The political influence of voters' interests on SEC enforcement

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ABSTRACT: I examine whether political influence as a response to voters' interest in employment levels is reflected in the enforcement actions of the Securities and Exchange Commission (SEC). I find that large employers are less likely to experience SEC enforcement actions. Next, I examine whether variations in politicians' sensitivity to employment levels result in variations in enforcement against large employers. I find that large employers are less likely to face enforcement actions during presidential elections if they are based in politically important states. Large employers also face fewer enforcement actions if they are based in high-unemployment states during elections of senators who serve on SEC oversight committees. Large employers based in high-unemployment districts enjoy lower enforcement if their congressmen serve on SEC oversight committees. The findings suggest that voters' interests are reflected in SEC enforcement.

Keywords: SEC enforcement actions; firm employment; political influence; voters' interests

JEL classification: D72, G18, M41, M43

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"[…] A decision to charge an entire company may well cause harm to some innocent persons – employees, customers and shareholders. So, the question is whether law enforcement interests are served by charging a company, knowing that the decision may create harm to innocent persons […]."

Robert Khuzami, former Director of SEC's Division of Enforcement, The Deal 2013

"Some have questioned whether it is appropriate for prosecutors to consider the consequences – direct and collateral – when they make a decision whether to indict a company. Of course they should; we want their decision to be thoughtful and in the public interest."

Mary Jo White, former Chair of the SEC, Speech at NYC Bar Association May 19, 2014

1. Introduction

The Securities and Exchange Commission (SEC) has been subject to increased scrutiny following its failure to detect several frauds, such as those executed by Bernard Madoff and Sir Allen Stanford (Henriques 2009; Stewart 2011; Waas 2012). A growing literature in accounting examines the reasons for such failure by investigating the SEC's choice of enforcement targets. These studies indicate that the resource constraints of the SEC, as well as political pressure on the SEC arising from firms' political connections, affect the agency's choice of enforcement targets (Correia 2014; Kedia and Rajgopal 2011; Yu and Yu 2011).

While these studies recognize that the SEC and its enforcement actions are subject to political influence, they do not consider that such influence may also reflect voters' interests—independent of firms' political connections or lobbying. Yet, economists such as Stigler (1971) and Peltzman (1976) have long maintained that politicians influence regulations and regulatory agencies to reflect *both* voters' and special interests to maximize political support. In this paper, I investigate whether voters' interests are reflected in the SEC's choice of enforcement targets.

The SEC enforcement actions can have serious consequences for firms. Prior studies show that sanctions by the SEC in combination with negative market reactions lead to lower firm value, shrinking of operations, and even bankruptcy (Karpoff, Lee, and Martin 2007, 2008a, 2008b). As these outcomes can lead to job losses, SEC enforcement actions not only affect the economic wealth of a firm's shareholders, but also that of a firm's employees.

Politicians are concerned about employment levels, because voters often view the health of the economy as a signal of the incumbent's economic competence (Kinder and Kiewiet 1979, 1981), and job losses can reduce voters' political support (Margalit 2011).

Politicians can influence the SEC by setting the SEC's budget or appointing the commissioners. According to Weingast (1984), these mechanisms involve little direct participation by politicians but instead create incentives for the SEC to consider politicians' interests. For instance, the appointment of commissioners allows the president and Senate to ensure that the SEC acts in line with their political agenda. The commissioners also often have political careers, lending them incentives to act in line with politicians' interests (Alesina and Tabellini 2007).[1] As the SEC has limited resources, it cannot investigate all firms and consequently has to make choices in its enforcement actions (SEC 2013a). Such choices are influenced by the politically appointed commissioners who control the enforcement process, as they set the SEC's enforcement priorities and authorize every enforcement action (GAO 2013; SEC 2013a, 2014a). In their decisions, commissioners and senior SEC staff may consider the harm to employees from an enforcement action. For instance, the opening quotes by Robert Khuzami and Mary Jo White above suggest that commissioners and senior SEC staff appear to consider such potential harm.[2] Speaking more directly to the role of political considerations in these decisions, the former Commissioner Paul Atkins emphasizes that, in line with "the will of Congress," the SEC weighs the costs and benefits of its enforcement actions (SEC 2005).

[1] Christopher Cox was a House Representative before becoming the SEC chairman in 2005. Commissioners Kathleen Casey and Kara Stein were staff directors of the U.S. Senate Banking, Housing and Urban Affairs Committee prior to their SEC appointments (Correia 2014; SEC 2015).

[2] According to Khuzami, Arthur Andersen provides a compelling case as to why an entire institution should not be charged, given that the SEC's and Department of Justice's (DOJ) enforcement efforts drove the firm to bankruptcy and all employees lost their jobs (Orol 2013).

Following these arguments, I propose that the SEC considers the potential harm to employees in its enforcement actions and perceives this harm to be greater, the larger a firm's workforce is. Consequently, I examine whether large employers are less likely to experience an SEC enforcement action.

To test this hypothesis, I use a sample of firms that were sanctioned by the SEC for violating Generally Accepted Accounting Principles (GAAP) as reported in Accounting and Auditing Enforcement Releases (AAERs) for the time period 1982-2012, and all other public firms that did not receive an AAER over this period. I investigate the relation between AAERs and large employers using two different research designs. First, I examine the incidence of AAERs against large employers in two samples of firms that have engaged in accounting irregularities and were thus potential SEC targets: firms subject to securities class-action lawsuits, and restatement firms. Second, I use a regression equation that relates AAERs to firms' number of employees using the large sample of AAER and non-AAER firms. In these tests, I control for other determinants of AAERs found in prior literature (e.g., Brazel, Jones, and Zimbelman 2009; Dechow, Ge, Larson, and Sloan 2011; Mergenthaler 2009) such as firm size, accounting quality, distance to the nearest SEC office, political contributions, and other monitors such as analyst following. Under both approaches, I find evidence that large employers are less likely to experience an AAER. The estimates suggest that an increase by one standard deviation in a firm's number of employees reduces the probability of SEC enforcement by about 22%. These findings are consistent with the hypothesis that political influence as a response to voters' interest in employment levels is reflected in SEC enforcement.

These findings, however, might be driven by unobserved correlated omitted firm characteristics of large employers. To alleviate this concern and to test more directly whether political influence as a response to voters' interests in employment levels is reflected in the

SEC's choice of enforcement targets, I further examine whether the SEC reduces its enforcement actions against large employers more strongly when politicians are more sensitive to employment levels.

Research in political economy suggests that elections are periods during which politicians are likely to take measures to ensure political support (e.g., Brown and Dinc 2005). I argue that the increased sensitivity to voters' interests during presidential elections results in stronger consideration of employment by the SEC, leading to less enforcement against large employers. While I find a lower probability of SEC enforcement during presidential elections years for all firms, I do not find that large employers are incrementally less likely to be subject to SEC enforcement during these periods. As political attention during presidential elections is typically directed towards politically important states (closely contested states with high Electoral College votes) (e.g., Grier, McDonald, and Tollison 1995; Margalit 2011), I refine the analysis and differentiate between more and less important states. I find that the lower probability of SEC enforcement against large employers is more pronounced in presidential election years if these firms are based in politically important states.

Next, I examine SEC enforcement behavior during elections of senators serving on SEC oversight committees, as these senators have greater influence on SEC enforcement (Weingast 1984), and focus on firms that employ a large workforce in the state where they are located.[3] As Wolfers (2002) shows that voters focus on the performance of the state economy relative to the national norm, senators are likely to be especially sensitive to the state's employment levels if the unemployment rate is higher than in other states. I find that

[3] To measure number of employees per state, I obtain data from Garcia and Norli (2012), who allocate firms' operations to their headquarter state based on the number of times the headquarter state name is mentioned in firms' 10-Ks relative to other state names.

the lower likelihood of SEC enforcement against large employers is more pronounced in election years of senators serving on SEC oversight committees if those firms are based in high-unemployment states.

Research in political economy also suggests that congressmen are sensitive to their district's unemployment levels, and take measures to remedy these higher levels. For example, Kiewiet and McCubbins (1985) find that districts with higher unemployment rates receive higher levels of appropriations. As congressmen serving on SEC oversight committees have greater influence on the SEC (Correia 2014; Weingast 1984), I examine whether the lower likelihood of SEC enforcement for firms that employ a large workforce in the district where they are located is more pronounced if the incumbent congressman serves on an SEC oversight committee and the district's unemployment rate is high. I find support for this hypothesis.

Prior studies have examined politicians' influence on the SEC arising from firms' political contributions (Correia 2004; Yu and Yu 2011). Thus, the question arises whether politicians influence the SEC to protect large employers because of those employers' political contributions (rather than the politicians' concerns about employment). While all tests control for firms' PAC contributions to politicians, large employers might also establish political connections via lobbying, or having former politicians or SEC staff as board members (e.g., Goldman, Rocholl, and So 2009; Yu and Yu 2011). To address this possibility, I rerun all tests and control for board members' career background as well as firms' lobbying expenditures. I find support for all hypotheses after including these controls.

Lower SEC enforcement against large employers might also be explained by higher accounting quality. While the tests include F-scores to control for firms' accounting quality, I use all three specifications of the F-score, discretionary accruals, and the incidence of

restatements in additional tests as proxies for accounting quality.[4] I find that large employers have lower accounting quality compared to their peers, suggesting that the reduced likelihood of SEC enforcement against them cannot be explained by higher accounting quality.

I also address three potential weaknesses of my research design. First, the high correlation between the number of employees and total assets (the measure of firm size) raises multicollinearity concerns (especially for the tests of H1).[5] To address this concern, I conduct additional tests such as propensity score matching (PSM). Using PSM, I match firms above the median value of employees with firms below the median that do not exhibit significant differences along observable firm characteristics such as firm asset size. PSM addresses potential multicollinearity, because the matched sample does not include both the number of employees and firm asset size (e.g., Ho, Imai, King, and Stuart 2007; Minutti-Meza 2013). I find support for H1. Second, the measure of firms' employment reflects firms' total employees instead of firms' U.S. employees. To address this, I obtain data on firms' U.S. and non-U.S. employees from Compustat Segments and find that about 90 percent of the sample firms do not have employees outside of the U.S. and that only a firm's U.S. employment is associated with a lower likelihood of SEC enforcement. Third, my approach to determine the number of employees located in the headquarter district based on the relative occurrence of the headquarter state name in firms' 10-Ks may not be adequate for firms with a geographically dispersed workforce. To address this, I follow Agrawal and Matsa (2013) and exclude firms from geographically dispersed industries, namely, retail, wholesale, and transport, and rerun my tests. My inferences hold.

[4] Dechow et al. (2011) developed the F-score by investigating financial, nonfinancial (such as abnormal changes in employment), and market characteristics of firms that have been subject to an AAER. The F-score can be used as a red flag for the likelihood of earnings misstatement.

[5] Across all models, the variance inflation factors are lower than 10 indicating that multicollinearity is not a problem (Wooldridge 2002). I address the potential multicollinearity concern more extensively in Section 5.

This paper makes three primary contributions. First, it contributes to the literature examining the political economy of SEC enforcement, which is an important but understudied area in accounting (e.g., Kothari, Ramanna, and Skinner 2010). In contrast to studies that focus on firms' attempts to affect SEC enforcement via lobbying and political contributions (Correia 2014; Yu and Yu 2011), my findings indicate that, in addition to such lobbying for firms' special interests, politicians' efforts to cater to voters' interests by protecting employment are systematically reflected in SEC enforcement. Thus, my results emphasize the importance of considering political influence as a response to *both* voters' and special interests in explaining the SEC's enforcement choices. This is an important difference, especially in light of a recent study by Heese, Khan, and Ramanna (2017), which finds that firms' political contributions do *not* result in lax SEC oversight, as is suggested by prior literature.

Second, my study contributes to the literature examining links between sources of political influence and regulators' enforcement decisions. For example, prior studies have documented how congressional committee representation affects regulatory enforcement actions in the context of tax audits (e.g., Young, Reksulak, and Shughart 2001), reviews by the Federal Trade Commission (e.g., Faith, Leavens, and Tollison 1982), or SEC enforcement (Correia 2014). My study complements these studies by providing nuanced insights into how powerful politicians, political events, and economic conditions strengthen the SEC's employment considerations in its choice of targets.

Third, my study contributes to the accounting literature examining the determinants of SEC enforcement (e.g., Brazel et al. 2009; Cox, Thomas, and Kiku 2003; Dechow et al. 2011; Files 2012; Mergenthaler 2009; Peterson 2008, 2012). One stream within this literature has shown that firm characteristics such as firm size (Mergenthaler 2009) or cooperation with the SEC (Files 2012) affect enforcement likelihood. Another stream has used SEC enforcement

actions to examine firm characteristics associated with accounting misstatements. Some of these studies use measures that involve the number of employees to identify abnormal operational changes that could predict misstatements (Brazel et al. 2009; Dechow et al. 2011). Empirically, my study builds on both streams by controlling for determinants of SEC enforcement that are related to a firm's number of employees such as firm size and abnormal operational changes. My study complements these papers by highlighting that a firm's number of employees is an important factor considered by the SEC in its enforcement decisions.

2. Hypotheses and institutional background

Protection of large employers

The SEC enforces firms' compliance with accounting regulation through enforcement actions, among other activities (SEC 2013b). Enforcement actions, however, can be costly for firms and stakeholders (e.g., Feroz, Park, and Pastena 1991; Karpoff et al. 2007, 2008a, 2008b; Palmrose, Richardson, and Scholz 2004). For instance, using a sample of 585 SEC enforcement actions, Karpoff et al. (2008b) find large negative abnormal returns around the announcement of SEC investigations and their resolution, even when the misconduct was disclosed earlier (for instance, through a restatement). Karpoff et al. (2007) show that about 34 percent of the firms that are subject to an SEC enforcement action file bankruptcy, and over 70 percent of these firms did so after the market learned about the SEC investigation (but only a minority before the public revelation of the investigation). Consequently, in addition to direct penalties, the SEC's enforcement actions can lead to significant reputational penalties imposed by the market, shrinking of operations, and even bankruptcy. In fact, Karpoff et al. (2008b) estimate that the reputational penalties are over 7.5 times the sum of all penalties imposed through the legal and regulatory system. Firms that survive face a mean dollar loss of $591.75 million from the enforcement action (Karpoff et al. 2008b).

These severe consequences may incentivize firms to attempt to influence SEC enforcement (Correia 2014; Watts and Zimmerman 1978). In fact, a considerable stream of literature building on Capture Theory has shown that firms can establish political connections via lobbying or political contributions, and that such politically connected firms are less likely to be involved in enforcement actions from regulators such as the Internal Revenue Service (Hunter and Nelson 1995), the Environmental Protection Agency (Mixon 1995), and the SEC (Correia 2014; Yu and Yu 2011).[6] While this literature recognizes that politicians influence regulators because of firms' political connections, politicians may also seek to influence the SEC independent of such connections. Economists such as Stigler (1971) and Peltzman (1976), for instance, have long maintained that politicians seek to maximize political support in the form of votes, and therefore ensure that their actions also promote voters' interests. As employment levels can influence voters' support (e.g., Margalit 2011), politicians have long supported large employers via bailouts, tax favors, subsidies, protection from negative newspaper coverage, or through their voting behavior (e.g., Hope, Li, Liu, and Wu 2018; Mian, Sufi, and Trebbi 2010; Peltzman 1984).[7]

SEC behavior under political influence

The SEC is influenced by Congress and the president. Congress decides on the SEC budget and has oversight of the agency, while the president appoints SEC commissioners with the advice and consent of the Senate (SEC 2015). [8] According to the Congressional

[6] Several studies in accounting have also used Public Interest Theory (e.g., Hail, Tahoun, and Wang 2017; Heese, Krishnan, and Moers 2016), Ideology Theory (e.g., Allen and Ramanna 2013), and Bias for Action Theory (e.g., Sunder 1988) to explain regulators' behavior.

[7] Chrysler, for instance, was bailed out by the government to prevent huge job losses and destabilization of the entire manufacturing sector at the end of the 1970s and 2000s.

[8] Several Congressional Committees are involved in setting the SEC budget and overseeing the SEC. The Senate Banking, Housing and Urban Affairs Committee, and the House Financial Services Committee oversee the SEC. The Senate Commerce Committee oversees securities issues, and the House Energy and Commerce Committee oversees accounting issues and the Financial Accounting Standards Board (FASB). The Senate and House Appropriations Committees decide on appropriations.

Dominance Theory, these control mechanisms create incentives for the agency to act in line with politicians' goals and involve little direct participation by politicians (e.g., Miller 2005; Oritani 2010; Weingast and Moran 1983; Weingast 1984).[9] For instance, the appointment of commissioners allows the president and Senate to influence the political views represented in the SEC. The commissioners often also have political careers, lending them incentives to act in accordance with politicians' interests (Alesina and Tabellini 2007; SEC 2013b).

The opening quotes of Robert Khuzami and Mary Jo White suggest that the politically appointed commissioners and senior SEC staff may act in accordance with politicians' incentives to protect large employers. In 2006, the SEC (2006) also issued a statement explaining its considerations in imposing penalties against corporations. In this statement, the SEC points out that "the likelihood a corporate penalty will unfairly injure investors, the corporation, or third parties weighs against its use as a sanction." It also highlights that "the extent of the injury to innocent parties" is a determinant of the propriety of a penalty. While these considerations apply to the determination of penalties, rather than the choice of enforcement targets, it is possible that the harm to innocent third parties such as employees is also a factor in the decision to target a firm for enforcement in the first place. Speaking more directly to the role of political considerations, the former Commissioner Paul Atkins emphasizes that the consequences of an enforcement action can be devastating to the firms involved, not to mention investors, employees, and many others, and consequently—in line with "the will of Congress"—the SEC weighs the costs and benefits of its actions (SEC

[9] As a more direct form of control, Arthur Levitt, the former Chairman of the SEC (2003), describes that politicians can actively intervene with an SEC investigation. He mentions strong pressure from Congress following the proposal of an audit independence rule: "Soon, I was spending almost all my time deflecting a barrage of phone calls, visits and letters from House and Senate Members."

2005). Such protection of large employers could be systematically reflected in the SEC enforcement process, which is shown in Figure 1.[10]

– Figure 1 here –

Given its resource constraints, the SEC must prioritize its enforcement actions (SEC 2013a). Such choices are influenced by the politically appointed commissioners. An enforcement action typically starts with a lead such as a restatement, auditor or management departure, or referrals from the SEC's Division of Corporation Finance, among many other sources (SEC 2013a). In designating a lead a priority, the assigned staff is encouraged to use discretion in balancing and weighing various considerations including the extent of misconduct, the programmatic importance, and the deterrence effect (SEC 2013a).[11] Subsequently, the staff has to decide whether to conduct an informal confidential investigation, called "matter under inquiry" (MUI). Again, staff are encouraged to use judgment in deciding whether it is appropriate to open a MUI (SEC 2013a). After this investigation, the staff provides a recommendation to the Branch Chief, Assistant, Associate and Regional Director as to whether to open a formal confidential investigation or close the MUI.[12] Based on that recommendation, senior officers can issue a Formal Order of Investigation (SEC 2013a). At the end of the investigation, senior officers review the case, after which the staff presents an enforcement recommendation to the Commission (GAO 2009), which votes in a closed meeting whether to approve or reject it (SEC 2013a). If the

[10] Figure 1 is adapted from Karpoff et al. (2008b). A more detailed description of the enforcement process can be found in the SEC enforcement manual (SEC 2013a).
[11] The programmatic importance, for instance, is determined by the enforcement priorities set by the Commission (i.e., the chair and other commissioners) in cooperation with senior staff (SEC 2014a).
[12] Each Office of Associate Director has one or more assistant directors. Branch chiefs report to assistant directors and supervise the work of investigative attorneys (GAO 2009).

Commission decides to file charges the investigation can result in a criminal investigation (conducted by the DOJ) or an administrative as well as civil action (handled by the SEC).[13]

A recent GAO (2013) study concludes that the consideration of case-specific aspects, the hierarchical structure of the SEC, the requirement to obtain higher-level authorization at each step of the enforcement process, and the arduous process of obtaining the Commission's approval for an enforcement action ensure that the SEC staff is likely to conduct investigations in line with the Commission's priorities.

Following these arguments, I propose that the SEC considers the potential harm to employees in its enforcement actions and perceives this harm to be greater for large employers. This argument is summarized in the following hypothesis:

HYPOTHESIS 1. *Large employers are less likely to be subject to SEC enforcement.*

Despite the arguments above, there are at least three reasons why it is not obvious that large employers have a lower likelihood of SEC enforcement relative to other firms. First, politicians may not perceive political influence on the SEC to protect large employers as an effective strategy to avoid job losses and cater to voters' interests. Second, even if politicians perceive this strategy as worthwhile, commissioners and senior SEC staff may not respond to such pressure because the benefits of sparing large employers engaging in accounting violations from enforcement actions may not outweigh the potential costs of being identified as a supporter of these transgressing firms. Considering the harm to employment may also often be in direct conflict with other important considerations such as the extent of misconduct, the deterrence effect, or, more broadly, unbiased enforcement. In addition, commissioners and senior SEC staff may not always want to take actions that benefit politicians currently in office; for example in situations where the incumbent politician does

[13] All proceedings that involve an accountant or an auditor receive a secondary designation by the SEC called an Accounting and Auditing Enforcement Release.

not stand for reelection. Third, it may be difficult for commissioners and senior SEC staff to systematically spare large employers from enforcement actions, because the enforcement process involves different staff across the SEC's hierarchy, many of which may want to pursue firms in violation of accounting standards independent of employment considerations.

Sources of political influence

Below I examine whether the SEC reduces its enforcement actions against large employers more strongly when political influence on the SEC is more pronounced. Specifically, I develop hypotheses based on three sources of political influence on the SEC: 1) presidential elections, 2) elections of senators serving on SEC oversight committees, and 3) districts represented by congressmen serving on SEC oversight committees.

Presidential elections

Research in political economy emphasizes that politicians are especially concerned about employment levels before elections, because voters often view the current health of the economy as a signal of the incumbent's economic competence (Kinder and Kiewiet 1979, 1981). In fact, Holbrook (1991) finds that for every percentage point of unemployment, the incumbent president's party loses 1.2% of the vote. Margalit (2011), who uses U.S. plant-level data and analyzes changes in the president's vote share in the presidential elections in 1996, 2000, and 2004, finds that voters are sensitive to the loss of local jobs. This creates incentives for regulators to protect employment before elections. For instance, Hunter and Nelson (1995) and Young et al. (2001) document lower IRS audit rates in states that are important for the presidential election. Thus, I hypothesize that the increased sensitivity to employment levels during elections results in less enforcement against large employers:

HYPOTHESIS 2a. *Large employers are less likely to be subject to SEC enforcement in presidential election years.*

Such lower enforcement for large employers is not necessarily constant across states. Prior research has shown that during presidential elections, attention is directed towards states rich in electoral votes that are tightly contested (Grier et al. 1995; Young et al. 2001). For instance, Grier et al. (1995) find that the president's decision to veto a bill can be predicted by the votes of senators from electorally important states. Thus, I hypothesize that in presidential election years lower enforcement is more pronounced for large employers based in politically important states (i.e., tightly contested states rich in electoral votes):

HYPOTHESIS 2b. *In presidential election years, large employers are less likely to be subject to SEC enforcement if they are based in politically important states.*

Elections of influential senators

Senators are also likely to be more concerned about the needs of their voters before elections. Regarding employment levels, senators are likely to be especially concerned about firms that are large employers in their state ("large state employers"). However, the ability to influence the SEC is likely to be stronger among senators who serve on SEC oversight committees (Weingast 1984).[14] As a consequence, the SEC has incentives to act in accordance with the interest of these senators even without these politicians actively intervening with the SEC's enforcement decisions. Thus, I hypothesize that large state employers are less likely to be subject to SEC enforcement in election years of senators serving on SEC oversight committees:

HYPOTHESIS 2c. *Large state employers are less likely to be subject to SEC enforcement in election years of senators serving on SEC oversight committees.*

[14] Senators serving on the Senate Appropriations, Banking, and Commerce Committee have the strongest influence on the SEC. Focusing on elections of senators serving on SEC oversight committees allows me to exploit the staggered election intervals of senators, which mitigates concerns about other economic shocks or regulatory changes affecting SEC enforcement. It also allows me to allocate political influence on the SEC to one of the two senators that represent each state. This is important because during my sample period almost all states had at least one of a state's two senators serving on an SEC oversight committee, resulting in a lack of variation in power over the SEC across states.

The importance of employment levels in election years is likely to be dependent on the employment situation in the senators' states. If the senators' state unemployment rate is higher than in other states, employment levels are likely to be more important, because voters evaluate the performance of the state economy relative to the national norm (Wolfers 2002). Therefore, I argue that lower enforcement against large state employers during elections of senators serving on SEC oversight committees is more pronounced if the state unemployment rate is high. Thus, I state the following hypothesis:

HYPOTHESIS 2d. *Large state employers are less likely to be subject to SEC enforcement in election years of senators serving on SEC oversight committees if the state unemployment rate is high.*

Districts represented by influential congressmen

Similar to senators, congressmen serving on SEC oversight committees have the ability to influence the SEC. Weingast (1984) argues that "it is not the Congress as a whole that is relevant for policymaking but rather the committee(s) with jurisdiction over the agency." In contrast, "members not on the relevant committee […] have substantially less power." As described above, members of the House serving on the House Appropriations, Banking and Commerce committee are likely to have the strongest influence on the SEC, providing incentives to the SEC to act in accordance with the interest of these congressmen. Thus, I argue that lower enforcement is more pronounced if the incumbent congressman serves on an SEC oversight committee, and state the following hypothesis:

HYPOTHESIS 2e. *Large district employers are less likely to be subject to SEC enforcement if the incumbent congressman serves on an SEC oversight committee.*

In a similar manner, congressional districts' employment levels affect voters' support for members of the House (e.g., Goodman and Kramer 1975). Unlike the president, members of the House must stand for reelection every two years, increasing the motivation to constantly

act in the interest of their voters.[15] Research in political economy also suggests that congressmen are sensitive to their district's unemployment levels, and could take measures to remedy these higher levels. Kiewiet and McCubbins (1985), for instance, find that appropriations are allocated to districts with high unemployment rates. I hypothesize that firms that are large employers in their congressional district ("large district employers") are less likely to be subject to SEC enforcement if their congressman serves on an SEC oversight committee and the district's unemployment rate is high:

HYPOTHESIS 2f. *Large district employers are less likely to be subject to SEC enforcement if the incumbent congressman serves on an SEC oversight committee and the district's unemployment rate is high.*

3. Data and research method

SEC enforcement data

To investigate SEC enforcement, I focus on a sample of AAERs, which has two main advantages. First, prior studies on the determinants of SEC enforcement typically use AAERs. Thus, using AAERs allows me to build on this literature in my empirical tests by controlling for factors previously identified as determinants of AAERs. Second, the data on AAERs is readily available from the Berkeley Center for Financial Reporting and Management, reducing differences in sample composition across studies. However, one disadvantage is that focusing on AAERs may limit the generalizability of my results to other SEC enforcement actions. Because the SEC has issued AAERs since 1982, my sample spans the time period 1982-2012, and consists of 3,403 AAERs, which refer to 1,838 misstated firm-years against 789 distinct firms.[16] After matching the misstatement events to firms' publicly available data in Compustat and the Center for Research in Security Prices (CRSP)

[15] Indeed, I do not find that large district employers face less enforcement during elections (untabulated).
[16] Multiple AAERs may pertain to a misstatement event as the SEC can take action against multiple officers as well as the firm itself. The number of AAERs ranges from one per firm to a high of 46 per firm (Enron).

and collecting data on missing data items for AAER firms, I have a remaining sample of 1,143 AAER firm-years, representing AAERs against 479 distinct firms (see Table 1, panel A). The misstatement of the average AAER firm in my sample lasts for two fiscal years. Including my sample of control firms, I arrive at an overall sample of 95,145 firm-year observations, representing 12,437 distinct firms.

– Table 1 here –

Methodology

Test of H1

To test whether large employers are less likely to be subject to SEC enforcement, I use two research designs. I examine the likelihood of being subject to an AAER using the following logistic regression model where the subscript *i* represents the firm and *t* the year:

$$AAER_{it} = \beta_0 + \beta_1 Employees_{it} + \sum_n \beta_n Controls_{it} + e_{it} \qquad (1)$$

AAER is equal to one during the misstatement period as reported in the Accounting and Auditing Enforcement Releases (Dechow et al. 2011).[17] I use the misstatement period as the dependent variable for two reasons. First, the negative consequences for firms largely materialize when the market first learns about an SEC investigation, instead of the release of the AAER, which lags behind the start of the enforcement period by on average over 50 months (Karpoff et al. 2008b). Second, although SEC investigations are supposed to be confidential, they can be revealed by the media, other stakeholders, or even the firm itself. Karpoff et al. (2008a, 2008b), for example, find public disclosures of SEC investigations for more than 50 percent of their fraud firms, occurring on average half a year after the violation ends. Thus, if the SEC aims to protect large employers, it is likely to initiate fewer

[17] If the misstatement period spans partial fiscal years, both fiscal years are set equal to one.

investigations against large employers. To provide insights on the typical start of SEC investigations for my sample, I collect data on firm and media reports about SEC investigations (see Appendix B). The descriptives suggest that investigations are typically initiated during the misstatement period (which lasts for two fiscal years for the average AAER firm). Consequently, I use the misstatement period as dependent variable.[18]

To identify large employers, I use the natural logarithm of the number of employees, denoted *Employees*. First, I examine the incidence of AAERs in a sample of firms subject to securities class-action lawsuits as well as restatements. Such firms potentially engaged in accounting irregularities and were thus potential SEC targets. I obtain a sample of 2,225 class-action lawsuits from the Stanford Law Database on Shareholder Lawsuits. After eliminating cases with missing data, dismissed cases, and financial industries, the sample is reduced to 1,088 class actions for the time period 1996 (first year of data availability) to 2008. I obtain restatements from the GAO and Glass-Lewis database. After eliminating cases with missing data, and financial industries, the sample is reduced to 1,952 restatements for the time period 1996 (first year of data availability) to 2007. As lawsuits and restatements are usually filed after the trigger event (Karpoff et al. 2008b), I include up to five years (if available) for all firms (i.e., AAER and non-AAER firms) prior to the filing of the lawsuit or restatement to capture the violation period. My final sample includes 261 (465) AAER firm-year observations for the lawsuit (restatement) sample. I run equation (1) on these two samples. H1 predicts a negative coefficient on β_1.

[18] It is possible that this research design introduces noise, as it assumes that the SEC starts its investigations during the misstatement period. For example, this research design is less likely to appropriately identify the start of the SEC investigation for firms that have misstated their financials over several years. As a robustness test (untabulated), I test all hypotheses using AAER firm-year observations from the last violation year and the year preceding as well as following the last violation year as dependent variable. The results for H1 and H2 are robust to this alternative research design. I also find evidence in support of H1, although with a smaller magnitude, using the AAER-release year as dependent variable (untabulated).

Second, I run equation (1) on the sample of firms that received an AAER and all other public firms that did not receive an AAER. Including my sample of control firms, I arrive at an overall sample of 95,145 firm-year observations. H1 predicts a negative coefficient on β_1.

I include several control variables. When running equation (1) on the subset of firms subject to a class-action lawsuit, I include the following variables to control for lawsuit characteristics. I obtain data for these variables from the Stanford Law Database on Shareholder Lawsuits. First, I include an indicator equal to one if the lawsuit involved a *GAAP Violation*, zero otherwise. Second, I include an indicator equal to one if the lawsuit was triggered by a *Restatement*, zero otherwise. Third, I include the natural logarithm of the number of days between the beginning of the lawsuit and the end of the lawsuit, denoted *Lawsuit Length*. Finally, I include the natural logarithm of the lawsuit settlement amount (set to zero if the lawsuit did not result in a settlement), denoted *Settlement Amount*.

When running equation (1) on the subset of firms subject to a restatement, I follow Files (2012) and include the following variables to control for the nature of the restatement. I obtain data for these variables from the GAO, Glass-Lewis, and Audit Analytics restatement databases. First, I include an indicator equal to one if the restatement involved a *GAAP Violation*, zero otherwise. Second, I include an indicator equal to one if a *Class-Action Lawsuit* was filed in response to the restatement, zero otherwise. Third, I include the natural logarithm of the number of days between the beginning of the misstated period and the end of the misstated period, denoted *Misstatement Length*. *Restate Magnitude* is the cumulative earnings effect of the restatement as a percent of total assets. *Damages* is the cumulative effect in stockholders' equity of the restatement as a percent of total assets. *Revenue* is equal to one if any portion of the restatement is due to revenue recognition problems, zero otherwise. *Lease* is equal to one if any portion of the restatement is due to the accounting for leases, zero otherwise. Lease restatements peaked in 2005 after the SEC clarified the

treatment of certain lease and leasehold improvements (Files 2012). *Rule Change* is equal to one if the restatement refers to an accounting rule (e.g., FAS 133, SAB 101), zero otherwise. *Number of Issues* is the natural logarithm of the number of different accounting issues per restatement. Files (2012) argues that *GAAP Violation*, *Misstatement Length*, *Damages*, *Revenue*, and *Number of Issues* could increase the likelihood that the SEC targets the firm for enforcement, while *Restate Magnitude*, *Lease*, and *Rule Change* decrease SEC enforcement likelihood (she does not make a prediction for *Class-Action Lawsuit*).

When using the lawsuit and restatement samples, I also control for the possibility that firms with more employees have more complex financial statements, triggering enforcement actions (Peterson 2008, 2012). I control for accounting complexity by including the natural logarithm of the number of *Words* in a firm's 10-K and the *Fog Index* of a firm's 10-K.[19]

Throughout all tests, I also include several additional control variables. First, to control for firms' reporting behavior, I use the F-score developed by Dechow et al. (2011) to predict AAERs.[20] The higher the F-score, the higher is the likelihood of earnings misstatement. As the SEC is more likely to investigate firms located closer to its offices (Kedia and Rajgopal 2011), I control for the distance between the county of firms' headquarters and SEC offices using *Proximate 100*, which is equal to one for firms located within 100 km of an SEC office.[21] As Correia (2014) and Yu and Yu (2011) find that politically connected firms enjoy

[19] I obtain data on *Words* and *Fog Index* from the WRDS SEC Analytics Suite. As the data is only available as of 1994, I do not include these variables when using the large sample of firms for the period 1982-2012. I find that the inferences hold when including these variables and restricting the sample to 1994-2012 (untabulated).

[20] The different F-scores include (1) only financial statement variables (*F-Score 1*), (2) financial statement, off-balance sheet, and nonfinancial variables (*F-Score 2*), and (3) financial statement, off-balance sheet, nonfinancial, and stock market-based variables (*F-Score 3*). I report tests using *F-Score 1*. The results are robust to using *F-Score 2*, *F-Score 3*, the components of each F-score, or discretionary accruals (untabulated).

[21] I use the Haversine formula to calculate the distance between counties and SEC offices, using the latitude and longitude of counties and SEC offices obtained from the U.S. Census Bureau Gazetteer. In 2007 the SEC elevated its district offices in Boston, Philadelphia, Atlanta, Fort Worth, Salt Lake City, and San Francisco to regional offices. Following Kedia and Rajgopal (2011), I only consider a firm's distance from the original regional offices located in Washington, New York, Miami, Chicago, Denver, and Los Angeles. Considering the additional regional offices does not affect my inferences.

favorable treatment by the SEC, I obtain data on firms' Political Action Committee (PAC) contributions from the Federal Election Commission's (FEC) website to control for firms' political connections.[22] I match PAC contributions to my sample of Compustat firms and scale firms' PAC contributions by average assets, denoted *PAC Contribution*.[23]

I also obtain data on the percentage of employees who are union members per four-digit SIC code, denoted *Union*, from the Union Membership and Coverage Database maintained by Hirsch and Macpherson (2003) to control for pressure from unions. I also include the natural logarithm of the number of analysts issuing earnings forecasts, denoted *Analyst Following*, and a *Fortune 500* indicator, equal to one if the firm is in the Fortune 500 index as reported in Compustat, to control for firm visibility (Correia 2014). I include firms' two-digit SIC code median-adjusted return on assets, denoted *ROA*, to control for firm performance. Next, I include a *Big 4* indicator, as clients of the Big 4 might be less likely to commit fraud (DeFond 1992). I use the *Market-to-Book* ratio to control for firms' growth expectations, as Dechow, Sloan, and Sweeney (1996) find that firms with higher growth opportunities are more likely to manipulate earnings. *Leverage* and *Z-score* are included to control for firms' financial distress (Cox et al. 2003). I also include *Abnormal Change in Revenue*, because Brazel et al. (2009) find that *Abnormal Change in Revenue*, defined as the difference between the change in revenues and the change in the number of employees, predicts AAERs. The intuition for this measure is that changes in financial measures (revenues) and non-financial measures (employees) should be consistent: if the difference between financial and non-financial performance becomes greater, misstatement risk increases. The correlation between

[22] Firms cannot contribute directly to PACs. Instead, PACs solicit contributions from executives, employees, and shareholders. The decision to distribute PAC contributions typically belongs to firms' executives (Correia 2014). PAC contributions are widely used as a proxy for political connections (see Milyo, Primo, and Groseclose 2000, for an overview), and the data is available for my whole sample period.

[23] The results are robust to using firms' PAC contributions instead of PAC contributions scaled by average assets (untabulated).

that measure and the number of employees is low at –0.009. The natural logarithm of *Firm Age* is included because fraud firms tend to be younger (Beneish 1997). Mergenthaler (2009) finds that firm size is positively associated with SEC enforcement likelihood. To control for size, I include the natural logarithm of firms' total assets, *Assets*. As controlling for assets holds firm size constant, it distinguishes labor-intensive from capital-intensive firms. I include industry and year fixed effects. Finally, I include fixed effects for the tenure of SEC Chairmen and U.S. Presidents to control for tenure-specific policy priorities. Standard errors are clustered by firm. Appendix A provides an overview of my variables.

Test of H2

To test hypothesis 2, I adjust equation (1) as described below and run it on the large sample of AAER and non-AAER firms (i.e., 95,145 firm-year observations). Due to data constraints explained in more detail below, the tests for hypotheses 2c-2f are limited to the period 1994-2012, reducing the sample to 41,357 firm-year observations.

To test hypothesis 2a, that large employers are less likely to be subject to SEC enforcement in presidential election years, I adjust the regression model for testing H1 by including a *Presidential Election* indicator equal to one in all presidential election years during the period 1982-2012 for my large sample of AAER and non-AAER firms. I interact this variable with *Employees*. H2a predicts a negative coefficient on the interaction of *Employees* and *Presidential Election*. To test hypothesis 2b, that in election years large employers are less likely to be subject to SEC enforcement if they are located in politically important states, I adjust the regression model for testing H2a by including *Important State* and interaction terms with large employers and the election year indicator. I follow Cebula, Duquette, and Mixon (2013) and express the political importance of a state by dividing the number of Electoral College votes at stake by the margin of victory for the winning candidate. The states are ranked in descending order for each presidential election (see

Appendix C). *Important State* is equal to one for the top ten most important states per election.[24] H2b predicts a negative coefficient on the interaction between *Presidential Election*, *Employees*, and *Important State*.

To test hypothesis 2c, that large state employers are less likely to be subject to SEC enforcement in election years of their state senator who is serving on an SEC oversight committee, I adjust the regression model for testing H1 by replacing *Employees* with *State Employees*, as these hypotheses focus specifically on state employment. *State Employees* is a firm's workforce per state, which I calculate by using data from Garcia and Norli (2012) on the relative occurrence of the headquarter state name relative to all other state names in firms' 10-Ks from 1994 onwards, limiting the tests for hypotheses 2c-2f to the period 1994-2012. I multiply a firm's total number of employees by this relative occurrence of the headquarter state name.[25] I also include *Senate Election*, which is equal to one for all senate election years of senators serving on SEC oversight committees. Finally, I include a *Congressional Election* indicator to control for congressional elections that overlap with senate elections. H2c predicts a negative coefficient on the interaction of *State Employees* and *Senate Election*. To test hypothesis 2d, that large state employers are less likely to be subject to SEC enforcement during elections of their senator serving on an SEC oversight committee if the state unemployment rate is high, I include *High Unemployment State*, which is equal to one for states with an unemployment rate larger than the average unemployment rate across states per year, as well as interactions with *State Employees* and *Senate Election*. H2d predicts a

[24] According to that metric Florida is in the top ten of the 2008 list. Obama-Biden won Florida's 27 Electoral College votes by a margin of 236,450 popular votes. California was near the bottom of the 2008 list, as its 55 Electoral College votes went to Obama-Biden by a margin of 3.3 million popular votes (Cebula et al. 2013).

[25] Garcia and Norli (2012) count the occurrence of state names in sections "Item 1: Business", "Item 2: Properties", "Item 6: Consolidated Financial Data", and "Item 7: Management's Discussion and Analysis" of firms' 10-Ks as of 1994 (when EDGAR filing started). The average firm mentions eight states in its 10-K, of which the headquarter state is referred to most often (i.e., 45 percent of all state occurrences).

negative coefficient on the interaction between *Senate Election*, *State Employees*, and high-unemployment states.

To test hypothesis 2e, that large district employers based in districts with a Congressman serving on an SEC oversight committee are less likely to be subject to SEC enforcement, I use the same firm employment measures as for H2c and H2d. However, in contrast to the tests for H2c and H2d, each firm is matched to the congressional district where its headquarters are located, allowing me to identify large district employers, denoted *District Employees*. In addition, I include *House SEC Committee*, which is equal to one if a firm's headquarters are located in a congressional district with a congressman serving on the House Appropriations, Banking or Commerce committee, and zero otherwise. To construct this variable I obtain data from Charles Stewart's Congressional Data webpage for all members of the House. H2e predicts a negative coefficient on the interaction of *District Employees* and *House SEC Committee*. To test hypothesis 2f, that large district employers headquartered in districts with high unemployment rates and congressmen serving on an SEC committee are less likely to be subject to SEC enforcement, I adjust the regression model for testing H2e by including a *High Unemployment District* indicator, and interactions with *District Employees* and *House SEC Committee. High Unemployment District*, which is equal to one for all districts with an unemployment rate larger than the average unemployment rate per year and state. H2f predicts a negative coefficient on the interaction between *House SEC Committee*, *District Employees*, and *High Unemployment District*.[26]

[26] http://web.mit.edu/17.251/www/data_page.html. In untabulated tests, I exclude all firms located in states that only have one congressional district (Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, and Wyoming) to rule out that the results are driven by state-level effects. The inferences are unaffected by excluding firm-year observations from these states.

Descriptive statistics

Table 2 reports descriptive statistics. As reported in Table 2, panel A, the mean (median) firm in my sample has 6,345 (763) employees of whom on average 11.1% are union members. The majority of firms are audited by a Big 4 auditor (81.4%) and 29.1% of the firms are located within 100 km of an SEC office. Firms that contribute to PACs spend on average $14,351 per year, firms that engage in lobbying spend on average $57,466, and 7.8% (0.6%) of the firms have a former politician (SEC staff) on their board.

Table 2, panel B shows the differences between AAER and non-AAER firms. AAER firms have a higher F-score, are located closer to an SEC office, are more profitable, are more likely to be audited by a Big 4 auditor, have more analysts following them, and are both younger and larger. This is consistent with the perception that the SEC is more likely to target large firms (Mergenthaler 2009) and firms located closer to its offices (Kedia and Rajgopal 2011). I also find that AAER firms contribute larger amounts to PACs, and have former politicians or SEC staff serving on their boards. AAER firms employ more people than the average non-AAER firm because AAER firms are much bigger in asset size, emphasizing the importance to control for the effect of firm size in the multivariate tests.

– Table 2 here –

4. Results

Test of H1

Table 3, panel A shows the results of estimating equation (1) in the sample of firms subject to class-action lawsuits and restatements. Consistent with H1, the negative and significant coefficient on *Employees* at $p<0.001$ in both models indicates that large employers

are less likely to receive an AAER.[27] In the subset of firms subject to lawsuits (Table 3, panel A, column 1), I find that *Restatement* and *GAAP Violation* increase the likelihood of SEC enforcement. In the subset of firms subject to restatements (Table 3, panel A, column 2), I find, similar to Files (2012), that *Class-Action Lawsuit* and *GAAP Violation* increase the likelihood of SEC enforcement, while *Lease* decreases it.

Table 3, panel B shows the results of estimating equation (1) in the large sample of AAER and non-AAER firms. Consistent with H1, the negative and significant coefficient on *Employees* at $p<0.001$ across all models indicates that large employers are less likely to receive an AAER, in the full sample (column 1), with additional controls for political connections (column 2), and excluding firms that made PAC contributions (column 3). These results are also economically significant: a one-standard-deviation increase in a firm's number of employees reduces the probability of SEC enforcement by about 22%.[28]

The control variables are in line with prior research. In particular, the positive and significant coefficient on the F-score indicates that firms with a higher F-score are more likely to receive an AAER (Dechow et al. 2011). Consistent with Kedia and Rajgopal (2011), I find that firms located closer to an SEC office are more likely to receive an AAER. I also find a negative and significant coefficient on *BIG 4*, suggesting that Big 4 clients are less likely to commit fraud (Brazel et al. 2009). Finally, larger (as measured by log assets), younger, and growth firms as well as firms with more analysts following them are more likely to be subject to an AAER.

– Table 3 here –

[27] The results are robust to using the lawsuit and violation period, respectively, instead of up to five years prior to the lawsuit filing and restatement, respectively (untabulated).

[28] To estimate the economic significance, I use the marginal effect associated with *Employees* from Table 3, panel B, column 1, which is –0.003 percentage points. The unconditional probability of SEC enforcement is 1.2%.

Test of H2

Column 1 in Table 4, panel A presents the results of the effect of employee size on the probability of being subject to SEC enforcement in presidential election years to test H2a. The results show a negative and significant coefficient on *Employees* and the election year indicator (both at $p<0.001$), suggesting that large employers generally enjoy a favorable treatment by the SEC, not just in election years, and that SEC enforcement is reduced during election years. However, I do not find that large employers are less likely to be subject to SEC enforcement in presidential election years, as the insignificant coefficient on the interaction term between election years and *Employees* indicates. While these results suggest a reduction in enforcement in election years, which is consistent with politicians' incentives to protect employees, they do not provide evidence that large employers are incrementally less likely to be subject to enforcement in election years. Column 2 of panel A presents the results of the effect of employee size on the probability of enforcement in presidential election years if a firm is based in a politically important state to test H2b. The results show a negative and significant coefficient on *Employees*, *Presidential Election*, and the interaction term between *Presidential Election*, *Important State*, and *Employees*. In line with H2b, I find evidence that the lower likelihood of enforcement against large employers during elections is stronger for firms based in politically important states, suggesting that the political influence on the SEC is more pronounced in electoral-vote rich states that are tightly contested.[29]

Column 2 of panel B presents the results of the effect of employee size on the probability of enforcement for large state employers in elections of senators serving on SEC oversight committees to test H2c.[30] The results show a negative and significant coefficient on *State*

[29] The results of H2a and H2b are robust to controlling for elections in which the incumbent did not stand for reelection (untabulated).
[30] For completeness, column 1 of Table 4, panel B presents the results for H1 using *State Employees*.

Employees, and the interaction with senate election year at p<0.10. Thus, the results provide some support for H2c, suggesting that large state employers are less likely to be subject to SEC enforcement during elections of senators serving on SEC oversight committees. Column 3 of panel B presents the results of estimating the probability of SEC enforcement for large state employers in election years of senators serving on SEC oversight committees if the state unemployment rate is high to test H2d. The results show a negative and significant coefficient on *State Employees* and the interaction term between *State Employees*, *Senate Election*, and *High Unemployment State*. Thus, these results support H2d and suggest that the SEC acts in line with senators' preference to protect employment during elections.[31]

Column 1 of panel C presents the results of estimating the probability of enforcement for large district employers based in districts with congressmen serving on SEC oversight committees, to test H2e. The results show a negative and significant coefficient on *District Employees*, but not on the interaction term between *District Employees* and *House SEC Committee*. Thus, I do not find support for H2e. Column 2 of panel C presents the results of estimating the probability of being subject to enforcement for large district employers based in high-unemployment districts and congressmen serving on SEC oversight committees to test H2f. The results show a negative and significant coefficient on *District Employees* and the interaction term between *District Employees*, *High Unemployment District*, and *House SEC Committee*. The results support H2f and suggest that the SEC acts in line with congressmen's preference for employment.

The results for the control variables remain largely unchanged from the results reported in Table 3, panel B and are thus not reported in Table 4 and not discussed in detail. A concern is that the tests of H2b, H2d, and H2f involve three-way interactions in nonlinear models

[31] The results of H2c and H2d are robust to controlling for elections in which the incumbent did not stand for reelection (untabulated).

(Wooldridge 2002). I rerun all models using a linear probability model (Wooldridge 2002), and find consistent results (untabulated). I also report the range of variance inflation factors (VIF) for the independent variables. The VIFs are lower than 10 indicating that multicollinearity is not a problem (Wooldridge 2002).

– Table 4 here –

5. Additional tests

Accounting quality of large employers

Large employers may face less enforcement because they have higher accounting quality. Therefore, in addition to controlling for accounting quality in my tests, I run several tests using all three specifications of the F-score (Dechow et al. 2011), discretionary accruals, and the incidence of restatements as proxies for accounting quality. Consistent with prior studies, I control for firm performance, Big 4 auditor, firms' growth expectations, leverage, firm age, and firm size (e.g., Beneish 1997; Dechow et al. 1996). As shown in Table 5, the coefficient on *Employees* is positive and significant in all models (except when using restatements as a proxy for accounting quality). While each of these measures has its drawbacks (Dechow et al. 2011; Price, Sharp, and Wood 2011), these findings suggest that large employers do not have higher accounting quality.

– Table 5 here –

Large employers' political connections

Even though I control for firms' political contributions in all tests, large employers might influence politicians using strategies that are not captured in my tests. Large employers might have former politicians or SEC staff as board members, or use lobbying to influence politicians (e.g., Goldman et al. 2009; Yu and Yu 2011). To address this concern, I rerun my tests and control for board members' career background as well as firms' lobbying expenditures. I find support for hypothesis 1 after including these additional controls (see

Table 3, panel B, column 2).[32] As firms cannot contribute directly to PACs but solicit contributions from executives, employees, and shareholders instead, larger employers might have larger PAC contributions and therefore enjoy lower enforcement. As I control for PAC contributions in all tests, I control for this possibility. In addition, I rerun the tests of H1 and exclude all firms that ever contributed to PACs. As reported in Table 3, panel B, column 3, the inferences hold for this smaller sample of firms.

Firm size

The results presented in Table 3 could be subject to multicollinearity concerns, as the correlation between *Employees* and *Assets* is 0.71, which is relatively high but below 0.80, the point beyond which multicollinearity becomes a concern (Christie, Kennelley, King, and Schaefer 1984; Wooldridge 2002).[33] To better address potential multicollinearity, I report the range of VIFs for the independent variables. Throughout all models, the VIFs are lower than 10, indicating that multicollinearity is not a problem (Wooldridge 2002). Having said that, I conduct two additional tests to address the high correlation between *Employees* and *Assets*.

First, I test H1 using propensity score matching (PSM), which is an effective strategy to address potential multicollinearity, because the matched sample does not include both *Employees* and *Assets* as independent variables (e.g., Ho et al. 2007; Minutti-Meza 2013). Specifically, I create a *Large Employer* indicator equal to one for firms larger than the median value of *Employees*, because PSM requires to create a treatment group based on *Employees*.[34] I form matched pairs by matching firms above the median value of *Employees* with firms below the median that do not exhibit significant differences along observable firm

[32] I also find that the inferences for H2 hold after including these additional controls (untabulated).
[33] Note that the market value of equity (an alternative measure for firm size) is more highly correlated with number of employees (i.e., 0.82). The tests of H2, which use *State* or *District Employees* are less likely to be subject to multicollinearity, as the correlation between those measure and assets is 0.63 and 0.58, respectively.
[34] I also conduct PSM creating treatment and control groups using *State Employees* and *State Employees* divided by a state's total workforce instead of *Employees*. I find results consistent with those presented in Table 6 when using these measures of *locally* large employers (untabulated).

characteristics such as firm asset size (Dehejia and Wahba 2002; Rosenbaum 2002) using the following probit regression model, where the subscript *i* represents the firm and *t* the year:

$$Large\ Employer_{it} = \beta_0 + \sum_{n} \beta_n \text{Controls}_{it} + e_{it} \qquad (2)$$

Controls includes the same variables as in Table 3, panel B, column 1. Because firms' number of employees is determined by factors such as industry, labor unions, and state policies (e.g., Greenstone 2002; Holmes 1998), I also include state fixed effects to control for state-specific laws (e.g., labor-specific laws, environmental laws, tax rules, etc.) that could affect firms' hiring and employee-location decisions. Table 6, panel A shows the results of the Probit propensity-score model (see column 1). I form matched pairs within a propensity score radius (or "caliper") of 0.00005.[35] After matching firm observations based on propensity scores from that model, I have a matched sample of 13,492 firm-year observations, including 167 AAER firm-year observations (see columns 2 and 3). Next, I examine the covariate balance between the treatment and control samples to ensure that my matched pairs are similar across all observable firm characteristics. As reported in Table 6, panel A, column 4, the mean comparisons of matched pairs indicate that the matching procedure successfully identifies pairs that do not exhibit any significant differences in the variables used in the matching procedure. As shown in Table 6, panel B, while large employers have a 0.84% probability of receiving an AAER, matched small employers have a probability of 1.55%, which is a statistically and economically significant difference. Thus, the propensity score matching provides additional evidence for H1.[36]

[35] The smaller the radius, the more likely it is to form pairs that do not exhibit significant differences on the observables (Dehejia and Wahba 2002). Nearest neighbor matching is less appropriate as the nearest small employer often differs significantly from a large employer. The matching is done without replacement. The results are robust to matching with replacement (untabulated).

[36] Nevertheless, these results might be susceptible to hidden bias caused by the omission of unobservable yet relevant variables. To quantify the potential impact of unobserved covariates, I relax the assumption that matched observations have the same probability of being a large employer (Rosenbaum 2002). I find that the

Second, I test H1 using a two-step approach (untabulated). In the first step, I regress *Employees* on the variables included in Table 3, panel B, column 1 such as *Assets*, and obtain the residuals from this model. In the second step, I estimate equation (1) and replace *Employees* with the residuals from the first step. As the correlation between the residuals and *Assets* is zero, these models are not affected by the high correlation between *Employees* and *Assets*. I find results consistent with those tabulated in Table 3, panel B, column 1.

An additional concern is that *Assets* does not sufficiently control for firm size. To examine this possibility, I conduct a test in which I add the natural logarithm of the market value of equity as a control for firm size (untabulated). I find that the coefficient on market value of equity is not statistically significant, suggesting that *Assets* sufficiently controls for firm size.

– Table 6 here –

U.S. versus non-U.S. employment

A concern with *Employees* is that it reflects firms' total employees instead of U.S. employees. To address this concern, I obtain data on firms' U.S. and non-U.S. number of employees from Compustat Segments. As reporting the number of employees per geographical region is not required under SFAS 131 and has been voluntarily reported only as of 1999, the information is available for only a small subset of my sample (i.e., 5,018 firm-years representing 1,217 firms). The descriptive statistics (untabulated) indicate that about 90 percent of the sample firms do not have employees outside of the U.S., suggesting that firms' total number of employees generally reflects the U.S. number of employees. I rerun equation

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results would still be significant if small employers were up to 1.50 times more likely to be a small employer. As no benchmark exists to determine whether a given hidden bias is large (Armstrong et al. 2010), this finding only provides an insight into robustness with respect to hidden bias. To more directly exploit the variation in *Employees*, I also test H1 by running equation (1) on the matched sample. The coefficient on *Employees* is negative and significant at $p<0.01$ (untabulated).

(1) but replace *Employees* with its U.S. and non-U.S. number of employees. As shown in Table 7, the coefficient on U.S. number of employees, but not on non-U.S. number of employees, is negative and significant, which is line with politicians' interests to protect U.S. employment. As firms that voluntarily report their number of employees across geographic areas may not be representative, these results should be interpreted with caution.

– Table 7 here –

6. Conclusions

Enforcement actions by the Securities and Exchange Commission are costly for firms, potentially creating harm to employees. I examine whether political influence as a response to voters' interest in employment levels is reflected in the SEC enforcement actions. My results indicate that large employers are less likely to be subject to an enforcement action. I further examine whether the SEC reduces its enforcement actions against large employers more strongly when political influence on the SEC is more pronounced. I find that the lower likelihood of SEC enforcement against large employers is stronger in presidential election years if firms are based in politically important states. Further, I find that large state employers are less likely to be subject to SEC enforcement during election years of senators who serve on SEC oversight committees if those employers are based in high-unemployment states. I also find that large district employers located in districts with high unemployment rates are less likely to be subject to SEC enforcement if the incumbent congressman serves on an SEC oversight committee. Overall, my results suggest that voters' interests are reflected in SEC enforcement, and thus emphasize the importance of considering political influence as a response to *both* voters' and special interests in explaining the SEC's enforcement choices.

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Appendix A

Overview of variables

Variable	Definition
Dependent Variables	
AAER	1 in the years a misstatement occurred as reported in the Accounting and Auditing Enforcement Releases, zero otherwise.
Variables of Interest	
Employees	Natural logarithm of a firm's total employees (i.e., Compustat item: EMP).
State Employees	Natural logarithm of a firm's employees per state, which I measure using data from Garcia and Norli (2012) on the occurrence of the headquarter state name relative to all other state names in firms' 10-Ks from 1994 onwards, and multiply a firm's total number of employees by this relative occurrence of the headquarter state name. I count the number of times firms' headquarter state name occurs in sections "Item 1: Business", "Item 2: Properties", "Item 6: Consolidated Financial Data", and "Item 7: Management's Discussion and Analysis" of firms' 10-Ks.
District Employees	Defined as *State Employees*, but each firm is matched to the congressional district where its headquarters are based.
Large Employer	1 if a firm's number of employees is larger than the median value of *Employees*, zero otherwise.
(Non-)U.S. Employees	Natural logarithm of a firm's (non-)U.S. employees (Compustat Segments item: EMPS).
Presidential Election	1 in presidential election years (i.e., 1984, 1988, 1992, 1996, 2000, 2004, 2008 and 2012), zero otherwise.
Senate Election	1 in senate election years of senators serving on SEC oversight committees (i.e., the Senate Banking, Housing and Urban Affairs Committee, the Senate Commerce Committee, or the Senate Appropriations Committee), zero otherwise.
Important State	1 for the top ten most important states for each U.S. presidential election, zero otherwise. The top ten most important states are defined by following the methodology of Cebula et al. (2013) who express the political importance of a state by dividing the number of Electoral College votes at stake by the margin of victory for the winning candidate. The states are then ranked in descending order for each U.S. presidential election (see Appendix C).
House SEC Committee	1 if a firm's headquarters are located in a congressional district of a member of the House who serves on the House Appropriations, Banking or Commerce committee, zero otherwise. To construct this variable, I obtain data from Charles Stewart's Congressional Data webpage for all members of the House for the period 1982-2012 and every firm is matched to a specific district based on its zip code as reported in Compustat.
High Unemployment State	1 if a state's unemployment rate is larger than the average unemployment rate across all states per year, zero otherwise. I obtain data on states' unemployment rates from the U.S. Census Bureau.
High Unemployment District	1 if a district's unemployment rate is larger than the average unemployment rate across all districts per state and year, zero otherwise. I obtain data on congressional districts' unemployment rates from the U.S. Census Bureau.
Class-Action Lawsuit Characteristics	
GAAP Violation	1 if the lawsuit involved a GAAP Violation as reported in the Stanford Law Database on Shareholder Lawsuits, zero otherwise.
Restatement	1 if the class-action lawsuit was triggered by a restatement as reported in the Stanford Law Database on Shareholder Lawsuits, zero otherwise.
Lawsuit Length	The natural logarithm of the number of days between the beginning of the lawsuit and the end of the lawsuit as reported in the Stanford Law

	Database on Shareholder Lawsuits.
Settlement Amount	The natural logarithm of the lawsuit settlement amount (set to zero if the lawsuit did not result in a settlement) as reported in the Stanford Law Database on Shareholder Lawsuits.
Restatement Characteristics	
GAAP Violation	1 if the lawsuit involved a GAAP Violation as reported in the GAO, Glass-Lewis, and Audit Analytics restatement databases, zero otherwise.
Class-Action Lawsuit	1 if a class-action lawsuit was filed in response to the restatement as reported in the Audit Analytics restatement database, zero otherwise.
Misstatement Length	The natural logarithm of the number of days between the beginning of the misstated period and the end of the misstated period as reported in the GAO and Audit Analytics restatement databases.
Restatement Magnitude	The sum of changes in net income for all periods affected by the restatement as reported in the Audit Analytics restatement database scaled by total assets.
Damages	The cumulative effect in stockholders' equity of the restatement as reported in the Audit Analytics restatement database scaled by total assets.
Revenue	1 if any portion of the restatement is due to revenue recognition problems as reported in the GAO and Audit Analytics restatement databases, zero otherwise.
Lease	1 if any portion of the restatement is due to the accounting for leases as reported in the Audit Analytics restatement database, zero otherwise.
Rule Change	1 if the restatement refers to an accounting rule (e.g., FAS 133, SAB 101) as reported in the Audit Analytics restatement database, zero otherwise. Number of Issues is the natural logarithm of the number of different accounting issues per restatement.
Number of Issues	Natural logarithm of the number of different accounting issues per restatement as reported in the GAO and Audit Analytics restatement databases.
Firm Characteristics	
F-score 1 (2) [3]	Predicted value = –7.893 + 0.79 × RSST accruals + 2.518 × Change in receivables + 1.191 × Change in inventory + 1.979 × % Soft assets + 0.171 × Change in cash sales – 0.932 × Change in ROA + 1.029 × Actual issuance (–0.15 × Abnormal change in employees + 0.419 × Existence of operating leases) [+ 0.082 × Market-adjusted stock return + 0.098 × lagged market-adjusted stock return]. Based on this predicted value the probability is calculated as $e^{(\text{predicted value})}/(1+e^{(\text{predicted value})})$. To arrive at the respective F-score the probability is divided by the unconditional probability (i.e., misstating firm-years / (non-misstating firm-years + misstating firm-years)). For more details see Dechow et al. (2011).
Mod. Jones Dis. Acc.	Discretionary accruals using the following modified Jones model as in Dechow et al. (2011): $\Delta WC_t = \beta_0 + \beta_1 (1/A_{t-1}) + \beta_2 \frac{\Delta S_t - \Delta Rec_t}{A_{t-1}} + \beta_3 \frac{\Delta PPE_t}{A_{t-1}} + e_t$) where $\Delta WC_t = \Delta AR_t + \Delta Inventory_t - \Delta AP_t - \Delta TP_t + \Delta other\ Assets\ (net)_t$. AR is accounts receivable, AP is accounts payable, TP is taxes payable. S is sales, Rec accounts receivables, A_{t-1} beginning of the year assets and PPE is property, plant and equipment. The unsigned estimated residuals are my proxy for discretionary accruals.
DD Dis. Acc.	Discretionary accruals according to Dechow and Dichev (2002), using the following OLS model: $\Delta WC_t = \beta_0 + \beta_1 (1/A_{t-1}) + \beta_2 CFO_{t-1} + \beta_3 CFO_t + \beta_4 CFO_{t+1} + e_t$ where ΔWCt is defined the same as for the modified Jones model and CFO is cash flow from operations. The unsigned estimated residuals are my proxy for discretionary accruals.
Restatement	1 in the years a restatement has been released by firms, zero otherwise. Restatements include intentional as well as unintentional misstatements as per the criteria of Hennes et al. (2008).

Proximate 100	1 if a firm's headquarters is located within 100 km distance to an SEC office, zero otherwise. For more details see Kedia and Rajgopal (2011).
PAC Contributions	A firm's PAC contributions as reported in the FEC dataset scaled by total average assets.
Lobbying Exp	A firm's lobbying expenditures as reported in the CRP dataset scaled by total average assets.
Connected Board	1 if one of a firm's board members has been president, (vice-)presidential candidate, senator, congressman, governor, mayor, chairman of the Party Caucus, member of the president's cabinet, White House staff, or campaign staff as reported in firms' Def 14a filings, zero otherwise.
Connected Board SEC	1 if one of a firm's board members has been SEC Commissioner or staff as reported in firms' Def 14a filings, zero otherwise.
Congressional Election	1 in all congressional election years, zero otherwise.
Union	Percentage of employees who are union members per four-digit SIC code as reported in the Union Membership and Coverage Database maintained by Hirsch and Macpherson (2003).
Analyst Following	Natural logarithm of the number of analysts issuing earnings forecasts for firms covered by IBES. Set equal to zero if the firm is not covered by IBES.
Fortune 500	1 if the firm is covered in the Fortune 500 index as reported in Compustat, zero otherwise.
ROA	Two-digit SIC code median-adjusted return on assets (i.e., Compustat item: IB / Total average assets).
Big 4	1 if a firm's auditor is a Big 4 auditor, zero otherwise.
Market-to-Book	Firm's market value scaled by firm's book value (Compustat item: CSHO × Compustat item: PRCC) / Compustat item: CEQ.
Leverage	Firm's long-term debt scaled by firm's total average assets (i.e., Compustat item: DLTT / Total average assets).
Z-score	Altman's Z-score is measured following Altman (1968) and is equal to 1.2 × [net working capital (ACT – LCT) / total assets (AT)] + 1.4 × [retained earnings (RE) / total assets] + 3.3 × [earnings before interest and taxes (PI + XINT) / total assets] + 0.6 × [market value of equity (CSHO × PRCC_F) / book value of liabilities (LT)] + 1.0 × [sales (SALE) / total assets]. I obtain this data from Compustat.
Firm Age	Natural logarithm of a firm's age; based on first time appearance in Compustat.
Abnormal Revenue Change	Following Brazel et al. (2009): Change in revenues – change in employees: $((SALE - SALE_{t-1}) / SALE_{t-1})) - ((EMP - EMP_{t-1}) / EMP_{t-1})$.
Words	The natural log of the number of words of firms' 10-K, as reported in the WRDS SEC Analytics Suite. This data is available as of 1994.
Fog Index	The Fog index, developed by Robert Gunning, calculated as follows: Fog = (words per sentence + percent of complex words) × 0.4. The index indicates the number of years of formal education a reader of average intelligence would need to read the text once and understand it, as reported in the WRDS SEC Analytics Suite. This data is available as of 1994.
Assets	Natural logarithm of a firm's total assets (i.e., Compustat item: AT).
SEC Chairman	Fixed effects for the tenure of each SEC Chairman as reported on the SEC homepage (http://www.sec.gov/about/sechistoricalsummary.htm).
U.S. President	Fixed effects for the tenure of each U.S. President.

Appendix B

Disclosure of informal and formal AAER investigations

Type of Announcement	Informal AAER Investigations (1)	Formal AAER Investigations (2)	Sum (1) and (2)
Firm Press Release – Total	8	0	8
Last Violation Year$_{t-1}$	0	0	0
Last Violation Year	3	0	3
Last Violation Year$_{t+1}$	4	0	4
Last Violation Year$_{t+2}$	1	0	1
Media Report – Total	65	49	114
Last Violation Year$_{t-1}$	6	4	10
Last Violation Year	16	12	28
Last Violation Year$_{t+1}$	27	19	46
Last Violation Year$_{t+2}$	13	13	26
Last Violation Year$_{t+3}$	3	1	4
Total	73	49	122

Notes: This table provides an overview of media reports and firms' press releases about SEC informal and formal AAER investigations matched to the violation years (as reported in the AAER) for the sample of AAER firms using Factiva. Informal investigations are the first step of the SEC enforcement process and formal investigations are the second step of the SEC enforcement process (see Figure 1).

Appendix C

Top 10 most important states per presidential election

Top 10, 1984		
Rank	**State**	**Score**
1	MN	85.00
2	RI	8.54
3	MA	5.83
4	MD	3.48
5	HI	3.37
6	IA	2.62
7	VT	2.39
8	PA	2.24
9	NY	2.11
10	WV	2.07

Top 10, 1988		
Rank	**State**	**Score**
1	VT	5.57
2	WA	5.35
3	IL	4.01
4	PA	3.78
5	MD	3.19
6	NM	3.07
7	MT	2.96
8	WV	2.57
9	WI	2.20
10	NY	2.15

Top 10, 1992		
Rank	**State**	**Score**
1	GA	11.61
2	NC	8.32
3	NH	7.47
4	NV	3.69
5	MT	3.57
6	AZ	3.37
7	WY	3.28
8	SD	3.11
9	FL	3.04
10	OH	2.84

Top 10, 1996		
Rank	**State**	**Score**
1	NV	13.47
2	KY	9.56
3	GA	7.67
4	CO	6.16
5	VA	4.38
6	SD	4.26
7	AZ	4.08
8	MT	4.07
9	TN	3.84
10	ND	2.63

Top 10, 2000		
Rank	**State**	**Score**
1	FL	1115.40
2	NM	327.31
3	WI	46.17
4	IA	40.47
5	OR	24.79
6	NH	13.29
7	NV	4.43
8	MN	4.09
9	MO	3.35
10	TN	3.28

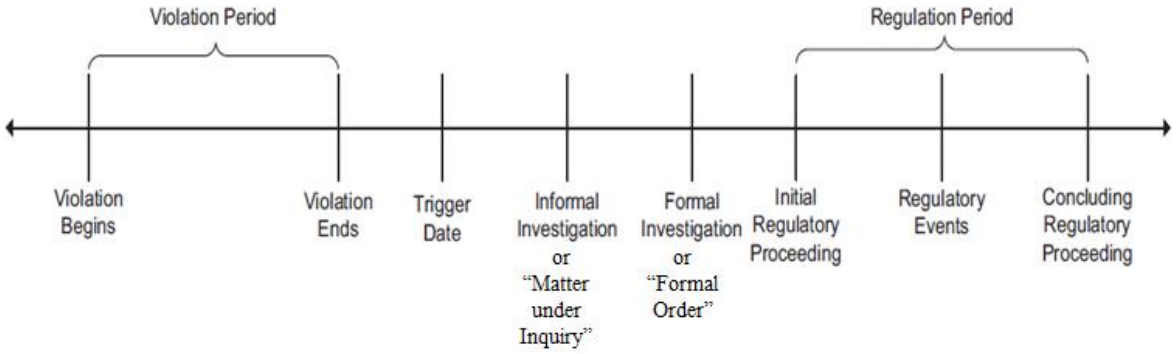
Top 10, 2004		
Rank	**State**	**Score**
1	WI	18.56
2	NM	13.34
3	IA	11.38
4	NH	9.57
5	NV	5.09
6	PA	3.61
7	OH	3.21
8	HI	2.36
9	DE	2.32
10	OR	2.28

Top 10, 2008		
Rank	**State**	**Score**
1	MO	86.85
2	NC	32.60
3	IN	11.94
4	MT	8.33
5	FL	3.52
6	ND	3.38
7	SD	2.88
8	OH	2.35
9	GA	2.26
10	NH	1.80

Top 10, 2012		
Rank	**State**	**Score**
1	FL	11.95
2	NC	4.99
3	OH	3.32
4	NH	3.09
5	NV	2.71
6	VA	2.67
7	AK	2.19
8	CO	2.00
9	IA	1.98
10	PA	1.95

Notes: This table includes an overview of the top ten most important states per presidential election for the period 1982-2012. I follow the methodology of Cebula et al. (2013) and express the political importance of a state by dividing the number of Electoral College votes at stake by the margin of victory for the winning candidate, denoted "Score." The states are then ranked in descending order for each U.S. presidential election based on that score.

Figure 1 Timeline of an SEC enforcement action



Notes: This figure shows the timeline of an SEC enforcement action and has been adapted from Karpoff et al. (2008).

TABLE 1
Sample selection

	Number of misstated firm-years	Number of distinct firms
AAER No. 1–No. 3180 from May 1982 to August 2012	1,838	789
Less: AAER firms without CIK	(251)	(133)
Less: Misstatements that cannot be linked to specific reporting periods	(84)	(44)
Less: AAER firms without CRSP and Compustat match	(331)	(123)
Less: AAER firms with missing data	(29)	(10)
Final AAER sample	**1,143**	**479**

Notes: Table 1, panel A includes an overview of the AAER sample selection. I obtain the AAER sample from Dechow et al. (2011). Their dataset consists of 3,403 SEC AAERs or 1,297 firm misstatement events issued between May 17th 1982 and August 31th 2012. The 1,297 firm misstatement events represent 1,838 misstated firm-years against 789 distinct firms. After dropping 251 (133) AAER firm-years (firms) without CIK, 84 (44) AAER firm-years (firms) with misstatements that cannot be linked to a specific reporting period, 331 (123) AAER firm-years (firms) that cannot be matched to CRSP and Compustat, and 29 (10) AAER firm-years (firms) with missing data for any of the variables used in the analyses, I arrive at my final AAER sample of 1,143 (479) AAER firm-years (firms).

TABLE 2
Descriptive statistics
Panel A: Summary statistics

Variables	N	Mean	Std.	Min	Median	Max
AAER	95,145	0.012	0.107	0	0	1
Employees	95,145	6.345	17.553	0.003	0.763	127.5
F-score 1	95,145	1.000	0.742	0.118	0.825	4.294
DD Dis. Acc.	89,453	0.068	0.079	0	0.041	0.379
Mod. Jones Dis. Acc.	94,921	0.060	0.067	0	0.036	0.333
Proximate 100	95,145	0.291	0.454	0	0	1
PAC Contributions	95,145	14,351	207,630	0	0	292,530
Lobbying Amount	49,701	57,466	228,720	0	0	1,740,000
Connected Board	37,794	0.078	0.267	0	0	1
Connected Board SEC	37,794	0.006	0.077	0	0	1
Union	95,145	0.111	0.117	0	0.071	0.501
Analyst Following	95,145	4.33	6.04	0	2	28
Fortune 500	95,145	0.083	0.275	0	0	1
ROA	95,145	–0.057	0.227	–1.16	0	0.294
Big 4	95,145	0.814	0.388	0	1	1
Market-to-Book	95,145	2.925	4.595	–12.751	1.859	30.107
Leverage	95,145	0.177	0.201	0	0.113	0.898
Z-score	95,145	4.288	7.865	–14.641	2.822	50.056
Firm Age	95,145	14.37	13.54	2	10	71
Abnormal Revenue Change	95,145	0.101	0.527	–1.381	0.045	3.413
Assets	95,145	1,818	7,093	1.74	130.99	43,453

Panel B: Statistics for AAER vs. non-AAER firms

Variables	N	AAER Sample (1)	Non-AAER Sample (2)	Difference (1) – (2)
Employees	95,145	12.49	6.27	6.22***
F-score 1	95,145	1.26	1.00	0.26***
DD Dis. Acc.	89,453	0.089	0.067	0.022***
Mod. Jones Dis. Acc.	94,921	0.080	0.058	0.022***
Proximate 100	95,145	0.359	0.290	0.069***
PAC Contributions	95,145	40,608	14,045	26,563***
Lobbying Amount	49,701	116,601	56,591	60,010***
Connected Board	37,794	0.138	0.077	0.061***
Connected Board SEC	37,794	0.017	0.006	0.011**
Union	95,145	0.089	0.111	–0.022***
Analyst Following	95,145	7.64	4.29	3.35***
Fortune 500	95,145	0.181	0.082	0.99***
ROA	95,145	–0.023	–0.057	0.034***
Big 4	95,145	0.846	0.815	0.031***
Market-to-Book	95,145	3.84	2.91	0.93***
Leverage	95,145	0.189	0.176	0.013**
Z-score	95,145	5.200	4.277	0.923***
Firm Age	95,145	13.36	14.38	–1.02**
Abnormal Revenue Change	95,145	0.099	0.101	–0.002
Assets	95,145	6,059	1,769	4,290***

Notes: The table displays the summary statistics for all variables (panel A) and a comparison of the average values of the variables for the AAER sample versus the non-AAER sample (panel B). ***, **, and * indicate the significance at the 1%, 5% and 10% level, respectively, of the difference between the means of the AAER sample as compared to the non-AAER sample. See Appendix A for variable definitions.

TABLE 3
Test of hypothesis 1
Panel A: Firm employment and SEC enforcement likelihood in class-action lawsuits and restatement samples

Variables	Prediction	AAER (1)	AAER (2)
Employees	(–)	–0.457***	–0.668***
		(0.01)	(0.00)
Class-Action Lawsuit Characteristics			
GAAP Violation		0.894***	
		(0.01)	
Restatement		1.283***	
		(0.00)	
Lawsuit Length		0.030	
		(0.79)	
Settlement Amount		–0.004	
		(0.85)	
Restatement Characteristics			
GAAP Violation			1.386***
			(0.00)
Class-Action Lawsuit			1.680***
			(0.00)
Misstatement Length			0.025
			(0.73)
Restatement Magnitude			0.243
			(0.38)
Damages			–0.439
			(0.34)
Revenue			0.103
			(0.69)
Lease			–1.496***
			(0.01)
Rule Change			0.009
			(0.98)
Number of Issues			0.316
			(0.12)
Firm Characteristics			
F-score 1		0.104	0.106
		(0.29)	(0.39)
Proximate 100		0.355	0.251
		(0.10)	(0.31)
PAC Contribution		0.002	–0.002
		(0.48)	(0.60)
Union		–0.894	–1.917
		(0.74)	(0.45)
Analyst Following		–0.028	0.348**
		(0.81)	(0.02)
Fortune 500		0.595*	0.430
		(0.10)	(0.24)
ROA		0.626	0.142
		(0.19)	(0.77)
Big 4		–0.058	0.255
		(0.89)	(0.72)
Market-to-Book		0.001**	0.001
		(0.03)	(0.15)
Leverage		0.551	0.582
		(0.12)	(0.16)
Z-score		–0.000	–0.004

	(0.95)	(0.48)
Firm Age	–0.215	–0.312**
	(0.12)	(0.03)
Abnormal Revenue Change	0.005	0.001*
	(0.30)	(0.08)
Fog Index	–0.021	0.045
	(0.89)	(0.80)
Words	0.050	0.023
	(0.33)	(0.68)
Assets	0.544***	0.706***
	(0.00)	(0.00)
Constant	–7.110***	–11.131***
	(0.01)	(0.01)
SEC Chairman FE	Yes	Yes
U.S. President FE	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
SE clustered by	Firm	Firm
Observations	3,685	5,864
Pseudo R-square	0.243	0.328
Variance Inflation Factor Range	1.03-7.97	1.01-7.49

Notes: The table presents the results on the relation between firm employment and SEC enforcement likelihood. The dependent variable in all models is an indicator variable equal to one in the years a firm misstated its financial statements as reported in Accounting and Auditing Enforcement Releases, and zero otherwise. The results reported are from a logistic regression estimation. The models differ in the variables included and sample composition. Column 1 reports the results using a sample of firms subject to class-action lawsuits for the period 1996-2008. Column 2 reports the results using a sample of restatement firms for the period 1996-2007. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

Panel B: Firm employment and SEC enforcement likelihood

Variables	Prediction	AAER (1)	AAER (2)	AAER (3)
Employees	(–)	–0.367***	–0.418***	–0.448***
		(0.00)	(0.00)	(0.00)
Firm Characteristics				
F-score 1		0.190***	0.235***	0.193***
		(0.00)	(0.01)	(0.00)
Proximate 100		0.401***	0.173	0.482***
		(0.00)	(0.37)	(0.00)
PAC Contribution		0.001	0.002	
		(0.39)	(0.27)	
Lobbying Exp			–0.001*	
			(0.10)	
Connected Board			0.305	
			(0.39)	
Connected Board SEC			0.542	
			(0.58)	
Union		–0.419	–0.690	–1.405
		(0.72)	(0.74)	(0.36)
Analyst Following		0.122*	0.293***	0.130
		(0.07)	(0.00)	(0.11)
Fortune 500		0.334*	–0.107	0.368
		(0.06)	(0.69)	(0.15)
ROA		–0.135	–0.004	–0.013
		(0.54)	(0.98)	(0.96)
Big 4		–0.781***	–0.495*	–0.752***
		(0.00)	(0.07)	(0.00)
Market-to-Book		0.033***	0.006	0.039***
		(0.00)	(0.64)	(0.00)
Leverage		0.218	0.240	0.047
		(0.39)	(0.52)	(0.88)
Z-score		–0.001	–0.000	–0.000
		(0.45)	(0.78)	(0.58)
Firm Age		–0.383***	–0.160	–0.495***
		(0.00)	(0.15)	(0.00)
Abnormal Revenue Change		0.001	0.001	0.001**
		(0.30)	(0.24)	(0.02)
Assets		0.543***	0.602***	0.537***
		(0.00)	(0.00)	(0.00)
Constant		–9.936***	–11.625***	–9.508***
		(0.00)	(0.00)	(0.00)
SEC Chairman FE		Yes	Yes	Yes
U.S. President FE		Yes	Yes	Yes
Industry FE		Yes	Yes	Yes
Year FE		Yes	Yes	Yes
SE clustered by		Firm	Firm	Firm
Observations		95,145	37,794	66,667
Pseudo R-square		0.162	0.229	0.144
Variance Inflation Factor Range		1.00-7.21	1.00-7.61	1.00-6.54

Notes: The table presents the results on the relation between firm employment and SEC enforcement likelihood. The dependent variable in all models is an indicator variable equal to one in the years a firm misstated its financial statements as reported in Accounting and Auditing Enforcement Releases, and zero otherwise. The results reported are from a logistic regression estimation. The models differ in the variables included and sample composition. Column 1 and 3 cover the period 1982-2012. Column 2 reports results including controls for firms' lobbying efforts, and firms' board members political as well as SEC connections. As the data on board members' biographies is only available from 2002 onwards, this model is limited to the period 2002-2012. Column 3 reports results excluding firms that ever reported any PAC contributions. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

TABLE 4
Test of hypothesis 2
Panel A: Firm employment and SEC enforcement likelihood in presidential election years

Variables	**Prediction**	**AAER (1)**	**AAER (2)**
Employees		–0.355***	–0.381***
		(0.00)	(0.00)
Presidential Election		–1.198***	–1.218***
		(0.00)	(0.00)
Employees × *Presidential Election*	(–)	0.038	0.060
		(0.31)	(0.13)
Important State			–0.083
			(0.76)
Important State × *Presidential Election*			0.683
			(0.11)
Important State × *Employees*			0.106
			(0.50)
Employees × *Presidential Election* × *Important State*	(–)		–0.512**
			(0.04)
Controls		Yes	Yes
SEC Chairman FE		Yes	Yes
U.S. President FE		Yes	Yes
Industry FE		Yes	Yes
Year FE		Yes	Yes
SE clustered by		Firm	Firm
Observations		95,145	95,145
Pseudo R-square		0.160	0.161
Variance Inflation Factor Range		1.00-7.53	1.00-7.55

Notes: The dependent variable in all models is an indicator variable equal to one in the years a firm misstated its financial statements as reported in Accounting and Auditing Enforcement Releases, and zero otherwise. The models differ in the variables included. *Employees* is the natural logarithm of a firm's employees. Column 1 provides the results for testing H2a and includes an interaction term with *Employees* and *Presidential Election*. Column 2 provides the results for testing H2b and includes, next to the variables included in column 1, *Important State*, and interaction terms with *Employees*, and *Presidential Election*. Column 1 and 2 span the period 1982-2012. The results reported are from a logistic regression estimation. *Controls* includes the same variables as in Table 3, panel B, column 1. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

Panel B: Firm employment and SEC enforcement likelihood in election years of senators serving on SEC oversight committees

Variables	Prediction	AAER (1)	AAER (2)	AAER (3)
State Employees		–0.229**	–0.210*	–0.215*
		(0.05)	(0.08)	(0.07)
Senate Election			–0.127	–0.122
			(0.42)	(0.48)
Senate Election × *State Employees*	(–)		–0.231*	–0.009
			(0.09)	(0.94)
High Unemployment State				0.276
				(0.19)
Senate Election × *High Unemployment State*				0.282
				(0.31)
State Employees × *High Unemployment State*				–0.003
				(0.98)
State Employees × *Senate Election* × *High Unemployment State*	(–)			–0.489**
				(0.03)
Controls		Yes	Yes	Yes
SEC Chairman FE		Yes	Yes	Yes
U.S. President FE		Yes	Yes	Yes
Industry FE		Yes	Yes	Yes
Year FE		Yes	Yes	Yes
SE clustered by		Firm	Firm	Firm
Observations		41,357	41,357	41,357
Pseudo R-square		0.172	0.174	0.174
Variance Inflation Factor Range		1.00-5.49	1.00-5.49	1.00-5.49

Notes: The dependent variable in all models is an indicator variable equal to one in the years a firm misstated its financial statements as reported in Accounting and Auditing Enforcement Releases, and zero otherwise. The models differ in the variables included and sample composition. *State Employees* is the natural logarithm of a firm's employees per state. I calculate a firm's number of employees per state by adjusting the total number of employees based on the occurrence of the headquarter state in a firm's 10-K relative to all other states from 1994 onwards (I obtain this data from Garcia and Norli (2012)). Column 1 provides the results for testing H1 using *State Employees*. Column 2 provides the results for testing H2c, and includes an interaction term with *State Employees* and *Senate Election*. Column 3 provides the results for testing H2d, and includes, next to the variables included in column 2, *High Unemployment State*, and interaction terms with *State Employees*, and *Senate Election*. The results reported are from a logistic regression estimation and span the period 1994-2012. *Controls* includes the same variables as in Table 3, panel B, column 1. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

Panel C: Firm employment and SEC enforcement likelihood in districts with Congressmen serving on SEC oversight committees

Variables	Prediction	AAER (1)	AAER (2)
District Employees		–0.225*	–0.240**
		(0.07)	(0.05)
House SEC Committee		0.010	–0.112
		(0.96)	(0.64)
District Employees × House SEC Committee	(–)	–0.026	0.172
		(0.88)	(0.30)
High Unemployment District			0.059
			(0.66)
House SEC Committee × High Unemployment District			0.322
			(0.27)
High Unemployment District × District Employees			0.029
			(0.78)
District Employees × House SEC Committee × High Unemployment District	(–)		–0.553**
			(0.05)
Controls		Yes	Yes
SEC Chairman FE		Yes	Yes
U.S. President FE		Yes	Yes
Industry FE		Yes	Yes
Year FE		Yes	Yes
SE clustered by		Firm	Firm
Observations		41,357	41,357
Pseudo R-square		0.173	0.173
Variance Inflation Factor Range		1.00-6.31	1.00-6.31

Notes: The dependent variable in all models is an indicator variable equal to one in the years a firm misstated its financial statements as reported in Accounting and Auditing Enforcement Releases, and zero otherwise. *District Employees* is the natural logarithm of a firm's employees per state. I calculate a firm's number of employees per state by adjusting the total number of employees based on the occurrence of the headquarter state in a firm's 10-K relative to all other states from 1994 onwards (I obtain this data from Garcia and Norli (2012)). I then match each firm to the congressional district where its headquarters are located. Column 1 provides the results for testing H2e, and includes an interaction term with *House SEC Committee* and *District Employees*. Column 2 provides the results for testing H2f, and includes, next to the variables included in column 1, *High Unemployment District*, and interaction terms with *District Employees*, and *House SEC Committee*. The results reported are from a logistic regression estimation and span the period 1994-2012. *Controls* includes the same variables as in Table 3, panel B, column 1. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

TABLE 5
Firm employment and accounting quality

Variables	F-score 1 (1)	F-score 2 (2)	F-score 3 (3)	Mod. Jones Dis. Acc. (4)	DD Dis. Acc. (5)	Restatement (6)
Employees	0.020**	0.016**	0.018*	0.003***	0.002***	–0.089
	(0.02)	(0.04)	(0.07)	(0.00)	(0.00)	(0.19)
ROA	0.238***	0.196***	0.232***	–0.043***	–0.048***	–1.066***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Big 4	–0.078***	–0.069***	–0.083***	–0.005***	–0.005***	–0.105
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.24)
Market-to-Book	0.006***	0.006***	0.007***	0.001***	0.001***	–0.013*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.07)
Leverage	0.360***	0.417***	0.399***	–0.007***	0.018***	0.267*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.08)
Firm Age	–0.069***	–0.074***	–0.054***	–0.004***	–0.005***	0.077*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.07)
Assets	0.015***	0.019***	0.014***	–0.008***	–0.006***	0.168***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	1.398***	1.081***	1.132***	0.094***	0.082***	–4.286***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
SE clustered by	Firm	Firm	Firm	Firm	Firm	Firm
Estimation technique	OLS	OLS	OLS	OLS	OLS	Logit
Observations	95,145	93,542	86,392	94,921	89,453	41,622
Adj. / Pseudo R-square	0.176	0.186	0.192	0.169	0.141	0.081

Notes: The table presents the results on the relation between firm employment and accounting quality. The dependent variable is a different proxy for a firm's accounting quality for the period 1982-2012 (columns 1-5) and 1997-2006 (column 6). The first three models use all three types of F-scores as defined by Dechow et al. (2011). The dependent variables in column 4 and column 5, respectively, are absolute discretionary accruals estimated from a modified Jones model as in Dechow et al. (2011) and estimated according to Dechow and Dichev (2002), respectively. The dependent variable in column 6 is an indicator variable equal to one in the years a restatement has been released by firms, and zero otherwise. The results reported for columns 1-5 are from an ordinary least squares regression estimation and for column 6 from a logistic regression estimation. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

TABLE 6
Propensity Score Matching
Panel A: Propensity score matching – First stage regression and covariate balance

Variables	Large Employer	Mean large employer	Mean matched small employer	Mean Difference
	(1)	(2)	(3)	(2) – (3)
F-score 1	0.207***	1.104	1.103	0.001
	(0.00)			(0.96)
Proximate 100	0.030	0.318	0.312	0.006
	(0.79)			(0.46)
PAC Contribution	–0.000	16.081	13.379	2.702
	(0.15)			(0.22)
Union	0.206***	0.100	0.102	–0.002
	(0.00)			(0.29)
Analyst Following	–0.080**	1.102	1.101	0.001
	(0.04)			(0.96)
Fortune 500	0.403	0.008	0.006	0.002
	(0.21)			(0.31)
ROA	1.096***	–0.013	–0.013	0.000
	(0.00)			(0.97)
Big 4	0.185**	0.839	0.840	–0.001
	(0.03)			(0.89)
Market-to-Book	–0.000	2.643	3.316	–0.673
	(0.19)			(0.61)
Leverage	–0.269*	0.180	0.180	0.000
	(0.08)			(0.96)
Z-score	–0.019***	4.432	4.421	0.011
	(0.00)			(0.97)
Firm Age	0.500***	2.310	2.311	–0.001
	(0.00)			(0.92)
Abnormal Revenue Change	–0.005***	0.058	0.169	–0.111
	(0.00)			(0.23)
Assets	2.023***	4.957	4.963	–0.006
	(0.00)			(0.79)
SEC Chairman FE	Yes			
U.S. President FE	Yes			
State FE	Yes			
Industry FE	Yes			
Year FE	Yes			
Observations	95,145	6,746	6,746	
Pseudo R-square	0.658			

Notes: Column 1 presents the propensity score estimation and columns 2-4 the covariate balance between the matched pairs. The dependent variable for model 1 is *Large Employer*, an indicator equal to one for all firm-year observations larger than the median value of *Employees*, zero otherwise. The results reported are from a Probit regression estimation for the period 1982-2012 and are used to calculate the propensity scores. Columns 2-4 report the average values of the variables used in my matching procedure after matching and the average difference in these variables of large employers and the matched small employers. Propensity scores for matching are obtained from the Probit model in column 1, and matching is conducted without replacement. To ensure the smallest propensity-score distance between the treatment and control firms, I apply a caliper matching estimator of 0.00005. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.

Panel B: Average treatment effect

Variables	**N**	**AAER**
Mean Large Employer	6,746	0.0084
Mean Matched Small Employer	6,746	0.0155
Mean Difference		**–0.0071*** **

Notes: This table reports the average treatment effect of being a *Large Employer* on the likelihood of receiving an AAER. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively. See Appendix A for variable definitions.

TABLE 7
U.S. vs. non-U.S. employment and SEC enforcement likelihood

Variables	Prediction	AAER (1)
U.S. Employees	(–)	–0.358*
		(0.09)
Non-U.S. Employees		0.003
		(0.78)
Controls		Yes
SEC Chairman FE		Yes
U.S. President FE		Yes
Industry FE		Yes
Year FE		Yes
SE clustered by		Firm
Observations		5,721
Pseudo R-square		0.173
Variance Inflation Factor Range		1.02-6.73

Notes: The table presents the results on the relation between U.S. and non-U.S. firm employment and SEC enforcement likelihood. The dependent variable is an indicator variable equal to one in the years a firm misstated its financial statements as reported in Accounting and Auditing Enforcement Releases, and zero otherwise for the period 1982-2012. The results reported are from a logistic regression estimation. The model partitions a firm's employees into U.S. versus non-U.S. employees for a subset of firms with available data in Compustat Segments. *Controls* includes the same variables as in Table 3, panel B, column 1. *P*-values are displayed in parentheses below the coefficient estimate. *, **, *** represent significance at the 10, 5, and 1 percent level (two-tailed), respectively; variables are winsorized at 1% and 99% levels. See Appendix A for variable definitions.