subsidiaries that were public before 1999. This table supports the argument that private observations are more concentrated than public observations as we have presumed; the parent's average ownership is 61.9% among private observations, and it is 7.1% among public observations. There is also a large difference in leverage between public observations and private observations. Private observations are 16 percentage points more highly leveraged than public observations. This difference in leverage is likely to reflect a difference in cost of capital since

only public companies have access to the public equity market (Brav (2009)).[23]

4. Result

4.1. Non-experimental framework

We first present estimation results under the non-experimental framework. Although regression based on this framework can suffer from endogeneity associated with the choices to list stocks, these models are useful to compare our results with those in related studies, especially those in Hanlon, Mills, and Slemrod (2007). We can also compare estimation results from this framework with those from the experimental framework. This comparison can highlight the advantages of the quasi-natural experiment especially when we obtain different estimation results from different frameworks.

Table 2 shows estimation results under the non-experimental framework. Columns (1) – (4) use the cross sectional models. Columns (5) – (8) use the firm-fixed effect models. Even numbered columns include the parent's ownership ratio as a control variable. Columns (3), (4), (7), and (8) include the subsidiary dummy as well as the interaction term of the private dummy and the subsidiary dummy. All the models are estimated using the OLS.

Private coefficient in column (1) shows that private companies are more tax aggressive than public companies on average, without considering the ownership structures. This result is qualitatively the same with that reported in Hanlon, Mills, and Slemrod (2007). In contrast, we observe a quantitative difference in the magnitude of the estimates. Table 5 in Hanlon, Mills, and Slemrod (2007) reports that the coefficients on private observation dummy on their tax aggressiveness measure are 0.0011 - 0.0013, depending on the measure's denominator that is

[23] This difference in leverage between public observations and private observations is consistent with Brav's (2009) finding in the U.K. Brav (2009) reports that the difference in leverage is 10 percentage points. Thus, the difference in leverage between public companies and private companies is larger in Japan than in the U.K.

either sales or assets. Table 4 of their paper reports that the standard deviation of their tax aggressiveness measures is 0.0055 - 0.0068. Thus, stock market listing decreases tax aggressiveness by about 0.2 standard deviations of the dependent variables. Our estimates of 0.059 in column (1) imply that stock market listing makes 0.03 standard deviation differences in tax aggressiveness, given that the standard deviation of the DD book tax difference is 2.03 in our paper. Hanlon, Mills, and Slemrod (2007) use the Tobit model, but a crude comparison between their estimates and our estimates in column (1) implies that our estimates are 3/20 of their estimates in magnitude.[24]

This result from column (1) is not robust under the non-experimental framework. Column (2) shows that the sign of the private coefficient has been reversed after controlling the parent's ownership ratio. Although column (3) shows that private observations are more tax aggressive than public observations when the ownership is concentrated, the statistical significance disappears when we control the parent's ownership ratio in column (4). These results imply that ownership structures are important factors behind the correlation between stock market listing and tax aggressiveness. This result is in line with recent finding that ownership structures play key roles in the relationship between agency conflicts and tax aggressiveness (Desai and Dharmapala (2006)).

Our estimation results with the firm-fixed effects provide clearer evidence that the finding

[24] The sign of other independent variables is broadly consistent with our predictions as well as with the sign reported in the previous studies such as Chen, Chen, Cheng, and Shevlin (2010). PPE, intangibility, and past accumulated losses have positive impacts on tax aggressiveness. This is in line with the findings by Chen et al. (2010) that are reported in Panel A of Table 4 of their paper. Firm size measured by lagged assets is positively associated with tax aggressiveness, which suggests that larger firms have more opportunities to avoid taxes. However, the sign is reversed when we include firm-fixed effects. The negative sign of the leverage coefficient is not consistent with our prediction based on the tax benefits of debt. In our context, leverage is likely to capture the difference in financing environments between public companies and private companies because only public companies have access to public equity markets. Thus, this coefficient might not have clear interpretation compared to previous studies. The negative sign on the profitability coefficient is inconsistent with our prediction as well as the findings by Chen et al. (2010). However, the statistical significance of the coefficient disappears when we include firm-fixed effects. Furthermore, we obtain positive and significant estimates when we use the experimental framework as we will show below.

from column (1) is not robust. All the models with firm-fixed effects show that neither the coefficients on the private dummy, the subsidiary dummy, nor the interaction term of these two variables are statistically significant. One possible implication of this result is that the finding by Hanlon, Mills, and Slemrod (2007) is not robust when we control firm-heterogeneity. Alternatively, we can interpret that this insignificance is a consequence of endogeneity of stock market listing. We provide evidence based on the experimental framework to address the endogeneity in the next subsection.

4.2. Experimental framework

4.2.1. Main finding

Tables 3 and 4 present estimation results under the experimental framework. Note that the number of observations decreases to 8432 because we only include subsidiaries in the experimental framework. The treatment group consists of 1866 observations and the control group consists of 6566 observations. Among the observations in the treatment group, 327 observations are private companies.

Table 3 presents the first-stage results to explain which factors affect the firms' decisions to go private. Column (1) uses the cross sectional framework, and column (2) includes firm-fixed effects. The F-statistic of the excluded instrument is 35.64 in column (1) and it is 34.31 in column (2). Therefore, the instruments are sufficiently strong. The sign of the past ownership IV coefficients are positive and significant at the 1% level in both models. Therefore, subsidiaries are more likely to be squeezed out when their parent's past ownership is higher. Since our models include the parent's current ownership ratio as a control variable, this table provides evidence that the past ownership itself explains a variation in stock delisting.

Table 4 presents second stage estimation results of the IV strategy. Columns (1) and (2) use

the cross sectional models. Columns (3) and (4) use the firm-fixed effect models. We use the IV strategy for columns (1) and (3). We use OLS for columns (2) and (4) for comparison.

The cross sectional models show that private observations are more tax aggressive than public observations. This finding is consistent with our prediction. A notable difference between the IV estimates and the OLS estimates is their magnitude; it is 1.65 under the IV strategy, and it is 0.60 under the OLS. Models including firm-fixed effects provide a sharper contrast between in OLS estimates and in IV estimates. Private coefficient is insignificant in the OLS model, while it is positive and significant in the IV model. Thus, our IV strategy provides consistent evidence that stock market listing reduces tax aggressiveness.

The estimate of 2.22 of the imputed private dummy in column (3) suggests that stock market listing has economically significant impacts on tax aggressiveness. The magnitude of this estimate implies that the difference in tax aggressiveness between public companies and private companies is more than one standard deviation of the DD book tax difference (2.03). We can interpret that the estimate is five times larger than that reported in Table 5 in Hanlon, Mills, and Slemrod (2007) if we measure the estimate by the standard deviation of the dependent variable. Therefore, we find a sizable impact of stock market listing on tax aggressiveness, compared to that found in Hanlon, Mills, and Slemrod (2007).

4.2.2. Robustness

In this subsection, we discuss concerns that may cause biases in IV estimates: weak instruments and a violation of exclusion restrictions. We have provided evidence that the instrument is strong by showing that F-statistic of the excluded instrument is over 10 in Table 3. To support the hypothesis that the exclusion restriction is not violated, we conduct two tests that show past ownership does not predict current tax aggressiveness.

The first test uses the data periods before the introduction of the legal systems for squeeze out. Without the legal systems, the indirect effect of the parent's past ownership on tax aggressiveness through a change in the costs for squeeze out does not exist. Therefore, zero coefficients on the past ownership IV when we use the data periods before the legal reforms provide support for the hypothesis that the exclusion restriction is not violated. Specifically, we restrict the data periods from 1994 to 1998, from 1994 to 1997, and from 1994 to 1996, respectively. Using a sample that consists of each of the data periods, we estimate the equation represented by

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

where index i represents company, index t represents year, index j represents industry, index n represents the number of lags of the past ownership IV that is the ownership ratio capped at 66.67, TA is the DD book tax difference, X is a matrix that includes control variables, μ represents firm-fixed effects and μ does not depend on i when we use cross sectional models, η is industry-year fixed effects, and ε is error terms.

Table 5 reports coefficients on the past ownership IV. The first row represents the data periods and the first column represents the number of lags of the past ownership IV. In each year period, we use both cross sectional models and firm-fixed effect models. Regression includes other covariates, but we do not report their estimates for simplicity of exposition. Note that each regression includes only one of the past ownership IV variables. For example, the second column of this table does not mean that we include four past ownership IV variables at the same time.

Table 5 shows that 13 of the 15 estimates of the past ownership IV coefficients are not

statistically significant. Two of the models exhibit negative, significant estimates at the 5% level when we use one-year lagged past ownership IV. These results suggest that the exclusion restriction is not violated as long as we use two or more years lagged ownership as the instrument. These findings also cause a concern that our estimates reported in Table 4 are biased. This is because we use the parent's ownership ratio evaluated in one-year before the legal reforms as the instrument. Consequently, the exclusion restriction might have been violated among subsidiaries that are squeezed out in 1999.

The second test to examine the possibility of a violation of the exclusion restriction is related to this point. The base IV regression uses past ownership evaluated principally in 1998 as the instrument. This choice of the year can be problematic as suggested in the previous paragraph. In addition, we have discussed that the introduction of the share exchanges might have been anticipated at least one year before its actual introduction. This implies that parents might have started to adjust ownership stakes of their subsidiaries before their actual introduction. Therefore, we evaluate the past ownership structure in over one year before 1999 to construct the instruments. We estimate equations (2) and (3) using the instruments. When the data do not tell the ownership ratio of the year of interest (for example, 1997) due to the unbalanced panel structures, we use one year before the year (that is, 1996) as we have conducted in our main IV regression.

Table 6 shows the estimation results. Columns (1) - (2), (3) - (4), (5) - (6), and (7) - (8) respectively evaluate the past ownership ratio principally in 1997, 1996, 1995, and 1994. Odd numbered columns use the cross sectional models, and even numbered columns include the firm-fixed effects.

All the models show that the estimates of the imputed private dummy coefficients are

positive and statistically significant at least at the 10% level. Estimates from the firm-fixed effect models are consistently significant at the 5% level. F-statistic of the excluded instruments is over 30 across all columns. Therefore, the instruments are strong when we use older information to construct the instruments as well. It is noteworthy that the F-statistic is relatively large when we use older information such as that in 1996. This observation can suggest that the estimates from these models using older information might be more reliable to evaluate the economic significance.

From Tables 4 and 6, the estimates of the imputed private dummy variable range from 1.17 to 2.22. These estimates correspond to 0.58 – 1.09 standard deviations of the DD book tax difference. The 0.58 standard deviation differences are more than twice as large as those found in Hanlon, Mills, and Slemrod (2007) based on the crude comparison. From the discussion in this subsection, we confirm that the impacts of stock market listing on tax aggressiveness are considerably larger than previously documented.

5. Conclusion

The existing firm-level variation in corporate tax aggressiveness has been puzzling in public economics and corporate finance. Recent literature focuses on agency conflicts in corporations as a crucial factor that reduces managerial incentives to take tax aggressive measures. Few studies have examined how the scrutiny from stock markets affects tax aggressiveness due to difficulties in having access to data that cover both public companies and private companies. We use unique datasets of Japanese corporations to test whether agency costs associated with stock market listing reduce tax aggressiveness.

A challenge in estimation is that stock market listing is a firms' choice. To mitigate this endogeneity concern, we use legal reforms in squeeze out as a quasi-natural experiment. Our

main models compare observations that changed from public subsidiaries to private subsidiaries as a result of these legal reforms. We also use subsidiaries that were private before the legal reforms as the control group. Therefore, this paper takes a difference-in-differences approach. Furthermore, we deal with the selection problem concerning which subsidiaries are to be squeezed out using an IV strategy.

We show that private companies are more tax aggressive than public companies among subsidiaries. This result is consistent with our prediction. We also provide evidence that the instrument is strong and that the exclusion restriction is not violated. Our findings suggest that stock markets monitor corporate tax avoidance. In other words, financial developments can encourage corporations to pay taxes. Our findings also imply that closer supervision over private companies can be necessary because private companies have stronger incentives to be tax aggressive than public companies.

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Table 1

Summary statistics

This table separately reports summary statistics among public observations, among private observations, and among total observations. The data periods are between 1994 and 2012. Private dummy is a variable that takes one when the observation is a private company. Subsidiary dummy is a variable that takes one when the firm's majority of shares are held by another company (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. Leverage is liabilities divided by lagged assets. Profitability is operating profit divided by lagged assets. PPE is fixed tangible assets divided by lagged assets. Intangibility is intangible assets divided by lagged assets. Ln(assets) is natural log of lagged assets. Past loss dummy is a variable that takes one when firms' before tax income aggregated across past five years is negative. DD (Desai-Dharmapala) book tax difference is the residuals from the regression of MP (Manzon-Plesko) book tax difference on total accruals and firm-fixed effects. MP book tax difference is before tax profits minus estimated taxable income, which is corporate tax liabilities divided by corporate income tax rates, divided by lagged assets.

	Public observation (22959 observations)		Private observation (17017 observations)		Total observation (39976 observations)	
	mean	sd	mean	sd	mean	sd
Private dummy	0.00	0.00	1.00	0.00	0.43	0.49
Subsidiary dummy	0.11	0.32	0.70	0.46	0.36	0.48
Parent's ownership	7.05	18.94	61.94	42.82	30.42	41.51
Leverage	49.51	21.03	65.51	24.02	56.32	23.71
Profitability	4.89	4.10	5.40	5.17	5.10	4.59
PPE	27.94	15.84	32.60	21.18	29.93	18.45
Intangibility	0.81	1.44	0.87	1.84	0.84	1.63
Ln(assets)	10.71	1.33	9.93	1.17	10.38	1.32
Past loss dummy	0.13	0.34	0.17	0.37	0.15	0.35

Table 2

Estimation results from the non-experimental framework

This table presents estimation results to examine whether private observations are more tax aggressive than public observations. The data periods are between 1994 and 2012. We use the entire sample. We use OLS for estimation. The dependent variable that measures tax aggressiveness is DD (Desai-Dharmapala) book tax difference. DD book tax difference is the residuals from the regression of MP (Manzon-Plesko) book tax difference, which is before tax profits minus estimated taxable income (that is corporate tax liabilities divided by corporate income tax rates) divided by lagged assets, on total accruals and firm-fixed effects. Private dummy is a variable that takes one when the observation is a private company. Subsidiary dummy is a variable that takes one when the firm's majority of shares are held by another company (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. Leverage is liabilities divided by lagged assets. Profitability is operating profit divided by lagged assets. PPE is fixed tangible assets divided by lagged assets. Intangibility is intangible assets divided by lagged assets. Ln(assets) is natural log of lagged assets. Past loss dummy is a variable that takes one when the firms' before tax income aggregated across past five years is negative. Standard errors reported in parentheses are clustered at the firm-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 2 - Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DD book tax difference							
Private dummy×			0.193***	0.091			-0.158	-0.082
Subsidiary dummy			(0.053)	(0.063)			(0.209)	(0.237)
Private dummy	0.059**	-0.063**	-0.092***	-0.093***	0.052	0.098	0.134	0.139
	(0.027)	(0.030)	(0.032)	(0.032)	(0.110)	(0.126)	(0.157)	(0.158)
Subsidiary dummy			0.038	-0.155**			0.274	0.297
			(0.043)	(0.073)			(0.231)	(0.235)
Parent's ownership		0.002***		0.003***		-0.002		-0.002
		(0.000)		(0.001)		(0.003)		(0.004)
Leverage	-0.004***	-0.004***	-0.004***	-0.004***	-0.008***	-0.008***	-0.008***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Profitability	-0.012**	-0.014***	-0.014***	-0.014***	-0.005	-0.005	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
PPE	0.002**	0.002***	0.002***	0.002***	0.005**	0.005**	0.005**	0.005**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Intangibility	0.029***	0.028***	0.028***	0.028***	0.065***	0.065***	0.065***	0.065***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.020)	(0.020)	(0.020)	(0.020)
Ln(assets)	0.090***	0.092***	0.091***	0.091***	-0.532***	-0.532***	-0.528***	-0.529***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.074)	(0.074)	(0.074)	(0.074)
Past loss dummy	1.621***	1.615***	1.617***	1.614***	1.178***	1.178***	1.178***	1.178***
	(0.062)	(0.062)	(0.062)	(0.062)	(0.057)	(0.057)	(0.057)	(0.057)
Firm-fixed effect	No	No	No	No	Yes	Yes	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Observations	39976	39976	39976	39976	39976	39976	39976	39976

Table 3

Estimation results from the experimental framework: The first stage estimation

This table presents the first stage estimation results of the IV strategy to examine whether private observations are more tax aggressive than public observations. The data periods are between 1994 and 2012. The sample includes only subsidiaries. The dependent variable is private dummy, which is a variable that takes one when the observation is a private company. The past ownership IV is constructed from the parent's ownership ratio evaluated principally in 1998 that is capped at 66.67. The past ownership IV takes this ownership ratio when the subsidiaries are in the treatment group and the year periods are in 1999 and afterwards. Otherwise, the past ownership IV takes zero. Treatment dummy takes one in 1999 and afterwards if the firms were listed before 1999. 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. Leverage is liabilities divided by lagged assets. Profitability is operating profit divided by lagged assets. PPE is fixed tangible assets divided by lagged assets. Intangibility is intangible assets divided by lagged assets. Ln(assets) is natural log of lagged assets. Past loss dummy is a variable that takes one when firms' before tax income aggregated across past five years is negative. Standard errors reported in parentheses are clustered at the firm-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Private observation dummy	
Past ownership IV	0.003***	0.002***
	(0.000)	(0.000)
Treatment dummy	-0.832***	
	(0.020)	
Parent's ownership	0.004***	0.010***
	(0.000)	(0.001)
Leverage	0.000	0.000**
	(0.000)	(0.000)
Profitability	-0.001*	-0.001*
	(0.001)	(0.001)
PPE	-0.000**	-0.001**
	(0.000)	(0.000)
Intangibility	0.002	0.002
	(0.003)	(0.003)
Ln(assets)	0.002	-0.001
	(0.004)	(0.016)
Past loss dummy	0.011	0.016**
	(0.008)	(0.008)
Firm-fixed effect	No	Yes
Industry-year dummy	Yes	Yes
F-statistic of excluded instrument	35.64	34.31
Observations	8432	8432

Table 4

Estimation results from the experimental framework: The second stage estimation

This table presents the second stage estimation results of the IV strategy to examine whether private observations are more tax aggressive than public observations. The data periods are between 1994 and 2012. The sample includes only subsidiaries. We use OLS or IV for estimation. The dependent variable that measures tax aggressiveness is DD (Desai-Dharmapala) book tax difference. DD book tax difference is the residuals from the regression of MP (Manzon-Plesko) book tax difference, which is before tax profits minus estimated taxable income (that is corporate tax liabilities divided by corporate income tax rates) divided by lagged assets, on total accruals and firm-fixed effects. Imputed private dummy is a variable that is obtained from the first stage estimation. Treatment dummy takes one in 1999 and afterwards if the firms were listed before 1999. 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. Leverage is liabilities divided by lagged assets. Profitability is operating profit divided by lagged assets. PPE is fixed tangible assets divided by lagged assets. Intangibility is intangible assets divided by lagged assets. Ln(assets) is natural log of lagged assets. Past loss dummy is a variable that takes one when firms' before tax income aggregated across past five years is negative. Standard errors reported in parentheses are clustered at the firm-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	DD book tax difference			
Imputed private dummy	1.653**	0.596***	2.219**	0.291
	(0.768)	(0.229)	(0.906)	(0.246)
Treatment dummy	1.225**	0.437**		
	(0.606)	(0.218)		
Parent's ownership	-0.005	-0.001	-0.023**	-0.002
	(0.004)	(0.002)	(0.010)	(0.004)
Leverage	-0.005***	-0.005***	-0.011***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.003)
Profitability	0.016**	0.015*	0.021**	0.019
	(0.008)	(0.008)	(0.010)	(0.011)
PPE	0.008***	0.007***	0.005	0.004
	(0.002)	(0.002)	(0.005)	(0.005)
Intangibility	-0.003	-0.000	0.082*	0.088
	(0.031)	(0.032)	(0.050)	(0.055)
Ln(assets)	-0.018	-0.017	-0.408***	-0.405***
	(0.030)	(0.030)	(0.126)	(0.141)
Past loss dummy	1.767***	1.780***	1.531***	1.568***
	(0.100)	(0.102)	(0.113)	(0.123)
Firm-fixed effect	No	No	Yes	Yes
Industry-year dummy	Yes	Yes	Yes	Yes
OLS or IV?	IV	OLS	IV	OLS
Observations	8432	8432	8432	8432

Table 5

Robustness: Data periods before the legal reforms

This table presents estimation results to examine whether past ownership predicts current tax aggressiveness. We restrict the data periods to either 1994 - 1998, 1994 - 1997, or 1994 - 1996. The sample includes only subsidiaries. We use OLS for estimation. The dependent variable that measures tax aggressiveness is DD (Desai-Dharmapala) book tax difference. DD book tax difference is the residuals from the regression of MP (Manzon-Plesko) book tax difference, which is before tax profits minus estimated taxable income (that is corporate tax liabilities divided by corporate income tax rates) divided by lagged assets, on total accruals and firm-fixed effects. The past ownership IV is constructed from the parent's ownership ratio that is capped at 66.67. The past ownership IV takes this ownership ratio when the subsidiaries are in the treatment group and the year periods are in 1999 and afterwards. Otherwise, the past ownership IV takes zero. The number in the parenthesis added after the past ownership IV in the table refers to the number of lags of the past ownership IV (for example, (t-1) refers to one year lag). We include the following variables as other covariates, but we only report the estimates of the past ownership IV coefficients. Treatment dummy takes one in 1999 and afterwards if the firms were listed before 1999. 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. Leverage is liabilities divided by lagged assets. Profitability is operating profit divided by lagged assets. PPE is fixed tangible assets divided by lagged assets. Intangibility is intangible assets divided by lagged assets. Ln(assets) is natural log of lagged assets. Past loss dummy is a variable that takes one when firms' before tax income aggregated across past five years is negative. Standard errors reported in parentheses are clustered at the firm-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	1994 - 1998		1994 - 1997		1994 – 1996	
Past ownership IV (t-1)	0.000	-0.071**	-0.002	-0.073**	-0.004	-0.081
	(0.010)	(0.031)	(0.010)	(0.031)	(0.013)	(0.091)
Past ownership IV (t-2)	0.010	0.035	0.008	0.002	0.001	
	(0.010)	(0.043)	(0.010)	(0.063)	(0.013)	
Past ownership IV (t-3)	0.014	0.023	0.022			
	(0.012)	(0.115)	(0.014)			
Past ownership IV (t-4)	0.014					
	(0.020)					
Other variables included	Yes	Yes	Yes	Yes	Yes	Yes
Firm-fixed effect	No	Yes	No	Yes	No	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	OLS	OLS	OLS	OLS	OLS	OLS

Table 6
Robustness: Past ownership evaluated in before 1998

This table presents the second stage estimation results of the IV strategy to examine whether private observations are more tax aggressive than public observations. The data periods are between 1994 and 2012. The sample includes only subsidiaries. We use IV for estimation. The dependent variable of the first stage estimation is private dummy, which is a variable that takes one when the observation is a private company. The instrument is constructed from the parent's ownership ratio evaluated principally either in 1997, 1996, 1995, or 1994 that is capped at 66.67. This instrument takes this ownership ratio when the subsidiaries are in the treatment group and the year periods are in 1999 and afterwards. Otherwise, it takes zero. "Instrument evaluated year" in the table represents the year in which we principally evaluate the past ownership ratio. "F-statistic of excluded instrument" represents the F-statistic of the excluded instrument at the first stage estimation. In the second stage IV estimation, the dependent variable that measures tax aggressiveness is DD (Desai-Dharmapala) book tax difference. DD book tax difference is the residuals from the regression of MP (Manzon-Plesko) book tax difference, which is before tax profits minus estimated taxable income (that is corporate tax liabilities divided by corporate income tax rates) divided by lagged assets, on total accruals and firm-fixed effects. Imputed private dummy is a variable that is obtained from the first stage estimation. We include the following variables as other covariates (treatment dummy, parent's ownership, leverage, profitability, PPE, ln(assets), and past loss dummy), but we report only the estimates of the imputed private dummy coefficients. Treatment dummy takes one in 1999 and afterwards if the firms were listed before 1999. 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. Leverage is liabilities divided by lagged assets. Profitability is operating profit divided by lagged assets. PPE is fixed tangible assets divided by lagged assets. Intangibility is intangible assets divided by lagged assets. Ln(assets) is natural log of lagged assets. Past loss dummy is a variable that takes one when firms' before tax income aggregated across past five years is negative. Standard errors reported in parentheses are clustered at the firm-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DD book tax difference							
Imputed private dummy	1.480*	2.104**	1.408**	1.846***	1.171**	1.664**	1.244**	1.627**
	(0.770)	(0.901)	(0.566)	(0.645)	(0.578)	(0.658)	(0.582)	(0.659)
Other variables included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Industry-year dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OLS or IV?	IV	IV	IV	IV	IV	IV	IV	IV
Instrument evaluated year	1997	1997	1996	1996	1995	1995	1994	1994
F-statistic of excluded instrument	36.87	34.27	71.43	85.27	63.20	79.22	60.24	74.39
Observations	8341	8341	8157	8157	8001	8001	7643	7643