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Do the SEC's Enforcement Preferences Affect Corporate Misconduct?

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Abstract

Recent frauds have questioned the efficacy of the SEC's enforcement program. We hypothesize that differences in firms' information sets about SEC enforcement and constraints facing the SEC affect firms' proclivity to adopt aggressive accounting practices. We find that firms located closer to the SEC and in areas with greater past SEC enforcement activity, both proxies for firms' information about SEC enforcement, are less likely to restate their financial statements. Consistent with the resource-constrained SEC view, the SEC is more likely to investigate firms located closer to its offices. Our results suggest that regulation is most effective when it is local.

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1.0 Introduction

In this paper, we investigate whether the Securities and Exchange Commission's (SEC) enforcement preferences influence a firm's likelihood of committing a violation. In the last year or so, the SEC has been excoriated for its failure to detect several high profile frauds such as those perpetrated by Bernard Madoff and Sir Allen Stanford. These recent failures come on top of the unprecedented increase in the number of firms that misrepresented financial statements in the last decade. In its defense, the outgoing SEC Commissioner, Linda Thomsen, laments the severe resource constraints faced by the SEC in investigating potential miscreants (Thomsen 2009). Surprisingly, there is little empirical work studying the impact of resource limitations facing the SEC and the overall effectiveness of the SEC's enforcement program.

In contrast to the meager work on SEC enforcement, there is a relatively large literature that studies crime at the individual level. This literature finds that whereas the benefits of committing a crime are clear, there is considerable uncertainty related to the expected cost of committing a crime (Sah 1987). Such uncertainty stems from the absence of publicly available data that can help the criminal accurately determine the probability of getting caught or the expected punishment, if caught. Consequently, the expected costs of committing a crime are essentially subjective to the criminal and hence criminals differ substantially in their perceived costs of engaging in criminal activity (see Glaeser, Sarcedote and Scheinkman 1996).

In much the same way, firms potentially differ in their perceived cost of committing a violation. Differences among firms in their perceived probability of being targeted by the SEC and the associated cost thereof affect their decision to violate laws. The "differentially informed criminal" hypothesis predicts that firms that are well informed about the SEC's enforcement activities are less likely to commit a violation. Aside from the "differentially informed

criminal," an SEC that is constrained in terms of time, effort and enforcement budgets is more likely to initiate investigations that are cheaper to implement. Assuming that the SEC's constraints and preferences are observed by all firms, the "constrained cop" hypothesis suggests that firms consider these constraints before deciding to commit a violation.

We study violations in firms' financial reporting, i.e., instances when firms violate GAAP. Our sample consists of 331 firms that announced income-decreasing restatements of their books of accounts due to accounting irregularities from January 1997 to June 2002 and all other public firms that did not restate over this period. We investigate whether firms' propensity to adopt aggressive accounting practices that result in income decreasing restatements is associated with firms' information sets about the SEC's activities and constraints.

To test the "differentially informed" criminal hypothesis we use two empirical proxies that capture differences across firms in their information sets related to the SEC's enforcement process: (i) distance to the SEC office; and (ii) past SEC enforcement activity in their county. The "differentially informed" criminal hypothesis predicts that the propensity of firms in the county to restate their accounts is positively (negatively) associated with the distance of firms to the SEC office (the number of past SEC enforcement actions in the county). Motivated by recent findings that geographic proximity is associated with informational advantages, we posit that the physical distance to SEC offices proxies for differences in their awareness of SEC activities.[1] Proximity may provide managers or their legal counsel access to information about the SEC's policing function, that is, potential issues the SEC is concentrating on and the SEC's opinion regarding when a transaction crosses the line from aggressive reporting to potential fraud. Transmission of sensitive and deliberately ambiguous information about the SEC's policing

[1] Informational advantages arising from geographic proximity have been documented in portfolio decisions (see Coval and Moskowitz 2001, Ivkovich and Weisbenner 2005), in the forecasting accuracy of analysts (see Malloy 2005), in the transfer of knowledge (see Audretsch and Stephan 1996 and Audretsch and Feldman 1996), and in the investment decisions of firms (see Kedia, Panchapagesan and Uysal 2005).

function is likely to be facilitated by social, civic, or business interactions between managers and geographically proximate SEC enforcement officials. Firms also learn about the SEC's activities by observing SEC investigations of other firms in the neighborhood. Therefore, we use past SEC activity in the county, measured as the number of firms in the county that were subject to enforcement actions in the past, as our second proxy for firms' exposure to and hence knowledge of SEC activities. However, past SEC enforcement actions can potentially also capture the number of criminals or "bad apples" in the county that have already been busted, leading thereby to a mechanically negative relation between past SEC activity in the county and propensity of the firms in the county to restate their accounts.

Our proxy for the SEC's resource constraints is based on a recent GAO report which suggests that SEC officials view travel outside their geographic jurisdiction as a significant cost affecting the efficient allocation of their investigative resources (GAO 07-830, 2007, 16). Geographically closer investigations are likely to be efficient not only because they are cheaper but also because the SEC is likely to know more about proximate firms. Proximity facilitates interactions between SEC officials and firms' executives that might inform the SEC about potential misconduct. Moreover, the SEC relies on tips about financial reporting irregularities to detect violations. Employees of proximate firms are more likely to be aware of the SEC and its detection policy, and are therefore more likely to blow the whistle about problems than employees of distant firms.

If a resource-constrained SEC is more likely to initiate investigations closer to its offices then firms anticipating such SEC behavior are less likely to commit a violation if they are located closer to the SEC. Therefore, the "constrained cop" hypothesis, along with the "differentially

informed criminal hypothesis" discussed previously, predicts that firms that are located closer to the SEC's offices are less likely to commit a violation.[2]

Though the impact of the "constrained cop" hypothesis is observed via firm decisions, one can test this hypothesis directly as well. To do so, we use a second dataset – that of SEC enforcements compiled by Karpoff, Lee and Martin (2008 a,b). The SEC does not target all firms that misreport and this dataset allows us to examine how potential resource constraints impact SEC enforcement relative to all firms that announce a restatement. In particular, our "constrained cop" theory implies that a resource constrained SEC is (i) more likely to investigate firms located closer to its offices; and (ii) more likely to investigate firms where accounting problems are public and visible. Prior media attention may have not only identified the more severe cases of misreporting, but may also have unearthed evidence making it cost effective for the SEC to pursue visible cases.[3]

As our empirical proxies rely on distance to the SEC offices and past and current SEC enforcement activity in the region where the firm is located, our basic unit of analysis is the county. Firms are assumed to be located in the county where they are headquartered. To control for the distribution of corporate headquarters, we measure the distribution of restating firms relative to the distribution of all public firms. We then examine whether the misreporting deviation, defined as the difference in the county's share of restating firms relative to the county's share of public firms, is related to proxies of information about the SEC's policing preferences including past SEC activity in the county.

We find evidence consistent with both the posited hypotheses. Consistent with the "differentially informed criminal" hypothesis, we find strong and robust evidence that counties

[2] The SEC offices considered are SEC headquarters in Washington D.C and regional offices located in New York City, NY; Miami, FL; Chicago, IL; Denver, CO; Los Angeles, CA. The analysis is robust to also including district offices in Boston, MA; Philadelphia, PA; Atlanta, GA; Fort Worth, TX; Salt Lake, UT; and San Francisco, CA.
[3] We thank the referee for suggesting this test.

that are geographically proximate to SEC offices (empirically measured as the closer of the SEC's national office or its five regional offices) have lower misreporting deviations. In particular, counties that are not proximate to SEC offices (more than 100 km away) are three and a half times more likely to announce an income decreasing restatement relative to proximate counties.

We also find that counties with higher prior SEC activity, and therefore with a greater knowledge of the regulator's policing function, have lower misreporting deviations. This is consistent with the "differentially informed" hypothesis. However, as mentioned earlier, this evidence is also consistent with the SEC having already busted "bad apples" in the county in the past. In sum, regions located closer to SEC offices and those with greater prior SEC investigations are less likely to violate GAAP.

Consistent with the "constrained cop" hypothesis, counties that are geographically proximate to the SEC's offices are associated with lower misreporting deviations. Firms located closer to the SEC rationally respond to the SEC's preference for investigating proximate firms by avoiding GAAP violations. As corroborative evidence, we find that, relative to all the firms that announce a restatement, the SEC is more likely to investigate firms that are located closer. Thus, in line with the GAO study citing travel as a constraint, the SEC displays a clear preference for investigating firms that are geographically proximate. We also find significant evidence that the SEC is more likely to investigate firms that attract higher media coverage after their restatement announcement. This is consistent with a constrained SEC minimizing costs by investigating firms with known accounting irregularities, as well as, the SEC catering to political pressures stemming from visible reporting violations highlighted by the media.

Our paper underscores the importance of geographically proximate regulators and better dissemination of information regarding SEC enforcement activities. Our results suggest that

effective regulation is best accomplished at the local level and geographical distance potentially constrains the SEC's enforcement efforts. This finding runs counter to the trend of centralized regulation seen in the U.S. and across the world. Contrary to the implications of our paper, the enforcement division of the SEC has recently established a centralized process for reviewing and approving new investigations in March 2007 (see http://www.gao.gov/new.items/d07830.pdf).

The paper also suggests that the SEC's preferences in its enforcement program (such as distance) affect a firm's decision to commit violations. This point is especially important because SEC enforcement is often regarded as a neutral, albeit noisy, proxy for corporate malfeasance. As the SEC's enforcement preferences drive corporate behavior, at a minimum, these preferences need to be understood and documented.

The remainder of the paper is organized as follows. Section 2 discusses the two hypotheses and Section 3 describes the sample and the methodology. Section 4 reports the empirical results and section 5 summarizes additional analyses. Section 6 concludes.

2.0 Hypothesis Development

2.1 The differentially informed criminal hypothesis

Crucial to the SEC's effectiveness is its enforcement program that brings enforcement actions against individuals and firms that violate securities law. If firms differ in their knowledge of SEC activities and ongoing investigations, they are likely to have different beliefs about both the probability of being investigated by the SEC and the penalties associated with such an investigation.

Sah (1991) and Glaeser et al. (1996) propose that a potential criminal's beliefs about the expected costs of committing a violation are essentially subjective to that criminal and hence vary across criminals. Since the decision to engage in illegitimate activities is a function of both

the expected costs and benefits of these activities (see Becker 1968), differences in the perceived cost of SEC enforcement will be reflected in differences in the firms' propensity to commit violations. In particular, firms with greater information about the SEC and therefore with a higher expected cost of SEC enforcement are less likely to commit a GAAP violation. Our first hypothesis, labeled the "differentially informed criminal hypothesis" suggests that proxies that capture differences in information about the SEC's regulatory process will be related to the likelihood of firms committing a violation.

We use two measures that might capture differences in information about the regulatory oversight of the SEC. The first proxy is the physical distance to the SEC and the second proxy is prior exposure to SEC investigations in the neighborhood. The use of physical distance to proxy for regulatory information is motivated by evidence in prior work that information advantages grow with geographic proximity. Coval and Moskowitz (2001) find that mutual fund investments in geographic proximate securities earn positive abnormal returns. Audretsch and Stephan (1996) document the importance of geographical proximity of scientists and their firms for innovative activity. With regard to individual criminal activities, Sah (1991) and Glaeser et al. (1996) discuss how geographic proximity affects the perceived cost of criminal activity.[4]

Proximity to the SEC's office may provide managers access to soft information about current SEC policies that are not explicitly documented. These could encompass the SEC decision criteria to begin an investigation, as well as, SEC views on when aggressive reporting turns into fraud. Such information about the SEC's policing function is inherently sensitive and

[4] A natural follow up question in response to the cited research is why should geographical proximity affect the information set of agents in the current age of information technology and electronic communication? Audretsch (2003) argues that the answer lies in recognizing that there are two types of information: (i) information, such as the price of gold on the New York Stock Exchange, that can be easily codified and has a singular meaning and interpretation; and (ii) information that is vague, difficult to codify and often serendipitously recognized. With electronic communication the marginal cost of transmitting easily codified information does not depend on geographic distance. However, the marginal cost of transmitting tacit knowledge or difficult to codify information increases with distance. We believe that information about accounting policies to account for a transaction, circumstances specific to a firm or set of firms and enforcement activities of the SEC is of the latter kind.

deliberately ambiguous. Geographical proximity is especially helpful in discovering sensitive and ambiguous information that will be disclosed only to close business contacts via casual conversations. In other words, managers who are geographically proximate to the SEC are more likely to be aware how far they can push the envelope in financial reporting. This happens because accountants and general counsel at firms that are geographically proximate (say in New York City rather than in Kansas City) are likely to have easier access to SEC personnel. Moreover, firms located closer to the SEC's office are also more likely to hire accountants and lawyers who have worked at the SEC before, especially if such professionals would rather not relocate to another geographic area (Kedia and Rajgopal 2009). In short, counties proximate to the SEC are likely to be better informed about the costs of regulatory oversight and hence less likely to adopt such practices.

Further, several papers find that the effect of geographic proximity is not linear, i.e., information travels only when agents are located within a distance threshold. Outside this threshold, there is little or no transfer of information. Coval and Moskowitz (2001) report that information transfers are more likely to occur when agents are located within 100km of each other (also see Malloy 2005, and Kedia, Panchapagesan and Uysal 2005). In line with this finding, we examine the effect of proximity to the SEC's office (located within a 100km or not) on firms restatement decisions.

Consistent with Sah (1991) and Glaeser et al. (1996), firms could also learn about regulatory oversight from their prior experience of dealing with the SEC or by observing SEC investigations of other firms in their neighborhood. In particular, if other firms in the county have been subject to SEC investigations in the past, firms are likely to be better informed about the costs of regulatory inquiry. To capture the experience of firms with the regulator, we use the

number of firms in the county that have been subject to SEC enforcement actions in the past, i.e., prior to 1997, the beginning of our sample period of financial restatements.

Our hypothesis, motivated by Becker (1968) and in line with a large literature on the economics of crime, is based on the assumption that agents respond to the costs and benefits of committing crime. However, recent experimental evidence by Gibson, Tanner and Wagner (2010) suggests that there may be "fixed" agents who do not manage earnings despite the economic incentives to do so. This alternate assumption suggests that there may be some firms that either never cheat or always cheat. Further, the SEC imposes non-monetary penalties on CEOs or CFOs found guilty of fraud by banning them from assuming corporate office in the future suggesting that these officers cannot misreport in the future. If there were a fixed number of "bad apples" or crooked firms in the county, then past SEC enforcement is a measure of the extent to which these "bad apples" have been caught. This interpretation mechanically predicts a lower misreporting deviation in the county with higher past SEC enforcement actions. Our hypothesis, on the other hand, is predicated on the premise that every "apple" could potentially become "bad," given the lack of adequate penalties. That is, a firm is potentially less likely to misreport if it learns about SEC enforcement via prior SEC enforcement actions in its county. Unfortunately, we do not have the data to distinguish between these two hypotheses.

2.2 The "constrained cop" hypothesis

The second hypothesis about the SEC's role is labeled the "constrained cop" hypothesis. Under this hypothesis, everyone, including the criminals, knows that the local cops are constrained in some way and have limited resources to make arrests. In the context of financial reporting, the SEC may suffer from constraints related to its enforcement budgets. The press is replete with stories about tightening SEC enforcement budgets before 2002, the last year of

restatements studied here (e.g., see Wall Street Journal 2003). For example, between 1991 and 2001, the number of cases opened by the SEC's Enforcement Division increased by 65% while the staff grew by 27%. This resource constraint faced by the SEC is also clear if we examine SEC budgets in Table 1a which shows that, as a percentage of total stock market capitalization, SEC budgets decline from 1996 to 1999 when they rise again to peak in 2008. In particular, the yearly average of the SEC's budget as a proportion of market capitalization is 0.004% (see column 6). During our sample period 1997-2002, the yearly average is merely 0.0028%. In terms of 2009 constant dollars, the yearly average SEC budget was $677.247 million relative to $476.405 million during our sample period 1997-2002.

We also scale the SEC budget by the total number of publicly listed firms in column 7.[5] The ratio of budget dollars to listed firms is more or less constant till 2001, i.e., for most of our sample period, after which it steadily increases. Two factors are responsible for this trend. First, the number of listed firms falls significantly beginning the year 2001 reflecting the failure of several technology firms. Second, the SEC's budgets have increased after the year 2001 because of the Enron scandal and the Sarbanes Oxley Act. Column 8 displays the President and his political affiliation during the years listed in the sample. However, as the SEC's funding is episodic in that budget increases follow the revelation of major accounting frauds, it is hard to infer any reliable relation between political affiliation and SEC budget authority.

Resource constraints on the SEC will lead to enforcement decisions that are consistent with these constraints. A recent GAO report on the SEC's enforcement process suggests that SEC officials view travel outside their geographic jurisdiction as a significant cost affecting the efficient allocation of their investigative resources (GAO 07-830, 2007, 16). The SEC

[5] The SEC budget requirements are likely to be a function of the number of traded firms along with the market capitalization. This is more likely after 2002, when The Sarbanes Oxley Act imposed the requirement that the SEC review a publicly traded firm every three years. However, this requirement does not strictly apply to our sample period, 1997-2002.

investigates firms closer to its offices because it takes less time and travel dollars to investigate a firm proximate to the SEC office and to maximize the number of cases investigated, given its time and budget constraints.

Moreover, the SEC is likely to be more knowledgeable about firms located closer to its offices. This is because closer interactions between the SEC and the executives of the firm can potentially inform the SEC about potential misconduct in proximate firms. Finally, note that the SEC relies on tips about financial reporting irregularities to detect misreporters. Employees of proximate firms are more likely to be aware of the SEC and this detection policy, and are therefore more likely to blow the whistle about problems than employees of distant firms.

A resource constrained SEC is also more inclined to investigate firms that have generated media attention. Extensive media scrutiny has likely identified the more egregious cases of misreporting, and unearthed evidence that will facilitate the SEC's case thus making the targeting of the visible firm more cost effective. Pursuing these visible cases has the added benefit of allowing the SEC to deal with political allegations that the SEC is lax in prosecuting known cases of corporate misconduct.

In sum, the "constrained cop" hypothesis predicts that the concentration of misreporting in a county (i) is negatively associated with the distance between the SEC offices and county where the firm is located; and more directly that the concentration of SEC enforcement actions issued in a county during the sample period 1997-2002; (ii) is decreasing in the distance to the SEC offices; and (iii) is increasing in media attention.

3.0 Data, Methodology and Geographical Clustering

We use two samples in the paper to test our hypotheses. The first sample, meant to capture potential violators, is the sample of firms that announce a restatement. The second

sample consists of firms that were targeted by the SEC via enforcement actions and is meant to capture policing activity against select violators. The restatement sample is described in detail in section 3.1 whereas SEC enforcement data are described in section 3.2.

3.1 The restatement sample

To construct our restatement sample, we begin with the list of restating firms compiled by the General Accounting Office (GAO 2002) that identifies 919 announcements of accounting restatements by 845 firms over January 1997 to June 2002. These announced restatements were due to alleged accounting irregularities resulting in material misstatements of financial reports and have been examined in several prior studies like Agrawal and Chadha (2005), Agrawal and Cooper (2006, 2007), Burns and Kedia (2006), Desai, Hogan and Wilkins (2006), Hribar and Jenkins (2004), and Srinivasan (2005).[6]

However, as prior work has shown that certain restatements in the GAO report are attributable to honest accounting mistakes as opposed to aggressive accounting (e.g., Palmrose and Scholz 2004, Agrawal and Cooper 2007), in this paper, we consider a conservative sample of restating firms to closely capture deliberate misreporting. To construct such a sample, we follow Agrawal and Cooper (2007, page 8) and exclude restatements with (i) positive impact on net income (as well as those for which we do not have any information on the magnitude of restatement); and (ii) restatements with no effect on net income. Agrawal and Cooper (2007) argue that auditors view restatements that do not decrease net income as less severe.[7] Nevertheless, section 5.2 and Table 8 report results with other measures of restatements.

[6] Note that the list specifically excludes routine restatements like those due to mergers and acquisitions, discontinued operations, stock splits among others.

[7] There are other potential ways to measure misreporting. For instance, we could have eliminated all restatements made to comply with SAB 101 and EITF guidelines. Whereas Palmrose and Scholz (2004) and Agrawal and Cooper (2007) argue that such restatements are likely to be less serious and technical in nature, Rountree (2003) finds that the average stock price reaction to SAB 101 restatements is negative. Our approach of retaining only

We further impose the restriction that restating firms have at least one year of sales in the period 1997 to 2001 and have data on company headquarters in COMPUSTAT. All non-restating firms with at least one year of sales over the period 1997 to 2001 and with data on the location of headquarters in COMPUSTAT are also included. We exclude firms whose headquarters are located outside the U.S. due to data unavailability. We hand-collect data on the impact of the restatement on net income. We were able to get data on the magnitude of restatement for 473 restatements.[8] We exclude restatements with a positive impact on net income (about 13%) or those with a zero impact on net income (about 14.6%). Our final sample consists of 331 income-decreasing restating firms with an average annual impact on net income that is about -20.57% of sales. Approximately 11,393 non-restating firms are included in our sample. The spatial unit of analysis over which we measure the concentration of misreporting is the county. Of the total 3,141 counties reported in the U. S. Census Bureau Gazetter, 985 or 31% of counties are home to at least one corporate headquarters and are therefore included in our sample.

3.2 The SEC investigation process and the SEC enforcement data

The data on SEC enforcement actions are obtained from and have been described in detail in Karpoff, Lee and Martin (2008 a, b) referred to as KLM. There are several distinct

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income-decreasing restatements is a compromise between these two extreme positions. Including only income decreasing restatements eliminates most of the SAB101 and EITF restatements. Only 37 SAB 101 income-decreasing restatements remain in our sample and the average overstatement of income for these cases is a non-trivial 11.21% of sales. Our proxy based on the income-effect of restatement is likely to capture misreporting better than other proxies such as the number of quarters restated and initiator of restatements that have been proposed in the literature. As an additional robustness check, we deleted all SAB 101 and EITF income-decreasing restatements and reran our tests but the reported results remain unchanged.

[8] The data is collected from the restatement announcement when available. For others, the data is obtained from the amended 10-Ks filed with the SEC. Data on the size of the restatement could not be obtained for some firms due to several reasons. First, some firms did not report the impact of the restatement on income. Second, some firms include events (such as restructuring charges and other one-time charges) other than restatements in the amended earnings number, and this makes it difficult to isolate the impact of the restatement on income. Third, some firms did not file an amended 10-K. We also exclude outlier observations for which the size of the restatement was more than 200% of restated net income.

steps in the SEC investigation process that are depicted in Figure 1 (reproduced from KLM). The public disclosure of the violation, referred to as the trigger date, could arise from a variety of sources. Examples include self-disclosures of malfeasance, restatements, auditor departures, unusual trading, probes by other federal agencies such as the Department of Defense and Environmental Protection Agency, along with delayed SEC filings, management departures, whistleblower charges, and routine reviews by the SEC. The SEC subsequently conducts an informal but confidential investigation of the target firm that may develop into a formal and public investigation if questionable activity is suspected or otherwise the SEC may drop the case.

The SEC's decision to file charges against the firm marks the beginning of the regulation period. As discussed in KLM, the beginning of the regulatory period is defined as the very first public release indicating that the SEC and/or DOJ has filed an action.[9] In contrast to the informal investigation, the beginning of the regulatory period is (i) publicly observed; and (ii) available for all investigations in the KLM data set. Hence, we use the beginning of the regulation period to date stamp the SEC involvement for our analyses.

Note that the timing of the various events depicted in Figure 1 is the most likely timing of these events constructed from publicly available data. However, unavailability of confidential data related to the SEC's informal investigation opens the possibility that these events could have occurred in a different order. For instance, a trigger date based on a restatement announcement by a firm could precede the start of an SEC investigation. Or, an SEC informal enquiry unearths issues that eventually cause the firm to restate. In this case, informal SEC investigation will precede the restatement or the trigger date.

[9] According to KLM these releases consist of documents from the following three sources: (i) SEC: litigation release, Securities Act Release, Exchange Act Release, Investment Company Act Release, Investment Adviser Act Release, Public Utility Holding Company Act Release, or Administrative Law Judge Release; and (ii) DOJ: press release; and (iii) District Courts: public court records.

The SEC investigation can proceed in one of three ways based on the gravity of the perceived irregularity. If the SEC feels that the culpable firm or the individual has committed a criminal offence, the SEC turns the investigation over to the Department of Justice (DOJ). If the SEC chooses to handle the disciplinary action itself, the SEC pursues the investigation through administrative or civil actions. A civil action is filed in federal court and an administrative action is the documentation of a discrepancy with a firm's accounting policy that does not warrant a civil suit action. The SEC gives a secondary designation called an Accounting and Auditing Enforcement Release (AAER) to all proceedings that involve an accountant or an auditor. To be clear, we use all SEC enforcements, not just AAERs, as our measure of the SEC's policing activity.

The SEC enforcement data, graciously provided by KLM, covers the years 1976-2009. We use SEC enforcements with a regulatory begin date that overlaps with the period for which we have our restatement data, i.e., 1/1997-6/2002 to capture SEC activity over this period. We have 132 SEC enforcement actions on firms with publicly available data over this time period, of which 39 overlap with our income decreasing restatements. We use SEC enforcements with regulatory begin dates before 1997 to proxy for the prior policing intensity of the SEC in the county.

3.3 Data limitation and sample selection issues

Based on the discussions in sections 3.1 and 3.2, several aspects of the sample of income decreasing restatements and SEC enforcement actions need to be clarified. First, all restating firms are not pursued by the SEC.[10] Had we restricted our sample of restating firms to only

[10] For our sample of 331 income decreasing restatements, only 23% were subject to SEC enforcement actions. Some of these SEC enforcements were initiated after the period under study i.e., 1997 to June 2002 and have not been included in our tabulated results.

those with SEC enforcement actions, we could not have examined whether the SEC chooses to pursue violators located closer to its offices.

Second, to test whether resource constraints of the SEC impact its enforcement we would ideally like to study all investigations undertaken by the SEC. However, as mentioned in the discussion of the SEC investigation process, data on informal investigations that did not eventually convert into formal SEC enforcement are not publicly available. [11] Hence, we are constrained to rely on the publicly available SEC enforcement actions to proxy for the SEC's policing efforts. Unfortunately, these enforcement actions represent the end product of such investigations as opposed to the initial investigations themselves.[12]

Third, the firms included in both samples, restating firms, as well as, the SEC targets are not random firms. To be labeled as a "misreporting" firm that is investigated by the SEC in our sample, all the following conditions must exist: (i) the firm violates GAAP (or makes a mistake); (ii) the firm decides to restate conditional on a GAAP violation or error; (iii) the SEC decides to investigate conditional on a restatement; (iv) based on the investigation, the SEC chooses to issue an enforcement action.

We acknowledge that we have not modeled (i) whether all firms that violate GAAP restate their books; (ii) how the SEC picks potential targets for investigation; and (iii) what firms does the SEC informally investigate but does not reveal their names to the public. Although these are very interesting questions, we are unable to come up with satisfactory data to identify

[11] We contacted the SEC and asked for data on the comment letters that they send out to the firms. These comment letters precede a formal investigation and an eventual enforcement action. We were informed that this data is confidential for our sample period, 1997-2002. However, comment letters are available after 2004 (see http://sec.gov/answers/edgarletters.htm).

[12] Though the date of the first involvement of the SEC is usually unavailable, in some cases the firm voluntarily discloses the presence (as opposed to the beginning) of an informal SEC investigation. This announcement date has been collected by KLM (2008 a, b) from press releases when available. In untabulated results we examine these voluntary disclosed events. If we were to assume that, for all cases where the firm does not reveal a SEC investigation date, the first date of SEC involvement is the beginning of the regulation period, we find that in about 89% of such cases the SEC investigation date follows the public announcement of the restatement. However, given the limitations of the data underlying this analysis, this statistic should be interpreted with caution.

GAAP violators who are either not caught or who the SEC chooses to investigate informally. However, to better appreciate the effect these selection criteria have on the firms in the sample, Table 1b reports the characteristics of such firms. We find that income decreasing restating firms are similar to non-restating firms in COMPUSTAT in size, leverage and growth opportunities. However, income decreasing restaters are older and more visible in that they are more likely to belong to the Fortune 500. When examining the characteristics of the firms that the SEC targets with regulatory action, we find that they are larger in size relative to all other income decreasing restaters. This is consistent with the perception that the SEC is more likely to target large firms.

3.4 Geographical concentration of restatements

To capture the degree of geographical concentration of income-decreasing restatements we estimate at the county level a measure of misreporting deviation defined as

$$misreporting\_deviation = \quad x_i - y_i \qquad (1)$$

$$x_i = County's\ share\ of\ \ restating\ \ firms = \frac{Number\ of\ \ sample\ \ firms\ in\ county_i\ that\ restated\_as\_per\_GAO}{Total\ \ restating\ \ firms\_as\_per\_GAO}$$

and

$$y_i = County's\ share\ of\ \ public\ \ firms = \frac{Number\ of\ \ firms\ headquartered\ in\ county_i}{Total\ \ firms\ in\ Compustat}$$

A positive (negative) misreporting deviation suggests that the county's share of misreporting firms is higher (lower) than the county's share of all public firms. Positive deviations indicate greater geographic concentration in misreporting. The assumption underlying the deviation measure is that misreporting should be randomly distributed. In other words, a county that accounts for 10% of all public firms should also account for 10% of restatements on average. Table 2 reports the distribution of misreporting at the state level (rather

than county level) for brevity. California, Massachusetts and Washington are the states with the greatest positive deviations. In particular, California accounts for 17.56% of all public firms, but 19.78% of all restating firms. On the other hand, Connecticut, New York, and New Jersey have the lowest deviations among states containing at least 2% of all corporate headquarters. That is, the share of restatements in these states is less than their share of public firms. For example, New York accounts for 9.27% of all public firms, but only 7.52% of restating firms.

3.5 Estimating distance

To estimate the distance between the counties and SEC offices, we use the latitude and longitude of counties and SEC offices obtained from the U. S. Census Bureau Gazetteer.[13] SEC offices considered are the SEC headquarters in Washington D.C and regional offices located in New York City, NY; Miami, FL; Chicago, IL; Denver, CO; Los Angeles, CA.[14] The distance to SEC offices is the lower of the distance to the SEC headquarters and the distance to regional headquarters.

To give the reader some flavor for the counties and distances involved, Table 3 reports data about the distance of firms located in the top 20 counties (sorted by number of firms headquartered in the county) to the SEC headquarters and the regional SEC office. This table raises a potential question about whether the analysis to follow ought to treat as endogenous

[13] The Haversine Formula is used to calculate the distance d_{12} between counties 1 and 2. Distance d_{12} is calculated as $d_{12} = R \times 2 \times \arcsin(\min(1, \sqrt{a}))$ where R is the radius of the earth ($\approx$ 6378 kilometers) and $a = (\sin(dlat/2))^2 + \cos(lat1) \times \cos(lat2) \times (\sin(dlon/2))^2$. In the above expression $dlat = lat2 - lat1$ and $dlon = lon2 - lon1$. Lat1 and lon1 are the latitude and longitude of county 1, and lat2 and lon2 are the latitude and longitude of the SEC office.

[14] In 2007, the SEC elevated its district offices located in Boston, MA; Philadelphia, PA; Atlanta, GA; Fort Worth, TX; Salt Lake, UT; and San Francisco, CA to regional offices and gave them responsibilities that are similar to their existing regional offices (See http://www.sec.gov/news/press/2007/2007-59.htm). As these were not regional offices during the period under study these offices have not been taken into account to calculate the distance to the SEC. For robustness we have estimated a model where we take district offices into account in our calculation to the nearest SEC offices. The results are untabulated and materially similar to those reported in the paper.

where the SEC chooses to locate its regional headquarters. However, note that the last SEC office (see http://www.sechistorical.org/collection/papers/1930/1935_SEC_Reg_Admin.pdf.) was established in Salt Lake City in 1954. As the opening of a new regional SEC office is a rare event and we have observed no change in SEC branch structure for nearly 40 years, SEC office location is for all practical purposes exogenous to the incidence of restatements in 1997 to 2002 period that we study.

4.0 Empirical Results

We begin by reporting differences in the characteristics of counties with a high (Group 3) and low (Group 1) misreporting deviation in Table 4. The average distance from SEC offices is 254 km for Group 1 which is 54% less than the mean distance of 392 km for the group with the largest misreporting deviation. The difference is statistically significant at the 1% level (t-statistic = 2.59). A similar picture emerges when we consider un-tabulated median distances.

As information advantages are unlikely to be linear in distance we define counties as being geographically proximate to the SEC if they are within 100km of the SEC (see Coval and Moskowitz. 2001). As seen in Table 4, about 49.4% of counties in the group with the smallest misreporting deviation are geographically proximate to the SEC. In comparison, only 27% of counties with the largest misreporting deviation are geographically proximate to the SEC, and the difference between these proportions is significant at the 1% level (t-statistic = 3.23).

The second proxy for potential differences in information about SEC oversight is the extent of past SEC activity in a county. As we want to use SEC enforcement before our sample begins its misreporting, we use all SEC enforcements filed before 1997 to construct our measure

of past policing activity.[15] Table 4 shows that counties with the lowest misreporting had 1.08 firms subject to SEC enforcements prior to 1997. This is not significantly higher than the 0.64 firms subject to SEC enforcements for counties with the highest misreporting deviations. This lack of significance in the univariate analysis could be due to the inability of this analysis to control for multivariate characteristics that might affect misreporting. Though there is no significant difference between group 1 and group 3 in past SEC activity, group 1 or the lowest misreporting group has significantly higher past SEC activity than for group 2. The t-test for this difference is 7.29 and suggests that counties with the lowest misreporting deviation have been exposed to significantly higher past SEC activity than the average county.

4.1 Controlling for county characteristics

In this section, we control for demographic characteristics of the counties and their potential influence on the misreporting intensity of counties. We proxy for county size by (i) land area in square miles obtained from the U.S. Census Bureau; and (ii) the number of corporate headquarters in the county. We control for county growth by including (i) the percentage change in population over 1990-2000; (ii) the percentage change in housing over 1990-2000; (iii) the number of new housing starts in 2000; (iv) the percentage change in private non-farm new establishments over 1990-1998; and (v) the percentage change in personal income over 1990-1998.[16] Misreporting is likely to be higher when investor optimism is high and at the end of a

[15] In particular, we use the beginning of the regulation period provided by Karpoff et al (2008a) to construct our variable, SEC enforcements prior to 1997. Because we would like to capture whether non-target firms learn from observable SEC actions, we want to identify the date when the SEC involvement is public i.e. the beginning of the regulation period. The fact the SEC might have been confidentially investigating the firm prior to this period is not relevant as other firms cannot learn about SEC practices from confidential enquires.

[16] Note that the different time periods over which county characteristics data is collected is driven by data availability intersected with the restatement sample period that runs from 1997-2002. Further, 1990, the other year for which data on new housing starts are available, was not proximate in time to the restatement announcements

rapid growth phase (see Bolton, Scheinkman, and Xiong 2003, Bebchuk and Bar-Gill 2003). However, we see little difference in these growth proxies across groups of counties ranked by misreporting deviations (see rows 5-11 in Table 4).

We introduce stock option usage in a county as a potential control variable for our analyses because some evidence (e.g., Bergstresser and Philippon 2006, Burns and Kedia 2006, and Cheng and Warfield 2005) suggests a positive association with the propensity to misreport although other evidence does not (e.g., Erickson, Hanlon and Maydew 2006, and Hribar and Nichols 2008). As we do not have details of compensation for CEOs of firms in each county, we control for potential differences in the use of stock options across counties by including a proxy for stock option usage in the region. In particular, we use the fraction of total state employees working in industries that are characterized by high option usage, referred to as "high incentive" industries.[17]

We use the 1997 Economic Census data to obtain employment in different sectors of the state. Industries are categorized as "high incentive industries" if they belonged to the following four NAICS codes: 334 (computer and electronic product manufacturing), 514 (information services and data processing services), 5415 (computer system design and related services) and 5417 (scientific research and development services).[18] The choice of these NAICS codes is based on prior evidence of industry patterns in the grant of incentives (See Core and Guay 2001,

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sample covering 1997-2001. Further, introducing population levels instead of changes does not affect the reported results.

[17] To respond to concerns that including the option usage variable may "throw the baby out with the bathwater," we estimated our models without the option usage variable and found no change in the reported inferences.

[18] Note that we have switched from SIC codes to NAICS here because the 1997 Economic Census data reports industry data using NAICS codes. NAICS is the North American Industry Classification System. The data is available on the website www.census.gov.

and Ittner, Lambert, and Larker 2001).[19] We find no evidence that the proxy for options usage is related to misreporting deviations of the counties (row 11 of Table 4). This lack of any effect is possibly because we have a noisy proxy for option usage.

We also control for state judicial quality. Though firms found to have violated GAAP rules are subject to federal securities laws, the integrity and quality of the state judicial system may potentially dissuade firms from adopting aggressive accounting practices. To examine if state judicial quality impacts the propensity of firms to misreport, we use the "overall state ranking" for the state reported in the 2001 State Liabilities Ranking Study conducted for the U.S. Chamber of Commerce (see Kahan 2004).[20] We also include firm size as a control variable because proxies for firm-specific incentives to misreport are often correlated with firm size.

The results from estimating a multiple regression model to determine the effect of regulatory oversight on the misreporting intensity are presented in model (1) of Table 5. The dependent variable is the misreporting deviation for the county for all columns. In model (1), we find that distance to the SEC is significantly positively related to the concentration of misreporting in counties. Thus, counties that are farther away from the SEC are likely to have higher misreporting concentration.[21]

As mentioned above, several recent papers that examine the role of geographic distance stress the importance of proximate agents. To examine this, we include a proximity dummy that takes the value of one if the county is within 100 km of the nearest SEC office in model 2. We

[19] Core and Guay (2001) find the maximum usage of options in software, pharmaceuticals and computers. Ittner, Lambert and Larker (2001) find larger option grants in new economy industries, i.e., computers, software, semiconductor manufacturing, telecommunications, networking and internet.

[20] This study was conducted by Harris Interactive group and is based on interviews with counsel and senior litigators. The overall rankings summarize states' rankings on (i) overall treatment of tort and contract litigation, (ii) treatment of class action law suits, (iii) punitive damages, (iv) timeliness of summary judgment, (v) discovery, (vi) scientific and technical evidence, (vii) judge's impartiality, (viii) judge's competence, (ix) juries' predictability, and (x) juries' fairness.

[21] We find that firm size is not statistically significant. Hence, we drop firm size from the other three models reported in Table 5.

find that the coefficient of the proximity dummy is negative and significant. The coefficient estimate is not only statistically significant but also points to economic significance. In particular, if a county becomes proximate to the SEC then such proximity is associated with a reduction in income decreasing restatements by 0.1 firms. Since the unconditional incidence of income decreasing restatements is 0.028 in a county, the estimated coefficient implies that counties that are not proximate are 3.5 times more likely to announce an income decreasing restatement.[22] This impact of distance on restatements is large but not surprising because the tabulated effect is not the incremental effect of decreasing a unit of distance but rather the effect of moving a county that was not proximate to one that is proximate to the SEC. In untabulated regressions, we find similar results when we use another cut-off of geographic proximity i.e., 150 km. In untabulated work, we also examine the effect of excluding counties that have no restating firms. By construction, counties with no restating firms have negative misreporting deviations. Excluding these counties reduces but does not eliminate the statistical significance of SEC distance variable.

As seen in models 1 and 2, the coefficient on the number of prior SEC enforcements in the county is negative and significant. This finding implies that prior SEC activity in a county significantly reduces the future financial misrepresentation by the firms in the county. The result raises the policy question of whether the SEC should spread its activities geographically via an enforcement division in every county or are there benefits to concentrating enforcement in certain counties.

[22] As there are 331 income decreasing restatements one less firm announcing an income decreasing restatement reduces the misreporting deviation by 1/331 or 0.003 if the number of firms in the county stay constant. The coefficient of Proximate Dummy is -0.00033 and a reduction of 0.00033 in misreporting deviation implies that 0.1 fewer firms are likely to announce income decreasing restatements in a county. The unconditional incidence of income decreasing restatements is 0.028 or 331/11742 in a county. Therefore a reduction of 0.1 firms implies that misreporting deviation is reduced 3.5 times its unconditional mean.

To study this question, we separately estimate the effect of prior enforcement when the number of prior enforcement actions is small (there are one or two prior enforcement actions in the county) as opposed to large (more than two enforcement actions in the county). Interestingly, we find that there is no significant effect when the number of prior enforcements is small (see model 3). Only large levels of prior SEC activity negatively impact the extent of misreporting in the county. This finding suggests that low levels of SEC activity do not lead firms to update their beliefs but a sustained and serious SEC presence in the county is likely to impact the firms' decisions regarding financial misrepresentation. Once again, the effect is not only statistically significant but also economically material.

In particular, if the county moves from having no prior enforcement actions to having a large number of prior enforcement actions, the misreporting deviation reduces by -0.001 or 0.33 fewer firms in a county announce an income decreasing restatement. As the unconditional incidence of income decreasing restatements is 0.028 firms per county, this statistic implies an almost ten-fold impact of past SEC action on firms' proclivity to misreport in a county. Note again that this ten-fold impact is not the marginal impact of having one more prior enforcement action but rather the effect of moving to a regime with large number of enforcements. Specifically, large number of enforcements (more than two prior enforcements in a county) represents the top 2% of all counties with respect to distribution of prior enforcement actions.

Though the evidence that past SEC activity is associated with lower financial misrepresentation is both statistically and economically significant, its interpretation is ambiguous. As discussed earlier, the evidence is consistent both with (i) the "differentially informed" criminal hypothesis which predicts that as firms learn from the past SEC enforcement actions they are less likely to misreport; and (ii) with the "fixed" bad apples hypothesis which predicts that the SEC has already caught many of the firms likely to cheat in the county. As the

time-series of restatements span a short five year period (1997-2002), constructing a convincing test of the idea that a bad apple is less likely to misreport again is difficult.[23] Consequently, we are unable to distinguish between these two hypotheses and hence cannot rule out the mechanical negative relation between prior SEC activity and misreporting deviations in the county, as suggested by the fixed bad apples hypothesis.

Consistent with the univariate results in Table 4 county characteristics in Table 5 do not significantly impact concentration of misreporting. State judicial quality also does not appear to impact misreporting concentration in counties. For robustness, we also proxied for state judicial quality via a dummy for the 10 worst states, and found similar results. Taken together, the significant results related to SEC distance and past enforcement activity are consistent with the "differentially informed criminal" hypothesis.

4.2 Other monitors

Thus far, we have examined the role played by regulators. A natural question arises about the role of other monitors such as analysts, institutional investors, the disciplining role of debt, and the takeover market. To examine potential monitoring by other agents, we construct the following variables:

(i) Industry adjusted mean number of analysts following firms in the county: We obtain the number of analysts providing annual earnings forecasts for firms from I/B/E/S tapes. We compute the average industry adjusted number of analysts following the firms in a county over the period 1997-2001 to proxy for the level of analyst monitoring in the geographic area. Industry is defined at the two digit SIC level.

(ii) Industry adjusted average institutional ownership of firms in the county: We obtain the fraction of the firm owned by institutions from SPECTRUM database from which we subtract the mean ownership of all firms in the same two digit SIC. We then

[23] A potentially longer time series of securities class litigation can be obtained from 1996 to 2008 from the Stanford Class Action Securities Clearing House. However, combining that dataset with our restatement data still restricts us to only five years of data. We found that out of the 1548 firms subject to class action lawsuits, 181 firms (11.6%) have been sued more than once. Whether this percentage of repeat offenders is large enough to rule out the fixed bad apples hypothesis is unclear.

calculate the average industry adjusted institutional ownership of firms in a county over the years 1997-2001.

(iii) Industry adjusted mean debt ratio of firms in the county: We measure firm leverage as the ratio of long-term debt to total assets. We then industry adjust this ratio and estimate the average leverage of all firms in the county over the period 1997-2001. The data to compute leverage comes from COMPUSTAT.

(iv) Average exit rate of firms in the county due to mergers and acquisitions: To examine the disciplining role of the takeover market, we identify all firms that exit due to mergers and acquisitions over the five years prior to the restatement. We then calculate the fraction of firms exiting at the county level.[24]

The *a priori* expectation is that the concentration of misreporting in a county is inversely related to the extent of outside monitoring as captured by the industry adjusted analyst coverage, level of institutional ownership, industry adjusted debt ratios, and the average exit rate of firms in the county.[25] Model 4 in Table 5 reports the results of introducing proxies for these other monitors.[26] Controlling for the role of these other monitors has no qualitative impact on our results. In sum, we continue to find significant evidence that counties located closer to the SEC and those with higher prior SEC activity are associated with significantly higher misreporting intensity.

4.3 Impact of geographic proximity on SEC's choice of firms to investigate

As discussed above, counties geographically closer to the SEC are less likely to adopt aggressive accounting practices consistent with the "differentially informed criminal"

[24] Several restating firms become takeover targets after announcing the restatement. Note that we focus on takeovers prior to the restatement to capture the disciplining role of the takeover market because we want to avoid confounding our proxy of takeover intensity with the outcome of the restatements.

[25] Debt ratios vary significantly across industries and it is quite likely that analyst coverage, as well as institutional ownership show industry variation. Therefore, we industry adjust these to capture higher than average monitoring and not merely cross sectional differences in industries.

[26] With the inclusion of outside monitors the number of counties with data drops to 920. For the purpose of this analysis we set counties with missing analyst coverage to zero. If we instead exclude these observations, the number of counties drops to 683 without material difference in the results. To address the possibility that analyst coverage and institutional ownership are potentially confounded by firm size, we introduce firm size defined as the industry adjusted mean firm's market capitalization in the county with no change in results.

hypothesis. The "constrained cop" hypothesis posits that the SEC, due to resource constraints, prefers to investigate firms that are located closer to its offices. We examine SEC enforcement actions initiated over 1997 to June 2002 to study whether the SEC is indeed more likely to investigate firms that are closer. The dependent variable is the fraction of firms in the county that were subject to SEC enforcement during the 1997 to June 2002 period and is constructed using the KLM dataset described earlier. We pick the time period 1997 to June 2002 to be consistent with the time period over which income-decreasing restatements, our main proxy of misreporting, are measured. As many counties have no firms with any enforcement actions, the dependent variable is zero for a large number of observations. Consequently, we report the results of a Tobit estimation that controls for this censoring of the data.

As seen in Table 6, model 1, the SEC issues more enforcement actions over the 1997 to June 2002 period to firms in counties that are closer. The results are consistent with the "constrained cop" hypothesis in that the number of SEC enforcement actions decreases with the distance between firms' headquarters and the SEC offices. Thus, the SEC is more likely to investigate firms that are located closer perhaps because of constraints related to budgets or information advantages it may possess due to geographic proximity.

Another implication of a "constrained" SEC is that it is more likely to target firms that have received public attention. This may arise both due to the SEC's desire to manage political pressure, as well as, its ability to leverage the media to identify more egregious cases of misreporting. To examine the role of media attention in the SEC enforcement process, we collect data on the number of news reports that are associated with an income decreasing restatement. We use Factiva and count all new reports till a month after the announcement of the restatement and aggregate all the news reports for firms located in a county. This variable referred to as "Media" is positive and significant in model 2 suggesting that the SEC is more

likely to investigate if media attention is high. Interestingly, this effect increases at a decreasing rate (the coefficient of Media squared is negative and significant) suggesting a diminishing influence of greater volume of media stories on SEC enforcement. The Pseudo R-squared after the introduction of the media variables in model 2 is higher at 33% relative to 20% for model 1 without these media variables and points to the importance of media coverage in SEC target selection.

One concern with the above analysis relates to the deflator of the dependent variable in models 1 and 2, i.e., the number of enforcement actions issued over 1997 to June 2002 divided by the number of firms in the county. One can plausibly argue that an alternate deflator should be the number of firms in the county that were aggressive in their financial reporting. Then, the dependent variable would become the ratio of firms pursued from among those that should have been pursued, but ostensibly were not because of constraints. To address this concern, models 3 and 4 in Table 6 reports results based on the ratio of enforcement actions over 1997 to June 2002 scaled by all restatements and income-decreasing restatements during 1997 to June 2002 respectively in the county.[27]

In model 4, we use the alternate measure of SEC distance, the Proximate 100 dummy, to capture distance from the SEC and find that the coefficient is positive and significant as expected. In other words, counties that are proximate are more likely to experience SEC enforcement action. The estimated coefficient of the proximate dummy of 0.33 and is significant at the 5% level. With respect to economic significance this finding implies that when a county that was not proximate moves to being proximate, the likelihood of SEC enforcement action

[27] In some case there was an SEC enforcement action initiated in the county but no restatement or income decreasing restatement. We exclude such observations from the analysis.

given income decreasing restatements increases by 3.8% relative to its unconditional mean.[28] We do not find any robust evidence that past enforcement actions are related to current SEC enforcement actions. Interestingly, the SEC does appear to increase its activity in high growth counties as the coefficient on housing starts is positive and significant.[29]

5.0 Additional Analyses

5.1 Firm level analyses

The unit of analysis used thus far is the county not the firm. We emphasize county-level analysis of misreporting in the paper for two reasons. First, the county, as opposed to the firm, is a natural unit of analysis in our context because our proxies for perceived costs such as distance from the SEC office and past SEC activity in the county do not display cross-firm variation for firms located in any given county. Second, our design is consistent with the economics literature that demonstrates clustering of crime or other economic activity for a spatial unit such a precinct, county or a state (e.g., Audretsch and Stephan 1996, Glaeser et al. 1996).

We recognize, however, that aggressive accounting is a firm-level decision and an analysis of the effect of the information set and experience of neighboring agents on firms' incentives to misreport can potentially complement the county-level results. Hence, in this section, we assess whether our proxies for perceived costs of reporting such as SEC distance and prior SEC activity explain misreporting intensity at the firm-level. Because proxies for

[28] The marginal effect (not reported in the table) for the proximate dummy is 0.0022. The unconditional probability of being uncensored is 55/961 or 0.057. The 55 in the numerator are the number of counties with SEC enforcement actions over the 1997 to June 2002 period and 961, the denominator, is the number of counties with data. This change of 0.0122 is therefore an increase of 3.8%.

[29] Sometimes the SEC observes a restatement and then decides to investigate while in other cases the SEC investigates a firm which eventually leads the firm to restate. An implication of the constrained cop hypothesis is that the SEC involvement is likely greater when it follows a public restatement as these are likely to be cost effective. As discussed we do not have reliable dates on the first involvement of the SEC as informal investigations are confidential. However, in untabulated analysis we study instances when the firm discloses SEC involvement voluntarily prior to the beginning of the regulatory period as reported in the KLM dataset. Consistent with the constrained cop hypothesis, we find that in 89% of the cases the SEC chooses to act after the announcement of the restatement. Note that for this analysis we have assumed that if there is no voluntary disclosure by the firm, the first date of SEC involvement is the beginning of the regulatory period.

perceived costs do not display much cross-sectional variation for firms in a county, our firm-level tests are likely to suffer from lower statistical power.

Table 7 presents results from a cross sectional probit analysis of factors that discriminate between restating and non-restating firms for which complete data are available. The sample consists of all firms on COMPUSTAT over the period 1997 to 2001, and firm level controls have been averaged over the time period. [30] We included dummies for two digit SIC 35, 36, 38 and 73 as these industries were over represented among restating firms. We also controlled for firm level characteristics such as market value, size, age, book to market and whether the firm is a member of the Fortune 500 index. To capture potential non linear effects of firm size we include a small size dummy that is set to one if the firm is below $200 million in market capitalization and zero otherwise, and the natural log of firm's market value.

There is a positive relation between the probability of restating and the distance to the SEC office. Model 1 also demonstrates that firm size is a significant factor in misreporting consistent with Burns and Kedia (2006). In model 2, we use the Proximate 100 dummy to capture distance from the SEC and find that is negative and significant, as expected. Consistent with prior results, other corporate monitors do not appear to be associated with a higher probability of restatement (model 3). In sum, the tenor of the results reported in Table 7 is similar to that documented in earlier tables when the unit of analysis is the county though somewhat weaker as expected. This is reassuring because observations at the county level are weighted differently from those at the firm level.

[30] We did not consider the effect of executive compensation such as stock options on the firm's propensity to restate because focus on executive compensation would require intersection of our sample with Execucomp that only covers S&P 1500 firms and would thus severely cut our sample size. As a compromise designed to preserve sample size, we have included two key instrumental variables for stock option plans (size and market to book) in the Probit analysis, consistent with Smith and Watts (1992).

5.2 Robustness with restatements with different severity

Thus far, we have considered only income decreasing restatements. In Table 8, we present county-level regressions (similar to Table 5) with different measures of restatements. We begin by examining whether our results hold when we include all restating firms, and not just income decreasing restating firms. In this expanded sample of restatements, we continue to find that the distance to the SEC is positive and significant (see model 1). When we use the proximate 100 dummy (see model 4) we find that proximate counties have lower misreporting deviations though the magnitude of this effect is smaller than that observed for income decreasing restatements as shown in Table 5. In both these specifications, we continue to find that prior exposure to SEC activity significantly reduces the misreporting deviation as captured by all restatements.

The significance of results in the expanded sample could be attributable either entirely to income decreasing restatements or to the inclusion of restatements with a positive effect on income. To shed light on this issue, we isolate firms that announce restatements that have a positive effect on net income, i.e., these are income increasing restatements (model 6). As income-increasing restatements are likely to be less severe if not benign restatements, we should observe no effect of SEC distance or prior exposure to SEC activity on the incidence of these restatements. Consistent with this hypothesis, we find that neither SEC distance nor prior exposure to SEC activity is significant in explaining the incidence of income increasing restatements.[31] As expected, similar results are obtained when we also include restatements with zero impact on net income (model 7). This result is reassuring as it suggests that our prior results of significant impact of SEC distance and prior SEC activity are likely not attributable to spurious or omitted factors.

[31] We thank the editor for suggesting this test. We find similar results when we use the proximate dummy to capture the distance to the SEC but we have not reported these in the paper for brevity.

Next, we examine whether our results hold with a more restrictive definition of misreporting. In particular, we require that the firm not only announce an income decreasing restatement but also be classified as an irregularity as per the criteria of Hennes, Leone and Miller (2008).[32] The data on irregular restatements was obtained from Andrew Leone's website. In contrast to the 331 income decreasing restatements we have only 107 of these income-decreasing restatements that are also classified as irregular. Restricting the sample to irregular restatements has the advantage of eliminating those restatements among the income- decreasing sample that might be errors (though note that the process of only including income decreasing restatements and removing others was meant to eliminate errors as well). However, the downside of this approach is that it eliminates several cases of milder misreporting. As a firm's choice of engaging in milder infractions is also likely to be impacted by their information about the SEC oversight process in much the same way as their decision to engage in more egregious misreporting, restricting the sample to only the most severe cases of misreporting (or irregularities) unnecessarily shrinks the sample and reduces power. Even with this conservative definition of misreporting, distance to the SEC (model 3), as well as the proximate dummy, continues to be significant (model 5). The effect of prior exposure to SEC activity also continues to be highly significant when we use this conservative definition of misreporting.[33]

[32]Note that we considered even finer cuts such as restatements of earnings releases but not financial statements or cases with more restated quarters but decided against embarking on such analyses because the usable sample sizes of such restatements tend to become uncomfortably small.

[33] Note that the R square in model 3 and 5 with the Hennes et al. dataset is substantially higher than those in other models. The Hennes et al. dataset attempts to identify only egregious restatements. It turns out that these restatements are distributed in fewer counties relative to the full set of all income decreasing restatements. Therefore, the control variables are able to better explain the distribution of these irregular restatements. As can be seen in Table 8, the R square for models that include only the control variables and exclude our test variables (distance to SEC office and prior SEC activity) is also very high. However, the marginal contribution of the test variables in explaining variation in the Hennes et al. data of irregular restatements is actually lower than that for income decreasing restatements.

5.3 Industry adjustment

The analyses thus far in the paper does not industry adjust the misreporting deviations in the fear that such an adjustment would "throw the baby out with the bath water." If all firms in the industry are identical, face identical incentives, have identical interchangeable managers, they all make the same identical decisions, including the decision to violate GAAP, industry adjustment could potentially remove the effect we are trying to estimate. However, a skeptic might counter that because industries tend to be geographically concentrated (Ellison and Glaeser 1997, Audretsch and Stephan 1996), the geographic concentration of misreporting could merely reflect the concentration in certain counties of industries with a higher propensity to restate. To address this criticism, in untabulated work, we compute an industry-adjusted deviation measure defined as follows:

$$ind\ adjusted\ misreporting\_concentration_i = x_i - indavg_i, \text{ where:}$$

$$indavg_i = weighted\ average\ industry\ restating\ share\ for\ county_i = \sum_j \frac{w_{ij} * Number\ of\ restating\ firms\ in\ ind_j}{Total\ restating\ firms}$$

$$w_{ij} = \frac{Number\ of\ firms\ in\ industry\ j\ and\ county\ i}{Total\ firms\ in\ industry\ j}, \text{ and } x_i \text{ is as defined in equation 1 above.}$$

The new measure assumes that misreporting should be proportional to the industry composition of the county. We repeat all the models reported in Table 5 with this new measure with no material change in our results.

5.4 Firm age as a confound

An alternate explanation for our results is as follows. The SEC chooses office locations based on the extent of economic development (and probably some form of political consideration). Assuming the price of land is high in such areas of high economic development, (such as Los Angeles, Chicago and New York), young firms, which potentially constitute the

most likely group of potential misreporters choose to begin operations elsewhere and they do so for economic reasons. In this alternative story, the results reported are an artifact of age rather than geography. To address this concern, we included firm age as a control variable in our analysis of a firm's proclivity to restate (Table 7) but found that firm age is not statistically significant. In an alternate specification (not reported), we inserted ln (age) and find that the coefficient is positive and significant. Thus, when the coefficient on age is significant, it acquires the opposite sign, i.e., older, not younger, firms are more likely to restate in our sample. However, distance to the SEC retains its significance as per Table 7 in both these age specifications.

6.0. Conclusions

In this paper, we investigate whether firms' awareness of the SEC's enforcement activities and publicly observed data on the SEC's resource constraints affect firms' propensity to violate GAAP. We find two interesting results about the SEC's role in influencing firms' decision to commit violations. First, counties closer to the SEC are associated with significantly lower misreporting deviations. Second, the misreporting intensity is decreasing in the past SEC enforcement actions in that county. The results support the "differentially informed criminal" hypothesis that firms have heterogeneous information about regulatory oversight and therefore, differ in their perceived costs of misreporting. However, this finding is also consistent with an alternative explanation that prior enforcement actions have busted many of the crooks, leaving fewer "bad apples" in the county. We also find evidence in support of the "constrained cop" hypothesis which posits that the SEC is more likely to investigate firms that are located closer to its offices and have received greater media attention.

The literature on the recent wave of earnings restatements has emphasized the role of the benefits and ex post costs of misreporting to the executives and firms, as well as, the failure of governance mechanisms in deterring misreporting. The evidence in this paper suggests differences in firms' ex-ante information sets. In other words, costs perceived by managers before they decide to misreport are potentially also important in explaining why firms adopt aggressive accounting practices in the first place. Mitigating these information differences either through the establishment of local SEC offices or via better disbursement of information might lower the probability of misreporting in the future.

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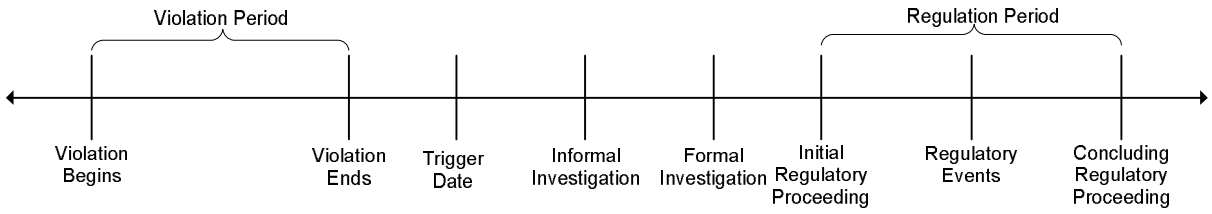
A Typical SEC Enforcement Action as depicted in Karpoff et al. (2008a)

Figure 1 – Timeline of an SEC Enforcement Action



Notes:

The violation period is the period during which the firm is alleged to have fraudulently managed earnings. Karpoff et al. (2008) define trigger dates as dates on which firm-initiated or other events reveal the existence of potential problems at the firm. Examples of trigger dates can be found in the text. Following a trigger event, the SEC gathers information through an informal inquiry that, if warranted, grows to a formal investigation. During the investigation period, the targeted firm may voluntarily issue a press release indicating that it is the target of an SEC informal inquiry or formal investigation. After an investigation, the SEC can decide to drop the case, proceed with an administrative or civil action, and/or refer it to the DOJ for parallel criminal prosecution. The SEC's decision to file charges against the firm marks the beginning of the regulation period. The beginning of the regulation period is publicly available for all firms and has been used to date stamp SEC enforcement actions in this paper.

Table 1a
SEC Budgets

This table displays SEC Budgets over the time period 1995 to 2009. The SEC budgets are obtained from their website http://www.sec.gov/foia/docs/budgetact.htm. Budget authority in column (2) refers to the budgeted amount that the SEC is allowed to spend in a particular year. Actual obligations in column (3) refer to the amounts actually spent by the SEC during that year. Both the budget authority and the actual obligations numbers are expressed in 2009 constant dollars where inflation is adjusted as per the Consumer Price Index (CPI) data obtained from the Bureau of Labor Statistics (ftp://ftp.bls.gov/pub/special.requests/cpi/cpiai.txt). The market capitalization in column 4 (number of firms in column 5) is the market value of all firms (total number of firms) that traded in December of the year and with data available on CRSP. Columns (6) and (7) express the SEC budget as a percentage of the total stock market capitalization and number of listed firms respectively. Column (8) shows the president in power in the respective year and his political affiliation with D = Democrat and R= Republican.

Year (1)	Budget authority 2009 dollars ('000) (2)	Actual obligations 2009 dollars (‘000) (3)	Market Capitalization ($’000) (4)	Number of firms (5)	SEC budget as a fraction of market capitalization (%) (6)	SEC budget as a fraction of number of firms (2009 $) (7)	President and his political affiliation (8)
1995	421,873	399,852	6,910,716,000	8,076	0.00435	52.24	Bill Clinton (D)
1996	410,434	404,449	8,479,096,000	8,634	0.00355	47.54	Bill Clinton (D)
1997	414,800	411,454	11,030,410,000	8,754	0.00282	47.38	Bill Clinton (D)
1998	413,558	408,494	13,551,510,000	8,446	0.00232	48.96	Bill Clinton (D)
1999	438,756	435,305	17,582,280,000	8,034	0.00194	54.61	Bill Clinton (D)
2000	469,058	460,131	16,056,360,000	7,920	0.00235	59.22	Bill Clinton (D)
2001	511,181	498,871	14,200,010,000	7,165	0.00298	71.34	Bill Clinton (D)
2002	611,075	579,399	11,301,460,000	6,777	0.00455	90.17	George W. Bush (R)
2003	833,146	720,297	14,991,940,000	6,542	0.00478	127.35	George W. Bush (R)
2004	918,841	854,881	16,944,200,000	6,545	0.00479	140.39	George W. Bush (R)
2005	1,001,959	973,674	17,996,370,000	6,623	0.00507	151.28	George W. Bush (R)
2006	940,876	929,393	20,384,290,000	6,765	0.00436	139.08	George W. Bush (R)
2007	911,371	905,060	21,147,100,000	6,894	0.00417	132.20	George W. Bush (R)
2008	901,786	901,102	12,671,960,000	6,708	0.00715	134.43	George W. Bush (R)
2009	960,000	960,000	16,600,430,000	6,470	0.00578	148.38	Barack Obama (D)

Table 1b: Sample Characteristics

The table displays average values of the variables over the period 1997-2002 for three groups of firms: (i) the non-restating sample; (ii) income decreasing restatements; and (iii) income decreasing restating firms that were subject to SEC enforcement action over the period 1997 to 2009. Leverage is the ratio of long term debt to total assets. Fortune dummy takes the value one if the firm has a rank assigned by Fortune magazine as reported in Compustat. Age is measured in years from the first date on which the firm appears in COMPUSTAT. *** in column 2 indicates the significance at the 1% level of the difference between the means of the non-restating sample as compared to the sample of income-decreasing restating firms. *** in column 3 indicates the significance at the 1% level of the difference between the means of the income decreasing restating firms and income-decreasing restating firms targeted by the SEC over the 1997 to June 2002 period.

	Non-Restating Sample	Income Decreasing Restating Firms	Income Decreasing Restaters targeted by SEC
	(1)	(2)	(3)
Market Value of Equity	1350	2116	6463***
Total Assets	2247	1987	5418***
Leverage	0.31	0.189	0.22
Book to Market	0.42	0.44	0.21
Fortune Dummy	0.05	0.12***	0.205
Age (in years)	14.03	16.01***	18.07
Number of Obs.	9647	331	39

Table 2
Misreporting Patterns Across States

This table reports the number of firms and restating firms for states in the U.S. Number of counties refers to the number of counties in that state that had at least one corporate headquarters. Number of firms refers to all active COMPUSTAT firms over the period 1997 to 2001. Restating firms are those that announced an income-decreasing restatement over the period 01/97 to 06/02. In column (6), share of restatements is the percentage of restating firms that are located in the state. Deviation refers to the difference between a state's share of restatement and its share of COMPUSTAT firms in this table. However, deviation as used elsewhere in the paper refers to the difference between a county's share of restatement and that county's share of COMPUSTAT firms.

State	Number of counties	Number of firms	Share of COMPUSTAT (%)	Number of restatements	Share of restatements (%)	Deviation (%)
(1)	(2)	(3)	(4)	(5)	(6)	(6)-(4)
AL	19	74	0.63	2	0.60	-0.03
AK	1	6	0.05	0	0.00	-0.05
AZ	3	150	1.28	4	1.21	-0.07
AR	15	43	0.37	3	0.91	0.54
CA	38	2059	17.56	142	19.78	2.22
CO	21	347	2.96	17	2.37	-0.59
CT	8	290	2.47	17	2.37	-0.1
DE	2	41	0.35	1	0.14	-0.21
DC	1	49	0.42	1	0.14	-0.28
FL	31	552	4.71	36	5.01	0.3
GA	40	315	2.69	21	2.92	0.23
HI	3	21	0.18	1	0.14	-0.04
ID	9	24	0.20	1	0.14	-0.06
IL	31	476	4.06	33	4.60	0.54
IN	41	165	1.41	11	1.53	0.12
IA	24	64	0.55	2	0.28	-0.27
KS	17	60	0.51	6	0.84	0.33
KY	19	73	0.62	2	0.28	-0.34
LA	19	79	0.67	2	0.28	-0.39
ME	11	28	0.24	1	0.14	-0.1
MD	13	223	1.90	11	1.53	-0.37
MA	12	588	5.02	46	6.41	1.39
MI	33	212	1.81	15	2.09	0.28
MN	28	307	2.62	19	2.65	0.03
MS	18	48	0.41	1	0.14	-0.27
MO	23	171	1.46	5	0.70	-0.76
MT	10	15	0.13	2	0.28	0.15
NE	8	40	0.34	4	0.56	0.22
NV	5	126	1.07	9	1.25	0.18
NH	9	48	0.41	3	0.42	0.01
NJ	20	537	4.58	25	3.48	-1.1
NM	5	23	0.20	0	0.00	-0.2
NY	51	1087	9.27	54	7.52	-1.75
NC	46	204	1.74	9	1.25	-0.49
ND	4	9	0.08	0	0.00	-0.08
OH	52	371	3.16	22	3.06	-0.1
OK	8	95	0.81	7	0.97	0.16
OR	14	115	0.98	8	1.11	0.13
PA	41	488	4.16	25	3.48	-0.68
RI	5	39	0.33	1	0.14	-0.19
SC	19	75	0.64	3	0.42	-0.22

Table 2 (Cont'd)
Misreporting Patterns Across States

State	Num of counties	Num of firms	Share of COMPUSTAT	Number of restatement	Share of restatements (%)	Deviation (%)
(1)	(2)	(3)	(4)	(5)	(6)	(6)-(4)
SD	5	13	0.11	1	0.14	0.03
TN	20	148	1.26	18	2.51	1.25
TX	55	1027	8.76	73	10.17	1.41
UT	8	131	1.12	6	0.84	-0.28
VT	7	19	0.16	0	0.00	-0.16
VA	57	285	2.43	16	2.23	-0.2
WA	14	208	1.77	16	2.23	0.46
WV	11	24	0.20	0	0.00	-0.2
WI	24	123	1.05	9	1.25	0.2
WY	7	9	0.08	0	0.00	-0.08

Table 3: Location of SEC offices and Distances to Top 20 Counties

The table reports data for the top 20 counties in the sample sorted by the number of firms headquartered (# firms) in the county. # restaters refers to the number of income-decreasing restaters during 01/97 to 06/02. Distance to SEC is the distance between the county where firms are headquartered and the closest SEC office, defined as the closer of the head quarters in Washington D.C. or the closest regional SEC office. Distances have been rounded off to the nearest kilometer.

Sr.no	# firms	# restaters	Distance to SEC HQ (km)	Distance to SEC Reg. Office (km)	County and State	SEC Office closest to the county
1	522	7	333	11	New York County, NY	New York City, NY
2	451	13	3694	8	Los Angeles County, CA	Los Angeles, CA
3	446	22	3879	347	Santa Clara County, CA	Los Angeles, CA
4	347	12	1960	1103	Harris County, TX	Denver, CO
5	317	8	1904	779	Dallas County, TX	Denver, CO
6	305	9	619	202	Middlesex County, MA	New York City, NY
7	276	8	3672	48	Orange County, CA	Los Angeles, CA
8	269	13	960	6	Cook County, IL	Chicago, IL
9	230	6	3612	121	San Diego County, CA	Los Angeles, CA
10	199	7	1518	352	Hennepin County, MN	Los Angeles, CA
11	174	2	403	61	Fairfield County, CT	New York City, NY
12	151	4	3885	394	Alameda County, CA	Los Angeles, CA
13	139	4	3228	85	Maricopa County, AZ	Los Angeles, CA
14	139	9	3930	370	San Mateo County, CA	Los Angeles, CA
15	138	7	3705	1487	King County, WA	Los Angeles, CA
16	129	3	877	877	Fulton County, GA	Washington, DC
17	121	1	1401	96	Palm Beach County, FL	Miami, FL
18	115	5	2390	0	Denver County, CO	Denver, CO
19	114	5	1456	41	Broward County, FL	Miami, FL
20	111	1	434	30	Suffolk County, NY	New York City, NY

Table 4: Descriptive Statistics for County Groups Sorted on Misreporting Intensity

This table displays the mean value of variables across groups of counties formed on the basis of their misreporting deviations. Group 1 (3) is the 10 (90) percentile for misreporting deviation. Group 2 represents the rest of the distribution. Misreporting deviation is the difference between the county's share of restatement during 01/97-06/02 and the county's expected restatement share as explained in the text. Distance from the SEC is the distance from the county to the closest SEC national or regional headquarters. SEC enforcements initiated prior to 1997 refer to the number of enforcement actions in the county prior to 1997. Proximate100 dummy takes the value one if the distance to the SEC is within 100km. For each of the groups of counties formed on the basis of misreporting deviations, we report the following county characteristics: (i) land area is the average land area in square miles of the counties; (ii) population change is the average percentage change in population over 1990 to 2000 in the county; (iii) housing change is the average percentage change in housing over the period 1990 to 2000 in the county; (iv) housing starts are the number of new housing starts in 2000 in the county; (v) change in private, non-farm establishments is the average percentage change in such establishments in the county over the period 1990 to 1998; (vi) change in personal income is the percentage change in personal income in the county over the period 1990 to 1998; (vii) employment in high incentive industries is the fraction of the state employees that are employed in industries with high stock options usage. Analyst following, institutional ownership, and leverage are calculated as the mean industry adjusted value of these for all firms in the county over 1997-2001. Leverage is defined as the ratio of long-term debt to total assets. Exit is the number of firms that were acquired in the county during 1991-1996. The t-statistics captures differences in means of Group 1 and 3. *,**,*** represent significance at the 10, 5, and 1 level respectively.

Row	Variable	Group 1	Group 2	Group 3	T-Stat (1)-(3)
	Dependent variable				
1	Mean deviation (%)	-0.002	0.002	0.0036	
	Test variables				
2	Average distance from the SEC (in km)	254	425	392	2.59***
3	SEC Enforcements prior to 1997	1.08	0.091	0.64	1.24
4	Promixate100 Dummy (%)	49.4	16.6	27.0	3.23***
	County variables				
5	Land Area (square miles)	1084	890	796	1.16
6	Population change (%)	16.75	13.73	17.12	0.12
7	Housing change (%)	24.4	15.90	17.09	0.86
8	Housing starts (million)	3488	860	3250	0.47
9	Change in private non-farm establishment(%)	16.91	16.78	17.71	0.26
10	Change in personal income (%)	56.01	53.06	57.76	0.50
11	Employment in high incentive industries	3.26	2.70	3.03	1.07
	Other monitors				
12	Ind. adjusted mean analyst coverage in county	0.078	-0.98	-0.005	0.27
13	Ind. adjusted mean institutional ownership in county (%)	0.017	-0.034	0.021	0.29
14	Industry adjusted mean leverage in county	-0.1413	0.43	-0.002	0.77
15	Exit due to M&A	5.42	0.60	5.14	0.20

Table 5: Evidence on the Differentially Informed Criminal Hypothesis

The dependent variable is county's misreporting deviation for all income decreasing restatements announced from 1997 to June 2002. We use the following county characteristics as control variables: (i) land area is the average land area in square miles of the counties; (ii) population change is the average percentage change in population over 1990 to 2000 in the county; (iii) housing change is the average percentage change in housing over the period 1990 to 2000 in the county; (iv) housing starts are the number of new housing starts in 2000 in the county; (v) change in private, non-farm establishments is the average percentage change in such establishments in the county over the period 1990 to 1998; (vi) change in personal income is the percentage change in personal income in the county over the period 1990 to 1998; (vii) employment in high incentive industries is the fraction of the state employees that are employed in industries with high stock options usage. Analyst following, institutional ownership, and leverage are calculated as the mean industry adjusted value of these for all firms in the county over 1997-2001. Leverage is defined as the ratio of long-term debt to total assets. Exit is the number of firms that were acquired in the county during 1991-1996. Number of firms refers to those that are headquartered in the county. SEC distance is the distance between county and the closest SEC national or regional headquarters. Proximate dummy is 1 if SEC distance is 100 km or less. SEC enforcements prior to 1997 is the number of firms in the county that were subject to enforcements prior to 1997. Ranking for state judicial quality is from the overall rankings in the 2001 U.S. Chamber of Commerce State Liabilities. Average firm size in county is the mean market value of firms headquartered in the county. Predicted signs are reported in brackets next to the variables. All coefficients for above have been multiplied by 10^6. Errors have been corrected for heteroscedasticity. P values are displayed in parenthesis below the coefficient estimate. *,**,*** represent significance at the 10, 5, and 1 level respectively. Intercepts have not been tabulated below. N=958 counties in models 1-3, N=920 counties in model 4.

	Model 1	Model 2	Model 3	Model 4
Test variables				
SEC distance (+)	0.351 (0.03)**		0.351 (0.02)*	0.351 (0.02)**
SEC enforcements prior to 1997 (-)	-0.001 (0.01)***	-0.001 (0.01)***		-0.001 (0.01)***
Proximate100 Dummy (-)		-0.00033 (0.05)**		
Low SEC enforcements prior to 97 (-)			-0.0003 (0.58)	
High enforcements prior to 1997 (-)			-0.001 (0.01)**	
County level controls				
Land area in square miles	-0.04 (0.5)	-0.023 (0.68)	-0.011 (0.86)	-0.03 (0.59)
% Population change (1990-2000)	-10.47 (0.32)	-6.355 (0.52)	-8.945 (0.38)	-11.136 (0.3)
% Housing change (1990-2000)	6.63 (0.55)	3.274 (0.73)	7.882 (0.43)	8.651 (0.4)
Housing starts in 2000	0.004 (0.96)	0.004 (0.96)	-0.026 (0.77)	-0.003 (0.98)
% change private income est.	-0.369 (0.91)	4.347 (0.51)	(0.094 (0.98)	5.081 (0.45)
% Personal income change(1990-98)	4.875 (0.5)	-0.37 (0.9)	3.192 (0.63)	-0.396 (0.91)
High incentive	48.852 (0.43)	32.453 (0.55)	14.88 (0.77)	22.169 (0.45)
Number of firms	35.447 (0.04)**	35.658 (0.04)**	37.613 (0.03)**	47.261 (0.41)
Ranking for state judicial quality	1.1357 (0.76)		0.504 (0.9)	
Average Firm Size in County	-0.002 (0.6)			

Table 5: Evidence on the Differentially Informed Criminal Hypothesis (Cont'd)

Mean analyst following in county				-7.73
				(0.65)
Mean institutional ownership (%)				181.01
				(0.47)
Mean leverage in county				0.685
				(0.29)
Exit rate due to M&A				107.443
				(0.41)
Adjusted r square (%)	0.19	0.19	0.21	0.20

Table 6
Evidence on the Constrained Cop Hypothesis

The dependent variable for all the models presented here is ratio of number of SEC enforcements initiated over 1997 to June 2002 divided by different scale variables. The results reported are from a Tobit estimation. In model 1 and 2, the scale variable is the number of firms in the county. In models 3 and 4, the scale variable is the number of restating firms and the number of income-decreasing restating firms in the county over 1997-June 2002 respectively. Model 4 uses the proximate dummy rather than SEC distance to capture proximity to the SEC. Variables repeated from Table 5 are defined in the notes to Table 5. Media is the number of new stories upto a month after the restatement announcement. Predicted signs are in brackets next to the variables. P values are displayed in parenthesis below the coefficient estimate. *,**,*** represent significance at the 10, 5, and 1 level respectively. Intercepts have not been tabulated below.

	Model 1	Model 2	Model 3	Model 4
Test variables				
SEC distance (-)	-0.00013	-0.00016	-0.00069	
	(0.04)**	(0.02)**	(0.01)***	
SEC enforcements prior to 1997 (?)	0.026	0.013	0.049	0.071
	(0.03)**	(0.35)	(0.27)	(0.14)
Proximate100 Dummy (+)				0.33
				(0.05)**
Media (+)		0.034	0.13	0.151
		(0.00)***	(0.00)***	(0.00)***
Media Squared (-)		-0.00044	-0.0016	-0.0019
		(0.00)***	(0.00)***	(0.00)***
County level controls				
Land area in square miles	-0.0001	-0.0001	-0.0002	-0.0002
	(0.02)**	(0.04)**	(0.01)***	(0.02)**
% Population change (1990-2000)	0.006	0.007	0.015	0.017
	(0.13)	(0.09)*	(0.29)	(0.28)
% Housing change (1990 – 2000)	-0.005	-0.004	-0.006	-0.002
	(0.16)	(0.26)	(0.68)	(0.91)
Housing starts in 2000	0.00003	0.00001	0.00007	0.00007
	(0.00)***	(0.10)*	(0.01)***	(0.02)**
Private, non-farm income establishments	-0.003	-0.003	-0.018	-0.019
	(0.16)	(0.17)	(0.06)*	(0.06)*
% Personal income change (1990-1998)	0.002	0.001	0.003	0.002
	(0.49)	(0.71)	(0.72)	(0.75)
High incentive	0.01	0.006	0.023	0.059
	(0.44)	(0.68)	(0.66)	(0.32)
Number of firms	-0.0001	-0.0005	-0.003	-0.003
	(0.8)	(0.37)	(0.14)	(0.11)
Observations	982	982	972	961
Pseudo r square (%)	0.20	0.33	0.32	0.398

Table 7: Firm Level Analyses of Misreporting

The dependent variable is one if the firm announced an income decreasing restatement over 1997-June 2002. SEC distance is the distance from the firm's headquarters to the closest SEC national or regional headquarters. SEC enforcements prior to 1997 represents SEC enforcements initiated against firms in the same county as the firm's headquarters. Proximate100 Dummy takes the value 1 if SEC distance is less than 100km. Market Value is the average market value of equity for the firm over the period 1997 to 2001. Small size dummy is 1 if the average market value of equity is less than $200 million. Fortune dummy is set to 1 if the firm is a member of the Fortune 500 group of companies. Leverage is the average ratio of long-term debt to total assets over the period 1997 to 2001. Book to market is the average ratio of the book value of equity to market value over the period 1997 to 2001. Firm age is measured in months from the first date on which the firm appears in COMPUSTAT. The values for other monitors is the average industry adjusted value of these for all firms in the county over 1997-2001. Several coefficients have been multiplied by 10^6 for expositional ease. Predicted signs are in brackets next to the variables. The regression includes four industry dummies for two digit industries 35, 36, 38 and 37 respectively. P values appear in parentheses. *,**,*** represent significance at the 10, 5, and 1 level respectively.

Variables	Model 1	Model 2	Model 3
Test variables			
SEC distance (x 10^6) (+)	127.843		112.318
	(0.05)**		(0.09)*
SEC enforcements prior to 1997 (x 10^6) (-)	-3,784.00	-3,252.34	-4,005.69
	(0.36)	(0.45)	(0.34)
Proximate100 Dummy (-)		-0.09	
		(0.09)*	
Control variables			
Market Value (x 10^6)	-7.847	-7.88	-7.781
	(0.05)**	(0.05)**	(0.06)*
Small size dummy	0.164	0.163	0.165
	(0.06)*	(0.06)*	(0.06)*
Ln (1+market value)	0.107	0.106	0.106
	(0.00)***	(0.00)***	(0.00)***
Fortune dummy	0.142	0.148	0.14
	(0.2)	(0.18)	(0.21)
Leverage	-0.025	-0.022	-0.027
	(0.71)	(0.73)	(0.7)
Book to Market (x 10^6)	0.001	0.001	0.001
	(0.81)	(0.82)	(0.81)
Firm age	0.002	0.002	0.002
	(0.38)	(0.39)	(0.45)
Other monitors			
Average industry adjusted analyst following			0.002
			(0.92)
Average industry adjusted institutional ownership			0.316
			(0.41)
Average industry adjusted leverage			0.018
			(0.24)
Observations	10414	10414	10375
Pseudo r-square after adjusting for degrees of freedom (%)	0.0248	0.0245	0.0255

Table 8
Proxies for Information Sets and County Misreporting Deviations for Sub-Samples of Restatements

The dependent variable is misreporting deviation per county where the deviation is computed as the difference between a county's share of restatements made during 01/97 to 06/02 from its expected share of restatement. However, the nature of restatements considered to compute this deviation changes depending on the column considered. Models 1 and 4 consider all restatements. Model 2 restricts the data to restatements that have a negative effect on earnings (the sample for the rest of the tables). Models 3 and 5 focus on the sample of 107 restatements flagged as accounting irregularities by Hennes et al. (2008) that overlap with our income decreasing sample. Model 6 examines a sample of only positive restatements while model 7 examines positive and zero restatements. All the control variables discussed in model 1 of Table 5 were included in the estimation but the coefficients on those variables have not been reported below for parsimony. The definitions of variables reported here appear in the notes to Table 5. All coefficients for the above variables have been multiplied by 10^6 unless otherwise mentioned. Predicted signs are in brackets next to the variables. Errors have been corrected for heteroscedasticity. T statistics are displayed in parenthesis below. *,**,*** represent significance at the 10, 5, and 1 level respectively. Intercepts have not been tabulated below.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Sample	All restatements	Only Negative Earnings	Only Negative and Irregular restatements	All restatements	Only Negative and Irregular restatements	Only Positive Earnings Restatements	Only Positive and Zero Earnings Restatements
Test variables							
SEC distance (+)	0.218	0.35	1.057			0.1168	0.1162
	(0.02)**	(0.03)**	(0.03)**			(0.63)	(0.63)
SEC enforcement prior to 97 (-)	-0.001	-0.001	-0.003	-0.001	-0.003	-0.0001	-0.00012
	(0.00)***	(0.01)***	(0.01)***	(0.00)***	(0.01)***	(0.56)	(0.56)
Proximate100 Dummy (-)				-0.00023	-0.001		
				(0.04)**	(0.06)*		
Coefficients on control variables used in model 1 of Table 5 estimated but not reported here for parsimony.							
Observations	958	958	958	958	958	958	958
Adjusted R square	0.21	0.19	0.69	0.21	0.69	0.021	0.0209
Adjusted R Square with only the control variables and no test variables	0.09	0.02	0.625	0.09	0.625	0.018	0.018