Regulatory Oversight and Reporting Incentives: Evidence from SEC Budget Allocations

Terrence Blackburne
The Wharton School
University of Pennsylvania
1300 Steinberg-Dietrich Hall
Philadelphia, PA 19104
tblac@wharton.upenn.edu

January 2014

Abstract

This study examines the determinants and consequences of regulatory oversight of corporate disclosures. I investigate the extent to which industry-level political activity influences the intensity of regulatory oversight, and whether variation in the intensity of oversight affects managers' reporting incentives. I exploit variation in the allocation of budgetary resources between the SEC's disclosure review offices as a source of variation in the oversight of financial reporting and disclosures. I find evidence of a significant relationship between industry-level political activity and visibility and the allocation of resources to each office. I then use the amount of budgetary resources allocated to each office as a proxy for the intensity of the SEC oversight that firms in a given industry face. I provide evidence that when SEC oversight is more intense managers report lower discretionary accruals, managers are less likely to issue financial reports that will be subsequently restated, and firms' bid-ask spreads decrease. Overall, the results suggest that SEC oversight plays an important role in shaping managers' reporting and disclosure incentives.

I am deeply indebted to the members of my dissertation committee for their patient guidance and insightful comments: Wayne Guay, Luzi Hail, Chris Ittner, Cathy Schrand, and Dan Taylor. I also thank Jennifer Altamuro, Chris Armstrong, Karthik Balakrishnan, Paul Fischer, Nick Gonedes, Mirko Heinle, Bob Holthausen, John Robinson, Stephanie Sikes, and Ro Verrecchia for their thoughtful discussions. In addition, two anonymous officials provided me with invaluable insight into the SEC's disclosure review and budgeting processes. My fellow Ph.D. students have provided much appreciated comments and support, particularly Michael Carniol and Stefan Schantl. Finally, I would like to thank the Wharton School and the Deloitte Foundation for their generous financial support.

1. Introduction

Both academics and practitioners have long debated the effectiveness of regulatory oversight on corporate behavior. Economists theorize that the political process leads to the capture of regulatory bodies by the firms they oversee, thus limiting regulation's effectiveness (e.g. Stigler 1971; Peltzman 1976). This skepticism concerning the general effectiveness of regulatory oversight has extended to debates regarding whether oversight by the Securities and Exchange Commission (SEC) affects managers' reporting and disclosure choices (e.g. Stigler 1964; Benston 1969; Seligman 2003; Bushee and Leuz 2005). Since its inception, the SEC has reviewed corporate disclosure filings to ensure their compliance with mandatory reporting and disclosure regulations (Seligman 2003). If it finds deficiencies in a firm's disclosure filing, the SEC may require the firm to amend the filing or restate its financial reports (Johnston and Petacchi 2013). This study contributes to the debate on the SEC's effectiveness by examining two questions related to its filing review process. First, to what extend do industry-level political factors affect the allocation of resources the SEC devotes to reviewing corporate disclosures? And, second, does variation in the intensity of SEC oversight affect managers' reporting and disclosure choices?

Researchers have generally acknowledged that limited budgetary and staffing resources constrain the SEC's activities (e.g. Pincus et al. 1988; Cox et al. 2003; Jackson and Roe 2009; Kedia and Rajgopal 2011). Several recent studies have used aggregate regulatory resources as input-based measures of oversight intensity (Coffee 2007; Jackson and Roe 2009; Christensen et al. 2011; Del Guercio et al. 2013). But, these studies do not examine how budgetary resources are allocated within the regulatory body. Thus, they can only provide insight into the forces that

shape the intensity of country-level regulatory oversight and how it affects corporate behavior at the country-level.

I attempt to look inside the "black box" by evaluating the SEC's reporting oversight activities directly. I use a novel panel dataset, obtained through request from the SEC, consisting of budget and staffing allocations for each disclosure review office within the SEC's Division of Corporation Finance. These offices provide a powerful setting for investigating factors that influence the intensity of SEC oversight and how its intensity affects managers' financial reporting incentives. The disclosure review offices are organized by industry and are established to ensure that information is disseminated to capital market participants. Therefore, their budget and staffing allocations do not reflect SEC activities related to other regulatory activities. The disclosure review offices carry out their mandate by reviewing the adequacy of firms' SEC filings and by helping firms interpret disclosure rules. Because firms are assigned to a disclosure review office on the basis of their four-digit SIC code, managers know in advance what office will review their firm's filings. Moreover, Johnston and Petacchi (2013) note that a substantial portion of the comment letters issued by these offices lead to amended filings. In addition, periodic filing reviews are a major source of leads that result in eventual SEC enforcement actions (Feroz et al. 1991). I exploit variation in the allocation of budgetary resources and workload between the disclosure review offices to construct a proxy of the intensity SEC's oversight of financial reporting and disclosures.

Regulatory budget allocations are an outcome of the political process. Economists have long argued that frictions in the political process result in the formation of interest groups that wield disproportionate influence over political outcomes (Stigler 1971; Peltzman 1976; Becker 1983). Therefore, I hypothesize that interest-group politics affect the allocation of resources

between the SEC's disclosure review offices. I measure industry-level political activity as the number of contributions made by firms in an industry to the congressional campaigns of candidates with a committee or subcommittee assignment relevant to the SEC. This is because Congress is the ultimate source of budgetary resources for bureaucratic entities within the United States, and members of Congress with relevant committee and subcommittee assignments wield disproportionate power over the regulatory bodies they oversee (Weingast and Moran 1983). I use the 2008 financial crisis as a shock to political visibility that likely shifted political power from financial services firms to investors. Consistent with the theory, I find evidence that the intensity of SEC oversight over corporate disclosures is associated with industry-level political contributions and political visibility. Specifically, I find statistically significant evidence that a one percent increase in industry-level political contributions is associated with a 0.12 to 0.31 percent decrease in the following years' office-level budget allocation. Moreover, the financial crisis is associated with a 9 to 15 percent increase in the budget allocation to the disclosure review office overseeing the financial services industry.

Next, I exploit the SEC's organizational structure and features of the federal budget process to empirically test whether variation in the intensity of SEC oversight affects managers' reporting and disclosure incentives. Theory suggests that if managers rationally anticipate the intensity of SEC oversight then it should affect their reporting choices (e.g. Fischer and Verrecchia 2000). Because SEC resources are allocated before managers make their reporting and disclosure choices, I test whether the intensity of SEC oversight affects managers' reporting and disclosure outcomes. I use three main proxies for managers' reporting and disclosure outcomes: discretionary accruals, the incidence of accounting restatements, and bid-ask spreads. I find evidence that, when SEC oversight is more intense, managers report lower discretionary

accruals and are significantly less likely to issue financial reports that are subsequently restated. In addition, I find evidence that SEC oversight has significant capital market benefits. My findings suggest that a one percent increase in my proxy for the intensity of SEC oversight results in a 0.3 percent decrease in firms' bid-ask spreads.

I conduct additional tests to assess whether the effect of SEC oversight on managers' reporting incentives varies with changes in the political environment. I draw upon the political cost hypothesis (Watts and Zimmerman 1978, 1986) and argue that politically active firms engage in the political process to reduce political costs. If politically active firms have "captured" the regulatory process, then changes in SEC oversight should have a smaller effect on their reporting incentives than for non-politically active firms (Gordon and Hafer 2005). Consistent with the theory, I find evidence that changes in the intensity of SEC oversight have a smaller effect on the propensity of politically active firms to issue accounting reports that are subsequently restated. However, I fail to find evidence that SEC oversight has a differential effect on politically active firms' discretionary accruals or bid-ask spreads. Likewise, I use the collapse of Lehman Brothers and the subsequent financial crisis as an inter-temporal shock that likely shifted the balance of political power from firms to investors. Consistent with theory that suggests such an event would increase demand for regulatory oversight, I find that managers' reporting choices are more responsive to SEC oversight during the time period after the Lehman Brothers collapse. The results generally support the notion that managers' reporting incentives are affected by political forces.

My study makes several contributions that should be of interest to academics and practitioners. First, it contributes to the nascent literature examining the SEC's filing review process. These studies have investigated characteristics that are common to firms that receive

SEC comment letters (Cassell et al. 2013, Johnston and Petacchi 2013) or determinants of compliance with specific disclosure requirements (Robinson et al. 2011). A common feature of these studies is that they rely on ex post indicators of SEC scrutiny, which are a joint function of SEC oversight and managers' behavior. Therefore, they are constrained in their ability to provide insight into the effects of SEC scrutiny on managers' ex ante reporting choices. I provide evidence that managers' strategically adjust their reporting and disclosure behavior in response to changes in the intensity of SEC oversight.

Second, my study provides insight into the literature that examines the relation between corporate political activity and financial reporting quality. Prior research finds that more politically active firms face lower enforcement penalties and evade detection for longer periods when they misreport financial information (Correia 2009; Yu and Yu 2011). I find evidence of a possible mechanism for these findings; namely, the SEC devotes fewer budgetary resources to detect misreporting when firms are more politically active.

Third, I contribute to the literature that examines the consequences of oversight by securities regulators on managers' reporting decisions. The literature in this area produces mixed results. Several studies suggest that enforcement is necessary for securities regulations to have any capital market effects (Bhattacharya 2002; Christensen et al. 2011; Christensen et al. 2013). However, prior research also finds that the SEC only uncovers a relatively small portion of the frauds that are eventually discovered (Dyck et al. 2010; Dyck et al. 2013). Because studies that use output-based measures of oversight intensity omit regulatory efforts to deter non-compliant behavior, an emerging literature uses input-based measures to identify variation in the intensity of regulatory oversight (Coffee 2007; Jackson and Roe 2009; Kedia and Rajgopal 2011; Hanlon et al. 2012; Del Guercio et al. 2013). My results suggest that the intensity of SEC disclosure

monitoring likely plays a role in deterring fraudulent reporting, which could explain the small portion of frauds that the SEC discovers.

Finally, my study contributes to a large literature in accounting that concludes that reporting outcomes are a function of managers' reporting incentives. Prior research in this area has shown that properties of financial reporting vary with legal institutions, listing status, governance structure, and contracting incentives related to debt and compensation (e.g. Ball et al. 2000; Beatty and Weber 2003; Bushman et al. 2004; Ball and Shivakumar 2005; Burgstahler et al. 2006; Armstrong et al. 2013). My results suggest that regulatory oversight provides an additional monitoring mechanism that affects managers' incentives when they make their reporting and disclosure choices.

The remainder of the paper is organized as follows. Section 2 provides institutional details regarding the SEC's oversight of firms' financial reporting and mandatory disclosure filings. Section 3 provides a review of the relevant literature and outlines my hypotheses. Section 4 provides a description of the data. I present the results of my analyses in Section 5. Finally, section 6 concludes.

2. Institutional Background—SEC Disclosure Oversight

In the United States, the SEC is responsible for enforcing securities regulations, including those related to mandatory disclosure filings and financial reporting. One tool that the SEC uses to enforce compliance with disclosure regulations is its filing review process. The SEC monitors firms' compliance with disclosure regulations by reviewing a subset of all corporate disclosure filings. The Sarbanes Oxley Act requires the SEC to conduct a review of a firm's financial statements at least once every three years. In addition, the SEC selectively reviews firms' filings when it believes they are likely to be deficient. If it identifies deficiencies in a firm's filings the

SEC sends the firm a comment letter, which may seek clarification, require additional disclosures, or direct the firm to amend the filing.

Within the SEC, the Division of Corporation Finance (DCF) is responsible for overseeing compliance with corporate disclosure regulations. The DCF, in turn, has twelve Disclosure Operations Offices that implement the filing review process[1]. These offices are organized based on industry; firms are assigned to a Disclosure Review Office based on their four-digit SIC code[2]. Under certain circumstances a firm's filing may be reviewed by a different office, such as when the filing is associated with a transaction that pertains to another office's area of expertise or if the Division is conducting targeted reviews of specific disclosure items. But, in general, each office's ability to review filings made by firms assigned to a different office is limited because their staffs maintain specific industry expertise.

The filing review process consists of four phases (GAO 2013). During the screening phase examiners use the selection criteria developed at the beginning of the fiscal year to determine the review's scope. Once the scope of the review is determined, filings enter the examination phase, where examiners evaluate whether the information under review in a filing is compliant with applicable regulations. If SEC examiners identify any deficiencies in a filing then they will propose comments soliciting information to correct them. The next step in the process is the closing of the filing review. During this phase examiners prepare a closing memorandum that documents the results of the review. The final phase in the process is the public posting of any SEC comments and firms' responses to them on the SEC's website.

[1] During the sample period I examine the Division maintained eleven Disclosure Operations Offices.
[2] Broad industry areas are presented in table 1. Current office assignments can be viewed at http://www.sec.gov/info/edgar/siccodes.htm.

The scope of filing reviews varies between full cover-to-cover reviews, where every aspect of a filing is reviewed in detail for compliance with SEC regulations, to targeted reviews where the DCF staff examines a single disclosure item. At the beginning of each fiscal year, when overall budgetary resources are known, DCF managers develop goals related to the number and scope of filing reviews; in addition, they suggest criteria that the Disclosure Operations Offices should use to identify firms subject to selective reviews (GAO 2013). The filing review process is labor-intensive. Typically, two members of the DCF staff review a selected filing to ensure consistency across all reviews. Budgetary limitations constrain both the quantity and scope of filing reviews that the SEC is able to undertake during any given period. Differences between office-level budget allocations thus result in both cross-sectional and inter-temporal variation in the intensity of SEC monitoring faced by firms.

The SEC offsets the costs of its operations with the fees it collects from firms subject to its oversight. However, unlike banking regulators, the SEC must obtain annual appropriations from Congress before it can access these funds. The SEC prepares its request for budgetary resources more than a year before they are ultimately implemented (Bealing 1994). In this sense, the budget allocation is exogenous with respect to unanticipated events that occur in the year it is being implemented. The staff first prepares an SEC-wide budget based on the prior year's appropriations that conforms to guidelines provided by the White House Office of Management and Budget (OMB). The budget staff then requests information on staffing requirements at the division and office level. Division and office officials report that requests for additional resources must be consistent with OMB guidance and thus do not necessarily reflect actual requirements (GAO 2002).

The SEC submits its initial fiscal year budget request to OMB roughly one year before the fiscal year start. Figure 1 contains a timeline of the federal budget process. While the budget request is under OMB review, the SEC has an opportunity to amend the overall request due to changes in policy. After OMB reviews and approves the SEC's request, the White House transmits it to Congress along with the overall President's Budget Request. Congress conducts budget hearings and provides the SEC with an annual appropriation at the beginning of every federal fiscal year[3]. Unlike most appropriations, which are only available for one year, SEC appropriations are available until they are expended. But, Congress typically rescinds any remaining balances at the end of the fiscal year.

The allocation of budgetary resources for the upcoming fiscal year is publically observable at the division-level once Congress passes an appropriations bill. However, cross-sectional variation in office-level budget allocations are not directly observable. Nonetheless, there are avenues through which managers may infer them indirectly. First, in addition to reviewing firms' SEC filings, the DCF also provides firms with interpretive guidance about disclosure regulations. Therefore, SEC staff have frequent interactions with firms' management, auditors, and legal counsel. Although the SEC is unwilling to disclose details regarding internal procedures, managers may be able to infer the extent of resource constraints faced by SEC staff based on their responsiveness to both formal and informal inquiries. Second, new job postings are publically available through the Office of Personnel Management, which makes it possible to estimate changes in office staffing levels; managers can easily obtain information about staff

[3] Each federal fiscal year ends on September 30 of that year. For example, fiscal year 2012 began on October 1, 2011 and ended on September 30, 2012.

changes within a given disclosure review office.[4] Finally, many firms retain outside legal counsel with experience as SEC staff to assist with SEC filing compliance (DeHaan et al. 2012).[5]

The SEC has some ability to reallocate resources internally during each fiscal year, but institutional frictions limit it its ability to do so. First, if the SEC wishes to reallocate budgetary resources in response to changing economic circumstances beyond an authorized threshold it must submit a reprogramming request to the House and Senate Appropriations Committees for their approval[6]. Second, because disclosure review staff have specialized industry expertise, their ability to assist other offices is limited. Staff do shift into other offices when opportunities arise. But, these moves are typically accompanied with increases in pay and are long-term. Thus, it does not appear that the SEC has flexibility in its staffing to adjust to rapid-changing market demands. The SEC faces the challenge of anticipating which set of firms are most likely to issue deficient filings, and hence where the demand for monitoring intensity is highest, more than a year before the managers of these firms make their disclosure choices.

3. Hypothesis Development and Literature Review

I examine whether the intensity of SEC oversight affects managers' reporting and disclosure choices. Before examining any effects, however, it is useful to consider factors that may shape the SEC's oversight behavior.

3.1 Theories of Regulatory Behavior

[4] See, for example, http://www.rrdonnelley.com/_documents/industry-solutions/financial_services/1-New_SEC_Developments.pdf

[5] In addition, numerous law firms specializing in SEC compliance issues advertise the experience of their staff as former SEC officials. See, for example, http://www.andrewskurth.com/practices-Corporate_Compliance_Investigations_Defense.html

[6] The threshold varies from year to year, but is typically set at the lessor of one million dollars or ten percent of the activity's budget.

A large body of literature in economics and political science seeks to explain the determinants of regulatory behavior.[7] Laffont and Tirole (1991) classify the dominant theories in this literature into two broad groups: "public interest" theories and "interest group" theories. "Public interest" theories typically take the perspective that regulatory agencies take actions to maximize social welfare. They posit that governments promulgate regulations to mitigate losses in welfare caused by perceived market failures (Shleifer 2005). In contrast, "interest group" theories view regulators as self-interested utility maximizers. Stigler (1971), Peltzman (1976), and Becker (1983) provide a foundation for this second set of theories. They argue that small interest groups are able to form coalitions more effectively than the general electorate when regulations will impose disproportionate marginal costs (or marginal benefits) on them. These coalitions are able to "capture" regulatory agencies and distort policy to maximize their interests rather than social welfare.

Weingast and Moran (1983) propose a "congressional dominance" extension to the early "interest group" theories by examining the role of the legislature in the regulatory process. They note that Congress both exercises oversight over regulatory agencies and allocates budgetary resources to those agencies that provide its members with the greatest marginal political benefits. Members of Congress seek membership on the oversight committees that provide them with the greatest political benefits, given their constituencies, and are therefore likely to be informed about issues affecting the agencies they oversee. They argue that political institutions have evolved such that interest groups affect regulatory behavior via their influence over Congress. Weingast (1984) applies the congressional dominance theory to investigate empirically behavior by the SEC and finds evidence that the SEC's success at implementing policy changes is

[7] For an extensive review of the literature in this area, see Mueller, 2003.

dependent on their alignment with congressional preferences. He further finds evidence that Congress rewards the SEC with larger budget allocations when it is more politically valuable.

The "interest group" theories outlined above suggest that corporate political contributions to congressional candidates are likely to affect the SEC's oversight activities. The political cost hypothesis, developed by Watts and Zimmerman (Watts and Zimmerman 1978), proposes that firms will make political contributions and accounting choices to minimize wealth transfers created by the political process. Consistent with this, Yu and Yu (2011) find evidence that corporate lobbying is associated with delayed fraud detection and a lower probability that fraud will be detected by regulators. Moreover, they find that firms increase lobbying activity following the initiation of the fraud. Correia (2009) finds similar patterns with corporate contributions to political campaigns and accounting quality. Thus, the extant empirical evidence suggests that firms use expenses on political activity to reduce the costs of misreporting.

However, prior empirical research has generally failed to document substantial links between political contributions and congressional voting behavior (Ansolabehere, deFigueiredo and Snyder 2003). If political contributions have little effect on congressional voting outcomes, then it is unclear whether and how firms use them to affect regulatory agencies' behavior. Given the lack of strong empirical associations between contributions and congressional voting, it is conceivable that political contributions are merely a consumption good. Corporate political contributions may simply be perquisites consumed by managers rather than expenditures made by firms to minimize political costs, and thus be a manifestation of unresolved agency problems within a firm.

Gordon and Hafer (2005) propose that corporate contributions to congressional campaigns need not affect congressional voting behavior to affect the behavior of regulatory

agencies. They argue that political contributions serve as a signal of firms' willingness and ability to impose political costs on the agencies that regulate them. Under this signaling hypothesis regulatory agencies will devote fewer resources to detect infractions committed by politically active firms because pursuing enforcement actions against them is more costly to the regulator than pursuing enforcement actions against non-politically active firms. Regulators at the SEC need not observe the political contributions directly if they are correlated with other behavior that the SEC does observe, such as inquiries from congressional staff or changes in firms' aggressiveness during regular interactions.

Prior research suggests the SEC is quite sensitive to potential costs when it decides whether to pursue an enforcement action; and—because of resource constraints—that the SEC only pursues cases when it believes it has a high probability of obtaining a successful outcome (Cox, Thomas and Kiku 2003). If political costs affect the intensity SEC oversight, then one would expect the SEC to devote fewer budgetary resources to oversee the filings of firms in industries that are politically active. However, the political cost hypothesis and prior empirical evidence suggests that firms that misreport accounting information are likely to increase their political activity. Therefore, if increases in political activity signal a higher probability of reporting deficiencies, it is conceivable that the SEC would increase the allocation of budgetary resources to these industries. I formally state the first hypothesis below (in the null form):

H1: Political activity and visibility do not affect the allocation of resources between the SEC's disclosure review offices.

3.2 The Intensity of SEC Oversight and Managers' Reporting Incentives

Does the intensity of SEC oversight affect managers' reporting and disclosure behavior? Economic theory suggests that more intense SEC oversight should increase ex ante compliance

with mandatory reporting and disclosure regulations if it increases the probability that non-compliance is detected (Becker 1968). Because monitoring and enforcement are costly activities, the intensity of regulatory oversight depends on the amount of budgetary resources devoted to it (Stigler 1970). Prior research suggests that budgetary resources are a binding constraint on SEC oversight activities (Cox, Thomas and Kiku 2003; Kedia and Rajgopal 2011). The allocation of budgetary resources within the SEC is thus a source of revealed oversight priorities given overall constraints. If managers make strategic reporting and disclosure decisions then, ceteris paribus, the extent of their compliance with reporting and disclosure regulations should be increasing in the amount of budgetary resources devoted to SEC oversight.

Theory suggests that managers will adjust their reporting and disclosure behavior in response to changes in the intensity of SEC oversight if they can either anticipate or observe it (Fischer and Verrecchia 2000). Consistent with this, Kedia and Rajgopal (2011) find evidence that managers of firms located closer to SEC offices—and thus likely to be more informed about the SEC's oversight activities—are less likely to misreport their financial statements. But, their evidence is indirect; because of data limitations they are unable to observe the SEC's oversight activities directly or to infer changes in them. While data limitations have prevented researchers from measuring changes in the intensity of SEC oversight, firms are likely to be aware of them—at least to some extent—because of their use of intermediaries (such as auditors or outside legal counsel) that have regular contact with SEC officials. Therefore, it is likely that managers are able to correctly infer the intensity of SEC oversight during any given accounting period.

Nonetheless, prior research has found surprisingly little evidence of a direct effect of SEC oversight on managers' reporting and disclosure behavior. Johnston and Petacchi (2013) provide a notable exception; they find evidence that firms improve the quality of their reporting and

disclosure following resolution of an SEC comment letter. Similarly, Robinson et al. (2011) find evidence that firms' compliance with mandated executive compensation disclosures is dependent on the identification of deficiencies by the SEC. A common feature of the studies that examine direct effects of SEC oversight, however, is that they rely on ex post measures of it and, therefore, they are unable to provide evidence regarding managers' strategic reporting and disclosure behavior.

Flexibility in accounting standards and variation in regulatory oversight provide scope for managers to exercise discretion when preparing mandated reports. The ability to exercise discretion allows managers to provide accounting information that more accurately reflects the underlying performance of the firm, but also provides them with scope to misreport earnings for self-serving purposes (Healy and Wahlen 1999). Theory posits that the amount of discretion that managers can exercise is a function of its costs and that managerial discretion used to bias information decreases the precision of mandated disclosure reports (Fischer and Verrecchia 2000). If increased regulatory oversight increases the costs to managers of using reporting discretion to bias financial reports then it should function as an ex ante commitment device that limits this behavior, resulting in more precise disclosures. To the extent that more precise disclosures generally reduce information asymmetry and level the playing field between investors, they should lower adverse selection costs that are manifested in bid-ask spreads (Diamond and Verrecchia 1991). I formally state the second hypothesis below (in the null form):

H2: The intensity of SEC oversight does not affect managers' financial reporting and disclosure behavior.

There is reason to expect variation in the effects of SEC oversight on managers' reporting and disclosure incentives. Prior research has shown that politically active firms are less subject to

SEC enforcement actions and less likely to have a restatement initiated by an SEC comment letter (Correia 2009). In addition, researchers have found that regulators take longer to discover fraud when it is committed by politically active firms (Yu and Yu 2011). The theory outlined above suggests that the SEC will devote fewer resources to monitoring compliance with disclosure regulations for politically connected firms because it is more costly to pursue enforcement actions against them, which provides a possible explanation for extant empirical findings (Gordon and Hafer 2005). If it is more costly for the SEC to pursue enforcement actions against politically connected firms then the reporting and disclosure behavior of these firms' managers should be less responsive to changes in the intensity SEC oversight than the behavior of managers of non-politically active firms. Another possibility, however, is that firms engaged in the political process are more aware of the SEC's oversight priorities. If this is the case, then managers of firms that are politically active may exhibit reporting and disclosure behavior that is more sensitive to changes in the intensity of SEC oversight.

Watts and Zimmerman (1978) introduced the hypothesis that political costs affect managers' reporting and disclosure incentives. They note that crises can heighten pressure on politicians and regulators to exact wealth transfers from regulated firms (Watts and Zimmerman 1986). The bankruptcy of Lehman Brothers in the fall of 2008 and subsequent financial crisis was a shock that likely focused political and regulatory attention on financial markets. This likely created increased pressure for the SEC to ensure that the reporting and disclosures made were especially high quality. Moreover, if managers anticipated increased regulatory scrutiny because of the shock to their political visibility then the theory outlined above suggests that their reporting and disclosure choices would be more responsive to SEC oversight than in prior periods. I formally state my third hypothesis (in the null form) below:

H3: The effect of SEC oversight on managers' financial reporting and disclosure behavior does not vary with firm-specific or economy wide political factors.

A large body of research in the accounting literature finds evidence that litigation risk affects managers' reporting and disclosure incentives (e.g. Skinner 1994; Skinner 1997). In addition, prior research has argued that class action litigation may serve a similar function in shaping managers' reporting incentives as SEC oversight (e.g. Coffee 2007). Jennings et al. (2011) find that class action lawsuits against a firm's peers is an effective deterrent against fraudulent financial reporting. This body of research suggests that class action litigation provides an alternative form of oversight regarding corporate disclosures. Thus, SEC oversight and class action litigation may be substitutes. Therefore, if private litigation activity is high then I expect managers reporting and disclosure behavior to be less responsive to SEC oversight than when it is low. I formalize this hypothesis below (in the null form):

H4: The effect of SEC oversight on managers' financial reporting and disclosure behavior does not vary with the amount of private litigation against peer firms.

4. Data

4.1 SEC Disclosure Review Office-Level Budget Allocations

I obtain internal SEC data regarding staffing and salary levels in each of the 12 disclosure review offices of the Division of Corporation Finance and the annual budgetary resources for the Division as a whole for fiscal years 2003 to 2012. These data are not publically available, but were provided to me by the SEC for research purposes. I use information on employees' pay grades and OPM pay tables to estimate the salary portion of office-level budgets for each year. I then allocate non-salary budgetary resources for the Division of Corporation Finance as a whole to each office based on its proportion of the overall number of Division of Corporation Finance

employees. To ensure consistency across time, I convert my estimate of budgetary resources into constant year 2010 dollars (*Dollars*) using CPI data obtained from the Federal Reserve. In fiscal year 2011, the Division split the office responsible for reviewing the filings of firms in the financial services industry into two offices. Because my analyses rely on lagged industry data, I consolidate the two financial services offices into a single entity. Therefore, my office-level sample consists of eleven offices over the ten year period, for 110 office-years. Descriptive statistics regarding budget and staffing allocations between the eleven offices and across the ten fiscal years are presented in Table 1.

There is fairly substantial cross-sectional and inter-temporal variance across each of the eleven offices and over time. Panel A shows the mean allocations by office. Over the sample period, the standard deviation of budgetary resources for each office ranges from 14.8% to 27% of the mean budget allocation. The largest variation is in Office 7, which reviews filings for the financial services industry and experienced a large increase in budgetary resources beginning in fiscal year 2010. Panel B of Table 1 shows the mean allocations of budgetary resources and staff for each fiscal year. There is a large increase in average budgetary resources from fiscal years 2004 to 2005, as the Division ramped up its ability to implement the enhanced disclosure reviews required by the Sarbanes Oxley Act, which became effective in 2002. Otherwise, there does not appear to be any inter-temporal pattern in the annual allocation of budgetary resources. While cross-sectional variation in the allocation of budgetary resources is not as substantial as inter-temporal variation, it remains reasonably large—ranging from 6.6% to 17.3% of the mean allocation for each year.

The data on staffing allocations allow me to calculate measures of staff turnover and supervisor tenure. Table 2, Panel A presents office-level and aggregate industry-level descriptive

statistics[8]. The average tenure of a disclosure review office supervisor (*SupervisorTenure*) in my sample is 8.3 years, and on average each office experiences 14.2% annual turnover (*SECTurnover*).

I calculate industry-level variables using all firms in the Compustat/CRSP universe. I obtain data on corporate political contributions from the Federal Election Commission, on restatement filing dates and beginning periods from Audit Analytics, and on IPOs from SDC Platinum. My industry-level measure of political activity, *IndPoliticalActivity,* is calculated as natural logarithm of the sum of candidates receiving political contributions from firms in a given industry in each year. I use two measures to capture the visibility of a given industry. The first, *OfficeRestatements,* is the natural log of the percent of firms assigned to a disclosure review office in a given year that restate accounting information. *OfficeRestatements* is an indicator of visible accounting-related problems in an industry, which should lead to increased political demand for regulatory oversight. The second, *Lehman,* is an indicator variable for financial services firms that is equal to one for fiscal years after the Lehman Brothers bankruptcy, which is a shock that likely increased the political visibility of the financial services industry. Next, I calculate controls for industry conditions that affect the workload of the disclosure review offices. *OfficeIPOs* is the natural log of one plus the number of IPOs made by firms assigned to the disclosure review office for each year. IPOs are a key driver of disclosure review workload, as initial offering materials are subject to a full review that takes 4-7 weeks of SEC staff time (GAO 2002). Finally, *OfficeCap* is the natural log of the aggregate market capitalization of all firms covered by a given disclosure review office.

4.2 Firm-Level Data

[8] I define industries based on the assignments made to the 11 disclosure review offices.

In addition to office- and industry-level data, I collect firm-level stock return and financial statement data from the CRSP/Compustat merged files from 2003 to 2011. I require a match between CRSP/Compustat and EDGAR to obtain information regarding firms' disclosure review office assignments. My final sample is constructed as the intersection of these three datasets and contains 22,434 firm years (4,509 firms) over the period 2003-2011. In addition I obtain—but do not require—restatement data from Audit Analytics, corporate political contribution data from the Federal Election Commission, and class action litigation data from the Stanford Securities Class Action Clearinghouse. I match each firm to the SEC office that reviews its disclosure filings, and measure the intensity of SEC oversight based on the federal fiscal year as of the filing date of the firm's 10-K.

I examine the relation between SEC oversight and three outcomes of managers' reporting and disclosure choices: the exercise of reporting discretion, misreporting, and information asymmetry. The first construct I consider is the amount of discretion that managers exercise when making accruals choices. I measure discretion using the absolute value of discretionary accruals (Dechow, Ge and Schrand 2010). I calculate my proxy, *AbsDA_MJ*, as the absolute value of the residual obtained from estimating the modified Jones model of accruals suggested by Dechow et al. (1995). I use discretionary accruals as my measure of discretion for two reasons. First, discretionary accruals are an input in the SEC's accounting quality model, which is used identify firms that may require closer regulatory scrutiny (Lewis 2012). If large discretionary accruals invite more intense regulatory oversight then managers may choose to report lower discretionary accruals in order to avoid political costs associated with increased regulatory review. Because the output from a discretionary accrual model is precisely what managers would manipulate to avoid SEC scrutiny, my setting is less contaminated by concerns

about misspecification of the accruals model and correlated omitted variables than typical studies that use discretionary accruals as a proxy for earnings management. Second, I do not necessarily want to measure intentional misrepresentation of firm performance. Managers may choose to exercise reporting discretion either to make earnings more informative for investors or to misreport earnings for other, self-serving, purposes (Guay et al. 1996; Healy and Wahlen 1999). Moreover, recent evidence suggests that overly optimistic discretion by managers is often the catalyst for future intentional misreporting (Schrand and Zechman 2012).

I use the incidence of an accounting restatement identified in the AuditAnalytics database as my measure of misreporting. I code the variable *Restatement* as an indicator equal to one for fiscal years in which managers initially provide information that is subsequently restated. Prior research suggests that a substantial number of restatements are due to unintentional error rather than the result of intentional misrepresentation (Hennes, Leone and Miller 2008). I include these restatements in my sample for two main reasons. First, if managers exercise more care in the preparation of financial reports when regulatory oversight is more intense then they should be less likely to make unintentional errors as well as provide intentional misrepresentations. Second, a substantial portion of restatements due to fraud are initiated by regulatory action. To the extent that the ability of the SEC to detect fraud is a function of its resource constraints, limiting the sample to these restatements magnifies any selection bias inherent in the restatement sample. To mitigate any selection bias that results because of the magnitude of SEC resources, I remove restatements that are initiated by SEC action.

Finally, I measure information asymmetry with *Spread*, which I calculate as the average effective bid-ask spread scaled by trade price using data from CRSP. Theory suggests that information asymmetry is directly manifest in bid-ask spreads (Glosten and Milgrom 1985).

And, prior empirical studies frequently use bid-ask spreads to proxy for information asymmetry (Gow, Taylor and Verrecchia 2012; Leuz and Verrecchia 2000).

I use three partitioning variables to measure heterogeneity in the effects of SEC oversight. The first partition I consider is firm-level political activity, *PolActive*, which is an indicator variable set to one for firm years where a firm's political action committee has made any donation to a candidate for Congress who is a member of a committee that oversees the SEC, or a member of the appropriations subcommittee that provides the SEC with its annual budgetary resources. The second partition I consider is an indicator for fiscal years following the bankruptcy of Lehman Brothers, which occurred in September 2008. The third partition I consider is litigation activity. Similar to Jennings et al. (2011) I calculate my proxy for private litigation, *PeerSuit* as an indicator variable equal to one for years in which a class-action lawsuit is filed against a peer firm in the firm's same 4 digit SIC grouping. In addition, I calculate several control variables that prior literature has shown to be associated with each of the three outcomes I test. The details of each of these variables are provided in Appendix A.

5. Results

5.1 Determinants of SEC Oversight Intensity

Table 3 presents the results from estimating the relation between budget allocations among disclosure review offices and their hypothesized determinants. I estimate a series of regressions using OLS in levels with office and year fixed effects and in first-differences with year fixed effects. To mitigate concerns about both cross-sectional and serial correlation, I calculate bootstrapped standard errors clustered by office and year[9]. Consistent with my

[9] I use bootstrapped standard errors clustered by office and U.S. fiscal year because the variable of interest is measured along these dimensions. The small number of groups potentially leads to over-rejection of the null hypothesis. Nonetheless, Gow, Ormazabal, and Taylor (2010) show that two-way clustered standard errors still outperform alternative procedures to control for cross-sectional and time-series dependence in the error terms.

prediction, I find a statistically significant negative relation between industry-level political activity and the office-level budget allocation in every specification that I estimate. The effects are economically meaningful; a one percent increase in industry-level political contributions is associated with a 0.14 to 0.31 percent decrease in the office-level budget allocation. I also find a significant positive relation between visibility resulting from the Lehman Brothers bankruptcy and budget allocations in all specifications.

In the levels specifications, I find a significant positive relation between the aggregate number of restatements in an industry and its office-level budget allocation. A one percent increase in restatements is associated with a 0.06 to 0.08 increase in an office's budget allocation. I fail to find a significant relation between the budget allocation and my two proxies for workload, *OfficeIPOs* and *OfficeCap*. When I estimate the relation between budget allocations and hypothesized determinants in first differences, I fail to find a statistically significant relation for restatements and industry market capitalization. However, I do find a significant relation for *OfficeIPOs*, which suggests that the budgets do increase with increases in workload.

To the extent that I have correctly specified determinants of SEC oversight intensity, my results suggest that SEC oversight is highly responsive to political costs.[10] I am unable, however, to rule out an alternative hypothesis. Namely, it is possible that politically active firms are more likely to comply with reporting disclosure regulations because of oversight by regulatory

---

Moreover, Cameron et al. (2008) show that calculating clustered standard errors using the bootstrap procedure generates rejection rates close to the hypothetical value.

[10] A concern about the levels specification is that the SEC uses the prior years' budget as the basis for developing the current years' budget, which could lead to autocorrelation in the residuals and be an important omitted variable. In untabulated robustness tests, I estimate a dynamic model that includes the lagged budget using the GMM procedure suggested by Arellano and Bond (1991). The results for political activity, restatements, and visibility following Lehman remain significant in these tests. However, the GMM procedure relies on asymptotic assumptions and may produce inconsistent estimates in small samples.

institutions other than the SEC. If this is the case, then the lower allocation of budgetary resources to offices that review filings in politically active industries may be a rational response to lower demand for SEC oversight.

5.2 SEC Oversight and Reporting Outcomes

I estimate a series of linear models to examine the effects of SEC oversight on three measures of reporting outcomes: discretionary accruals, restatements, and bid-ask-spreads. Variation in the intensity of SEC oversight in the proxy I use is the result of changes in office-level budgets from year to year. I estimate the model using office fixed effects to control for any time invariant office characteristics. In addition, I estimate the linear models using firm fixed effects to control for time invariant firm characteristics. Assignment to disclosure review offices is based on firms' four digit SIC code, which changes for some firms in my sample. Therefore, I estimate a version of the models containing both firm and office fixed effects to capture changes in oversight intensity for these firms. The use of year fixed effects mitigates concerns about omitted macroeconomic variables that may be correlated with the intensity of SEC oversight and financial reporting outcomes as well as removes any common time trends. Nonetheless, the use of year fixed effects removes a substantial (and the most visible) source of variation in SEC oversight—that resulting from changes in the overall SEC disclosure review budget. Because of this, my tests are likely to produce conservative estimates of the effect of SEC oversight.

Estimates from regressions of managers' financial reporting discretion as a function of SEC oversight and controls are provided in Table 4. I find evidence of a statistically significant and economically meaningful relation between SEC oversight and the magnitude of discretionary accruals across all specifications that I estimate (t-statistics between -1.99 and -4.44). The results suggest that a one standard deviation increase in SEC oversight is associated

with a decrease in unsigned discretionary accruals equal to 0.2 to 0.34 percent of total assets for the mean firm. This is a considerable effect, given that the average firm in the sample has an ROA of 3.4 percent. The evidence is consistent with managers choosing to exercise less discretion in their financial reporting when the intensity of SEC oversight is higher.

I present the results from estimating a linear probability model of the likelihood of an accounting restatement as a function of SEC oversight in Table 5.[11] Once again, I find a consistently negative relation between the intensity of SEC oversight and the likelihood that managers will provide accounting information that is eventually restated. The results are statistically and economically significant. In the linear probability model I use scaled decile ranks to measure all of the dependent variables, which allows me to interpret the coefficients as the change in probability of a restatement that results when moving from the bottom to the top decile of each variable. I find that, for the mean firm, moving from the bottom to top decile of SEC oversight results in a 2.4 to 3.3 percent reduction in the probability that a firm provides accounting information that is eventually restated. Given that the unconditional probability of a restatement is 4.1 percent, the results suggest that the SEC's filing review process has a highly economically significant effect on reducing the incidence of restatements.

The results from estimating regressions of bid-ask spreads as a function of SEC oversight are presented in Table 6. The use of a log-log specification allows me to interpret the coefficients as elasticity of spreads with respect to SEC oversight. I find statistically significant relations in the majority of models that I estimate, and a negative relation between bid-ask spreads and the

[11] I use a linear probability model for three reasons. First, the use of fixed effects estimators in nonlinear models such as logit and probit specifications produces biased estimates, particularly when the number of years in a panel is small (Greene 2004). Second, the use of firm fixed effects with non-linear models severely limits my analysis because of the rarity of accounting restatements. Finally, I use interaction terms in my tests for heterogeneous treatment effects. Ai and Norton (2003) argue that it is often infeasible to calculate marginal effects from interaction terms in probit and logit models. These problems are relatively straightforward to address in a linear probability model.

intensity of SEC oversight in all of the models. A one percent increase in SEC oversight is associated with a 0.28 to 0.31 percent decrease in firms' bid-ask spreads. The results suggest that large, sustained increases in SEC oversight are likely to yield substantial liquidity benefits.

In the aggregate, the results from my estimates of the main effects of SEC oversight on managers' reporting outcomes suggest that SEC oversight has a small, but meaningful, effect on managers reporting decisions.

5.3 Heterogeneity in the Effects of SEC Oversight

I conduct partitioned tests using of three sources of potential heterogeneity in the effects of SEC oversight on managers' reporting and disclosure choices. In the first set of tests, I partition the sample into politically active versus non-politically active firms. The results are presented in Panel A of Table 7. I fail to find a statistically significant coefficient on the interaction between SEC oversight and the partitioning variable for political activity for discretionary accruals and bid-ask spreads. However, I do find a significant positive coefficient on the interaction between SEC oversight and the indicator for politically active firms. For politically active firms, variation in SEC oversight appears to have no effect on the probability of a restatement, which is consistent with the hypothesis that these managers will be less responsive to regulatory oversight because it is costly for regulators to pursue enforcement actions against them. Overall, I find mixed evidence that the effects of SEC oversight vary with firm-level political activity.

I next use the bankruptcy of Lehman Brothers as an unexpected shock to political visibility. I interact an indicator variable for periods following the Lehman Brothers bankruptcy with my proxy for the intensity of SEC oversight. Panel B of Table 7 presents the results of my analysis. I find that following the collapse of Lehman brothers the intensity of SEC oversight has

a significantly stronger effect on the likelihood of an accounting restatement and bid-ask spreads. I do not find evidence of a change in the general effect of SEC oversight and discretionary accruals in the post Lehman period. While the evidence is mixed, my findings generally suggest that a shift in political power from firms to investors is followed by increased responsiveness of managers' reporting and disclosure incentives to changes in SEC oversight.

Finally, I test whether SEC oversight has an incremental effect beyond private litigation. I follow Jennings et al. (2011) and measure private litigation activity with an indicator variable, *PeerSuits*, equal to one for years in which a firm in the same four digit SIC code was subject to class action litigation in the prior year. I find a positive coefficient on the interaction between *SEC_Oversight* and *PeerSuits* for tests using all three dependent variables. However, the coefficient is only statistically significant in the tests using bid-ask spreads. My failure to find significant relation between private litigation and measures of accounting quality may be because I use an output-based measure of litigation activity. Therefore, my measure of litigation activity may not appropriately capture litigation's effects on managers' ex ante reporting incentives. Nonetheless, the results are consistent with the notion that SEC oversight and private litigation are substitutive governance mechanisms.

5.4 Robustness Tests

I conduct two falsification tests as robustness checks to further validate my empirical findings. First, I conduct my main tests using the prior years' proxy for the intensity of SEC oversight as a falsification test to mitigate concerns that my findings may be driven by industry-level trends. Panel A of Table 8 presents the results when lagged *SEC_Oversight* is used at the treatment variable. I fail to find evidence of a significant relation between SEC oversight and reporting outcomes. This suggests that proximate changes in the intensity of SEC oversight drive

the results I find rather than office-level trends. Second, I conduct a placebo test by randomly assigning firms to a disclosure review office and substituting the randomly assigned level of *SEC_Oversight* for the actual level. The results of the placebo test are presented in Panel B of Table 8. I fail to find any significant relation between the placebo and firms' reporting outcomes. These tests further validate my findings that variation in the intensity of oversight at the SEC-office level affects managers' reporting incentives.

6. Conclusion

This study provides insights into forces that shape the SEC's oversight of corporate disclosures and provides plausibly causal evidence that SEC oversight affects managers' reporting and disclosure incentives. Consistent with extant theories of regulatory behavior, I find evidence that the intensity of SEC oversight is associated with industry-level political activity and visibility. However, I find mixed results when I test whether political activity at the firm level reduces the effect of SEC oversight on managers' behavior. My findings suggest that previously documented results regarding the relationship between firms' political activity and misreporting may result because the SEC devotes fewer resources to monitoring these firms' disclosure filings. Overall, the results suggest that managers exercise less financial reporting discretion and are less likely to misreport financial information when SEC oversight is more intense. SEC oversight also has some beneficial capital market consequences, as bid-ask spreads are lower when oversight is more intense.

The variation in the intensity of oversight in my sample is limited. Yet, even after controlling for inter-temporal variation in oversight intensity I find evidence that oversight affects managers' behavior. This suggests that SEC oversight has a first order effect on managers' reporting and disclosure incentives. Nonetheless, policy makers should exercise

caution before drawing any conclusions from my results. I find limited evidence of a substitutive relationship between SEC oversight and monitoring via private litigation, which suggests that private monitoring mechanisms may meet market demand for oversight of financial reporting and disclosures. In addition, I only examine potential benefits of SEC oversight. It is not clear whether the observed benefits of more intense SEC oversight outweigh their costs. Regardless of the desirability of SEC oversight, my results suggest that the SEC does play an important role in shaping managers' incentives to provide information to capital markets.

Bibliography

Ai, C., and E. Norton, 2003. Interaction terms in logit and probit models, *Economics Letters* 80, 123-129.

Ansolabehere, S., J. deFigueiredo, and J. Snyder, 2003. Why is There So Little Money in U.S. Politics?, *Journal of Economic Perspectives* 17, 105-130.

Armstrong, C., D. Larcker, G. Ormazabal, and D. Taylor, 2013. The Relation Between Equity Incentives and Misreporting: The Role of Risk-Taking Incentives, *Journal of Financial Economics* 109.

Ball, R., S. P. Kothari, and A. Robin, 2000. The effect of international institutional factors on properties of accounting earnings, *Journal of Accounting and Economics* 29, 1-51.

Ball, R., and L. Shivakumar, 2005. Earnings quality in UK private firms: Comparative loss recognition timeliness, *Journal of Accounting and Economics* 39, 83-128.

Bealing, W., 1994. Actions Speak Louder than Words: An Institutional Perspective on the Securities and Exchange Commission, *Accounting Organizations and Society* 19, 555-567.

Beatty, A., and J. Weber, 2003. The effects of debt contracting on voluntary accounting method changes, *The Accounting Review* 78, 119-142.

Becker, G., 1968. Crime and Punishment: An Economic Approach, *Journal of Political Economy* 76, 169-217.

Becker, G., 1983. A Theory of Competition Among Pressure Groups for Political Influence, *Quarterly Journal of Economics* 98, 371-400.

Benston, G., 1969. The Value of the SEC's Accounting Disclosure Requirements, *The Accounting Review* 44, 515-532.

Bhattacharya, U., and H. Daouk, 2002. The World Price of Insider Trading, *Journal of Finance* 57, 75-108.

Burgstahler, D., L. Hail, and C. Leuz, 2006. The Importance of Reporting Incentives: Earnings Management in European Private and Public Firms, *The Accounting Review* 81, 983-1016.

Bushee, B., and C. Leuz, 2005. Economic Consequences of SEC Disclosure Regulation: Evidence from the OTC Bulletin Board, *Journal of Accounting and Economics* 39, 233-264.

Bushman, R., Q. Chen, E. Engel, and A. Smith, 2004. Financial accounting information, organizational complexity, and corporate governance systems, *Journal of Accounting and Economics* 37, 167-201.

Cameron, C., J. Gelbach, and D. Miller, 2008. Bootstrap-Based Improvements for Inference with Clustered Errors, *Review of Economics and Statistics* 90, 414-427.

Cassell, C., L. Dreher, and L. Meyers, 2013. Reviewing the SEC's Review Process: 10-K Comment Letters and the Cost of Remediation, *Accounting Review* Forthcoming.

Christensen, H., L. Hail, and C. Leuz, 2011. Capital-Market Effects of Securities Regulation: Hysteresis, Implementation, and Enforcement, *Working paper*.

Christensen, H., L. Hail, and C. Leuz, 2013. Mandatory IFRS Reporting and Changes in Enforcement, *Working paper*.

Coffee, J., 2007. Law and the Market: The Impact of Enforcement, *University of Pennsylvania Law Review* 156, 229-311.

Correia, M., 2009. Political Connections, SEC Enforcement and Accounting Quality, *Working paper*.

Cox, J., R. Thomas, and D. Kiku, 2003. SEC Enforcement Heuristics: An Empirical Inquiry, *Duke Law Journal* 53, 737-779.

Dechow, P., W. Ge, and C. Schrand, 2010. Understanding Earnings Quality: A Review of the Proxies, Their Determinants and Their Consequences, *Journal of Accounting and Economics* 50, 344-401.

DeHaan, E., S. Kedia, K. Koh, and S. Rajgopal, 2012. Does the Revolving Door Affect the SEC's Enforcement Outcomes?, *Working paper*.

Del Guercio, D., E. Odders-White, and M. Ready, 2013. The Deterrence Effect of SEC Enforcement Intensity on Illegal Insider Trading, *Working paper*.

Diamond, D., and R. Verrecchia, 1991. Disclosure, Liquidity, and the Cost of Capital, *The Journal of Finance* 46, 1325-1359.

Dyck, A., A. Morse, and L. Zingales, 2010. Who Blows the Whistle on Corporate Fraud?, *Journal of Finance* 65, 2213-2253.

Dyck, A., A. Morse, and L. Zingales, 2013. How Pervasive is Corporate Fraud?, *Working paper*.

Feroz, E., K. Park, and V. Pastena, 1991. The Financial and Market Effects of the SEC's Accounting and Auditing Enforcement Releases, *Journal of Accounting Research* 29, 107-142.

Fischer, P., and R. Verrecchia, 2000. Reporting Bias, *The Accounting Review* 75, 229-245.

GAO, 2002. *SEC Operations: Increased Workload Creates Challenges*.

GAO, 2013. *Securities and Exchange Commission: Continued Management Attention Would Strengthen Internal Supervisory Controls*(Washington, DC).

Glosten, L., and P. Milgrom, 1985. Bid, Ask, and Transaction Prices in a Specialist Market with Heterogeneously Informed Traders, *Journal of Financial Economics* 14, 71-100.

Gordon, S., and C. Hafer, 2005. Flexing Muscle: Corporate Political Expenditures as Signals to the Bureaucracy, *American Political Science Review* 99, 245-261.

Gow, I., G. Ormazabal, and D. Taylor, 2010. Correcting for Cross-Sectional and Time-Series Dependence in Accounting Research, *The Accounting Review* 85, 483-512.

Gow, I., D. Taylor, and R. Verrecchia, 2012. On the Complementary Relation between Earnings and Private Information, *Working paper*.

Greene, W., 2004. The Bias of the Fixed Effects Estimator in Nonlinear Models, *The Econometrics Journal* 7, 98-119.

Guay, W., S. P. Kothari, and R. Watts, 1996. A Market-Based Evaluation of Discretionary-Accruals Models, *Journal of Accounting Research* 34, 83-105.

Hanlon, M., J. Hoopes, and N. Shroff, 2012. The Effect of Tax Authority Monitoring and Enforcement on Financial Reporting Quality, *Working paper*.

Healy, P., and J. Wahlen, 1999. A Review of the Earnings Management Literature and Its Implications for Standard Setting, *Accounting Horizons* 13, 365-383.

Hennes, K., A. Leone, and B. Miller, 2008. The Importance of Distinguishing Errors from Irregularities in Restatement Research: The Case of Restatements and CEO/CFO Turnover, *The Accounting Review* 83, 1487-1519.

Jackson, H., and M. Roe, 2009. Public and Private Enforcement of Securities Laws: Resource-Based Evidence, *Journal of Financial Economics* 93, 207-238.

Jennings, J., S. Kedia, and S. Rajgopal, 2011. The Deterrent Effects of SEC Enforcement and Class Action Litigation, *Working paper*.

Johnston, R., and R. Petacchi, 2013. Regulatory Oversight of Financial Reporting: Securities and Exchange Commission Comment Letters, *Working paper*.

Kedia, S., and S. Rajgopal, 2011. Do the SEC's Enforcement Preferences Affect Corporate Misconduct?, *Journal of Accounting and Economics* 51, 259-278.

Laffont, J., and J. Tirole, 1991. The Politics of Government Decision-Making: A Theory of Regulatory Capture, *Quarterly Journal of Economics* 106, 1089-1127.

Leuz, C., and R. Verrecchia, 2000. The Economic Consequences of Increased Disclosure, *Journal of Accounting Research* 38, 91-124.

Lewis, C., 2012. https://www.sec.gov/News/Speech/Detail/Speech/1365171491988#.UpLkQOJwh-h.

Mueller, D., 2003. *Public Choice III*(Cambridge University Press, Cambridge).

Peltzman, S., 1976. Toward a More General Theory of Regulation, *Journal of Law and Economics* 19, 211-240.

Pincus, K., W. Holder, and T. Mock, 1988. *Reducing The Incidence of Fraudulant Financial Reporting: The Role of the Securities and Exchange Commission*(SEC and Financial Reporting Institute, Los Angeles).

Robinson, J., Y. Xue, and Y. Yu, 2011. Determinants of Disclosure Noncompliance and the Effect of the SEC Review: Evidence from the 2006 Mandated Compensation Disclosure Regulations, *The Accounting Review* 86, 1415-1444.

Schrand, C., and S. Zechman, 2012. Executive Overconfidence and the Slippery Slope to Financial Misreporting, *Journal of Accounting and Economics* 53, 311-329.

Seligman, J., 2003. *The Transformation of Wall Street: A History of the Securities and Exchange Commission and Modern Corporate Finance*, 3rd edition.(Aspen Publishers).

Shleifer, A., 2005. Understanding Regulation, *European Financial Management* 11, 439-451.

Skinner, D., 1994. Why firms voluntarily disclose bad news, *Journal of Accounting Research* 32, 38-60.

Skinner, D., 1997. Earnings Disclosures and Stockholder Lawsuits, *Journal of Accounting and Economics* 23, 249-282.

Stigler, G., 1964. Public Regulation of the Securities Markets, *The Business Lawyer* 19, 721-753.

Stigler, G., 1970. The Optimum Enforcement of Laws, *Journal of Political Economy* 78, 526-536.

Stigler, G., 1971. The Theory of Economic Regulation, *The Bell Journal of Economics and Management Science* 2, 3-21.

Watts, R., and J. Zimmerman, 1978. Towards a positive theory of the determination of accounting standards, *The Accounting Review* 53, 112-134.

Watts, R., and J. Zimmerman, 1986. *Positive Accounting Theory*(Prentice Hall, Upper Saddle River, NJ).

Weingast, B., 1984. The Congressional-Bureaucratic System: A Principal Agent Perspective (with Applications to the SEC), *Public Choice* 44, 147-191.

Weingast, B., and M. Moran, 1983. Bureaucratic Discretion or Congressional Control? Regulatory Policymaking by the Federal Trade Commission, *Journal of Political Economy* 91, 765-800.

Yu, F., and X. Yu, 2011. Corporate Lobbying and Fraud Detection, *Journal of Financial and Quantitative Analysis* 46, 1865-1891.

APPENDIX A: VARIABLE DEFINITIONS

Disclosure Review Office- and Industry-Level Variables

$Dollars_t$	The estimated budget allocation in constant 2010 year dollars for the Division of Corporation Finance disclosure review office for the fiscal year.
$Workload_t$	The number of reporting firms filing a 10-K report for the Division of Corporation Finance disclosure review office for the fiscal year.
$SEC\_Oversight_t$	The natural log of *Dollars/NoRptFirms*.
$Positions_t$	The number of staff in the Division of Corporation Finance disclosure review office during fiscal year.
$IndPoliticalActivity_{t-1}$	The natural log of one plus the number of contributions to congressional campaigns made by corporate political action committees of firms assigned to the disclosure review office during the year.
$OfficeRestatements_{t-1}$	The natural log of the number of the percent of firms covered by a disclosure review office filing restatements during the fiscal year.
$Lehman_t$	An indicator variable equal to one for the financial services industry in years following the bankruptcy of Lehman Brothers.
$OfficeIPOs_{t-1}$	The natural log of 1 plus the number of initial public offerings for a given disclosure review office during the year.
$OfficeCap_{t-1}$	The natural log of the aggregate market capitalization of all firms in a given disclosure review office during the year.

Measures of Reporting and Disclosure Outcomes

$AbsDA\_MJ_{t+1}$	The absolute value of discretionary accruals scaled by total assets, calculated as residuals from an estimate of the modified Jones (1991) model of accruals calculated for each two digit SIC code and fiscal year.
$Restatement_{t+1}$	An indicator variable equal to one for years when the firm begins reporting accounting information that is later restated.
$Spreads_{t+1}$	The natural log of the bid-ask spread scaled by price.

Firm Controls

$Size_t$	The natural log of the firm's market value of equity at the end of the fiscal year.
BM_t	The book value of equity scaled by market value of equity at the end of fiscal year.
$Leverage_t$	Total liabilities divided by total assets.
$FirmAge_t$	The number of years that the firm appears on Compustat.
ROA_t	Net income scaled by total assets.

Ret_t	Buy-and-hold returns over the fiscal year.
$1/Price_t$	The inverse of the firm's stock price at the end of fiscal year.
$Turnover_t$	The average monthly share volume scaled by shares outstanding over the fiscal year.
$Beta_t$	The slope coefficient estimated from a market model of monthly security returns over the fiscal year.
$Volatility_t$	The standard deviation of monthly returns over the fiscal year.
$PolActive_t$	An indicator variable equal to one for firm years in which a firm's political action committee makes a contribution to a congressional campaign.
$PeerSuit_{t-1}$	An indicator variable equal to one for firm years in which class-action is filed against peer firms in the same 4 digit SIC industry grouping.

Figure 1
Timeline of the Budget Process

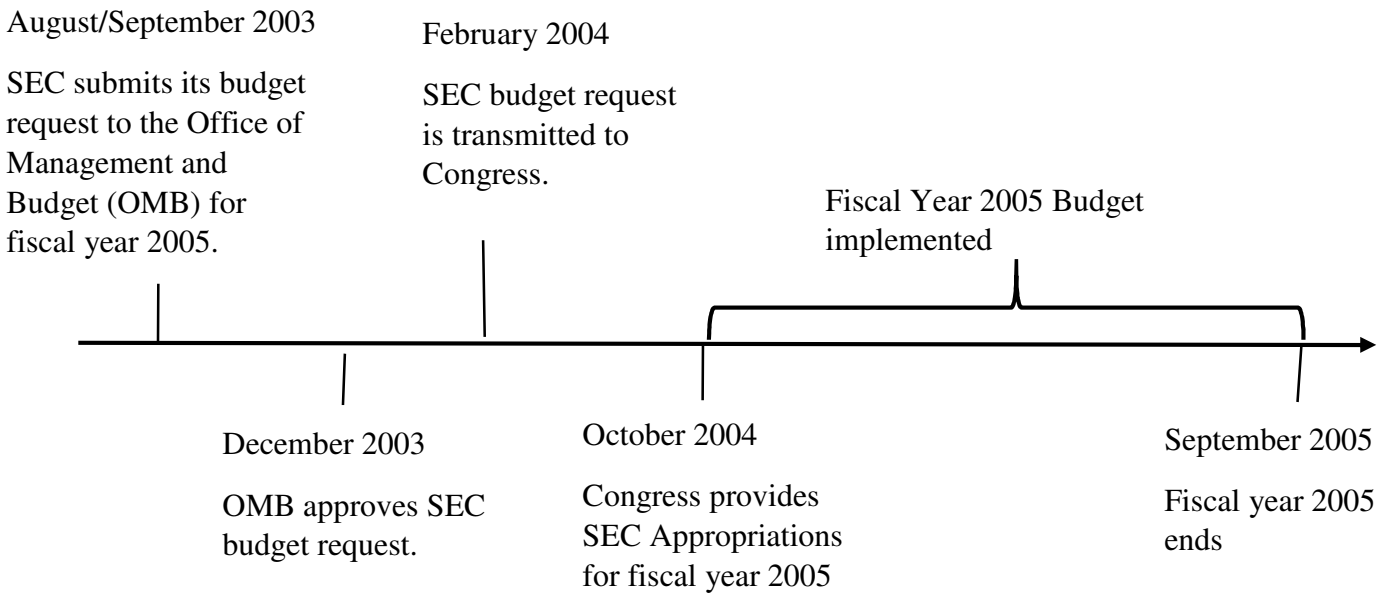

Table 1
Office-Level Resource Allocations

This table presents descriptive statistics for the disclosure review offices in the SEC's Division of Corporation Finance from fiscal years 2003-2012. Panel A reports resource and workload allocations for each of the 11 disclosure review offices across the entire sample period. Panel B reports the resource and workload allocations across all offices for each fiscal year. All variables are as defined in Appendix A.

Panel A: Summary Statistics by Office

Office	Primary Industry	Statistic	Dollars	Positions	Workload
1	Healthcare and Insurance	Mean	8389.5	32.0	828.2
		Std	*1411.6*	*3.8*	*115.7*
2	Consumer Products	Mean	8766.8	33.8	641.8
		Std	*1477.3*	*4.5*	*55.3*
3	Information Technologies and Services	Mean	8731.0	33.2	1067.4
		Std	*1571.8*	*3.7*	*215.3*
4	Natural Resources	Mean	9639.7	36.5	599.7
		Std	*2074.0*	*4.1*	*31.1*
5	Transportation and Leisure	Mean	9207.6	35.1	835.4
		Std	*1975.3*	*5.0*	*100.4*
6	Manufacturing and Construction	Mean	9230.0	35.1	842.2
		Std	*1731.1*	*4.3*	*67.1*
7	Financial Services	Mean	9676.5	35.5	1082.0
		Std	*2610.8*	*5.6*	*172.1*
8	Real Estate and Commodities	Mean	8229.9	31.7	833.0
		Std	*1298.1*	*4.2*	*68.6*
9	Beverages, Apparel, and Mining	Mean	7464.5	28.4	624.0
		Std	*1103.7*	*2.4*	*75.1*
10	Electronics and Machinery	Mean	8692.8	32.9	836.0
		Std	*1785.1*	*4.3*	*53.3*
11	Telecommunications	Mean	8925.4	34.0	720.9
		Std	*1640.8*	*4.9*	*73.1*

Table 1
Office-Level Resource Allocations

Panel B: Summary Statistics by Fiscal Year

Fiscal Year	Statistic	Dollars	Positions	Workload
2003	Mean	6280.07	25.72	910.4
	Std	*452.31*	*1.95*	*280.2*
2004	Mean	5932	36.27	871.5
	Std	*394.16*	*2.49*	*251.6*
2005	Mean	9896.61	38.27	859.9
	Std	*774.6*	*3*	*227.9*
2006	Mean	10135.73	34.72	832.1
	Std	*1065.64*	*3.93*	*192.1*
2007	Mean	8371.95	33.55	834.6
	Std	*786.76*	*3.36*	*165.6*
2008	Mean	8696.48	34.72	835.9
	Std	*557.65*	*2.15*	*149.7*
2009	Mean	8926.36	32.72	797.5
	Std	*904.18*	*3.26*	*120.8*
2010	Mean	10144.53	34.45	745.9
	Std	*1000.34*	*3.72*	*99.8*
2011	Mean	10328.1	33.09	721.3
	Std	*1480.62*	*4.76*	*95.6*
2012	Mean	9427.86	31.18	691.9
	Std	*1630.33*	*5.1*	*96.9*

Table 2
Summary Statistics

This table presents summary statistics for the data used to analyze the determinants of SEC monitoring intensity. Panel A contains aggregate industry-level statistics for the firms reviewed by the 11 SEC disclosure review offices between fiscal years 2003 and 2012 for a total of 110 office-years. The remaining panels contains summary statistics at the firm-level for the time period between fiscal years 2003 and 2011, and cover a total of 22,434 firm years. Panel B reports descriptive statistics for the measures of information quality. Panel C reports summary statistics for selected firm characteristics. All variables are defined as in Appendix A.

Panel A: Office-Level Measures

	Mean	*Std*	*25th*	*Median*	*75th*
Ln(Dollars)	9.063	0.212	8.976	9.102	9.210
Workload	810.10	184.78	668.00	795.50	890.00
SEC_Oversight	2.390	0.322	2.185	2.442	2.634
IndPoliticalActivity	8.017	0.538	7.708	8.095	8.397
OfficeRestatements	3.285	0.515	2.996	3.314	3.689
Lehman	0.036	0.188	0.000	0.000	0.000
OfficeIPOs	2.238	0.847	1.609	2.398	2.773
OfficeCap	13.918	0.532	13.624	14.031	14.296
SupervisorTenure	8.264	5.421	5.000	8.000	11.000
SECTurnover	14.223	6.798	10.000	13.043	18.421

Panel B: Measures of Information Quality

	N	*Mean*	*Std*	*25th*	*Median*	*75th*
Spreads	22434	0.005	0.012	0.001	0.001	0.003
Discretion	22434	5.431	6.952	1.536	3.484	6.777
Restatements	22434	0.041	0.198	0.000	0.000	0.000

Table 2
Summary Statistics

Panel C: Firm Characteristics

	N	*Mean*	*Std*	*25th*	*Median*	*75th*
Size	22434	6.712	1.727	5.531	6.588	7.792
BM	22434	0.527	0.342	0.279	0.456	0.694
Leverage	22434	0.475	0.221	0.297	0.482	0.640
FirmAge	22434	22.442	15.683	10.000	17.000	32.000
ROA	22434	0.034	0.112	0.012	0.044	0.085
Ret	22434	0.136	0.537	-0.190	0.082	0.371
1/Price	22434	0.064	0.046	0.029	0.048	0.088
Turnover	22434	1.940	1.615	0.830	1.535	2.521
Volatililty	22434	0.113	0.060	0.071	0.101	0.141
Beta	22434	1.261	1.190	0.530	1.128	1.857
PolActive	22434	0.159	0.366	0.000	0.000	0.000
PostLehman	22434	0.216	0.411	0.000	0.000	0.000
PeerSuit	22434	0.331	0.471	0.000	0.000	1.000

Table 3
Determinants of SEC Oversight Intensity

This table presents the results from an OLS regression of the natural log of office-level budget allocations as a function of hypothesized determinants of SEC oversight intensity. Columns (1) - (3) present the results using a levels specification, while columns (4) - (6) present the results from estimating the regression in first-differences. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using bootstrapped standard errors clustered by office and fiscal year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	Ln(Dollars$_t$)			*ΔLn(Dollars$_t$)*		
	(1)	(2)	(3)	(4)	(5)	(6)
IndPoliticalActivity$_{t-1}$	-0.205**	-0.137**	-0.119**	-0.306***	-0.293***	-0.291***
	(-2.35)	(-2.30)	(-2.12)	(-3.20)	(-2.99)	(-3.06)
OfficeRestatements$_{t-1}$	0.091***		0.062*	0.026		0.035
	(4.25)		(1.80)	(0.93)		(1.44)
Lehman$_{t-1}$		0.287**	0.252*		0.204**	0.216**
		(2.09)	(1.91)		(2.11)	(2.11)
OfficeIPOs$_{t-1}$	0.001	0.002	0.004	0.021*	0.021***	0.019**
	(0.11)	(0.56)	(0.52)	(1.81)	(3.10)	(2.01)
OfficeCap$_{t-1}$	-0.025	0.017	0.007	-0.042	-0.041	-0.051
	(-0.56)	(0.27)	(0.11)	(-0.52)	(-0.52)	(-0.71)
Office Effects	Yes	Yes	Yes	No	No	No
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	110	110	110	99	99	99
R^2	0.910	0.908	0.912	0.867	0.875	0.877

Table 4
SEC Oversight and Financial Reporting Discretion

This table presents the results from an OLS regression of discretionary accruals as a function of SEC oversight intensity and control variables. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using bootstrapped standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	AbsDA_MJ$_t$		
	(1)	(2)	(3)
SEC_Oversight$_t$	-1.084***	-0.645**	-0.883***
	(-4.44)	(-1.99)	(-4.19)
Size$_{t-1}$	-0.591***	-0.398	-0.396**
	(-9.38)	(-1.60)	(-2.31)
BM$_{t-1}$	-1.359**	0.769	0.788
	(-2.30)	(1.17)	(1.09)
Leverage$_{t-1}$	-1.963***	-0.025	-0.025
	(-3.33)	(-0.05)	(-0.03)
FirmAge$_{t-1}$	-0.016***	0.027	0.011
	(-3.92)	(0.39)	(0.13)
ROA$_{t-1}$	-7.522***	-3.199	-3.208*
	(-6.55)	(-1.49)	(-1.81)
Ret$_{t-1}$	-0.901***	-0.682***	-0.682***
	(-4.29)	(-6.12)	(-6.82)
IndPoliticalActivity$_{t-1}$	1.055**	0.151	0.636
	(2.34)	(0.30)	(1.28)
OfficeRestatements$_{t-1}$	0.116	0.051	0.057
	(0.51)	(0.13)	(0.14)
OfficeIPOS$_{t-1}$	-0.297**	-0.407***	-0.394**
	(-2.24)	(-2.94)	(-2.27)
OfficeCap$_{t-1}$	0.079	0.079	0.112
	(0.41)	(0.28)	-0.32
Year Effects	Yes	Yes	Yes
Office Effects	Yes	No	Yes
Firm Effects	No	Yes	Yes
Observations	22,434	22,434	22,435
R^2	0.082	0.452	0.452

Table 5
SEC Oversight and Misreporting

This table presents the results from estimating a linear model over fiscal years 2003 to 2011 of the likelihood of an accounting restatement as a function of SEC oversight and scaled decile ranks of control variables. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using bootstrapped standard errors clustered by SEC disclosure review office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	Restatement$_t$		
	(1)	(2)	(3)
SEC_Oversight$_t$	-0.024*	-0.033**	-0.028**
	(-1.94)	(-2.03)	(-2.14)
Size$_{t-1}$	-0.008	0.070**	0.071***
	(-1.62)	(2.24)	(2.87)
BM$_{t-1}$	0.016***	0.034***	0.034***
	(2.92)	(3.67)	(4.07)
Leverage$_{t-1}$	0.013***	0.015	0.015
	(2.85)	(1.41)	(1.42)
FirmAge$_{t-1}$	-0.008	-0.074***	-0.068***
	(-1.46)	(-3.09)	(-2.82)
ROA$_{t-1}$	-0.019***	0.017*	0.017**
	(-2.65)	(1.93)	(2.10)
Ret$_{t-1}$	-0.011**	0.008	0.008
	(-2.01)	(1.10)	(1.14)
IndPoliticalActivity$_{t-1}$	-0.018	-0.015	-0.030**
	(-1.53)	(-1.02)	(-2.17)
OfficeRestatements$_{t-1}$	0.012*	0.012**	0.012***
	(1.88)	(2.42)	(2.64)
OfficeIPOS$_{t-1}$	-0.000	0.000	0.001
	(-0.02)	(0.15)	(0.29)
OfficeCap$_{t-1}$	-0.004	0.000	0.002
	(-0.47)	(0.01)	(0.18)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	No	Yes
Firm Effects	No	Yes	Yes
Observations	22,434	22,434	22,434
R^2	0.009	0.224	0.225

Table 6
SEC Oversight and Information Asymmetry

This table presents the results from an OLS regression of the natural log of bid ask spreads as a function of SEC oversight intensity and control variables. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using bootstrapped standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	Ln(Spreads)$_t$		
	(1)	(2)	(3)
SEC_Oversight$_t$	-0.305***	-0.280***	-0.308***
	(-2.65)	(-2.65)	(-3.30)
Size$_{t-1}$	-0.413***	-0.208***	-0.208***
	(-18.55)	(-4.97)	(-5.44)
BM$_{t-1}$	0.004	0.039	0.039
	(0.09)	(0.82)	(0.78)
1/Price$_{t-1}$	3.243***	1.300**	1.298**
	(5.25)	(2.25)	(2.91)
Turnover$_{t-1}$	-0.157***	-0.103***	-0.103***
	(-9.47)	(-9.20)	(-9.35)
Volatility$_{t-1}$	2.321***	1.238***	1.239***
	(6.59)	(4.22)	(4.06)
Beta$_{t-1}$	-0.067***	-0.027***	-0.027***
	(-5.49)	(-4.25)	(-4.43)
IndPoliticalActivity$_{t-1}$	0.118	0.066	0.074
	(1.48)	(0.86)	(0.67)
OfficeRestatements$_{t-1}$	0.062	0.114***	0.115***
	(1.55)	(2.97)	(3.20)
OfficeIPOS$_{t-1}$	0.019	-0.010	-0.005
	(0.64)	(-0.44)	(-0.22)
OfficeCap$_{t-1}$	0.015	-0.028	-0.015
	(0.28)	(-0.53)	(-0.31)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	No	Yes
Firm Effects	No	Yes	Yes
Observations	22,434	22,434	22,434
R^2	0.725	0.902	0.902

Table 7
Heterogeneity in the Effect of SEC Oversight

This table presents evidence on cross-sectional variation in the relationship between SEC oversight and managers' financial reporting and disclosure behavior. Panel A presents evidence of variation in the effect for politically active firms. Panel B presents evidence of variation in the effect before and after the bankruptcy of Lehman Brothers in 2008. Panel C provides evidence on the interaction between the intensity of SEC oversight and the intensity of private litigation. In each panel, columns (1) and (3) are estimated using OLS and column (2) is estimated using a linear probability model with scaled decile ranks. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using bootstrapped standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 7
Heterogeneity in the Effect of SEC Oversight

Panel A: Politically Active Firms

	AbsDA_MJ$_t$ (1)	*Restatement*$_t$ (2)	*Ln(Spreads)*$_t$ (3)
SEC_Oversight$_t$	-0.825*	-0.028**	-0.281***
	(-1.74)	(-2.06)	(-2.67)
*PolActive*SEC_Oversight*$_t$	-0.329	0.026**	-0.144
	(-1.07)	(2.32)	(-1.53)
PolActive$_{t-1}$	1.117	-0.009	0.333
	(1.13)	(-0.73)	(1.51)
Size$_{t-1}$	-0.404*	0.070**	-0.208***
	(-1.72)	(2.41)	(-4.96)
BM$_{t-1}$	0.77	0.034***	0.039
	(1.10)	(2.78)	(0.73)
Leverage$_{t-1}$	-0.056	0.015	
	(-0.05)	(1.51)	
FirmAge$_{t-1}$	0.009	-0.063***	
	(0.10)	(-2.99)	
ROA$_{t-1}$	-3.200*	0.017**	
	(-1.77)	(1.71)	
Ret$_{t-1}$	-0.683***	0.008	
	(-5.27)	(0.87)	
1/Price$_{t-1}$			1.342**
			(2.52)
Turnover$_{t-1}$			-0.103***
			(-9.38)
Volatility$_{t-1}$			1.246***
			(4.10)
Beta$_{t-1}$			-0.027***
			(-5.70)
IndPoliticalActivity$_{t-1}$	0.626	-0.070***	0.09
	(1.04)	(-2.93)	(0.76)
OfficeRestatements$_{t-1}$	0.054	0.015**	0.113***
	(0.11)	(2.06)	(3.18)
OfficeIPOS$_{t-1}$	-0.394***	-0.001	-0.003
	(-2.66)	(-0.06)	(-0.11)
OfficeCap$_{t-1}$	0.113	-0.007	-0.016
	(0.28)	(-0.86)	(-0.34)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes
Observations	22,434	22,434	22,434
R^2	0.452	0.225	0.899

Table 7
Heterogeneity in the Effect of SEC Oversight

Panel B: Post Lehman Brothers Bankruptcy

	$AbsDA\_MJ_t$ (1)	$Restatement_t$ (2)	$Ln(Spreads)_t$ (3)
$SEC\_Oversight_t$	-0.854*	-0.014	-0.141***
	(-1.76)	(-1.10)	(-3.97)
$Lehman*SEC\_Oversight_t$	-0.124	-0.034**	-0.124**
	(-0.56)	(-2.42)	(-2.45)
$Size_{t-1}$	-0.393***	0.066*	-0.186***
	(-2.84)	(1.91)	(-4.82)
BM_{t-1}	0.785***	0.035*	0.050**
	(2.90)	(1.68)	(2.45)
$Leverage_{t-1}$	-0.026	0.004	
	(-0.05)	(0.18)	
$FirmAge_{t-1}$	0.047	-0.074*	
	(0.52)	(-1.75)	
ROA_{t-1}	-3.211***	0.026	
	(-5.01)	(1.62)	
Ret_{t-1}	-0.679***	0.006	
	(-6.31)	(0.42)	
$1/Price_{t-1}$			1.496***
			(3.31)
$Turnover_{t-1}$			-0.103***
			(-8.77)
$Volatility_{t-1}$			1.242***
			(4.20)
$Beta_{t-1}$			-0.026***
			(-4.47)
$IndPoliticalActivity_{t-1}$	0.638*	-0.073**	0.122
	(1.72)	(-2.17)	(1.45)
$OfficeRestatements_{t-1}$	0.062	0.014	0.057
	(0.29)	(0.79)	(1.63)
$OfficeIPOS_{t-1}$	-0.379***	0.015	0.020
	(-3.64)	(0.83)	(0.78)
$OfficeCap_{t-1}$	0.071	-0.035	-0.042
	(0.25)	(-1.55)	(-0.89)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes
Observations	22,434	22,434	22,434
R^2	0.452	0.225	0.900

Table 7
Heterogeneity in the Effect of SEC Oversight

Panel C: SEC Oversight and Litigation Activity

	AbsDA_MJ$_t$ (1)	*Restatement*$_t$ (2)	*Ln(Spreads)*$_t$ (3)
SEC_Oversight$_t$	-0.893*	-0.028**	-0.312***
	(-1.68)	(-2.34)	(-3.60)
*PeerSuits*SEC_Oversight*$_t$	0.052	0.007	0.087**
	(0.16)	(0.52)	(2.39)
PeerSuits$_{t-1}$	-0.010	-0.004	-0.218**
	(-0.05)	(-0.48)	(-2.22)
Size$_{t-1}$	-0.398**	0.058	-0.208***
	(-2.00)	(1.42)	(-5.31)
BM$_{t-1}$	0.786**	0.033	0.036
	(2.09)	(1.17)	(0.72)
Leverage$_{t-1}$	-0.028	0.004	
	(-0.04)	(0.15)	
FirmAge$_{t-1}$	0.011	-0.089**	
	(0.15)	(-2.11)	
ROA$_{t-1}$	-3.209**	0.025	
	(-2.49)	(1.26)	
Ret$_{t-1}$	-0.682***	0.006	
	(-4.43)	(0.40)	
1/Price$_{t-1}$			1.318**
			(2.51)
Turnover$_{t-1}$			-0.102***
			(-8.74)
Volatility$_{t-1}$			1.244***
			(3.98)
Beta$_{t-1}$			-0.027***
			(-4.37)
IndPoliticalActivity$_{t-1}$	0.632	-0.080**	0.059
	(1.48)	(-2.40)	(0.79)
OfficeRestatements$_{t-1}$	0.055	0.027	0.109***
	(0.22)	(1.51)	(3.35)
OfficeIPOS$_{t-1}$	-0.394***	0.006	-0.010
	(-4.27)	(0.31)	(-0.38)
OfficeCap$_{t-1}$	0.113	-0.032	-0.023
	(0.53)	(-1.53)	(-0.51)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes
Observations	22,434	22,434	22,434
R^2	0.452	0.225	0.900

Table 8
Robustness Tests

This table presents the results from two falsification tests. Panel A presents the results when the lagged value of *SEC_Oversight* is used in place of the current period. Panel B presents results from randomly assigning firms to an SEC disclosure review office. In each panel, columns (1) and (3) are estimated using OLS and column (2) is estimated using a linear probability model with scaled decile ranks. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using bootstrapped standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Panel A: Lagged Oversight

	$AbsDA\_MJ_t$	$Restatement_t$	$Ln(Spreads)_t$
	(1)	(2)	(3)
$SEC\_Oversight_{t-1}$	-1.080	0.014	-0.004
	(-1.13)	(0.76)	(-0.03)
$Size_{t-1}$	-0.483**	0.026	-0.178***
	(-2.09)	(1.18)	(-4.18)
BM_{t-1}	0.914	0.032**	0.031
	(1.10)	(2.03)	(0.49)
$Leverage_{t-1}$	-0.357	0.001	
	(-0.39)	(0.09)	
$FirmAge_{t-1}$	-0.041	-0.007	
	(-0.39)	(-0.19)	
ROA_{t-1}	-1.971	0.015	
	(-1.16)	(1.50)	
Ret_{t-1}	-0.763***	-0.003	
	(-4.99)	(-0.25)	
$1/Price_{t-1}$			1.565***
			(2.81)
$Turnover_{t-1}$			-0.099***
			(-8.67)
$Volatility_{t-1}$			1.263***
			(3.82)
$Beta_{t-1}$			-0.026***
			(-3.89)
$IndPoliticalActivity_{t-1}$	0.954	-0.027	0.099
	(1.28)	(-1.28)	(0.81)
$OfficeRestatements_{t-1}$	0.047	0.004	0.102***
	(0.10)	(0.34)	(3.30)
$OfficeIPOS_{t-1}$	-0.407**	0.008	0.007
	(-2.34)	(0.78)	(0.23)
$OfficeCap_{t-1}$	0.244	-0.024*	-0.227**
	(0.52)	(-1.71)	(-2.04)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes
Observations	20,194	20,194	20,194
R^2	0.468	0.241	0.906

Table 8
Robustness Tests

Panel B: Placebo Tests

	$AbsDA\_MJ_t$	$Restatement_t$	$Ln(Spreads)_t$
	(1)	(2)	(3)
$SEC\_Oversight_t$	0.346	-0.007	-0.034
	(1.04)	(-1.01)	(-1.04)
$Size_{t-1}$	-0.365	0.043*	-0.178***
	(-1.60)	(1.96)	(-5.53)
BM_{t-1}	0.720	0.024**	0.071
	(1.05)	(1.96)	(1.42)
$Leverage_{t-1}$	-0.049	0.004	
	(-0.05)	(0.38)	
$FirmAge_{t-1}$	0.391	-0.056**	
	(0.99)	(-2.01)	
ROA_{t-1}	-3.234**	0.014	
	(-2.36)	(1.57)	
Ret_{t-1}	-0.663***	0.006	
	(-4.18)	(0.63)	
$1/Price_{t-1}$			1.582***
			(3.91)
$Turnover_{t-1}$			-0.100***
			(-9.82)
$Volatility_{t-1}$			1.206***
			(3.89)
$Beta_{t-1}$			-0.026***
			(-5.00)
$IndPoliticalActivity_{t-1}$	-0.027	-0.064***	0.087**
	(-0.07)	(-3.62)	(1.96)
$OfficeRestatements_{t-1}$	0.023	0.016***	0.020
	(0.09)	(3.07)	(0.66)
$OfficeIPOS_{t-1}$	-0.279**	-0.016	-0.024**
	(-2.38)	(-1.15)	(-2.42)
$OfficeCap_{t-1}$	-0.040	-0.000	-0.016
	(-0.19)	(-0.04)	(-0.37)
Year Effects	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes
Observations	22,434	22,434	22,434
R^2	0.452	0.224	0.901