THE EFFECT OF PUBLIC DISCLOSURE ON REPORTED TAXABLE INCOME: EVIDENCE FROM INDIVIDUALS AND CORPORATIONS IN JAPAN

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The behavioral response to public disclosure of income tax returns figures prominently in policy debates about its advisability. Although supporters stress that disclosure encourages tax compliance, policy debates proceed in the absence of empirical evidence about this, and any other, claimed behavioral impact. This paper provides the first such evidence by examining the behavioral response to the Japanese tax return public notification system. The analysis suggests that, when there is a threshold for disclosure, a non-trivial number of both individual and corporate taxpayers whose tax liability would otherwise be close to the threshold will under-report so as to avoid disclosure — a response in the opposite direction from that stressed by supporters of disclosure. An analysis of corporations’ financial data offers no evidence that these companies’ taxable income declined after the end of the disclosure system.

Keywords: tax enforcement, tax avoidance, public tax disclosure

JEL Codes: H24, H25, H26

I. INTRODUCTION

Public disclosure of private income tax return information is an intriguing tax policy instrument. Supporters argue that it improves tax compliance and informs public policy discussion, while detractors deride it as an invasion of privacy that could have negative compliance effects. Public disclosure has a long history in the United States and elsewhere. The U.S. income tax contained disclosure provisions for both corporations and individuals during the Civil War, and again in the 1920’s and 1930’s; disclosure...
of corporate tax information received a flurry of attention in 2003, including proposed legislation introduced in the U.S. Congress. Recently, President Barack Obama’s 2012 Framework for Business Tax Reform calls for elements of corporate income tax disclosure. Similarly, Australia is currently considering implementing a system of income tax disclosure. Norway, Sweden, and Finland all currently require some type of public disclosure of taxable information. In Japan, the focus of this study, public disclosure of both individual and corporation income tax information was required from 1950 until 2004.

The academic literature has extensively examined tax disclosure and privacy (e.g., Lenter, Shackelford, and Slemrod, 2003), asserting links between disclosure and compliance. However, these analyses, and the policy debate surrounding them, have proceeded in the complete absence of empirical evidence about the effects of income tax disclosure, so that any arguments supporting or opposing disclosure are necessarily made in complete ignorance of empirical data or actual outcomes — we know essentially nothing about the impact of tax disclosure rules on taxpayer behavior.

In this paper, we begin to fill the vacuum of empirical evidence about the behavioral response to disclosure by offering some evidence from the Japanese experience with public disclosure of private tax information. We analyze data on individual and corporate (both public and private) taxpayers in Japan, focusing in part on the period surrounding the abolition of disclosure in 2004, and look for evidence consistent with a behavioral response to income tax disclosure. First, we examine whether individuals who would otherwise be just over the threshold for required disclosure responded to the disclosure system by manipulating their reported taxable income to fall just below the disclosure threshold. Specifically, we examine the distribution of implied taxable income just above the reporting threshold, and find that a non-trivial number of individual taxpayers manipulated their income so as to avoid disclosure, creating “missing tax returns” in the distribution of reported taxable income. This suggests that part of the response to the Japanese disclosure system was in the opposite direction (i.e., toward reduced reports of tax liability) than that stressed by supporters of disclosure. We obtain this result for all of the years of taxable income for which we have data, and the finding is robust to a variety of assumptions about our estimation techniques.

We then extend this analysis to the population of Japanese corporations that were subject to disclosure. We find evidence similar to that found in the individual taxpayer data — many corporations manipulated their income so as to avoid disclosure, suggesting that, at the threshold, income tax disclosure results in decreased reported taxable


2 In the absence of the required data (data for individuals below the threshold, or data for the period after disclosure), we are unable to answer questions regarding the effect of disclosure on the reported taxable income of the entire distribution of income.
income. However, while there are no publicly available data to examine the effect of disclosure on the entire distribution of individuals’ reported taxable income, the same is not true of corporations. In order to examine the effect of the disclosure system on corporations, we examine Japanese firms’ implied taxable income to determine whether the abolition of the disclosure system resulted in a decrease of taxable income. Our analysis of corporations’ financial data offers no evidence that these companies’ taxable income declined after the end of the disclosure system. In sum, we find that both corporate and individual taxpayers perceive disclosure as costly and want to avoid it but, for corporate taxpayers, the disclosure regime does not appear to increase firms’ reported taxable income.

This research makes several contributions. First, it offers what is, to the best of our knowledge, the first evidence regarding taxpayer response to a system of income tax disclosure. It suggests that in regimes where disclosure is not universal, one consequence will be taxpayer efforts to avoid disclosure, implying that both firms and individuals perceive tax disclosure as imposing costs on them. Second, it contributes to the literature on taxable income elasticity by documenting that the taxable income of both individuals and firms in Japan in the neighborhood of the income threshold is subject to manipulation and therefore somewhat elastic. Indeed, public disclosure is an example of a tax system instrument whose setting affects the elasticity of taxable income to tax rate changes, in line with the observation developed in Slemrod and Kopczuk (2002) that this elasticity is endogenous to a range of tax policy instruments.

The paper proceeds in six parts. In Section II, we motivate the paper and discuss the history of income tax disclosure in both the United States and Japan. Section III discusses the behavioral response we expect to see in response to income tax disclosure. Section IV presents our results regarding the response of individuals to income tax disclosure. Section V provides evidence of the response of firms to tax disclosure. Section VI concludes.

II. MOTIVATION

The debate over income tax disclosure has a long history, particularly in the United States. Supporters of income tax disclosure have long argued that it improves income tax compliance. One of its early American supporters, progressive Senator Robert LaFollette, Jr., argued in the 1930s that if a person “knows that his return is a matter of public record, he will hesitate a long time before he will resort to any device designed to relieve him of his fair share of the tax.” Others have argued that the information pro-

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3 Laury and Wallace (2005) use experimental methods to analyze the relationship between the perceptions of confidentiality and taxpayer compliance and find some evidence suggesting that, when individuals perceive a breach of confidentiality, they increase their level of compliance.

vided by public disclosure enriches the public debate about tax policy by revealing, for example, the extent of income inequality. Many other benefits of disclosure have been claimed. For example, H.R. 1556, a bill proposed in April of 2003 which would have mandated corporate income tax disclosure in the United States for public corporations, asserted that its objective was to “facilitate analysis of financial statements, to permit inspection of true corporate tax liability and understand the tax strategies undertaken by corporations, to discourage abusive tax sheltering activities, and to restore investor confidence in publicly traded corporations (U.S. Congress, 2003).”

Opponents of income tax disclosure have objected to the associated invasion of privacy. An opponent of the earlier U.S. disclosure regime, Senator Louis Murphy (D-IA), stated that disclosing income tax data is equivalent to taking “the curtains and shades from the homes of our taxpayers and pull[ing] out the walls of the bathroom to assure that the Peeping Toms shall have full and unobstructed opportunity to feast their eyes on the [tax return]” (Leff, 1984, pp. 70–71). Some who oppose disclosure worry that public access to such information will expose taxpayers, particularly wealthy ones, to those who might take advantage of that information. This was also one of the principal arguments made leading up to the elimination after 2005 of the Japanese system (Tax Advisory Commission, 2005). Similarly, opponents of corporate tax disclosure assert that disclosure would result in unnecessary loss of proprietary information for firms (Lenter, Shackelford, and Slemrod, 2003).

A. Experience with Disclosure in the United States and Other Countries

Disclosure has figured prominently in the history of the U.S. income tax. The first U.S. income tax, enacted during the Civil War, included publicity features. The 1862 Act permitted the public to examine the names of taxpayers and the amounts of their tax liabilities and, with the Revenue Act of 1864, newspapers started to publish lists of taxpayers, their reported incomes, and the amounts of taxes they paid. In 1871, the income tax was allowed to expire, in part, because of privacy concerns (Pomp, 1993).

The modern U.S. income tax was introduced in 1913, and the Revenue Act of June 2, 1924, made public the names and addresses of individuals and corporations filing tax returns. For example, in the 1980s, the Citizens for Tax Justice used accounting data and calculated the effective tax rates for many public firms and publicized the result. They decried the fact that many firms were paying very little in corporate income tax. It was the publicity given to this (already public but seldom examined) tax information that enriched the public debate on corporate tax reform, and, in part, brought about the Tax Reform Act of 1986 (Pomp, 1993).

One additional advantage of disclosure is that it could improve private contracting. Instead of relying on information directly shared between contracting parties (which could be subject to manipulation, and has costs related to providing that information), contracting parties concerned with the counterparties’ ability to fulfill a contract could obtain some information about the other parties’ financial condition via their disclosed tax report.

A more complete discussion of tax disclosure in the U.S. and other countries is presented in Lenter, Shackelford, and Slemrod (2003), on which this discussion draws.
returns along with their respective tax payments. Before the 1924 elections, newspapers across the country published the names and tax payments of large companies, celebrities, and local residents. President Calvin Coolidge, elected in 1924, and his Secretary of Treasury, Andrew Mellon, vigorously opposed making tax return information public, citing privacy and safety reasons for individuals, and suggesting that such disclosures could compromise business secrecy.\textsuperscript{8} In 1926 the law was changed so that only the names and addresses of taxpayers were public.

After a 1934 Senate committee investigating the 1929 stock market crash revealed that many wealthy owners of financial institutions had paid no income tax in the years after 1929, Congress inserted a publicity provision in the 1934 Revenue Act. This provision generated intense controversy. One month before tax returns were due to be filed (in 1935), a campaign to repeal the disclosure provision urged people — many of whom were not affected by disclosure — to petition their representatives in Congress to oppose disclosure. Soon thereafter Congress repealed the disclosure provisions, which was signed into law before the publicity provisions came into effect (Kornhauser, 2002; Leff, 1984).

No similar disclosure provision has been implemented since that time in the United States at the federal level,\textsuperscript{9} and there are now stringent provisions that limit the dissemination of tax return information, even within the U.S. government.\textsuperscript{10} However, although 1935 marked the end of the public disclosure of income tax information in the United States, in the wake of the Enron and WorldCom scandals, there was a renewed burst of interest in the issue of the public disclosure of corporate tax return information. While some tax information had long been revealed in the financial statements filed by public companies (Pomp, 1993), the Enron and WorldCom scandals made the public (and policy makers) more aware of how little can be ascertained about a firm’s real tax payments from its financial statements (McGill and Outslay, 2002; Hanlon, 2003).\textsuperscript{11} In April, 2003, a bill was introduced into Congress that would have provided for public

\textsuperscript{8} At the time, an article in \textit{The Wall Street Journal} highlighted the problem of firms’ proprietary information being released: “The extent to which all kinds of persons, partnerships and corporations are swarming to collector’s offices, to obtain supposedly useful information about their business competitors, is already a scandal” (“Income Tax Absurdities,” \textit{The Wall Street Journal}, November 3, 1924, p. 1).

\textsuperscript{9} A few U.S. states, including Massachusetts and Wisconsin, still have some form of tax disclosure. In Wisconsin, this has recently allowed reporters to discover that SC Johnson, a large private firm, remitted no Wisconsin income tax from 2000 to 2008 (Johnston, 2011). Investigative journalism is one example of a possible use of tax disclosure.

\textsuperscript{10} Many of these restrictions on intergovernmental tax disclosure were put in place in response to Richard Nixon’s attempts to obtain taxpayer information from the IRS for political purposes not authorized by law (Benedict and Lupert, 1978). Some disclosure within the U.S. government is allowed by §6103(i)(3) of the Internal Revenue Code, which permits disclosure of tax return data by the IRS to other governmental agencies if the information is evidence of the violation of a federal crime.

\textsuperscript{11} As noted by McGill and Outslay (2002) and Hanlon (2003), a public firm’s financial statements are not fully revealing about a firm’s actual tax payments made to the IRS. Among other reasons, this is because accounting under Generally Accepted Accounting Principles (GAAP) and tax accounting under the Internal Revenue Code are not identical.
disclosure of certain corporate tax return information, including information necessary to reconcile the firm’s tax information with its financial statement information; it was not enacted. In 2007, U.S. public firms were required to increase their public disclosure of information related to uncertain tax positions. More recently, an article in The New York Times (2010) entitled “Should Tax Bills Be Public Information?” has brought this topic back in to the public debate (Bernasek, 2010). President Obama’s 2012 Framework for Business Tax Reform calls for “improved transparency” through, among other means, “greater disclosure of annual corporate income tax payments” (The White House and The U.S. Department of the Treasury, 2012, p. 10). All of this debate and these policy proposals have occurred in the complete absence of empirical evidence of taxpayer responses to income tax disclosure.

While the United States has had, then abolished, and recently debated, public income tax disclosure, such systems are in place in several countries. Among Organisation for Economic Co-operation and Development (OECD) countries, Norway, Sweden, Finland, and Japan (until it was abolished in 2005) allow some form of public access to some tax information. Australia is currently considering a new disclosure requirement, which would require firms to disclose taxes payable and taxable income (Commonwealth of Australia, 2013). One important feature of the Australian proposal is that, like the disclosure system in Japan, only taxpayers above a certain income threshold (AUD 100 million) would be subject to the disclosure rule. This proposed feature makes the findings of our study, which studies a disclosure regime that applies only to taxpayers above certain thresholds, especially relevant. Lastly, while few jurisdictions disclose full tax information for their general population, in certain countries, there is public disclosure of information about convicted tax evaders.

B. The Japanese Income Tax Disclosure Regime

Japan, the setting for our study, implemented a system of tax disclosure in 1950. Concern about tax evasion figured prominently in the development of a post-war Japanese income tax system. In the thorough post-war income tax revision of 1947, a third-party reporting system (offering a reward if the information provided contributed

12 While FIN 48 (now ASC 740-10) did not mandate the disclosure of actual tax return information, it did increase disclosure of tax information for public U.S. firms. Consistent with the findings in this paper, public firms appear to have taken actions to avoid this mandated disclosure (Blouin et al., 2010). Inconsistent with the findings of our paper but consistent with our hypothesis, there is some preliminary evidence that the increased tax disclosure required by FIN 48 (or potentially the forced calculation of the figures to be disclosed) reduced firm tax aggressiveness, at least at the state level (Gupta, Mills, and Towery, 2013).

13 Norwegian tax returns have been public information since 1863, but until 2002 it was only possible to see other people’s tax information by applying in person at a tax office. In 2002 the information was published, and made easily searchable, on the Internet or through a text-messaging service for mobile phones (“Norwegian Tax Office Tells All Online,” BBC News, October 11, 2002, http://news.bbc.co.uk/2/hi/not_in_website/syndication/monitoring/media_reports/2321301.stm). For an analysis of the consequences of moving disclosed tax information to the Internet, see Slemrod, Thoresen, and Bø (2013).
to discovering tax evasion) and a tax return inspection system (under which for a fee private citizens could inspect all tax returns) were introduced. However, based on the Shoup Mission report of 1949, the tax return inspection system was abolished and, in 1950, the taxpayer “notification system” for high-income taxpayers was introduced in its place. The third-party reporting system was abolished in 1954, but the high-income taxpayer notification system continued until tax year 2004 for individuals (2005 for corporations). The last notification in 2005 (2006) corresponded to tax year 2004 (2005) information.

The high-income taxpayer notification system was designed to prevent underestimated tax declarations and tax evasion. The idea was that the system would introduce the possibility that tax evasion would be discovered by third parties if the amount of a declaration was strikingly low compared to the lifestyle and other publicly known information about the taxpayers. Under the notification system, the name, address, and the amount of taxable income (until tax year 1982) or income tax liability (after 1982) of the affected individual taxpayers were publicly posted on a bulletin board in each tax office for a period of about two weeks. The information was often collected and published by private companies, and frequently attracted media attention.

Similar aims motivated the corporate notification system, which was introduced at the same time. Disclosure applied to corporations whose taxable income exceeded the thresholds noted in Table 1. The information disclosed included the corporate name, taxable income, the tax office to which the tax was remitted, the name of the company’s president, and the beginning and ending day of the accounting year. The information was usually posted publicly at the tax office within three months after the company submitted its tax return, and was public for at least one month. Although the corporate tax information apparently attracted less frequent media attention than the individual tax information, it was at times collected and published by private publishing companies.

Notably, under the disclosure system it was possible for individuals to avoid the notification system by filing a corrected income tax declaration in April after underestimating tax liability in an initial return subject to the notification deadline of March 31. This would subject the taxpayer to arguably small penalties for arrears and understatement. There is, however, no evidence about the extent of this behavior. Corporations could not use a similar method because both the initial tax report and any corrected tax report were subject to disclosure. However, any corrections initiated by the tax authority were not subject to disclosure. There is anecdotal evidence that some companies asked the tax authorities to correct their understated tax reports so they could avoid notification. There is, however, no systematic evidence about the extent of this behavior.14

From the beginning of the notification system, disclosure was required only of taxpayers whose reports exceeded a high threshold of tax liability or taxable income. Until

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14 Anecdotal evidence gathered from conversations with Japanese tax professionals suggests that companies would occasionally understate their tax liability to escape disclosure with an implicit understanding with the tax office that it would correct the report without penalty, thus undermining the disclosure but not underpaying their tax liability.
### Table 1
Details of the Japanese Tax Notification System, Tax Years 1950–2004

**Panel A. Individual Tax Notification System, Tax Years 1950–2004**

<table>
<thead>
<tr>
<th>Tax Year</th>
<th>Range of Notification</th>
<th>Approximate Number of Taxpayers Subject to Notification</th>
<th>Percentage of Taxpayers Subject to Notification</th>
</tr>
</thead>
<tbody>
<tr>
<td>1950–1951</td>
<td>Taxable Income &gt;500,000 yen</td>
<td>90,000–150,000</td>
<td>2.1–4.3</td>
</tr>
<tr>
<td>1952–1956</td>
<td>Taxable Income &gt;1,000,000 yen</td>
<td>30,000–70,000</td>
<td>1.2–3.2</td>
</tr>
<tr>
<td>1957–1962</td>
<td>Taxable Income &gt;2,000,000 yen</td>
<td>20,000–150,000</td>
<td>1.1–6.3</td>
</tr>
<tr>
<td>1963–1969</td>
<td>Taxable Income &gt;5,000,000 yen</td>
<td>30,000–170,000</td>
<td>1.2–3.9</td>
</tr>
<tr>
<td>1970–1982</td>
<td>Taxable Income &gt;10,000,000 yen</td>
<td>80,000–440,000</td>
<td>1.7–6.7</td>
</tr>
<tr>
<td>1983–2004</td>
<td>Tax Liability &gt;10,000,000 yen (taxable income ~ 34,000,000)</td>
<td>70,000–180,000</td>
<td>0.9–2.0</td>
</tr>
</tbody>
</table>

**Panel B. Corporate Tax Notification System, Tax Years 1950–2004**

<table>
<thead>
<tr>
<th>Tax Year</th>
<th>Range of Notification</th>
<th>Approximate Total Number of Corporations</th>
<th>Approximate Number of Corporations Subject to Notification</th>
</tr>
</thead>
<tbody>
<tr>
<td>1950–1951</td>
<td>Taxable Income &gt;2,000,000 yen</td>
<td>208,000–239,000</td>
<td></td>
</tr>
<tr>
<td>1952–1956</td>
<td>Taxable Income &gt;4,000,000 yen</td>
<td>269,000–424,000</td>
<td></td>
</tr>
<tr>
<td>1957–1969</td>
<td>Taxable Income &gt;20,000,000 yen</td>
<td>449,000–952,000</td>
<td></td>
</tr>
<tr>
<td>1970–2004</td>
<td>Taxable Income &gt;40,000,000 yen</td>
<td>1,000,000–2,915,000</td>
<td>69,000–84,000</td>
</tr>
</tbody>
</table>

The Effect of Public Disclosure on Reported Taxable Income

The effect of public disclosure on reported taxable income was defined in terms of taxable income, and thereafter it was defined in terms of tax liability.\(^{15}\) For corporations, the threshold was defined in terms of taxable income for all the years of the disclosure regime. The thresholds were high enough so that, over the course of the disclosure law, only between 0.9 percent and 6.7 percent of individual taxpayers were subject to the notification rule. In 2004, 2.4 percent of all corporations were subject to disclosure. The threshold for disclosure was increased five times over its life. Table 1 presents relevant statistics about the evolution of the system.

In its 2005 report, the Japanese Tax Advisory Commission recommended the elimination of the notification system, asserting that it “is being utilized in various ways inconsistent with its initial aim, and there are various reports of the disclosure being a factor in causing crimes and harassment … Based on circumstances such as these, the system of disclosure should be abolished.”\(^{16}\) Following this recommendation, the notification system was abolished by the Act on the Protection of Personal Information, which became law on April 1, 2005. This Act stipulated that the last notification date for individuals was May 31, 2005, and was February, 2006 for corporations.\(^{17}\)

Because the notification process reveals the number and tax liabilities of high-income individual taxpayers (and little other information is publicly released about the tax system, even in aggregated form), some researchers have used the disclosed information to study the effects of tax policies (e.g., Makino, 1997). Others have utilized the longitudinal nature of the data to study the evolution of individual incomes in Japan.\(^{18}\) Researchers have also used the corporate tax disclosure data to evaluate properties of book-tax differences for Japanese firms (Goto, Hirohisa, and Yamashita, 2007). But the impact of the disclosure system itself on taxpayer behavior has not been studied empirically heretofore.

### III. HOW PUBLIC DISCLOSURE AFFECTS TAXPAYER BEHAVIOR

Taxpayer responses to disclosure hinge upon whether they consider disclosure to present a cost or provide a benefit. It is possible that taxpayers view disclosure of income as a positive signal of their success, gaining from the publicity of publicly revealed evidence of their high income. If this is the case, we may expect taxpayers to take actions to make disclosure more likely in order to receive the publicity that public disclosure would provide.

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\(^{15}\) The Japanese tax year for most individuals is January 1 to December 31.

\(^{16}\) See Tax Advisory Commission (2005) — translation provided by Lingua Science Corporation of Ann Arbor, MI. The Tax Advisory Commission was established by the Japanese government in 1953 to review the tax system and to formulate annual tax changes as well as long-run tax policy (Ishi, 1993).

\(^{17}\) Disclosure has returned to Japan. As of March 31, 2010, the Financial Services Agency requires corporate executives with salaries exceeding ¥100 million to publicly disclose their salaries.

\(^{18}\) For example, see Hashimoto (1995). Moriguchi and Saez (2008), in their study of the evolution of income concentration in Japan from 1885 to 2002, use different data sources. Ichikawa (1991) uses the notification data to analyze earnings patterns by occupation and educational status.
Alternatively, in what we consider the more plausible case, if the disclosure system imposed (avoidable) real costs on taxpayers, then one would expect to see a behavioral response to the system of disclosure. This will be the case whether these costs are those claimed by supporters of tax disclosure (increasing the costs of income tax noncompliance by increasing the probability of detection), or those claimed by opponents of disclosure (privacy costs for individual taxpayers or proprietary costs for firms). If, in addition, taxpayers are able to manipulate taxable income so as to avoid disclosure, we would expect that some taxpayers manipulate their reported taxable income so as to fall below the disclosure threshold.

If, as is reasonable, the costs of manipulating one’s income are increasing in the amount of manipulation needed to escape disclosure, then the frequency of reports with taxable income or tax liability just above the threshold should be less than what it would be in the absence of a disclosure rule, and the discrepancy should decline as taxable income increases above the threshold. This hypothesis is consistent with the (heretofore untested) claim in Lenter, Shackelford, and Slemrod (2003, p. 826) that “if disclosure is costly, then some taxpayers will respond to disclosure by [taking actions] to avoid the disclosure requirements.”

IV. ANALYSIS OF INDIVIDUAL INCOME TAX DISCLOSURE DATA

A. Data Source

In order to examine the behavioral response of individual taxpayers to the disclosure system, we analyze data from the tax years 2001, 2002, and 2003. These data include information about tax liability and an estimate of the taxable income corresponding to the tax liability. These data were purchased from General Legal Security, Inc., a Japanese company that compiles the data from the public notification records.

B. Analysis

We begin our examination of the behavioral response to the abolition of the Japanese disclosure system by examining the distribution of taxable income obtained from the disclosure data and searching for “missing tax returns” immediately above the disclosure threshold.

19 This is similar to several recent papers that examine taxpayer behavior around notches and kinks, using taxpayers’ bunching around thresholds to examine taxpayer behavior (Slemrod, 2010). For example, Saez (2010) uses kinks in both the earned income tax credit and the individual income tax rate schedule to estimate individuals’ compensated elasticity of taxable income. As in Saez (2010), we use taxpayer behavior around a threshold to make inferences about taxpayer behavior.

20 The company claimed in its online advertising that the compilation of these data follows the regulations of the Act on the Protection of Personal Information. Note that, although the name and address of the individual taxpayers are included in the public disclosure, they are not part of the database that is available for purchase. Other databases, such as the Who’s Who, do have this information for some years, but not in electronic form. In scattered cases the publishers supplemented the disclosure data with information about occupation. While ideally we would like to obtain some taxpayer identifier and link taxpayers over time to examine behavior over time, we have not been able to obtain such an identifier.
threshold. This, of course, requires an assumption about the counterfactual distribution for taxable income. There is considerable evidence that, absent disclosure, income for the top percentiles of the individual income distribution follows a Pareto distribution.\textsuperscript{21} Piketty and Saez (2006), and Moriguchi and Saez (2008) for Japan in particular, argue that the Pareto distribution is a reasonable approximation for top incomes.\textsuperscript{22} Given this assumption, we analyze the micro-disclosure data to determine whether there is evidence in the disclosure data of “missing tax returns” with taxable incomes corresponding to tax liabilities just above the disclosure threshold.

To do this, we first use a maximum-likelihood procedure to estimate the parameters of a Pareto distribution based on the disclosure information. We then compare the actual number of returns just above the threshold to the predicted number of returns based on the estimated Pareto distribution.\textsuperscript{23} Observing “too few” actual tax returns with taxable income immediately above the threshold would be consistent with some taxpayers manipulating their tax liabilities in order to avoid public disclosure. This manipulation may take the form of illegally not reporting taxable income (tax evasion), increasing tax deductions or tax credits and legally decreasing the tax liability (such as increasing the amount of charitable contributions during the year), delaying the receipt of income until the following tax year (timing effects), or simply generating less taxable income (by working less or curtailing business activities).

Below, we report several variations of our estimates of the number of “missing tax returns” due to manipulation and explain our robustness tests. We find that our results are robust to the use of these alternative procedures. We perform these calculations for tax year 2003 and then briefly summarize similar findings obtained for tax years 2001 and 2002.

1. Estimation Based on Reported Individual Taxable Income

We begin by estimating the distribution of individual taxable income. The advantage of focusing on income is that the prior literature concludes that top incomes follow a Pareto distribution (Moriguchi and Saez, 2008). The disadvantage is that we do not have information on actual reported taxable income, but rather have to estimate taxable income using tax liability. This is done by “grossing up” the tax liability by the applicable statutory tax schedule to arrive at an estimate of taxable income.\textsuperscript{24} This assumes a predictable mapping between taxable income and tax liability, which is likely

\textsuperscript{21} Pen (1971) discusses the basis for this claim and theories about why it might be true.
\textsuperscript{22} Our analysis below is necessarily based on imputed taxable income, a slightly different concept from the one studied by, for example, Piketty and Saez (2006), who focus on gross income (before deductions) reported on tax returns.
\textsuperscript{23} Since 1999 the disclosure thresholds have always been higher than the threshold for the top marginal tax rate (the top rate has applied to income over ¥18 million from 1999 to 2013), so we are not in danger of conflating responses to marginal tax rates with responses to disclosure rules.
\textsuperscript{24} For example, the ¥10 million (approximately $97,000 in 2005) tax liability threshold corresponds to ¥34,432,000 (approximately $332,000 in 2005) of taxable income according to the formula used by the data provider. The general formula is: estimated taxable income = (tax liability + 2,740,000)/0.37, where 37 percent was the top statutory marginal tax rate throughout our sample period.
to introduce some error into our measure of taxable income (for example, by ignoring tax credits for individual taxpayers). However, there is no reason to believe that the conversion of tax liability to taxable income would bias our estimate of the extent of “missing” tax returns.

Using these taxable incomes, we estimate the Pareto parameter with a maximum likelihood procedure using all the disclosed tax returns for tax year 2003. This produces an estimated Pareto index of 2.213. The top panel of Figure 1 shows a histogram of taxable income, where the bin width is ¥338,435, as well as the estimated Pareto distribution for taxable incomes between the disclosure threshold of ¥34,432,000 and ¥50 million. The comparison suggests that there are fewer returns reported just above the disclosure cutoff in the empirical distribution than in the estimated Pareto distribution.

In order to quantify the extent of this phenomenon, we next compare the estimated Pareto distribution to a non-parametric, kernel density estimation, with a bandwidth of ¥1,037,694, to the actual probability density function. We use the Epanechnikov kernel function and Silverman’s optimal bandwidth technique, as used as the default procedure in STATA, and a boundary correction procedure. The estimated Pareto distribution and the kernel density estimate are shown together in the lower panel of Figure 1. The kernel density estimate lies clearly below the estimated Pareto distribution for disclosed incomes close to the threshold.

We can derive an estimate of the number of “missing” returns by calculating the area between the two curves in the range between the threshold and where the estimated Pareto probability density function first clearly crosses the kernel density function (indicated by the shaded area in the lower panel of Figure 1).

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25 We performed extensive analyses examining whether the lognormal distribution would be an appropriate alternative to the Pareto distribution. Our analyses revealed that the lognormal distribution does not fit the upper portion of Japanese income well, and that the Pareto distribution achieves a much superior fit. The Pareto probability density function is \( f(x) = \frac{ak^a}{x^{a+1}}, x \geq k \). We refer to \( a \) as the Pareto parameter. The procedure uses the \texttt{paretofit} command that can be downloaded from Statistical Software Components (at “Boston College Department of Economics Statistical Software Components,” Boston College, http://ideas.repec.org/s/boc/bocode.html).

26 An extremely useful test would examine the distribution of taxable incomes just below the disclosure threshold. However, because our data are made available as a result of the disclosure requirement, we cannot pursue such an approach.

27 The kernel density estimator is a generalization of a histogram estimator to obtain smoother density estimates than a histogram density estimator. The kernel density estimates are obtained by choosing a kernel function and a smoothing parameter (bandwidth). See Cameron and Trivedi (2005) for the details of the derivation of the kernel density estimator and the optimal bandwidth choice.

28 The boundary correction procedure makes use of the \texttt{kdens} command that can be downloaded from Statistical Software Components (at “Boston College Department of Economics Statistical Software Components,” Boston College, http://ideas.repec.org/s/boc/bocode.html). We also repeated the procedure using twice, and half, the Silverman bandwidth. Using twice the bandwidth generates results that are very similar to the reported results. In some cases, using half the bandwidth approximately halves the estimated number of cases (but keeps the estimated percentage of “missing” returns about the same) because the estimated kernel density first intersects the estimated Pareto distribution at a lower value of taxable income. This is not surprising because reducing the bandwidth reduces the smoothness of the kernel density and so makes it more likely that at some point its density exceeds the density of the estimated Pareto distribution.
Figure 1
Comparing the Actual to Fitted Distribution of Individual Taxable Income Using All Returns, Tax Year 2003

Histogram and Fitted Pareto Distribution of Individual Taxable Income

Kernel Density Estimated and Fitted Pareto Distribution of Individual Taxable Income
When we do so, we get an estimate of 870 missing returns, which is 4.1 percent of the number of returns predicted by the Pareto distribution. It is important to note that, as previously mentioned, the cost of manipulating one’s income to below the threshold is plausibly increasing in the distance from the threshold. This is consistent with the data, as the “missing tax returns” seem to be primarily located near the threshold and then dissipate as one moves further from the threshold.

The behavioral response to the disclosure system is almost certainly understated by the procedure just described because the Pareto parameter is estimated including the range of returns we hypothesize are affected by the manipulated responses themselves. In other words, the Pareto parameter meant to approximate the counterfactual is based on a distribution of taxable income with some returns that are absent — the returns whose tax liability was manipulated so as not to be disclosed. To investigate this issue, we repeat the previous exercise but estimate the Pareto parameter using only the reports with tax liabilities that exceed ¥40,039,000 of estimated taxable income, at which point the two curves of the kernel density estimate and the fitted Pareto distribution first intersect. Note that the Pareto parameter estimated in this alternative manner should be higher if, as hypothesized, there are “missing” returns just above the threshold. Indeed, the estimate is higher than before, 2.256 compared to 2.213.

Comparing the newly estimated Pareto distribution to the distribution from the kernel density procedure suggests that the number of missing returns is 1,221, or 5.3 percent of the number of returns predicted by the Pareto distribution. Thus, using this procedure produces an estimate of the extent of the missing disclosed returns that is 40 percent higher than the previous method. The relevant graphs are shown in Figure 2.

While we obtain a point estimate of 870 missing returns using all disclosed tax information, this estimate does not suggest the likelihood that such an estimate could be obtained as a result of random chance. To establish that our results are statistically significant, we perform a bootstrapping test. We use our estimated Pareto parameter, 2.213, and the threshold value, ¥34,432,000, to generate 73,936 observations selected from the Pareto distribution (this is the number of observations in the disclosure data in 2003). We then obtain a kernel density estimate using these data, and calculate the implied number of “missing tax returns” by comparing the Pareto distribution to the kernel density estimate. We perform this procedure 1,000 times and calculate the number of times an estimate as large as the one we obtained from the actual disclosure data, 870, would be obtained from randomly generated observations that truly follow a Pareto density function. Of the 1,000 times we perform this procedure, the maximum number of implied “missing tax returns” is 666, and the minimum is 2, with a mean number of “missing tax returns” equal to 243. This means that in exactly zero out of 1000 draws a random generation of data from a Pareto distribution yields a value greater than

---

29 The use of the point where the kernel density function and the Pareto distribution intersect is somewhat arbitrary. It may be the case that individuals who are missing from the distribution as a result of the disclosure system would be present in the distribution in the absence of the disclosure system to the right of this intersection, or to the left of it. We calculate using the intersection only for convenience.
Figure 2
Comparing the Actual and Fitted Distribution of Individual Taxable Income Using Only “Unmanipulated” Returns, Tax Year 2003
than the estimate of 870 missing tax returns generated from the actual disclosure data. This translates into a highly significant bootstrapped estimate of a p-value related to our estimate of “missing tax returns”.

We also conduct a Kolmogorov-Smirnov goodness-of-fit test in order to corroborate our bootstrapped estimates. The Kolmogorov-Smirnov procedure tests whether the actual empirical distribution of data is the same as a hypothesized distribution of the data. In our case, our null hypothesis is that taxable income follows a Pareto distribution, which would suggest no manipulation to avoid disclosure. We test this null hypothesis on several portions of the empirical distribution of taxable income. First, in the region between ¥34,432,000 and ¥40,039,000, where our prediction of manipulation to avoid disclosure would be consistent with a rejection of the null hypothesis, it is rejected. The Kolmogorov-Smirnov test suggests that between ¥34,432,000 and ¥40,039,000 the empirical data are statistically different (at a p=0.011 level) from a Pareto distribution.

However, we fail to reject the null hypothesis for other portions of the distribution of taxable incomes. For incomes between 40.039 and 50 million, 50 and 60 million, 60 and 70 million, 70 and 80 million, 80 and 90 million, and 90 to 100 million yen, the Kolmogorov-Smirnov test suggests that the empirical distribution found in the disclosures is statistically indistinguishable from the Pareto distribution. These results both confirm our prediction of manipulation near the threshold (resulting in a deviation from the Pareto distribution), and confirm that assuming a Pareto distribution is justified for the unmanipulated portion of the distribution of taxable income.

2. Results for Tax Years 2001 and 2002

To check that the previously described results for tax year 2003 were not an anomaly, we repeated the same tests for tax years 2001 and 2002. The results are summarized in Table 2, which reveals that we obtain very similar results for 2001 and 2002 as we find for 2003. For each of the two methodologies, the estimated number of missing returns in 2002 is below the estimated number in either 2001 or 2003. The details of these calculations are available from the authors.

3. A False Experiment Robustness Test

As another robustness test we repeat our methodology using four arbitrarily picked threshold levels, all of which exceed the real threshold level of ¥34,432,000. We conduct the exact same analysis as when using the real threshold, discarding all data below our new arbitrary thresholds, and estimating the number of “missing returns” based only on incomes above the arbitrarily chosen thresholds. In all four cases, our method produces a trivially small number of “missing returns.” For example, when we use ¥70

30 Because the data are unavailable due to the nature of the disclosure system, we cannot investigate an arbitrarily lower threshold.
million as our false experiment threshold, we obtain a Pareto parameter of 2.229, and estimate a total of three missing returns, compared to 870 using the actual disclosure threshold. We obtain similar results when we examine the false thresholds of 50 million, 60 million, and 80 million yen. This result suggests that our missing returns are not merely a result of our distributional assumption or some other methodological problem.

4. A Potential Alternative Explanation

The presence of some gap in density just over the threshold is also consistent with disclosure causing people to increase their reported income. To see this, imagine that disclosure causes that all taxpayers subject to disclosure to increase their reported income by 100 yen. Then no one would report between, for example (for firms), ¥40,000,100, and the density of the rest of the distribution to the right of the threshold would increase imperceptibly. This would occur if taxpayers whose (previously optimal) reported income lay above the threshold would increase reported income if they felt that disclosure increased the probability that understatement would

Table 2

<table>
<thead>
<tr>
<th>Tax Year</th>
<th>Based on All Returns</th>
<th>Based on “Unmanipulated” Returns</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimated Pareto Parameter</td>
<td>Estimated Number of Missing Returns</td>
</tr>
<tr>
<td>2001</td>
<td>2.2500</td>
<td>982</td>
</tr>
<tr>
<td>2002</td>
<td>2.2461</td>
<td>754</td>
</tr>
<tr>
<td>2003</td>
<td>2.2129</td>
<td>870</td>
</tr>
</tbody>
</table>

Note: This table documents the estimated Pareto parameter obtained when fitting the disclosure data to a Pareto distribution and the resulting number of missing returns estimated when using that Pareto parameter. This analysis is done using all the data available (Based on All Returns), and only using returns that are sufficiently above the threshold that they are likely not manipulated (Based on “Unmanipulated” Returns), for the years 2001, 2002, and 2003.

31 This also requires that taxpayers under the threshold do not over-report to be disclosed (as they could perceive that not being disclosed signals their income is below the threshold, which could increase the threat of reporting by peers who believe the taxpayer to have a true income that merits disclosure). Hasseldine et al. (2007) provides evidence that taxpayers that fall below (non-public) disclosure thresholds may increase their declared income and subject themselves to increased disclosure if subjected to potential tax authority scrutiny.
be detected and punished (and if this decrease in probability outweighed the extra tax liability they would incur from reporting additional income). In addition, someone whose optimal reported income had been just above the threshold would need to weigh the benefit of under-reporting enough to avoid disclosure against the cost of deviating from their otherwise-optimal report (i.e., the associated increase in tax authority scrutiny from under-reporting).

Further, we observe that the hole in the distribution seems to be decreasing in size with the distance from the threshold, which is consistent with the expected cost of under-reporting increasing with its magnitude. Lastly, anecdotal evidence is rich with assertions that taxpayers under-report to avoid disclosure, but we have not heard examples of people over-reporting because of disclosure. Nevertheless, taxpayer over-reporting as a result of disclosure could be part of the explanation for what we observe.\(^{32}\)

V. ANALYSIS OF CORPORATE INCOME TAX DATA

A. Data Source

Our examination of the behavioral response of corporate taxpayers to the system of tax disclosure begins with an analysis of income tax disclosure data from the 2005 calendar year. This data set includes 68,824 observations, with information about taxable income (current and the previous three years if disclosed), corporation name, the tax office to which the tax was remitted, business sector, whether the company is public or private, the name of the company’s president,\(^{33}\) and the beginning and ending month of the accounting year.\(^{34}\) The corporate data were purchased from Diamond, Inc., a Japanese company that compiles the data from the public notification records and other publicly available information.

B. Analysis of Disclosure Data

We follow the same procedure as outlined above to analyze the corporate data, assuming that the distribution of disclosed corporate taxable incomes in the upper tail follows a Pareto distribution. We assume a Pareto distribution in order to be consistent with our analysis for individual taxpayers, because doing so seems to fit the data well, and because several corporate-level attributes have been found to be distributed following

\(^{32}\) We are grateful to Brian Erard for clarifying this issue to us.

\(^{33}\) All text-based data that we have obtained is available only in Kanji (or the native character system for the foreign firms operating in Japan), which has limited our ability to use these data.

\(^{34}\) Most companies’ accounting periods span one year. The data show that a small fraction of companies (1,608 of 68,824) have accounting periods shorter than one year, of which 289 have a half year or shorter accounting period. For this latter group, the disclosure threshold was ¥20 million rather than ¥40 million. In the analysis that follows we exclude all the companies with an accounting period that is six months or less.
the Pareto distribution. For example, Axtell (2001) notes that employees, revenue, and market capitalization all follow a Pareto distribution. The estimated Pareto index for reported corporate income in the 2005 calendar year is 0.860 when estimated over all observations, and is 0.900 when using only observations with reported taxable income over ¥75,022,000, at which point the two curves of the kernel density estimate and the fitted Pareto distribution based on the former Pareto parameter (0.860) first intersect (where we argue the “unmanipulated returns” begin). The top panel of Figure 3 shows a histogram of corporate taxable income, where the bin width is ¥1,111,089, as well as the estimated Pareto distribution of corporate taxable income between the threshold, ¥40 million, and ¥90 million. The lower panel of Figure 3 compares the estimated kernel density for taxable incomes below ¥90 million, with bandwidth of ¥3,497,544, to the former estimated Pareto distribution, while Figure 4 does the same for the latter estimated Pareto distribution.

As with the individual data, we find evidence that is consistent with behavior designed to avoid disclosure. The number of missing returns based on the Pareto distribution estimated from all disclosed returns is 1,380 (4.9 percent of the number of returns predicted by the Pareto distribution), while the number based only on the “unmanipulated” returns is 2,449 (7.4 percent of the number of returns predicted by the Pareto distribution); this compares to 68,824 corporations that disclosed taxable income and 2,915,259 corporations overall. These estimates are almost twice as high as the estimates for individual tax returns calculated above.

The phenomenon of companies understating taxable income to just below 40 million yen in order to avoid disclosure was well-known during the disclosure era, if rarely publicly discussed. Indeed, the practice was common enough to have merited a moniker: “39 companies.” Anecdotally, these “39 companies” manipulated their taxable income so as to fall below the threshold in part so they could hide their profitability from suppliers who might seek better contractual terms from an apparently more profitable firm.

Note also that most of the companies around the disclosure threshold, where we find evidence of under-reporting to avoid disclosure, are not particularly large and are generally not public corporations that of necessity must provide financial information to the public on a regular basis. For example, of the 1,000 smallest firms on the disclosure list (those that fall just above the threshold) in 2005, only one of them is a public corporation. It is reasonable to expect that, for public firms which face capital markets pressures and must report financial information to the public, the effect of disclosure might be different. For example, Rice (1992) finds that private firms are more tax aggressive than public firms, likely because private firms face fewer constraints on disclosure lower taxable incomes.

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35 We obtained corporate data only for 2005. Thus we cannot pursue the possibility that corporate behavior may have been different in 2005 than in other years (especially years not so close to the abolition of disclosure).
Figure 3
Comparing the Actual to Fitted Distribution of Corporate Taxable Income Using All Returns, Calendar Year 2005

Histogram and Fitted Pareto Distribution of Corporate Taxable Income

Kernel Density Estimated and Fitted Pareto Distribution of Corporate Taxable Income
Figure 4
Comparing the Actual to Counterfactual Distribution of Corporate Taxable Income Using Only “Unmanipulated” Returns, Calendar Year 2005
C. Analysis of Public Corporate Financial Data

1. Hypothesis and Test Design

To this point all of the analysis has been directed toward the hypothesis that some taxpayers will understate their reports in order to avoid disclosure. Another important issue, one that underlies much of the support for disclosure, is whether taxpayers subject to disclosure will restrain their under-reporting because of a fear that the public nature of their report will raise suspicion about under-reporting, and ultimately increase the chance of detection and punishment for noncompliance. Investigating this hypothesis is, however, hampered by the fact that, by its nature, the disclosure data are not available after the notification system was abolished.

However, for public corporations, the situation is more promising because firms’ publicly disclosed financial statements are available both before and after abolition, and from these financial statements it is possible to estimate a firm’s taxable income. To examine whether public firms altered their taxable income after the abolition of the disclosure system, we next analyze data from Compustat Global, which contains financial statement information about public firms located throughout the world. Our sample consists of the universe of firms covered by Compustat Global incorporated in Japan for which the relevant financial statement data are available for all the years considered.

We estimate the model

$$TIDiff_i = \beta_0 + \beta_1 Treatment_i + \beta_2 UnemDiff_i + \beta_3 UnemDiffSq_i + \beta_4 IndustryFixedEffects_i,$$

where $TIDiff_i$ denotes firm $i$’s estimated taxable income in year $t$ less the firm’s estimated taxable income in year $t - 1$, scaled by total assets in year $t - 1$; $Treatment$ is one if the firm was subject to disclosure in year $t - 1$ but not in year $t$, and zero otherwise; $UnemDiff$...
is the average monthly unemployment rate, as defined by the OECD, in year $t$ minus the average monthly unemployment rate in year $t - 1$; $UnemDiffSq$ equals $(UnemDiff)^2$; and Industry Fixed Effects is a vector of dummy variables indicating membership in a specific industry (where industries are defined by two-digit SIC-type codes).

We define Treatment using both the fiscal year end of the firm and the year in which the observation occurs. Our identification strategy relies upon the fact that the law mandated that disclosure would be abolished in February, 2006. However, because firms have two or three (depending on the type of company) months to file their tax returns, and some time is required for the National Tax Authority to process and disclose the returns, we observe that almost no public firms disclosed that had fiscal year ends in November 2005, and that no firms disclosed with fiscal year ends in December and thereafter. Thus, a firm with an October 2005 fiscal year end was likely to face disclosure (subject only to being above the threshold level of taxable income, which is likely for a public firm), while a firm with a fiscal year end of November 2005 and thereafter was not likely to face disclosure. Therefore, we define Treatment as a dummy variable that equals one if $t = 2005$ and the fiscal year end of a firm was November or December or if $t = 2006$ and the fiscal year end of a firm was between January and October, and equals zero otherwise.

The unemployment variables are intended to control for the effect of cyclical and secular economic conditions that would affect true, and therefore possibly reported, changes in corporate earnings. We include industry fixed effects to allow for any other systematic differences in firms from different industries. Given that industry membership is highly correlated with the fiscal month end of a firm, industry effects also partially control for systematic differences in firms with different fiscal year end months.

2. Results from Public Firm Data

We start by presenting the data visually. Figure 5 depicts the mean and the median $TIDiff$ by fiscal year end for the time period for which continuous data are available, from June 1992 to February 2010, with a vertical line indicating roughly where disclosure was no longer required of firms (after October 2005). Figure 6 focuses on a 24-month window, starting from six months before the end of disclosure to 18 months after it.

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38 We considered coding the Treatment variable using the actual disclosure data to specify which firms actually faced disclosure ex post. However, this has two serious problems. First, it assumes that firms who were ultimately not disclosed anticipated non-disclosure and acted accordingly; this may not be the case. Second, and more importantly, given that some firms did not face disclosure because their income turned out to be under the threshold limit of income, coding Treatment using ex post disclosure data would mechanically be related to firms having lower income and thus would create an endogeneity problem that would undermine a causal interpretation of the estimated coefficients.

39 For example, in Japan firms with March fiscal year ends are on average larger than other firms, and there are many more March fiscal year end firms than any other month.
Figure 5
Mean and Median $TIDiff$, by Fiscal Year End, June 1992 to February 2010

Notes: This graph displays the mean and median differenced taxable income ($TIDiff$) from 1993 to 2010 for all Japanese firms available on Compustat Global that meet our sample selection criteria. The vertical line near November 2005 represents the last fiscal year end date for firms which could reasonable have been assumed to be subject to the corporate tax disclosure regime.
Notes: This graph displays the mean and median differenced taxable income (TIDiff) from May 2005 to April 2007 for all Japanese firms available on Compustat Global that meet our sample selection criteria. The vertical line near November 2005 represents the last fiscal year end date for firms which could reasonable have been assumed to be subject to the corporate tax disclosure regime.
ended. The figures do not provide clear visual evidence of distinctly lower values of $TIDiff$ in the year after disclosure was abolished. However, there is a clear trend downward beginning in about 2007. While this downward trend in taxable income may have been in part the result of the abolition of disclosure, we have no way of compellingly identifying that this is the case.

We estimate the above regression with all the data available from Compustat Global, which includes observations from 1992 to 2010. This panel of data includes 59,667 observations and 4,714 unique firms. The results of this regression are shown in Table 3, using the least absolute deviation estimator in Column 1 and ordinary least squares (OLS) in Column 2. Notably the estimated coefficient on the treatment dummy for both specifications is positive; using two-way clustering of standard errors by year and fiscal year end month (for the OLS estimator), this estimated coefficient is statistically different from zero at a one percent level.

The estimated positive sign is inconsistent with the hypothesis that disclosure restrains aggressive tax behavior. There are several possible explanations for this result, which highlight the limitations in interpreting our estimated coefficients. For example, because the Japanese accounting system for public firms is highly conformed, the tax situation of public corporations in Japan is effectively disclosed even in the absence of the notification system. This is not inconsistent with our earlier finding that a non-trivial amount of firms understated their taxable income to avoid disclosure, because this finding related to relatively small, mostly non-public corporations. As mentioned above, of the 1,000 disclosed firms with the smallest disclosed tax liability, only one was a public corporation.

Another caveat to our identification strategy is that it relies upon firms being able to perfectly anticipate the end of the disclosure system and react quickly to it by changing their tax avoidance strategies. Absent this assumption, we would be left with trying to attribute the change in taxable incomes after 2007 to the end of disclosure, which is infeasible. Finally, our strategy assumes that tax disclosure was the only factor differentially affecting treated observations and untreated observations. For example, we rule out the possibility that the Japanese tax authority considered tax disclosure as part of their enforcement policy arsenal and, given its abolition, increased their level of tax auditing, which may have resulted in increased tax compliance (Slemrod, Blumenthal, and Christian, 2001; Hoopes, Mescall, and Pittman, 2012).

We performed many robustness tests to see how sensitive the above results are to different specifications. First, we included year fixed effects, to better control for macroeconomic factors that change by year and affect all firms the same. Including year fixed effects 40 These 4,714 firms over the entire 1992–2010 period contrast to the 68,824 firms that were subject to disclosure in calendar year 2005. The set of firms used for this test and for our distribution fitting are thus fundamentally different sets of firms.

41 Japan’s book and tax systems, while not completely conformed, are more closely conformed than most other countries. Indeed, in a sample of 33 major countries, Atwood, Drake, and Myers (2010) find that Japan has the fifth most book-tax conformed system.
Table 3
Public Corporation Response to the Abolition of Tax Disclosure

Panel A. Descriptive Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
<th>25th Percentile</th>
<th>Median</th>
<th>75th Percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td>TIDiff</td>
<td>59,667</td>
<td>0.01</td>
<td>0.05</td>
<td>−0.01</td>
<td>0</td>
<td>0.01</td>
</tr>
<tr>
<td>Treatment</td>
<td>59,667</td>
<td>0.07</td>
<td>0.25</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>UnemDiff</td>
<td>59,667</td>
<td>0.11</td>
<td>0.36</td>
<td>−0.26</td>
<td>0.13</td>
<td>0.32</td>
</tr>
<tr>
<td>UnemDiffSq</td>
<td>59,667</td>
<td>0.14</td>
<td>0.19</td>
<td>0.04</td>
<td>0.08</td>
<td>0.16</td>
</tr>
</tbody>
</table>

Panel B. Regression Results

**Dependent Variable: TIDiff**

<table>
<thead>
<tr>
<th>Independent Variables</th>
<th>Coefficient (t-statistic) (1)</th>
<th>Coefficient (t-statistic) (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment</td>
<td>0.00298*** (18.41)</td>
<td>0.00592*** (6.46)</td>
</tr>
<tr>
<td>UnemDiff</td>
<td>−0.00284*** (−20.73)</td>
<td>−0.00997** (−2.49)</td>
</tr>
<tr>
<td>UnemDiffSq</td>
<td>0.00224*** (9.23)</td>
<td>0.00830 (1.56)</td>
</tr>
<tr>
<td>Constant</td>
<td>−0.00011 (−0.25)</td>
<td>0.00106 (1.60)</td>
</tr>
<tr>
<td>Industry Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Quantile Regression</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Observations</td>
<td>59,667</td>
<td>59,667</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.0048</td>
<td>0.035</td>
</tr>
</tbody>
</table>

Notes: The dependent variable, TIDiff, is the firm’s estimated taxable income less the firm’s estimated taxable income in the prior year, scaled by prior year’s assets. Treatment is one if the firm was subject to disclosure in year $t−1$ but not in year $t$, and zero otherwise. UnemDiff is the average monthly unemployment rate, as defined by the OECD, in year $t$ minus the average monthly unemployment rate in year $t−1$. UnemDiffSq is the squared value of UnemDiff. Industry Fixed Effects are defined by two-digit SIC codes. The t-statistics for the OLS regression (included in parentheses) are calculated using standard errors clustered by fiscal year end month, and year. Asterisks denote two-tailed significance at the 1% (***), 5% (**), and 10% (*) levels.
fixed effects yields results that are similar to those tabulated.\textsuperscript{42} We also estimate the model with pre-tax book income as the dependent variable to account for the fact that estimating taxable income introduces error into the measurement of taxable income by, for example, under-estimating taxable income for firms which have tax credits that decrease their current tax expense. Doing this yields similar coefficients on the treatment indicator.\textsuperscript{43}

Finally, we estimated our model using a more limited time series, constraining the control group of untreated firms-years. This may yield different estimates if, for example, there was some large structural change in the Japanese economy not captured by our control variables (including our year fixed effects) that made the earlier portion of the time series incomparable to the later time period. We restrict our sample to the year in which the regime change took place (the Japanese fiscal year 2005, which includes April 2005–March 2006), essentially comparing the differenced taxable incomes of firms with fiscal year ends in March 2005 to October 2005 to firms with fiscal year ends November 2005 to February 2006. This generates a negative and significant estimated coefficient on the treatment indicator, the opposite sign to what we found in the specifications discussed above, which is consistent with the hypothesis that disclosure restrained aggressive tax planning. This result is not definitive because we cannot confidently argue that there was not a large, relevant structural shift in the Japanese economy during our sample period not captured by our control variables.\textsuperscript{44} The result does, though, suggest that our baseline result is sensitive to the time period used in the estimation and, as such, should be interpreted with caution.

Our failure to find evidence that the abolition of disclosure leads to decreased taxable income for large public corporations could result from the limitation of our data (public firms only), from our identification strategy, or both. However, why abolishing disclosure might be associated with an increase in reported taxable income, as the regression results from Table 3 suggest, is more difficult to explain.

\textsuperscript{42} In order to better control for macroeconomic factors, we also estimate the model controlling for: (1) percentage changes in population; (2) percentage changes in exchange rates between the yen and the dollar; (3) percentage changes in the GDP; and (4) percentage changes in the spot price of west Texas intermediate oil. We also include all of these new controls squared (for a total of 8 additional macroeconomic controls). While 7(4) of these 8 controls load significantly in the median (OLS) regression, they make only moderate improvements to the R-squared, and they do not materially change the coefficient on Treatment.

\textsuperscript{43} In another related test, we regressed the changes in the difference between book income and estimated taxable income (the book-tax difference, or BTD) on the variables in the baseline model. This variable is a measure of both tax avoidance and financial reporting quality, and large BTDs are interpreted as suggesting either aggressive tax planning, aggressive financial statement reporting practices, or both (Hanlon, 2005; Hanlon and Heitzman, 2010). Regressing differenced BTDs on our treatment variable and controls yields a negative and significant coefficient on Treatment. This is inconsistent with increased tax avoidance following the abolition of the disclosure regime.

\textsuperscript{44} Absent some structural shift in the Japanese economy, there is little reason to believe that discarding usable data and limiting our sample to only two years is the best sample selection procedure. Further, limiting our time series would not allow the estimation to take into account the true time-series variation in corporate taxable income.
D. Analysis of Both Public and Private Firm Data

One limitation of studying the public firms in the Compustat Global data is that public firms are arguably the least likely to react to a change in the system of public disclosure, given the widespread availability of financial information on such firms. In this section, we analyze a proprietary micro-level firm data set from the Ministry of Finance (MOF) of Japan, the “Financial Statements Statistics of Corporations by Industry, which covers both public and private firms.”45 This allows us to study the behavior of firms which are not otherwise subject to a large amount of public financial disclosure outside of the tax disclosure system. This proprietary data set can be accessed only after a lengthy application process, and can only be physically accessed at one location in Tokyo. Firms receive a survey from the Ministry of Finance which collects 133 data items about the firm, most of which come directly from financial statement data (not tax returns).46 Our panel dataset from the MOF contains observations from 1993 to 2009, and contains 180,049 observations representing 44,196 unique firms (the actual sample size varies based on our choice of dependent variable); 93.7 percent of these firms are private. One key detail of the survey procedure used to collect these data is that the frequency with which firms are subject to these surveys depends on the size of the firm — very large firms are required to respond to the survey annually, while smaller firms are subject to the survey on a random basis. This survey technique chosen by the MOF will affect our identification strategy, as will be explained later.

We use this micro-level data to estimate the following regression

\[
TI_{it} = \beta_0 + \beta_1 \text{After Disclosure}_{it} + \beta_2 \text{After Disclosure}_{it} \times \text{Private}_{it} + \beta_3 \text{Unemployment}_{it} \\
+ \beta_4 \text{UnemploymentSquared}_{it} + \text{FirmFE}_{it} + \text{YearFE}_{it} + \text{After Disclosure}_{it} \\
* \text{IndustryFE}_{it},
\]

where \(TI_{it}\) denotes firm \(i\)’s taxable income in year \(t\) divided by asset in year \(t\). We use the firm’s estimated taxable income, pretax income, and tax expense as three different proxies for taxable income. \(\text{After Disclosure}\) is one for the time after the end of the disclosure regime (based on the fiscal year end, November 2005) and \(\text{After Disclosure}_{it} \times \text{Private}_{it}\) is one if the firm is private and the time period is after the end of the disclo-

45 We would have also liked to use these data to replicate our distributional analysis on the full distribution of taxpayers (i.e., including those below the threshold). This is not possible for two reasons. First, the MOF data are financial statement data, and thus lack an exact measure of taxable income, forcing us to estimate the taxable income of the firm. Distributional analysis requires a precise value for taxable income (a value of ¥39.999 million needs to be differentiated from a value of ¥40.000 million). Second, the MOF data are survey data, and an examination of the data suggests that firms often round their reported figures, yielding numbers that are not precise enough for fitting a distribution.

46 While response to these surveys is mandated by law, there is little effort put into verifying the accuracy of the responses, and penalties for responding incorrectly are apparently light.
sure regime. Unemployment is the average monthly unemployment rate, as defined by the OECD, in year t and UnemploymentSquared equals \((Unemployment)^2\). After Disclosure * Industry FE is a vector of dummy variables indicating membership in a specific industry and the time after the end of the disclosure regime.

The dependent variable in this regression, TI, is a proxy for the firm’s taxable income. Since we lack actual tax return data, we rely upon three different proxies for taxable income. We winsorize all of the three types of dependent variables. The dependent variable is not differenced, which is different from the dependent variable in Model 1, where the dependent variable is TIDiff (differenced taxable income). The reason for this change is that in the micro data available from the Ministry of Finance, smaller firms are not often surveyed every year. Thus, using a differenced dependent variable requires that firms have two years of consecutive data, which effectively eliminates many smaller firms from our sample. Because we believe that smaller, private firms are more likely to exhibit a behavioral response to the abolition of tax disclosure, we instead include firm fixed effects, a procedure that, like differencing, adjusts for firm-specific but time-invariant effects.

Our hypothesis deals with the behavioral response to the abolition of disclosure for firm/years occurring after the end of the disclosure regime (as captured by After Disclosure), and we expect this response to be larger for private firms (as captured by After Disclosure*Private). This is because, by virtue of financial reporting to investors, we expect the difference in the overall level of public information available about private firms as a result of the end of disclosure to be much greater than the difference for public firms.

Although macroeconomic trends certainly affect the firms’ taxable income, our identification strategy requires the assumption that such trends do not differentially impact public and private firms in similar industries (as controlled for using the After Disclosure * Industry FE variables). Thus, while relying only on the After Disclosure dummy variable to measure a response to the abolition of disclosure would falsely attribute decreasing taxable incomes due to the business cycle (for example), identification achieved using the differential response of public and private firms to abolition requires the assumption that, starting in the treatment year of 2005, general economic conditions impact public and private firms similarly. While this is arguably less of an extreme assumption than assuming that no unobserved macroeconomic trend affected taxable incomes after disclosure, it is nevertheless non-trivial.

47 A firm’s public/private status is not recorded in the MOF data. We make this determination based on merging the 2005 Shikiho list (a list of all publicly traded firms in Japan) with our dataset based on firm name. This classification is imperfect because the merge relies upon firm name (and not a unique identifier, which is not available in the MOF data), and because it characterizes public/private status as a firm fixed effect (relying upon the firm’s public/private status in 2005). Given that firms both go public and go private after having been public, public/private is not, in reality, a firm fixed effect. However, we see no reason why this would bias our results.

48 While we tabulate only un-differenced taxable income using these data, using differenced taxable income does not change the tenor of our findings. Clustering standard errors by firm also does not change our inference.
Table 4
Public and Private Corporation Response to the Abolition of Tax Disclosure

Panel A. Descriptive Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
<th>25th Percentile</th>
<th>Median</th>
<th>75th Percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td>Estimated Taxable Income</td>
<td>180,049</td>
<td>0.040</td>
<td>0.053</td>
<td>0.003</td>
<td>0.021</td>
<td>0.056</td>
</tr>
<tr>
<td>Pre-Tax Income</td>
<td>153,896</td>
<td>0.050</td>
<td>0.056</td>
<td>0.013</td>
<td>0.032</td>
<td>0.066</td>
</tr>
<tr>
<td>Tax Expense</td>
<td>180,049</td>
<td>0.017</td>
<td>0.022</td>
<td>0.001</td>
<td>0.009</td>
<td>0.024</td>
</tr>
<tr>
<td>After Disclosure</td>
<td>202,858</td>
<td>0.268</td>
<td>0.443</td>
<td>0</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>After Disclosure * Private</td>
<td>202,858</td>
<td>0.217</td>
<td>0.412</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Unemployment</td>
<td>202,858</td>
<td>4.176</td>
<td>0.832</td>
<td>3.389</td>
<td>4.3</td>
<td>4.8</td>
</tr>
<tr>
<td>Unemployment Squared</td>
<td>202,858</td>
<td>18.131</td>
<td>6.684</td>
<td>11.485</td>
<td>18.49</td>
<td>23.040</td>
</tr>
</tbody>
</table>

Panel B. Regression Results

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Estimated Taxable Income</th>
<th>Pre-tax Income</th>
<th>Tax Expense</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient (t-statistic)</td>
<td>Coefficient (t-statistic)</td>
<td>Coefficient (t-statistic)</td>
</tr>
<tr>
<td>Independent Variables</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>After Disclosure</td>
<td>−0.03413*** (−3.89)</td>
<td>−0.01478 (−1.37)</td>
<td>−0.01404*** (−3.75)</td>
</tr>
<tr>
<td>After Disclosure * Private</td>
<td>0.00668*** (12.58)</td>
<td>0.00337*** (5.54)</td>
<td>0.00289*** (12.73)</td>
</tr>
<tr>
<td>Unemployment</td>
<td>−0.00347 (−0.65)</td>
<td>0.00468 (0.77)</td>
<td>0.00115 (0.50)</td>
</tr>
<tr>
<td>Unemployment Squared</td>
<td>0.00050 (0.84)</td>
<td>−0.00040 (−0.58)</td>
<td>−0.00009 (−0.36)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.04332*** (4.55)</td>
<td>0.03856*** (3.56)</td>
<td>0.01644*** (4.04)</td>
</tr>
</tbody>
</table>

Year Fixed Effect: Yes
After Disclosure * Industry Fixed Effect: Yes
Firm Fixed Effect: Yes

Observations: 180,049
R-squared: 0.025

Notes: The dependent variables, Estimated Taxable Income, Pre-Tax Income and Tax Expense, are three different proxies for the firms’ taxable income and are all scaled by assets. The value of After Disclosure is one for the time after the end of the disclosure regime. The value of After Disclosure * Private is one if the firm is private and for the time after the end of the disclosure regime. Unemployment is the average monthly unemployment rate for Japan, as defined by the OECD, in year t. Unemployment Squared is the squared value of Unemployment. Asterisks denote two-tailed significance at the 1% (***), 5% (**), and 10% (*) levels, calculated using standard errors after including firm fixed effects. Clustering by firm does not change our inference.
We estimate Model 2 using ordinary least squares and tabulate the results in Table 4. Columns 1, 2 and 3 use estimated taxable income, pre-tax income, and tax expense as proxies for the taxable income of the firm. The negative sign on After Disclosure suggests that, on average, firms did decrease their taxable incomes after the disclosure regime ended. However, the estimated coefficient on After Disclosure*Private, contrary to our expectation, is reliably positive across all three proxies for taxable income. This suggests that relative to public firms, private firms’ taxable incomes increased after disclosure. 49

To be sure, this micro level analysis has a number of limitations. Its strength relative to the public-firm-only analysis is that it involves smaller private firms that one might expect to have the most significant reaction to the abolition of disclosure. It also allows firms to take time to implement their tax avoidance technologies, as the identification strategy does not rely upon an immediate response. But, like the prior test, it is limited by its use of an estimate of taxable income instead of actual taxable income. In addition, it uses firm survey data that have not been independently verified or tested. As a result, our inability to find evidence that disclosure resulted in improved compliance should not be considered a closed question but rather one that future research should address. 50

VI. CONCLUSIONS

Public disclosure of tax return information is one weapon in the arsenal of tax enforcement policy instruments, one that has been employed historically, is currently in use in several countries, and for corporate returns, has recently been debated in the United States. The potential behavioral response to public disclosure of income tax returns figures prominently in policy debates about its advisability. Strikingly, all debates proceed in the near-complete absence of empirical evidence about the effects of public disclosure on behavior. This paper provides what, to our knowledge, is the first empirical evidence about the behavioral response to a tax return public notification system. Our analysis of data from the Japanese disclosure regime suggests that, when there is a threshold for disclosure, a non-trivial number of both individual and corporate taxpayers whose tax liability would otherwise be close to the threshold will under-report so as to avoid disclosure, provoking a response opposite to that stressed by supporters of disclosure. This suggests that some taxpayers perceive costs associated with disclosure, and they seek to avoid these costs by avoiding disclosure.

49 While inconsistent with our previous results, this finding would be consistent with private firms having substantial propriety costs associated with tax disclosure. For example, if, absent disclosure, suppliers are no longer able to use information on the firm’s profitability in setting prices, we may expect income to rise after the end of disclosure for private firms. This result could also be viewed in combination with our first hypothesis, which suggests that some taxpayers (those just under the threshold avoiding disclosure) will increase their taxable income without disclosure. To examine this possibility, we estimated Model 2 using the MOF data on a sample of firms that are well above the disclosure threshold (taxable income above 80 million yen). We find similar results as when using our full sample, suggesting that disclosure threshold effects do not explain our counterintuitive findings regarding our second hypothesis.

50 As in our prior test using public firm data, we perform a variety of sensitivity tests where we alter the model estimated, the estimation technique, and the variable calculation procedures. None of our analyses provides consistent support for our hypothesis that the abolition of disclosure resulted in firms decreasing their taxable incomes.
However, our analysis of corporate taxpayers in Japan finds no consistent evidence that this cost associated with disclosure is related to the ability of public disclosure to restrain tax avoidance. We are unable to find consistent evidence that firms decreased their taxable income after the abolition of tax disclosure. This suggests that other costs, such as proprietary costs (which, unlike tax related costs, are increasing in profitability), may be the reason that firms near the threshold took action to get below the disclosure threshold.

These findings will be useful to policymakers in considering enactment of a disclosure policy and designing a specific disclosure policy. For example, our findings suggest that non-universal disclosure will trigger avoidance behavior. If the level of taxable income or tax liability determines whether a taxpayer is subject to disclosure, as it did in Japan, the result can be a perverse revenue effect, as some taxpayers will manipulate their reported taxable income downward, decreasing revenues. If, as was proposed in H.R. 1556 in the United States in 2003, the disclosure system only applies to public firms, then, as Lenter, Shackelford, and Slemrod (2003, p. 826) suggest, some firms “might choose to withdraw from the public capital markets, rather than release their tax information.”

In short, policy makers need to carefully consider the basis for requiring disclosure and try to anticipate the consequences resulting from taxpayers trying to avoid disclosure.

There are, to be sure, caveats to generalizing our results. Our strong result applies only to disclosure systems where not all taxpayers must disclose. The behavioral response will also depend on the social stigma or reward from a public disclosure of high taxable income, which arguably varies across countries and cultures. Specifically, results from our study in Japan may not be applicable in other countries, where taxpayers’ incentives to make public their private information may be different, where the elasticity of taxable income may be different, or where tax “morale” or norms about tax compliance may be different. Nevertheless, this research does establish with some degree of confidence that public disclosure will change behavior, sometimes in unintended ways, as most tax policy instruments do.

Another recent example of taxpayers’ efforts to escape increased disclosure (albeit not public disclosure) involves Schedule M-3 in the United States. This new corporate income tax schedule, which requires a reconciliation between book and tax income and has the potential to allow the IRS to more efficiently audit companies, is required of all firms above a certain asset threshold that file Form 1120 and keep financial statements based on Generally Accepted Accounting Principles (GAAP). Dennis-Escoffier (2004) suggests that closely-held firms should consider escaping disclosure by simply ceasing to prepare GAAP financial statements; companies near the margin might also manipulate assets to stay below the asset threshold. Hasseldine et al. (2007) shows that increased perceptions of enforcement may encourage taxpayers to submit to increased disclosure requirements, suggesting that taxpayers’ willingness to disclose is also a function of tax authority enforcement.

Using survey data on tax compliance compiled by the World Economic Forum, La Porta et al. (1999) report a tax compliance index for Japan of 4.41, and for the United States of 4.47. In a sample with 49 countries with a standard deviation of 1.002, this difference is not statistically significant. However, Torgler (2004) reports that Asian countries, and Japan in particular, generally have higher tax morale than do other OECD countries. He measures tax morale in terms of the willingness of survey participants to justify cheating on their taxes. Thus, while there may not be material differences in tax compliance in Japan and other OECD countries, the Japanese may be less willing to justify what cheating does go on.
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