

# **The Effect of Government Transparency on Corporate Tax Avoidance: Evidence from State Freedom of Information Laws**

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# **The Effect of Government Transparency on Corporate Tax Avoidance: Evidence from State Freedom of Information Laws**

## **Abstract**

This paper examines the effect of government transparency on corporate tax avoidance using staggered changes in state freedom of information (FOI) laws to measure changes in state government transparency. Based on the tax morale literature, we argue that firms are more willing to pay taxes when they perceive the government to be more transparent and thus more trustable. Using a difference-in-differences design, we find that state FOI law improvement reduces state income tax avoidance by firms headquartered in the government's jurisdiction. The effect is stronger for firms headquartered in counties without strong civic norms emphasizing civic responsibility, those headquartered in more corrupt states, and those with greater tax exposure in their headquarters states. The law improvement increases not only corporate income tax revenues but also sales tax and individual income tax revenues for the state government.

**JEL Classification:** K4; M4

**Keywords:** Freedom of Information Laws; Government Transparency; Government Accountability; State Taxes; Tax Avoidance

## 1. Introduction

Due to escalating calls for accountability and openness, the movement towards improved government transparency has gained significant momentum globally (Relly and Sabharwal 2009; Bauhr and Grimes 2014). Government transparency concerns the availability of information about the internal workings or performance of the government (Porumbescu, Meijer, and Grimmelikhuijsen 2022).<sup>1</sup> While government transparency is generally considered one of the fundamental pillars of functional democracy and government accountability (Rourke 1960; Redford 1969; Alt and Lassen 2006), its impacts on private sectors, such as corporate behaviors and outcomes, are not well understood yet.<sup>2</sup>

From the perspective of a firm as “a nexus of contracts” (Jensen and Meckling 1976), firms have a variety of explicit and/or implicit contractual relationships with the government, such as government contracts, subsidies, tax obligation, and regulations. Thus, it is important to understand how transparency of the government as a contracting party affects corporate behaviors and outcomes. Understanding such impacts is also important for evaluating the social and economic implications of government transparency. We intend to contribute to this literature by studying whether and how government transparency affects corporate tax avoidance. The importance of this questions is evident because of the contractual relationship between firms and the government, which stems from the former’s obligation to pay taxes to the latter and the latter’s duty to spend this tax revenue to benefit the society, including the former.

Specifically, we examine how changes in state freedom of information (FOI) laws, which

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<sup>1</sup> For example, Piotrowski and Van Ryzin (2007) define government transparency as “the ability to find out what is going on inside a public sector organization through avenues as such as open meeting, access to records, the proactive posting of information on Web sites, whistle-blower protections, and even illegally leaked information.”

<sup>2</sup> Recent research that studies the effect of government transparency on corporate behaviors and outcomes include Hope, Jiang, and Vyas (2022), Cordis, Hsu, and Zhang (2022), and He, Li, Li, and Zhang (2023). We discuss these studies in Section 2.1.

change transparency of state governments, affect corporate state tax avoidance. Access to information is a central component of government transparency (Redford 1969; Piotrowski and Van Ryzin 2007). FOI laws, which grant citizens, organizations, and corporations the right to access a broad spectrum of records retained by the government, play an important role in underpinning government transparency in the United States. At the state level, each state has enacted its own FOI laws (e.g., the Texas Public Information Act), with different levels of enforcement and exceptions. Over the past decades, the majority of states have introduced fresh FOI laws, modified existing ones, or completely revised their statutes. These changes generally result in easier access to government records (Cordis and Warren 2014). We exploit these plausibly exogenous changes in state FOI laws to examine the causal effect of government transparency on corporate tax avoidance.

We hypothesize that the *perceived* improvement in government transparency due to the enhancement of state FOI laws can increase firms' willingness to pay state taxes through enhanced tax morale. Lutter and Singhal (2014) define tax morale as an umbrella term capturing nonpecuniary motivations for tax compliance as well as factors that fall outside the standard expected utility framework pioneered by Allingham and Sandmo (1972). The literature of tax morale, particularly the reciprocity view, suggests that tax compliance is affected by perceptions of the legitimacy of and attitudes toward the government, and perceptions about the fairness of the tax schedule (Luttmer and Singhal 2014). Prior research has provided evidence consistent with the reciprocal motive of individual taxpayers (e.g., Alt et al. 2010; Capasso et al. 2021; Besley et al. 2023). In the corporate setting, Chow et al. (2023) show that reciprocal motive can affect corporate tax avoidance behavior. They find that corporate tax avoidance increases with ground-level ozone pollution, which is perceived as an indicator that the government does not efficiently use tax money

to provide high quality public goods.

We argue that government transparency, as a crucial component of government accountability (Rourke 1960; Redford 1969; Alt and Lassen 2006), can increase taxpayers' trust in the government and enhance their tax morale through the reciprocity motive. Transparency enables the public to not only critically examine government decisions and hold officials accountable, but also to actively engage in the democratic process and shape public policies. It can help prevent corruption, enhance government performance, contribute to legitimacy, and promote principles of good governance (Roberts 2006; Grimmelikhuijsen et al. 2013; Cordis and Warren 2014; Cucciniello et al. 2017). All these are expected to increase taxpayers' trust in the government and their tax morale (Tolbert and Mossberger 2006; Cook et al. 2010). Consistent with this argument, prior research suggests that individuals are more willing to pay taxes when the government budget or spending is more transparent (Alt et al. 2010; Capasso et al. 2021).<sup>3</sup>

Ex ante, our prediction is not obvious because improved FOI laws may not necessarily reduce, or even increase corporate state tax avoidance. The literature has not reached a consensus on the reciprocal motive of tax compliance, with somewhat mixed evidence (Luttmer and Singhal 2014; Mascagni 2018; Slemrod 2019), and it is unclear whether the evidence supporting the motive in individual taxpayers can be necessarily extended to corporate taxpayers (Ariel 2012; OECD 2019; Belnap et al. 2023). Moreover, increased government transparency may backfire and decrease political trust if it exposes the limitations of government or feeds higher expectation and eventually greater disappointment (Cooke et al. 2010; De Fine Licht 2011; Kimball and Patterson 1997). Research also suggest that the transparency-trust link depends on contexts, such national

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<sup>3</sup> These studies do not dilute our contributions because the compliance behavior of corporate taxpayers could differ from individual ones (Hanlon and Heitzman 2010; OECD 2019), and these studies typically use survey data to measure individuals' tax compliance.

cultural values (Grimmelikhuijsen et al. 2013). Finally, it is possible that when firms have better access to government records, they may become more informed regarding how to play with the tax laws and manage to save more taxes (Bac 2001).

To empirically investigate how changes in state FOI laws affect state tax avoidance, we employ the FOI law scores for each state over time compiled by Cordis et al. (2022). They rate the strength of state FOI laws on a scale from one to ten based on ten criteria. Their scores show significant time series variations in the law strength, which allows us to explore the within-state law changes to estimate the causal effect of government transparency on state tax avoidance. Specifically, our identification utilizes improvement of FOI laws in twelve states (e.g., Arkansas in 2002 and Pennsylvania in 2003), as well as the weakening of laws in five states (e.g., Alabama in 2005 and Illinois in 2011).

We measure state tax avoidance inversely with state effective tax rate (ETR) (Dyreng, Lindsey, and Thornock 2013; Shevlin, Thornock, and Williams 2017), as well as adjusted state ETR—state ETR benchmarked against firms with similar size in the same industry (Balakrishnan, Blouin, and Guay 2019; Li, Shevlin, and Zhang 2022).<sup>4</sup> As our setting entails staggered changes of state FOI laws, we implement a difference-in-differences (DiD) design using the stacked-regression approach suggested by Baker, Larcker, and Wang (2022). Further, instead of conducting a simple DiD analysis using the pre-post comparison, we use the changes of FOI law scores as the treatment variable to estimate how the treatment effect varies with the direction and magnitude of law changes (Bourveau, She, and Zaldokas 2020; Cordis et al. 2022).

A firm pays income taxes to a state government if the firm has a substantive economic

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<sup>4</sup> While our hypothesis also applies to other state corporate taxes (e.g., sales taxes), we focus on state income taxes because of data availability. Our state-level analysis in Section 4.8 based on state sales tax revenues is consistent with FOI law improvement also enhancing firms' willingness to pay sales taxes.

footprint (i.e., nexus) in the state. Traditionally, such a nexus was determined by physical presence, but some states now base it on sales and other economic activities (Ma and Thomas 2020). Because a corporate headquarters is sufficient to create a nexus and expose a firm to income taxes in the state (Shevlin et al. 2017; Lee et al. 2022), we identify firms headquartered in the states with FOI law score changes as treatment firms, and firms in the states without FOI changes in the period around our estimation window as control firms.<sup>5</sup> This is a conservative strategy to construct the treatment and control groups because the treatment firms could also pay taxes outside their headquarters states, and the control firms could also pay income taxes in the treatment states, due to the tax nexus. To lower the possibility that the control firms' state taxes could also be affected by the FOI law change of the treatment state, we remove firms that have a significant subsidiary in the treatment state from the control group.

For the sample period 1995–2016, we find that an average firm's state ETR and adjusted state ETR decrease significantly with per unit increase in the FOI law score of its headquarters state. This result suggests that firms reduce state tax avoidance after their headquarters state strengthens the FOI law, and the effect is stronger for greater improvement. To put the economic effect in perspective, if a state without a FOI law (with score 0) passes a FOI in the strongest form (with score 10), the average state ETR of firms headquartered in that state would increase by 0.7 percentage points, which account for 14% of the average state ETR in the sample. Such an effect is economically significant. We further show that the differences in the trends of state ETRs and adjusted state ETRs are similar between the treatment and control firms in the pre-FOI-change

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<sup>5</sup> Baker et al. (2022) suggest using never treated units as the clean control group. However, because all states have some types of FOI laws in the past several decades, we must rely on states that do not have FOI changes in the estimation window as control states. To mitigate the concern that the previously treated firms in the control group could bias our results, we require the control state not have a FOI law change not only in the estimation window, but also in the previous three years.

years, supporting the parallel trends assumption. We find consistent evidence when separately examining states with large improvement in FOI laws (i.e., the FOI law score increases by at least two points) and those with small changes (i.e., one point change in the FOI law score). When a state's FOI law score improves by at least two points, the average state ETR of firms headquartered in that state increases by 0.5 percentage points, which accounts for about 10% of the sample mean of state ETR. In contrast, the effect is insignificant in states with small changes of FOI laws, and the effect is significantly weaker than that in states with large FOI law changes.

We next conduct two cross-sectional tests to support that the effect documented above is due to the enhanced tax morale following state FOI law improvement. First, we explore how the documented effect varies with local civic norms. Prior research argues that managers tend to have a stronger positive prior about the government and tax if the local civic norms emphasize civic responsibilities more (Luttmer and Singhal 2014; Hansan et al. 2017; Chow et al. 2023). When such civic norms are strong, the effect of government transparency on firms' willingness to pay tax through tax morale would be weaker. As expected, we find that the treatment effect is significantly stronger for firms located in the low-civic-norm counties within a state than that of firms in the high-civic-norm counties in that state.<sup>6</sup> Second, we find that the treatment effect resulting from FOI law improvement is stronger in states that were more corrupt in the pre-law-change period. This finding is also consistent with state FOI law improvement increasing state government accountability and thus firms' incentives to pay taxes—the effect is expected to be stronger when the state government is less accountable in the pre-law-change period.<sup>7</sup>

We also examine how the estimated treatment effect varies with a firm's tax exposure to its

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<sup>6</sup> As we partition the sample within states, we are essentially holding constant the change of tax enforcement for the treatment firms in the two subsamples.

<sup>7</sup> This argument only applies to states with FOI law improvement; the prediction would be the opposite for states with weakening laws. Thus, we focus on states with FOI law improvement for this analysis.



headquarters state. As we discuss above, a firm also pays state taxes to other states in which it has substantial business operations. Because our dependent variable is total state ETR (not for a specific state), which is a blended rate of the state tax expense for a firm across all state and subnational jurisdictions where the firm has operations (Shevlin et al. 2017), we expect the treatment effect would be stronger for firms with higher tax exposures to their headquarters state. Measuring a firm's tax exposure to non-headquarters states with the percentage of out-of-state subsidiaries (Ma and Thomas 2020) and firm size (Shevin et al. 2017), we find evidence consistent with our prediction.

To corroborate our firm-level findings and provide additional evidence on how state FOI law improvement affects firms' and residents' incentives to pay taxes in general, we further explore the impact of the law changes on state tax revenues. We find that when a state has a large improvement in FOI laws (i.e., an increase in the FOI law score of at least two points), its corporate income tax revenue, sales tax revenue, individual income tax revenue, total income tax revenue, and total tax revenue all increase significantly relative to control states. These results are not only consistent with our firm-level results, but also suggest that when a state government is more transparent, it collects more taxes in general, including the collection of sales taxes and individual income taxes. These results are consistent with enhanced tax morales due to FOI law improvement for both businesses and individuals.

Our study makes several contributions. First, it contributes to the literature of government transparency, in particular, the line of research on how government transparency affects firm behaviors and outcomes (e.g., Cordis et al. 2022; Hope et al. 2022; He et al. 2023). Our evidence suggests that improving the public's access to government records could improve firms' willingness to pay taxes. Our evidence adds to prior research on the link between government

transparency and political trust (e.g., Tolbert and Mossberger 2006; Cook et al. 2010). While our evidence is based on state government transparency, the implications could be extended to federal government agencies. In addition, our state-level analysis sheds light on how government transparency affects government tax revenues in general.

Second, our study adds to the literature of tax avoidance by identifying one prominent government feature that can shape firm tax avoidance behaviors—government transparency. While a long line of studies is devoted to understanding the determinants of tax avoidance, these studies typically focus on firm and managerial characteristics (Hanlon and Heitzman 2010), despite taxes are a joint outcome of firms’ and governments’ characteristics and decisions. Our study adds to the research that shows the role of the government in corporate tax planning behaviors (e.g., Hoopes et al. 2012; Alm, Martinez-Vazquez, and McClellan 2016), and more importantly, the literature of tax morale (Luttmer and Singhal 2014; Mascagni 2018). Our evidence complements Chow et al. (2023) and provides additional support for the reciprocity view of tax morale, as well as tax morale in general, in the corporate setting (e.g., Hansan et al. 2017; DeBacker et al. 2015).

## **2. Prior Literature and Institutional Background**

### ***2.1 Literature on Government Transparency and Firm Behaviors***

There is a long literature in public administration, political science, and economics on how government transparency affects the functioning of government (e.g., Gavazza and Lizzeri 2009; Benito and Bastida 2009; Cordis and Warren 2014). This literature shows that transparency is effective in achieving certain government outcomes, such as reducing corruption and improving financial management, but it is no cure-all and does not always have positive outcomes (Cucciniello, Porumbescu, and Grimmelhuijsen 2017). However, there is limited evidence on

how government transparency affects corporate behaviors and outcomes.

Hope et al. (2022) show that government transparency is positively associated with firm-level operational efficiency and access to external financing in emerging economies. They attribute the finding to the information role of governments in collecting and disseminating information about local economic conditions. Cordis et al. (2022) show that strengthened state FOI laws lead to lower plant pollutions in the jurisdiction, and the overall level of pollution in a state is negatively associated with total number of FOI requests received by the state's environmental department or agency.

Several studies examine how different aspects of government transparency affect firms' incentives or opportunities to contract with the government. Using the Italian setting, Coviello and Mariniello (2014) find that publicizing a public procurement auction increases the number of bidders for the auctioned project. Using data from India and Indonesia, Lewis-Faupel, Neggers, Olken, and Pande (2016) examine whether electronic procurement (e-procurement), which increases access to information and reduces personal interactions with potentially corrupt officials, improves procurement outcomes. Their evidence suggests that e-procurement facilitates entry from higher quality contractors. He, Li, Li, and Zhang (2024) show that when the U.S. federal government agencies are more likely to grant Freedom of Information Act (FOIA) requests requesting government contractors' proprietary information, firms' incentives to bid for government contracts diminish.

## ***2.2 State Freedom of Information Laws***

Established at both federal and state levels, FOI laws play an important role in underpinning governmental transparency in the United States. These legal provisions grant citizens, organizations, and corporations the right to access a broad spectrum of records retained by the

government. At the federal level, the FOIA, enacted in 1966, allows the public to request any federal agency’s records unless the information falls under one of the nine exemptions (Klein, Li, and Zhang 2020; He et al. 2024). At the state level, each state has enacted its own FOI laws, with different levels of enforcement and exceptions. One such example is the Texas Public Information Act (PIA), originally known as the Texas Open Records Act. Under the Texas PIA, any person, not just Texas residents, has the right to request access to government records, subject to certain exceptions. The law applies to all Texas governmental bodies created by Texas’ executive or legislative branches, including all boards, commissions, and committees. The Texas PIA, similar to other state FOI laws, embodies a commitment to transparency and accountability.

While all states currently have FOI laws, the particulars of the legal stipulations encompassed in the laws exhibit considerable variations across different states and periods. The variations in laws primarily span in dimensions such as exemptions, enforcement measures and sanctions, as well as fees and associated costs (Winkler and Byers 2010). For example, Colorado Public Records Act imposes explicit penalties for public entities for violations (C.R.S. § 24-72-201), whereas under the Alabama Public Records Law, no enforcement procedures are prescribed (Ala. Code § 36-12-40). The Maryland Public Information Act supports the general “public interest exemption” — information custodians are given the discretion to exempt certain parts of records if they believe the disclosure would be against the public interest (Md. Code Ann., State Gov’t §§ 10-611 to 10-628, Section 10-618), while the Florida Public Records Law does not explicitly support such an exemption (Fla. Stat. § 119.01 – 119.15 (1995)).<sup>8</sup> Cordis and Warren (2014) evaluate each state FOI law’s strength based on night criteria and classify the states into strong and weak FOI law states, with roughly equal number of states in each category. Connecticut, Indiana, Louisiana,

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<sup>8</sup> Detailed information about each state’s FOI law can be found at Open Government Guide provided by Reporters Committee for Freedom of the Press (<https://www.rcfp.org/open-government-guide/>).

Colorado, and Vermont are among the strong FOI law states, while South Dakota, Alabama, Arizona, Wyoming, and Nevada are among the weak states.

Over the past fifty decades, the majority of states have introduced fresh FOI laws, modified existing ones, or completely revised their statutes. This movement has primarily been an endeavor to bolster these laws, frequently to define or expand their range in response to advancements in technology, rulings from the judiciary, or opinions expressed by the Attorney General (Cordis and Warren 2014).<sup>9</sup> For instance, in 2009, the South Dakota Legislature enacted a relatively comprehensive FOI law, South Dakota Sunshine Law. The law finally established a clear presumption that government records are deemed open records unless a specific law provides an exemption or exclusion. Another example is Pennsylvania's Right to Know Law, which was first adopted in 1957 and amended extensively in 2002. The 2002 amendments impose civil penalties on agency officials for violations and criminal penalties for violations with the intent. To reduce the cost for a requester, the amendments impose limits on the fees that could be charged and grant a waiver if the disclosure is in the public interest. The amendments also allow for the award of litigation costs to a requester if the court reverses an agency's decision. In addition, government agencies are required to respond to a FOI request within ten business days.

### **3. Hypothesis Development**

In the benchmark economic model of tax compliance developed by Allingham and Sandmo (1972), tax avoidance is determined by tax rates, the detection probability, the level of penalty, and taxpayers' risk-aversion. The literature of tax morale argues that tax compliance may also be affected by nonpecuniary factors, such as intrinsic motivation to pay taxes and social norms (Lutter

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<sup>9</sup> Cordis and Warren (2014) find that during their sample period (1986–2009), 12 states switched from weak to strong FOI laws.

and Singhal 2014; DeBacker, Heim, and Tran 2015; OECD 2019). Lutter and Singhal (2014) review research on tax morale and conclude that tax morale is indeed an important component of tax compliance. They argue that tax morale can be incorporated into Allingham and Sandmo's (1972) economic framework by adding to the taxpayer's utility function an additional utility term related to the nonpecuniary factors.<sup>10</sup>

One mechanism of tax morale is reciprocity, in which the incentive to pay taxes depends on taxpayers' relationship to the government, such as public goods provided by the government, their perceptions of the fairness of the tax system and the legitimacy of the government, and their attitudes toward the government (Luttmer and Singhal 2014). Prior research has presented evidence consistent with the reciprocity view (e.g., Hallsworth et al. 2014; Chow et al. 2023; Giacobasso et al. 2023). Hallsworth et al. (2014) show in the UK setting that late payment of individual taxes decreases in response to reminder letters emphasizing how tax revenue is used to finance public goods. In the corporate setting, Chow et al. (2023) find that corporate tax avoidance increases with ground-level ozone pollution, which they argue can be perceived as an indicator of inefficient use of tax money by the government in providing high quality public goods.

Based on the reciprocity view, we hypothesize that the enhanced state government transparency due to state FIO law improvement can increase taxpayers' trust in the state government and thus enhance their tax morale. This hypothesis is based on the following two important arguments. First, perceived government transparency can increase citizens' trust in the government and thus their tax morale. Government transparency is generally considered one of the fundamental components of government accountability (Rourke 1960; Redford 1969; Alt and Lassen 2006). Transparency not only enables the public to critically examine government

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<sup>10</sup> While Allingham and Sandmo's (1972) theory is for individual tax compliance, it can also be applied to corporate taxpayers (Hanlon and Heitzman 2010).

decisions and hold officials accountable, but also allows the public to actively involve in the democratic process and to shape public policies. Empirical evidence supports that government transparency can bolster accountability (Peisakhin 2012; Cordis and Warren 2014; Duguay, Rauter, and Samuels 2023). For instance, Cordis and Warren (2014) demonstrate that reinforcing state FOI laws reduces corruption among state and local government officials. Thus, government transparency is generally believed to increase citizens' trust in the government (Bok 1997; Cook et al. 2010). Consistent with government transparency enhancing tax morale, research suggests that individuals are more willing to pay taxes when the government budget or spending is more transparent (Alt et al. 2010; Capasso et al. 2021). For example, Capasso et al. (2021) show that a country's fiscal transparency is positively associated with citizens' willingness to pay taxes, measured using the World Values Survey.

The second argument is that managers' tax morale can affect their firms' tax avoidance behaviors.<sup>11</sup> In theory, this is possible under the agency framework of corporate tax avoidance (Slemrod 2004; Crocker and Slemrod 2005; Chen and Chu 2005). Due to the separation of ownership and control, managers make tax decisions based on their own preferences and incentives. Empirical evidence suggests that managers can significantly influence their firms' tax avoidance behavior. Dyreng, Hanlon, and Maydew (2010) find that individual executives are important determinants of corporate tax avoidance; moving between the top and bottom quartiles of executives results in approximately an 11-percent change in GAAP effective tax rate. Hasan et al. (2017) find that the levels of social capital in U.S. counties are systematically related to tax avoidance activities of firms headquartered in the counties. Chow et al. (2023), which we discuss above, provide direct empirical evidence that managers' reciprocal motive can affect their firms

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<sup>11</sup> We acknowledge that tax morale can also affect shareholders' decisions on tax avoidance. However, we contend that tax avoid decisions are primarily made by managers (Hanlon and Heitzman 2010).

tax avoidance behavior.

*Hypothesis: The enhancement in state FOI laws reduces corporate state tax avoidance in that state.*<sup>12</sup>

Our hypothesis is not without tension. There are reasons to believe that enhanced FOI laws might not reduce—and could even escalate—corporate state tax avoidance. Firstly, previous studies have not reached a consensus on the reciprocal motivation behind tax compliance among individual taxpayers (Luttmer and Singhal 2014; Mascagni 2018). For example, Blumenthal et al. (2001) find little evidence that providing information about how tax revenues are spent can improve tax compliance. Second, most research of tax morale focuses on individual taxpayers; the research on tax morale in businesses is limited (OECD 2019). Despite of the evidence in Chow et al. (2023), it is still unclear whether the evidence supporting the reciprocal motive in individual taxpayers can be necessarily extended to corporate taxpayers (OECD 2019). Thirdly, research also suggest that the transparency-trust link depends on contexts, such national cultural values (Grimmelikhuijsen et al. 2013), and increased government transparency may even backfire and decrease political trust if it exposes the limitations of government or feeds higher expectation and eventually greater disappointment (Cooke et al. 2010; De Fine Licht 2011; Kimball and Patterson 1997). Finally, as firms gain easier access to government records, they could become more adept at navigating tax laws, which could possibly result in greater tax avoidance. Bac (2001) posits a related idea, suggesting that increased government transparency can spur corruption by providing potential bribers with better information on whom to approach.

Another potential channel through which a strengthened state FOI law can affect firms' state tax avoidance is that the public (e.g., media and activists) could obtain a firm's tax records through

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<sup>12</sup> While our hypothesis is framed around the improvement of FOI laws for convenience, we similarly predict that weakening such laws will increase state tax avoidance.



FOI law requests and use the information to monitor firms' tax behaviors. This channel hinges on whether the public can access a firm's tax records through the FOI laws. Based on our reading, this channel is unlikely because tax records are generally treated as confidential under state FOI laws and therefore are not subject to FOI law requests. For example, the Pennsylvania government clearly states on its website that "tax records are confidential and may be released only to the taxpayer or pursuant to a release signed by the taxpayer."<sup>13</sup>

One may argue that greater government accountability induced by FOI law improvement could also translate into stricter tax enforcement, which can deter corporate tax avoidance. However, ex ante, it is unclear whether FOI law improvement can necessarily lead to stricter tax enforcement. As state governments aim to stimulate the local economy, increased accountability does not necessarily mean stricter tax collection, given the potentially negative impacts of corporate taxes on investments (Djankov et al 2010).<sup>14</sup> Our cross-sectional analysis in Section 4.6.1 also suggests that our finding of reduced tax avoidance is unlikely to be due to stricter tax enforcement.

## **4. Empirical Analyses**

### ***4.1 Variable Measurement and Research Design***

#### ***4.1.1 Strengths of state FOI laws***

As discussed in Section 2.2, every state has its own FOI laws and these laws exhibit substantial variations across different states and periods. Researchers have developed scoring systems to evaluate the effectiveness of these laws in providing the public with access to

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<sup>13</sup> Source: <https://www.revenue.pa.gov/TaxLawPoliciesBulletinsNotices/Right-to-Know>.

<sup>14</sup> Djankov et al. (2010) find that higher effective corporate tax rates significantly hamper aggregate investment. They posit that more efficient governments can adeptly strike a balance between tax collection and promoting business growth.

government records and improving government transparency (Cordis and Warren 2014; Cordis et al. 2022). We adopt Cordis et al.'s (2022) scoring system which scores the strength of state FOI laws on a scale from one to ten. Specifically, the scoring system gives one point for a state in a year if the state FOI law meets one of the following criteria: (1) has a presumption in favor of disclosure; (2) does not include a generic public interest exemption; (3) limits the fees that could be charged on FOI requests; (4) does not charge fees for the time spent searching and collecting information; (5) waives the cost of searching or copying information if the disclosure is in the public interest; (6) has criminal penalties for a government agency's noncompliance with FOI laws; (7) has civil penalties for a government agency's noncompliance with FOI laws; (8) provides for the award of attorneys' costs to a successful plaintiff in a FOI case; (9) requires the agency to respond to a FOI request within 15 days; and (10) allows for appeal if an agency denies a FOI request. A higher FOI score indicates easier access to government records and higher government transparency. While the theoretical value of the score is between zero and ten, it ranges from one to nine in our sample period.

As explained below in Section 4.1.3, our main research design identifies the changes of state FOI law scores and explores how tax avoidance behaviors respond to such changes. We use Cordis et al.'s (2022) Table IA1 to identify seventeen states with FOI law changes that meet our sample selection requirements described in Section 4.2. These states are listed in Appendix A. Twelve states strengthened the FOI laws, while five make the laws weaker by one point. Among the twelve states that strengthen the laws, nine increase the score by only one point, while three have an increase larger than two points.

#### ***4.1.2 Tax avoidance measures***

We measure state tax avoidance inversely with two ETR measures: state ETR (*SETR*) and

adjusted state ETR (*Adj. SETR*). While a firm's ETR in a certain state should be ideally calculated using the state income tax expense in that state divided by the taxable income in that state, due to data limitation, firms disclose only total state tax expense and total pre-tax domestic (or worldwide or foreign) income. Thus, we follow prior studies to calculate *SETR* as the total state income tax expenses scaled by pre-tax domestic income (e.g., Dyreng, Lindsey, and Thornock 2013; Shevlin, Thornock, and Williams 2017). A lower value of *SETR* suggests greater state tax avoidance. As *SETR* is not well defined below zero or above one, we truncate it at zero and one (e.g., Chen et al, 2010; Chen et al. 2019).

To control for tax planning opportunities, we also follow prior research to benchmark a firm's state tax avoidance relative to that of firms of similar size in the same industry and calculate adjusted state ETR (e.g., Balakrishnan et al. 2019; Li, Shevlin, and Zhang 2022). Specifically, *Adjusted SETR* is defined as a firm's *SETR* minus the average *SETR* in the same industry and size group on a yearly basis. Consistent with Balakrishnan, Blouin, and Guay (2019), industry is defined based on Fama-French 48 industry classification and firm size groups are formed by quintiles of total assets.

#### **4.1.3 Research design**

We exploit changes in state FOI scores over time and design a difference-in-differences test to examine the impact of state FOI laws on corporate state tax avoidance. For each year, we identify state FOI law changes and create a cohort consisting of seven years around the change (three years before and after the change). We classify states with a FOI law change as treatment states and require that they did not experience any other FOI law changes in the previous six years to ensure that the pre-period is not contaminated. We use states that do not experience any FOI changes in the ten-year window (from  $t-6$  to  $t+3$ ) as control states. We exclude states without

corporate income taxes from both the treatment and control states.<sup>15</sup>

To illustrate the cohort approach, consider the year 2002, in which Arkansas, New Jersey, and West Virginia experienced FOI law changes. After ensuring that these three states did not experience any FOI law changes from 1996 to 2001, we classify them as treatment states for the cohort of year 2002. Then, we classify other states that did not experience any FOI law changes in the period 1996–2005 as control states. Finally, we keep years 1999–2005 for the treatment and control states as the cohort of year 2002. We create cohorts for the other years with state FOI law changes in a similar way.

A firm pays income taxes to a state government if the firm has a substantive economic footprint (i.e., nexus) in the state. Prior research argues that a corporate headquarters is sufficient to create a nexus and expose the firm to income taxes in the state (Shevlin et al. 2017; Lee et al. 2022). Thus, we identify firms headquartered in the states with FOI law score changes as treatment firms, and firms in the states without FOI changes in the period around our estimation window as control firms.<sup>16</sup> We note that this way of selecting treatment and control groups is conservative, because while traditionally the nexus was based on physical presence, some states now determine it based on sales and other economic activities (Ma and Thomas 2020). Thus, the treatment firms could also pay taxes in other states, and the control firms could also pay income taxes in the treatment states, due to sales and other economic activities. To lower the possibility that the control firms' state taxes could also be affected by the FOI law change of the treatment state, we remove firms that have a significant subsidiary in the treatment states from the control group.

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<sup>15</sup> These states include Nevada, Ohio, South Dakota, Texas, Washington, and Wyoming.

<sup>16</sup> Baker et al. (2022) suggest to use never treated units as the clean control group. However, because all states have some types of FOI laws in the past several decades, we have to rely on states that do not have FOI changes in the estimation window as control states. To mitigate the concern that the previously treated firms in the control group could bias our results, we require the control state not have a FOI law change not only in the estimation window, but also in the previous three years.

We stack all cohorts and estimate the following OLS model:

$$State\ ETR = \alpha + \beta \times \Delta FOI + \gamma \times Controls + Cohort-Firm\ FE + Cohort-Year\ FE + \varepsilon, \quad (1)$$

where  $\Delta FOI$  equals the FOI law score change for treatment firms in the change year and the subsequent three years and zero otherwise. Consider Pennsylvania, whose FOI score increases from one to eight in year 2003, as an example. We code  $\Delta FOI$  as seven for firms headquartered in Pennsylvania from 2003 to 2006, and zero for all other observations in the cohort (including observations of control firms).<sup>17</sup> The coefficient  $\beta$  therefore estimates the effect of one-unit change in state FOI law score on the dependent variable. The dependent variable, *State ETR*, is one of the two state tax avoidance measures: *SETR* and *Adj SETR*. For each cohort, we include firm fixed effects to absorb time-invariable firm characteristics and year fixed effects to absorb time-varying economy-wide shocks to all firms. We interact these fixed effects with the cohort indicators to make the fixed effects cohort-specific (Barrios 2021; Baker et al. 2022). As the FOI law changes occur at the state level, we cluster standard errors by state across all cohorts.

Our research design is essentially the stacked-regression approach, recommended by Baker et al. (2022), to estimate causal effects using a difference-in-differences (DiD) design with staggered events, except that we use  $\Delta FOI$  as the treatment variable, not an indicator variable. With the change indicator replaced by  $\Delta FOI$ , we essentially estimate the variation in the estimated DiD treatment effect based on the degree of changes. Cordis et al. (2022) employ a similar approach to study the impact of FOI law changes on corporate pollution.

*Controls* refers to a battery of firm-level and state-level control variables. Following prior literature (e.g., Shevlin et al. 2017; Chen et al. 2019; Lee et al. 2022), we control for firm characteristics that could affect corporate tax planning, including firm size (*Size*), market-to-book

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<sup>17</sup> As we have only information of the change year, we assume the change occurs in the midpoint of the calendar year and code all fiscal years ended in or after June of that year as the post-change period.

ratio (*MTB*), profitability (*ROA*), leverage (*LEV*), the existence (*NOL Dummy*) and the change (*NOL Change*) of net operating loss carry-forward, foreign income (*FORINC*), capital intensity (*PPE*), intangible assets (*Intangible*), and equity income (*EQINC*). We also control for state characteristics that may impact corporate tax avoidance strategies, including state macroeconomic variables and tax-related state statues. The macroeconomic variables include state GDP growth (*GDP*) and unemployment rate (*Unemp*). The tax statues include state corporate income tax rate (*CIT*), the weight of sales factors in apportionment formulas (*SalesFac*), and indicator variables for the passage of throwback legislations (*Thrbk*), combined reporting requirements (*Combined*), and addback statues (*Addback*) (e.g., Lee et al. 2022; Ma and Thomas 2020; Dyreng et al. 2013). Appendix B provides detailed definitions of these variables.

#### **4.2 Data and Summary Statistics**

Our sample period is from 1995 to 2016, which starts three years before the FOI law change of New Hampshire in 1998 and ends three years after the FOI law changes of Delaware in 2013. We obtain accounting information from Compustat. After requiring the availability of the ETR measures and control variables, our final sample consists of 66,564 firm-year observations, among which 94% are control firms.<sup>18</sup>

Table 1 reports summary statistics for the sample firms. The average *SETR* is 5.1%. By construction, the mean of *Adj SETR* is close to zero. The mean of  $\Delta FOI$  is 0.054. The small mean is primarily due to the low fraction of treatment firms, as well as states with negative  $\Delta FOI$ . Conditional on  $\Delta FOI$  being positive, its mean is 3.2. When  $\Delta FOI$  is negative, its value is always -1. The average firm has a leverage ratio of 16.0% and *ROA* of 9.0%. The average market capitalization is 3.9 billion dollars. The average state corporate income tax rate is 7.6%. The

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<sup>18</sup> The sample could include some control firm-years multiple times, because a control firm-year could be included in multiple cohorts. When duplicates are removed, the sample consists of 25,974 firm-year observations.

average weight placed on sales in a state’s apportionment formula in the calculation of state taxes is 57.7%. 51.9% of states have passed throwback legislation, and 42.5% and 26.5% have passed combined reporting and addback legislations, respectively.

### **4.3 Baseline Results**

Table 3 reports the OLS regression results of estimating equation (1). Columns 1-2 and 3-4 present the results for *SETR* and *Adj SETR*, respectively. In column 1, we exclude control variables to mitigate the concern that some control variables may also be affected by the FOI law changes. The estimated coefficient of  $\Delta FOI$  is 0.0006 and significant ( $t$ -stat = 3.81). This result suggests a firm reduces state tax avoidance after its headquarters state strengthens the FOI law. In column 2, we include all control variables. The coefficient of  $\Delta FOI$  continues to be positive and significant, and the magnitude is comparable to that in column 1, 0.0007 vs. 0.0006. The coefficient in the full model in column 2 suggests that when a state strengthens its FOI law by one point in Cordis et al.’s (2022) scoring system, the average state ETR of firms headquartered in that state increases by 0.07% relative to the control firms. To put the economic effect in perspective, if a state without a FOI law passes a FOI in the strongest form (with score 10), the average state ETR of firms headquartered in that state would increase by 0.7 percentage points, which account for 14% of the average state ETR in the sample. Such an effect is economically significant.

The results based on *Adj ETR* reported in columns 3 and 4 are consistent with those in columns 1 and 2. The estimated coefficient of  $\Delta FOI$  in columns 3 and 4 are the same as the corresponding one in columns 1 and 2, respectively, and both are statistically significant. The economic magnitude of the coefficient in the full model in column 4 can be interpreted in the same manner as for column 2: if a state without a FOI law passes a FOI in the strongest form, the average

adjusted state ETR of firms headquartered in that state would increase by 0.7 percentage points, which account for 18% of the standard deviation of adjusted state ETR in the sample.<sup>19</sup>

The effects of the control variables are generally consistent with prior studies (e.g., Chen et al. 2019; Lee et al. 2022). We find that state ETRs are higher for less profitable (*ROA*) and growth (*MTB*) firms and firms with higher foreign income (*FORINC*). Among the five state tax statute characteristics, only *SalesFac* loads significantly — it is negatively related to the state ETR, whereas the coefficient becomes marginally insignificant when the dependent variable is adjusted state ETR. The generally insignificant effects of tax statute variables are expected because we use firm fixed effects for each cohort. If there is no change in a state tax statute variable, its effect would be absorbed by the firm fixed effects of that cohort.

#### ***4.4 Tests of Parallel Trends Assumption***

As explained in Section 4.2, our research design in Table 2 is essentially a DiD with staggered events, except that we use  $\Delta FOI$  as the treatment variable, not an indicator variable. An important assumption for the validity of a DiD is the parallel trends assumption. We provide evidence on the validity of the assumption by exploring the difference in the trends of the dependent variables between the treatment and control firms in the years right before the law change. Specifically, we create a series of dummy variables—*Pre(-2)*, *Pre(-1)*, *Post(0)*, *Post(+1)*, *Post(+2)*, and *Post(+3)*—to indicate the individual year relative to the FOI law change year (i.e., year 0), and estimate the following regression:<sup>20</sup>

$$\begin{aligned} \text{State ETR} = & \alpha + \beta_1 \times \text{Pre}(-2) + \beta_2 \times \text{Pre}(-1) + \beta_3 \times \text{Post}(0) \times \Delta FOI + \beta_4 \times \text{Post}(+1) \times \Delta FOI \\ & + \beta_5 \times \text{Post}(+2) \times \Delta FOI + \beta_6 \times \text{Post}(+3) \times \Delta FOI + \gamma \times \text{Controls} \\ & + \text{Cohort-Firm FE} + \text{Cohort-Year FE} + \varepsilon. \end{aligned} \quad (1)$$

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<sup>19</sup> As the mean of *Adj ETR* is close to zero by design, we use the standard deviation as a benchmark to evaluate the economic effect.

<sup>20</sup> A similar design is used to test the parallel trends assumption in Bourveau et al. (2020) and Cordis et al. (2022).



The estimation results are reported in Table 3. The four columns correspond to the four models in Table 2. The coefficients of  $Pre(-2)$  and  $Pre(-1)$  are insignificant in all regressions, suggest that the difference in the trends of state ETRs and adjusted state ETRs are similar between the treatment and control firms in the years right before the law change. This result supports the parallel trends assumption. The coefficient of  $Post(0) \times \Delta FOI$  is insignificant in all regressions, while the coefficients of  $Post(+1) \times \Delta FOI$ ,  $Post(+2) \times \Delta FOI$ , and  $Post(+3) \times \Delta FOI$  are all significantly positive except for that of  $Post(+3) \times \Delta FOI$  in column 3, which is positive but insignificant. Thus, the treatment effect first takes place in the first year after the FOI law change and persists into the second and third years.

#### ***4.5 Separate Analysis for Large vs. Small FOI Law Changes***

As a robustness test, we explore an alternative design by separately estimating the DiD treatment effect for states with large vs. small FOI law changes and compare the effects. Specifically, we replace  $\Delta FOI$  in equation (1) with an indicator variable  $Post$ , which flags the post-change years for treatment firms. If the FOI law change is +1 or -1, we classify it as a small change; all the other changes are classified as large changes. This approach differs from the one in Table 2 in that we do not impose a linear structure on how the treatment effect changes with the degree of FOI law changes. It also allows us to explore when an FOI law change would be large enough to lead to a significant treatment effect.

In the sample period, changes in FOI law with a magnitude of at least two points all relate to law improvements. These states include Arkansas (from 6 to 8 in 2002), New Jersey (from 2 to 7 in 2002), and Pennsylvania (from 1 to 8 in 2003). Panel A of Table 4 reports the treatment effect of such large improvements. The coefficient of  $Post$  is 0.005 and significant in both models with and without control variables (columns 1 and 2). This result suggests that when a state's FOI law

score improves by at least two points, the average state ETR of firms headquartered in that state increase by 0.5 percentage points, which account for about 10% of the sample mean of state ETR. The results based on adjusted state ETR in columns 3 and 4 lead to a similar conclusion.

Nine states improve FOI law scores by one point in our sample period: Connecticut in 2009, Delaware in 2013, Hawaii in 2000, Maine in 2005, Minnesota in 2001, Minnesota in 2011, Missouri in 2004, Nebraska in 2001, West Virginia in 2002. Panel B of Table 4 report the treatment effect of such small improvements. The coefficient of *Post* is insignificant in all regressions, suggesting that a small improvement in FOI laws does not change firms' state tax avoidance significantly. Moreover, the difference between the corresponding coefficients of *Post* in Panels A and B is significant for all model specifications. Thus, when the magnitude of FOI law improvement is larger, its effect on firms' state tax avoidance is also stronger, consistent with the finding in Table 2. Panel C of Table 4 report the treatment effect of small FOI law score decreases ( $\Delta FOI = -1$ ). Five states decrease the FOI law score by one: Alabama in 2005, Illinois in 2011, Iowa in 2012, New Hampshire in 1998, and New York in 2009. Consistent with the result in Panel B, the treatment effect is insignificant in all models.<sup>21</sup> However, the difference between the negative of each coefficient of *Post* in Panel C and the corresponding coefficient in Panel A is significant only for the models of state ETRs (columns 1 and 2).<sup>22</sup>

#### **4.6 Cross-sectional Tests**

To provide evidence that our finding of reduced tax avoidance is due to enhanced tax morale, we explore how the documented effect varies with local civic norms. If the local civic norms emphasize civic responsibilities, managers tend to have a stronger positive prior about the

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<sup>21</sup> This finding does not suggest that weakening state FOI laws fails to increase corporate state tax avoidance, as the magnitudes of the law changes are small in the treatment states with weakened laws.

<sup>22</sup> We take the negative of each coefficient in Pane C because the coefficient represents the effect of one point *decrease* in the FOI law score.

government and tax, which can weaken managers' reciprocity-driven tax payment incentives (Luttmer and Singhal 2014; Chow et al. 2023). Consistent with this notion, Chow et al. (2023) find that the positive effect of ozone pollution on corporate tax avoidance is weaker in counties with such civic norms, measured a higher census response rate or election turnout. Accordingly, we predict that if tax morale drives our finding, the result should be weaker in counties with stronger civic norms that emphasize on civic responsibilities.

Following Hasan et al. (2017), we measure civic norms as the first principal component of election turnout and Census response rate in a firm's headquarter county (*Civic Norm*). We then partition the sample of each cohort into two subsamples based on the median of *Civic Norm* in the pre-law-change year in each state. By partitioning the sample with a state, we essentially hold the change of tax enforcement constant for the high- and low-civic norm groups. As the civic norm data from the Northeast Regional Center for Rural Development (NRCRD) are only available in 1990, 1997, 2005, 2009, and 2014, we follow prior studies to use the most recent data for each event year (e.g., Hansan et al. 2017).

Panel A of Table 5 reports the results of estimating the effect of  $\Delta FOI$  separately for each subsample. For both *SETR* and *Adj. SETR*, we find consistent results that the treatment effect is significantly positive in the subsample of low *Civic Norm*, 0.0012 ( $t$ -stat = 3.37) and 0.0010 ( $t$ -stat = 3.08) respectively, but insignificant in the subsample of high *Civic Norm*, -0.0001 ( $t$ -stat = -0.25) and 0.0001 ( $t$ -stat = 0.13) respectively, and the difference is significant.<sup>23</sup> For each dependent variable, the magnitude of the treatment effect in the high-civic-norm subsample is larger than that in the full sample in Table 3, 0.0012 vs. 0.0007 for *SETR* and 0.0010 vs. 0.0007 for *Adj. SETR*.

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<sup>23</sup> The insignificant effect in the high-civic-norm subsample suggests that the treatment effect becomes insignificant when the tax morale mechanism is muted. Thus, our main finding is unlikely to be driven by stricter tax enforcement following FOI law improvement. Note that as we partition the sample within states, we are essentially holding constant the change of tax enforcement, if any, for the treatment firms in the two subsamples.

The results are consistent with enhanced tax morale is being the primary mechanism of the effect we document in Table 3. Panel B reports the results based on the treatment states with large FOI improvement ( $\Delta FOI > 1$ ). The results are fairly consistent with those in Panel B and lead to the same inference.

As Hasan et al. (2017) document that civic norms emphasizing civic cooperativeness lead to less tax avoidance, one may be concerned that the stronger treatment effect in the low-civic-form subsample in Table 5 could be due to their lower ETR levels in the pre-law-change period—firms avoiding more taxes in the pre-law-change period have larger room to reduce tax avoidance following the state FOI law improvement. We note that this argument only applies to states with FOI law improvement; the prediction would be the opposite for states with weakening laws. Nevertheless, to mitigate this concern, we compare the average *SETR* (*Adj. SETR*) in the pre-law-change period years between the treatment firms in the high- and low-civic-norm subsamples. We find no significant difference for either variable.<sup>24</sup> In an untabulated analysis, we regress *SETR* (*Adj. ETR*) on the indicator variable for the low-civic-norm subsample, the control variables in equation (1), and industry and year fixed effects using the pre-law-change observations of treatment firms. The coefficient for the indicator variable is insignificant for both dependent variables: 0.0004 ( $t$ -stat = 0.08) for *SETR* and -0.0017 ( $t$ -stat = -0.34) for *Adj. SETR*. Thus, treatment firms in the low-civic-form subsample do not have lower state ETR levels in the pre-law-change period, mitigating the above concern.<sup>25</sup>

To further mitigate this concern, we explore whether the treatment effect is indeed stronger for firms that avoid more taxes in the pre-law-change period. Specifically, we calculate each firm's

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<sup>24</sup> The average pre-law-change *SETR* (*Adj. SETR*) is 0.0489 (0.0002) for treatment firms in the high-civic-norm subsample, compared to 0.0479 (-0.0014) for those in the low-civic-norm subsample.

<sup>25</sup> Our finding does not contradict Hasan et al's (2017) because our comparison is based on the pre-treatment years of treatment firms and we focus on state tax avoidance.

average *Adj. SETR* in the pre-law-change years in each cohort and classify firms in the bottom half in the headquarters state as aggressive firms. The untabulated results suggest that the treatment effect tends to be weaker, not stronger, for firms that avoid more taxes in the pre-law-change period.

#### ***4.6.2 State corruption in the pre-law-change period***

Our central argument is that state FOI law improvement can increase state government accountability, which enhances tax morale and lead to less state tax avoidance by firms. The effect on tax avoidance is expected to be stronger when the state government is more corrupt and thus less accountable in the pre-law-change period. To test this prediction, we follow prior research (e.g., Cuny et al. 2020; Parsons et al. 2018) to measure corruption using the number of corruption convictions by Department of Justice (DOJ) in each state scaled by the number of full-time government employees, averaged over the pre-event period (*Corruption*). Data on convictions are collected from DOJ's annual "Report to Congress on the Activities and Operations of Public Integrity Section," and data on the number of government employees are from Census Bureau's Annual Survey of Public Employment & Payroll.

Since the prediction applies to only the cases of FOI law improvement, we exclude cohorts of treatment states with weakening laws. As *Corruption* is measured at the state level, we partition the sample by state. Specifically, among the 12 treatment states strengthening FOI laws, we first classify the top 6 states with a higher value of *Corruption* as high-corruption states, and the remaining states as low-corruption states. Then, in each cohort, if all treatments states are in the high (low) group, all control states are also classified into the high (low) group. This is the case for all cohorts except cohort 2001, for which the treatments states are split between the high and low groups. For this cohort, we also split the control states into the high and low groups based on

the median of *Corruption* of all control states in that cohort. We separately estimate the treatment effect of  $\Delta FOI$  for each subsample and report the results in Table 6. For both *SETR* and *Adj. SETR*, the treatment effect is significantly positive in both the high- and low-corruption subsamples. However, consistent with our prediction, the effect is significantly stronger in the high-corruption subsample, 0.0019 vs. 0.0005 for *SETR* and 0.0016 vs. 0.0004 for *Adj. SETR*.

#### **4.6.3 Tax exposure to the headquarters state**

As we discuss in Section 4.1.3, a firm pays income taxes to a state government if it has a substantive economic footprint (i.e., nexus) in the state. Our analyses above assume that a firm has a tax nexus in its headquarters state, so that the FOI law improvement in its headquarters state can impact the firm's state tax avoidance in that state. While this assumption is reasonable, considering that our dependent variable is total state ETR, which is a blended rate of the state tax expense for a firm across all state and subnational jurisdictions where the firm has operations (Shevlin et al. 2017), we expect the treatment effect would be stronger for firms with higher tax exposures to their headquarters state. In this case, the tax morale towards the headquarters state government would have a stronger impact on the firm's state ETR.

To test this prediction, we first follow Ma and Thomas (2020) and measure a firm's out-of-state tax exposure with the number of out-of-state subsidiaries. Specifically, we calculate the percentage of a firm's out-of-state subsidiaries, averaged over the pre-law-change period. This variable is denoted as *OutStatSub*. It is coded as zero for a firm without out-of-state subsidiaries.<sup>26</sup> For each cohort, we partition the sample based on the median of *OutStatSub* within a state. The results reported in Panel A of Table 7 indicate that the treatment effect is significantly stronger in the subsample of firms with high *OutStateSub*.

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<sup>26</sup> 46% of firm-year observations in our sample do not have out-of-state subsidiaries.

Prior studies also use firm size as a proxy for a firm's tax exposure to the headquarters state (Shevlin et al. 2017; Ma and Thomas 2020). The idea is that the more concentrated a firm's activity is within its headquarters state, proxied with small firm size, the more likely that the total state ETR is representative of the firm's overall state tax burden in its headquarters state (Shevlin et al. 2017). Thus, we examine how the treatment effect we document varies with firm size, expecting the effect to be stronger for smaller firms.<sup>27</sup> For each cohort, we partition the sample based on the median of firm size within a state. Panel B of Table 7 reports that the treatment effect is significantly stronger for smaller firms. Overall, the results in Table 7 are consistent with our expectation that the treatment effect is stronger for firms with greater tax exposure to their headquarters states.

#### ***4.8 State-level Analyses***

To corroborate our firm-level findings and provide additional evidence on how state FOI law improvement affects state governments' tax collection efforts, we further examine how state FOI law changes affect state tax revenues (e.g., Lee et al. 2022). If firms headquartered in the state reduces tax avoidance after the state FOI law improvement, we expect to observe increases in total corporate income tax revenues, total income tax revenues, and total tax revenues for the state government. The state-level analysis also helps address a concerns in our firm-level analysis—as firms may also pay taxes to non-headquarters-states, state ETR is not a precise measure of tax morale toward the headquarters state and the estimated treatment effect may not be completely attributed to the FOI law changes in the treatment states. For this purpose, we also examine

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<sup>27</sup> This analysis could be confounded by the possibility that firms with different sizes could face different public monitoring regarding tax issues and have different degrees of political connections, which could affect their state tax avoidance status in the pre-law-change period and thus the strength of the treatment effect. However, the direction of such effects is ambiguous a priori. On one hand, large firms could be subject to more intensive public monitoring and avoid fewer taxes. On the other hand, they are more likely to have political connections, which could help with their tax avoidance. Empirically, in Table 2, firm size is not significantly related to state ETR, which mitigates this concern to some extent.

individual income tax revenues and sales tax revenues. If tax payers become more willing to pay taxes in general following the FOI law improvement, we would also observe an increase in these two tax revenues.

We collect the tax revenue data for each state-year from the United States Census Bureau's Annual Survey of State Government Tax Collections. We use a similar design as in the firm-level analysis, for both the full sample and the sample of treatment states with large FOI law improvement, using the natural logarithm of each type of tax revenues as the dependent variable. These variables are denoted as *CorpIncTax*, *IndIncTax*, *TotalIncTax*, *SaleTax*, and *TotalTax*, respectively, for corporate income taxes, individual income taxes, total income taxes, sales taxes, and total taxes. We use each state-year as a regression unit, and accordingly replace cohort-firm fixed effects with cohort-state fixed effects and include all state-level control variables from equation (1), as well as state population (*Population*), personal income tax rate (*PersonTaxRate*), and sales tax rate (*SalesTaxRate*) as the control variables.

Table 8 reports the results of this analysis. Panels A and B present the results for the full sample and the sample of treatment states with large FOI law improvement, respectively. We use the same set of control variables for all types of tax revenues to account for the possibility that the state government may strategically plan for collection efforts across different types of taxes.<sup>28</sup> In Panel A, the coefficient of  $\Delta FOI$  is positive in all columns and it is significant when the dependent variable is total income tax revenue and total tax revenue. In Panel B, the coefficient of *Post* is significantly positive in all columns. These results, especially those in Panel B, are not only consistent with our firm-level results in Section 4.5, but also suggest that when a state government becomes more transparent, it strengthens tax collection in general, including the collection of sales

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<sup>28</sup> For example, the government may be more lenient in collection of individual income taxes when it is able to collect more corporate income taxes.



taxes and individual income taxes. To put the economic effect in perspective, the coefficients in Panel B indicate that corporate income tax (individual income tax, total income tax, sales tax, total tax) revenues increase by 15.3% (6.8%, 7.8%, 5.9%, 5.7%) following a large improvement in FOI laws.

## **5. Conclusion**

We study whether and how government transparency affects corporate tax behaviors using the setting of staggered state FOI law changes, which change the public's access to state and local government records. Using a difference-in-differences design, we show that that increased government transparency due to the FOI law improvement leads to less state income tax avoidance by firms located in the government's jurisdiction, and the effect is stronger for greater improvement in the FOI law. Cross-sectionally, the documented effect is weaker for firms headquartered in counties with stronger civic norms emphasizing civic responsibility, and is stronger for firms located in more corrupt states and firms with greater tax exposure in their headquarters states. Moreover, consistent with the firm-level findings, we document that the FOI law improvement leads to significant increases in the state government's revenues from corporate income taxes, sales taxes, and individual income taxes, as well as total income tax revenues and total tax revenues. Our findings are consistent with the notion that firms are more willing to pay taxes when they perceive the government to be more transparent and thus more trustable.

Our study makes a significant contribution to the field of government transparency, especially within the context of research exploring how government transparency impacts firm behavior and outcomes. Our findings suggest that enhancing public access to government records can improve corporate tax compliance. Our research also contributes to the tax literature by highlighting government transparency as a key factor influencing corporate tax avoidance

behaviors. In this respect, we add to the tax morale literature and build on existing research that underscores the role of government in shaping corporate tax planning strategies.

Government transparency broadly refers to the extent to which information about various government activities is accessible to the public. In our study, we focus on the specific dimension of public access to state and local government records under FOI laws. However, this is only one facet of government transparency. Future research could delve into other aspects of government transparency and investigate related corporate outcomes, such as workplace safety and the implications for shareholder value.

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## Appendix A: Treatment States with FOI Law Changes

In the table below, we list the treatment states with FOI law changes included in our analysis, based on the year of change. We identify these states from Table IA1 of Cordis et al. (2022) after imposing our sample selection requirements.

<b>State</b>	<b>Year of Change</b>	<b>Change in FOI Law Score</b>
New Hampshire	1998	-1 (6 to 5)
Hawaii	2000	1 (4 to 5)
Minnesota	2001	1 (4 to 5)
Nebraska	2001	1 (5 to 6)
Arkansas	2002	2 (6 to 8)
New Jersey	2002	5 (2 to 7)
West Virginia	2002	1 (5 to 6)
Pennsylvania	2003	7 (1 to 8)
Missouri	2004	1 (6 to 7)
Alabama	2005	-1 (2 to 1)
Maine	2005	1 (5 to 6)
Connecticut	2009	1 (8 to 9)
New York	2009	-1 (7 to 6)
Illinois	2011	-1 (7 to 6)
Minnesota	2011	1 (5 to 6)
Iowa	2012	-1 (5 to 4)
Delaware	2013	1 (5 to 6)

## Appendix B: Variable Definitions

Variable	Definition
<i>Addback</i>	An indicator variable that equals one if the state has passed addback legislations, and zero otherwise.
<i>Adj. FETR</i>	Industry-size adjusted federal ETRs, defined as a firm's <i>FETR</i> adjusted for the average <i>FETR</i> in the same industry and size group on a yearly basis. Following Balakrishnan, Blouin, and Guay (2019), industry is defined based on Fama-French 48 industry classification and firm size groups are formed by quintiles of total assets.
<i>Adj. SETR</i>	Industry-size adjusted state ETRs, defined as a firm's <i>SETR</i> adjusted for the average <i>SETR</i> in the same industry and size group on a yearly basis. Following Balakrishnan, Blouin, and Guay (2019), industry is defined based on Fama-French 48 industry classification and firm size groups are formed by quintiles of total assets.
<i>CIT</i>	State corporate income tax rate.
<i>Civic Norm</i>	The first principal component from a factor analysis based on Census response rate and election turnout in a firm's headquarter county. As the Northeast Regional Center for Rural Development (NRCRD) only collects data in years 1990, 1997, 2005, 2009, and 2014, we use the most recent data for each event year.
<i>Combined</i>	An indicator variable that equals one if the state has passed combined reporting legislations, and zero otherwise.
<i>CorpIncTax</i>	The natural logarithm of corporate income tax revenues collected by the state government.
<i>Corruption</i>	The number of public officials convicted of corruption by Department of Justice in a state, scaled by the number of full-time-equivalent government employees, averaged over the pre-event period.
<i>EQINC</i>	Equity income in earnings (ESUB) scaled by lagged total assets.
<i>FETR</i>	Federal income tax expense (TXFED + TXDFED) scaled by pre-tax domestic income (PIDOM) and truncated at 0 and 1. We require pre-tax domestic income to be positive. PIDOM is calculated as pretax total income (PI) minus pretax foreign income (PIFO) if missing. PIFO is set to be zero if missing.
<i>FOI</i>	State FOI score developed by Cordis, Hsu, and Zhang (2022)
<i>FORINC</i>	Foreign income (FIFO) scaled by lagged total assets.
<i>GDP</i>	State GDP growth rate.
<i>IndIncTax</i>	The natural logarithm of individual income tax revenues collected by the state government.
<i>Intangible</i>	Intangible assets (INTANG) scaled by total assets.
<i>LEV</i>	Financial leverage measured as long-term debt (DLTT) scaled by total assets.
<i>MTB</i>	Market-to-book ratio measured as market value of equity (PRCC_F×CSHO) scaled by book value of equity (CEQ).
<i>NOL Change</i>	The change in net operating loss carry forward (TLCF) scaled by lagged total assets.



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<i>NOL Dummy</i>	An indicator variable that equals one if a firm's net operating loss carry forward (TLCF) is positive as of the beginning of the year, and zero otherwise.
<i>OutStateSub</i>	The percentage of out-of-state subsidiaries of a firm, averaged over the pre-law-change period. If a firm does not disclose any out-of-state subsidiary in Exhibit 21 of its 10-K, the variable is coded as zero.
<i>PersonTaxRate</i>	State personal income tax rate.
<i>Population</i>	The natural logarithm of population in a state.
<i>Post(+1)</i>	An indicator variable that equals one for a firm headquartered in the treatment state that experience FOI changes one year prior, and zero otherwise.
<i>Post(+2)</i>	An indicator variable that equals one for a firm headquartered in the treatment state that experience FOI changes two years prior, and zero otherwise.
<i>Post(+3)</i>	An indicator variable that equals one for a firm headquartered in the treatment state that experience FOI changes three years prior, and zero otherwise.
<i>Post(0)</i>	An indicator variable that equals one for a firm headquartered in the treatment state that experience FOI changes in that same year, and zero otherwise.
<i>PPE</i>	Capital intensity, measured as net property, plant, and equipment (PPENT) scaled by total assets (AT).
<i>Pre(-1)</i>	An indicator variable that equals one for a firm headquartered in the treatment state that experience FOI changes one year afterward, and zero otherwise.
<i>Pre(-2)</i>	An indicator variable that equals one for a firm headquartered in the treatment state that experience FOI changes two years afterward, and zero otherwise.
<i>ROA</i>	Return on assets measured as income before extraordinary items scaled by lagged total assets.
<i>SalesFac</i>	The weight of sales in percentage in a given state's apportionment formula in a given year.
<i>SaleTax</i>	The natural logarithm of total sales tax revenues collected by the state government.
<i>SaleTaxRate</i>	State sales tax rate.
<i>SETR</i>	State income tax expense (TXS + TXDS) scaled by pre-tax domestic income (PIDOM) and truncated at 0 and 1. We require pre-tax domestic income to be positive. PIDOM is calculated as pretax total income (PI) minus pretax foreign income (PIFO) if missing. PIFO is set to be zero if missing.
<i>Size</i>	The natural logarithm of the market value.
<i>Thrbwk</i>	An indicator variable that equals one if the state has passed throwback legislations, and zero otherwise.
<i>TotalIncTax</i>	The natural logarithm of total income tax revenues collected by the state government.
<i>TotalTax</i>	The natural logarithm of total tax revenues collected by the state government.
<i>Unemp</i>	State unemployment rate.
$\Delta FOI$	A variable that equals the value of <i>FOI</i> change in the year of state FOI law change and three years after the change for firms headquartered in the state, and zero otherwise.

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**Table 1. Summary Statistics**

	N	Mean	Q1	Median	Q3	STD
<i>SETR</i>	66,564	0.051	0.026	0.046	0.067	0.042
<i>Adj. SETR</i>	66,564	0.002	-0.021	-0.002	0.018	0.040
<i>ΔFOI</i>	66,564	0.054	0.000	0.000	0.000	0.602
<i>ΔFOI (ΔFOI&gt;0)</i>	1,396	3.163	1.000	1.000	5.000	2.613
<i>ΔFOI (ΔFOI&lt;0)</i>	835	-1.000	-1.000	-1.000	-1.000	-1.000
<i>Market Cap (in millions)</i>	66,564	3,881.674	147.477	596.256	2,072.363	12,448.373
<i>Size</i>	66,564	6.366	4.994	6.391	7.636	1.974
<i>MTB</i>	66,564	3.070	1.377	2.135	3.548	3.372
<i>ROA</i>	66,564	0.090	0.039	0.071	0.118	0.074
<i>LEV</i>	66,564	0.160	0.001	0.112	0.269	0.172
<i>NOL Dummy</i>	66,564	0.280	0.000	0.000	1.000	0.449
<i>NOL Change</i>	66,564	-0.002	0.000	0.000	0.000	0.030
<i>FORINC</i>	66,564	0.012	0.000	0.000	0.008	0.029
<i>PPE</i>	66,564	0.253	0.070	0.178	0.370	0.232
<i>Intangible</i>	66,564	0.138	0.000	0.056	0.216	0.178
<i>EQINC</i>	66,564	0.001	0.000	0.000	0.000	0.005
<i>GDP</i>	66,564	5.182	3.481	5.073	6.739	2.913
<i>Unemp</i>	66,564	5.677	4.500	5.300	6.400	1.967
<i>CIT</i>	66,564	7.639	6.250	7.900	8.840	1.546
<i>SalesFac</i>	66,564	57.656	50.000	50.000	50.000	20.182
<i>Thrbk</i>	66,564	0.519	0.000	1.000	1.000	0.500
<i>Combined</i>	66,564	0.425	0.000	0.000	1.000	0.494
<i>Addback</i>	66,564	0.265	0.000	0.000	1.000	0.441

The table reports summary statistics for the sample. The sample period is from 1995 to 2016. Variable definitions are in Appendix B.

**Table 2. Effect of FOI Law Changes on Corporate State Tax Avoidance**

	(1)	(2)	(3)	(4)
	<i>SETR</i>		<i>Adj. SETR</i>	
<i>ΔFOI</i>	0.0006*** (3.81)	0.0007** (2.49)	0.0006*** (3.37)	0.0007*** (2.72)
<i>Size</i>		-0.0008 (-1.27)		-0.0008 (-1.47)
<i>MTB</i>		0.0003** (2.52)		0.0002 (1.48)
<i>ROA</i>		-0.1189*** (-15.87)		-0.1004*** (-13.54)
<i>LEV</i>		0.0036 (1.20)		0.0028 (0.81)
<i>NOL Dummy</i>		0.0005 (0.44)		0.0002 (0.19)
<i>NOL Change</i>		0.0046 (0.33)		0.0035 (0.35)
<i>FORINC</i>		0.2113*** (6.80)		0.1968*** (6.72)
<i>PPE</i>		-0.0072 (-1.51)		-0.0038 (-0.68)
<i>Intangible</i>		0.0038 (0.49)		0.0071 (1.03)
<i>EQINC</i>		-0.1145 (-1.38)		-0.0886 (-1.03)
<i>GDP</i>		0.0001 (0.42)		-0.0000 (-0.38)
<i>Unemp</i>		0.0002 (0.29)		0.0002 (0.37)
<i>CIT</i>		0.0012 (1.57)		0.0010 (1.12)
<i>SalesFac</i>		-0.0001* (-1.70)		-0.0001 (-1.60)
<i>Thrbk</i>		-0.0004 (-0.18)		-0.0000 (-0.02)
<i>Combined</i>		-0.0023 (-1.12)		-0.0017 (-0.81)
<i>Addback</i>		0.0002 (0.22)		0.0005 (0.42)
<i>Intercept</i>	0.0512*** (5,770.18)	0.0593*** (7.03)	0.0017*** (171.39)	0.0097 (1.12)
Cohort-Firm FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	66,564	66,564	66,564	66,564
Adj. R-squared	0.428	0.446	0.361	0.376

This table reports OLS regression results of the effect of state FOI law changes on corporate state tax avoidance. The sample period is from 1995 to 2016. Variable definitions are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.

**Table 3. Timing of the Effect of FOI Law Changes on Corporate State Tax Avoidance**

	(1)	(2)	(3)	(4)
	<i>SETR</i>		<i>Adj. SETR</i>	
<i>Pre(-2)</i>	-0.0026 (-1.12)	-0.0016 (-0.69)	-0.0015 (-0.75)	-0.0005 (-0.27)
<i>Pre(-1)</i>	-0.0011 (-0.80)	-0.0002 (-0.13)	-0.0006 (-0.50)	0.0003 (0.31)
<i>Post(0) × ΔFOI</i>	0.0000 (0.14)	0.0002 (0.66)	0.0002 (0.56)	0.0003 (1.12)
<i>Post(+1) × ΔFOI</i>	0.0007*** (3.05)	0.0009*** (2.94)	0.0007*** (3.07)	0.0009*** (3.18)
<i>Post(+2) × ΔFOI</i>	0.0005** (2.19)	0.0006* (1.97)	0.0005* (1.77)	0.0007* (1.88)
<i>Post(+3) × ΔFOI</i>	0.0006* (1.99)	0.0007* (1.89)	0.0006 (1.48)	0.0007* (1.68)
Controls	N	Y	N	Y
Cohort-Firm FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	66,564	66,564	66,564	66,564
Adj. R-squared	0.428	0.446	0.361	0.376

This table reports OLS regression results of examining the timing of the effect of state FOI law changes on corporate state tax avoidance. The sample period is from 1995 to 2016. Variable definitions are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.

**Table 4. Separate Tests for Large versus Small FOI Law Changes**

<b>Panel A: Treatment States with <math>\Delta FOI &gt; 1</math></b>				
	(1)	(2)	(3)	(4)
	<i>SETR</i>		<i>Adj. SETR</i>	
<i>Post</i>	0.0050*** (4.36)	0.0048*** (2.77)	0.0039*** (3.22)	0.0040** (2.04)
Controls	N	Y	N	Y
Cohort-Firm FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	12,628	12,628	12,628	12,628
Adj. R-squared	0.429	0.447	0.345	0.359
<b>Panel B: Treatment States with <math>\Delta FOI = 1</math></b>				
	(1)	(2)	(3)	(4)
	<i>SETR</i>		<i>Adj. SETR</i>	
<i>Post</i>	-0.0001 (-0.06)	-0.0001 (-0.10)	0.0003 (0.28)	0.0002 (0.16)
<i>p</i> -value for diff. with coef. in Panel A	0.001	0.018	0.023	0.079
Controls	N	Y	N	Y
Cohort-Firm FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	43,735	43,735	43,735	43,735
Adj. R-squared	0.417	0.435	0.342	0.357
<b>Panel C: Treatment States with <math>\Delta FOI = -1</math></b>				
	(1)	(2)	(3)	(4)
	<i>SETR</i>		<i>Adj. SETR</i>	
<i>Post</i>	0.0020 (0.83)	0.0017 (1.11)	-0.0004 (-0.17)	-0.0008 (-0.72)
<i>p</i> -value for whether the negative of the coef. is different from that in Panel A	0.009	0.006	0.152	0.160
Controls	N	Y	N	Y
Cohort-Firm FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	30,234	30,234	30,234	30,234
Adj. R-squared	0.433	0.451	0.349	0.364

This table reports OLS regression results of separately examining the effect of state FOI law changes on corporate state tax avoidance for large versus small FOI changes using a simple difference-in-differences design. Panel A reports the results for large FOI law changes ( $\Delta FOI > 1$ ). The sample period is from 1999 to 2006. Panels B and C report the results for small FOI law changes,  $\Delta FOI = 1$  and  $\Delta FOI = -1$ , respectively. The sample period is from 1997 to 2016 for Panel B and from 1995 to 2015 for Panel C. Variable definitions

are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.

**Table 5 Cross-sectional Test Based on Pre-law-change Local Civic Norms**

<b>Panel A: Full Sample</b>				
Partitioning Variable	<i>Civic Norm</i>			
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Dependent Variable	<i>SETR</i>		<i>Adj. SETR</i>	
<i>AFOI</i>	0.0012*** (3.37)	-0.0001 (-0.25)	0.0010*** (3.08)	0.0001 (0.13)
<i>p</i> -value for Low vs. High	0.039		0.093	
Control Variables	Y	Y	Y	Y
Cohort-State FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	37,711	26,553	37,711	26,553
Adj. R-squared	0.458	0.431	0.386	0.363
<b>Panel B: Treatment States with <i>AFOI</i>&gt;1</b>				
Partitioning Variable	<i>Civic Norm</i>			
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Dependent Variable	<i>SETR</i>		<i>Adj. SETR</i>	
<i>Post</i>	0.0076*** (3.52)	-0.0025 (-1.15)	0.0060** (2.87)	-0.0005 (-0.16)
<i>p</i> -value for Low vs. High	0.002		0.086	
Control Variables	Y	Y	Y	Y
Cohort-State FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	7,245	4,906	7,245	4,906
Adj. R-squared	0.456	0.431	0.379	0.359

This table reports OLS regression results of examining how the effect of state FOI law changes on corporate state tax avoidance changes with local civic norms emphasizing civic cooperativeness in the pre-law-change period. The sample period is from 1995 to 2016 for Panel A and from 1999 to 2006 for Panel B. Variable definitions are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.



**Table 6 Cross-sectional Test Based on Pre-law-change Corruption Level**

Partitioning Variable	<i>Corruption</i>			
	High	Low	High	Low
	(1)	(2)	(3)	(4)
Dependent Variable	<i>SETR</i>		<i>Adj. SETR</i>	
<i>ΔFOI</i>	0.0019*** (4.24)	0.0005** (2.44)	0.0016*** (2.85)	0.0004** (2.20)
<i>p</i> -value for Low vs. High		0.006		0.046
Control Variables	Y	Y	Y	Y
Cohort-State FE	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y
N	21,442	29,186	21,442	29,186
Adj. R-squared	0.425	0.449	0.348	0.382

This table reports OLS regression results of examining how the effect of state FOI law changes on corporate state tax avoidance changes with state government corruption level in the pre-law-change period. The sample period is from 1997 to 2016. Variable definitions are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.

**Table 7 Cross-sectional Test Based on Tax Exposure to Headquarters State**

**Panel A: Out-of-state Subsidiaries**

Partitioning Var	<i>OutStateSub</i>							
	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var	<i>SETR</i>				<i>Adj. SETR</i>			
<i>ΔFOI</i>	0.0013*** (3.43)	-0.0002 (-0.49)	0.0010*** (3.37)	0.0001 (0.31)				
<i>Post</i>					0.0100*** (5.51)	-0.0007 (-0.31)	0.0079*** (3.20)	0.0009 (0.33)
<i>p</i> -value for Low vs. High	0.006		0.061		0.001		0.051	
Control Variables	Y	Y	Y	Y	Y	Y	Y	Y
Cohort-State FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	33,103	33,461	33,103	33,461	5,724	6,904	5,724	6,904
Adj. R-squared	0.465	0.429	0.388	0.366	0.476	0.425	0.388	0.359
Sample	Full Sample				Treatment States with <i>ΔFOI</i> >1			

**Panel B: Firm Size**

Partitioning Var	Firm Size							
	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var	<i>SETR</i>		<i>Adj. SETR</i>		<i>SETR</i>		<i>Adj. SETR</i>	
<i>ΔFOI</i>	0.0016** (2.41)	-0.0000 (-0.04)	0.0014** (2.28)	0.0001 (0.24)				
<i>Post</i>					0.0100*** (3.20)	0.0010 (0.36)	0.0082*** (2.19)	0.0018 (0.80)
<i>p</i> -value for Low vs. High		0.044		0.088		0.032		0.148
Control Variables	Y	Y	Y	Y	Y	Y	Y	Y
Cohort-State FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	32,204	34,360	32,204	34,360	6,140	6,488	6,140	6,488
Adj. R-squared	0.456	0.426	0.387	0.364	0.455	0.430	0.377	0.363
Sample	Full Sample				Treatment States with $\Delta FOI > 1$			

This table reports OLS regression results of examining how the effect of state FOI law changes on corporate state tax avoidance changes with firms' tax exposures to the headquarters states. In Panel A, state tax exposure is measured inversely with the percentage of out-of-state subsidiaries. In Panel B, state tax exposure is measured inversely with firm size. The sample period is from 1995 to 2016 for columns 1–4 of each panel, and is from 1999 to 2006 for columns 5–8 of each panel. Variable definitions are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.

**Table 8 Effect of FOI Law Changes on Tax Revenues of State Government**

<b>Panel A: Full Sample</b>					
	(1)	(2)	(3)	(4)	(5)
	<i>CorpIncTax</i>	<i>IndIncTax</i>	<i>TotalIncTax</i>	<i>SaleTax</i>	<i>TotalTax</i>
<i>ΔFOI</i>	0.017 (0.88)	0.006 (1.13)	0.009* (1.77)	0.008 (1.48)	0.009** (2.34)
<i>Population</i>	0.017** (2.20)	0.007*** (3.81)	0.007*** (3.91)	0.005*** (3.14)	0.008** (2.15)
<i>GDP</i>	-0.019 (-1.14)	-0.022*** (-4.22)	-0.021*** (-3.32)	-0.006 (-1.16)	-0.015*** (-3.16)
<i>Unemp</i>	0.027 (0.74)	0.002 (0.12)	0.024*** (2.85)	-0.022 (-1.60)	-0.010 (-0.60)
<i>CIT</i>	0.127* (1.74)	-0.008 (-0.48)	0.017 (0.57)	0.026* (1.86)	0.032 (1.63)
<i>SalesFac</i>	-0.188** (-2.03)	0.022 (0.68)	-0.017 (-0.46)	-0.013 (-0.40)	0.023 (0.67)
<i>Thrbwk</i>	0.036 (0.76)	-0.002 (-0.13)	0.012 (0.78)	0.038** (2.15)	0.005 (0.39)
<i>Combined</i>	-0.023 (-0.97)	0.007 (0.43)	0.008 (0.55)	-0.005 (-0.59)	-0.009 (-0.71)
<i>Addback</i>	0.019 (0.41)	-0.003 (-0.16)	0.009 (0.47)	0.080*** (3.30)	0.039*** (3.23)
<i>PersonTaxRate</i>	-3.714 (-0.39)	2.030 (0.36)	-3.674 (-0.70)	-0.805 (-0.13)	-1.657 (-0.34)
<i>SalesTaxRate</i>	0.017 (0.88)	0.006 (1.13)	0.009* (1.77)	0.008 (1.48)	0.009** (2.34)
<i>Intercept</i>	1.093* (1.73)	0.847** (2.33)	1.217*** (3.51)	1.012** (2.50)	1.158*** (3.56)
Cohort-State FE	Y	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y	Y
N	2,625	2,471	2,625	2,310	2,625
Adj. R-squared	0.964	0.997	0.996	0.996	0.993

  

<b>Panel B: Treatment States with <math>\Delta FOI &gt; 1</math></b>					
	(1)	(2)	(3)	(4)	(5)
	<i>CorpIncTax</i>	<i>IndIncTax</i>	<i>TotalIncTax</i>	<i>SaleTax</i>	<i>TotalTax</i>
<i>Post</i>	0.142** (2.26)	0.066** (2.63)	0.075*** (3.26)	0.057** (2.35)	0.055*** (3.25)
Control Variables	Y	Y	Y	Y	Y
Cohort-State FE	Y	Y	Y	Y	Y
Cohort-Year FE	Y	Y	Y	Y	Y
N	448	420	448	392	448
Adj. R-squared	0.970	0.998	0.997	0.996	0.998

This table reports OLS regression results of the effect of state FOI law changes on the tax revenues of the state government. Panel A reports the results for the full sample; Panel B reports the results for the sample of treatment states with the change of FOI law score strictly greater than one. The sample period is from 1995 to 2016 for Panel A, and from 1999 to 2006 for Panel B. The sample size of column 2 (4) of each panel is smaller due to the exclusion of states that do not impose individual income taxes (sales taxes). Variable definitions are in Appendix B. Robust standard errors are clustered by state. *t*-statistics are reported in parentheses. \*\*\*, \*\* and \* indicate significance at the 0.01, 0.05, and 0.10 two-tailed levels, respectively.