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University of Pennsylvania, tblac@wharton.upenn.edu

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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.

Degree Type

Dissertation

Degree Name

Doctor of Philosophy (PhD)

Graduate Group

Accounting

First Advisor

Wayne Guay

Second Advisor

Luzi Hail

Keywords

Disclosure Regulation, Enforcement, Financial Reporting Incentives, Political Economy

Subject Categories

Accounting | Economics

REGULATORY OVERSIGHT AND FINANCIAL REPORTING INCENTIVES: EVIDENCE
FROM SEC BUDGET ALLOCATIONS

Terrence Patrick Blackburne

A DISSERTATION

in

Accounting

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

Degree of Doctor of Philosophy

2014

Supervisor of Dissertation

Wayne Guay, Professor, Accounting

Co-Supervisor of Dissertation

Luzi Hail, Associate Professor, Accounting

Graduate Group Chairperson

Eric Bradlow, Professor of Marketing, Statistics, and Education

Dissertation Committee

Wayne Guay, Professor, Accounting

Luzi Hail, Associate Professor, Accounting

Catherine M. Schrand, Professor, Accounting

Daniel Taylor, Assistant Professor, Accounting

REGULATORY OVERSIGHT AND FINANCIAL REPORTING: EVIDENCE FROM
SEC BUDGET ALLOCATIONS

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Terrence Patrick Blackburne

DEDICATIONS

To Christopher and Anna, for bringing a new purpose to my life.

To Gayle, whose unconditional love and support provides me with sustenance.

And to my parents, who taught me to never give up.

ACKNOWLEDGMENT

I am deeply indebted to the members of my dissertation committee for their patient guidance: Wayne Guay (Co-chair), Luzi Hail (Co-chair), Cathy Schrand, and Dan Taylor. My committee constantly challenged me to question my assumptions and to have sound reasoning behind all of my decisions. I will always be grateful for their contribution to my intellectual growth.

I also thank Jennifer Altamuro, Chris Armstrong, Karthik Balakrishnan, Paul Fischer, Nick Gonedes, Mirko Heinle, Chris Ittner, Bob Holthausen, Zoe-Vonna Palmrose, Shiva Rajgopal, Karthik Ramanna, John Robinson, Stephanie Sikes, Roger Silvers and Ro Verrecchia for their thoughtful discussions. My fellow Ph.D. students have provided much appreciated comments and support, particularly Michael Carniol and Stefan Schantl.

I have also benefited greatly from the comments of seminar participants at the University of Pennsylvania, Emory University, Northwestern University, Columbia University, UCLA, the University of Utah, the University of Washington, Stanford University, Harvard Business School, and the Pennsylvania State University.

Finally, I would like to thank the Wharton School and the Deloitte Foundation for their generous financial support.

ABSTRACT

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Terrence Patrick Blackburne

Wayne Guay

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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.

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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 extent do industry-level political factors affect the allocation of resources that 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, theoretical

arguments suggest that country-level economic shocks could affect both the aggregate amount of resources allocated to regulatory oversight and disclosure outcomes (Bertomeu and Magee 2011), which raises concerns that cross-country empirical studies may omit key macroeconomic variables from their analyses. By examining a source of within-country variation in regulatory oversight, this study overcomes some of the challenges that cannot be addressed in a cross-country setting.

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 average 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 candidate increase in the number of political candidates supported per firm in a given industry is associated with a 56 to 208 thousand dollar decrease in the budgets allocated to review that industry's mandatory disclosure filings. Moreover, I document a substantial increase in the budgetary resources allocated to oversee disclosure filings made by firms in the financial services industry following the financial crisis.

Next, I exploit the SEC's organizational structure and features of the federal budget process to empirically test whether variation in SEC oversight capacity affects managers' reporting and disclosure incentives. I measure SEC oversight capacity as dollars per megabyte of mandatory disclosure filings and use this measure as a proxy for the intensity of SEC oversight. 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 five proxies for managers' reporting and disclosure outcomes: discretionary accruals, the incidence of accounting restatements, the incidence of fraudulent disclosures, bid-ask spreads, and the market premium for illiquidity. I find evidence that, when SEC oversight is more intense, managers report lower discretionary accruals. Moreover, they are significantly less likely to issue financial reports that are subsequently restated or make fraudulent disclosures. In addition, I find evidence that SEC oversight has significant capital market benefits. My findings suggest that a one standard deviation increase in my proxy for the intensity of SEC oversight results in a 2.5 to 8.8 percent decrease in the mean firm's bid-ask spread and a 14.2 to 25.6 percent reduction in its illiquidity premium.

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 capacity should have a smaller effect on their reporting incentives than for non-politically active firms (Stigler 1971; Peltzman 1976). I fail to find evidence that SEC oversight has a consistent differential effect on politically active firms' reporting and disclosure outcomes. This suggests that the effects of corporate political activity on SEC oversight may be an unintended consequence of efforts to influence other regulatory processes. In addition, 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 weak evidence that managers' reporting choices are more responsive to SEC oversight during the time period after the Lehman Brothers collapse.

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. An increase in observed outputs could be the result of either increased activity on the part of the regulator, or reduced compliance with regulatory requirements on the part of managers. Therefore, these studies are constrained in their ability to provide insight into the effects of SEC scrutiny on managers' strategic 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. The evidence suggests, however, that this results from a rational response by the SEC to increased costs of enforcement rather than managers obtaining a quid pro quo from their political activity.

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

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 process¹. These offices are organized based on industry; firms are assigned to a Disclosure Review Office based on their four-digit SIC code². 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

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>.

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.

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 year³. 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

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.

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 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 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 approval⁶. 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.

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.

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

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

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

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 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 an alternative to theories of regulatory capture. They argue 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. A key corollary to the signaling hypothesis is that it implies firms' political activity will have an effect on *all* regulatory bodies that oversee them. Thus, a firm's engagement in the political process that is intended to reduce oversight by a regulator such as the Environmental Protection Agency may have the unintended consequence of affecting activity at the SEC. Moreover, regulators need not observe the political contributions directly if they are correlated with other behavior that the regulator 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 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 constrain the SEC's 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. The SEC's capacity to oversee firms' mandatory disclosure filings is a function of both resources and workload. To the extent that capacity is a binding constraint on SEC oversight, then it should capture the intensity of oversight. If managers make strategic reporting and disclosure decisions then, *ceteris paribus*, the extent of their compliance with reporting and disclosure regulations should be increasing with the SEC's oversight capacity. Alternatively, it is possible that oversight capacity is not binding, either because regulators could be captured, effort averse, or incompetent. If this were the case, then variation in oversight capacity may have little or no influence on managers' behavior.

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 SEC's oversight capacity 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 either because the SEC is captured or because it is more costly to pursue enforcement actions against them. If firms' investment in political activity results in the capture of SEC regulators, 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. Finally, a failure to observe either no relationship or an inconsistent relationship between the responses to SEC oversight for politically active firms could

result if their engagement in the political activity I measure is not directed toward SEC activities.

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.

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, SEC pay tables, and the SEC's congressional budget submissions 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.

I collect data on mandatory disclosure filings subject to Division of Corporation Finance Review from EDGAR. The number of firms is the number of unique firms for an office that have submitted a mandatory filing in each year. There is likely to be wide variation in the complexity of firms' financial disclosures, which in turn should affect the SEC's ability to review them. Loughran and McDonald (2014) present evidence that the size, in megabytes, of a firm's financial filings on EDGAR is a good measure of their complexity. Therefore, I calculate the variable *Workload* as the aggregate size, in megabytes, of all transactional filings deposited on EDGAR that are subject to review by a disclosure review office. This measure of workload is designed to capture variation in both the volume and complexity of workload conducted by a disclosure review office. Finally, I measure *Capacity* as the amount of dollars (in thousands) per megabyte of mandatory filings that each Disclosure Review Office has available to it in a given fiscal

year. Descriptive statistics regarding budget, staffing, and workload 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 9.7% to 22.3% 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. Nonetheless, there is a clear upward trend in the volume (in megabytes) of corporate disclosure filings, which reflects an increase in mandatory disclosure requirements following Sarbanes-Oxley and Dodd-Frank legislation. This trend in filing size has driven a general downward trend in oversight capacity.

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, *IndPolitics*, is calculated as the average 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 in each year. I use two measures to capture the visibility of a given industry. The first, *PctAAERs*, is the percent of firms assigned to a disclosure review office in a given year that are subject to an SEC Accounting and Auditing Enforcement Release. *PctAAERs* is an indicator of visible accounting-related problems in an industry and SEC activity, 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

In addition to office- and industry-level data, I construct a firm-level sample using firms that filed annual reports on EDGAR between fiscal years 2003 and 2012. I require a match between EDGAR and CRSP/Compustat to obtain information firms' financial information and market values. I exclude firms that have both a market capitalization and total revenues less than 75 million dollars because they are subject to different disclosure requirements than larger firms. My final sample is constructed as the intersection of these

three datasets and contains 29,142 firm years (5,159 firms) over the period 2003-2012. 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 are likely to 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. In addition, it is unlikely that SEC resource allocations are correlated with any omitted variables in the

discretionary accruals model. Finally, 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 first 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). Therefore, I only include restatements initiated by auditor or regulatory scrutiny. 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. However, this bias works *against* my predicted outcome as I predict a negative correlation between budgetary resources and restatements.

I also use the incidence of class action litigation, identified from the Stanford Securities Class Action Clearinghouse database, as another proxy for misreporting. The litigation data has some important advantages over the restatement data in my setting. First, litigation may arise from insufficient or misleading disclosures that are not reflected in firms accounting statements. Therefore, this data has potential to capture the broader range of disclosures subject to review by the Division of Corporation Finance. Second,

private litigation should be less affected by SEC resource constraints than measures that rely on SEC actions (Dyck et al. 2013). Some key disadvantages of the litigation data is that many lawsuits are frivolous, or not related to deficiencies in disclosure. To mitigate these problems, I only include lawsuits arising due to material misrepresentations of firm performance or insufficient disclosure. And, I eliminate lawsuits that have been dismissed by the courts.

Finally, I use two measures of information asymmetry. My first measure is *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). In addition, there is a well-established body of theoretical literature that links information asymmetry with liquidity (Vayanos and Wang 2012). Therefore, consistent with prior empirical studies (e.g. Gow, Taylor and Verrecchia 2012; Ng, Verrecchia and Weber 2010), I use the Amihud (2002) measure of the illiquidity premium.

I use two 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. 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 year⁸. Consistent with my prediction, I find a statistically significant negative relation between industry-level political activity, *IndPolitics* and the office-level budget allocation. The effects are economically meaningful; a one candidate increase in the average number of congressional candidates receiving political contributions per firm in an industry is followed by a 56 to 207 thousand dollar decrease in the budget allocated to the disclosure review office overseeing it. This represents a 0.6 to 2.2 percent reduction in the mean office's budget allocation. I also find a significant positive relation between

⁸ 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. Moreover, Cameron et al. (2008) show that calculating clustered standard errors using the bootstrap procedure generates rejection rates close to the hypothetical value.

visibility resulting from the Lehman Brothers bankruptcy and the budget allocated to oversight of the financial services industry.

In the levels specifications, I find a significant positive relation between the percent of firms in an industry subject to an Accounting and Auditing Enforcement Release (AAER) in the prior period and the subsequent office-level budget allocation. A one percent increase in firms subject to an AAER is followed by a 243 to 420 thousand dollar increase in the office's budget allocation. This suggests that the SEC allocates resources in response to publically observable deficiencies in an industry's accounting practices. I fail to find a significant relation between the budget allocation and two of my measures of workload, *Workload* and *OfficeCap*. I document a negative and significant relation between resource allocations and the number of IPOs in an industry, which suggests that the SEC does a poor job of anticipating workload. When I estimate the relation between budget allocations and hypothesized determinants in first differences, I fail to find a statistically significant relation for political activity and AAERs, although the sign of these effects remains unchanged. One possible explanation for this finding is a reduction in the variation of both dependent and independent variables when they are differenced.

To the extent that I have correctly specified the determinants of SEC oversight intensity, my results suggest that SEC oversight is highly responsive to political costs.⁹ I

⁹ 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.

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 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

For the analysis of the relationship between SEC oversight and firm-level reporting outcomes, I use a measure of abnormal capacity, *SEC_Capacity*, as my proxy for the intensity of SEC oversight. Because I have shown the resource allocation to Disclosure Review Offices is affected by aggregate industry-level activity, I calculate *SEC_Capacity* as the residual from the following industry-level regressions:

$$\begin{aligned} Capacity_{i,t} = & \alpha + \beta_1 IndPolitics_{i,t} + \beta_2 PctAAER_{i,t} + \beta_3 OfficeCap_{i,t} \\ & + \beta_4 OfficeCap_{i,t} + \varepsilon_{i,t} \end{aligned}$$

Variation in the intensity of SEC capacity in the proxy I use is the result of changes in both office-level budgets and office-level workload from year to year, after controlling for common industry factors.

I then estimate a series of linear models to examine the effects of SEC oversight on five measures of reporting outcomes: discretionary accruals, restatements, fraudulent disclosures, bid-ask-spreads, and the illiquidity premium. Bertomeu and Magee (2011) provide a theoretical justification for an endogenous relation between financial reporting quality and regulatory oversight resulting from common macroeconomic shocks. These shocks occur at the country-level as they shift the balance of political power between

groups favoring greater regulatory oversight and those favoring less. 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 because the political environment in my setting is constant across all firms. In addition, year effects remove any common time trends. This is an important feature that distinguishes my study from studies in an international setting that examine the effects of enforcement (Jackson and Roe 2007, Christensen et al. 2011, Christensen et al. 2013), where country-year level measures of enforcement may be endogenously related to country-year macroeconomic shocks.

I estimate versions of 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 also estimate a version of the models containing both firm and office fixed effects to capture changes in oversight intensity for these firms

Estimates from regressions of managers' financial reporting discretion as a function of *SEC_Capacity* and controls are provided in Table 4. I find evidence of a statistically significant and economically meaningful relation between *SEC_Capacity* and the magnitude of discretionary accruals across all specifications that I estimate (t-statistics between -1.96 and -2.16). The results suggest that a one standard deviation increase in *SEC_Capacity* is associated with a decrease in unsigned discretionary accruals equal to 0.9 to 1.7 percent of total assets. This is a considerable effect, equivalent to a 4.9 to 9.7 (6.4 to 12.4) percent reduction in the magnitude of the mean

(median) firm's total unsigned discretionary accruals. The evidence is thus 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 probit model of the likelihood of an accounting restatement as a function of SEC oversight in Panel A of table 5. I use a random effects model in columns 3 and 4 to account for unobservable firm-specific characteristics.¹⁰ Once again, I find a consistently negative relation between *SEC_Capacity* and the likelihood that managers will provide accounting information that is eventually restated. The results are statistically significant in three of the four specifications. And, the marginal effects are economically meaningful. For the mean firm, moving across the interquartile range in *SEC_Capacity* results in a 0.4 to 0.46 percent reduction in the probability that a firm's financial statements will eventually be restated. This is especially large when one notes that the unconditional probability of a restatement for firms in my sample is only 2.2 percent.

Table 5, panel B presents the results of estimating the probit model with litigation as the dependent variable. As with the specification with restatements as the dependent variable, I find a consistently negative relation between *SEC_Capacity* and fraud. In this case, the relation is significant in all four of the specifications I test. Once again, the marginal effects are meaningful; moving across the interquartile range in *SEC_Capacity* results in a 0.37 to 0.44 percent reduction in the probability that a firm will make fraudulent disclosures. The economic significance of this effect is even more striking

¹⁰ The use of a large number of fixed effects in non-linear models is both computationally intensive and can result in an incidental parameters problem. The use of a random-effects model allows me to control for time invariant firm characteristics.

when one considers that the unconditional probability of fraud in my sample is only 1.1 percent.

The results from estimating regressions of bid-ask spreads as a function of *SEC_Capacity* are presented in Table 6, panel A. I find a negative relationship between spreads and *SEC_Capacity* in all the models that I estimate. This relationship is statistically significant in three of the four specifications I examine. Furthermore, the economic magnitude of the relationship is substantial. I document evidence that a one standard deviation increase in *SEC_Capacity* is associated with a 2.5 to 8.8 percent reduction in the mean firm's bid-ask spreads. I also find a negative relationship between the illiquidity premium and *SEC_Capacity*. (results are presented in Table 6, panel B). As with bid-ask spreads, the findings are statistically significant in three of the four specifications I test, and large in economic impact; a one standard deviation increase in *SEC_Capacity* is associated with a 14.2 to 25.6 percent reduction in the mean firm's illiquidity premium.

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

5.3 Heterogeneity in the Effects of SEC Oversight

I conduct partitioned tests using of two 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 consistent relation for the

coefficient on the interaction between SEC oversight and the partitioning variable for political activity accruals and bid-ask spreads and the outcomes I measure. One potential explanation for the mixed evidence is that firms engage in the political activity I measure to influence regulators other than the SEC. If this is the case, then the relationship I document between political activity and the SEC's internal budget allocations in Table 3 likely arise due to the Gordon and Hafer (2005) signaling hypothesis rather than because of regulatory capture.

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 firms whose disclosures are reviewed by the disclosure review office with responsibility for the financial services sector and *SEC_Capacity*. 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 stronger effect on all of the outcomes I measure except for bid-ask spreads, although the relationship is only significant for the illiquidity premium. While the evidence is weak, 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. One possible explanation for the lack of statistically significant findings could be that *SEC_Capacity* is generally a binding constraint on the overall intensity of SEC oversight. Therefore, the overall effect of the Lehman crisis on oversight may have been subsumed by the corresponding increase in *SEC_Capacity*.

5.4 Robustness Tests

5.4.1 Determinants of SEC Resource Allocations

A concern regarding my estimates of the relationship between industry-level political activity and the SEC's allocation of resources to its disclosure review offices is the possibility that they may be jointly determined. I conduct two separate analyses to mitigate this concern. The first is a simultaneous equations model that assumes an endogenous relation between SEC resource allocations and industry-level political activity. The identification of the simultaneous equation model requires an instrument for each endogenous variable in the system of equations.

I use an indicator variable coded as one following a change in assistant director overseeing a disclosure review office to instrument for the *Dollars* allocated to a given disclosure review office. A change in assistant director is likely to be negatively correlated with resource allocations, as newer directors have less ability to negotiate for additional resources in internal budget negotiations. In addition, SEC assistant director turnover is a strong candidate as an instrument for the resource allocation because it is likely to be idiosyncratic and thus uncorrelated with firms' incentives to engage in political activity beyond its effect on SEC resource allocations. I am unable to directly infer why assistant directors leave the SEC. However, for a substantial portion of my sample period—between fiscal years 2008 and 2012—the primary reason for supervisor turnover at the SEC was due to retirements (GAO 2013b).

I following prior literature (Grier et al. 1996; Hill et al. 2013) and use industry concentration, which I measure as the average Herfindahl Index calculated by 4-digit SIC

code, as a source of plausibly exogenous variation in industry-level incentives to engage in the political process. More concentrated industries are less likely to suffer from free-rider problems and thus be able to reap greater gains from political activity. And, industry concentration is unlikely to be correlated with SEC resource allocations, other than via its effect on industry-level political activity.

The results from estimating the simultaneous equations model are presented in Table 8, panel A. I find evidence that industry-level political activity has a substantial negative effect on the allocation of resources to an SEC disclosure review office as seen in the first column. Consistent with the notion that this effect is an unintended consequence of firms' political activity, I fail to find evidence that SEC oversight affects industry-level political behavior. The partial F -statistic for *AvgHerf* is 11.55, which suggests that it is a strong instrument for *IndPolitics*. However, the partial F -statistic for $\Delta Supervisor$ is only 2.94, which suggests that it is a weak instrument for *Dollars*. The weakness of this instrument could result in biased coefficients. Therefore, I conduct a second analysis that relies solely on variation in *AvgHerf* to identify the effect of *IndPolitics* on *Dollars*.

The result of the instrumental variables analysis is presented in Table 8, panel B. As in the prior analyses, I document a large negative effect of *IndPolitics* on the resources allocated to an SEC Disclosure Review Office. The partial F -statistic for *AvgHerf* is 11.15, which suggests that the instrument is strong. Overall, the results in Table 8 suggest that the SEC devotes fewer resources to review mandatory disclosure filings when industries are more politically active.

5.4.2 SEC Oversight Capacity and Reporting Outcomes

5.4.2.1 Office-Level Analysis

Because the treatment of interest, *SEC_Capacity*, is an office-level variable, I conduct a separate analysis of the outcomes calculated at the office-level. I follow the procedure in Hail and Leuz (2006) to calculate office-level outcome variables. To account for heterogeneity within disclosure review offices, I first calculate yearly regressions for each outcome with firm-level controls and SEC office fixed effects. I then use the office fixed effect as an office-level measure of the outcome. The results of this analysis are presented in Table 9. As with the firm-level analyses, I document a consistent negative relation between SEC oversight capacity and office-level measures of reporting and disclosure quality. In the office-level analysis, however, the capital market benefits of SEC oversight are not statistically significant.

5.4.2.2 Falsification 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 10 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 my measures of reporting and disclosure outcomes. This suggests that the results I find are not driven by 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.

5.4.2.3 Exclusion of Financial Services

Following the financial crisis, in fiscal year 2011, the Division split the office responsible for reviewing the filings of firms in the financial services industry into two offices. A concern is that the large increase in resources allocated to the financial services industry as a result of the crisis could be driving my results. Therefore, my final set of robustness tests excludes firms in the financial services industry from my firm-level analyses. The results of this analysis are provided in Table 10, panel C. As can be seen, excluding financial services firms generally increases the magnitude and significance of my findings. Therefore, it is unlikely that my findings are driven by the increase in oversight of these firms.

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, and not because the SEC is captured. Overall, the results suggest that managers exercise less financial reporting discretion and are less likely to misreport financial information when the SEC has a greater capacity to review their mandatory disclosure filings. SEC oversight also has some beneficial capital market consequences, as bid-ask spreads and the illiquidity premium are lower when oversight is more intense.

The variation in the SEC's oversight capacity in my sample is limited. Yet, even after controlling for inter-temporal and industry-level variation in oversight capacity 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. Prior research suggests that private monitoring activities may be more efficient than SEC enforcement (Dyck et al 2010). In addition, I only examine potential benefits of SEC oversight. It is not clear whether the observed benefits of greater SEC oversight capacity 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.

APPENDIX

VARIABLE DEFINITIONS

Disclosure Review Office- and Industry-Level Variables

Dollars The estimated budget allocation in thousands of constant 2010 year dollars for the Division of Corporation Finance disclosure review office for the fiscal year.

Workload The size, in megabytes, of mandatory filings on EDGAR subject to potential review by the Division of Corporation Finance disclosure review office for the fiscal year.

Capacity *Dollars/Workload.*

SEC_Capacity The residual from the following office-level regression:

$$\begin{aligned} \text{Capacity}_{i,t} &= \alpha + \beta_1 \text{IndPolitics}_{i,t} + \beta_2 \text{PctAAER}_{i,t} \\ &+ \beta_3 \text{OfficeCap}_{i,t} + \beta_4 \text{OfficeCap}_{i,t} + \varepsilon_{i,t} \end{aligned}$$

Positions The number of staff in the Division of Corporation Finance disclosure review office during fiscal year.

IndPolitics The average 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 in each year.

Lehman An indicator variable equal to one for the financial services industry in years following the bankruptcy of Lehman Brothers.

<i>PctAAERs</i>	The percent of firms assigned to a Disclosure Review Office subject to an AAER in the prior year.
<i>OfficeCap</i>	The natural log of the aggregate market capitalization of all firms in a given disclosure review office during the year.
<i>ΔSupervisor</i>	An indicator variable equal to one following a change in Assistant Director for a given Disclosure Review Office.
<i>AvgHerf</i>	The average Herfindahl Index for firms subject to review by a Disclosure Review Office, calculated at the four digit SIC level.

Measures of Reporting and Disclosure Outcomes

<i>AbsDA_MJ_t</i>	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.
<i>Restatement_t</i>	An indicator variable equal to one for years when the firm reports accounting information that is later restated.
<i>Fraud</i>	An indicator variable equal to one for years when a firm initially fails to disclose material information or makes a material misrepresentation of information that later results in class-action litigation
<i>Spreads_t</i>	The natural log of the bid-ask spread scaled by price.
<i>Illiquidity</i>	The Amihud (2002) measure of the illiquidity premium

Firm Controls

<i>Size_t</i>	The natural log of the firm's market value of equity at the end of the fiscal year.
<i>BM_t</i>	The book value of equity scaled by market value of equity at the end of fiscal year.
<i>Leverage_t</i>	Total liabilities divided by total assets.
<i>FirmAge_t</i>	The number of years that the firm appears on Compustat.
<i>ROA_t</i>	Net income scaled by total assets.
<i>Ret_t</i>	Buy-and-hold returns over the fiscal year.
<i>Financing</i>	An indicator equal to one if a firm issues debt or equity greater than twenty percent of its book assets.
<i>Acquisition</i>	An indicator equal to one if a firm makes an acquisition that contributes to its sales.
<i>1/Price_t</i>	The inverse of the firm's stock price at the end of fiscal year.
<i>Turnover_t</i>	The average monthly share volume scaled by shares outstanding over the fiscal year.
<i>Beta_t</i>	The slope coefficient estimated from a market model of monthly security returns over the fiscal year.
<i>Volatility_t</i>	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.

Figure 1
Timeline of the Budget Process

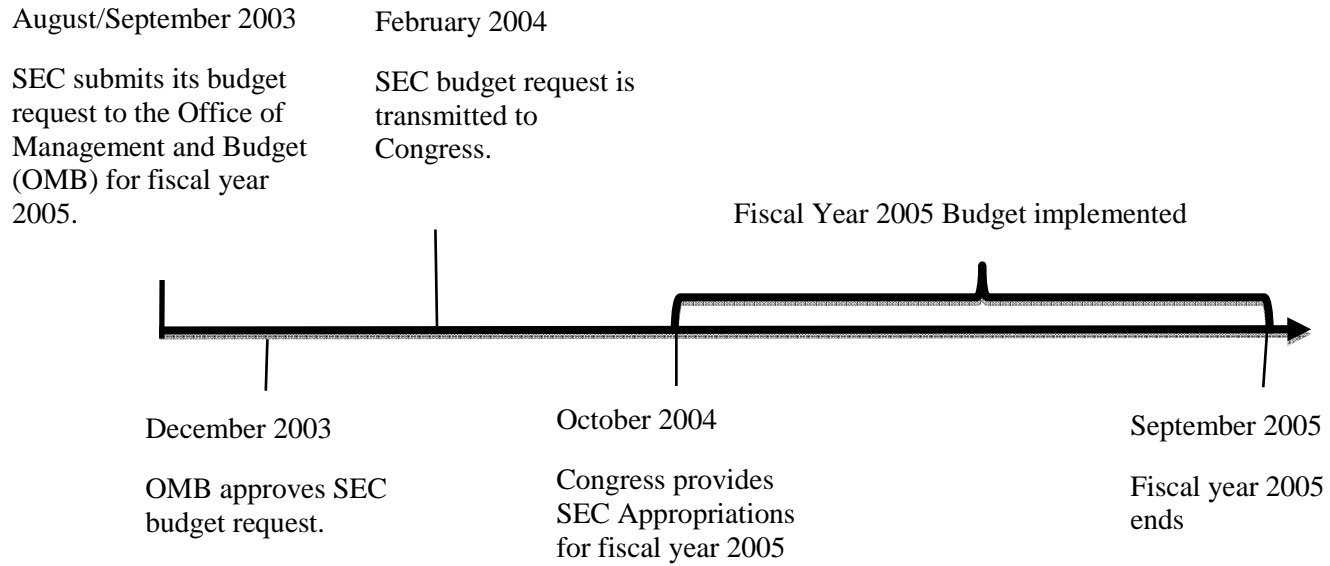


Table 1: Office-Level Resource Allocations

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.

Table 1 (Cont'd): Office-Level Resource Allocations

Panel A: Summary Statistics by Office

<i>Office</i>	<i>Primary Industry</i>	<i>Statistic</i>	<i>Dollars</i>	<i>Positions</i>	<i>#Firms</i>	<i>Workload</i>	<i>Capacity</i>
1	Healthcare and Insurance	Mean	8964.77	32.0	824.0	13416.5	0.837
		<i>Std</i>	<i>1219.01</i>	<i>3.8</i>	<i>114.7</i>	<i>7833.9</i>	<i>0.349</i>
2	Consumer Products	Mean	9383.56	33.8	641.8	14292.3	0.924
		<i>Std</i>	<i>1300.04</i>	<i>4.5</i>	<i>55.3</i>	<i>10218.6</i>	<i>0.472</i>
3	Information Technologies & Services	Mean	9322.92	33.2	1067.4	11713.3	0.891
		<i>Std</i>	<i>1370.64</i>	<i>3.7</i>	<i>215.3</i>	<i>5178.3</i>	<i>0.258</i>
4	Natural Resources	Mean	10283.58	36.5	599.7	10013.1	1.402
		<i>Std</i>	<i>1802.64</i>	<i>4.1</i>	<i>31.1</i>	<i>6026.6</i>	<i>0.822</i>
5	Transportation and Leisure	Mean	9832.01	35.1	835.4	11897.6	0.978
		<i>Std</i>	<i>1746.69</i>	<i>5.0</i>	<i>100.4</i>	<i>6138.2</i>	<i>0.357</i>
6	Manufacturing and Construction	Mean	9831.93	35.1	842.2	11606.0	1.042
		<i>Std</i>	<i>1470.11</i>	<i>4.3</i>	<i>67.1</i>	<i>6469.0</i>	<i>0.423</i>
7	Financial Services	Mean	10177.35	35.5	1082.0	28003.0	0.598
		<i>Std</i>	<i>2268.01</i>	<i>5.6</i>	<i>172.1</i>	<i>30430.9</i>	<i>0.293</i>
8	Real Estate and Commodities	Mean	8801.29	31.7	833.0	12621.4	0.905
		<i>Std</i>	<i>1185.09</i>	<i>4.2</i>	<i>68.6</i>	<i>8535.0</i>	<i>0.383</i>
9	Beverages, Apparel, and Mining	Mean	7936.51	28.4	624.0	11372.7	0.993
		<i>Std</i>	<i>767.80</i>	<i>2.4</i>	<i>75.1</i>	<i>5641.2</i>	<i>0.775</i>
10	Electronics and Machinery	Mean	9279.18	32.9	836.0	10901.3	1.052
		<i>Std</i>	<i>1574.55</i>	<i>4.3</i>	<i>53.3</i>	<i>6176.7</i>	<i>0.427</i>
11	Telecommunications	Mean	9545.72	34.0	720.9	10983.5	0.972
		<i>Std</i>	<i>1502.67</i>	<i>4.9</i>	<i>73.1</i>	<i>4403.2</i>	<i>0.303</i>

Table 1 (Cont'd): Office-Level Resource Allocations

Panel B: Summary Statistics by Fiscal Year

<i>Fiscal Year</i>	<i>Statistic</i>	<i>Dollars</i>	<i>Positions</i>	<i>#Firms</i>	<i>Workload</i>	<i>Capacity</i>
2003	Mean	6605.35	25.72	910.4	4199.2	1.74
	<i>Std</i>	488.57	1.95	280.2	1190.2	0.71
2004	Mean	7913.13	36.27	871.5	5992.4	1.38
	<i>Std</i>	537.02	2.49	251.6	1359.8	0.32
2005	Mean	10905.17	38.27	859.9	8336.5	1.34
	<i>Std</i>	862.43	3.00	227.9	1526.4	0.24
2006	Mean	10008.35	34.72	832.1	9844.5	1.04
	<i>Std</i>	1094.60	3.93	192.1	1480.1	0.21
2007	Mean	9383.01	33.55	834.6	11447.4	0.84
	<i>Std</i>	899.73	3.36	165.6	2105.8	0.17
2008	Mean	9445.38	34.72	835.9	11498.6	0.84
	<i>Std</i>	606.45	2.15	149.7	2176.5	0.13
2009	Mean	9140.66	32.72	797.5	11475.2	0.82
	<i>Std</i>	928.05	3.26	120.8	2777.6	0.16
2010	Mean	10705.56	34.45	745.9	14874.2	0.76
	<i>Std</i>	1086.88	3.72	99.8	4507.2	0.18
2011	Mean	10372.11	33.09	721.3	22170.5	0.52
	<i>Std</i>	1491.76	4.76	95.6	10709.2	0.16
2012	Mean	9483.85	31.18	691.9	33635	0.33
	<i>Std</i>	1583.40	5.10	96.9	24550.9	0.10

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 2012, and cover a total of 29,142 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

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>Std Dev</i>	<i>25th</i>	<i>Median</i>	<i>75th</i>
<i>Dollars</i>	110	9396.26	1583.81	8437.86	9466.11	10300.67
<i>Positions</i>	110	33.47	4.64	31.00	34.00	36.00
<i>#Firms</i>	110	897.63	429.17	666.00	803.50	941.00
<i>Workload</i>	110	13347.33	11760.73	8265.35	10578.31	14894.61
<i>Capacity</i>	110	0.96	0.49	0.65	0.90	1.23
<i>IndPolitics</i>	110	4.12	1.90	2.49	4.05	5.27
<i>PctAAER</i>	110	0.84	0.54	0.39	0.73	1.15
<i>OfficeCap</i>	110	13.97	0.51	13.71	14.03	14.32
<i>OfficeIPOs</i>	110	0.03	0.13	0.00	0.00	0.00

Panel B: Measures of Information Quality

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>Std Dev</i>	<i>25th</i>	<i>Median</i>	<i>75th</i>
<i>AbsDA_MJ</i>	29,142	17.181	15.938	5.144	13.349	24.326
<i>Restatement</i>	29,142	0.022	0.147	0.000	0.000	0.000
<i>Fraud</i>	29,142	0.011	0.104	0.000	0.000	0.000
<i>Spreads</i>	29,142	0.555	1.209	0.092	0.177	0.454
<i>Illiquidity</i>	29,142	1.538	28.219	0.001	0.004	0.039

Table 2 (Cont'd): Summary Statistics

Panel C: Firm Characteristics

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>Std Dev</i>	<i>25th</i>	<i>Median</i>	<i>75th</i>
<i>Size</i>	29,142	6.520	1.709	5.279	6.377	7.611
<i>BM</i>	29,142	0.590	0.480	0.279	0.472	0.741
<i>Leverage</i>	29,142	0.481	0.226	0.300	0.487	0.651
<i>FirmAge</i>	29,142	21.812	15.211	10.000	16.000	30.000
<i>ROA</i>	29,142	-0.001	0.168	-0.010	0.035	0.075
<i>Ret</i>	29,142	0.178	0.622	-0.181	0.084	0.383
<i>Financing</i>	29,142	0.224	0.417	0.000	0.000	0.000
<i>Acquisition</i>	29,142	0.116	0.320	0.000	0.000	0.000
<i>I/Price</i>	29,142	0.111	0.154	0.031	0.056	0.120
<i>Turnover</i>	29,142	9.482	8.130	3.959	7.302	12.262
<i>Volatility</i>	29,142	0.031	0.018	0.019	0.027	0.037
<i>Beta</i>	29,142	1.085	0.550	0.705	1.067	1.428
<i>PolActive</i>	29,142	0.163	0.369	0.000	0.000	0.000
<i>Lehman</i>	29,142	0.006	0.077	0.000	0.000	0.000

Table 3: Determinants of SEC Oversight Intensity

This table presents the results from an OLS regression of SEC Disclosure Review Office 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.

Table 3 (Cont'd): Determinants of SEC Oversight Intensity

	<i>Dollars</i>			<i>ΔDollars</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>IndPolitics</i>	-207.613*** (-3.67)	-110.668* (-1.67)	-162.311*** (-2.90)	-115.472 (-0.89)	-55.815 (-0.37)	-92.967 (-0.78)
<i>PctAAER</i>	419.820** (2.18)		344.071** (2.26)	256.789 (1.13)		242.738 (1.09)
<i>Lehman</i>		1,986.334** (2.34)	1,900.489** (2.22)		851.618*** (3.22)	789.610*** (3.04)
<i>OfficeCap</i>	-91.295 (-0.37)	357.902 (0.84)	377.979 (1.00)	-251.076 (-0.42)	-161.581 (-0.23)	-154.143 (-0.22)
<i>OfficeIPOs</i>	-551.567 (-1.27)	-927.614*** (-2.88)	-833.495*** (-2.62)	-1,106.529*** (-3.29)	-1,166.010*** (-3.39)	-1,142.472*** (-3.48)
<i>Workload</i>	1,393.964 (1.06)	629.077 (0.45)	636.638 (0.49)	352.634 (0.20)	91.656 (0.05)	265.197 (0.15)
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	0.820	0.835	0.839	0.738	0.737	0.740

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 capacity and control variables. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	<i>AbsDA_MJ</i>			
	(1)	(2)	(3)	(4)
<i>SEC_Capacity</i>	-3.790** (-2.10)	-2.073** (-2.00)	-2.125** (-2.16)	-1.944** (-1.96)
<i>Size</i>	-0.809*** (-3.78)	-0.790*** (-3.34)	-0.328 (-0.62)	-0.300 (-0.58)
<i>BM</i>	-3.361*** (-2.76)	-2.693** (-2.01)	-0.494 (-0.70)	-0.444 (-0.64)
<i>Leverage</i>	-4.687* (-1.96)	-2.798 (-0.99)	4.447*** (3.72)	4.633*** (3.84)
<i>Firm_Age</i>	-0.006 (-0.21)	0.015 (0.57)	0.688 (1.14)	0.729 (1.15)
<i>ROA</i>	-4.845** (-2.09)	-3.313 (-1.57)	0.223** (2.11)	2.313 (1.16)
<i>Ret</i>	0.135 (0.73)	0.266 (1.46)	2.249 (1.14)	0.217** (2.03)
<i>Financing</i>	0.649 (1.14)	0.880 (1.53)	-0.278 (-0.70)	-0.248 (-0.65)
<i>Acquisition</i>	-0.102 (-0.21)	-0.783* (-1.71)	-0.420** (-2.55)	-0.415** (-2.37)
Year Effects	Yes	Yes	Yes	Yes
Office Effects	No	Yes	No	Yes
Firm Effects	No	No	Yes	Yes
Observations	29,142	29,142	29,142	29,142
R ²	0.055	0.084	0.479	0.480

Table 5: SEC Oversight and Misreporting

This table presents the results from estimating the likelihood of misreporting as a function of SEC oversight capacity. The dependent variable for panel A is the incidence of financial reporting that is subsequently restated. The dependent variable for panel B is the incidence of fraudulent disclosure that is subsequently subject to class-action litigation. All variables are as defined in Appendix A. For both panels, columns (1) and (2) are estimated using a probit model, t-statistics appear in parentheses and are calculated using standard errors clustered by SEC disclosure review office and year. And, columns (3) and (4) are estimated using a random effects probit model with standard errors clustered by firm. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Panel A: Restatements

	<i>Restatement</i>			
	(1)	(2)	(3)	(4)
<i>SEC_Capacity</i>	-0.155** (-1.98)	-0.193 (-1.46)	-0.318* (-1.89)	-0.337* (-1.68)
<i>Size</i>	0.057* (1.72)	0.059* (1.73)	0.167*** (2.80)	0.162*** (2.75)
<i>BM</i>	0.098** (2.20)	0.098** (2.25)	0.210* (1.74)	0.197 (1.63)
<i>Leverage</i>	0.255*** (2.67)	0.252*** (2.65)	0.352 (1.18)	0.262 (0.85)
<i>Firm_Age</i>	-0.005** (-2.37)	-0.006** (-2.56)	-0.011* (-1.68)	-0.013* (-1.85)
<i>ROA</i>	-0.223 (-1.17)	-0.246 (-1.16)	-0.271 (-0.85)	-0.318 (-1.02)
<i>Ret</i>	0.003 (0.07)	0.002 (0.04)	-0.002 (-0.03)	-0.003 (-0.05)
<i>Financing</i>	0.006 (0.08)	0.004 (0.05)	0.044 (0.47)	0.044 (0.48)
<i>Acquisition</i>	0.083*** (2.72)	0.080*** (2.83)	0.107 (1.16)	0.110 (1.20)
Year Effects	Yes	Yes	Yes	Yes
Office Effects	No	Yes	No	Yes
Firm Effects	No	No	Yes	Yes
Observations	29,142	29,142	29,142	29,142

Table 5 (Cont'd): SEC Oversight and Misreporting

Panel B: Fraud

	<i>Fraud</i>			
	(1)	(2)	(3)	(4)
<i>SEC_Capacity</i>	-0.321*** (-2.65)	-0.296** (-2.02)	-0.327*** (-4.03)	-0.299*** (-2.68)
<i>Size</i>	0.081*** (4.11)	0.074*** (3.84)	0.083*** (4.99)	0.075*** (4.62)
<i>BM</i>	-0.161*** (-6.88)	-0.138*** (-5.70)	-0.166** (-2.34)	-0.140** (-2.03)
<i>Leverage</i>	0.004 (0.02)	0.070 (0.44)	0.004 (0.03)	0.070 (0.69)
<i>Firm_Age</i>	-0.005 (-1.57)	-0.004 (-1.31)	-0.005*** (-2.86)	-0.004** (-2.07)
<i>ROA</i>	-0.247** (-2.39)	-0.092 (-0.79)	-0.245* (-1.95)	-0.090 (-0.69)
<i>Ret</i>	0.063** (2.04)	0.067** (2.01)	0.062 (1.62)	0.067* (1.78)
<i>Financing</i>	0.128*** (2.59)	0.147*** (2.74)	0.130** (2.44)	0.148*** (2.80)
<i>Acquisition</i>	-0.010 (-0.20)	-0.008 (-0.14)	-0.011 (-0.15)	-0.008 (-0.12)
Year Effects	Yes	Yes	Yes	Yes
Office Effects	No	Yes	No	Yes
Firm Effects	No	No	Yes	Yes
Observations	29,142	29,142	29,142	29,142

Table 6: SEC Oversight and Information Asymmetry

This table presents the results from an OLS regression of proxies for information asymmetry 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 standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Panel A: Bid-Ask Spreads

	(1)	(2)	(3)	(4)
<i>SEC_Capacity</i>	-0.037 (-0.67)	-0.112*** (-3.13)	-0.033*** (-3.83)	-0.034*** (-3.61)
<i>Size</i>	-0.138*** (-5.40)	-0.138*** (-5.40)	-0.145*** (-2.94)	-0.145*** (-2.96)
<i>BM</i>	0.120* (1.79)	0.101 (1.61)	-0.018 (-0.27)	-0.019 (-0.27)
<i>1/Price</i>	1.319*** (5.36)	1.363*** (5.32)	0.262 (1.15)	0.265 (1.16)
<i>Turnover</i>	-0.023*** (-6.06)	-0.022*** (-5.92)	-0.020*** (-4.19)	-0.021*** (-4.20)
<i>Volatility</i>	3.189*** (8.95)	3.197*** (9.18)	2.193*** (6.80)	2.193*** (6.80)
<i>Beta</i>	-0.485*** (-6.51)	-0.498*** (-6.46)	-0.169*** (-5.58)	-0.169*** (-5.52)
Year Effects	Yes	Yes	Yes	Yes
Office Effects	No	Yes	No	Yes
Firm Effects	No	No	Yes	Yes
Observations	29,142	29,142	29,142	29,142
R ²	0.395	0.397	0.767	0.768

Table 6 (Cont'd): SEC Oversight and Information Asymmetry

Panel B: Illiquidity

	<i>Illiquidity</i>			
	(1)	(2)	(3)	(4)
<i>SEC_Capacity</i>	-0.009*** (-4.97)	-0.005 (-1.23)	-0.005*** (-4.36)	-0.005*** (-4.35)
<i>Size</i>	-0.005 (-1.60)	-0.005 (-1.59)	-0.008 (-1.57)	-0.008 (-1.57)
<i>BM</i>	0.014* (1.73)	0.013* (1.71)	-0.004 (-0.51)	-0.004 (-0.51)
<i>1/Price</i>	0.080*** (3.33)	0.082*** (3.37)	0.038 (1.36)	0.039 (1.41)
<i>Turnover</i>	-0.001*** (-2.60)	-0.001*** (-2.66)	-0.001** (-2.29)	-0.001** (-2.30)
<i>Volatility</i>	0.099 (1.42)	0.100 (1.42)	0.059 (1.32)	0.059 (1.33)
<i>Beta</i>	-0.025*** (-2.73)	-0.027** (-2.55)	-0.008 (-1.26)	-0.008 (-1.24)
Year Effects	Yes	Yes	Yes	Yes
Office Effects	No	Yes	No	Yes
Firm Effects	No	No	Yes	Yes
Observations	29,142	29,142	29,142	29,142
R-squared	0.018	0.019	0.536	0.536

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. In each panel, columns (1), (4), and (5) are estimated using OLS and standard errors are clustered by office and year. Columns (2) and (3) are estimated using a random effects probit model with standard errors clustered by firm. All variables are as defined in Appendix A. t-statistics appear in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 7 (Cont'd): Heterogeneity in the Effect of SEC Oversight

Panel A: Politically Active Firms

	<i>AbsDA_MJ</i>	<i>Restatement</i>	<i>Fraud</i>	<i>Spread</i>	<i>Illiquidity</i>
	(1)	(2)	(3)	(4)	(5)
<i>SEC_Capacity</i>	-1.807*	-0.436**	-0.268**	-0.037***	-0.008***
	(-1.81)	(-2.08)	(-2.41)	(-5.31)	(-3.72)
<i>SEC_Capacity*PolActive</i>	-0.794***	0.616**	-0.234*	0.018	0.013***
	(-3.66)	(2.15)	(-1.81)	(0.40)	(2.93)
<i>PolActive</i>	0.009	-0.326	-0.002	0.018	-0.004*
	(0.01)	(-1.46)	(-0.03)	(0.91)	(-1.82)
<i>Size</i>	-0.298	0.184***	0.073***	-0.145**	-0.008
	(-0.57)	(2.93)	(4.40)	(-2.53)	(-1.54)
<i>BM</i>	-0.436	0.219*	-0.143**	-0.019	-0.004
	(-0.63)	(1.78)	(-2.07)	(-0.24)	(-0.51)
<i>Leverage</i>	4.689***	0.292	0.070		
	(3.98)	(0.93)	(0.69)		
<i>Firm_Age</i>	0.728	-0.013*	-0.004**		
	(1.15)	(-1.87)	(-2.00)		
<i>ROA</i>	2.319	-0.323	-0.087		
	(1.16)	(-1.01)	(-0.66)		
<i>Ret</i>	0.215**	-0.003	0.067*		
	(2.06)	(-0.05)	(1.78)		
<i>Financing</i>	-0.247	0.042	0.149***		
	(-0.64)	(0.45)	(2.82)		
<i>Acquisition</i>	-0.411**	0.104	-0.008		
	(-2.30)	(1.11)	(-0.11)		
<i>1/Price</i>				0.264	0.039
				(0.98)	(1.39)
<i>Turnover</i>				-0.021***	-0.001**
				(-3.60)	(-2.28)
<i>Volatility</i>				2.192***	0.059
				(5.67)	(1.32)
<i>Beta</i>				-0.169***	-0.008
				(-4.70)	(-1.25)
Year Effects	Yes	Yes	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes	Yes	Yes
Observations	29,142	29,142	29,142	29,142	29,142
R ²	0.481	0.385	0.049	0.768	0.536

Table 7 (Cont'd): Heterogeneity in the Effect of SEC Oversight

Panel B: Post Lehman Brothers Crisis

	<i>AbsDA_MJ</i>	<i>Restatement</i>	<i>Fraud</i>	<i>Spread</i>	<i>Illiquidity</i>
	(1)	(2)	(3)	(4)	(5)
<i>SEC_Capacity</i>	-1.873*	-0.337*	-0.299***	-0.035***	-0.005***
	(-1.95)	(-1.68)	(-2.69)	(-4.24)	(-3.80)
<i>SEC_Capacity*Lehman</i>	-2.322	-0.211	-0.492	0.327	-0.138***
	(-0.35)	(-0.49)	(-0.24)	(0.98)	(-7.94)
<i>Lehman</i>	7.531***	0.120	-0.311	0.025	-0.049***
	(7.39)	(0.15)	(-0.39)	(0.19)	(-3.95)
<i>Size</i>	-0.292	0.162***	0.074***	-0.145***	-0.008
	(-0.56)	(2.75)	(4.63)	(-2.96)	(-1.57)
<i>BM</i>	-0.488	0.197	-0.140**	-0.018	-0.004
	(-0.71)	(1.64)	(-2.02)	(-0.26)	(-0.52)
<i>Leverage</i>	4.644***	0.264	0.069		
	(3.82)	(0.86)	(0.68)		
<i>Firm_Age</i>	0.726	-0.013*	-0.004**		
	(1.15)	(-1.86)	(-2.07)		
<i>ROA</i>	2.281	-0.318	-0.089		
	(1.14)	(-1.01)	(-0.68)		
<i>Ret</i>	0.218**	-0.003	0.066*		
	(2.12)	(-0.05)	(1.78)		
<i>Financing</i>	-0.282	0.043	0.148***		
	(-0.73)	(0.47)	(2.80)		
<i>Acquisition</i>	-0.408**	0.111	-0.009		
	(-2.30)	(1.21)	(-0.13)		
<i>1/Price</i>				0.263	0.039
				(1.15)	(1.41)
<i>Turnover</i>				-0.021***	-0.001**
				(-4.20)	(-2.29)
<i>Volatility</i>				2.194***	0.060
				(6.80)	(1.33)
<i>Beta</i>				-0.169***	-0.007
				(-5.52)	(-1.24)
<i>Year Effects</i>	Yes	Yes	Yes	Yes	Yes
<i>Office Effects</i>	Yes	Yes	Yes	Yes	Yes
<i>Firm Effects</i>	Yes	Yes	Yes	Yes	Yes
<i>Observations</i>	29,142	29,142	29,142	29,142	29,142
<i>R²</i>	0.482	0.374	0.048	0.768	0.536

Table 8: Instrumental Variable Analysis

This table presents the results from tests using instrumental variables. All variables are as defined in Appendix A. Panel A presents the results from estimating the following simultaneous equations model:

$$Dollars_{i,t} = \beta_1 IndPolitics_{i,t} + \gamma_1 X_{i,t} + \varphi_1 \Delta Supervisor_{i,t} + \varepsilon_{i,t}$$

$$IndPolitics_{i,t} = \beta_2 Dollars_{i,t} + \gamma_2 X_{i,t} + \varphi_2 AvgHerf_{i,t} + \eta_{i,t}$$

Standard errors are calculated using the generalized method of moments. Panel B presents the results from estimating the effect of *IndPolitics* on *Dollars* using 2SLS. Column (1) presents the first stage output and column (2) presents the second-stage estimates. Standard errors clustered by office and year.

t-statistics appear in parentheses and ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Panel A: Simultaneous estimation of SEC Oversight and Industry Political Activity

	<i>Dollars</i> (1)	<i>IndPolitics</i> (2)
<i>IndPolitics</i>	-558.986* (-1.85)	
<i>Dollars</i>		0.001 (0.74)
<i>PctAAER</i>	656.189** (2.17)	0.780*** (2.76)
<i>OfficeIPOs</i>	-393.286 (-0.62)	1.016 (1.18)
<i>OfficeCap</i>	-184.960 (-0.43)	-0.023 (-0.05)
<i>Workload</i>	991.209* (1.73)	-1.008 (-0.92)
$\Delta Supervisor$	-577.240* (-1.78)	
<i>AvgHerf</i>		28.852** (2.25)
Office Effects	Yes	Yes
Year Effects	Yes	Yes
Partial <i>F</i> -Statistic for $\Delta Supervisor$	2.94	
Partial <i>F</i> -Statistic for <i>AvgHerf</i>		11.55
Observations	110	110
R ²	0.806	0.835

Table 8 (Cont'd): Instrumental Variable Analysis

Panel B: Instrumental Variables Estimation

	<i>IndPolitics</i>	<i>Dollars</i>
	(1)	(2)
<i>AvgHerf</i>	20.980** (2.28)	
<i>IndPolitics</i>		-612.502** (-2.01)
<i>PctAAER</i>	0.880** (2.51)	701.941* (1.83)
<i>OfficeIPOS</i>	-0.075 (-0.14)	-130.014 (-0.36)
<i>OfficeCap</i>	0.609 (1.29)	-290.897 (-0.54)
<i>Workload</i>	-0.323 (-1.06)	920.600 (0.70)
Office Effects	Yes	Yes
Year Effects	Yes	Yes
Partial <i>F</i> -Statistic for <i>AvgHerf</i>	11.15	
Observations	110	110
R ²	0.901	0.794

Table 9: Industry-Level Outcomes

This table presents the results from estimating the relationship between SEC oversight capacity and industry-level outcomes. The dependent variables are calculated by first estimating firm-level regressions on the full set of firm-specific controls plus officefixed effects. I subsequently regress these office fixed effects on the office-level variables. All variables are as defined in Appendix A. t-statistics appear in parentheses and are calculated using standard errors clustered by office and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	<i>AbsDA_MJ</i>	<i>Restatement</i>	<i>Fraud</i>	<i>Spread</i>	<i>Illiquidity</i>
	(1)	(2)	(3)	(4)	(5)
<i>Capacity</i>	-2.174*** (-2.90)	-0.011*** (-3.49)	-0.009*** (-3.46)	-0.031 (-0.81)	-0.009 (-0.93)
<i>IndPolitics</i>	-0.712* (-1.76)	-0.000 (-0.62)	-0.000 (-0.25)	-0.004 (-0.27)	-0.003** (-2.28)
<i>PctAAER</i>	3.323*** (6.13)	0.004*** (4.18)	-0.003 (-0.88)	0.010 (0.29)	-0.016 (-1.26)
<i>OfficeCap</i>	2.620*** (7.20)	-0.011* (-1.83)	-0.004* (-1.67)	0.085 (1.63)	-0.029 (-1.40)
<i>OfficeIPOs</i>	1.857* (1.77)	-0.000 (-0.44)	0.005** (2.17)	0.170 (1.26)	0.015 (1.61)
Year Effects	Yes	Yes	Yes	Yes	Yes
Observations	110	110	110	110	110
R ²	0.652	0.899	0.655	0.970	0.811

Table 10: Robustness Tests

This table presents the results from three robustness 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. And, Panel C presents the results when financial services firms are excluded from the sample. In each panel, columns (1), (4), and (5) are estimated using OLS with standard errors clustered by office and year. Columns (2) and (3) are estimated using a random effects probit model with standard errors clustered by firm. All variables are as defined in Appendix A. t-statistics appear in parentheses and ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Panel A: Lagged Oversight

	<i>AbsDA_MJ</i>	<i>Restatement</i>	<i>Fraud</i>	<i>Spread</i>	<i>Illiquidity</i>
	(1)	(2)	(3)	(4)	(5)
<i>SEC_Capacity</i>	-1.036 (-0.79)	-0.317 (-1.48)	0.022 (0.18)	0.005 (0.16)	-0.005 (-1.08)
<i>Size</i>	-0.130 (-0.18)	0.110* (1.76)	0.074*** (4.39)	-0.164*** (-2.92)	-0.009 (-1.33)
<i>BM</i>	-0.229 (-0.25)	0.227* (1.76)	-0.142* (-1.89)	-0.062 (-0.84)	-0.004 (-0.30)
<i>Leverage</i>	4.371*** (3.21)	0.203 (0.63)	0.117 (1.06)		
<i>Firm_Age</i>	1.519*** (3.13)	-0.012* (-1.69)	-0.003 (-1.61)		
<i>ROA</i>	1.497 (0.57)	-0.348 (-1.12)	0.006 (0.04)		
<i>Ret</i>	0.066 (0.32)	-0.034 (-0.53)	0.061 (1.51)		
<i>Financing</i>	-0.257 (-0.45)	-0.005 (-0.04)	0.178*** (3.17)		
<i>Acquisition</i>	-0.508* (-1.81)	0.091 (0.90)	0.016 (0.23)		
<i>1/Price</i>				0.207 (0.65)	0.036 (0.87)
<i>Turnover</i>				-0.021*** (-3.34)	-0.001* (-1.87)
<i>Volatility</i>				2.174*** (5.42)	0.058 (0.94)
<i>Beta</i>				-0.163*** (-4.27)	-0.009 (-1.02)
Year Effects	Yes	Yes	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes	Yes	Yes
Observations	26,055	26,055	26,055	26,055	26,055
R ²	0.493	0.364	0.049	0.752	0.478

Table 10 (Cont'd): Robustness Tests

Panel B: Placebo Tests

	<i>AbsDA_MJ</i>	<i>Restatement</i>	<i>Fraud</i>	<i>Spread</i>	<i>Illiquidity</i>
	(1)	(2)	(3)	(4)	(5)
<i>SEC_Capacity</i>	-0.072 (-0.21)	-0.115 (-0.82)	0.115 (1.49)	-0.000 (-0.18)	0.003 (0.48)
<i>Size</i>	-0.220 (-0.36)	0.163*** (2.75)	0.075*** (4.66)	-0.001* (-1.94)	-0.003 (-0.62)
<i>BM</i>	-0.403 (-0.49)	0.192 (1.58)	-0.141** (-2.05)	-0.000 (-0.37)	-0.007 (-0.67)
<i>Leverage</i>	4.586*** (3.22)	0.256 (0.83)	0.065 (0.64)		
<i>Firm_Age</i>	0.744 (1.01)	-0.012* (-1.83)	-0.004** (-2.10)		
<i>ROA</i>	2.085 (0.88)	-0.360 (-1.16)	-0.104 (-0.80)		
<i>Ret</i>	0.200 (1.41)	-0.005 (-0.10)	0.065* (1.72)		
<i>Financing</i>	-0.247 (-0.55)	0.050 (0.55)	0.146*** (2.76)		
<i>Acquisition</i>	-0.436** (-1.99)	0.106 (1.14)	-0.009 (-0.13)		
<i>I/Price</i>				0.002 (1.02)	0.038 (1.37)
<i>Turnover</i>				-0.000*** (-5.55)	-0.002*** (-4.69)
<i>Volatility</i>				0.309*** (11.62)	2.330*** (5.29)
<i>Beta</i>				-0.003*** (-7.88)	-0.018** (-2.01)
Year Effects	Yes	Yes	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes	Yes	Yes
Observations	29,142	29,142	29,142	29,142	29,142
R ²	0.480	0.384	0.047	0.807	0.541

Table 10 (Cont'd): Robustness Tests

Panel C: Excluding Financial Services Firms

	<i>AbsDA_MJ</i> (1)	<i>Restatement</i> (2)	<i>Fraud</i> (3)	<i>Spread</i> (4)	<i>Illiquidity</i> (5)
<i>SEC_Capacity</i>	-1.681* (-1.66)	-0.288 (-1.40)	-0.393*** (-3.34)	-0.041*** (-4.19)	-0.004*** (-5.58)
<i>Size</i>	-0.286 (-0.50)	0.174*** (2.74)	0.057*** (3.29)	-0.142*** (-2.72)	-0.008* (-1.78)
<i>BM</i>	-0.573 (-0.74)	0.226* (1.74)	-0.186** (-2.24)	-0.032 (-0.43)	-0.008 (-0.90)
<i>Leverage</i>	4.861*** (3.87)	0.247 (0.74)	-0.039 (-0.34)		
<i>Firm_Age</i>	0.735 (1.15)	-0.014* (-1.87)	-0.002 (-1.14)		
<i>ROA</i>	2.679 (1.28)	-0.344 (-1.06)	0.015 (0.11)		
<i>Ret</i>	0.117 (1.04)	-0.006 (-0.11)	0.063 (1.61)		
<i>Financing</i>	-0.292 (-0.71)	0.062 (0.65)	0.145*** (2.58)		
<i>Acquisition</i>	-0.403** (-2.23)	0.101 (1.06)	-0.008 (-0.11)		
<i>1/Price</i>				0.308 (1.40)	0.035 (1.57)
<i>Turnover</i>				-0.021*** (-4.07)	-0.001** (-2.22)
<i>Volatility</i>				2.160*** (6.40)	0.051* (1.81)
<i>Beta</i>				-0.168*** (-5.60)	0.001 (0.28)
Year Effects	Yes	Yes	Yes	Yes	Yes
Office Effects	Yes	Yes	Yes	Yes	Yes
Firm Effects	Yes	Yes	Yes	Yes	Yes
Observations	26,712	26,712	26,712	26,712	26,712
R ²	0.477	0.384	0.049	0.774	0.594

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