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DOES THE THREAT OF WHISTLEBLOWING REDUCE ACCOUNTING FRAUD?

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ABSTRACT

This paper examines the deterrence effect of whistleblowing threats on the probability of accounting fraud. I predict that firms' exposure to whistleblowing laws can create ex ante incentives for managers to deter fraud. If a state has adopted a False Claims Act (FCA), whistleblowers who report fraud involving that state's pension fund investments are eligible for monetary rewards. Using staggered adoption of FCAs by states between 1987 and 2010, and comparing firms that are now exposed to a state FCA due to state pension fund ownership in the firm with other control firms, I find firms' exposure to the threat of whistleblowing under the FCA reduces the probability of accounting fraud by 7%. Also, when the Securities and Exchange Commission (SEC) introduced the Dodd-Frank whistleblower provision in 2011, the probability of accounting fraud declined more pronouncedly among firms that had not been affected previously by state FCAs. I also find firms' exposure to whistleblowing threats reduces audit fees by 5%, consistent with concerned managers tightening internal controls to detect fraud, which can substitute for external audits and/or reduce the control risk auditors face. Overall, this paper sheds light on the policy debate over the effectiveness of whistleblower provisions in preventing fraud.

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]

Regulators increasingly rely on private citizens to detect corporate fraud. Federal and state governments have implemented whistleblower provisions that provide financial rewards and protection from retaliation to encourage more whistleblowers to come forward. For example, the high-profile Enron scandal prompted the Sarbanes-Oxley Act (SOX, 2002) that strengthened whistleblower protections and compliance monitoring. Bernard Madoff's Ponzi scheme that unraveled in 2008 also motivated the US Securities and Exchange Commission (SEC) to implement the Dodd-Frank whistleblower program (2011).[1] Prior studies on whistleblowing document types of whistleblowers (Dyck et al. 2010), the role of whistleblower involvement in enforcement actions (Call et al. 2016), and the determinants or outcomes of whistleblowing allegations (Bowen et al. 2010; Wilde 2017). Despite such growing legislative and academic attention on whistleblowers, our understanding of the effectiveness of whistleblowing provisions on deterring fraud is limited.

In this paper, I examine whether regulatory initiatives intended to encourage whistleblowing on financial fraud can deter accounting manipulation ex ante.[2] I rely on two

[1] The Enron and Madoff cases 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.

[2] This ex ante (or unconditional) approach is different from the ex post (or conditional) approach of Wilde (2017) that uses actual whistleblowing allegations to examine changes in financial misreporting after firms are caught by whistleblowers.

whistleblower provisions that have arguably increased the whistleblowing risk to firms, one at the state level and one at the federal level. First, a False Claims Act (FCA) protects whistleblowers who bring to light fraud against a government. In particular, under state FCAs, whistleblowers can obtain financial rewards by reporting financial fraud of a firm whose shares are owned by a state pension fund (e.g., California Public Employees Retirement System) if that state (e.g., California) has an FCA with a general *qui tam* provision (Rapp 2007). A *qui tam* refers to a provision that allows a private citizen to file a lawsuit on behalf of the government and obtain a portion of the money recovered. Unlike *qui tam* provisions that only cover Medicaid fraud, FCAs with a general *qui tam* provision (a general FCA hereafter) can protect against financial fraud. Therefore, if a state passed a general FCA and the state-sponsored pension fund holds the shares of a firm, it exposes the firm to whistleblowing threats (treatment firms), regardless of the firm's location. In other words, I use within-firm variation in state pension fund ownership and variations in state FCAs, such as the year of the passage and the scope of FCA coverage, to identify firms that are more or less subject to whistleblowing laws.

Second, I also use the SEC's whistleblower program that was implemented in 2011 as a part of the Dodd-Frank Act (2010). This federal whistleblower provision aims to combat financial fraud. The rule allows whistleblowers to provide tips directly to the SEC, and has a bounty provision that provides a financial reward to whistleblowers if their tips lead to successful enforcement actions. I use firms that were previously exposed to state FCA provisions as a control group. I expect treatment firms that were not exposed previously to any state FCAs to respond more pronouncedly to the new federal whistleblower provision relative to the control group. Without the FCA control group, evaluating the effectiveness of the SEC provision would be

difficult because the federal rule affected all US firms subject to securities law violations at the same time.

The FCA and SEC whistleblower provisions can be good instruments for whistleblowing threats because, unlike SOX, which has a mere anti-retaliation provision, they both provide financial bounties to whistleblowers. Law and economics studies have emphasized the importance of monetary incentives for whistleblowers to come forward (Arce 2010; Dyck et al. 2010; Rapp 2012a). Then, I hypothesize that for managers engaging in misconduct, the enhanced fear of getting caught by whistleblowers increases the likelihood of them correcting the problem because of their legal liabilities and career concerns. Managers who are not personally involved in fraud could also be concerned when they are uncertain whether fraud is occurring at their firm, because not all fraud is committed by the top manager. Moreover, being aware that whistleblowing is more likely to happen, managers would want to know about ongoing problems before regulators do, because both the Department of Justice and the SEC place a premium on a firm's self-disclosure of problems.[3] Hence, the bounty model of whistleblower provisions can create powerful incentives for managers to tighten internal controls to detect and correct ongoing fraud or prevent future fraud. As a result, the probability of accounting fraud will decrease.

I acknowledge, however, that the deterrence effect might be small if whistleblowers and management do not respond to whistleblower provisions. First, both potential whistleblowers and managers might not be aware of the whistleblower rules. Second, the provisions providing monetary rewards might not be enough to compensate for the risks whistleblowers face, such as economic costs and career concerns. Also, given that many whistleblowers come forward for ethical reasons (Arce 2010; Rapp 2012b), the monetary rewards might not effectively incentivize

[3] Dinkoff, A. *Corporate Compliance Programs after Dodd-Frank.* Weil, Gotshal & Manges LLP. See http://www.weil.com/~/media/files/pdfs/corporate_compliance_post_dodd-frank_aelc_oct.pdf for details.

them to blow the whistle on the margin. For example, increasing monetary rewards could attract whistleblowers who have a stronger preference for money. Then, whistleblowing activity is more likely to be interpreted as a monetary decision rather than as a pro-social activity (Benabou and Tirole 2006). This will reduce the incentives of whistleblowers who choose to do so for integrity. Lastly, management of firms that have effective internal hotline and compliance programs in place might not respond to the introduction of rules that allow external whistleblowing.

To quantify the deterrence effect of whistleblowing provisions on accounting fraud, I rely on imputed measures of the probability of accounting manipulation: the F-score (Dechow et al. 2011) and M-score (Beneish 1999). I use these measures because they can be easily calculated based on the fraud prediction models using financial statement variables and have reasonable predictive power.[4] Also, because the economic construct of interest in this paper is the probability of a firm engaging in fraud, I use the imputed measures as a close approximation to fraud likelihood. The alternative research design of using detected frauds in other whistleblowing papers (Bowen et al. 2010; Dyck et al. 2010; Call et al. 2016) is not ideal for a study on the deterrence effect of whistleblowing provisions on the probability of fraud. The probability of a fraud being detected is the probability of a firm engaging in fraud times the detection likelihood (Dyck et al. 2014; Gow et al. 2016). Therefore, if whistleblowing laws help to both reduce the fraud probability and increase the detection rate, detected frauds simply capture the net impact of the two opposing effects.

Despite the desirable features of using imputed scores, they have some limitations. Given that the F-score and M-score are constructed using detected frauds in Dechow et al. (2011) and

[4] Dechow et al. (2011) document that their prediction model correctly classifies 70% of misstating firms as misstating (i.e., the type II error rate is 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.

Beneish (1999), they may simply proxy for the probability of detected fraud instead of the probability of existing 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. I also address the potential measurement error issue by estimating the treatment effect using firm and year fixed effects. If the measurement error of the F-score 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 identifying assumption here is that firms that are exposed to a whistleblower provision and firms that are not would have otherwise similar changes in the F-score.[5] I estimate a series of robustness tests to ensure that the measurement error in the F-score and M-score is not systematically correlated with the measure of exposure to whistleblowing (i.e., changes in state fund ownership and the passage of whistleblower provisions by states). I also have a test addressing the concern that the score measures may capture nondiscretionary or real effects in addition to discretionary effects. For example, the treatment effect may simply reflect risk-taking behavior decreasing after the exposure to the whistleblowing threats even without any effect on the probability of fraud. Additionally, I break down the measures into individual components to further examine whether the changes in the underlying construct of the F-score and M-score after firms' exposure to whistleblower rules are consistent with the pattern of misstatement or restatement found in the prior literature.

[5] I will provide evidence supporting the parallel trends assumption in Section 4.2.

I exploit 24 separate state pension funds located in 16 distinct states between 2001 and 2010. State pension funds disclose their public equity holdings in the SEC 13F filings. After merging 13F data with COMPUSTAT, I identify firms that became exposed to whistleblowing threats under state FCAs between 2001 and 2010 because their shares were owned by at least one state pension fund located in a state with an FCA (hereafter FCA state funds).

In the FCA analysis, I find the probability of fraud, as measured by the F-score and M-score, decreases by 7% when firms are owned by FCA state pension funds. In additional tests, I find changes in revenue and accrual-related subcomponents of the F-score and M-score are the primary drivers of the 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 FCA does not impose any obligations on companies. Therefore, observed effects in a firm are attributable solely to the threat of whistleblowing.

In addition, the lowered probability of fraud will translate to the lowered control risk auditors face (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). Then auditors likely charge lower audit fees. However, auditors might also consider their clients' exposure to whistleblowing a risk. In this case, auditors would increase audit efforts or fee premiums. Consistent with the former conjecture, I find that audit fees decrease by 5% after firms are exposed to state FCAs. To the extent that the lowered probability of fraud is driven by improvement in internal controls, this result suggests internal controls and external audits are substitutes.

I further examine whether the percentage of shares of a firm owned by pension funds has an additional treatment effect. The deterrence effect could be stronger when managers are aware of their exposure to state FCA through state pension fund ownership and this condition is more likely to be satisfied when firms' shares are more intensively owned by state pension fund. However, I find the intensity of ownership by funds does not incrementally reduce the probability of fraud or audit fees. This finding implies exposure to whistleblowing laws itself is important for the deterrence effect rather than the level of ownership held by state pension funds.

To address potentially endogenous effects related to the state-level decision to pass FCAs, I use two alternative specifications. First, I include firms' headquarter state × year fixed effects. Second, I exclude firm-year observations after firms' headquarter state passed an FCA. If the treatment effect was driven by firms whose likelihood of engaging in fraud are endogenously correlated with the firm's headquarter state's decision to pass an FCA, not by firms exposed to FCAs through any state pension fund's investment as I predict, the treatment effect should be attenuated under the two alternative specifications that separate fund ownership effect from any time-varying effect coming from firms' headquarter location. The results are robust to these alternative specifications.

In addition, the deterrence effect occurs only when firms are owned by state pension funds of states with a general FCA that can cover financial fraud. In a falsification test where I redefine a new treatment variable based on firms' exposure to a Medicaid-only FCA, which protects only against Medicaid fraud, the FCA has no impact on the probability of fraud or on audit fees. This differential treatment effect of FCA type addresses the concern that funds' stock selection toward certain types of firms, regardless of whether the fund is exposed to a general or Medicaid-only FCA, might be driving the main results. I also find that the treatment effect is not driven by state

pension funds' endogenous stock selection in response to their state's FCA adoption. Moreover, the deterrence effect of whistleblowing threats remains mostly unchanged even after I include separate indicator variables for ownership of each state pension fund or funds' state in addition to firm and year fixed effects, which rules out the possibility that a particular fund or state where the fund is located is driving my results.

In the analysis of the SEC's whistleblower program (2011) during the sample period of 2008 to 2014, when firms not already exposed to state FCAs are exposed to the SEC whistleblower provision, the probability of fraud decreases by 7.3%, and audit fees decrease by 4% relative to firms that were already exposed to FCAs. The treatment effect is very close to the effect estimated in the FCA setting, which corroborates the deterrence effect of whistleblowing laws. The SEC analysis also helps to alleviate the concern in the FCA test that pension funds located in states with a general FCA likely have different stock selection than funds located in states with a Medicaid-only FCA, resulting in no effect in the falsification test. Because the degree to which firms are exposed to the SEC whistleblowing is determined by whether their shares were owned by FCA pension funds, not by which fund or state invested in the firm, the SEC test does not suffer from the correlated omitted variable: stock selection caused by the type of FCA.

This paper contributes to accounting, finance, and law literature in several respects. First, it quantifies the effect of whistleblowing laws on the probability of fraud, using new regulatory settings that facilitate my identification strategy. This approach is different from prior whistleblowing literature relying on detected fraud or restatement of accounting information (Bowen et al. 2010; Dyck et al. 2010; Call et al. 2016; Wilde 2017). Inferences based on ex post outcome variables can be misleading because whistleblower provisions likely increase the likelihood of detection while reducing the likelihood of existing fraud. Thus, the observed net

effect on detected fraud is not informative of the changes in fraud likelihood. To overcome this problem, I define treatment and control groups based on their exposure to whistleblowing laws due to state pension funds' ownership, and use imputed measures of the probability of accounting manipulation as a close approximation to the prevalence of fraud. Second, this paper asks an unexplored question of whether financial incentives can encourage more whistleblowing, and how the fear of exposure by whistleblowers incentivizes truthful reporting by managers. Given the state FCA and federal SEC whistleblower provisions provide a significant financial incentive that was not available under SOX, this paper addresses whether these whistleblower provisions with bounty models change managers' misreporting incentives effectively. Third, traditional corporate governance literature has overlooked the monitoring role of employees or other players. My paper provides insight into the mechanism through which different players in the market can effectively affect corporate governance as a potential whistleblower and its impact on external auditing. Lastly, this paper informs regulators and policymakers and sheds light on the policy debate over the effectiveness of whistleblower laws. The findings illuminate the integral role of whistleblowers in the transparent flow of information, and suggest whistleblower provisions represent a useful and understudied policy lever in making financial markets work better.

2. Related Literature, Background, and Hypothesis Development

2.1. Whistleblowing Literature

Following a series of corporate scandals and subsequent legislation on whistleblower programs, recent studies provide evidence that whistleblowing plays a key role in uncovering financial fraud. Dyck et al. (2010) document types of whistleblowers. Employees are the most common type of whistleblower (12% of a 216 corporate fraud sample during 1996-2004), followed by media, analysts, and short-sellers. The traditional corporate governance literature has given

little attention to employees as important players (Dyck et al. 2010; Rapp 2010). Bowen et al. (2010) investigate the characteristics of firms getting 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. (2016) find that employee whistleblowers play an important role in the investigation process by providing valuable information to regulators and facilitating the enforcement actions against target firms. They show that whistleblower involvement is associated with more severe enforcement actions, including more penalties and longer prison sentences.[6]

Baloria et al. (2015) 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 as the provision is expected to improve shareholder protection more for these firms. Wilde (2017) examines whether whistleblowing allegations subsequently deter fraud due to the threat of increased monitoring. He finds that, following the employee whistleblowing allegations, firms tend to reduce financial misreporting and tax aggressiveness relative to their matched control firms.

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

[6] Unlike prior studies using fraud cases uncovered by employee whistleblowers, the inference of my paper is not limited to employee whistleblowing.

economic pressure to remain silent. Social bonds with their co-workers and loyalty to employers make whistleblowers 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).

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, 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). 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, 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 an 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. Taken together, private securities lawsuits have limitations as an instrument of corporate governance relative to whistleblowers (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 whistleblower laws have adopted anti-retaliation and/or bounty provisions. The federal FCA is the oldest US whistleblower provision.[7] Under the FCA, it is unlawful for any entity to knowingly submit a fraudulent claim to the government or defraud the government. In 1986, the 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 US 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 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 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.[8] Together, these features of the FCA have made the act incredibly successful in encouraging more whistleblowers to bring fraud cases.[9] Recently, more whistleblowers have begun to pursue cases even after the government has decided to step back from intervening, implying FCA defendants need to take FCA cases seriously.[10]

[7] 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).
[8] www.morganlewis.com
[9] 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 FCA cases filed in 2015 (632 out of 737) (www.gibsondunn.com).
[10] 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).

States have adopted their own versions of the FCA modeled after the federal FCA at staggered times since 1987. State FCAs vary in terms of whether they protect against only Medicaid fraud or fraud in general, including financial fraud. As of 2010, 17 states and the District of Columbia have general FCAs, and 11 states have Medicaid-only FCAs. The remaining 22 states have not yet adopted FCAs (Appendix B).

Most FCA cases are concentrated in healthcare or defense industries. 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). Still, 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 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).[11] Whistleblowers can claim financial rewards by providing evidence of securities fraud involving the state funds under that state's FCA, as long as the state has a general *qui tam* provision.[12] In my paper, I exploit the staggered passage of an FCA by states and state pension fund ownership of a firm for my identification strategy to examine how firms' exposure to state FCAs changes management's misreporting incentives.

2.3. The SEC Whistleblower Program

[11] 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).
[12] See Appendix B for state-by-state provisions.

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. Despite the fact that SOX was the first federal whistleblower statute to cover financial fraud, 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 can be easily ignored (Kohn 2013). Moreover, SOX's anti-retaliation provision does not protect whistleblowers outside of the firm.

The SEC whistleblower statute of 2011 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

2.4.1. Whistleblowing and Accounting Fraud

To study the deterrence effect of whistleblowing, I exploit differences between firms that are more or less affected by a state FCA as pension funds buy the shares of a firm. Treated firms are those exposed to a general FCA and hence the threat of whistleblowing on financial fraud after a state pension fund located in a state with a general FCA owns their shares. I 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 who are not involved in fraud would be concerned, because they are unaware of all ongoing fraud potential whistleblowers know about. The concerned managers likely tighten internal controls to detect ongoing fraud and correct it before whistleblowers move first. For example, concerned management may assess internal controls and discover their inventory and receivables were too aggressive. Then they likely write down these accounts to avoid potential whistleblowing allegations. In this process, management may devote more resources to internal controls by employing more internal auditors and increasing internal audit hours. Taken together, faced with enhanced whistleblowing threats under the FCA, concerned management likely takes action in a way that reduces the likelihood of accounting manipulation.

[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, and 3,923 tips in 2015. During 2015, the most common complaints were about corporate disclosures and financials (17.5%), false or misleading statements in a company's offering memoranda (15.6%), and manipulation (12.3%), followed by insider trading, Foreign Corrupt Practices Act (FCPA), and so on. See https://www.sec.gov/whistleblower/reportspubs/annual-reports/owb-annual-report-2015.pdf for detail.

To examine the deterrent effect of whistleblowing, ideally, we would first observe all ongoing fraud and see whether the amount or severity of it declines after firms became subject to whistleblower statutes. However, fraud is inherently unobservable, and studying detected fraud does not tell us how big the iceberg is (Dyck et al. 2014). Moreover, because whistleblower statutes are expected to reduce the likelihood of fraud and increase the likelihood of detection, the observed net effect on detected fraud is not informative of the changes in underlying fraud. Therefore, I rely on the imputed measure of the probability of fraud as a closer approximation to underlying fraud, such as the F-score (Dechow et al. 2011) or M-score (Beneish 1999).

I 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).[14] The F-score captures what misstating management typically does to mask their true firm performance.[15] 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. 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.[16] A higher F-score or M-score is associated with a higher probability of accounting misstatement. Therefore, I

[14] I 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.
[15] The detailed calculation of the F-score and M-score is provided in Appendix C.
[16] This paper focuses more on the F-score, because the sample size drops for M-score tests due to missing values of components. Also, interpreting the economic magnitude of the F-score is easier.

expect both the F-score and M-score to decrease when FCA state pension funds own the shares of a firm and expose the firm to enhanced whistleblowing threats.

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, I expect concerned managers to implement more intensive and effective assessments of internal controls over those accounts after the firm becomes subject to whistleblowing under the FCA. Then the remediation of misstatements or material weaknesses in those accounts would be the main driver in lowering the F-score and M-score. Ultimately, this remediation would improve the quality of financial reporting (Ashbaugh-Skaife et al. 2008; Bedard et al. 2012).

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 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 on 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, 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.

2.4.2. Whistleblowing and Audit Risk

As a potential outcome of the deterrence effect of whistleblowing threats, I examine audit fees. Prior literature finds audit fees are positively associated with clients' risk factors, including internal control weaknesses (Raghunandan and Rama 2006; Hogan and Wilkins 2008; Hoitash et al. 2008; Hoag and Hollingsworth 2011; Aobdia et al. 2016) and fraud risk (Doogar et al. 2010; Cassell et al. 2016). Auditors face greater engagement risk when the likelihood is high that the client's internal controls would not prevent or detect misstatement in a timely manner. In this case, auditors likely increase audit efforts by increasing the testing of accounts and the number of audit hours and/or by assigning more experienced staff to the audit, all of which increase audit fees (Hribar et al. 2014). Auditors also charge fee premiums when the probability of accounting fraud is high to compensate for the auditors' potential reputational loss or legal liability. If management is no longer involved in accounting fraud or improves internal controls over financial reporting due to heightened whistleblowing threat, the lowered probability of misstatement or engagement risk for auditors should be priced in equilibrium. However, auditors might also consider their clients' exposure to whistleblowing a risk and charge higher fees. Therefore, the impact of whistleblowing on audit fees is an empirical question.

2.4.3. Medicaid False Claims Act

This paper focuses on the general FCA because financial-fraud whistleblowing can be rewarded only when a state FCA includes a general *qui tam* provision. By contrast, a Medicaid-only 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 California, New York, Florida, and Virginia state pension funds. These are the states that adopted an FCA with a general *qui tam* provision rather than a Medicaid *qui tam* provision. Although these lawsuits were against a bank, other non-bank firms' involvement in financial fraud using money invested by state pension funds can also be a target of whistleblowing under state FCAs (Rapp 2007; Rapp 2010).

Importantly, I exclude healthcare industries from the sample to ensure Medicaid-only FCAs do not affect any of my sample firms. Therefore, I should not find the deterrent effect in the test using Medicaid FCAs if, as I predict, general FCAs cause the deterrent effect. This falsification test is informative in validating my instrument for whistleblowing threats—firms' exposure to general FCAs—by alleviating the concern that something else both general and Medicaid-only FCA funds have in common, such as stock selection by state pension funds, is driving the results.

2.4.4. The SEC Whistleblower Program

To corroborate the deterrent effect of whistleblower laws, I also use the SEC whistleblower program. Both state FCA and SEC whistleblower statutes have bounty provisions, which are important to motivate more 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-only), the SEC whistleblower program, as a federal rule, targets all US companies engaging in securities fraud. Given that the program was created as part of the Dodd-Frank Act (2010), isolating which firms are more or less likely to be affected by the SEC whistleblowing statute is difficult. To overcome this problem, I combine the state FCA and the federal SEC provision to identify new treatment and control groups. I split firms based on whether they had been exposed to any state FCA whistleblower provision before the SEC whistleblower program went into effect. I claim that for firms that were not exposed to whistleblowing threat through state FCA funds before the SEC whistleblower provision (during 2008- 2010), the impact of the SEC provision should be greater because it is their first time being exposed to such a threat. Managers in such firms will likely consider the new provision a greater threat than control firms that were already exposed to state FCAs and will take more actions to improve internal control systems and reduce the likelihood of fraud.

3. Sample Selection and Research Design

3.1. Sample Selection

As summarized in Table 1 Panel A, the initial sample consists of US 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, I aggregate the sample at the firm-year level because a firm is held by multiple funds in a given year. I 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, I can keep treatment firms that became exposed to FCAs as FCA state funds started buying shares in them, and control firms that remained independent of any state pension funds. I exclude firms in the healthcare industry to ensure healthcare companies that are subject to Medicaid-only FCAs do not drive the treatment effect of

general FCAs. I also eliminate financial firms because Dechow et al. (2011) did not include them when computing the F-score. After I merge the sample with Audit Analytics and COMPUSTAT business segment data, the final sample has 23,862 firm-year observations with 4,353 unique firms during 2001 through 2014. In the analysis using state FCAs, I use 18,543 firm-years (3,164 unique firms) during 2001 to 2010. In the analysis using the SEC whistleblower provision, I use 7,016 firm-years (1,105 unique firms) during 2008 to 2014.

3.2. Research Design

3.2.1. State False Claims Act

To examine how firms respond to the threat of whistleblowing, I first exploit firms' exposure to state FCAs. Using panel data, with firms indexed by *i* and years indexed by *t*, I estimate the following regression model:

$$F\text{-}score_{it} = \beta_1 FCA\_G_{it} + \beta_2 OWN_{i,t-1} + \sum_k \lambda_k Controls_{it} + Firm\ FE + Year\ FE + \varepsilon_{it}, \quad (1)$$

where *FCA_G* is an indicator variable that takes the value of 1 if a firm is exposed to at least one state's general FCA through the state's pension fund investing in that firm. *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 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 an FCA (20% of treatment firms). In the former case, *OWN* and *FCA_G* indicators change from 0 to 1 at the same time when an FCA state pension fund buys the shares of the firm. 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. To calculate *OWN* and *FCA_G*, I first define two indicators with the

initial fund-holdings-year-level sample: one coded as 1 if firm *i* was held by a pension fund of the state *s* in year *t-1* and the other coded as 1 if that state has a general FCA in year *t*. Because the majority of firms in the sample are held by multiple funds in a given year, I take the maximum value of the first indicator variable to compute *OWN* when I aggregate the fund-holdings data at the firm level:

$$OWN_{i,t-1} = Max^{s}\left\{I(\text{owned by a pension fund of state } s_{i,t-1})\right\}.$$

I multiply the two indicators and take the maximum value to compute *FCA_G* when I aggregate the fund-holdings data at the firm level:

$$FCA\_G_{it} = Max^{s}\left\{I(\text{owned by a pension fund of state } s_{i,t-1}) \times I(\text{state } s \text{ has general FCA}_{t})\right\}.$$

Defining the indicator variable of fund ownership based on lagged ownership (*OWN)* addresses the selection issue that funds might change their portfolio in the expectation of FCA adoption in their states. I assume that in the year prior to the passage, state-fund managers did not expect the rule change.[17] In very rare cases in my sample, some firms that were initially affected by FCAs become unaffected, because existing FCA state pension funds sell their ownership of the firm. I drop firm-years of non-exposure following their first exposure to an FCA.[18]

To prevent future FCA litigation, management in a firm whose shares are owned by FCA state pension funds would tighten their internal controls or reduce the likelihood of committing

[17] I relax this assumption in an additional analysis (not tabulated) by using ownership at t-2 instead of at t-1 (i.e., I 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.
[18] I find the results do not change when I keep such cases and code them as treated firm-years (*FCA_G*=1), because I expect that firms remain subject to FCAs once they are owned by FCA state pension funds even after funds leave the firm.

fraud. Then such a deterrent effect of whistleblowing should manifest in the lower likelihood of fraud as measured by the F-score or M-score. Therefore, I expect β_1 to be negative.

I include control variables that could affect the F-score and M-score for reasons unrelated to the whistleblowing threat. Because the F-score and M-score consist of measures based on accruals and performance, I control for firm size, market to book, sales volatility, whether a firm had a loss, restructuring activities, mergers or acquisitions, and discontinued operations during the year (Cassell et al. 2016). I also control for institutional ownership, Big 4 auditor, industry growth, leverage, free cash flow, and net financing activities in my main specification. I include firm fixed effects because I exploit within-firm variation in fund ownership through which firms are exposed to FCA whistleblowing threats. Firm fixed effects also control for time-invariant firm characteristics that are associated with the F-score and M-score. I 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.[19]

In the audit fees test, I replace the dependent variable in equation (1) with *ln(audit fees)* and estimate the following regression model for firms indexed by *i* and years indexed by *t*:

$$\ln(audit\ fees)_{it} = \beta_1\ FCA\_G_{it} + \beta_2\ OWN_{i,t-1} + \sum_k \lambda_k\ Controls_{it} + Firm\ FE + Year\ FE + \varepsilon_{it}. \quad (2)$$

I expect β_1 to be negative if the threat of whistleblowing leads managers to improve internal controls over financial reporting, thereby lowering external audit inputs or engagement risk for auditors. However, if clients being subject to FCAs increases litigation risk for auditors in general, I expect the opposite outcome.

[19] As a robustness check, I re-estimate my main regressions using firms' headquarter state × year fixed effects instead of year fixed effects. Results are reported in Table 6.

Following the literature, I control for factors that are known to affect audit fees (Hogan and Wilkins 2008; Hribar et al. 2014), along with firm and year fixed effects. Control variables include firm size, firm performance, complexity and volatility of business, the number of segments, an indicator for a Big4 auditor, audit tenure, existence of internal control weaknesses, and liquidity risk.

3.2.2. The SEC Whistleblower Program

To examine the impact of increased exposure to whistleblowing driven by the SEC whistleblower law, I draw on the FCA setting. I 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 previous FCA analysis, I used firm-year observations during the 2001-2010 period. In this analysis, I rely on firms during the 2008-2014 period because the SEC provision went into effect on August 13, 2011. I 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 FCA state funds during those years (*NoFCA_G* = 0). I estimate the following difference-in-differences regression model for firms indexed by *i* and years indexed by *t*:

$$F-score_{it} = \beta_1\ NoFCA\_G_i \times SECWB_t + \sum_k \lambda_k\ Controls_{it} + Firm\ FE + Year\ FE + \varepsilon_{it}, \quad (3)$$

where *SECWB* is an indicator variable equal to 1 if a firm's fiscal year end is 2011 or onward. The variable of interest is the interaction term between *NoFCA_G* and *SECWB* (*NoFCA_G*×*SECWB*). I 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. I also test whether the adoption of the SEC whistleblower provision affects audit fees.

4. Results

4.1. Descriptive Statistics

Table 1 Panel B summarizes the composition of my sample firms. Out of 3,164 firms used for FCA tests, 443 (1,189) were never (always) affected by general FCAs during 2001 to 2010. The remaining 1,532 firms experienced changes in their exposure to general FCAs (*FCA_G* goes from 0 to 1). Of the 1,532 firms experiencing a change in their exposure to general FCAs during 2001-2010, 533 also experienced changes in their exposure to Medicaid-only FCAs (*FCA_M* goes from 0 to 1) during this period. Table 1 Panel C shows that 550 firms (36%) out of 1,532 treatment firms first got exposed to general FCAs in 2004.[20]

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 funds contract with outside investment management, portfolio holdings for each individual fund 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 when the state funds actively manage their investment that exceeds $100 million, and hence report 13F filings under their names.[21] For this reason, I identified 24 separate state pension funds located in 16 distinct states between 2001 and 2010 (Table 2). According to the 2009 Wilshire report on state retirement systems, 125 state pension funds were sponsored by 50 states and the District of Columbia in 2009. The total market value of these pension plan assets was $2,014 billion, of which $636 billion (31.6%) was invested in 1,500 US public equities. My sample contains 21 state

[20] To check the sensitivity of my results to a particular group of firms that were exposed to general FCAs in a particular year, I perform a Jackknife procedure in my untabulated empirical analysis. For each year during 2002 and 2010, I drop firms that were first affected by general FCAs in that year and calculate separate treatment effects. The magnitude of treatment effects remain stable.

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

pension funds in 2009 (17% of the population funds) that invested in US public equities whose market value totaled $319 billion, accounting for 50% of the total market value of US equities invested by the 125 state pension funds. As shown in Appendix B, the state pension funds in my sample are sponsored by 16 different states (in the shaded rows) that represent a reasonable portion of the state pension fund universe.

Table 3 provides descriptive statistics for the firm-year observations used in the FCA analysis. It shows that 76% (41%) of firm-years from 2001 to 2010 are affected by general (Medicaid-only) FCAs (*FCA_G* or *FCA_M* = 1). The mean (median) F-score is 0.978 (0.861), and the mean (median) M-score is -2.674 (-2.641).

4.2. Effect of the State False Claims Act on the Probability of Accounting Fraud

Table 4 reports results of the effect of exposure to the general FCA (*FCA_G*) on the probability of fraud. In Panel A, I 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 and audit fees. To ensure the coefficient on *FCA_G* is not picking up confounding factors other than potential whistleblowing threats, I add a set of control variables in column 2 and compare the coefficient estimate to that in column 1. I 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 affect the F-score. The effect remains stable in column 3 (a base model), in which I add more control variables that are somewhat directly related to components of the F-score. I include them because they are still useful to control for potential confounding factors such as loss, free cash flow, and net financing (Cassell et al. 2016). 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 0.11 standard deviations in the F-score).[22] Although statistically insignificant, the positive effect of *OWN* on the F-score suggests firms with zero state fund ownership might have had low external financing needs and hence a low probability of managing financial statement numbers.

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. I replace *FCA_G* indicator in my main specification in Table 4 column 3 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., 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 on average two to three years for firms to respond to the increased threat of whistleblowing 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 statistically not different from zero.

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 would be that state pension fund managers may endogenously change the fund portfolio in response to enhanced FCA protection. To address this concern, I remove the treatment variation coming from fund ownership change and focus on the treatment variation driven by states' FCA adoption. In column 4, I keep firm-year observations with state fund ownership being equal to 1 and exclude the *OWN* indicator variable from the regression model 1. Hence, this specification

[22] If I 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%.

treats treatment firms that are affected by 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, it addresses the concern that the treatment effect in my main tests is completely driven by endogenous ownership change by state pension funds due to FCA protection.

In column 5, I examine whether the percentage of shares of a firm owned by a state pension fund is associated with a larger treatment effect of whistleblowing threats. For example, management may not be aware of their firm's exposure to FCAs if a pension fund buys only one share of 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. Therefore, I expect to see a differential effect on the intensive margin as management awareness of the buy-in becomes more acute. Also, when more and more state funds start investing a firm, which likely has a positive association with the percentage of shares of the firm owned by these funds, the threat of whistleblowing driven by their states' FCA may become larger.

I define a *HIGH* indicator variable that is equal to 1 if the percentage of shares of a firm held by FCA state funds is above the median percentage of sample firms with non-zero fund ownership during the year. The regression model estimated in column 5 adds *HIGH* and an interaction term between *FCA_G* and *HIGH* (*FCA_G*×*HIGH*) to model (1). The coefficient estimate on *FCA_G*×*HIGH* (-0.012) is negative, but statistically insignificant, whereas the main

treatment effect of *FCA_G* remains stable. This result indicates the intensity of fund ownership of a firm does not have an additional treatment effect on the probability of fraud, supporting the use of an indicator variable to capture when a firm is first exposed to the FCA.

Table 4 Panel B reports the effect of exposure to FCAs on individual components of the F-score. To understand which account or underlying construct is most responsible for the decline in the F-score documented in Panel A, I 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 I regress the predicted value from Model 1 of Dechow et al. (2011) before monotonic transformation to the F-score. By identity and linearity of ordinary least squares, the coefficient estimate in column 1 should be equal to the sum of the coefficient estimates in each column multiplied by each loading value used to calculate the predicted value.[23] I 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 80% of the decrease in the F-score ((0.034+0.027)/0.077=0.079).

In Panel C, I report the effect of exposure to FCAs on the M-score and each component. In column 1, I 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 0.27 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 concerned management likely conducts remediation of misstatements in revenue recognition and accruals when they are exposed to whistleblowing threats under FCA. Overall, the F-score and M-score decrease when firms are exposed to general

[23] -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)

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 above 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, there is another concern about these score measures. The F-score and M-score might suffer from a similar concern as earnings management measures in that they may be influenced by nondiscretionary or real effects in addition to discretionary or financial statement management effects. For example, I might find my 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 bad effects as well as good effects. To examine whether there was a reduction in growth after firms' exposure to whistleblowing threats under the state FCAs, I regress future sales growth and market share on my main treatment variable *FCA_G* (not tabulated). I find no evidence that future growth decreases after the FCA exposure, which alleviates the concern that managers may reduce their risk-taking behavior and the F-score is simply picking up this non-discretionary effect.

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

Table 5 presents the impact of firms' exposure to FCA on audit fees. In columns 1 and 2, the coefficient estimate on *FCA_G* is negative and statistically significant. The stable coefficient estimate in columns 1 and 2 suggests omitted variables do not influence the estimated treatment effect of FCAs. Focusing on the main specification in column 2, the coefficient estimate on

FCA_G (-0.051) indicates the threat of whistleblowing driven by state FCAs is associated with a 5% reduction in audit fees, which amounts to about $27,000 on average.[24] This result supports the prediction that firms likely improve their internal controls to detect misstatements on a timely basis when whistleblowers are more encouraged to bring them to light under the state FCA. Then auditors would face a lower control risk or expend less audit effort, which leads to lower audit fees. This result suggests internal controls and external audits are substitutes.

In column 3, I focus on the treatment effect driven by FCA passage by states by dropping firm-years without fund ownership. The size of the deterrent effect gets slightly larger and remains statistically significant. This result indicates firms' exposure to FCA due to pension-sponsoring states' passage of an FCA lowers the probability of accounting fraud by 6.6% relative to firms without any change in exposure to FCAs.

In column 4, I do not find an incremental effect of intensive FCA fund ownership on audit fees. In fact, the sign of the coefficient estimate (0.042) on *FCA_G*×*HIGH* is positive and statistically insignificant. This finding likely implies the substitution effect between internal controls and external audits, as suggested by the significantly negative estimate on the main coefficient *FCA_G* (-0.06), can be mitigated because auditors may assess the higher intensity of FCA fund ownership as a risk and charge a higher risk premium.

4.4. Endogenous FCA Adoption by States

One potential concern would be that states' passage of FCAs could be endogenously correlated with changes in the F-score. 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

[24] This economic amount is calculated based on the mean audit fees of treated firms in the pre-*FCA* period ($524,700).

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 the fund ownership change, it will be an issue for a subset of the 20% of treatment firms that became subject to the general FCA due to the passage of FCAs. Importantly, the endogeneity problem arises only among firms that are invested in by a pension fund sponsored by the state in which the firm is located. 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, I repeat my main tests only using a variation of firms whose fund ownership is coming from a different state as their headquarters location. First, I use firms' headquarters state × year fixed effects. Second, I drop firm-year observations of treatment firms after their headquarters state passed a general FCA. If firms' F-score and headquarter state's decision to pass a general FCA are correlated to some extent, the treatment effect observed in Table 4 should get smaller when I drop firm-years after the passage of the law in their headquarters states. The results are documented in Table 6.

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, I replace year fixed effects with firms' headquarters state × year fixed effects. In columns 3 and 6, I exclude firm-years after a firm's headquarter state passed a general FCA. In all specifications, the coefficient on *FCA_G* remains stable, assuaging the concern about the endogenous FCA adoption by states.

4.5. Falsification Test Using the Medicaid FCA

Note that 80% of treatment firms are subject to whistleblowing under FCAs when FCA state funds initiate stock ownership of the firm rather than when state funds with ownership are exposed to new FCAs due to the passage of the law in that state. Therefore, a selection problem can exist if state funds invest in certain types of firms for reasons related to the F-score. 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, I use the Medicaid-only FCA. The general and Medicaid-only FCAs share common features, although only the general FCA protects against financial fraud. Therefore, if some common features of general and Medicaid-only FCA state funds, such as the funds' investment preferences, drove the effect of general FCAs on the F-score and audit fees observed in Tables 4 and 5, we would continue to see the same treatment effect in this Medicaid-only FCA test. Importantly, the Medicaid-only FCA should not affect the F-score and audit fees in any way, because the sample does not include healthcare industries. Moreover, if any concurrent enforcement change along with the passage of general and Medicaid-only FCAs is driving the 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, I 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, I regress the F-score on the exposure to Medicaid-only FCAs (*FCA_M*). For this test, I drop firms that also experienced changes in their exposure to general FCAs at the same time to avoid any spurious effect from exposure to general FCAs. This restriction significantly reduces

the sample size to 2,333 observations (372 firms) from 18,543 observations (3,164 firms). I find the coefficient estimate on FCA_M (-0.036) is roughly half the -0.069 estimate on FCA_G in column 1 and is statistically insignificant.

In column 3, I additionally drop firms that were always affected by general FCAs (i.e., those with *FCA_G* = 1 during all sample periods). The rationale is that if firms have already been exposed to one type of FCA, they are less likely to respond to the later introduction of the other type of FCA. For example, if a general FCA is already in effect, the incremental effect of additional exposure to a Medicaid-only FCA on the probability of fraud or audit fees can be small. The reasoning is not that a Medicaid-only FCA does not protect against accounting fraud, as I predicted. Rather, the general FCA has already had time to make an impact. This additional restriction drops the sample size to 221 observations (65 firms). The coefficient estimate (0.175) on FCA_M becomes positive and statistically insignificant.

In columns 4 to 6, I replicate the same specifications used in columns 1 to 3 after replacing the F-score with audit fees. I do not observe the treatment effect of Medicaid-only FCAs for audit fees. Taken together, the results are consistent with the argument that whistleblowing under an FCA can threaten managers and curb their misreporting incentives only when the FCA covers financial fraud. Moreover, funds' stock selection does not drive the treatment effect of FCAs.

4.6. Sensitivity of Treatment Effects to Pension Fund Characteristics

As Table 2 shows, some state pension funds such as CalPERS account for a large portion of the sample. This unbalanced sample composition raises a concern about characteristics of particular funds driving the main effect. I 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 8, I include indicators for each state fund's ownership along with firm and year fixed

effects. As an alternative specification, I include indicators for each funds' state to account for time-invariant fund states' characteristics in columns 3 and 6. For states that have only one state pension fund in my sample (e.g., Colorado), an indicator for the state and an indicator for that state's fund are indistinguishable.

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 weaker. Overall, even after controlling for characteristics of pension funds and fund states, the main effects remain largely unchanged, especially for the F-score test.

4.7. Effect of the SEC Whistleblower Program

In this section, I test the deterrence effect of whistleblower laws using the SEC's whistleblower provision (2011). Although testing the effectiveness of the federal law is interesting, this test is also useful to ensure the selection issue of pension funds did not drive the observed effects of the FCA. The falsification test using Medicaid-only FCAs in section 4.3 provides evidence that addresses state pension funds' selection issue. However, an unresolved problem remains: heterogeneous fund characteristics. Pension funds located in states with a general FCA might differ from those in states with a Medicaid-only FCA. Knowing their investment can be protected under the general FCA, funds from general FCA states may invest more aggressively by buying stocks with higher F-scores (Peltzman Effect).[25] Note that some of the systematic differences in the F-score between firms owned by general FCA funds versus Medicaid-only FCA funds should be stripped out by firm fixed effects.

[25] 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.

I 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). In column 1 of Table 8, the coefficient estimate on *NoFCA_G*×*SECWB* (-0.073) is statistically significant and has an economically similar effect as 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, although the coefficient estimate (-0.04) is slightly smaller than in the FCA test.

In columns 2 and 4, I estimate falsification tests using a sample split based on firms' prior exposure to Medicaid-only FCAs. 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) becomes smaller than that in column 3, but they are not statistically different (Chi-square test statistic = 0.64). Overall, these results indicate the impact of the SEC's whistleblower provision was more pronounced for firms that were not previously exposed to a general FCA that includes whistleblowing threats for financial fraud, relative to firms exposed.

5. Conclusion

In this paper, I empirically test whether whistleblowing laws are effective in deterring accounting fraud ex ante. Specifically, I examine how much the likelihood of accounting fraud falls when a firm becomes subject to state- and federal-level whistleblowing laws. These laws provide bounties to whistleblowers when their tips lead to successful enforcement actions for

fraudulent firms. If this monetary compensation encourages whistleblowers to come to light, the exposure to these laws would increase the threat of whistleblowing to firms.

For my first identification approach, 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, I find that when firms become exposed to FCAs, the probability of fraud decreases by 7% and audit fees decrease by 5%. Second, I exploit the SEC's Dodd-Frank whistleblower provision of 2011. This federal law affected all US firms at the same time, but its effect is more pronounced for firms without prior exposure to FCAs. I find the economic effects similar to those in the state FCA setting.

I estimate a series of tests to ensure the robustness of my 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, my results are consistent with the notion that the enhanced whistleblowing threats cause managers to tighten internal controls to detect ongoing fraud or reduce managers' incentive to engage in fraud, thereby reducing the probability of fraud and/or the control risk auditors face. This result also suggests internal controls and external audits are substitutes.

This paper contributes to fraud and corporate governance literature by showing whistleblowing being an effective governance mechanism that can reduce the fraud probability. Therefore this paper informs the policy debate over the effectiveness of whistleblowing provisions in preventing fraud. For future research, I will examine the direct mechanism through which managers enhance internal controls to deter fraud when they face a greater threat of whistleblowing.

I will also examine under which conditions whistleblowing laws have stronger deterrence effects, such as when there is poor corporate governance or for more decentralized firms.

Appendix A: Variable Definitions

Variable	Description
Main treatment 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
HIGH	An indicator equal to 1 if the percentage of shares of a firm held by FCA state pension funds is above the median percentage of sample firms with non-zero fund ownership during the year, and zero otherwise
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
Control variables	
OWN	An indicator equal to 1 if at least one state pension fund owns the firm, and zero otherwise
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

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 my 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, I 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:

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

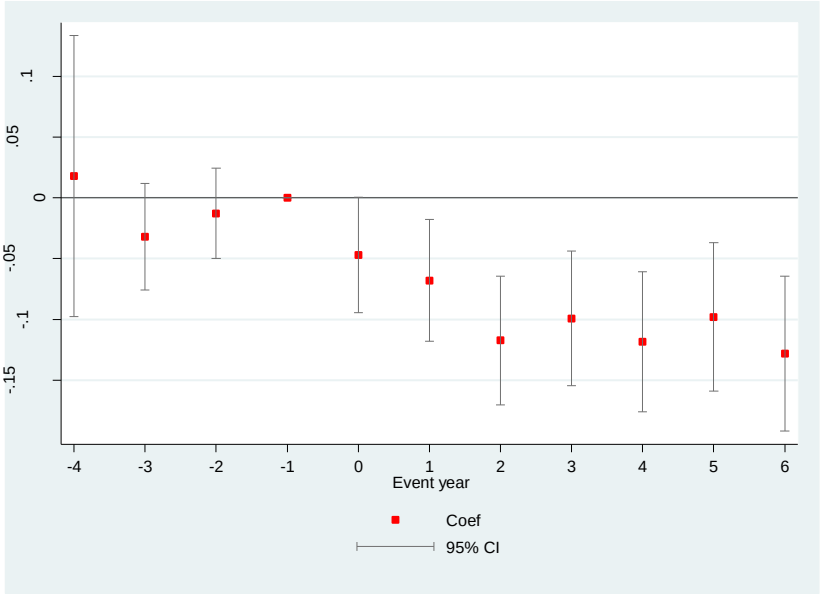
The higher M-score is associated with a higher probability of misstatement.

Appendix D: Figures and Tables

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 my main specification in Table 4 column 3, I 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

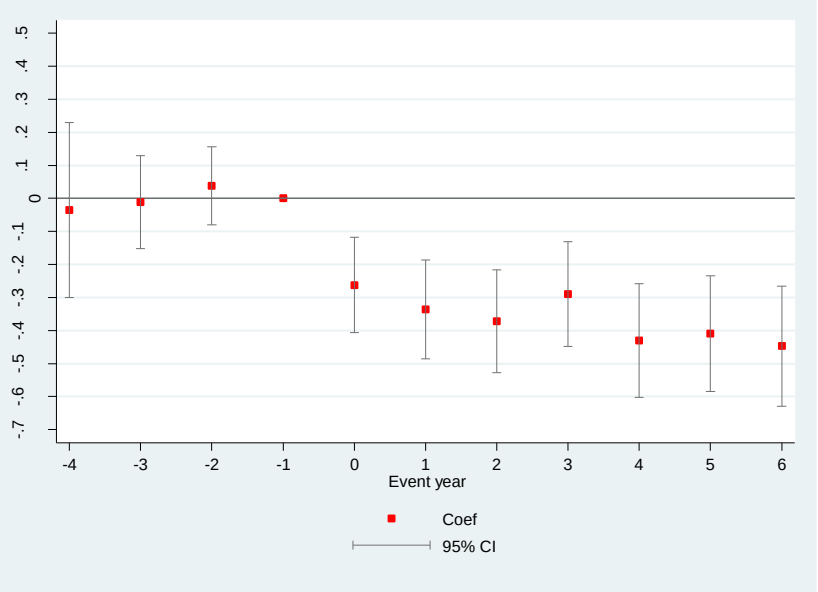


Table 1. Sample Description

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 Winsorize at the bottom and top 1% and drop firm-years with missing observations (leaving firms-years during 2001-2014)	
Total	**23,862 (4,353 firms)**
For state FCA 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 treatment firms are first exposed to general FCAs.

Year	2002	2003	2004	2005	2006	2007	2008	2009	2010	Total
Freq.	155	134	550	192	90	118	93	111	89	1,532
%	10.1	8.75	35.9	12.5	5.87	7.7	6.07	7.25	5.81	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 my 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

Notes: The sample observations are at the firm-year level during 2001-2010. Definitions of variables are provided in Appendix A.

Variable	N	mean	sd	p1	p50	p99
FCA_G	18,543	0.761	0.427	0	1	1
FCA_M	18,543	0.409	0.492	0	0	1
F-score	18,543	0.978	0.561	0.164	0.861	2.820
M-score	11,915	-2.674	0.957	-6.087	-2.641	0.288
Audit fees (in 1000s)	18,543	1,284	1,893	55	629.8	10,200
OWN	18,543	0.789	0.408	0	1	1
Total Assets (in m)	18,543	2,007	5,405	6.122	365.7	30,225
MTB	18,543	2.560	4.442	-6.597	1.914	17.93
InstOwn	18,543	0.551	0.307	0.00256	0.598	1
BIG4	18,543	0.773	0.419	0	1	1
AudTenure	18,543	5.376	2.879	1	5	12
StdRev	18,543	0.228	0.321	0.00517	0.133	1.572
Merger	18,543	0.0731	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
IndGrowth	18,543	1.046	0.0956	0.832	1.063	1.313
Leverage	18,543	0.755	0.544	0.133	0.707	2.127
Loss	18,543	0.304	0.460	0	0	1
FreeCF	18,543	0.0258	0.164	-0.539	0.0423	0.315
NetFin	18,543	0.731	0.443	0	1	1
ROA	18,543	0.00973	0.193	-0.654	0.0383	0.317
ln(Segment)	18,543	1.386	0.640	0	1.386	2.565
InvRec	18,543	0.276	0.185	0.0119	0.249	0.769
LtDebt	18,543	0.503	0.356	0.0719	0.474	1.500
StdROA	18,543	0.0806	0.229	0.00143	0.0353	0.645
Growth	18,543	0.109	0.347	-0.497	0.0682	1.284
ICW	18,543	0.0484	0.215	0	0	1

Table 4. Effect of Exposure to the General 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). Column 5 reports whether the intensity of state pension fund ownership has an additional treatment effect. Ownership is high (*HIGH*=1) if the percentage of shares of a firm held by FCA state pension funds is above the median percentage of sample firms with non-zero fund ownership during the year. 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***	-0.066**
	(-2.464)	(-2.782)	(-2.927)	(-3.538)	(-2.568)
OWN	0.023	0.024	0.015		0.013
	(0.871)	(0.945)	(0.610)		(0.466)
HIGH					-0.013
					(-0.242)
FCA_G×HIGH					-0.012
					(-0.225)
ln(Assets)	0.229***	0.227***	0.200***	0.207***	0.202***
	(14.759)	(13.853)	(12.743)	(11.712)	(12.814)
MTB		0.001	0.001	0.000	0.001
		(1.072)	(0.793)	(0.220)	(0.785)
InstOwn		-0.039	-0.074**	0.073*	-0.066*
		(-1.022)	(-2.026)	(1.813)	(-1.789)
BIG4		-0.044***	-0.038**	-0.011	-0.037**
		(-2.728)	(-2.562)	(-0.588)	(-2.516)
StdRev		0.036	0.031	0.023	0.030
		(1.621)	(1.388)	(0.948)	(1.352)
Merger		0.140***	0.127***	0.124***	0.127***
		(9.120)	(8.508)	(8.069)	(8.517)
Discont		-0.076***	-0.063***	-0.054***	-0.063***
		(-7.113)	(-5.963)	(-4.881)	(-5.989)
Restructure		-0.045***	-0.037***	-0.029***	-0.037***
		(-4.931)	(-4.211)	(-3.185)	(-4.237)
IndGrowth		0.197***	0.201***	0.180***	0.199***
		(5.135)	(5.495)	(4.939)	(5.431)
Leverage		0.032*	0.033**	0.088***	0.033**
		(1.807)	(2.523)	(3.227)	(2.553)
Loss			-0.176***	-0.152***	-0.176***
			(-15.655)	(-12.884)	(-15.688)

FreeCF			-0.633***	-0.771***	-0.633***
			(-9.472)	(-10.233)	(-9.470)
NetFin			0.159***	0.111***	0.158***
			(19.434)	(13.308)	(19.360)
Observations	18,543	18,543	18,543	14,344	18,543
R-squared	0.646	0.652	0.681	0.722	0.682
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Panel B. Effect on individual components of the F-score

Notes: This table reports estimation results when I regress each component of the F-score (Dechow et al. 2011) on exposure to the 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, I 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, I break down the predicted value and regress each component on the exposure to FCA (*FCA_G*). At the bottom of the table, I 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 definition. 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***** (-3.450)	-0.009 (-0.969)	-0.013*** (-4.933)	-0.002 (-0.801)	-0.014** (-2.184)	-0.037 (-1.606)	-0.006 (-0.781)	-0.005 (-0.565)
Observations	18,543	18,543	18,543	18,543	18,543	18,543	18,543	18,543
R-squared	0.725	0.384	0.301	0.288	0.892	0.375	0.197	0.570
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 on the M-score and individual components

Notes: This table reports estimation results when I regress the M-score (Beneish 1999) or its individual components on exposure to the 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, I break down the M-score and regress each component on the exposure to FCA (*FCA_G*). At the bottom of the table, I 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 definition. 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
R-squared	0.409	0.215	0.270	0.213	0.433	0.202	0.279	0.275	0.520
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 the General FCA on Audit Fees

Notes: This table reports the results of regression model (2), which estimates the effect of exposure to the 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). Column 4 reports whether the intensity of state pension fund ownership has an additional treatment effect. Ownership is high (*HIGH*=1) if the percentage of shares of a firm held by FCA state pension funds is above the median percentage of sample firms with non-zero fund ownership during the year. 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**	-0.051**	-0.066***	-0.060**
	(-2.213)	(-2.245)	(-2.628)	(-2.573)
OWN	-0.129***	-0.100***		-0.092***
	(-5.245)	(-4.217)		(-3.734)
HIGH				-0.021
				(-0.370)
FCA_G×HIGH				0.042
				(0.750)
ln(Assets)	0.357***	0.380***	0.341***	0.377***
	(25.650)	(26.633)	(19.979)	(26.332)
Loss		0.071***	0.055***	0.072***
		(6.895)	(4.711)	(6.972)
ROA		-0.069**	-0.064	-0.069**
		(-2.180)	(-1.546)	(-2.179)
InvRec		0.248***	0.164*	0.248***
		(3.386)	(1.900)	(3.384)
Quick		-0.017***	-0.018***	-0.017***
		(-4.761)	(-4.188)	(-4.738)
LtDebt		0.048*	0.138***	0.048*
		(1.925)	(3.373)	(1.925)
StdROA		0.038	0.008	0.038
		(1.277)	(0.424)	(1.277)
Growth		-0.050***	-0.043***	-0.050***
		(-4.575)	(-3.232)	(-4.539)
Merger		0.020*	0.023*	0.020*
		(1.664)	(1.872)	(1.672)
ln(Segment)		0.080***	0.095***	0.080***
		(5.713)	(6.396)	(5.730)
Foreign		0.036**	0.035**	0.036**
		(2.459)	(2.263)	(2.437)
BIG4		0.183***	0.095***	0.182***
		(9.905)	(4.088)	(9.890)

AudTenure		0.013***	0.009***	0.012***
		(4.625)	(3.108)	(4.602)
ICW		0.300***	0.276***	0.300***
		(16.565)	(14.954)	(16.561)
Observations	18,543	18,543	14,344	18,543
R-squared	0.939	0.945	0.944	0.945
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Table 6. Test for Endogenous Effect from Political Economy of a Firm's Headquarter State

Notes: This table reports the results of robustness tests for 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, I replace year fixed effects with firms' headquarter state × year fixed effects. In columns 3 and 6, I exclude firm-years after firms' headquarter states passed general FCAs. In the F-score (audit fees) tests, I 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	(3) Drop after state passed FCA	(4) Table 5 Column 2	(5) Within HQ state	(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***	-0.069***	-0.069***	-0.051**	-0.049**	-0.052**
	(-2.927)	(-2.948)	(-2.967)	(-2.245)	(-2.151)	(-2.186)
Observations	18,543	18,504	17,386	18,543	18,504	17,386
R-squared	0.681	0.690	0.686	0.945	0.946	0.944
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & State×Year	Firm & Year	Firm & Year	Firm & State×Year	Firm & 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, I regress *F-score* and *ln(audit fees)* on exposure to the Medicaid-only FCA (*FCA_M*), respectively. In columns 2 and 5, I drop firms that experienced changes in exposure to the general and Medicaid FCAs at the same time. In columns 3 and 6, I keep firms that have never been affected by general FCAs. In the F-score (audit fees) tests, I 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) Drop if *FCA_G*=0→1	(3) Keep only if *FCA_G*=0	(4) Table 5 Column 2	(5) Drop if *FCA_G*=0→1	(6) Keep only if *FCA_G*=0
Dependent Variable:	F-score	F-score	F-score	ln(audit fees)	ln(audit fees)	ln(audit fees)
FCA_G	-0.069***			-0.051**		
	(-2.927)			(-2.245)		
FCA_M		-0.036	0.175		-0.022	0.029
		(-1.461)	(0.740)		(-0.806)	(0.113)
Observations	18,543	2,333	221	18,543	2,333	221
R-squared	0.681	0.701	0.768	0.945	0.933	0.973
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Table 8. Sensitivity of Treatment Effects to Pension Fund Characteristics

Notes: This table reports the sensitivity of main treatment effects to a characteristic of a state pension fund. In columns 2 and 5, I include indicators for each state fund's ownership along with firm and year fixed effects. In columns 3 and 6, I include indicators for each state from which a state pension fund ownership of a firm is coming. In the F-score (audit fees) tests, I 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**	-0.051**	-0.037	-0.038*
	(-2.927)	(-2.205)	(-2.524)	(-2.245)	(-1.620)	(-1.664)
Observations	18,543	18,543	18,543	18,543	18,543	18,543
R-squared	0.681	0.683	0.683	0.945	0.945	0.945
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year

Table 9. 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 program. For the Medicaid-only FCA tests, I exclude firms that were not affected by 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, I 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
R-squared	0.749	0.764	0.977	0.977
Controls	Yes	Yes	Yes	Yes
Fixed effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year

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