Whistleblowing Threat and Corporate Disclosure*

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1. Introduction

Regulators worldwide are increasingly using whistleblower programs to detect and deter corporate misconduct (Dasgupta and Kesharwani 2010; Dey et al. 2021). In the United States, at least 29 states have enacted false claims acts (FCAs) designed to foster whistleblowing. Prior studies find that external whistleblowers, such as the media, customers, competitors, and regulators, play an important role in whistleblowing (Dyck et al. 2010).[1] As external whistleblowers potentially rely on a firm's public disclosure for information, a natural, unanswered question is how the threat of whistleblowing affects firms' incentives to provide disclosure to the public. We fill the void in the literature by providing evidence on this question.

The threat of a whistleblower can impose significant costs on firms. The threat exists not only for firms with misbehaviors but also for firms without misconduct. Misbehaving firms identified by a whistleblower usually have to pay a nontrivial amount to settle their FCA liabilities.[2] Even firms without violations may still suffer from the risk of being falsely accused by a whistleblower and having to deal with a frivolous accusation (Miceli and Near 1992; Howse and Daniels 1995; Gobert and Punch 2000; Levmore and Porat 2012).[3] Thus, when outsiders' whistleblowing incentives are heightened, managers may have less incentive to provide public disclosures that could facilitate the detection of their firms' misbehaviors or attract frivolous accusations.

[1] Dyck et al. (2010) find that 46% of the whistleblowers of corporate misconduct are people external to the firms.
[2] For example, in 2009 Pfizer agreed to pay $1 billion to settle U.S. False Claims Act (FCA) liabilities, with a $102 million reward going to the whistleblowers. Bank of America once paid $16.65 billion for selling fraudulently labelled mortgage products to government-insured mortgage companies, of which $170 million was paid to the whistleblowers. Source: https://www.qui-tam-attorney.com/10-largest-qui-tam-whistleblower-rewards.html.
[3] Frivolous whistleblowing claims are not uncommon. For instance, a recent government survey suggests that more than 98% whistleblowing cases received by regulators are baseless allegations.
Source: https://www.osha.gov/sites/default/files/Data-and-Statistics-for-FY16-to-FY21.pdf.

We use the staggered adoptions of state FCAs to identify exogenous increases in firms' risk of being whistleblown, and examine how managers' disclosure behaviors change after the adoptions (e.g., Lee et al. 2019; Berger and Lee 2022). The state FCA adoptions provide an ideal setting to study the research question for three reasons. First, state FCA laws offer financial rewards and protection to whistleblowers who expose fraud against the state government, thereby increasing the whistleblowing risk for firms operating in the adopting states. State FCAs impact not only firms with explicit contracts (e.g., procurement, lease, or loan contracts) with the state government but also firms with implicit contractual obligations to the government, such as taxes. For instance, a firm is obligated to provide revenue to the government through state taxes and would violate the state FCA if it fails to fulfill its tax obligations.[4] Second, the state FCA adoptions are plausibly exogenous to firms' disclosure decisions, which allows us to provide causal evidence on how an increase in the whistleblowing threat impacts disclosure. Third, the state FCA adoptions are staggered over time. This staggered occurrence of events mitigates the risk of confounding events and thus offers better identification than a single event (e.g., the Sarbanes-Oxley Act, the Dodd-Frank Act).

An important consideration in examining the impact of whistleblowing laws on managers' disclosure incentives is that the disclosed information should be useful for a potential whistleblower. We argue that information about a firm's economic links with other firms, such as supply-chain information, could be useful to potential whistleblowers. Firms do not operate in a vacuum but rather in a nexus of contractual relationships with other firms (e.g., customers and

[4] An important usage of the state general FCA has been to fight state tax evasions. For example, in a recent FCA litigation case in New York, a whistleblower successfully showed that Bristol Myers Squibb, a New York-headquartered firm, was avoiding New York state taxes. The case ended up being settled for a $6.2 million fine, with the whistleblower receiving $1.1 million of the total $6.2 million payment (source: https://ag.ny.gov/press-release/2014/ag-schneiderman-announces-62-million-settlementwith-lantheus-medical-imaging).

suppliers) and economic agents (e.g., employees) (Jensen and Meckling 1976). Thus, relevant information, including confidential information, about a firm's business activities, including its misconduct, may be possessed by or reflected in their economically connected firms, such as customer firms (e.g., Cohen and Frazzini 2008; Cen et al. 2017). When a firm's economic links are revealed to the public, potential whistleblowers could "delve into" the firm by seeking relevant public information, and more importantly, acquiring relevant confidential information through those connected firms, thus increasing the firm's likelihood of being accused by a whistleblower (e.g., Li et al. 2022).

For the reasons discussed above, we focus on firms' disclosure choices about the identities of their customers (e.g., Ellis et al. 2012; Li et al. 2018).[5] The customer identities can be useful to whistleblowers in the context of state FCAs for several reasons. First, disclosing customer identities to the public essentially opens a window for potential external whistleblowers to *privately* acquire useful information through the customers. It is well recognized in the literature that customers may possess confidential information about their suppliers because of their business relationship with the suppliers (e.g., Baiman and Rajan 2002; Li and Zhang 2008; Kong et al. 2013; Cen et al. 2017). Therefore, exposing customers' identities to the public potentially increases the risk of confidential information useful to whistleblowers leaking through their customers.

Second, *publicly* available customer information may contain useful information for whistleblowers to detect FCA violations. For instance, knowing customer identities would make it easier to detect the miscalculation of sales tax or other FCA violations related to state taxes. This

[5] While disclosure of a firm's economic link with its other trade partners (such as suppliers) may also be useful for external whistleblowers, we focus on customers because of data availability. Although Regulation S-K mandates disclosure of major customer identities, in practice around 40% of major customer identities are not disclosed, probably due to proprietary cost concern (Ellis et al. 2012; Li et al. 2018) or SEC's loose enforcement (Li et al. 2022). Thus, major customer identity disclosure, while mandated, has a significant voluntary component, and has been used in the literature as a voluntary disclosure setting to test theories of voluntary disclosure (e.g., Ellis et al. 2012; Li et al. 2018).

notion is supported by Li et al. (2022), who show that customers' financial information can be used to detect the supplier's revenue manipulation because customers' purchase information is useful in inferring the supplier's sales.

Finally, customer information is particularly useful in detecting violations by state government contractors because the price a contractor charges to non-government customers can be used to evaluate whether the government has been overcharged. For example, in October 2006, Oracle paid $98.5 million to settle an FCA whistleblower lawsuit in which the U.S. Justice Department claimed Oracle overcharged the government by tens of millions of dollars. The complaint alleges "various schemes Oracle used to give commercial customers deeper discounts than the GSA schedule provided" (U.S. v. Oracle Corp., 1:07-cv-00529).[6]

We predict that a firm is more likely to hide the identities of its customers after its headquarters state adopts an FCA. When outsiders' whistleblowing incentives are heightened after the state FCA adoption, it is costlier for managers to disclose customers' identities to the public because potential external whistleblowers, such as the media, competitors, and industry regulators, may attempt to acquire information through or about the disclosed customers and use this information to detect and prove the firm's FCA violations.

We use the staggered adoption of general state FCAs (i.e., FCAs that cover any kind of fraud committed against the state) to estimate a generalized difference-in-differences (DiD) model for the sample period 1994–2010 (e.g., Bertrand and Mullainathan 2003; Armstrong et al. 2012; Li et al. 2018).[7] Consistent with firms withholding more information after an increase in

[6] While the whistleblower of this case is not an external whistleblower, this case illustrates the particular usefulness of confidential information possessed by corporate customers (e.g., their contract price) in FCA violation detections. External whistleblowers may potentially acquire such information though a corporate customer.

[7] We start our sample in 1994 due to the availability of headquarters state data (e.g., Li et al. 2018), and end the sample in 2010 to avoid the confounding effect of the Dodd-Frank whistleblower provision, which was adopted in 2011.

whistleblowing threat, we find that firms significantly decrease their disclosure of customer identities following the adoption of a general state FCA. The effect is not only statistically significant but also economically meaningful. The passage of a general state FCA is associated with a 9.3% decrease in customer identity disclosure. The validity of our DiD design relies on the assumption that absent the FCA adoptions, firms located in FCA states exhibit similar disclosure trends to firms located in non-FCA states. To provide evidence on this assumption, we estimate a dynamic model. The model shows that the trend of the difference in customer identity disclosures for treatment and control firms prior to the FCA adoptions is stable, which bolsters the validity of our inferences.

We next examine how the changes in firms' customer identity disclosure vary cross-sectionally depending on firms' incentives to minimize the whistleblowing risk. As the primary purpose of reducing public disclosure is to prevent whistleblowing, we first examine how the likelihood of attracting an external whistleblower moderates the documented effect. Consistent with outsiders such as competitors, the media, and industry regulators playing an important role in a whistleblowing event (Dyck et al. 2010), we find that the nondisclosure effect is stronger for firms more likely to face wishtleblowing threats from competitors, journalists, and industry regulators (i.e., firms operating in more competitive industries, firms covered by more local media, and firms whose industries are more regulated).[8] In our second cross-sectional test, we examine how the firms' degree of exposure to the government moderates the documented effect. We find that the effect is stronger in firms that have governments as a customer. This finding is consistent

[8] While proprietary costs are an important determinant of disclosure decisions (e.g., Huang et al. 2017; Li et al. 2018), our cross-sectional results based on industry competition are not due to the proprietary costs, because it is unlikely that the proprietary costs could change with state FCA adoptions.

with the notion that firms with more business exposure to the state government are more concerned about violating state FCAs.

Because our staggered DiD design uses previously treated observations as controls for subsequently treated observations, it could be subject to a potential bias (Baker et al. 2022; Barrios 2021). To allay this concern, we follow Baker et al's (2022) suggestions to conduct a battery of robustness tests, including 1) estimating stacked regressions, 2) deleting early treated observations, and 3) removing all covariates. Across all tests, we find results consistent with our main test results. Next, we conduct two falsification tests. First, we assess whether firms change their disclosure practices following the passage of state FCAs that target only Medicaid fraud. We expect these FCAs to have a weaker effect or no effect on customer identity disclosure, and our results are consistent with this prediction. Second, we predict that the adoption of general state FCAs will not affect managers' incentives to issue another type of disclosure—management forecasts—because the forecasted information (e.g., earnings) will soon be realized and available and thus is not useful in detecting FCA violations. Our evidence is consistent with this prediction.

One could be concerned that the decrease in customer identity disclosure might be attributed to changes in firms' customer bases instead of a reporting choice. To explore this possibility, we examine whether firms' customer bases change after state FCA adoptions and find no significant results.[9] This finding minimizes the aforementioned concern.[10] In addition, consistent with whistleblowing threat imposing costs on both well-behaving firms and fraud firms, we find that firms with different misbehaving likelihoods reduce disclosure similarly after state

[9] Specifically, we test whether there are any changes in 1) the number of customers that a firm sells to, and 2) a firm's total sales to its major customers, after state general FCA adoptions, finding no significant effects.

[10] The result of no significant change in customer base is also consistent with the notion that firms do not seem to adjust their real relationship with customers in response to the heightened whistleblowing threat. This is understandable because changing customer base could be very costly.

FCA adoptions. We also conduct several robustness tests to rule out the possibility that our main results are driven by a particular subset of states, such as those that explicitly identify state tax violations in the coverage of state FCA.

In additional analyses, we explore the effect of the federal-level whistleblower program from the Dodd-Frank Act to corroborate our findings from the state FCA adoptions.[11] Using firms located in states that have not passed a general state FCA as the treatment group and firms located in states that enacted general FCAs before Dodd-Frank as the control group (Berger and Lee 2022), we find modest evidence that the treatment firms reduce their customer identity disclosure after Dodd-Frank. We also explore whether state FCA adoptions have a similar effect on firms' disclosure of their subsidiaries, which, like customer information disclosure, reveals the firms' economic links with other firms. We find that firms do reduce their subsidiary identity disclosure following the state FCA adoptions. These results are consistent with our argument that firms strategically withhold information to minimize whistleblowing risk.

Our study contributes to the literature on the economic impact of whistleblowing on corporate behavior. The extant literature focuses on the disciplining effect of whistleblowing laws and shows that firms with higher whistleblowing risk reduce misbehavior such as financial misreporting (e.g., Wiedman and Zhu 2020; Berger and Lee 2022) and aggressive tax behavior (e.g., Wilde 2017; Amir et al. 2018; Lee et al. 2019). Taking a new perspective, our study provides evidence that firms with higher whistleblowing risk are also more careful about what they disclose to the public. Our study complements the extant literature by providing evidence that firms facing

[11] One of the key provisions of the Dodd-Frank Act was its creation of new whistleblower programs. We caveat, though, that Dodd-Frank introduced changes in several different aspects of the economy. For example, the act increased the liability of Credit Rating Agencies (CRAs) for issuing inaccurate ratings and made it easier for the SEC to impose sanctions on CRAs for material misstatements and fraud (Dimitrov et al. 2015). The change in CRA oversight could affect firms' voluntary disclosure practices (Basu et al. 2022), which may bias against our finding any results.

heightened whistleblowing threat are not only more careful about what they do, but also more careful about what they say. Both dimensions—what to say and what to do —are important first-order considerations for firms facing heightened whistleblowing threat.[12] Our findings and the extant literature collectively suggest that firms take steps to minimize the risk of being whistleblown.

Our study also adds to the disclosure literature by showing an additional cost of disclosure — by potentially attracting external whistleblowers' attention, a firm's public disclosure may impose substantial costs on the firm. To the best of our knowledge, we are the first to provide evidence that the whistleblowing threat could lead to an additional disclosure cost. In addition, our study has potential policy implications, as U.S. regulators increasingly rely on whistleblowers to detect corporate misconduct. Both federal and state legislatures have passed more whistleblower protection and incentive laws. Our study points to an unintended consequence of providing stronger whistleblowing incentives.

The remainder of this paper proceeds as follows. Section 2 reviews prior studies and describes the empirical setting. Section 3 develops our hypothesis. Section 4 describes the research design and sample and presents the main empirical analyses. Section 5 provides additional analyses. Section 6 concludes.

2. Prior Literature and Empirical Setting

2.1 Prior Studies

[12] When a firm is closely monitored by external parties for misconduct for whistleblowing purpose, the natural response is to reduce misconduct and/or hide information that could help with misconduct detection. Thus, "what to do" and "what to say" could both be first-order considerations. However, when to take one strategy versus the other depends on the tradeoff of respective costs and benefits and thus is unclear ex ante. Prior studies focus on "what to do" (e.g., Berger and Lee 2022), while our study provides evidence on "what to say."

Prior studies have shown that whistleblowers play an important role in discovering corporate misconduct and enforcing the penalties for it (e.g., Bowen et al. 2010; Call et al. 2018). Analyzing 81 employee whistleblowing allegations related to financial misconduct between 1989 and 2004, Bowen et al. (2010) provide evidence that whistleblowing is an important mechanism for discovering information about agency problems at firms. Using a data set of employee whistleblowing allegations obtained from the U.S. government and the enforcement actions of financial misrepresentation, Call et al. (2018) find that whistleblower involvement in enforcement actions is associated with higher monetary penalties for targeted firms and employees and with longer prison sentences for culpable executives. Their evidence suggests that whistleblowers are an important source of information for regulators regarding financial misconduct.

Whistleblower programs generally have two prominent features—financial rewards to whistleblowers and protection from retaliations (Dye et al. 2021; Heese and Pérez-Cavazos 2021). Both features are important in motivating whistleblowers. Analyzing FCA whistleblower lawsuits filed with district courts, Dye et al. (2021) provide evidence that raising financial incentives for whistleblowing increases the number of lawsuits filed with the regulator. Using large increases in unemployment insurance benefits as a proxy for shocks to retaliation costs, Heese and Perez-Cavazos (2021) find reducing retaliation costs increases the number of employee whistleblowing cases.

What is particularly relevant to our study is whether external whistleblowers are active in whistleblowing. Dyck et al. (2010) examine all reported corporate fraud cases in large U.S. firms between 1996 and 2004 to determine which actors are involved in the revelation of the fraud. They find that fraud detection does not rely on traditional corporate governance actors, such as investors, the SEC, and auditors, but rather on a variety of nontraditional players, including both corporate

insiders (e.g., employees) and outsiders (e.g., journalists, short sellers, competitors, industry regulators). Outsiders appear to blow the whistle nearly as often as insiders: Dyck et al. (2010) report that 46% of the whistleblowers of corporate misconduct are people who are external to the firm, such as competitors, customers, and analysts.

Our study relates to the literature on the impact of the whistleblowing threat on corporate behavior. Prior research shows that the whistleblowing threat plays an important role in deterring corporate misbehavior such as financial misreporting and tax aggressiveness, and that an increase in the whistleblowing threat motivates firms to reduce their misconduct (e.g., Wilde 2017; Wiedman and Zhu 2020; Berger and Lee 2022). Using a sample of employee whistleblower cases obtained from the U.S. government, Wilde (2017) documents that firms that are subject to whistleblowing allegations exhibit significant decreases in tax aggressiveness and financial misreporting. Berger and Lee (2022) find that exposure to the whistleblower provision in the Dodd-Frank Act reduces the treatment firms' likelihood of accounting fraud by 17%, relative to control firms. Using the staggered passage of state FCAs in the five states that explicitly mention state tax violations as a natural experiment, Lee et al. (2019) find that the state FCA passage reduces state tax avoidance. Our study adds to this line of research by providing evidence that the threat of whistleblowing motivates firms to reduce their disclosure of information that could be useful in detecting corporate misbehavior.

2.2 The State False Claim Acts Adoption Setting

We use states' FCA adoptions to identify the change in the whistleblowing threat (e.g., Lee et al. 2019; Berger and Lee 2022). The state FCA laws originate in and are modeled after the federal FCA which imposes liability on persons and companies defrauding the federal government. The federal FCA was enacted as early as 1863 with its original purpose to target fraudulent acts

by a supplier to the Union Army during the Civil War. Since its passage, the federal FCA has been amended several times, aiming to include whistleblower provisions to make it easier for the government and whistleblowers to file a suit (Rapp 2012). Its 1986 amendment introduced a qui tam provision that allows whistleblowers to file actions on behalf of the government and receive a portion of any recovered damages. The Federal FCA has been an important tool to combat fraud against the federal government (Engstrom 2013; Heese and Pérez-Cavazos 2019). It has helped the federal government recover billions of dollars from frauds since 1986. Over the period of 1987–2016, 16,187 FCA cases were filed. In fiscal year 2019 alone, the U.S. Department of Justice recovered over $3 billion from the federal FCA cases.[13]

The success of the federal FCA has inspired many states to use similar legislations to deter and detect the fraud against the state government. Since 1987, 28 states plus the District of Columbia have adopted their own FCA laws (refer to Table 1 for the timeline of state FCA adoptions).[14] Similar to the federal FCA, state FCAs offer financial rewards to whistleblowers who expose fraud against a government. All state FCAs include an anti-retaliation provision modeled after the federal law, which protects whistleblowers against explicit and de facto retaliation (Lee et al. 2019). While the state FCA laws are modeled after the federal FCA, their coverages vary. Many state FCAs encourage whistleblowers to file cases regarding any kind of fraud committed against the state; we refer to these as "general FCAs" (e.g., California, North Carolina, Virginia, and Tennessee). Other state FCAs cover only healthcare or Medicaid fraud; we refer to these as "Medicaid FCAs" (e.g., Colorado, Georgia, Maryland, and Michigan).[15] Some state FCAs cover

[13] Source: https://www.justice.gov/opa/pr/justice-department-recovers-over-3-billion-false-claims-act-cases-fiscal-year-2019.
[14] Source: https://pricebenowitz.com/false-claims-act-lawyer/state/.
[15] In our main sample we focus on state "general FCAs" and exclude firms in states that adopt only Medicaid FCAs.

"political subdivisions" such as a city or town within the state (e.g., California and Nevada); others do not (e.g., Iowa and Indiana).

State FCA adoptions offer an ideal setting to identify the change in the threat of whistleblowing for three reasons. First, state FCAs provide financial rewards and protections to whistleblowers to encourage whistleblowing, which increases the threat of whistleblowing for firms in the state (Dye et al. 2021; Heese and Perez-Cavazos 2021). State FCAs impact not only firms that have explicit contracts with the state government (e.g., procurement, lease, or loan contracts), but also impact firms that lack such contracts yet have implicit contractual obligations to the government. For example, firms are obligated to pay state tax and thus implicitly contract with the state government because of their tax obligations. As firms' sales tax and income tax payments are important sources of state government revenue, accounting for about 25% of the total, an important use of the state general FCA has been to fight state tax evasion.[16] Some state FCAs (e.g., Delaware and Florida) even explicitly encourage whistleblowing of tax violations against the state (Lee et al. 2019).[17]

Second, the state FCA adoptions are normally driven by reasons unrelated to firms' disclosure decisions. The state FCAs are in general adopted through the political process, which is beyond the control of any individual firms. Thus, the adoptions are plausibly exogenous to firms' decisions. Third, the state FCA adoptions are staggered over time. This staggered occurrence of events offers better identification than a single event (e.g., the Sarbanes-Oxley Act, the Dodd-Frank Act) and helps allay concerns about confounding events.

3. Hypothesis Development

[16] Source: https://www.nasra.org/revenue.
[17] Lee et al. (2019) study the effect of FCA adoptions on tax avoidance and discuss, in detail, several cases where FCA laws were used in litigation. See footnote 4 for an example.

3.1 Whistleblowing Threat and Importance of External Whistleblowers

As we discuss in Section 2, whistleblowers play an important role in detecting corporate misconduct and exposing it to the public (Bowen et al. 2010; Dey et al. 2021). What is particularly relevant to our study is whether external whistleblowers are active in whistleblowing. While internal whistleblowers like employees may still use information from a firm's public disclosure to complement their insider information, external whistleblowers are likely to rely much more heavily on publicly disclosed information. Contrary to the common perception of whistleblowers as corporate insiders who witness and expose fraud, Dyck et al. (2010) report that 46% of whistleblowing involving corporate misconduct is done by outsiders such as competitors, media, customers, and regulators. For example, firms sometimes use whistleblower laws to sue competitors for unfair business practices that give them an improper competitive advantage.[18]

The threat of whistleblowing may impose significant costs on firms. These costs not only exist for misbehaving firms but also extend to firms that have not actually misbehaved because of the risk of being falsely accused (Nan et al. 2021). Whistleblowers often misjudge the situation and file frivolous complaints (Miceli and Near 1992). Opportunistic whistleblowers may even intentionally file false accusations for the sake of financial rewards (Levmore and Porat 2012). Dealing with frivolous accusations can be costly, even if the allegations are eventually proved false. For some highly publicized cases, firms concerned about the adverse reputational and related consequences may even have to offer financial settlements for baseless accusations (Howse and Daniels 1995). Given that outsiders actively engage in whistleblowing, we hypothesize that when

[18] For instance, in 2018, the federal government settled a FCA *qui tam* complaint brought by a family-owned business, Moldex-Metric, Inc., against its much larger competitor, 3M Company, which alleged that 3M supplied defective dual-ended combat arms earplugs to the U.S. Defense Logistics Agency.
Source: https://www.whistleblowerllc.com/doj-values-competitor-whistleblower/.

firms face a higher threat of being targeted by a whistleblower, they will reduce the disclosure of information that is potentially useful for external whistleblowers.

3.2 Usefulness of Customer Information in Misconduct Detection

An important consideration in testing the aforementioned hypothesis is that the information disclosed should be potentially useful to external whistleblowers. We propose that information about a firm's economic links with other firms, such as supply-chain information, could be useful to external whistleblowers in detecting firm violations. As Jensen and Meckling (1976) argue, firms operate in a nexus of contractual relationships with other firms (e.g., customers and suppliers) and economic agents (e.g., employees). Because the business activities of economically connected firms are interrelated, a firm's business activities, including its misconduct, could leave a trace with connected firms (e.g., Cohen and Frazzini 2008). When a firm's economic links are revealed to the public, potential whistleblowers could detect the firm's misconduct through the publicly available information of the related firms. For instance, when a firm cooks its books to inflate its sales, its reported sales do not match customer firms' purchase records (Li et al. 2022). More importantly, a potential whistleblower could actively acquire relevant private information through firms linked to the focal firm, increasing both the likelihood of whistleblowing and the chance that whistleblowing will succeed.

We focus on firms' disclosure choices about their major customers' identities (e.g., Ellis et al. 2012; Li et al. 2018), because revealing customer identities could facilitate whistlebowing. First, when a firm's customer identities are known to the public, potential whistleblowers could collect useful inside information about the firm *privately* through the customers. It is well recognized in the literature that customers may possess confidential information about suppliers because of their relationship-specific investments, repeated transactions, and frequent interactions (Baiman and

Rajan 2002; Li and Zhang 2008; Kong et al. 2013; Cen et al. 2017). For example, suppliers may have relationship-specific investments tailored specifically for customers and allow customers to be heavily involved in their production and personnel training; these allow customers to access confidential information about the suppliers (Cen et al. 2017). In addition, executives from major customers may serve on the suppliers' board (Albuquerque et al. 2011), and customer firms' employees likely interact frequently with suppliers' employees (Crawford et al. 2020). Such interactions also allow customers to have private information about the supplier firms. Potential whistleblowers could manage to obtain such information if they know the supplier's customers. Thus, disclosing customer identity to the public essentially opens a window for whistleblowers to acquire confidential information through customers.

Second, when customer identities are known, *publicly* available customer information may be useful in detecting FCA violations such as miscalculation of sales tax or state income tax. Tax issues are an important reason for whistleblowing under state FCAs (Lee et al. 2019). For instance, in November 2019, B&H was sued by a whistleblower under New York's FCA for failing to pay sales tax on reimbursements it received from manufacturers for discounts that were passed on to its customers. Prior studies have shown that customer information is helpful in predicting the supplier firm's performance and revenue fraud (e.g., Cohen and Frazzini 2008; Li et al. 2022). Thus, customer disclosure may contain information that can be used to detect a firm's state tax violations. For instance, Li et al. (2022) find that customers' accounting information combined with information about customers publicly disclosed by suppliers can be used to detect the supplier's revenue fraud, because when a supplier cooks its books to inflate its sales, its reported sales do not match customer firms' purchase records. Brown et al. (2022) find that IRS attention

toward a customer firm is positively associated with IRS attention toward the supplier firm. Their finding suggests that a customer firm's tax information may reveal suppliers' tax irregularities.

Moreover, customer information is particularly useful for detecting and proving FCA violations by government contractors. When a firm has both non-government customers and government customers, potential whistleblowers can collect information from the nongovernment customers to identify and prove government procurement fraud. For instance, the price charged to non-government customers can be used to evaluate whether the government has been overcharged. This was the case in October 2006, when Oracle paid $98.5 million to settle a whistleblower-initiated FCA lawsuit. In the lawsuit, the U.S. Justice Department claimed Oracle had overcharged the government by tens of millions of dollars and alleged "various schemes Oracle used to give commercial customers deeper discounts than the GSA schedule provided" (U.S. v. Oracle Corp., 1:07-cv-00529).

3.3 Hypotheses

Outsiders' whistleblowing incentives are heightened after a state adopts a general FCA due to the financial rewards and protection from retaliation provided by the law (Dye et al. 2021; Heese and Pérez-Cavazos 2021). Thus, it will be costlier for firms in that state to disclose major customers' identities, because potential external whistleblowers, such as analysts, the media, short-sellers, customers, and competitors, could use the customer information to detect and prove the firm's FCA violations. The disclosed customer information may also be useful for internal whistleblowers (e.g., employees) whose insider information about the violation is incomplete. Thus, we predict that a firm will be more likely to hide major customer identities after its headquarters state adopts a general FCA. This leads to our main hypothesis:

H1: Firms are more likely to withhold major customer identities in their annual reports after their headquarters states adopt general FCAs.

Next, we examine how the predicted effect varies cross-sectionally with firms' incentives to minimize the risk of an accusation by a whistleblower. As the primary purpose of withholding customer identities is to prevent whistleblowing, we expect that firms with more exposure to external whistleblowers, such as those that attract higher media coverage or that are scrutinized by more competitors, will have a stronger incentive to reduce disclosures after the adoption of a general state FCA. This leads to our first cross-sectional prediction:

H2a: The effect of the general state FCA adoptions on firms' tendency to withhold major customer identities is stronger for firms facing more potential external whistleblowers.

While almost all firms operating in a state have an implicit contractual relationship with the state government through their state tax obligations and are thus subject to general state FCAs, firms with explicit contractual relationships with the state government—through government procurement contracts, for example—have additional exposure to the state FCA litigation risk. States and municipalities have substantial contracting budgets and purchase goods and services from many contractors (Millman 2019).[19] As we discuss above, customer information is particularly useful for detecting and proving FCA violations by government contractors. Thus, we further predict that firms with explicit business relationships with the state/local government are more concerned about the state FCA litigation risk and thus are more likely to withhold customer identities after their states adopt general FCAs. This leads to our second cross-sectional prediction:

H2b: The effect of the general state FCA adoptions on firms' tendency to withhold major customer identities is stronger for firms with more business contracts with the state government.

[19] For instance, New York City spends about $20 billion per year purchasing goods and services through about 40,000 procurement actions (Millman 2019).

There are several reasons why we might not observe a reduction in the disclosure of customer identities after general state FCA adoptions. First, firms could refrain from defrauding the state government after the adoptions (Berger and Lee 2022; Lee et al. 2019), which would reduce their need to withhold relevant information. Second, under the heightened risk of being accused by a whistleblower, managers may disclose more information to signal their innocence, especially when withholding information is viewed as a red flag. Third, withholding customer identities is costly because firm disclosure has capital market benefits. If the capital market motives dominate the costs of greater whistleblower risk, we will not observe a reduction in disclosure after the FCA adoption. Thus, whether firms reduce disclosure of their customer identities after the state FCA adoptions is an open empirical question.

4. Research Design

4.1 Variable Measurement

We use the adoption of state general FCAs to examine how the whistleblower threat affects firms' disclosure decisions. We use the dummy variable *FCA_general* to indicate firm-years after the adoption of state general FCAs. *FCA_general* equals one if a firm's headquarters state has passed the state general FCA, and zero otherwise. As discussed in Section 2.2, firms operating in a state are governed by the state regulations and also have implicit contracts with the state government (e.g., through state taxes obligations). We thus focus on a firm's operating state (i.e., headquarters state) to identify whether a firm is subject to a state FCA.[20] Table 1 presents a timeline of state FCA adoptions, including the type of FCA adopted and whether the law contains a qui tam provision.

[20] We acknowledge that firms not operating in a state might also have contractual relationship with the state government. This possibility may lead to misclassification of treatment firms into the control group and biases against us finding the result.

Our main measure of voluntary disclosure is the disclosure of customer identity (e.g., Ellis et al. 2012; Li et al. 2018). Under Regulation S-K, firms have to disclose the identity of, and the sales amount to, all major customers (i.e., customers that account for more than 10% of a firm's total sales). Nevertheless, due to weak enforcement, around 40% of major customer identities are not disclosed (Ellis et al. 2012). Thus, in practice, managers have discretion over the disclosure choice. More importantly, prior studies suggest that managers' discretion over customer identity disclosure is nonrandom and is driven by predictable economic factors, such as proprietary costs (Ellis et al. 2012; Li et al. 2018). Thus, major customer identity disclosure, while mandated, has a significant voluntary component, and has been used in the literature as a voluntary disclosure setting to test theories of voluntary disclosure (e.g., Ellis et al. 2012; Li et al. 2018).

Following Li et al. (2018), we construct two measures of customer identity disclosure. The first measure, *Ratio1*, reflects the percentage of reported customers whose identities are revealed. The second measure, *Ratio2*, is the sales-weighted percentage of reported customers whose identities are revealed. One might be concerned that when customer identities are not disclosed for the current year, users of financial reports could still learn those identities from past disclosures. This would reduce the benefit, to the firm, of withholding customer identities in the current year. However, the customer-supplier relationship is fluid and evolving, and withholding information in the current year should still help to hide the identities of new customers (Li et al. 2018).

4.2 Regression Model

We use a generalized difference-in-differences (DiD) specification to examine the effect of the state FCA adoptions on the disclosure of customer identities. Following prior research (e.g., Bertrand and Mullainathan 2003; Armstrong et al. 2012; Bourveau et al. 2018), we estimate the following regression model:

$$Log(1 + Disclosure_{i,t,s}) = \beta_0 + \beta_1 FCA\_general_{s,t} + Controls + \gamma_i + \omega_t + \theta_s + \eta_{i,t,s}, \quad (1)$$

where the subscripts *i*, *s*, and *t* refer to firm *i*, state *s*, and year *t*, respectively. $Disclosure_{i,s,t}$ is one of our two measures of customer identity disclosure, *Ratio1* and *Ratio2*. Following prior studies (e.g., Ellis et al. 2012; Li et al. 2018), we take the log for the dependent variable to reduce the skewness and add one to avoid taking the log of zero.

We include firm fixed effects (γ_i) and year fixed effects (ω_t) to control for the effect of time-invariant firm heterogeneity and the general time trend of customer identity disclosure. We include headquarters-state fixed effects (θ_s) to control for the change in headquarters location during the sample period, following Li et al. (2018).[21] As the FCA adoption is at the headquarters state level, we cluster standard errors by headquarters state to account for any within-state dependence (Petersen 2009). Our key variable of interest is *FCA_general*. H1 predicts a negative coefficient of *FCA_general*.

With firm and year fixed effects, equation (1) can be interpreted as a DiD design (Armstrong et al. 2012; Bourveau et al. 2018). The coefficient on *FCA_general* captures the change in customer identity disclosure by firms after their headquarters state adopts a general FCA (treatment firms), relative to the change by firms operating in all states that do not adopt FCAs in the event year (control firms). This regression approach accounts for the fact that the state FCA adoptions occur in different states at different times. The staggered occurrence of the state FCA adoption events means that our control firms include not only firms headquartered in states that never have a state FCA adoption during our sample period but also firms operating in states that adopted state FCAs in previous years or that adopt them in subsequent years. Recent studies raise

[21] Our results are robust to dropping firms that change their headquarters states during the sample period and to including only firm and year fixed effects.

the concern that using already treated firms as the control firms for later treated firms may bias the estimation (Baker et al. 2022; Barrios 2021; Cengiz et al. 2019). We conduct a series of robustness tests to address this concern in Section 5.5.

Controls refers to control variables. We control for firm and industry characteristics that prior studies show possibly affect firms' disclosure of customer identities (Ellis et al. 2012; Li et al. 2018). First, prior studies show that proprietary costs of disclosure affect a firm's decision to disclose customer identities (Ellis et al. 2012; Li et al. 2018; Huang et al. 2017), so we include R&D expenditures (*RD*), advertising expenditure (*Advertising*), intangible assets (*Intangibility*), and industry concentration (*HHI*) to control for the impact of those costs. As R&D information is missing for many firms, we also include an indicator variable (*Missing_RD*) to indicate whether the amount of R&D expenditure is missing (Koh and Reeb 2015). Second, to capture the effect of capital market incentives, we control for an indicator variable for whether a firm has a seasoned equity offering (*SEO*) and an indicator for merger and acquisition activities (*MA*). Third, we include firm size (log(*Size*)) and an indicator variable for large auditors (*Big_N*) to control for the demand for information and audit quality, respectively. Finally, we include state GDP growth (*GDP*) and unemployment rate (*Unemployment*) to control for the economic conditions of firms' headquarters state. Detailed definitions of all variables are provided in Appendix A.

5. Empirical Results

5.1 Sample and Summary Statistics

We obtain firms' financial data from Compustat, customer disclosure information from Compustat Segment Files, and stock return information from CRSP. The state level GDP data and unemployment data are from the U.S. Census Bureau and the U.S. Bureau of Labor Statistics, respectively. To identify the headquarters state for each firm, we use the 10-K Headers Database

constructed by Bill McDonald, which identifies firms' historical headquarters states (e.g., Li et al. 2018; Huang et al. 2020).[22] As the historical headquarters data first become available in 1994, we follow Li et al. (2018) and start our sample that year.[23] We end our sample in 2010, the year before the Dodd-Frank whistleblower provision is enacted.

To focus on the general state FCAs (which cover any kind of fraud committed against the state government), we exclude firms in states that adopt only Medicaid FCAs (which cover only healthcare or Medicaid fraud against the state government).[24] We delete financial firms and firms headquartered outside the United States (e.g., Li et al. 2018). We require non-missing data to measure the test and control variables in Equation (1). Our final sample includes 23,703 firm-years over the sample period of 1994–2010, including 16,821 firm-years from states that have adopted the state general FCA and 6,882 firm-years from states that never adopted state FCAs.

Table 2 presents summary statistics for the sample. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers. On average, a firm discloses 59% (58%) of its (sales-weighted) customers' identities, with substantial cross-sectional variation (standard deviation is 0.433 and 0.453, respectively). These numbers are comparable to those reported by Li et al. (2018). Approximately half of firm-year observations are located in states with a general state FCA in effect. An average firm in our sample has a market capitalization of $951.9 million, with R&D expenditure, intangible assets, and advertising expenses accounting for 37.4%, 18.0%, and 1.2% of its revenues, respectively. In addition, 32% of firm-years have missing

[22] The Compustat database only provides the most recent state of the headquarters for each firm.
[23] Our main results are robust to using the sample period from 1977 by backfilling the headquarters states in early years using the headquarters states in 1994.
[24] As a falsification test, we include firms in states that only adopted Medicaid FCAs and test the effects of general FCAs and Medicaid FCAs separately. We find that Medicaid FCA adoption does not lead to a significant reduction of customer identity disclosure. This finding is expected because a Medicaid FCA only covers healthcare or Medicaid fraud, while our story mainly applies to tax and/or procurement contract violations. Please refer to Section 5.6 for more details.

R&D expenditures, 78% use big N auditors, 4% have merger and acquisition activity, and 6% have a seasoned equity offering.

5.2 Impact of State General FCA Adoptions on Disclosure of Customer Identities (H1)

Table 3 presents the results of estimating Equation (1). Columns 1 and 2 present the results for *Ratio1* without and with control variables, respectively. Similarly, the results for *Ratio2* are reported in Columns 3 and 4. The coefficient of *FCA_general* is negative and statistically significant across all columns, indicating that firms reduce their disclosure of customer identities after their headquarters states adopt general FCAs. [25] The effect is economically significant. The coefficient of *FCA_general* in Column 2 (when *Ratio1* is the dependent variable) is -0.035, suggesting that the passage of a general state FCA is associated with a 9.3% decrease in customer identity disclosure.[26] Similarly, the coefficient of *FCA_general* in Column 4 (when *Ratio2* is the dependent variable) is -0.039, suggesting that the passage of a general state FCA is associated with a 10.4% decrease in customer identity disclosure. These results are consistent with our H1 prediction that firms are more likely to withhold customer identities in their annual reports after their headquarters states adopt general FCAs.

Turning to the control variables, we find that the coefficient on R&D expenditures (*RD*) is significantly negative, which is consistent with the finding in prior studies that firms with higher proprietary costs of disclosure are more likely to conceal their customer identities (Ellis et al. 2012; Li et al. 2018). The significantly positive coefficient of large auditors (*Big_N*) is consistent with a

[25] In an untabulated robustness test, we estimate Equation (1) after replacing firm fixed effects with industry fixed effects. The coefficients of $FCA\_General$ remain significantly negative, which bolsters our confidence that the results are not driven by a particular model choice.

[26] The average *Ratio1* for firms in the treatment states before the state FCA adoptions is 0.582. The coefficient -0.035 suggests that benchmarking against the change of control firms, an average treatment firm's *Ratio1* is $e^{[\ln(1 + 0.582) - 0.035]} - 1$, which equals 0.527. Thus, the relative change in *Ratio1* is (0.527 - 0.582)/ 0.582 = -9.3%. We use a similar approach to calculate the economic effects for *Ratio1* and *Ratio2* for the subsequent analyses.

prior finding that firms with stricter auditor monitoring disclose more (e.g., Ellis et al. 2012).

5.3 Dynamic Analysis

The validity of our empirical strategy requires that the treatment and control firms follow parallel trends in customer identity disclosure in the absence of the state FCA adoptions. To provide evidence on whether the parallel trends assumption is met, we augment our Equation (1) model to estimate a dynamic difference-in-difference model with lead-lag terms. Specifically, we replace *FCA_general* in Equation (1) with a series of indicators, *FCA_Year(i)*, *i*=-5, -4, …, 3 , 4+. *FCA_Year(-5)* equals one if a firm is headquartered in a state that will adopt a general FCA in five years and zero otherwise. The other indicator variables are defined similarly, except that *FCA_Year(4+)* is equal to one if a firm is headquartered in a state that adopted a general FCA four or more years earlier.

Columns 1 and 2 of Table 4 report the results for *Ratio1* and *Ratio2*, respectively. For both disclosure measures, the coefficients of *FCA_Year(-5)*, *FCA_Year(-4)*, *FCA_Year(-3)*, *FCA_Year(-2)*, and *FCA_Year(-1)* are all statistically insignificant, indicating that treatment firms and control firms exhibit similar customer identity disclosure trends in the pre-adoption period. These results support the parallel trends assumption and bolster the validity of our research design. For *Ratio2*, our sales-weighted customer identity disclosure measure, we observe that the coefficients of *FCA_Year(1)*, *FCA_Year(2)*, *FCA_Year(3)*, and *FCA_Year(4+)* are negative and significant, indicating that firms reduce their customer identity disclosure in the first year after the passage of the state general FCA. For our unweighted measure, *Ratio1*, we find that the coefficients of *FCA_Year(1)*, *FCA_Year(2)*, *FCA_Year(3)*, and *FCA_Year(4+)* are all negative. However, only the coefficients for year 2 and later are statistically significant. These results are consistent with firms withholding their customer identity information after the passage of state general FCAs.

5.4 Cross-sectional Analyses

Our cross-sectional analyses examine how the predicted effect on customer identity disclosure varies cross-sectionally with firms' incentives to minimize the risk of being whistle-blown. We observe two key empirical challenges for these analyses. First, the partition variables can be affected by the adoptions of the state general FCAs, which may result in biased estimates of the treatment effects (Gormley and Matsa 2011). To alleviate this concern, we construct the partition variables for the cross-sectional tests based on ex-ante firm characteristics measured in the year prior to the adoption of the state general FCA (e.g., Gormley and Matsa 2011; Bourveau et al. 2018).

Another challenge comes from the staggered timing of the adoptions of the state general FCAs in different states over the sample period. In a staggered DiD design, an observation can be a control and a treatment in different years for different treatment events, making it difficult to measure ex-ante characteristics. To address this issue, we follow prior studies (e.g., Gormley and Matsa 2011; Bourveau et al. 2018; Appel 2019) and use a cohort-based matching approach for the cross-sectional analysis. In particular, for each year in which a state general FCA is adopted, we create a cohort made of firms headquartered in the states that pass the general FCA in that year (treatment firms) and of all other firms in the states that do not adopt a general state FCA during the nine-year window of the adoption year (control firms). We keep the observations for the nine-year window around the adoption year for both treatment and control firms.[27]

After creating the cohort for each adoption year, we stack all cohorts together and then create a partition variable for each of the ex ante characteristics of interest. For each cross-sectional

[27] For example, for the cohort of the event year 1998 for D.C., we have the treatment firms headquartered in D.C. over 1994–2002 and the control firms headquartered outside D.C., and we require that the control firms' headquarters states not adopt a general FCA over 1994–2002.

test, we augment Equation (1) by including the partition variable (*Part.Var*) and its interaction with the state general FCA indicator (*FCA_general*). Specifically, we estimate the following model:

$$Log(1 + Disclosure_{i,t,s}) = \beta_0 + \beta_1 FCA\_general_{s,t} \times Part.Var + \beta_2 FCA\_general_{s,t} + \beta_3 Part.Var + Controls + \gamma_i + \omega_t + \theta_s + \eta_{it}. \quad (2)$$

5.4.1 Likelihood of Attracting External Whistleblowers (H2a)

H2a predicts that the nondisclosure effect is stronger for firms that face more potential external whistleblowers. As Dyck et al. (2010) find that external whistleblowers such as competitors, the media, and industry regulators play an important role in uncovering and blowing the whistle on corporate misconduct, we use three angles to test this hypothesis.

First, competitors may take advantage of whistleblower laws to sue a firm in order to gain a competitive advantage, so we conjecture that firms operating in an industry with more intense competition may attract more whistleblowers—such firms are likely to be closely watched by more competitors for whistleblowing purpose.[28] We measure the likelihood of attracting a competitor as a whistleblower with the variable *Competition*, defined as the negative of the percentage of sales from the four largest firms in a five-digit NAICS industry based on U.S. Census data during one year prior to the FCA adoption year (Ali et al. 2014).[29] A higher value of *Competition* indicates more intense competition and thus a higher likelihood of attracting competitors as whistleblowers. To address potential measurement error, we transform *Competition* to its decile rank (1–10).

[28] In this test, we explore how the effect of the state FCA adoption on customer identify disclosure varies with industry competition, not the effect of industry competition on customer identity disclosure. While proprietary costs are an important determinant of disclosure decisions (e.g., Huang et al. 2017; Li et al. 2018), our finding is not due to industry competition measuring proprietary costs, because it is unlikely that the proprietary costs could change with state FCA adoptions.

[29] Compared with Compustat data that only include public firms, U.S. Census data consider both public firms and private firms and thus better suits the purpose of our test because competitor whistleblowers may come from both public and private firms.

Columns 1 and 2 of Table 5 present the results of estimating Equation (2) using *Competition* as the partition variable. The coefficient of *FCA_general* × *Competition* is negative and statistically significant for both columns (p-value < 0.01 and p-value <0.05, respectively), indicating that the negative effect of a state general FCA adoption on customer identity disclosure is stronger for firms that face stronger competition, consistent with H2a.

Second, we use local media (*Local_media*) as another partition variable to capture the likelihood of attracting a local journalist as an external whistleblower. *Local_media* is defined as the number of local newspapers in a firm's headquarters state in the year prior to the FCA adoption. As in the case of *Competition*, we transform *Local_media* to its decile rank (1–10). We predict that the negative effect of state general FCA adoption will be stronger for firms with high local media coverage (e.g., Gao et al. 2020; Heese et al. 2021). The coefficient of *FCA_general* × *Local_media* in Columns 3 and 4 of Table 5 suggest that firms facing higher local media coverage are more likely to reduce their customer identity disclosure after their states' general FCA adoptions, consistent with H2a.

Finally, as Dyck et al. (2010) find industry regulators are also important external whistleblowers, we predict that the effect of state general FCA adoption will be stronger for firms operating in highly regulated industries. Such firms are likely watched by more industry regulators for whistleblowing purpose. We follow Dyck et al. (2010) and partition the sample using an indicator variable for whether a firm operates in a highly regulated industry (*Industry_regulation*). The significantly negative coefficients of *FCA_general* × *Industry_regulation* in Columns 5 and 6 of Table 5 indicate that the nondisclosure effect is indeed stronger for firms facing more industry regulators. Together, the results in Table 5 suggest that when state general FCAs heighten the

whistleblowing risk, firms that are exposed to more potential external whistleblowers are more likely to reduce their disclosure of customer identities, relative to other firms in their states.

5.4.2 Business Exposure to State Governments (H2b)

To test the H2b prediction that the effect of state general FCA adoptions on customer identity disclosure will be stronger for firms that have business contracts with the state government, we first need to measure whether a firm has such contracts. As data on state government contracts is unavailable, we use the existence of government customers as a proxy. We create an indicator variable, *Gov_contractor*, which equals one if a firm has government customers in the Compustat customer segment file in the year prior to its state's adoption of a general FCA and zero otherwise, and use this indicator as the partition variable in Equation (2).[30] While this variable is a noisy measure of whether a firm has business contracts with its state government, our assumption that firms with government customers are more likely to have business contracts with their state governments seems reasonable.

Table 6 presents the results for our cross-sectional test based on government contracting. The coefficient of *FCA_general* × *Gov_contractor* is negative and statistically significant for both columns (p-value < 0.05 and p-value <0.01, respectively), indicating that the negative effect of the state general FCA adoptions on customer identity disclosure is stronger for firms that are more likely to have business exposure to state government. This finding also corroborates our inference that the nondisclosure effect we documented is attributable to the FCA adoptions.

5.5 Addressing Potential Biases in Staggered DiD Designs

[30] In the Compustat customer segment file, the specific data for state government customers is not available and is pooled into a variable that capture all domestic government customers including all federal, state, and local governments. Thus, the government contractor indicator variable is noisy because federal contractors are also included. This measurement error, however, tends to work against finding the result.

The fact that states passed general FCAs in different years minimizes the concern that our results are driven by a concurrent confounding event. However, the standard two-way fixed effect (TWFE) staggered DiD design is subject to a potential bias because it uses already treated units as comparison units for later treated units (Baker et al. 2022; Barrios 2021; Goodman-Bacon 2021). Following Baker et al.'s (2022) suggestions, we conduct multiple tests to mitigate this concern. Table 7 presents the results.

First, we estimate a stacked regression model in which we create event-specific datasets around each adoption year as we do for our cross-sectional test (see Section 5.4). We then stack the datasets in relative time to estimate the average treatment effect, interacting the fixed effect with event indicators. This approach mitigates concerns about the bias due to heterogeneous treatment effects (Goodman-Bacon 2021; Baker et al., 2022), since it avoids using the early treated observations as potential controls. Table 7, Columns 1 and 2 present the results. For both measures of disclosure, we find that the coefficient of *FCA_general* is negative and statistically significant, indicating that firms reduce their customer identity disclosure after the passage of a state general FCA. The magnitude of the coefficient is comparable to our estimates in Table 3, further bolstering the validity of our results.

Another way to address the potential bias in a staggered DiD design is to repeat the analyses after deleting early treatment observations from the sample (Baker et al. 2022). In Columns 3 and 4 of Table 7, we repeat our analyses after dropping the three states (California, Florida, and Illinois) that adopted a general FCA before our sample started. Across both columns, the coefficient of *FCA_general* is negative and statistically significant, indicating that our results are robust.

Finally, Baker et al. (2022) suggest that the inclusion of covariates could affect the average treatment effect estimates, since some covariates might change as a result of the treatment. In

Columns 1 and 3 of Table 3, we report the results without including any covariates. The coefficient of *FCA_general* is still negative and significant in both columns, indicating that our results are robust to this alternative design.

5.6 Falsification Tests

To further bolster the validity of our findings, we conduct two falsification tests. First, we augment our sample to include states that passed Medicaid FCAs. As these FCAs only cover healthcare- or Medicaid-related misconduct, for which customer identity information may not be as useful in the detection, we should not find any change in firms' customer identity disclosures after the adoption. Columns 1 and 2 of Table 8 present the results. As expected, in both columns, the coefficient of *FCA_Medicaid*—an indicator variable for firm-years with Medicaid FCA—is insignificant, while the coefficient of *FCA_general* is still negative and significant. These results are consistent with the whistleblowing threat—not selection bias or correlated omitted variables—explaining the negative effect of general state FCAs on customer identity disclosures (Berger and Lee 2022).

In our second falsification test, we examine whether state general FCA laws affect the disclosure of information that is not useful for whistleblowers. For example, management earnings forecasts are not informative about potential violations because the actual earnings number will become publicly available soon after the forecast is issued. We therefore expect firms to have little incentive to hide such information when the whistleblowing threat is heightened. To assess this, we re-estimate Equation (1) using management earnings forecasts (*MF_EPS*) and management forecasts of non-earnings information (e.g., sales, capital expenditure, and other forecasts) (*MF_Non-EPS*) as the dependent variables. Columns 3 and 4 of Table 8 report the results. For both management forecast measures, the coefficient of *FCA_general* is insignificant, indicating that an

increase in whistleblower threat does not affect managers' guidance decisions. These results suggest that firms reduce only the disclosures that could be useful for whistleblowers.

5.7 Other Robustness Tests

5.7.1 Are the results driven by changes in customer base?

One could be concerned that a reduction in disclosure of customer identity could be due to changes in firms' customer base instead of a reporting choice. A firm's customer base may change over time. If a customer is lost, our disclosure measure could also change even though the disclosure strategy is the same. We conduct two analyses to address this concern. First, we test whether the number of customers and total sales to major customers change after state general FCA adoptions.[31] We find no significant effect in either test (untabulated).[32] These results suggest that a firm's customer base does not change significantly after state FCA adoptions, which minimizes the aforementioned concern. These results also suggest that firms do not seem to adjust their real relationship with customers in response to the heightened whistleblowing threat, presumably because adjusting the customer base could be very costly. Second, we add the number of major customers a firm reports as an additional control in our main test and find quantitatively similar results (untabulated). These analyses bolster our confidence that our finding is mostly driven by changes in managers' disclosure decisions instead of changes in the customer base.

5.7.2 Are the results mainly driven by misbehaving firms?

While we argue that non-misbehaving firms are also concerned about whistleblowing threat because of potential frivolous claims (Miceli and Near 1992; Howse and Daniels 1995;

[31] Specifically, we estimate Equation (1) using log(1+#customers) and log(1+total sales from major customers) as the dependent variables.
[32] The *p*-value for the coefficient of *FCA_general* is 0.640 when the dependent variable is log(1+# of customers); it is 0.173 when the dependent variable is log(1+ total major customer sales).

Gobert and Punch 2000; Levmore and Porat 2012), it is possible that such concern is stronger for misbehaving firms and thus our results are mainly driven by those firms. To explore this possibility, we estimate Equation (1) using subsamples of firms with high versus low likelihood of misbehaving, measured with Beneish's (1999) M-score and Dechow et al.'s (2011) F-Score. We find that the results are not significantly different between the two groups (untabulated for brevity). This finding is consistent with our argument that well-behaved firms may also be concerned about the risk of false allegations and may therefore use nondisclosure to minimize the likelihood of being whistleblown.

5.7.3 Are the results mainly driven by a subset of states?

In the coverage descriptions in their general FCAs, some states explicitly identify state tax violations as violations, while other states implicitly cover them using a more general term. [33] To address the concern that our results are mainly driven by the states that explicitly mention tax violations in their FCAs, we estimate Equation (1) after splitting the *FCA_General* variable into *FCA_TaxExplicit* and *FCA_nonTaxExplicit*. *FCA_TaxExplicit* (*FCA_nonTaxExplicit*) is an indicator variable that equals one for the post-adoption period of the state general FCAs *with* (*without*) explicit provisions for tax violations. The coefficients of both variables are significantly negative, and the difference is not significant (untabulated for brevity). These results suggest that whether or not a state general FCA explicitly mentions state tax violations in its coverage description does not change the impact of the FCA adoption on firms' disclosure incentives.

In addition, to address the concern that our main results are driven by a particular state with some unique features, we repeat our main analysis by respectively: 1) removing the firms operating

[33] The states that explicitly mention tax-related fraud in the FCA include Florida, Nevada, Illinois, Delaware, Indiana, New Hampshire, Rhode Island, and New York (Lee et al. 2019).

in Florida, Texas, and Washington because these states have no state income tax; 2) removing the firms operating in New York because anecdotes suggest New York more actively uses its state FCA to chase tax-misbehaving firms; and 3) removing large states like California and New York. Our results are robust in all these additional tests (untabulated).

6. Additional Analyses

6.1 Impact of the Dodd-Frank Act on Customer Identity Disclosure

In the aftermath of the 2008 financial crisis, the United States Congress passed the Dodd-Frank Act as part of a vast regulatory effort to ensure corporate accountability and compliance. Dodd-Frank contains six major provisions, which introduced a number of changes in different aspects of the economy. One of these provisions was the creation of new federal whistleblower programs that are designed to reward and protect individuals who report violations in the financial markets or by U.S. companies doing business abroad. The final rules of the new whistleblower program went into effect on August 12, 2011. In this subsection, we explore whether the nondisclosure effect we document can be generalized to the Dodd-Frank whistleblowing program. We caveat, however, that the Dodd-Frank Act has a broader intent than the state general FCAs, which makes it difficult to disentangle the effect of the increased whistleblowing threat from the changes introduced by other provisions of the act (see footnote 11).

Following Berger and Lee (2022), we conjecture that the effect of the Dodd-Frank whistleblowing program should be stronger for firms headquartered in states that *do not* have state general FCA adoptions. Therefore, we estimate a standard DiD model using firms in states that have not passed a general FCA as the treatment group and firms in states that have passed a general FCA as the control group. Specifically, we estimate the following model:

$$Log(1 + Disclosure) = \beta_0 + \beta_1 Treat \times Post + Controls + \gamma_i + \omega_t + \theta_s + \eta_{it}, \quad (3)$$

where *Treat* is an indicator variable that equals one for treatment firms, and zero otherwise. *Post* is an indicator variable that equals one after 2011, and zero otherwise. *Disclosure* and *Controls* are defined as in Equation (1). The coefficient of *Treat*×*Post* captures the effect of the Dodd-Frank whistleblowing provision on firms' disclosure. We estimate Equation (3) over the sample period of 2009–2013, which covers two years prior to and two years after the effective date of the Dodd-Frank whistleblowing provision.

Table 9 presents the results of estimating Equation (3). The coefficient of *Treat* ×*Post* is negative across both columns and is significant for *Ratio2*, providing modest evidence that firms reduce their customer identity disclosure following the adoption of the Dodd-Frank whistleblowing provision. These results suggest that after the provision's implementation, firms try to minimize the whistleblowing threat by withholding customer identity information from potential external whistleblowers, which corroborates our findings for the state FCA adoptions.

6.2 Impact of State FCA Adoptions on Subsidiary Disclosure

We also explore whether our prediction that firms withhold more information after their states enact general FCAs applies to other disclosures of their economic links with other firms. In particular, we investigate the impact on firms' disclosure of their subsidiaries (e.g., Dyreng et al. 2020). [34] Like information on customer identity, subsidiary information could be useful in evaluating whether a firm's state taxes have been miscalculated and, more importantly, could allow whistleblowers to acquire relevant information.

Table 10 presents the results for the subsidiary disclosure analysis. We consider two disclosure measures: the number of disclosed foreign subsidiaries (*# of foreign subs*, Column 1)

[34] Anecdotal evidence and academic research show that firms *strategically* withhold information about their subsidiaries despite no apparent change in firm operations (Gramlich and Whiteaker-Poe 2013; Dyreng et al. 2020).

and the number of disclosed subsidiaries in tax havens (*# subs in tax heaven*, Column 2). We estimate Equation (1) using the log transformation of each measure as the dependent variable and a modified set of control variables from Dyreng et al. (2020). For both dependent variables, the coefficient of *FCA_general* is negative and statistically significant, indicating that firms do reduce their subsidiary disclosure after their states adopt general FCAs. These results are consistent with our finding for customer identity disclosure and further suggest that when the whistleblowing threat is heightened, firms strategically withhold information to minimize the risk of whistleblowing.[35]

6.3 Internal whistleblowers

In this subsection, we explore whether the threat from internal whistleblowers influences the firms' decision to withhold customers' identities following the enactment of state general FCA laws. While we argue that external whistleblowers potentially rely more on a firm's public disclosure, because firms' insiders (e.g., employees) are more likely to possess private insider information, it is possible that internal whistleblowers may still use public disclosure to corroborate their private insider information. Heese and Pérez-Cavazos (2021) find that firms with higher total unemployment benefits are likely to face higher internal whistleblowing pressure because they face more potential whistleblowers due to the lower retaliation cost of whistleblowing. Thus, we conduct cross-sectional analyses based on the total unemployment benefits available to employees in the year prior to the enactment of the FCA law. We find no significant difference in the effects between firms with high versus low internal whistleblowing threats from employees.

[35] We caveat that our analysis in this test is subject to a data limitation because we cannot accurately identify the existence of subsidiaries due to lack of Internal Revenue Service data. We need to rely on a firm's self-disclosure of subsidiaries to infer whether the firm has subsidiaries. However, a firm's not disclosing subsidiaries may simply because it does not have a subsidiary.

This result suggests that the threat from external whistleblowers is presumably the main driver of the effect we document. We do not tabulate this analysis for brevity.

7. Conclusion

Motivated by the fact that almost half of whistleblowers are external to the firm (Dyck et al. 2010), this study examines how the threat of a whistleblowing allegation affects managers' disclosure incentives. Specifically, we examine the changes in firms' disclosure of customer identities around the staggered adoptions of state general FCAs. Using a difference-in-differences analysis, we find that firms significantly reduce the disclosure of their customers' identities after their states adopt general FCAs. Cross-sectionally, we find that the decrease in customer identity disclosure is stronger for firms that are more likely to attract external whistleblowers and firms that have more business exposure with the state government. Collectively, our findings are consistent with firms responding to heightened whistleblowing risk by withholding information that is potentially useful for whistleblowers.

Our study contributes to the literature on the economic impact of whistleblowing on corporate behavior by showing that firms are more careful about the information they disclose to the public when they face a higher whistleblowing threat. Our finding complements the extant literature that focuses on the disciplining effect of whistleblowing laws. It adds to the understanding of the effect of whistleblowing threat on firm behavior by showing that firms facing a heightened whistleblowing threat are more careful not only about what they do but also about what they disclose to the public.

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Appendix A Variable Definitions

Variable	Definition
Advertising	Total advertising expenditure divided by total sales in year t.
Big_N	An indicator variable that equals one for a Big N auditor in year t, and zero otherwise. Auditors with codes 01–08 on the Compustat auditor code are considered big auditors.
Competition	The percentage of sales from the top largest four firms in an industry based on U.S. Census data during one year prior to the FCA adoption year. We multiply the measure by -1 so that a higher value corresponds to higher competition.
FCA_general	An indicator variable that equals one for the post-adoption period of the state general FCAs, and zero otherwise.
FCA_medicaid	An indicator variable that equals one for the post-adoption period of the state Medicaid FCAs, and zero otherwise.
FCA_year(-5)	An indicator variable that equals one for the year which is 5 years prior to the state general FCA adoption, and zero otherwise.
FCA_year(-4)	An indicator variable that equals one for the year which is 4 years prior to the state general FCA adoption, and zero otherwise.
FCA_year(-3)	An indicator variable that equals one for the year which is 3 years prior to the state general FCA adoption, and zero otherwise.
FCA_year(-2)	An indicator variable that equals one for the year which is 2 years prior to the state general FCA adoption, and zero otherwise.
FCA_year(-1)	An indicator variable that equals one for the year which is 1 year prior to the state general FCA adoption, and zero otherwise.
FCA_year(0)	An indicator variable that equals one for the year of the state general FCA adoption.
FCA_year(1)	An indicator variable that equals one for the year which is 1 year after the state general FCA adoption.
FCA_year(2)	An indicator variable that equals one for the year which is 2 years after the state general FCA adoption.
FCA_year(3)	An indicator variable that equals one for the year which is 3 years after the state general FCA adoption.
FCA_year(4+)	An indicator variable that equals one for the year which is more than 3 year after the state general FCA adoption.
GDP	GDP growth of the state in year t.
Gov_contractor	An indicator variable equal to one if a firm reports government customers in segment reporting during one year prior to the FCA adoption year, and zero otherwise.
HHI	Herfindahl-Hirschman index based on total sales in year t.
Industry_regulation	An indicator variable that equals 1 for the health care (SIC 5122 and 8000–8999), utilities (SIC 4900–4999), and transportation industries (SIC 3700–3799) during one year prior to the FCA adoption year, and zero otherwise.
Intangibility	Intangible assets divided by total sales at the end of year t.
MA	An indicator variable that equals to 1 if there is an M&A in year t, and zero otherwise.
MF_EPS	Number of management earnings forecasts for a firm in year t.
MF_Non-EPS	Number of non-earnings-related forecasts for a firm in year t.
Missing_RD	An indicator variable that equals to 1 if R&D is missing in year t, and zero otherwise.

Variable	Definition
Local_media	The number of local newspapers in a firm's headquarter state during one year prior to the FCA adoption year.
Ratio1	Percentage of identified customers in the segment reporting in year t.
Ratio2	Sales-weighted percentage of unidentified customers in the segment reporting in year t.
RD	Total R&D divided by total sales in year t.
SEO	An indicator variable that equals one if there is a seasoned equity offering in year t, and zero otherwise.
Size	Total assets of year t in million dollars.
Unemployment	Unemployment rate of the state in year t.

Table 1 State FCA Adoptions

Adoption year	State	FCA type	Qui tam provision
1987	California	General	Yes
1992	Illinois	General	Yes
1994	Florida	General	Yes
1995	Texas	Medicaid	Yes
1996	Nebraska	Medicaid	No
1997	Louisiana	Medicaid	Yes
1998	D.C.	General	Yes
1999	Nevada	General	Yes
2000	Delaware	General	Yes
2000	Massachusetts	General	Yes
2001	Hawaii	General	Yes
2001	Tennessee	General	Yes
2003	Virginia	General	Yes
2004	New Mexico	General	Yes
2005	Indiana	General	Yes
2005	Montana	General	Yes
2005	New Hampshire	Medicaid	Yes
2007	Georgia	Medicaid	Yes
2007	Missouri	Medicaid	No
2007	New York	General	Yes
2007	Oklahoma	General	Yes
2008	Michigan	Medicaid	Yes
2008	New Jersey	General	Yes
2008	Rhode Island	General	Yes
2008	Wisconsin	Medicaid	Yes
2009	Arizona	Medicaid	No
2009	Arkansas	Medicaid	No
2009	North Carolina	General	Yes
2010	Colorado	Medicaid	Yes

This table reports the states that have adopted state FCAs and the corresponding effective years, the FCA type, and the qui tam provision. It is reproduced from Appendix B of Berger and Lee (2022).

Table 2 Descriptive Statistics

	N	Mean	S.D.	0.25	Median	0.75
Ratio1	23,703	0.588	0.433	0.000	0.750	1.000
Ratio2	23,703	0.582	0.453	0.000	0.834	1.000
Log(1+*Ratio1*)	23,703	0.420	0.300	0.000	0.560	0.693
Log(1+*Ratio2*)	23,703	0.413	0.313	0.000	0.607	0.693
FCA_general	23,703	0.484	0.500	0.000	0.000	1.000
RD	23,703	0.374	1.477	0.000	0.028	0.170
Intangibility	23,703	0.180	0.444	0.000	0.025	0.166
Advertising	23,703	0.012	0.046	0.000	0.000	0.002
HHI	23,703	0.252	0.192	0.107	0.200	0.327
Missing_RD	23,703	0.315	0.465	0.000	0.000	1.000
MA	23,703	0.039	0.194	0.000	0.000	0.000
SEO	23,703	0.061	0.239	0.000	0.000	0.000
Size	23,703	951.949	3,536.687	30.761	120.153	490.070
Log(size)	23,703	4.866	1.954	3.426	4.789	6.195
Big_N	23,703	0.782	0.413	1.000	1.000	1.000
GDP	23,703	3.103	2.604	1.600	3.400	4.700
Unemployment	23,703	5.752	1.812	4.667	5.417	6.333

This table reports the summary statistics for the main sample. The sample consists of 3,344 firms (16,821 observations) located in the states that adopted general state FCAs prior to 2010 and 1,379 firms (6,882 observations) located in states that never adopted state FCAs over 1994–2010. We exclude firm-years headquartered in states that adopt Medicaid FCAs. The sample period is from 1994 to 2010. All continuous variables are winsorized at the 1% level. Variable definitions are provided in Appendix A.

Table 3 Impact of State FCA Adoptions on Disclosure of Customer Identity

Dependent Variable:	Log(1+*Ratio1*)		Log(1+*Ratio2*)	
	(1)	(2)	(3)	(4)
FCA_general	**-0.034****	**-0.035*****	**-0.038*****	**-0.039*****
	(0.017)	**(0.008)**	**(0.003)**	**(0.002)**
RD		-0.003**		-0.002**
		(0.021)		(0.029)
Intangibility		-0.009		-0.005
		(0.179)		(0.584)
Advertising		-0.070		-0.106
		(0.402)		(0.133)
HHI		-0.013		0.010
		(0.588)		(0.674)
Missing_RD		0.003		0.006
		(0.821)		(0.680)
MA		0.008		0.005
		(0.439)		(0.599)
SEO		0.010		0.006
		(0.128)		(0.519)
Log(*Size)*		0.004		0.005
		(0.172)		(0.124)
Big_N		0.016*		0.014*
		(0.069)		(0.091)
GDP		-0.000		-0.001
		(0.634)		(0.507)
Unemployment		-0.006		-0.005
		(0.222)		(0.260)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Adj. R-Squared	0.583	0.583	0.589	0.589
Observations	23,703	23,703	23,703	23,703

This table reports the results of estimating Equation (1). The sample period is from 1994 to 2010. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All regressions include firm, year, and headquarters state fixed effects. Standard errors are clustered at the headquarters state level. All continuous variables are winsorized at 1% level. Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

Table 4 Dynamic Analysis

Dependent Variable:	Log(1+*Ratio1*)		Log(1+*Ratio2*)	
	(1)		(2)	
	Coefficient	*p*-value	Coefficient	*p*-value
FCA_Year(-5)	**-0.003**	**0.889**	**-0.001**	**0.978**
FCA_Year(-4)	**0.003**	**0.930**	**0.009**	**0.779**
FCA_Year(-3)	**-0.002**	**0.935**	**0.000**	**0.996**
FCA_Year(-2)	**-0.006**	**0.782**	**-0.016**	**0.515**
FCA_Year(-1)	**-0.031**	**0.172**	**-0.038**	**0.160**
FCA_Year(0)	-0.030	0.118	-0.036	0.108
FCA_Year(1)	-0.045	0.105	-0.052*	0.076
FCA_Year(2)	-0.048*	0.069	-0.052*	0.057
FCA_Year(3)	-0.048*	0.053	-0.053**	0.044
FCA_Year(4+)	-0.062**	0.013	-0.065**	0.018
Control variables	Yes		Yes	
Firm fixed effects	Yes		Yes	
Year fixed effects	Yes		Yes	
State fixed effects	Yes		Yes	
Adj. R-Squared	0.583		0.589	
Observations	23,703		23,703	

This table presents the results of testing the parallel trends assumption. *FCA_Year(-5)*, *FCA_Year(-4)*, *FCA_Year(-3)*, *FCA_Year(-2)*, and *FCA_Year(-1)* are indicator variables that are equal to one for the years which are 5 years, 4 years, 3 years, 2 years, and 1 year prior to the FCA adoption, respectively. *FCA_Year(0)* is an indicator variable equal to one for the year of the state general FCA adoption. *FCA_Year(1)*, *FCA_Year(2)*, *FCA_Year(3)*, and *FCA_Year(+4)* are indicator variables that are equal to one for the years that are 1 year, 2 years, 3 years, and more than 3 years after the state general FCA adoption, respectively. The results of control variables are omitted. The sample period is from 1994 to 2010. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All regressions include firm, year, and headquarters state fixed effects. Standard errors are clustered at the headquarters state level. All continuous variables are winsorized at the 1% level. Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

Table 5 Likelihood of Attracting External Whistleblowers and Impact of State FCA Adoptions on Disclosure of Customer Identities

Partition Variable	*Competition*		*Local_media*		*Industry_regulation*	
Dependent Variable	Log(1+*Ratio1*)	Log(1+*Ratio2*)	Log(1+*Ratio1*)	Log(1+*Ratio2*)	Log(1+*Ratio1*)	Log(1+*Ratio2*)
	(1)	(2)	(3)	(4)	(5)	(6)
FCA_general × *Par. Var.*	**-0.008***** **(0.001)**	**-0.008**** **(0.028)**	**-0.004*** **(0.097)**	**-0.004**** **(0.015)**	**-0.073**** **(0.036)**	**-0.105***** **(0.000)**
FCA_general	-0.062*** (0.004)	-0.069*** (0.008)	0.004 (0.822)	0.002 (0.897)	-0.019 (0.196)	-0.020 (0.137)
Par. Var.	-0.005** (0.045)	-0.005* (0.059)	0.005 (0.164)	0.005 (0.206)	-0.017 (0.814)	0.008 (0.914)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.650	0.652	0.646	0.650	0.646	0.650
Observations	32,082	32,082	35,849	35,849	35,849	35,849

This table presents the results for the cross-sectional variation of the effect of the state general FCA adoptions on customer identity disclosure based on the likelihood of attracting external whistleblowers. We construct a cohort sample around each FCA adoption year over the 9-year window of [-4, +4] and group all cohorts together. In each cohort, treatment firms are firms in states that pass general FCAs in that year, and control firms are firms in states that do not adopt a general FCA over the 9-year window. Columns (1) and (2) report the results with *Competition* as the partitioning variable, where *Competition* is defined as the percentage of sales from the top four firms in an industry based on U.S. Census data during the year prior to the FCA adoption year, multiplied by -1. Columns (3) and (4) report the results with *Local_media* as the partitioning variable, where *Local_media* is defined as the number of local newspapers in a firm's headquarters state during the year prior to the FCA adoption year. Columns (5) and (6) report the results with *Industry_regulation* as the partitioning variable, where *Industry regulation* is an indicator variable that equals 1 for the health care (SIC 5122 and 8000–8999), utilities (SIC 4900–4999), and transportation industries (SIC 3700–3799) during the year prior to the FCA adoption year, and zero otherwise. All continuous partitioning variables are decile-ranked. The sample period is from 1994 to 2010. Since we require both pre- and post-FCA observations, the final sample consists of the FCA adoption years in 1998, 1999, 2000, 2001, 2005, 2007, 2008, and 2009. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All regressions include firm, year, and headquarters state fixed effects. Standard errors are clustered at the headquarter states level. All continuous variables are winsorized at the 1% level. Other variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

Table 6 Business Exposure to Governments and Impact of State FCA Adoptions on Disclosure of Customer Identity

Dependent Variable	Log(1+*Ratio1*)	Log(1+*Ratio2*)
	(1)	(2)
FCA_general × *Gov_contractor*	**-0.030***	**-0.050****
	(0.079)	**(0.035)**
FCA_general	-0.019	-0.018
	(0.220)	(0.179)
Gov_contractor	0.018*	0.013
	(0.086)	(0.288)
Other control variables	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Adj. R-Squared	0.646	0.650
Observations	35,849	35,849

This table presents the results for the cross-sectional variation of the effect of the state general FCA adoptions on customer identity disclosure based on business exposure to the state government. We construct a cohort sample around each state general FCA adoption year over the 9-year window of [-4, +4] and group all cohorts together. In each cohort, treatment firms are firms in states that pass general FCAs in that year, and control firms are firms in states that do not adopt a general FCA over the 9-year window. The partition variable is *Gov_contractor*, an indicator variable equal to one if a firm reports government customers in segment reporting during the year prior to the FCA adoption year, and zero otherwise. The sample period is from 1994 to 2010. Since we require both pre- and post-FCA observations, the final sample consists of the FCA adoption years in 1998, 1999, 2000, 2001, 2005, 2007, 2008, and 2009. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All regressions include firm, year, and headquarters state fixed effects. Standard errors are clustered at the headquarters state level. All continuous variables are winsorized at the 1% level. Other variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

Table 7 Robustness Analysis

	Cohort sample		Dropping early adoption states	
Dependent Variable:	Log(1+*Ratio1*)	Log(1+*Ratio2*)	Log(1+*Ratio1*)	Log(1+*Ratio2*)
	(1)	(2)	(3)	(4)
Test variables:				
FCA_general	**-0.025***	**-0.024***	**-0.028****	**-0.027****
	(0.084)	**(0.055)**	**(0.031)**	**(0.021)**
Control variables:				
RD	-0.004*	-0.005*	-0.001	-0.001
	(0.095)	(0.055)	(0.454)	(0.551)
Intangibility	-0.014	-0.006	-0.009	0.000
	(0.320)	(0.673)	(0.440)	(0.995)
Advertising	0.127	0.087	0.045	-0.002
	(0.357)	(0.462)	(0.704)	(0.984)
HHI	-0.034	-0.010	-0.030	0.004
	(0.124)	(0.715)	(0.334)	(0.892)
Missing_RD	-0.029	-0.034	0.003	0.005
	(0.208)	(0.257)	(0.889)	(0.805)
MA	-0.009	-0.007	0.003	0.001
	(0.450)	(0.659)	(0.870)	(0.947)
SEO	-0.001	-0.007	0.003	-0.005
	(0.900)	(0.416)	(0.705)	(0.642)
Log(*Size*)	0.001	0.000	0.005	0.005
	(0.785)	(0.994)	(0.249)	(0.251)
Big_N	0.017	0.016	0.026**	0.025*
	(0.271)	(0.338)	(0.039)	(0.054)
GDP	0.000	0.000	-0.001	-0.001
	(0.814)	(0.794)	(0.576)	(0.447)
Unemployment	-0.005	-0.006	-0.007	-0.008
	(0.533)	(0.360)	(0.225)	(0.257)
Firm fixed effects	No	No	Yes	Yes
Year fixed effects	No	No	Yes	Yes
State fixed effects	No	No	Yes	Yes
Firm-event fixed effects	Yes	Yes	No	No
Year-event fixed effects	Yes	Yes	No	No
State-event fixed effects	Yes	Yes	No	No
Adj. R-Squared	0.654	0.660	0.604	0.609
Observations	34,820	34,820	14,173	14,173

This table reports the results of the tests to address the potential estimation bias in staggered difference-in-differences analysis. Columns 1 and 2 are based on the cohort sample used in Table 5. Columns 3 and 4 drop firm-years located in early adoption states (California, Illinois, and Florida) based on the main sample in Table 3. The regressions in Columns 1 and 2 include year-event, firm-event, and headquarters-state-event fixed effects; the regressions in Columns 3 and 4 include year, firm, and headquarters state fixed effects. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All continuous variables are winsorized at the 1% level. Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

Table 8 Falsification Analysis

	Medicaid FCA		Disclosure not helpful for detection	
Dependent Variable:	Log(1+*Ratio1*)	Log(1+*Ratio2*)	Log (1+*MF_EPS*)	Log (1+*MF_Non-EPS*)
	(1)	(2)	(3)	(4)
FCA_general	**-0.036****	**-0.038*****	**-0.017**	**-0.038**
	(0.012)	**(0.003)**	**(0.596)**	**(0.590)**
FCA_medicaid	**-0.018**	**-0.024**		
	(0.134)	**(0.192)**		
RD	-0.003**	-0.003**	-0.008***	-0.011***
	(0.049)	(0.040)	(0.001)	(0.003)
Intangibility	-0.007	-0.002	0.038***	-0.010
	(0.347)	(0.776)	(0.001)	(0.693)
Advertising	-0.073	-0.115*	0.120	-0.031
	(0.342)	(0.076)	(0.314)	(0.957)
HHI	-0.006	0.010	-0.055	-0.106
	(0.774)	(0.650)	(0.219)	(0.357)
Missing_RD	-0.009	-0.004	-0.008	-0.038
	(0.503)	(0.777)	(0.834)	(0.479)
MA	0.004	0.002	0.082***	0.037
	(0.704)	(0.872)	(0.001)	(0.269)
SEO	0.006	0.002	-0.053***	-0.050
	(0.298)	(0.849)	(0.000)	(0.305)
Log(*Size*)	0.003	0.005*	0.059***	0.120***
	(0.149)	(0.063)	(0.000)	(0.000)
BIG4	0.010	0.007	0.127***	0.101***
	(0.217)	(0.445)	(0.000)	(0.005)
GDP	-0.001	-0.001	-0.006**	-0.002
	(0.235)	(0.140)	(0.042)	(0.672)
Unemployment	-0.003	-0.002	0.011	-0.037*
	(0.482)	(0.609)	(0.176)	(0.070)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Adj. R-Squared	0.585	0.591	0.528	0.678
Observations	29,384	29,384	23,703	7886

This table reports the results of falsification tests. Columns 1 and 2 include the firm-years located in states that adopt Medicaid FCAs and present the results of estimating the effect of both general FCA adoption (*FCA_general*) and Medicaid FCA adoption (*FCA_Medicaid*) on customer identity disclosure. Columns 3 and 4 report the results of estimating the effect of general FCA adoptions (*FCA_general*) on management earnings forecasts (*MF_EPS*) and management non-earnings forecasts (*MF_Non-EPS*), respectively. The sample period is from 1994 to 2010 in Columns 1-3 and from 2004 to 2010 in Column 4. All regressions include firm, year, and headquarters state fixed effects. Standard errors are clustered at the headquarters state level. All continuous variables are winsorized at 1% level. Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

Table 9 Impact of Dodd-Frank on Disclosure of Customer Identities

Dependent Variable:	Log(1+*Ratio1*)	Log(1+*Ratio2*)
	(1)	(2)
Treat × *Post*	**-0.016**	**-0.024***
	(0.252)	**(0.078)**
RD	-0.000	-0.000
	(0.693)	(0.948)
Intangibility	-0.000	-0.003
	(0.979)	(0.837)
Advertising	-0.037	-0.191
	(0.845)	(0.298)
HHI	-0.014	0.014
	(0.820)	(0.824)
Missing_RD	-0.038	-0.045
	(0.452)	(0.391)
MA	0.009*	0.005
	(0.100)	(0.395)
SEO	0.009	0.012
	(0.231)	(0.194)
Log(*Size)*	0.001	0.001
	(0.820)	(0.817)
BIG4	0.005	0.003
	(0.707)	(0.795)
GDP	-0.000	0.001
	(0.975)	(0.463)
Unemployment	-0.002	-0.000
	(0.792)	(0.991)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Adj. R-Squared	0.860	0.859
Observations	4,742	4,742

This table reports the results of estimating the effect of the Dodd-Frank whistleblower program on customer identity disclosure. The sample period is from 2009 to 2013 (2 years before and 2 years after the adoption of Dodd-Frank whistleblower laws). Treatment (control) firms are firms headquartered in states that had not (had) adopted state FCAs before the Dodd-Frank Whistleblower provision was enacted. *Treat* is an indicator variable that equals 1 if a firm is a treatment firm, and 0 otherwise. *Post* is a dummy variable that equals 1 for the years in and after 2011, and 0 otherwise. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All regressions include year, firm, and headquarters state fixed effects. Standard errors are clustered at the headquarters state level. All continuous variables are winsorized at the 1% level. Variable definitions are in Appendix A. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table 10 Impact of State FCA Adoptions on Subsidiary Disclosure

Dependent Variable:	Log(1+# *of foreign subs*)	Log(1+# *of subs in tax heaven*)
	(1)	(2)
FCA_general	**-0.090****	**-0.066***
	(0.013)	**(0.055)**
Log(size)	0.299***	0.237***
	(0.000)	(0.000)
Leverage	0.048	-0.038
	(0.512)	(0.543)
GDP	-0.001	-0.003*
	(0.671)	(0.087)
Unemployment	0.041***	0.025
	(0.007)	(0.101)
Book-to-Market	0.011	0.006
	(0.363)	(0.451)
PPE	0.301**	0.185
	(0.036)	(0.255)
ROA	-0.170***	-0.136***
	(0.000)	(0.000)
MA	0.056***	0.027
	(0.004)	(0.134)
Big4	0.002	0.065**
	(0.937)	(0.015)
Analyst following	0.005**	0.003
	(0.033)	(0.137)
TAX(<=0)	0.003	-0.005
	(0.770)	(0.726)
Foreign Income	0.037***	0.042***
	(0.004)	(0.001)
Missing Foreign Income	-0.193***	-0.099***
	(0.000)	(0.000)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Adj. R-Squared	0.869	0.823
Observations	19,085	19,088

This table reports the results of estimating the effect of the state general FCA adoptions on firms' subsidiary disclosure. We estimate Equation (1) using the log transformation of the number of disclosed foreign subsidiaries (# *of foreign subs*, Column 1) and the number of disclosed subsidiaries in tax havens (# *subs in tax heaven*, Column 2) as the dependent variables and a modified set of control variables from Dyreng et al. (2020). The sample period is from 1994 to 2010. We exclude firm-years headquartered in states that adopt Medicaid FCAs. All regressions include firm, year, and headquarters state fixed effects. Standard errors are clustered at the headquarters state level. All continuous variables are winsorized at the 1% level. Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.