Do Corporate Whistleblower Laws Deter Accounting Fraud?

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Abstract

This paper examines the deterrence effect of whistleblower laws on the probability of accounting fraud. We exploit a quasi-experimental design using state False Claims Acts (FCAs), under which reporting evidence of fraud at a firm invested in by a state's pension fund can result in monetary rewards from that state government. Using staggered adoption of FCAs by states between 2001 and 2010, we compare the accounting fraud probability of firms owned by state pension funds after the state adopts an FCA to that of firms not exposed to any state's FCA. We find that firms' exposure to FCAs and hence a higher risk of whistleblowing reduces the likelihood of accounting fraud by 5% to 9%. Consistent with the reduced likelihood of fraud lowering the risks of auditing the affected firms, audit fees are 4.5% to 6% lower after a firm is exposed to state FCAs relative to firm-years not treated by FCA exposure. Conversely, using proprietary data on internal audit activities for a small subset of our sample, we find little evidence consistent with increased investment in internal auditing being a mechanism by which whistleblowing exposure lowers fraud risk. Finally, we find the introduction of the federal Dodd-Frank whistleblower provision by the SEC in 2011 reduces the probability of accounting fraud to a greater extent for firms that had not been affected previously by state FCAs, corroborating the deterrence effect of the state and federal whistleblower laws.

JEL Classification: G34; G38; K22; M41; M48

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1. Introduction

> "Complex economic wrongdoing cannot be detected or deterred effectively without the help of those who are intimately familiar with it. Law enforcement will always be outsiders to organizations where fraud is occurring. They will not find out about such fraud until it is too late, if at all… Given these facts **insiders who are willing to blow the whistle are the only effective way to learn that wrongdoing has occurred**…"
>
> -The 2008 Senate Judiciary Committee Report [emphasis added]

After several high-profile financial frauds in recent years, regulators have taken steps to strengthen whistleblower provisions. For example, the Sarbanes-Oxley Act (SOX 2002) mandates increased whistleblower protections and compliance monitoring, and the Dodd-Frank whistleblower program (implemented by the U.S. Securities and Exchange Commission (SEC) in 2011), provides financial rewards to whistleblowers who report financial fraud directly to the SEC.[1] Although regulators believe these whistleblower laws can deter accounting fraud, some question their effectiveness because whistleblower tips can be frivolous and handling them is costly. Despite the growing attention to this debate, relatively little is known about the effectiveness of whistleblower laws in deterring accounting fraud.

We examine whether whistleblower laws have a causal effect on the deterrence of fraud. Our identification strategy exploits the novel setting of U.S. states' staggered adoption of whistleblower laws called False Claims Acts (FCAs hereafter). We examine whether the underlying (or *ex ante*) likelihood of accounting fraud in a firm indeed decreases after exposure to

[1] The Enron and Madoff cases (which, in turn, motivated passage of SOX and Dodd-Frank) involve internal and external whistleblowers, respectively. In the Enron scandal, the Enron vice president, Sherron Watkins, first called Enron's accounting into question and warned the then-Enron CEO, Kenneth Lay, of accounting irregularities. In Madoff's Ponzi scheme, a financial analyst, Harry Markopolos, attempted to bring the fraud to light by providing tips to the SEC several times, which the SEC overlooked.

an FCA. Then we study a possible mechanism behind this deterrence effect using a proprietary data set of firms' internal control practices.

Unlike prior studies that have used detected fraud (e.g. Bowen et al. 2010; Dyck et al. 2010; Call et al. 2018), we measure the underlying probability of fraud by using proxies of the probability of accounting manipulation: the F-score (Dechow et al. 2011) and M-score (Beneish 1999).[2] One empirical challenge in an investigation into the deterrence effect of whistleblower laws is that accounting fraud is unobserved unless and until it is detected (Dyck et al. 2017; Gow et al. 2016). Thus, using detected (or *ex post*) fraud would not allow us to accurately quantify how a firm's propensity to engage in accounting fraud changes in response to exposure to a whistleblower law.

Another important challenge in empirical research concerning the causal impact of whistleblower laws is that federal laws such as SOX and the SEC's Dodd-Frank whistleblower program are applied to all public U.S. firms at almost the same time. In such a setting, it is hard to find an appropriate control group and isolate the deterrent effect from other concurrent events. We address this challenge with a quasi-experimental design by using the staggered passage of FCAs by U.S. state governments. An FCA whistleblower law aims to protect whistleblowers who bring to light fraud against a government and offers financial rewards to them. Thus, fraud at a firm that is invested in by a state government via the state's pension funds can be interpreted as defrauding the state government (Rapp 2007). A firm engaging in fraud is regulated by an FCA law when both of the following are true: (1) a state adopts (or has previously adopted) an FCA provision covering accounting fraud and (2) at least one state-sponsored pension fund from that state begins

[2] We use these measures because they can be easily calculated using financial statement variables and have reasonable predictive power. Dechow et al. (2011) document that their prediction model correctly classifies about 70% of misstating firms as misstating (i.e., the type II error rate is about 30%). Beneish et al. (2013) show that in the out-of-sample test, Beneish (1999)'s earnings manipulation detection model correctly predicted 12 of the 17 most famous earnings manipulators during 1998-2002 including Enron. In the results section, we will show their validity as measures of underlying fraud likelihood.

to invest in (or already was invested in) the firm (regardless of whether or not the firm is headquartered in the same state as the pension fund). Thus, because FCAs were adopted by states at staggered times, the extent to which a firm is influenced by FCA laws varies depending on state fund ownership in the firm and variation in state FCAs, such as the year of passage and the scope of FCA coverage. In sum, we identify firms that are more or less subject to state FCA whistleblower laws and examine these laws' causal effect on the likelihood of accounting fraud.

We predict that, faced with the increased risk of whistleblowing after exposure to state FCAs, managers have stronger incentives to avoid accounting misstatements and improve financial reporting quality. In addition, because both the Department of Justice and the SEC place a premium on a firm's self-disclosure of problems, managers would want to uncover existing problems before whistleblowers report and regulators investigate. Hence, the FCA whistleblower provisions can create powerful incentives for companies to tighten their internal controls to detect and correct ongoing fraud. As a result, the likelihood of accounting fraud occurrence will decrease.

Our research design uses 24 separate state pension funds located in 16 states. We observe firms with shares that were owned by at least one state pension fund located in a state with an FCA between 2001 and 2010, and view them as being exposed to a higher risk of whistleblowing under state FCAs. We find the probability of fraud, as measured by the F-score and M-score, decreases by 5% to 9% when firms are owned by FCA state pension funds, suggesting that firms' exposure to a state FCA regulation has the deterrence effect.[3] In additional tests, we find changes in revenue and accrual-related subcomponents of the F-score and M-score are the primary drivers of the

[3] The identifying assumption is that firms that are exposed to a whistleblower provision and firms that are not would have otherwise similar changes in our measures of fraud probability. We estimate a series of robustness tests to ensure that measurement error in the F-score and M-score is not systematically correlated with the measure of exposure to whistleblowing provisions (i.e., changes in state fund ownership and the passage of an FCA by that state). We also have a test addressing the concern that the scores may capture nondiscretionary or real effects in addition to discretionary effects. We provide the results in Section 5.2.

decrease in the probability of fraud, consistent with the prior literature showing that revenue recognition is the most common type of accounting misstatement (Files et al. 2009; Dechow et al. 2011). Note that unlike SOX, which imposed internal control reporting and changed corporate governance, the state FCAs do not impose any obligations on companies. Therefore, observed effects at a firm are attributable solely to the risk of whistleblowing under state FCAs.

It is an underlying assumption for the aforementioned FCA deterrent effect that managers (or their advisors) are aware of which state pension funds own their company's stocks and of each state's FCA statute. The pension fund literature has found that public pension funds (even relatively passive pension funds) influence firm governance through shareholder proposals and informal discussions (Guercio and Hawkins 1999; Appel et al. 2016; Schmidt and Fahlenbrach 2017). Moreover, Guercio and Hawkins (1999) document that firms targeted by pension funds tend to experience more future shareholder lawsuits relative to control firms. This line of evidence suggests that firm managers have incentives to keep track of ownership changes by public pension funds and the legal liability consequences associated with their ownership.

We investigate whether this assumption is indeed borne out in the data by examining whether the deterrence effect is stronger when managers are more likely to be aware of pension funds' investment. We predict that when a higher percentage of shares of a firm is owned by a particular state's pension fund, the managers pay more attention to that state's legal environment.[4] Consistent with this prediction, we find evidence that higher ownership by an FCA state pension fund reduces the probability of fraud by more.

Moving past presenting the main effect, we also test a plausible mechanism behind the deterrence effect. First, we rely on the audit risk model (Statement on Auditing Standards No. 47,

[4] One reason a state pension fund's stock purchase can cause firms to be more attentive to that state's regulatory and legal environment is that public pension funds are potentially subject to political pressure (Romano 1993).

American Institute of Certified Public Accountants [AICPA] 1983). It suggests that if managers improve internal controls aimed at detecting accounting misstatement or fraud, fraud risk will decrease. In such an event, to maintain overall audit risk at a certain level, auditors will reduce their effort level, thereby charging less fees (Raghunandan and Rama 2006; Hogan and Wilkins 2008; Hoitash et al. 2008; Doogar et al. 2010; Hoag and Hollingsworth 2011; Aobdia et al. 2016, Cassell et al. 2016). Thus, we predict that exposure to a higher risk of whistleblowing under FCAs will reduce audit fees as a result of the lowered fraud risk. Consistent with this prediction, we find audit fees are 4.5% to 6% lower after a firm is exposed to state FCAs relative to firm-years not treated by FCA exposure. In addition, using proprietary data on internal audit activities for a small subset of large firms, we observe whether managers invest more resources in their internal audit activities to deter fraud. We find little evidence that internal audit costs, hours and staff size increase with the company's exposure to FCA regulation, inconsistent with concerned managers tightening their internal controls.

To validate our FCA setting as an appropriate instrument of increased risk of whistleblowing, we run a series of robustness tests. First, the documented effect could reflect state pension funds selecting to invest in firms about to reduce their fraud likelihood rather than FCA exposure deterring fraud. However, the hypothesized FCA deterrence effect occurs only when firms are owned by state pension funds of states with a general FCA provision that covers accounting fraud. This implies that in a falsification test where we replace the treatment variable with firms' exposure to a Medicaid FCA (an FCA that protects against Medicaid fraud only), the FCA should have no impact on the probability of fraud. In other words, if pension funds' stock selection explained the main effect, we should continue to see the same result in this falsification test because the measure of exposure to a Medicaid FCA is also based on funds' stock investments

in firms. However, we find no effect on audit fees associated with the Medicaid FCAs, although we do find some evidence of an effect on fraud probability.

In addition, prior literature finds that state pension funds have home bias when they invest (Brown et al. 2015). If such home bias is extreme, state fund ownership may proxy for the location of a firm's headquarters state instead of the effect of the state's whistleblower law. If this is the case, it raises the further concern that states' FCA adoptions are endogenous. To address this concern, we repeat our main tests using only firm-years in which states with state pension fund ownership of the firm differ from the firm's headquarters state and find the results remain unchanged. Finally, the deterrence effect of whistleblowing risks remains stable even after we include separate indicator variables for ownership of each state pension fund or funds' state in addition to firm and year fixed effects, ruling out the possibility of a particular fund or FCA state driving our results.

Last, to corroborate the deterrence effect of whistleblower laws under FCAs and to see whether this effect is generalizable to other whistleblower laws, we use the SEC's Dodd-Frank whistleblower program implemented in 2011. Similar to FCAs, the program offers financial rewards to whistleblowers if their tips to the SEC lead to successful enforcement actions. Using firms that were already exposed to state FCA provisions as a control group, we expect that firms not previously exposed to any state FCAs will respond more strongly to the new federal whistleblower program because it is their first exposure to whistleblower laws with a bounty model. This approach allows us to evaluate the effectiveness of the SEC provision, which would otherwise be difficult because the federal rule simultaneously affected all U.S. firms subject to securities law violations (Baloria et al. 2017; Wiedman and Zhu 2018). We find that when firms not yet exposed to state FCAs are exposed to the SEC whistleblower provision, the probability of fraud decreases

by 7% and audit fees decrease by 4%, relative to firms that were already exposed to FCAs. The treatment effects are very close to the effects estimated in the FCA setting, corroborating the deterrence effect of whistleblower laws. Moreover, although the falsification test using Medicaid FCAs did not eliminate the possibility of something other than the whistleblowing threat driving the effect of general FCAs on lowering fraud risk, the SEC whistleblower provision results provide considerable comfort that the whistleblowing threat indeed explains our finding that general FCA exposure results in reduced fraud probability.

We contribute to the accounting, finance, and law literatures by being the first to provide causal identification of the impact of whistleblowing legislation on the probability of a firm conducting fraud. Establishment of the causal link and the resulting estimates of the magnitude of the benefit of whistleblowing legislation are needed to inform cost/benefit analysis of such rules (Leuz and Wysocki 2016). Prior research supports the association of whistleblowing legislation with benefits, but provides neither causal evidence nor estimates of the magnitude of the benefits in the population of affected firms. Thus, a causal identification approach that exploits staggered treatment in combination with *ex ante* measures of fraud probability are critical to assessing the magnitude of the fraud probability reduction benefit of whistleblowing legislation. As such, we complement Wilde (2017), who employs an *ex post* approach to examining the association of employee whistleblowing events with firms' financial misreporting and tax aggressiveness.

Further, we document the role of internal auditing and internal controls as a mechanism for reducing fraud risk in response to whistleblowing laws and examine how external audit fees are affected. These results add to the finding in Ege (2015) that misconduct firms improve their internal audit quality after their misconduct is revealed. Finally, we contribute to the policy debate around the bounty modqel of whistleblowing laws by providing one crucial input (quantification

related to the benefit side) of the debate. Iwasaki (2018) shows that the bounty model can cause a trade-off between employee internal whistleblowing and external whistleblowing by incentivizing employees to bypass internal reporting systems and directly go to the regulatory agency for financial rewards. Moreover, given internal reporting can help managers identify and correct the problems before they become more serious and costly (Stubben and Welch 2018), the net cost/benefit of the bounty model is ambiguous. Baloria et al. (2017) and Wiedman and Zhu (2018) show the SEC whistleblower provision, which includes financial incentives, is associated with benefits. We, however, are the first both to establish a causal link for these benefits as well as to estimate their magnitude. We do so for both the state FCA whistleblowing laws as well as the federal Dodd-Frank whistleblowing legislation more recently implemented by the SEC.

2. Related Literature, Institutional Background, and Hypothesis Development

2.1. Whistleblowing Literature

Following a series of corporate scandals and subsequent legislation on whistleblower programs, recent studies provide evidence related to whistleblowing in uncovering detected financial frauds. Dyck et al. (2010) document types of whistleblowers. Although the corporate governance literature has given little attention to the governance role played by employees (Dyck et al. 2010; Rapp 2010), they are the most common type of whistleblower (12% of a 216 corporate fraud sample during 1996-2004), followed by media, analysts, and short-sellers. Bowen et al. (2010) investigate the characteristics of firms caught by employee whistleblowers, and the economic consequences of the whistleblowing events. They find these firms tend to be large and growing firms with relatively weak internal or external monitoring and have had recent employee layoffs. They also document that, following whistleblowing allegations, firms tend to experience a negative market reaction, restate their earnings, and become subject to shareholder litigation.

Call et al. (2018) find employee whistleblowers play an important role in the investigation process by providing valuable information to regulators and facilitating enforcement actions against target firms. They show that whistleblower involvement is associated with more severe enforcement actions, including more penalties and longer prison sentences.

Baloria et al. (2017) evaluate the SEC whistleblower provision by examining whether this provision is beneficial to shareholders. They compare lobbying firms that lobbied against the provision to non-lobbying firms and find lobbying firms tend to have weaker internal whistleblowing programs. The lobbying firms experience more positive market reaction around the dates related to the implementation of the regulation, consistent with the provision being expected to improve shareholder protection more for these firms. While Baloria et al.'s (2017) evidence indicates the shareholders of a more affected group of firms expected a greater net benefit from the SEC whistleblower provision, its design does not allow for an examination or quantification of the sources of benefit or of the mechanisms by which the benefits occur.

Wilde (2017) examines whether employee whistleblowing allegation events subsequently deter fraud due to the threat of increased monitoring. He finds that, following such allegations, firms tend to reduce financial misreporting and tax aggressiveness for at least two years relative to their matched control firms. Wilde provides evidence that employee whistleblowing actions about past misreporting deter future misreporting and are thus suggestive that, more broadly, the threat of whistleblowing could significantly deter fraud. We investigate whether this is indeed the case, quantify the deterrent effect, and examine the role of internal controls as a mechanism for the effect. Moreover, unlike prior studies using fraud cases uncovered by employee whistleblowers, the inference of our paper is not limited to employee whistleblowing as anyone knowing evidence of fraud can be a whistleblower under an FCA.

Wiedman and Zhu (2018) examine the deterrent effect of the SEC whistleblower program using Canadian firms as a control group. They document reductions in aggressive financial reporting that are greater for U.S. firms than for the Canadian control group and that are bigger for U.S. firms with weaker internal control programs. These findings suggest that whistleblower programs with a bounty model may have a deterrent effect on fraud as well as on aggressive reporting and that one mechanism by which they may do so is via incentivizing firms threatened with whistleblowing to improve their internal controls.

Most whistleblowers come forward for ethical reasons. For example, they choose to blow the whistle to maintain personal integrity, avoid complicity, and protect the public (Benabou and Tirole 2006; Arce 2010; Rapp 2012b). However, they simultaneously face great social and economic pressure to remain silent (Call et al. 2016). Social bonds with their co-workers and loyalty to employers influence whistleblowers to stay silent (Rapp 2010; Rapp 2012). In addition to such a conflict between individual and organizational values, whistleblowers also incur significant economic costs, such as legal costs and loss of jobs and reputation (Rapp 2012).

Although this study aims to quantify one benefit of whistleblowing provisions – the deterrence effect, there are costs to be considered in the cost-benefit analysis of these policies as well. When Congress proposed the SEC's whistleblower program, for example, many practitioners were concerned that whistleblowers would bypass corporate internal reporting systems in favor of reporting directly to the SEC, jeopardizing the internal hotlines and compliance mechanisms (Archambeault and Webber 2015; Stubben and Welch 2018). Some disgruntled employees could abuse the provision by reporting frivolous or misleading complaints to obtain bounties. Dealing with such cases is costly for both firms and regulators. Moreover, some unintended outcomes could arise, such as hiring friends who are less likely to blow the whistle or granting stock options to

employees to tie their financial incentives to the firm's stock value that increases when they remain silent about wrongdoing (Call et al. 2016).

Although the deterrent function of private securities lawsuits in corporate governance has been studied extensively, the literature on the role of whistleblowers as a corporate governance mechanism is relatively small. Many private securities lawsuits are filed too late to prevent fraud, because most of them are driven by earnings restatements or stock price drops, not by evidence of fraud (Rapp 2010). Moreover, restating earnings can be risky in the securities litigation process because a plaintiff can use voluntary disclosures as evidence of managers' misconduct (Rogers and Van Buskirk 2009). On the other hand, whistleblowing is about providing regulators with evidence of ongoing misconduct or potential fraud. Thus, whistleblowers may play a more effective role as a corporate governance mechanism than private securities lawsuits (Rapp 2010).

2.2. The False Claims Act and State Pension Funds

To compensate for economic and social costs that whistleblowers face, state and federal governments have adopted whistleblower laws that include anti-retaliation and bounty provisions. The federal False Claims Act is the oldest U.S. whistleblower law.[5] Under the federal FCA, it is unlawful for any entity to knowingly submit a fraudulent claim to the government or defraud the government. In 1986, the federal FCA was amended to include a *qui tam* bounty provision and a "dual plaintiff" structure. Under the *qui tam* provision, a private citizen (also called a "relator") can file a lawsuit on behalf of the U.S. government and obtain a portion of the money recovered. The "dual plaintiff" structure allows whistleblower plaintiffs to continue the lawsuit and obtain financial bounties even if the federal government declines to intervene (Rapp 2012b). If that

[5] The federal FCA was enacted in 1863 in response to fraud against government, such as providing defective weapons to the Union Army. It is also known as the "Informer's Act" or the "Lincoln Law" (Rapp 2012b).

lawsuit is successful, a relator receives 15%-25% of the recoveries if the government joins the case, or 25%-30% if the relator pursues the case on his or her own.

Given the wide range of conduct the federal FCA covers, no single rule exists for determining damages under the act. However, defendants are responsible for treble damages (the amount lost ×3) and costs incurred by the whistleblower or government in prosecuting the case, and also required to pay a mandatory penalty.[6] Taken together, these features of the federal FCA have made the act incredibly successful in encouraging more whistleblowers to bring fraud cases.[7] Recently, more whistleblowers have begun to pursue cases even after the government has decided to step back from intervening, implying federal FCA defendants need to take federal FCA cases seriously.[8]

Most federal FCA cases are concentrated in the healthcare or defense industries in which the government is a major buyer (Bowen et al. 2010; Dyck et al. 2010).[9] Until the passage of SOX, financial fraud against shareholders was not considered a matter of public concern and was not protected under whistleblower laws (Rubinstein 2007). Under the FCA theory, securities fraud does not directly harm the federal government.

On the other hand, state governments view financial fraud differently. Unlike the federal government, state governments can invest their funds in publicly traded companies in the form of

[6] www.morganlewis.com

[7] As of 2015, whistleblowers have filed more than 10,000 federal *qui tam* suits since the 1986 amendments, resulting in more than $33 billion in recoveries. Of that sum, whistleblowers have received $5.3 billion in awards. Whistleblowers initiated approximately 86% of the federal FCA cases filed in 2015 (632 out of 737) (www.gibsondunn.com).

[8] In 2015, 32% of total *qui tam* suit recoveries came from cases in which the government declined to intervene but the whistleblower continued to pursue on the government's behalf (www.gibsondunn.com).

[9] Because the FCA provides monetary rewards to whistleblowers, some studies take the view that employees in healthcare and defense industries (which receive a large amount of government spending) have greater incentives to blow the whistle under an FCA. They use this industry classification as a measure of whistleblowers' monetary incentives (Bowen et al. 2010; Dyck et al. 2010). Our paper, however, focuses on not-yet-exploited features of the FCA that also apply to non-healthcare and non-defense industries. To make sure our results are not driven by the healthcare and defense industries, we drop these industries from the sample.

state retirement or pension funds. Then financial fraud involving the state pension funds becomes a subject of false claims against the state government (Rapp 2007).[10] In this case, whistleblowers can claim financial rewards under that state's FCA by providing evidence of securities fraud involving the state funds' investment, as long as the state has an appropriate *qui tam* provision.[11]

States have adopted their own versions of FCAs modeled after the federal FCA at staggered times since 1987 and their FCAs vary in terms of the coverage of fraud (Appendix B). As of 2010, 17 states and the District of Columbia have FCAs that covers fraud in general including financial fraud (*general* FCAs hereafter) whereas 11 states have FCAs that protect against only Medicaid fraud (*Medicaid* FCAs hereafter). The remaining 22 states have not yet adopted FCAs. This paper focuses on the general FCA because financial-fraud whistleblowing can be rewarded only when a state has a general FCA (i.e., an FCA that includes a general *qui tam* provision).[12] By contrast, a Medicaid FCA is limited to Medicaid fraud, such as paying kickbacks to pharmacies or doctors using money funded through the Medicaid program of state government. Whether the threat of whistleblowing on Medicaid fraud can influence managers' incentives to misstate earnings is less obvious. For example, recent state FCA lawsuits against the Bank of New York Mellon were filed by the California, New York, Florida, and Virginia state pension funds. These are states that adopted a general FCA rather than a Medicaid FCA. Although these lawsuits were against a bank, this example has implications for other non-bank firms in that their involvement in financial fraud

[10] For example, the state of New York passed a general FCA in 2007. According to the website of the Attorney General of New York (http://www.ag.ny.gov), "the New York State False Claims Act provides incentives for whistleblowers to report matters where governmental entities, such as **pension funds**, have been defrauded. The provision allows a whistleblower to recover between 15 and 25 percent of the recovery made on behalf of New York" (emphasis added).
[11] See Appendix B for state-by-state provisions.
[12] General FCAs cover both Medicaid fraud and non-Medicaid fraud including financial fraud. For example, California, which adopted a general FCA in 1987, has recovered approximately $254 million ($353 million) from (non-)Medicaid cases since 1999 (Bucy et al. 2010).

using money invested by state pension funds can also be a target of whistleblowing only under general FCAs (Rapp 2007; Rapp 2010).

2.3.The SEC's Dodd-Frank Whistleblower Provision

The SEC created the whistleblower program as one of the main provisions of the Dodd-Frank Act (Section 21F). The Dodd-Frank Act 924(d) directs the SEC to establish a separate office within the SEC's division of enforcement to administer the whistleblower program. The program went into effect on August 12, 2011, and whistleblower tips reported after the implementation date are eligible for financial awards. The goal of the program is to receive high-quality tips on securities law violations that can be used to detect and halt fraud earlier. Anyone who has information concerning a potential securities law violation can submit tips to the SEC.

The key features of the SEC whistleblower statute are that it targets financial fraud and has a bounty provision. Whistleblowers are eligible for monetary rewards of 10%-30% of the cash collection if the information an individual provides leads to a successful enforcement action and monetary sanctions exceeding $1 million. This bounty program took the whistleblower protection of SOX several steps further. SOX was the first federal whistleblower statute to cover financial fraud, but a mere anti-retaliation provision of SOX was not enough to incentivize employee whistleblowers to risk their careers (Rapp 2012b). Even though SOX also mandated a confidential complaint process for employees to report fraud and accounting irregularities directly to the audit committee, most internal complaints are unobservable and may not be acted on (Kohn 2013). Moreover, SOX's anti-retaliation provision does not protect whistleblowers outside of the firm.

The SEC whistleblower statute of 2011 also allows individuals to report tips directly to the SEC anonymously to protect them from their employer's retaliation. To address the concern that allowing whistleblowers to report directly to the SEC can jeopardize a firm's internal compliance

or hotline program, the SEC increases payment to those who reported internally first. The number of whistleblower tips has been increasing each year since the inception of the program in 2011, and many of them are related to manipulation of accounting and financial numbers.[13]

2.4. Hypothesis Development

The Deterrent Effect of FCA Whistleblower law

To study the deterrent effect of state FCAs, we exploit the staggered passage of an FCA by states and variation in state pension funds' ownership of firms to identify the extent to which a firm is exposed to state FCAs. Treated firms are those exposed to a general FCA and hence a higher risk of whistleblowing about financial fraud after a state pension fund located in a state with a general FCA owns their shares.

We expect managers of treated firms who are concerned about a whistleblower reporting their misconduct to the regulator under an FCA would be less inclined to engage in fraud. Even managers not involved in fraud would be concerned, because they are unaware of all ongoing fraud that potential whistleblowers know about. These concerned managers likely tighten internal controls to detect ongoing fraud and correct it before whistleblowers move first. For example, concerned managers may assess internal controls and discover their inventory and net receivables assets are overstated. Then they likely write down these accounts to avoid potential whistleblowing. In this process, management may devote more resources to internal controls. Taken together, faced

[13] Since its inception in 2011, the whistleblower program has received 3,001 whistleblower tips in 2012, 3,238 tips in 2013, 3,620 tips in 2014, 3,923 tips in 2015, 4,218 tips in 2016, 4,484 tips in 2017, and 5,282 tips in 2018. The SEC has collected $1.7 billion in total monetary sanctions and awarded over $326 million to 59 individual whistleblowers since the beginning of the program. During 2018, the most common complaints were about offering fraud (20%), corporate disclosures and financials (19%), and manipulation (12%), followed by trading and pricing, insider trading, Foreign Corrupt Practices Act (FCPA), and so on. See https://www.sec.gov/sec-2018-annual-report-whistleblower-program.pdf for details.

with enhanced risk of whistleblowing under the FCA, concerned managers will take action in a way that reduces the likelihood of accounting fraud:

H1: *The probability of accounting fraud of a firm decreases after firms are exposed to a general state FCA.*

The Mechanism of the Deterrence Effect

To better understand the mechanism behind the deterrence effect, we examine whether concerned managers take actions to prevent whistleblowing as described above. First, according to the audit risk model (Statement on Auditing Standards No. 47, American Institute of Certified Public Accountants [AICPA] 1983), auditors adjust their effort level in the event of lowered risk of material misstatement to maintain the overall audit risk at a certain level. For example, they will likely reduce the testing of accounts and the number of audit hours, or assign less experienced staff to the audit, all of which manifest in lowered audit fees (Raghunandan and Rama 2006; Hogan and Wilkins 2008; Hoitash et al. 2008; Doogar et al. 2010; Hoag and Hollingsworth 2011; Hribar et al. 2014; Aobdia et al. 2016, Cassell et al. 2016). In other words, if management is no longer involved in accounting fraud or improves internal controls over financial reporting due to heightened whistleblowing risks, the lowered fraud risk for auditors should be priced in equilibrium. It is also likely that auditors charge lower fee premiums when they are less susceptible to potential reputational loss or legal liability due to lowered clients' fraud risk. Therefore, we predict that when fraud risk decreases as a result of managers' efforts to improve internal controls in the face of a heightened risk of whistleblowing under FCAs, audit fees are lowered.

H2(a): *Audit fees are lower after a firm is exposed to a general state FCA relative to firm-years not treated by FCA exposure.*

Next, we examine the internal control mechanism more directly. Prawitt et al. (2012) find firms' internal audit quality is negatively associated with the risk of accounting misstatement and Ege (2015) finds that misconduct firms improve their internal audit quality after their misconduct is uncovered. Following this line of findings, we examine whether managers invest more in their firm's internal control activities to detect and prevent accounting misstatement or fraud when they believe there is a higher risk of getting caught by whistleblowers. For example, the concerned managers may hire more internal auditors or increase training costs for internal audit employees as an effort to detect ongoing fraud. However, if managers are committing fraud, they may simply correct the problem and change their tone in the financial statements without exerting additional efforts to improve internal controls. Therefore, we test whether the costs associated with internal audit activities and internal audit hours indeed increase after firms are exposed to an FCA.

H2(b): *Firms invest more in internal audit activities after they are exposed to a general FCA.*

The SEC Whistleblower Program

To corroborate the deterrent effect of whistleblower laws, we also examine the SEC whistleblower program. Both state FCA and the SEC whistleblower statutes have bounty provisions, which are important to motivate whistleblowers to come forward. However, unlike the state FCA setting, which varies in the year of passage and types of fraud protected against (general vs. Medicaid), the SEC whistleblower program, as a federal rule, targets all U.S. companies engaging in securities fraud. Moreover, given that the program was created as a part of the Dodd-Frank Act (2010), it is difficult to isolate which firms are more or less likely to be affected by the SEC whistleblowing statute in terms of identification strategy (Baloria et al. 2017; Wiedman and Zhu 2018). To better address this identification challenge faced by the prior literature, we rely on the setting of state FCAs to define which firms are more affected by the federal SEC provision.

We split firms based on whether they had been exposed to any state FCA general whistleblower provision before the SEC whistleblower program went into effect. We predict that for firms that were not exposed to a whistleblowing risk through state FCA pension funds before the SEC whistleblower provision (i.e., during 2008-2010), the impact of the SEC provision should be greater because it is their first time being exposed to such a shock. Managers in such firms will likely consider the new provision a greater risk than control firms that were already exposed to general state FCAs and will take more actions to improve internal control systems and reduce the likelihood of fraud.

H3: *After the Dodd-Frank whistleblower regulation, the probability of accounting fraud of a firm decreases more for those that have not yet been exposed to any general state FCAs.*

3. Sample Selection

As summarized in Table 1 Panel A, our initial sample consists of U.S. public equity holdings of state pension funds disclosed in Thomson Reuters 13F institutional holdings filings from 2000 through 2014. After merging them with COMPUSTAT variables, we aggregate the sample at the firm-year level because a firm is held by multiple funds in a given year. We also keep firms that are not matched with 13F filings (i.e., those that were not owned by any state pension fund during the entire, or a portion of the sample period). Thus, we can keep treatment firms that became exposed to FCAs as FCA state funds started buying their shares, and control firms that remained independent of any state pension funds. We exclude firms in the healthcare industry to ensure healthcare companies that are subject to Medicaid FCAs do not drive the treatment effect of general FCAs. We also eliminate financial firms to be consistent with Dechow et al. (2011) who did not include them when computing the F-score. After we merge the sample with Audit Analytics and the COMPUSTAT business segment data, the final sample has 22,645

firm-year observations with 3,233 unique firms during 2001 through 2014. In the analysis using state FCAs, we use 18,543 firm-years (3,164 unique firms) during 2001 to 2010. In the analysis using the SEC whistleblower provision, we use 7,016 firm-years (1,105 unique firms) during 2008 to 2014.

We obtain proprietary data on firms' internal audit activities from the Institute of Internal Auditors' Audit Intelligence Suite (AIS) database.[14] The AIS dataset consists of survey responses from firms' chief audit executives who are seeking to access benchmarking metrics. The Institute of Internal Auditors provides aggregated benchmarking numbers on selected metrics to the member organizations. Under the conditions of anonymity and confidentiality, we were granted access to individual-level response data from fiscal years of 2005 to 2015. In the final sample, 369 (288) firm-years are matched with our FCA sample during 2005 to 2010 for internal audit costs (internal audit hours).[15]

4. Research Design

4.1. The Measure of Probability of Accounting Fraud

To examine the deterrent effect of whistleblowing laws, we would ideally first observe all ongoing fraud and see whether its amount or severity declines after firms become subject to whistleblower laws. However, fraud is inherently unobservable, and studying *detected* fraud does not tell us how much undetected fraud is occurring (Dyck et al. 2017). Moreover, because whistleblower laws are expected to reduce the likelihood of fraud and increase the likelihood of detection, the observed detected fraud, which is the net of these two opposing effects, is not

[14] The AIS was formerly known as the Global Auditing Information Network (GAIN). The GAIN dataset was used in a handful of papers (Prawitt et al. 2009; Lin et al. 2011; Messier et al. 2011; Prawitt et al. 2012; Ege 2015).
[15] One limitation of using the AIS dataset is the considerable reduction its use creates for our sample size.

informative about the changes in underlying fraud. Therefore, we rely on imputed measures of fraud probability as closer approximations to underlying fraud, using the F-score (Dechow et al. 2011) and M-score (Beneish 1999).

We calculate the F-score using a prediction model based on financial statement variables capturing accrual quality (non-cash net operating assets, changes in receivables and inventory, and percentage of soft assets), firm performance (changes in cash sales and return on assets), and external financing measures (equity and debt issuance).[16] The F-score captures what misstating management typically does to mask their true firm performance.[17] For example, misstatement periods are associated with unusually high accruals and inflated sales through receivables. The gross margin can also be overstated by overstating inventory, thus reducing cost of goods sold. Firms with more soft assets, such as non-cash and non-PP&E assets, are subject to more discretion for earnings management.

The underlying economic construct of the M-score is similar to that of the F-score. The M-score is also computed based on financial statement variables, such as credit sales, gross margin, asset quality, sales growth, depreciation, SG&A expenses, leverage, and accruals.[18] A higher F-score or M-score is associated with a higher probability of accounting misstatement. Therefore, we expect a firm's F-score and M-score to decrease when FCA state pension funds own the shares of the firm and thus expose the firm to an enhanced risk of whistleblowing.

Given that the F-score is calculated based on AAER firms that violated generally accepted accounting principles (GAAP), the measure is useful in gauging the probability of accounting

[16] We use Model 1 in Dechow et al. (2011). Models 2 and 3 include off-balance-sheet, non-financial, or stock market-based variables as additional predictors, which decreases the number of observations without changing inference.
[17] The detailed calculations of the F-score and M-score are provided in Appendix C.
[18] This paper focuses mainly on the F-score, because the sample size drops dramatically for M-score tests due to missing values in its components. Also, interpreting the economic magnitude of the F-score is easier. The inference of the main empirical results remain similar across tests using these two measures.

fraud. Dechow et al. (2011) find the average F-score during the misstatement years is 1.9 and declines to the normal level of 1 for the three years following the misstatement. This time-series pattern of the F-score makes the imputed measure informative for learning about the deterrent effect of whistleblowing by comparing firm-years before and after a firm's exposure to FCA pension funds.

However, given that the F-score and M-score are constructed using detected frauds, they may simply proxy for the probability of detected fraud instead of the probability of existing or underlying fraud. Even in this case, these imputed measures are still useful in predicting the direction of the change in the probability of existing fraud. The probability of existing fraud should decrease as long as the probability of detected fraud decreases under the reasonable assumption that the detection probability will go up if regulators receive more whistleblower tips after the implementation of whistleblower regulations. Moreover, to the extent that the F-score captures the probability of detected fraud, our estimated deterrent effect provides an informative gauge of the lower bound of underlying fraud probability. We also address the potential measurement error issue by estimating the treatment effect using firm and year fixed effects. If the measurement error of the scores were similar across time within firms, firm fixed effects would strip it out. Year fixed effects account for year-specific shocks that likely affect the F-score across firms.

The F-score can also capture earnings management within GAAP, as evidenced by a high F-score during the pre-misstatement period (in years t-3 to t-1). This pattern is consistent with managers relying on the flexibility of GAAP before resorting to more aggressive earnings management that leads to SEC enforcement actions (Dechow et al. 2011). The optimistic bias of overconfident executives can also be responsible for a high F-score during the pre-misstatement period (Schrand and Zechman 2012). Whistleblowers are unlikely to receive bounties under FCAs

for reporting evidence on earnings management within GAAP, because proving the intent to defraud would be hard. However, to the extent that whistleblowing could make managers cautious about financial reporting in general, even earnings management within GAAP can decrease, thereby lowering the F-score. Also, managers who fear whistleblowing can voluntarily correct the early stages of intentional fraud allowed in GAAP.

4.2. The Deterrence Effect of the False Claims Act

We examine the deterrence effect of the FCAs on the probability of accounting fraud (*F-score* or *M-score*) by exploiting the level of firms' exposure to state FCAs. Using panel data, with firms indexed by *i* and years indexed by *t*, we estimate the following regression model:

$$\Pr(Fraud)_{it} = \beta_1 FCA_G_{i,t} + \beta_2 OWN_{i,t-1} + \sum_k \lambda_k Controls_{it} + Firm\ FE + Year\ FE + \varepsilon_{it}, \quad (1)$$

where *FCA_G* is our variable of interest that takes the value of 1 if a firm is exposed to at least one state's general FCA through an investment by at least one of that state's pension funds. *OWN* is an indicator variable that takes the value of 1 if a firm's shares were owned by at least one state pension fund in the lagged year. For treatment firms, *FCA_G* goes from 0 to 1 when the first state pension fund investing in the firm in the lagged year is the one located in a state that already passed a general FCA (80% of treatment firms), or when one of the states with pension fund ownership of the firm passes a general FCA in the current year (20% of treatment firms). In the former case, the *OWN* and *FCA_G* indicators change from 0 to 1 at the same time when an FCA state pension fund buys in, whereas in the latter case *FCA_G* changes after *OWN* does. Therefore, the inclusion of the *OWN* indicator variable can help isolate the effect of exposure to an FCA from the effect of changes in fund ownership.

We define the indicator variable of fund ownership (*OWN)* based on the lagged year's ownership to address the selection issue that funds might change their portfolio in the expectation

of FCA adoption in their states. We assume that in the year prior to the passage, state-fund managers did not expect the rule change.[19] In very rare cases in our sample, some firms that were initially affected by FCAs become unaffected, because existing FCA state pension funds sell their ownership of the firm. We drop firm-years of non-exposure following their first exposure to an FCA.[20] Consistent with the deterrent effect of FCAs, we expect β_1 to be negative.

We include control variables that could affect the F-score and M-score for reasons unrelated to the whistleblowing risk. Because the F-score and M-score consist of measures based on accruals and performance, we control for firm size (*ln(Assets)*), market to book (*MTB*), sales volatility (*StdRev*), whether the firm had a loss (*Loss*), restructuring activities (*Restructure*), mergers or acquisitions (*Merger*), and discontinued operations (*Discont*) during the year (Cassell et al. 2016). We also control for institutional ownership (*InstOwn*), Big 4 auditor (*BIG4*), industry growth (*IndGrowth*), leverage (*Leverage*), free cash flow (*FreeCF*), and net financing activities (*NetFin*) in our main specification. We include firm fixed effects because we exploit within-firm variation in fund ownership through which firms are exposed to FCA whistleblowing threats. Firm fixed effects control for time-invariant firm characteristics that are associated with the F-score and M-score. We also include year fixed effects to control for changes over time in factors other than whistleblower provisions that affect the likelihood of fraud in all firms equally.[21]

4.3. The Deterrence Effect of the SEC Whistleblower Program

[19] We relax this assumption in an additional analysis (not tabulated) by using ownership at t-2 instead of at t-1 (i.e., we use ownership at t-2 as an instrument for ownership at t-1). The magnitude of the result in the F-score test remains stable.

[20] We find the results do not change when we keep such cases and code them as treated firm-years (*FCA_G*=1), because we expect that firms remain subject to FCAs once they are owned by FCA state pension funds even after funds leave the firm.

[21] As a robustness check, we re-estimate our main regressions using firms' headquarter state × year fixed effects instead of year fixed effects. Results are reported in Table 8.

To examine the impact of increased exposure to whistleblowing driven by the SEC whistleblower law, we draw on the FCA setting. We expect the federal rule change to have a greater effect on firms that were not exposed to any state FCA before the introduction of the SEC statute. In the FCA analysis, we use firm-year observations during the 2001-2010 period. In this analysis, we rely on firms during the 2008-2014 period because the SEC provision went into effect on August 12, 2011. We split firms into those that were not under the influence of a general FCA from 2008 to 2010 (*NoFCA_G* = 1) and those with shares owned by general FCA state funds during those years (*NoFCA_G* = 0). We estimate the following difference-in-differences regression model for firms indexed by *i* and years indexed by *t*:

$$\Pr(Fraud)_{it} = \beta_1 \, NoFCA_G_i \times SECWB_t + \sum_k \lambda_k \, Controls_{it} + Firm\ FE + Year\ FE + \varepsilon_{it}, \quad (2)$$

where *SECWB* is an indicator variable equal to 1 if a firm's fiscal year end is 2011 or onward.[22] The variable of interest is the interaction term between *NoFCA_G* and *SECWB* (*NoFCA_G*×*SECWB*). We expect β_1 to be negative because after the adoption of the SEC whistleblower provision, the probability of fraud would decrease more for firms that were not previously exposed to any general FCA relative to those that were exposed.

5. Empirical Results

5.1. Descriptive Statistics

Table 1 Panel B summarizes the composition of our sample. Out of 3,164 firms used in FCA tests, 443 (1,189) firms were never (always) affected by general FCAs during 2001 to 2010.

[22] Because the SEC rule takes effect on the same calendar date (August 12, 2011) for all firms, we use the fiscal year of 2011 as a best approximation when defining the pre- and post-SEC rule periods. This may cause some classification errors for a subset of firms with fiscal year ends for 2011 occuring before the effective date. Our results are unchanged if we use an alternative (more precise) cutoff of August 2011 or drop all observations from fiscal year of 2011.

The remaining 1,532 firms experienced changes in their exposure to general FCAs (*FCA_G* goes from 0 to 1) and so are used as treatment firms. Of these 1,532 firms, 533 also experienced changes in their exposure to Medicaid FCAs (*FCA_M* goes from 0 to 1) during this period. Table 1 Panel C shows that 242 (16%) out of 1,532 treatment firms first got exposed to general FCAs in 2004.[23]

Institutional investment managers are required to disclose public equity holdings by filing Form 13F with the SEC quarterly when their assets under management exceed $100 million. However, if state pension funds contract with outside investment management, portfolio holdings for each individual fund are not disclosed separately, but instead are aggregated at the security level for each investment manager (Brown et al. 2015). Therefore, the public equity portfolio holdings of state pension funds are identifiable only when the state funds actively manage their own investment that exceeds $100 million, and hence report 13F filings under their own names.[24] For this reason, we are able to identify 24 separate state pension funds located in 16 distinct states between 2001 and 2010 as detailed in Table 2.

According to the 2009 Wilshire report on state retirement systems, there were 125 state pension funds that are sponsored by 50 states and the District of Columbia in 2009. The total market value of these pension plans' assets was $2,014 billion, of which $636 billion (31.6%) was invested in 1,500 U.S. public equities. Our sample contains 21 state pension funds in 2009 (17% of the population funds) that invested in U.S. public equities whose market value totaled $319 billion, accounting for 50% of the total market value of U.S. equities invested in by the 125 state pension funds. As shown in Appendix B, the state pension funds in our sample are sponsored by

[23] To check the sensitivity of our results to a particular group of firms that were exposed to general FCAs in a particular year, we perform a Jackknife procedure (not tabulated). For each year during 2002 and 2010, we drop firms that were first affected by general FCAs in that year and calculate separate treatment effects. The magnitude of treatment effects remains stable.

[24] This requirement implies the control group in our empirical tests could include firms owned by state pension funds that did not disclose their investment portfolios. This possibility biases our results against the effect we find.

16 different states (in the shaded rows) that represent a reasonable portion of the state pension fund universe and provide variation in FCA coverage to exploit.

Table 3 Panel A provides descriptive statistics for the firm-year observations used in the FCA analysis. It shows that 76 percent (41 percent) of firm-years from 2001 to 2010 are affected by general (Medicaid) state FCAs. The mean percentage of shares of a firm held by state pension funds (*SH_PCT*) is 1.6 percent whereas the mean percentage of shares held by pension funds from FCA states (*FCA_G_PCT*) is 0.6 percent. The mean (median) F-score is 0.978 (0.861), and the mean (median) M-score is -2.674 (-2.641). Panel B summarizes the sample of the internal control mechanism tests. Because the internal audit data are available for a small fraction of firms beginning from the fiscal year of 2005, merging the data with our main FCA test sample loses many observations. In addition, the merged sample contains very large firms during the sample period of 2005 to 2010. The mean values of external audit fees ($3.9 million) and total assets ($9.6 billion) are within the top 10 percent of the sample of Panel A. The mean internal audit costs are $2.9 million, which is somewhat smaller than external audit fees. On average, our sample firms spend 1,832 hours on internal audit to provide direct assistance to external auditors while external auditors spend 13,533 hours conducting their audit. Firms increase the size of internal audit staff by an average of 4.6 percent per year.

5.2. The Deterrent Effect of the State False Claims Act

Table 4 reports the results of the effect of exposure to the general FCA (*FCA_G*) on the probability of fraud (Hypothesis 1). In Panel A, we use the F-score as the dependent variable. Column 1 is a benchmark that has the main indicator *FCA_G* and two covariates, the *OWN* indicator and the size variable. Firm size is always controlled for due to its significance in explaining the F-score. To ensure the coefficient on *FCA_G* is not picking up confounding factors

other than potential whistleblowing risks, we add more control variables in column 2 and compare the coefficient estimate to that in column 1. We cluster standard errors at the firm level.

The results show the coefficient estimate on *FCA_G* (-0.068) in column 2 is not attenuated even after controlling for variables that potentially affect the F-score. The effect remains stable in column 3 (a base model), in which we have additional control variables (loss, free cash flows, and net financing). These variables are related to components of the F-score but included to control for potential confounding factors. The coefficient estimate is economically and statistically significant. In economic terms, the coefficient translates to about a 7% decrease in the probability of fraud (or a decrease of 0.12 standard deviations in the F-score).[25]

To assess the parallel trends assumption and how quickly managers respond to their exposure to the threat of whistleblowing, Figure 1 displays the trend of counter-factual treatment effects on the F-score (in Panel A) and M-score (in Panel B) over the event years. We replace *FCA_G* indicator in our main specification in Table 4 column 3 with separate indicators for each event year of treatment firms, except for the year immediately preceding the firms' first exposure to state FCAs (i.e., the benchmark year) and map out their coefficient estimates with 95% confidence intervals. In both figures, the trend suggests the exposure event significantly lowers the probability of fraud for treatment firms. It takes about three years for firms to fully respond to the increased risk of whistleblowing under FCAs and the lowered probability of fraud remains relatively stable afterward. The trends for treatment and control firms also seem parallel prior to treatment as evidenced by estimated treatment effects during the pre-period being insignificantly different from zero.

[25] If we multiply the coefficient -0.069 in column 3 by the unconditional probability of fraud 0.0037 (Dechow et al. 2011), the probability of fraud decreases by 0.026 percentage points relative to the average probability of fraud, 0.37%.

Note that firms are treated either when states with fund ownership pass an FCA or when state pension funds from a state with an FCA buy the shares of the firm. In the latter case, a potential concern is that changes to the fund's portfolio by state pension fund managers are endogenous. For example, a fund manager from a state introducing FCA protection may become more likely to add firms to the portfolio that have relatively high fraud risk, but subsequent reduction of the fraud risk at such firms could reflect mean reversion rather than a causal impact of FCA treatment.

To address this concern, we remove the treatment variation coming from fund ownership changes and thus isolate the treatment variation driven by states' FCA adoption. In column 4, we keep firm-year observations with state fund ownership being equal to 1 and exclude the *OWN* indicator variable from regression model 1. Hence, this specification treats firms that are affected by an FCA due to new investment by FCA state funds as control firms, of which *FCA_G* remains one throughout the sample period. Hence, the identifying variation is coming from changes in firms' FCA exposure after states pass FCAs. The size of the effect increases to -0.089 and remains statistically significant. This result implies firms' exposure to FCA due to states' passage of an FCA lowers the probability of accounting fraud by 9% relative to firms without any change in exposure to FCAs. Given that the size of the treatment effect documented in columns 1 to 3 does not shrink in this test where we hold the effect coming from ownership change constant, it addresses the concern that the treatment effect in our main tests is completely driven by endogenous ownership changes by state pension funds due to their home state adopting general FCA protection.

In column 5, we examine whether the percentage of shares of a firm owned by a state pension fund is associated with a larger treatment effect of whistleblowing risks. Management may

be less likely aware of their firm's exposure to FCAs if a pension fund buys only a very small position in the firm. In other words, the more intensively funds buy into the firm, the more likely managers are to be aware of their exposure to FCAs. Moreover, to the extent that the potential damages to pension fund investors increase with value of the shares owned in a fraud firm, we expect an incremental deterrence effect on the intensive margin from greater ownership.

We define *SH_PCT* as the percentage of shares of a firm held by any state pension funds and *FCA_G_PCT* as the percentage of shares held by FCA states' pension funds. The regression model estimated in column 5 replaces *FCA_G* with *FCA_G_PCT* and accordingly, *OWN* with *SH_PCT*, respectively. The coefficient estimate on *FCA_G_PCT* (-1.886) is negative and statistically significant. This result indicates the intensity of FCA states' fund ownership of a firm has an additional deterrent effect on the probability of fraud, supporting the deterrent effect of the FCA whistleblower law. In economic terms, for each 1 standard deviation increase in shares invested by FCA state pension funds, the probability of fraud decreases by 1.32 percent.

Table 4 Panel B reports the effect of exposure to FCAs on individual components of the F-score. According to prior literature, revenue recognition is the most common type of accounting misstatement (Files et al. 2009; Dechow et al. 2011). Bowen et al. (2010) document that 80% of financial whistleblowing cases disclosed in the media are earnings management–related allegations (e.g., overbilling that overstates revenue). Moreover, Bedard et al. (2012) document that revenues, receivables, inventory, and taxes are the most frequently occurring types of material weaknesses in internal controls under SOX Section 404. Thus, we expect the remediation of misstatements or material weaknesses in revenue, receivables, or inventory accounts to be the main drivers in lowering the F-score and M-score.[26]

[26] Ultimately, this remediation would improve the quality of financial reporting (Ashbaugh-Skaife et al. 2008; Bedard et al. 2012).

We estimate equation (1) by replacing the dependent variable with each component of the F-score (columns 2 through 8). As a benchmark, column 1 reports the coefficient estimate on *FCA_G* (-0.077) when we regress the predicted value from Model 1 of Dechow et al. (2011) before monotonic transformation to the F-score. By identity and the linearity of ordinary least squares, the coefficient estimate in column 1 should be equal to the sum, for columns 2 to 8, of the coefficient estimates in each column multiplied by each loading value used to calculate the predicted value.[27] We find that changes in receivables (column 3) and soft assets that are neither cash nor property, plant, and equipment (PP&E) (column 5) account for nearly 80% of the decrease in the F-score ((0.034+0.027)/0.077=0.079).

In Panel C, we report the effect of exposure to FCAs on the M-score and each component. In column 1, we regress the M-score on FCA_G as a benchmark. The coefficient estimate on FCA_G (-0.306) is statistically and economically significant (a decrease of 0.32 standard deviations in the M-score). Similar to the F-score, the receivables and accruals components are most responsible for the drop in the M-score (columns 2, 4, 5, and 9). Taken together, the breakdown analysis of the F-score and M-score suggests a concerned manager likely remediates misstatements in revenue recognition and accruals when she is exposed to higher whistleblowing risks under an FCA. Overall, the F-score and M-score decrease when firms are exposed to general state FCAs through investment by state pension funds. The magnitude is statistically and economically significant in all specifications, which suggests the effectiveness of whistleblowing laws in reducing the likelihood of accounting manipulation.

Although the F-score and M-score disaggregation results show that it is the variables within the scores most associated with misreporting which seem to have the most explanatory power,

[27] This means that -0.077 = 0.79×(-0.009) + 2.518×(-0.013) + 1.191×(-0.002) + 1.979×(-0.014) + 0.171×(-0.037) + (-0.932)×(-0.006) + 1.029×(-0.005).

there is another concern about these score measures. They might suffer from a similar concern as earnings management measures in being influenced by nondiscretionary or real effects in addition to discretionary or financial statement management effects. For example, we might find our F-score results even if the whistleblowing treatment did not necessarily reduce the probability of financial statement fraud, but did reduce various risk-taking initiatives at the treated firms. In other words, the F-score could go down because of the decrease in the risk-taking or firm growth even if the probability of fraud did not decrease if the F-score captures nondiscretionary effects of a firm. Then being treated by whistleblowing legislation might have negative effects as well as positive effects. To examine whether there was a reduction in growth after firms' exposure to state FCAs, we regress future sales growth and market share on our main treatment variable, *FCA_G* (not tabulated). We find no evidence that future growth decreases after the FCA exposure.

5.3.Mechanism Tests

5.3.1. Effect of the State False Claims Act on Audit Fees

Table 5 presents the impact of firms' exposure to FCA on audit fees (Hypothesis 2(a)). In this audit fees test, we replace the dependent variable in equation (1) with the natural logarithm of audit fees. We expect the coefficient on FCA_G to be negative if the higher risk of whistleblowing makes managers improve internal control quality, thereby lowering fraud risk and/or fee premiums charged by auditors. Following the literature, we control for factors that are known to affect audit fees in the prior literature (Hogan and Wilkins 2008; Hribar et al. 2014) along with firm and year fixed effects. These control variables include firm size (*ln(Assets)*), firm performance (*Loss* and

ROA), complexity and volatility of business (*ln(Segment)*, *Foreign*, *StdROA*, *Growth*, and *Merger*), Big4 auditors (*BIG4*), audit tenure (*AudTenure*), and liquidity risk (*Quick* and *InvRec*).[28]

In column 1 where ownership change and size are controlled for, the coefficient estimate on *FCA_G* (-0.052) is negative and statistically significant. When we include a full set of control variables in column 2 (our main specification), the coefficient estimate on *FCA_G* (-0.044) is marginally significant. Although the attenuated magnitude of the coefficient may raise a concern that omitted variables might influence a part of the estimated treatment effect of FCAs, the result in column 2 indicates that the risk of whistleblowing driven by exposure to state FCAs is associated with 4.5% lower audit fees relative to firm-years not treated by FCA exposure, which amounts to about $23,600 on average.[29] We note that the median annual increase in audit fees for the 2001-2010 period for our sample is 7% (untabulated), implying that exposure to state FCAs tends to reduce the rate of increase in audit fees rather than actually leading to annual audit fees decreasing.

Next we examine whether the impact of FCAs on audit fees is also observed in alternative model specifications used in Table 4 Panel A as robustness checks. In column 3, we focus on the treatment effect driven by FCA passage by states by including only firm-years for which the firm was owned by at least one state pension fund in the lagged year. The size of the coefficient estimate on *FCA_G* (-0.058) gets slightly larger and remains statistically significant. In column 4, we find an incremental effect on the intensive margin (i.e., for the extent of FCA fund ownership) on audit

[28] Although known to be one of the important determinants of audit fees, internal control weaknesses are excluded in the main model specification both because we hypothesize exposure to a general FCA affects audit fees due in part to the affecting internal controls and also to avoid any sample reduction. Note that internal control weaknesses are reported under SOX 404 only from 2004 whereas the FCA tests focus on a longer period of time between 2001 and 2010. The results, however, are robust to inclusion of internal control weaknesses as a control variable or to an alternative specification using industry fixed effects.

[29] Because the log transformation of the dependent variable can approximate the percentage change for small coefficient estimates, the -0.044 coefficient estimate is being transformed into a 4.5% reduction in audit fees (i.e., $\exp(0.044)-1 \approx 0.04498$). The dollar impact on audit fees is calculated based on the mean audit fees of treated firms in the pre-*FCA* period ($524,700).

fees. The coefficient estimate (-1.849) on *FCA_G*×*PCT* is negative and statistically significant, with the magnitude implying that a 1 standard deviation in shares invested in by FCA state pension funds reduces audit fees by 1.29 percent ($6,769). The Table 5 findings suggest the deterrence effect of FCA whistleblower laws causes reductions in the fraud risk that auditors face.

In Table 6, we examine whether managers of FCA affected firms increase investments in their internal audit function (Hypothesis 2(b)) using three different proxies for internal audit quality: internal audit costs (*IntAudCost*), internal audit hours (*IntAudHour*), and the percentage change in the number of internal audit staff (%*ChIntAudStaff*). In columns 1, 3, 4 and 6, we use our main regression model with an indicator variable of general FCA exposure as a treatment variable.[30] Because our sample firms are very large, most firms are treated under state FCAs resulting in only 13 untreated observations during 2005-2010. We expect internal audit costs/hours/staff size to increase if concerned managers employ more audit staff, raise their salaries, offer more internal audit training, or spend more hours on internal auditing.

In column 1, we find no evidence exposure to FCAs leads firms to increase internal audit costs (as the coefficient estimate on *FCA_G* is negatively signed, although not significantly different from zero). In column 3, we find that internal audit hours increase by a statistically insignificant 471 hours (an increase of 0.18 standard deviations in internal audit hours). In column 4, we find an insignificant 910 hour positive impact of FCA exposure on internal audit hours when further controlling for external audit efforts using external audit hours (in addition to external audit fees). We also test whether the size of internal audit staff increases faster for firms exposed to state FCAs and find no significant result in column 6.

[30] In this analysis, we control for industry fixed effects instead of firm fixed effects. Because many firms have only one data point in the internal audit dataset, most of the variation in the exposure to FCAs comes at the firm level.

To explore whether the insignificant results described above may reflect the lack of variation in treatment, we replace the indicator variable of exposure (*FCA_G*) with the percentage of the firm's shares held by FCA states' pension funds (*FCA_G_PCT*) in columns 2, 5, and 7. We find statistically insignificant results for internal audit costs (column 2) and internal audit hours (column 5) but a positive and marginally significant result for the percentage change in the size of internal audit staff (column 7). In economic terms, for each one percent increase in shares invested by FCA state pension funds, the internal audit staff size increases 16.5 percent faster.

Overall, these results are inconsistent with the prediction that FCA exposure led firms to choose to improve their internal audit quality. The findings may reflect the lack of power from our small sample of very large firms with available internal audit information. Alternatively, other possible mechanisms may be at work in providing channels for whistleblowing risks to reduce fraud and thus decrease risks to external auditors. For example, top management's tone and the nature of the goals and incentives provided to middle management may change toward a more conservative posture regarding financial reporting when whistleblowing threats increase.

5.4. Falsification Test Using the Medicaid FCA

Note that 80% of treatment firms are subject to whistleblowing under FCAs due to FCA state funds initiating stock ownership of the firm rather than due to state funds with ownership becoming exposed to a newly passed FCA law in their home state. Therefore, a selection problem can exist if state funds invest in certain types of firms for reasons related to future F-score changes. For example, state funds may prefer growth and riskier firms that are characterized by a high F-score. If this high F-score is reversed in the following years due, say, to mean reversion, the selection could be driving the negative coefficient estimate on *FCA_G* reported in Table 4.

To address this selection issue, we use Medicaid FCAs. The general and Medicaid FCAs share common features, but only the general FCA can protect against financial fraud. Thus, if some common features of funds such as funds' investment preferences, regardless of whether they are from general or Medicaid FCA states, drive the effect of general FCAs on the likelihood of fraud, we will continue to see the same treatment effect in this Medicaid FCA test. Importantly, the Medicaid FCA should not affect the likelihood of fraud to the extent that we succeed in eliminating from our sample any healthcare industries. Recall that this is true for the primary industry of any potential sample firm, although it remains possible that some sample firms could have a secondary presence in healthcare (in, e.g., a segment of a multi-segment firm). If some concurrent enforcement change along with the passage of general and Medicaid FCAs is driving our initial result, the Medicaid FCA test will provide a similar treatment effect as in the general FCA tests. The results are reported in Table 7.

In columns 1 to 3, we examine the effect of FCAs on the F-score. Column 1 provides a benchmark treatment effect of general FCAs (-0.069) reported in column 3 of Table 4 Panel A. In column 2, we repeat the column 1 estimation on the reduced sample we describe next for column 3 and obtain a slightly more substantial treatment effect of -0.079, although its statistical significance is more marginal than that in column 1 due to the reduced sample size. In columns 2 and 3, we retain only those column 1 observations that experienced, during our sample years, changes in exposure to both general and Medicaid FCAs (but in different years to avoid any spurious effect that could occur if both the Medicaid and general FCA exposures occurred in the same year). This allows us to compare the impact of the two different types of FCAs on a constant sample, which ends up consisting of 3,024 observations (411 firms) versus the 18,543 observations (3,164 firms) in the column 1 estimation.

We find a treatment effect of Medicaid FCAs in column 3 of -0.044. A Chi-square test for the equality of coefficient estimates between columns 2 and 3 fails to reject the null that the general FCA treatment estimate from column 2 is equal to the Medicaid FCA treatment estimate in column 3. Thus, for the F-score dependent variable, the Medicaid FCA falsification test raises some concern that our initial finding of general FCA exposure reducing fraud risk could reflect common features of state pension funds such as funds' investment preferences or concurrent enforcement changes that may accompany passage of general and Medicaid FCA laws. We further assess the extent of this concern in Section 5.7, which examines the SEC Whistleblower program.

In columns 4 to 6, we replicate the same specifications used in columns 1 to 3 after replacing the F-score with audit fees. Column 4 repeats our initial base model finding on audit fees (from column 2 of Table 5), repeating the initial finding of a coefficient estimate of -0.044 as the treatment effect of general FCAs when the natural logarithm of external audit fees is the dependent variable. In column 5, the coefficient estimate for the treatment effect of general FCAs increases to -0.088 when we retain only those column 4 observations that experienced, during our sample years, changes in exposure to both general and Medicaid FCAs (in different years). In contrast, using this same 3,024 observation (411 firm) sample in column 6, we find the treatment effect of Medicaid FCAs is to slightly increase audit fees, with the coefficient estimate being 0.032. The Chi-square test rejects (at better than the 1% significance level) the null that the general FCA treatment estimate from column 5 is equal to the Medicaid FCA treatment estimate in column 6. Thus, we do not observe the treatment effect of Medicaid FCAs on audit fees, consistent with the argument that whistleblowing under an FCA curbs misreporting incentives only when the FCA covers financial fraud.

5.5. Endogenous FCA Adoption by States

One potential concern is that states' passage of FCAs could be endogenously correlated with changes in the F-score. It is likely that some states have passed an FCA because of a high frequency of fraud against the state government. If the fraudulent firms were characterized by high F-scores, the mean reversion of the F-score after the passage of the FCA might drive the treatment effect observed in Table 4. Although this potential endogeneity problem might not be a serious issue for the 80% of treatment firms whose *FCA_G* indicator switches on due to a fund ownership change, it could be an issue for the 20% of treatment firms that became subject to the general FCA due to the passage of FCAs. Importantly, this endogeneity problem arises only among firms that are invested in by a pension fund in their headquarters states. Brown et al. (2015) find state pension funds are more likely to invest in in-state firms located in a county that contributed to the current governor's campaign in the most recent election. If the firm's political influence is somewhat correlated with factors that affect the F-score, this endogeneity issue is more plausible.

To address this concern, we repeat our main tests using only firms whose fund ownership is coming from a different state than their headquarters state. First, we use firms' headquarters state × year fixed effects. Second, we drop firm-year observations of treatment firms after their headquarters state passed a general FCA. If firms' F-score and headquarters state's decision to pass a general FCA are correlated to some extent, the treatment effect observed in Table 4 should get smaller when we drop firm-years after the passage of the law in their headquarters states. The results are documented in Table 8.

Columns 1 and 4 provide benchmark treatment effects of general FCAs on F-scores and audit fees reported in column 3 of Table 4 Panel A and column 2 of Table 5, respectively. In columns 2 and 5, we replace year fixed effects with firms' headquarters state × year fixed effects. In columns 3 and 6, we exclude firm-years after a firm's headquarters state passed a general FCA.

In all specifications, the coefficient on *FCA_G* remains stable, assuaging the concern about endogenous FCA adoption by states is driving our results.

5.6. Sensitivity of Treatment Effects to Pension Fund Characteristics

Del Guercio and Hawkins (1999) show that some state pension funds such as CalPERS and CalSTRS have a significant influence on firm policies through shareholder proposals. For example, firms targeted by CalPERS tend to experience more voluntary internal changes such as senior executive turnover, asset sales, voluntary restructurings, and layoffs. Moreover, as Table 2 shows, CalPERS accounts for 13 percent of funds' ownership of firms in our sample. Therefore, this unbalanced sample composition raises a concern about characteristics of particular funds driving the main effect. We alleviate this issue by including fund ownership fixed effects in the model to adjust for time-invariant fund-specific characteristics. In columns 2 and 5 of Table 9, we include indicators for each state fund's ownership along with firm and year fixed effects. As an alternative specification, we include indicators for each funds' state to account for time-invariant fund states' characteristics in columns 3 and 6.[31]

In columns 2 and 3, the size of the deterrence effect of firms' exposure to general FCAs on the F-score gets slightly smaller but remains statistically significant. In columns 5 and 6, the effect of exposure to general FCAs on audit fees becomes smaller and statistically insignificant.[32] Overall, even after controlling for characteristics of pension funds and fund states, the main effects remain largely unchanged, especially for the F-score test.

5.7. Effect of the SEC Whistleblower Program

[31] For states that have only one state pension fund in our sample (e.g., Colorado), these two types of indicators are indistinguishable.

[32] However, when we control for internal control weaknesses, the statistical significance improves in columns 5 and 6.

In this section, we examine the deterrence effect of the SEC's whistleblower provision (Hypothesis 3). Although testing the effectiveness of the federal law alone is interesting, this test, in addition to the falsification test using Medicaid FCAs in section 5.4, is also useful to assess whether our general FCA main results may be due to the selection criteria state pension funds use to choose their investments. Relative to Medicaid FCA funds, pension funds located in states with a general FCA may invest more aggressively by buying risky stocks with higher F-scores (Peltzman Effect)[33] knowing their investment can be protected under the general FCA. Then, the effect of exposure to the SEC whistleblower provision would be rather small for firms that were not invested in previously by general FCA funds because they are relatively safer assets as identified by low F-scores. However, we find the opposite results as shown in Table 10. Although the falsification test using Medicaid FCAs did not eliminate the possibility of something other than the whistleblowing threat driving the effect of general FCAs on lowering fraud risk, the Table 10 results provide considerable comfort that the whistleblowing threat indeed explains our finding that general FCA exposure results in reduced fraud probability.

We require firms to have at least six years of observations during the sample period of 2008 to 2014. The treatment group has 223 firms (*NoFCA_G* = 1), and the control group has 882 (*NoFCA_G* = 0). Interestingly, the coefficient estimate on *NoFCA_G*×*SECWB* (-0.073) in column 1 is statistically significant and has an economically similar effect as general FCAs. This result implies that for firms that were not exposed to a general FCA before the adoption of the SEC whistleblower provision, the SEC provision lowers the probability of fraud by 7% relative to firms that were already exposed. In column 3, the impact on audit fees is also statistically significant and the size of the coefficient estimate (-0.04) is also similar as in the FCA test.

[33] The Peltzman Effect (Peltzman 1975) refers to the tendency of people to increase riskier behavior in response to a safety regulation, thus offsetting the benefit of the regulation.

In columns 2 and 4, we estimate falsification tests using a sample split based on firms' prior exposure to Medicaid FCAs. Note that this sample split should not have effects in theory unless other unobserved factors drove the treatment effect because our sample excludes healthcare as a primary industry for all observations. The treatment group (*NoFCA_M* = 1) has 438 firms, and the control group (*NoFCA_M* = 0) has 444. In column 2, the coefficient estimate on *NoFCA_M*×*SECWB* (0.024) becomes positive and statistically insignificant. The coefficient estimates in columns 1 and 2 are significantly different (Chi-square test statistic = 17.94). In column 4, the magnitude of the coefficient estimate on *NoFCA_M*×*SECWB* (-0.026) is smaller than that in column 3 but statistically significant at 10% significance level. The coefficient estimates in columns 3 and 4 are not statistically different (Chi-square test statistic = 0.64). Overall, the results on F-score indicate the impact of the SEC's whistleblower provision was more pronounced for firms that were not previously exposed to a general FCA, corroborating the deterrent effect of whistleblower laws that include a bounty model.

6. Conclusion

We test the causal impact of state and federal whistleblower laws on reducing fraud probability, quantify the effect on both fraud probability and external audit fees, and test the role of internal audit investments as a potential mechanism for the effect. Prior and contemporaneous research suggests whistleblower laws are associated with a benefit, but the approaches in these studies preclude both identification of whether the association is causal and quantification of the benefit for the population of affected firms. Both are needed to move toward the evidence needed for a benefit/cost analysis of whistleblowing legislation.

The state and federal whistleblower laws that we test provide bounties to whistleblowers when their tips lead to successful enforcement actions for fraudulent firms. If this monetary

compensation encourages whistleblowers to come forward, the exposure to these laws increases the risk of whistleblowing in the firms exposed to these rules.

Specifically, when a firm's shares are invested in by a state pension fund from a state with a general FCA, the firm becomes subject to the state's FCA. Using within-firm variation in state pension fund ownership interacted with states' staggered implementation of the FCA, we find that when firms become exposed to FCAs, the probability of fraud decreases by 5% to 9%. In addition, external audit fees are 4.5% to 6% lower after a firm is expose to state FCAs relative to firm-years not treated by an FCA.

We examine, using a proprietary dataset, whether this deterrent effect is achieved by improvement in internal audit quality as measured by increased internal audit costs, hours and staffing. Our results are inconsistent with internal audit quality improvements being a mechanism by which the whistleblowing laws achieve the reductions in fraud risk and audit fees that we document. While one possible explanation for this lack of findings is the small sample of extremely large firms for which we have access to internal audit data (and the resulting lack of variation in treatment within this sample), our results also suggest the possibility that other mechanisms than internal auditing play a larger role in facilitating the impact of whistleblowing legislation with a bounty model. Future research that is able to examine the internal audit mechanism on a larger and more diverse sample or that examines alternative mechanisms (such as changes in top management's tolerance for accounting risk and shifts in the nature of accounting-based targets and incentives) could provide valuable additional insight into the channels by which whistleblowing laws achieve their impact.

We also document the deterrent effect of the Dodd-Frank whistleblower regulation is more pronounced for firms without prior exposure to FCAs, with the economic effects similar to those

in the state FCA analysis. The fact that our setting allows us to identify a substantial control group that is already treated by similar FCA provisions even prior to passage of the Dodd-Frank legislation is an important advantage given the difficulties prior research confronts in attempting to construct an appropriate control group when testing the Dodd-Frank whistleblowing rule.

We provide a series of tests to ensure the robustness of our results. The results show that endogenous stock selection by state funds, states' decision to pass an FCA, or characteristics of a particular fund or funds' state does not drive the observed treatment effect of the FCA. Collectively, our results are consistent with the notion that the enhanced whistleblower regulation with a bounty model effectively motivates managers not to engage in fraud, and we provide reasonable causal quantification of this effect on the population of affected firms. Therefore, this paper informs the policy debate over the effectiveness of whistleblowing provisions in preventing fraud.

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Appendix A: Variable Definitions

Variable	Description
Main independent variables	
FCA_G	An indicator equal to 1 if at least one of the owners of a firm is a state pension fund located in a state with a general FCA, and zero otherwise
FCA_M	An indicator equal to 1 if at least one of the owners of a firm is a state pension fund located in a state with a Medicaid-only FCA, and zero otherwise
OWN	An indicator equal to 1 if at least one state pension fund owns the firm, and zero otherwise
SH_PCT	The percentage of shares of a firm held by any state pension funds
FCA_G_PCT	The percentage of shares of a firm held by FCA states' pension funds
NoFCA_G	An indicator equal to 1 if a firm was not exposed to any general FCA during 2008-2010, and zero otherwise
NoFCA_M	An indicator equal to 1 if a firm was not exposed to any Medicaid-only FCA during 2008-2010, and zero otherwise
SECWB	An indicator variable equal to 1 if fiscal year end is 2011 or onward, and zero otherwise
Dependent variables	
F-score	The predicted probability of earnings misstatement developed in Dechow et al. (2010); calculation is provided in Appendix C
M-score	The predicted probability of earnings misstatement constructed by Beneish (1999); calculation is provided in Appendix C
ln(audit fees)	The natural logarithm of audit fees in thousands of dollars
IntAudCost	The total internal audit costs including costs incurred for internal audit employees' salary, bonuses, benefits, travel, training, service purchases, and other overhead costs. (Source: *The institute of internal auditors' Audit Intelligence Suite database*)
IntAudHour	The total internal audit hours worked on the most recently completed external audit (i.e., the total number of hours requested by external auditors to provide in external audit assistance) (Source: *The institute of internal auditors' Audit Intelligence Suite database*)
%ChIntAudStaff	The percentage change of internal audit staff size (Source: *The institute of internal auditors' Audit Intelligence Suite database*)
Control variables	
ln(Assets)	The natural logarithm of total assets in millions of dollars
Loss	An indicator equal to 1 if net income is less than zero, and zero otherwise
Foreign	An indicator equal to 1 if foreign exchange income is not zero during the year, and zero otherwise
Merger	An indicator equal to 1 if a merger or acquisition occurred during the year, and zero otherwise
Discont	An indicator equal to 1 if a firm discontinued operations during the year, and zero otherwise
Restructure	An indicator equal to 1 if a firm had restructuring activities during the year, and zero otherwise
MTB	Market-to-book ratio calculated as the market value of equity divided by the book value of equity
Leverage	Leverage calculated as current liability plus long-term debt divided by total assets

FreeCF	Free cash flow calculated as the sum of cash from operations less capital expenditures scaled by lagged total assets
NetFin	An indicator equal to 1 if net stock issuance or debt issuance is positive, and zero otherwise
BIG4	An indicator equal to 1 if the auditor is from Deloitte & Touche, Ernst & Young, PricewaterhouseCoopers, KPMG, or Arthur Andersen (until 2002)
AudTenure	Auditor tenure calculated as the length of the auditor-client relationship in number of years
InstOwn	The percentage of shares held by institutions
IndGrowth	Industry (2-digit SIC code) sales growth
StdRev	Volatility of company sales calculated as the standard deviation of revenue deflated by lagged total assets over the past three years
ROA	Return on assets calculated as operating income after depreciation divided by lagged total assets
ln(Segment)	The natural logarithm of the number of business and geographic segments
InvRec	Inventory and receivables-to-assets ratio
Quick	Quick ratio calculated as the sum of current assets less inventories scaled by current liabilities
LtDebt	Long-term debt divided by total assets
StdROA	Standard deviation of ROA over the past three years
Growth	One-year sales growth
ICW	An indicator equal to 1 if a firm reported non-zero internal control weaknesses during the year (from 2004)
ExtAudHour	The total external audit hours worked on the most recently completed external audit (Source: *The institute of internal auditors' Audit Intelligence Suite database*)

Appendix B: State False Claims Acts

Notes: This table summarizes state-by-state FCA provisions as of 2010 (Bucy et al. 2010; Rapp 2012b). The column for FCA Type shows whether the state has adopted its own FCA, and if it has, whether it covers fraud in general (General) or Medicaid fraud only (Medicaid). The column for qui *tam* represents whether the state's FCA has a provision that allows a private citizen to file a lawsuit on behalf of the state government and obtain a portion of the money recovered. The shaded states are those used in our empirical analysis.

State	Year Passed	FCA Type	Qui tam	Code Section
Alabama		no		
Alaska		no		
Arizona	2009	Medicaid	no	Rev. Stat. Ann. § 36-2918
Arkansas	2009	Medicaid	no	Ark. Code Ann. §§ 20-77-901 to -902
California	1987	General	yes	Cal. Gov't Code § 12650 et seq
Colorado	2010	Medicaid	yes	§Senate Bill (S.B.) 10-167
Connecticut		no		
DC	1998	General	yes	DC ST § 2-308.14
Delaware	2000	General	yes	Del. Code Ann. Tit. 6, § 1201 et seq
Florida	1994	General	yes	Fla. Stat. § 68.081 et seq
Georgia	2007	Medicaid	yes	Ga. Code § 49-4-4168
Hawaii	2001	General	yes	Haw. Rev. Stat. § 661-22 et seq
Idaho		no		
Illinois	1992	General	yes	Ill. Comp. Stat. Ann. § 175/1 et seq
Indiana	2005	General	yes	Ind. Code § 5-11-5.5
Iowa		no		
Kansas		no		
Kentucky		no		
Louisiana	1997	Medicaid	yes	La. Rev. Stat. Ann. § 46:438 et seq
Maine		no		
Maryland		no		
Massachusetts	2000	General	yes	Mass Ann. Laws Ch. 12 § 5(A)-(O)
Michigan	2008	Medicaid	yes	MCL § 400.601 et seq
Minnesota		no		
Mississippi		no		
Missouri	2007	Medicaid	no	Mo. Ann. Stat.§191.905(1), (3), (4), (11)
Montana	2005	General	yes	Mont. Code, Ch. 465 HB 146
Nebraska	1996	Medicaid	no	§Neb. Rev. Stat. Ann. § 68-936
Nevada	1999	General	yes	Nev. Rev. Stat. § 357.010 et seq
New Hampshire	2005	Medicaid	yes	New Hamp. RSA § 167:61
New Jersey	2008	General	yes	N.J.S.A. § 2A:32C-1
New Mexico	2004	General	yes	N.M. Stat. Ann. § 27-14-1 et seq
New York	2007	General	yes	NY Stat. § 39-13-187 et seq

(Continued)

State	Year Passed	FCA Type	Qui tam	Code Section
North Carolina	2009	General	yes	N.C. Gen. Stat. §§ 1-605, -608
North Dakota		no		
Ohio		no		
Oklahoma	2007	General	yes	Section 5053, title 63
Oregon		no		
Pennsylvania		no		
Rhode Island	2008	General	yes	Ch. 9-1.1-1
South Carolina		no		
South Dakota		no		
Tennessee	2001	General	yes	§ 4-18-101 et seq
Texas	1995	Medicaid	yes	Tex. Hum. Res. Code § 36.001-36.117
Utah		no		
Vermont		no		
Virginia	2003	General	yes	Va. Code Ann. § 8.01-216.1
Washington		no		
West Virginia		no		
Wisconsin	2008	Medicaid	yes	§ 20.931
Wyoming		no		

Appendix C: Calculation of the F-score and M-score

F-score (Dechow et al. 2011)

To calculate the F-score, we use Model 1 in Dechow et al. (2011) based on a prediction model using financial statement variables capturing accrual quality (noncash net operating assets, changes in receivables and inventory, and percentage of soft assets), firm performance (changes in cash sales and return on assets), and the market-related measure (equity and debt issuance). Dechow et al. (2011) select these measures by performing backward elimination in the estimation of logistic models for the various determinants of misstatements. They regress an indicator variable that is equal to 1 for firm-years involving a misstatement of AAER firms during 1982-2005 on the selected sets of predictors to estimate the coefficients on each component of the F-score, and compute the predicted value as follows:

$$\begin{aligned}\text{Predicted Value} = {} & -7.893 + 0.79 \times \text{Changes in noncash operating assets(rsst_acc)} \\ & + 2.518 \times \text{Changes in receivables(ch_rec)} + 1.191 \times \text{Changes in inventory(ch_inv)} \\ & + 1.979 \times \text{\% of Non-Cash and Non-PP\&E(soft_assets)} \\ & + 0.171 \times \text{Changes in cash sales(ch_cs)} + (-0.932) \times \text{Changes in ROA(ch_roa)} \\ & + 1.029 \times \text{Equity or debt issuance(issue)}\end{aligned}$$

$$\text{Probability} = \frac{e^{\text{Predicted value}}}{1 + e^{\text{Predicted value}}}$$

After calculating the probability of misstatement from the predicted value above, they compute the F-score by dividing the probability by the unconditional probability of misstatement. The unconditional probability, 0.0037, is the ratio of the number of misstatement firms (494) over the total number of firms (494 misstatement firms + 132,967 non-misstatement firms) in their sample. If a firm's F-score is greater than 1, the probability of misstatement is higher than the unconditional expectation, which can be thought of as a red flag. Therefore, the F-score provides the likelihood a firm is engaging in accounting misstatement relative to unconditional expectation. The higher F-score is associated with a higher probability of misstatement.

M-score (Beneish 1999)

The M-score is a probability of accounting manipulation calculated based on Beneish (1999)'s model. Using the eight accounting variables, he estimates an unweighted probit model with 50 manipulators during 1982-1988 and 1,708 industry-matched controls firms. Using the coefficient estimates from the model, the M-score is calculated as follows:

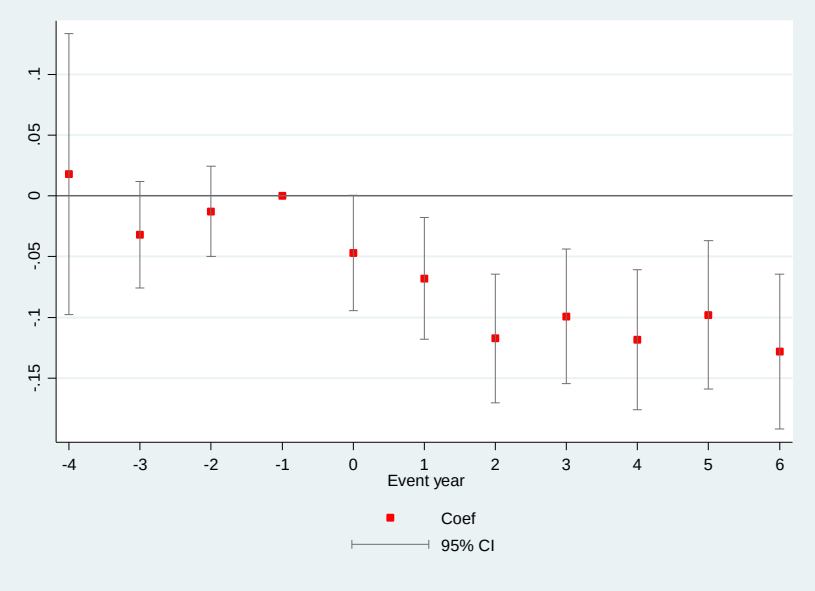
$$\begin{aligned}\text{M-score} = {} & -4.84 + 0.920 \times \text{Days' sales in receivables}(DSRI) + 0.528 \times \text{Gross Margin}(GMI) \\ & + 0.404 \times \text{Asset quality}(AQI) + 0.892 \times \text{Sales growth}(SGI) + 0.115 \times \text{Depreciation rate}(DEPI) \\ & + (-0.172) \times \text{SG\&A expenses}(SGAI) + (-0.327) \times \text{Leverage}(LVGI) + 4.679 \times \text{Accruals}(ACCR)\end{aligned}$$

The higher M-score is associated with a higher probability of misstatement.

Figure 1. Trend of the Counter-Factual Treatment Effects on the Likelihood of Fraud

Notes: The figures map out the counter-factual treatment effects of firms' exposure to state FCAs on the F-score (in Panel A) and M-score (in Panel B) over event years of treatment firms. Using our main specification in Table 4 column 3, we replace *FCA_G* indicator with separate indicators for each event year of treatment firms, except for the immediately preceding year of firms' first exposure to state FCAs (i.e., event year equal to -1 is the benchmark year). The figures display coefficient estimates on indicators for each event year and their 95% confidence intervals.

Panel A: Effects on the F-score



Panel B: Effects on the M-score

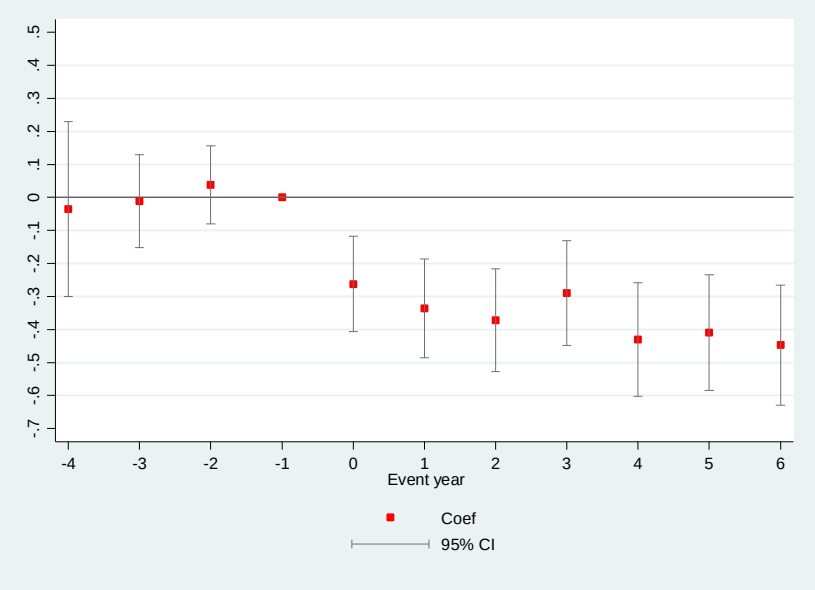


Table 1. Sample Description

Panel A: Sample selection

Selection	N of obs.
Initial fund-firm-year-level sample:	
Merge 13(f) filings with COMPUSTAT variables (during 2000-2014)	400,680 (13,961 firms)
Aggregate at the firm-year level	110,620 (13,961 firms)
Eliminate healthcare industry	12,743 (1,676 firms)
Eliminate financial industry	31,850 (3,833 firms)
Subtotal	**66,027 (8,452 firms)**
Merge with Audit Analytics, and COMPUSTAT business segment data Truncate at the bottom and top 1% and drop firm-years with missing observations (leaving firms-years during 2001-2014)	
Total	**22,645 (3,233 firms)**
For state FCAs analysis (during 2001-2010):	
Keep firm-years with at least two years of data	**18,543 (3,164 firms)**
For SEC whistleblower program analysis (during 2008-2014):	
Keep firm-years with at least six years of data	**7,016 (1,105 firms)**

Panel B: Number of firms affected by general and/or Medicaid-only FCAs during 2001-2010

Notes: *FCA_G (FCA_M)* is an indicator variable that takes the value of 1 if a firm is exposed to at least one general (Medicaid-only) FCA through a state's pension fund investing in that firm. The Medicaid-only FCA sample is used in the falsification test.

N of firms		General FCA (*FCA_G*)			
		0	0 →1	1	Total
Medicaid-only	0	429	953	242	1,624
FCA	0 →1	7	533	231	771
(*FCA_M*)	1	7	46	716	769
	Total	443	1,532	1,189	3,164

Panel C: Distribution of event year

Notes: This table presents when 1,532 treatment firms are first exposed to general FCAs.

Year	2002	2003	2004	2005	2006	2007	2008	2009	2010	Total
Freq.	37	66	242	223	171	187	173	178	255	1,532
%	2.42	4.31	15.80	14.56	11.16	12.21	11.29	11.62	16.64	100

Table 2. State Pension Funds in the Sample

Notes: This table reports the list of state pension funds that owned shares of firms in our sample during 2001-2010 as identified in 13F filings. In column 2, N indicates the total number of firm-years each fund holds during 2001-2010.

State Pension Fund Name	N	Percent
Alaska Retirement Management Board	6	0.01
California Public Employees Retirement System	14,543	12.82
California State Teachers Retirement System	5,176	4.56
Colorado Public Employees Retirement Association	9,867	8.7
Florida State Board of Administration	10,871	9.58
Kentucky Teachers Retirement System	6,036	5.32
Missouri Employee Retirement System	591	0.52
Montana Board of Investments	24	0.02
New Jersey Better Educational Savings Trust Fund	126	0.11
New Jersey Common Pension Fund A	861	0.76
New Jersey Common Pension Fund D	1	0
New Jersey Common Pension Fund E	25	0.02
New Jersey State Employees Deferred Compensation	459	0.4
New Mexico Educational Retirement Board	2,207	1.95
New York State Common Retirement Fund	8,153	7.19
New York State Teachers Retirement System	6,362	5.61
Ohio Public Employees Retirement System	11,888	10.48
Ohio Teachers Retirement System	9,862	8.69
Oregon Public Employees Retirement Fund	810	0.71
Pennsylvania Public School Employees System	8,459	7.46
Texas Employees Retirement System	1,890	1.67
Texas Teachers Retirement System	6,057	5.34
Virginia Retirement System	5,048	4.45
Wisconsin Investment Board	4,142	3.65
Total	113,464	100

Table 3. Descriptive Statistics

Note: All variables are defined in Appendix A.

Panel A. Sample of state FCAs analysis during 2001-2010

Variable	N	mean	sd	p5	p50	p95
FCA_G	18,543	0.761	0.427	0	1	1
FCA_M	18,543	0.409	0.492	0	0	1
FCA_G_PCT	18,543	0.006	0.007	0	0.004	0.018
OWN	18,543	0.789	0.408	0	1	1
SH_PCT	18,543	0.016	0.019	0	0.010	0.043
F-score	18,543	0.978	0.561	0.313	0.861	2.010
M-score	11,915	-2.674	0.957	-3.990	-2.641	-1.403
Audit fees (in thousands)	18,543	1,284	1,893	86	629.8	4,865
Total Assets (in millions)	18,543	2,007	5,405	17.64	365.7	9,460
MTB	18,543	2.560	4.442	0.302	1.914	7.770
InstOwn	18,543	0.551	0.307	0.029	0.598	0.995
BIG4	18,543	0.773	0.419	0	1	1
AudTenure	18,543	5.376	2.879	1	5	11
StdRev	18,543	0.228	0.321	0.017	0.133	0.723
Merger	18,543	0.073	0.260	0	0	1
Discont	18,543	0.164	0.370	0	0	1
Restructure	18,543	0.300	0.458	0	0	1
Foreign	18,543	0.247	0.431	0	0	1
IndGrowth	18,543	1.046	0.096	0.871	1.063	1.188
Leverage	18,543	0.755	0.544	0.238	0.707	1.337
Loss	18,543	0.304	0.460	0	0	1
FreeCF	18,543	0.026	0.164	-0.194	0.042	0.204
NetFin	18,543	0.731	0.443	0	1	1
ROA	18,543	0.010	0.193	-0.269	0.0383	0.187
ln(Segment)	18,543	1.386	0.640	0	1.386	2.303
InvRec	18,543	0.276	0.185	0.0362	0.249	0.631
LtDebt	18,543	0.503	0.356	0.131	0.474	0.912
StdROA	18,543	0.081	0.229	0.004	0.035	0.255
Growth	18,543	0.109	0.347	-0.265	0.068	0.563

Panel B. Sample of the internal control analysis during 2005-2010

Variable	N	mean	sd	p5	p50	p95
IntAudCost (in thousands)	369	2,864	2,828	400	1,799	8,902
IntAudHour	288	1,832	2,609	100	829	6,500
%ChIntAudStaff	376	0.046	0.286	-0.180	0	0.330
ExtAudHour	255	13,533	12,485	790	10,550	38,100
Audit fees (in thousands)	376	3,874	3,433	553.5	2,699	12,459
Total Assets (in millions)	376	9,591	12,779	306.8	3,891	41,444

Table 4. Effect of Exposure to a General State FCA on the Probability of Accounting Fraud
Panel A. Effect on the F-score

Notes: This table reports the results of regression model (1) that estimates the effect of exposure to a general FCA (*FCA_G*) on the F-score. *FCA_G* is an indicator variable that takes the value of 1 when a firm is exposed to at least one state's FCA with a general *qui tam* provision through the state's pension fund holding the shares of that firm. *OWN* is an indicator variable that is equal to 1 if a firm's shares were owned by at least one state pension fund in the lagged year. In column 4, sample firm-years are included only when a firm is owned by at least one state pension fund in the lagged year (i.e., when *OWN* is equal to 1). In column 5 *FCA_G_PCT* (*SH_PCT*) is the percentage of shares of a firm held by FCA (any) state pension funds. All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm (3,164 clusters in columns 1, 2, 3, and 5, and 2,497 clusters in column 4), and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)	(5)
Model:	Control size	Add controls	Base Model	Keep if *OWN* = 1	Intensive margin
Dependent Variable:	F-score	F-score	F-score	F-score	F-score
FCA_G	**-0.062****	**-0.068*****	**-0.069*****	**-0.089*****	
	(-2.464)	**(-2.782)**	**(-2.927)**	**(-3.538)**	
FCA_G_PCT					**-1.886****
					(-2.295)
OWN	0.023	0.024	0.015		
	(0.871)	(0.945)	(0.610)		
SH_PCT					-0.625*
					(-1.935)
ln(Assets)	0.229***	0.227***	0.200***	0.207***	0.203***
	(14.759)	(13.853)	(12.743)	(11.712)	(12.841)
MTB		0.001	0.001	0.000	0.001
		(1.072)	(0.793)	(0.220)	(0.763)
InstOwn		-0.039	-0.074**	0.073*	-0.076**
		(-1.022)	(-2.026)	(1.813)	(-2.051)
BIG4		-0.044***	-0.038**	-0.011	-0.030**
		(-2.728)	(-2.562)	(-0.588)	(-2.037)
StdRev		0.036	0.031	0.023	0.028
		(1.621)	(1.388)	(0.948)	(1.290)
Merger		0.140***	0.127***	0.124***	0.127***
		(9.120)	(8.508)	(8.069)	(8.485)
Discont		-0.076***	-0.063***	-0.054***	-0.062***
		(-7.113)	(-5.963)	(-4.881)	(-5.907)
Restructure		-0.045***	-0.037***	-0.029***	-0.036***
		(-4.931)	(-4.211)	(-3.185)	(-4.108)
IndGrowth		0.197***	0.201***	0.180***	0.196***
		(5.135)	(5.495)	(4.939)	(5.379)
Leverage		0.032*	0.033**	0.088***	0.033***
		(1.807)	(2.523)	(3.227)	(2.593)
Loss			-0.176***	-0.152***	-0.175***
			(-15.655)	(-12.884)	(-15.574)
FreeCF			-0.633***	-0.771***	-0.633***
			(-9.472)	(-10.233)	(-9.494)

NetFin			0.159***	0.111***	0.157***
			(19.434)	(13.308)	(19.203)
Observations	18,543	18,543	18,543	14,344	18,543
Adjusted R^2	0.573	0.580	0.615	0.663	0.615
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Panel B. Effect of Exposure to a General State FCA on Individual Components of the F-score

Notes: This table reports estimation results when we regress each component of the F-score (Dechow et al. 2011) on exposure to a general FCA (*FCA_G*) to assess which component is most responsible for the decline in F-scores reported in Table 4 Panel A. In column 1, we use the predicted value, instead of the F-score, as the dependent variable as calculated by

$$\begin{aligned}\text{Predicted Value} = {} & -7.893 + 0.79 \times \text{Changes in noncash operating assets(rsst_acc)} \\ & + 2.518 \times \text{Changes in receivables(ch_rec)} + 1.191 \times \text{Changes in inventory(ch_inv)} \\ & + 1.979 \times \text{\% of Non-Cash and Non-PP\&E(soft_assets)} \\ & + 0.171 \times \text{Changes in cash sales(ch_cs)} + (-0.932) \times \text{Changes in ROA(ch_roa)} \\ & + 1.029 \times \text{Equity or debt issuance(issue)}\end{aligned}$$

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In columns 2 to 8, we break down the predicted value and regress each component on the exposure to FCA (*FCA_G*). At the bottom of the table, we report adjusted coefficients by multiplying the estimated coefficient by each loading value used to calculate the predicted value. The sum of these adjusted coefficients is equal to the coefficient estimated in column 1 by construction. Control variables used in column 3 of Table 4 Panel A are included in all specifications. All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm (3,164 clusters), and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Dependent Variable:	(1) Predicted Value	(2) rsst_acc	(3) **ch_rec**	(4) ch_inv	(5) **soft_assets**	(6) ch_cs	(7) ch_roa	(8) issue
FCA_G	**-0.077*** **	-0.009	-0.013***	-0.002	-0.014**	-0.037	-0.006	-0.005
	(-3.450)	(-0.969)	(-4.933)	(-0.801)	(-2.184)	(-1.606)	(-0.781)	(-0.565)
Observations	18,543	18,543	18,543	18,543	18,543	18,543	18,543	18,543
Adjusted R^2	0.667	0.256	0.156	0.140	0.869	0.245	0.030	0.481
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year
Multiply coefficients by		0.79	2.518	1.191	1.979	0.171	-0.932	1.029
Sum = **- 0.077**		-0.007	**-0.034**	-0.002	**-0.027**	-0.006	0.006	-0.005

Panel C. Effect of Exposure to a General State FCA on the M-score

Notes: This table reports estimation results when we regress the M-score (Beneish 1999) or its individual components on exposure to a general FCA (*FCA_G*) to assess which component is most responsible for the decline in M-scores reported in column 1. The M-score is calculated by

$$\begin{aligned}\text{M-score} = {} & -4.84+0.920\times\text{Days' sales in receivables}(DSRI)+0.528\times\text{Gross Margin}(GMI)\\ & +0.404\times\text{Asset quality}(AQI)+0.892\times\text{Sales growth}(SGI)+0.115\times\text{Depreciation rate}(DEPI)\\ & +(-0.172)\times\text{SG\&A expenses}(SGAI)+(-0.327)\times\text{Leverage}(LVGI)+4.679\times\text{Accruals}(ACCR)\end{aligned}$$

In columns 2 to 9, we break down the M-score and regress each component on the exposure to FCA (*FCA_G*). At the bottom of the table, we report adjusted coefficients by multiplying the estimated coefficient by each loading value used to calculate the M-score. The sum of these adjusted coefficients is equal to the coefficient estimated in column 1 by construction. Control variables used in column 3 of Table 4 Panel A are included in all specifications. All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm (2,368 clusters), and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	M-score	**DSRI**	GMI	**AQI**	**SGI**	DEPI	SGAI	LVGI	**ACCR**
FCA_G	**-0.306*** **	-0.071**	-0.017	-0.210*	-0.073***	-0.037**	0.053***	-0.018	-0.016**
	(-4.294)	(-2.318)	(-0.724)	(-1.767)	(-3.570)	(-2.214)	(3.460)	(-0.639)	(-2.092)
Observations	11,676	11,676	11,676	11,676	11,676	11,676	11,676	11,676	11,676
Adjusted R^2	0.257	0.013	0.082	0.010	0.287	-0.004	0.093	0.088	0.397
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	firm & year	firm & year	firm & year	firm & year	firm & year	firm & year	firm & year	firm & year	firm & year
Multiply coefficients by		0.920	0.528	0.404	0.892	0.115	-0.172	-0.327	4.679
Sum = **- 0.306**		**-0.065**	-0.009	**-0.085**	**-0.065**	-0.004	-0.009	0.006	**-0.074**

Table 5. Effect of Exposure to a General State FCA on Audit Fees

Notes: This table reports the results of regressions that estimate the effect of exposure to a general FCA (*FCA_G*) on audit fees (*ln(audit fees)*). *FCA_G* is an indicator variable that takes the value of 1 when a firm is exposed to at least one state's FCA with a general *qui tam* provision through the state's pension fund holding the shares of that firm. *OWN* is an indicator variable that is equal to 1 if a firm's shares were owned by at least one state pension fund in the lagged year. In column 3, sample firm-years are included only when a firm is owned by at least one state pension fund in the lagged year (i.e., when *OWN* is equal to 1). In column 4, *FCA_G_PCT* (*SH_PCT*) is the percentage of shares of a firm held by FCA (any) state pension funds. All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm (3,164 clusters in columns 1, 2, and 4, and 2,497 clusters in column 3), and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)
Model:	Control size	Base Model	Keep if *OWN* = 1	Intensive margin
Dependent Variable	ln(audit fees)	ln(audit fees)	ln(audit fees)	ln(audit fees)
FCA_G	**-0.052****	**-0.044***	**-0.058****	
	(-2.213)	**(-1.906)**	**(-2.303)**	
FCA_G_PCT				**-1.849****
				(-2.399)
OWN	-0.129***	-0.105***		
	(-5.245)	(-4.383)		
SH_PCT				-0.031
				(-0.091)
ln(Assets)	0.357***	0.382***	0.340***	0.376***
	(25.650)	(26.583)	(19.793)	(25.918)
Loss		0.074***	0.056***	0.078***
		(6.970)	(4.623)	(7.214)
ROA		-0.068**	-0.068	-0.069**
		(-2.081)	(-1.485)	(-2.096)
InvRec		0.243***	0.157*	0.233***
		(3.254)	(1.762)	(3.097)
Quick		-0.018***	-0.019***	-0.019***
		(-4.883)	(-4.204)	(-4.891)
LtDebt		0.063**	0.156***	0.068***
		(2.459)	(3.467)	(2.646)
StdROA		0.040	0.010	0.043
		(1.393)	(0.529)	(1.610)
Growth		-0.051***	-0.041***	-0.046***
		(-4.644)	(-2.932)	(-4.220)
Merger		0.018	0.019	0.019
		(1.454)	(1.535)	(1.548)
ln(Segment)		0.080***	0.095***	0.077***
		(5.631)	(6.325)	(5.309)
Foreign		0.036**	0.036**	0.041***
		(2.401)	(2.236)	(2.675)
BIG4		0.186***	0.097***	0.207***
		(9.679)	(3.937)	(10.664)

AudTenure		0.012***	0.008***	0.014***
		(4.131)	(2.659)	(4.853)
Observations	18,543	18,543	14,344	18,543
Adjusted R^2	0.927	0.931	0.929	0.930
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Table 6. Effect of Exposure to a General State FCA on Internal Audit Quality

Notes: This table reports the results of regressions that estimate the effect of exposure to a general state FCA on internal audit cost (*IntAudCost*), internal audit hours (*IntAudHour*), and the percentage change in the number of internal audit staff (*%ChIntAudStaff*). *FCA_G* is an indicator variable that takes the value of 1 when a firm is exposed to at least one state's FCA with a general *qui tam* provision through the state's pension fund holding the shares of that firm. *OWN* is an indicator variable that is equal to 1 if a firm's shares were owned by at least one state pension fund in the lagged year. *FCA_G_PCT* (*SH_PCT*) is the percentage of shares of a firm held by (any) FCA state pension funds. All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by industry, and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	IntAudCost			IntAudHour		%ChIntAudStaff	
FCA_G	**-718.739**		**470.913**	**909.637**		**-0.014**	
	(-1.441)		**(0.647)**	**(1.354)**		**(-0.144)**	
FCA_G_PCT		**-3,181.628**			**-767.488**		**16.538***
		(-0.116)			**(-0.022)**		**(1.943)**
OWN	193.749		623.154	-137.868		-0.141	
	(0.300)		(0.821)	(-0.177)		(-1.170)	
SH_PCT		-4,684.121			24,090.821		-7.839*
		(-0.381)			(1.186)		(-2.021)
ln(Assets)	759.978***	782.211***	-219.274	-104.280	-331.851*	-0.003	0.001
	(5.789)	(6.147)	(-1.128)	(-0.476)	(-1.727)	(-0.171)	(0.063)
MTB	-12.063	-12.876	23.002	21.702	22.845	0.002	0.002
	(-0.709)	(-0.731)	(1.282)	(0.959)	(1.295)	(1.404)	(1.149)
InstOwn	-1,246.398	-1,192.613	-115.945	67.637	-484.380	-0.202	-0.153
	(-1.513)	(-1.407)	(-0.155)	(0.094)	(-0.717)	(-1.680)	(-1.687)
BIG4	-1,111.895*	-1,151.183*	-415.252	-495.211	-435.858	0.078	0.048
	(-1.707)	(-1.753)	(-0.584)	(-0.733)	(-0.698)	(0.670)	(0.480)
ln(audit fees)	1,551.675***	1,531.031***	1,437.296***	1,481.827***	1,526.818***	-0.034	-0.058**
	(4.452)	(4.203)	(4.697)	(3.621)	(5.448)	(-1.540)	(-2.191)
StdRev	-43.300	32.651	549.295	1,003.691	296.393	-0.157	-0.132
	(-0.106)	(0.076)	(0.688)	(1.069)	(0.394)	(-0.792)	(-0.676)
Merger	-270.407	-263.050	-0.846	-111.370	-29.703	0.062	0.069*
	(-1.225)	(-1.177)	(-0.002)	(-0.199)	(-0.057)	(1.611)	(2.030)
Restructure	-564.662**	-543.923**	-772.765**	-557.947*	-873.571**	-0.018	0.002
	(-2.327)	(-2.338)	(-2.106)	(-1.996)	(-2.578)	(-0.632)	(0.072)
Foreign	-856.615**	-861.105**	-1,245.956**	-632.489	-1,206.500***	-0.058*	-0.041
	(-2.212)	(-2.116)	(-2.630)	(-1.302)	(-2.758)	(-1.802)	(-1.590)
Leverage	749.626	732.833	-345.018	-733.116	-151.353	-0.064	-0.039
	(0.976)	(0.912)	(-0.384)	(-0.716)	(-0.159)	(-0.608)	(-0.431)
Loss	-166.451	-171.857	564.589	341.060	418.934	0.064	0.074
	(-0.413)	(-0.431)	(0.827)	(0.566)	(0.586)	(0.957)	(1.245)
ROA	978.032	798.387	2,715.548	3,521.038	2,510.212	0.156	0.150
	(0.473)	(0.403)	(0.813)	(0.975)	(0.767)	(0.623)	(0.613)
ICW	298.102	278.186	-1,486.155***	-1,472.094***	-1,465.770***	0.024	0.003
	(0.534)	(0.518)	(-4.249)	(-4.015)	(-4.669)	(0.507)	(0.056)

ExtAudHour				-0.012			
				(-0.878)			
Observations	369	369	288	255	288	376	376
Adjusted R^2	0.659	0.659	0.102	0.080	0.106	0.061	0.099
Fixed effects	Industry & Year	Industry & Year	Industry & Year	Industry & Year	Industry & Year	Industry & Year	Industry & Year

Table 7. Falsification Test Using the Medicaid FCA

Notes: This table reports the results of a falsification test that compares the effect of exposure to the general FCA versus Medicaid-only FCA on F-scores and audit fees. Columns 1 and 4 report benchmark results from column 3 of Table 4 Panel A and column 2 of Table 5, respectively. In Columns 2-3 and 5-6, we use a sab-sample of firms that that experienced changes in exposure to the general and Medicaid FCAs in different years. In Columns 2 and 5, we regress *F-score* and *ln(audit fees)* on exposure to a general state FCA (*FCA_G*), respectively. In Columns 3 and 6, we regress *F-score* and *ln(audit fees)* on exposure to a Medicaid-only state FCA (*FCA_M*), respectively. In the F-score (audit fees) tests, we include a full set of control variables used in column 3 of Table 4 Panel A (column 2 of Table 5). All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm, and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Model:	Table 4(A) Column 3	Sub-sample		Table 5 Column 2	Sub-sample	
Dependent Variable:	Fscore	Fscore	Fscore	ln_audfee	ln_audfee	ln_audfee
FCA_G	-0.069***	-0.079*		-0.044*	-0.088**	
	(-2.927)	(-1.766)		(-1.906)	(-2.413)	
FCA_M			-0.044			0.032
			(-1.540)			(1.223)
Chi-square test for equality of coefficients		0.43			7.46	
(p-value)		(0.5096)			(0.0063)	
Observations	18,543	3,024	3,024	18,543	3,024	3,024
Adjusted R^2	0.615	0.594	0.593	0.931	0.899	0.899
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Table 8. Test for Endogenous FCA Adoption by States

Notes: This table reports the results of robustness tests for an endogenous effect coming from firms' headquarter states. Columns 1 and 4 report benchmark results from column 3 of Table 4 Panel A and column 2 of Table 5, respectively. In columns 2 and 5, we replace year fixed effects with firms' headquarter state × year fixed effects. In columns 3 and 6, we exclude firm-years after firms' headquarter states passed general FCAs. In the F-score (audit fees) tests, we include a full set of control variables used in column 3 of Table 4 Panel A (column 2 of Table 5). All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm, and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Model:	(1) Table 4(A) Column 3	(2) Within HQ state×Year	(3) Drop after state passed FCA	(4) Table 5 Column 2	(5) Within HQ state×Year	(6) Drop after state passed FCA
Dependent Variable:	F-score	F-score	F-score	ln(audit fees)	ln(audit fees)	ln(audit fees)
FCA_G	-0.069*** (-2.927)	-0.069*** (-2.948)	-0.069*** (-2.967)	-0.044* (-1.906)	-0.042* (-1.815)	-0.047* (-1.920)
Observations	18,543	18,504	17,386	18,543	18,504	17,386
Adjusted R^2	0.615	0.615	0.617	0.931	0.931	0.930
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & HQ state×Year	Firm & Year	Firm & Year	Firm & HQ state×Year	Firm & Year

Table 9. Sensitivity of Treatment Effects to Pension Fund Characteristics

Notes: This table reports the sensitivity of the main treatment effects to a characteristic of a state pension fund. In columns 2 and 5, we include indicators for each state fund's ownership along with firm and year fixed effects. In columns 3 and 6, we include indicators for each state from which a state pension fund ownership of a firm is coming. In the F-score (audit fees) tests, we include a full set of control variables used in column 3 of Table 4 Panel A (column 2 of Table 5). All control variables are defined in Appendix A. The sample period is from 2001 to 2010. Standard errors are clustered by firm, and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Model:	(1) Table 4(A) Column 3	(2) Fund Indicators	(3) Fund state indicators	(4) Table 5 Column 2	(5) Fund indicators	(6) Fund state indicators
Dependent Variable:	F-score	F-score	F-score	ln(audit fees)	ln(audit fees)	ln(audit fees)
FCA_G	-0.069***	-0.053**	-0.060**	-0.044*	-0.031	-0.032
	(-2.927)	(-2.205)	(-2.524)	(-1.906)	(-1.340)	(-1.351)
Observations	18,543	18,543	18,543	18,543	18,543	18,543
Adjusted R^2	0.615	0.617	0.617	0.931	0.931	0.931
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Table 10. Effect of the SEC Whistleblower Program

Notes: This table reports the results of regression model (3), which estimates the effect of the SEC's whistleblower program on the F-score (columns 1 and 2) and audit fees (columns 3 and 4). In columns 1 and 3, *NoFCA_G* takes the value of 1 if firms were not exposed to any general state FCA between 2008 and 2010 (i.e., before the inception of the SEC whistleblower program in 2011). In columns 2 and 4, *NoFCA_M* takes the value of 1 if firms were not exposed to any Medicaid-only state FCA before the inception of the SEC program. For the Medicaid-only FCA tests, we exclude firms that were not affected by a general FCA between 2008 and 2010 (*NoFCA_G=1*) to avoid any confounding effect from firms that respond to the adoption of SEC whistleblower provision. *SECWB* is an indicator variable equal to 1 if firms' fiscal year end is 2011 or onward. The interaction term *NoFCA_G×SECWB* (*NoFCA_M×SECWB*) is the main variable of interest that captures whether the impact of the SEC whistleblower program was greater for firms that were not previously exposed to general (Medicaid-only) FCAs than those exposed. In the F-score (audit fees) tests, we include a full set of control variables used in column 3 of Table 4 Panel A (column 2 of Table 5). All control variables are defined in Appendix A. The sample period is from 2008 to 2014. Standard errors are clustered by firm, and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)
Dependent Variable:	F-score	F-score	ln(audit fees)	ln(audit fees)
NoFCA_G×SECWB	-0.073**		-0.040**	
	(-2.445)		(-2.103)	
NoFCA_M×SECWB		0.024		-0.026*
		(1.404)		(-1.802)
Chi-square test for equality of coefficients	17.94		0.64	
(p-value)	(<0.0001)		(0.4254)	
Observations	7,016	5,676	7,016	5,676
Adjusted R^2	0.701	0.720	0.972	0.973
Controls	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year