

The road not taken: A comparison of AAERs and securities class actions

ABSTRACT

Despite the controversial debate over the role of public enforcement and private litigation in detecting and deterring financial misreporting, we have only scant literature comparing their enforcement outcomes: SEC-sanctioned cases (AAERs) and settled class actions against which the SEC did not file cases (SCALs). This paper documents systematic differences between the two. Specifically, AAERs exhibit a larger magnitude of accruals prior to misreporting, as well as greater financing needs and insider trading during manipulation periods. After controlling for case backlogs in the SEC and the courts, the misreporting amount and period of AAERs are also greater and longer than those of SCALs, although SCALs represent greater settlement amounts. Further analysis indicates that resource constraints do not critically undermine the SEC investigations to detect more material misreporting cases. However, plaintiff investors appear to go forum shopping to earn greater settlement proceeds from SCALs. Finally, relative to SCALs, AAERs experienced significant drops in firm performance, analyst following, and CEO tenure around SEC sanctions. Overall, this study provides consistent evidence supporting the SEC's optimization of detection rates under resource constraints and the strategic interaction between SEC enforcement and private litigation.

KEYWORDS

AAERs, Securities and Exchange Commission, securities class actions, resource constraints, detection rate.

JEL Classifications: M41, M48.

1. INTRODUCTION

The Securities and Exchange Commission (SEC) is chronically strained according to the U.S. General Accounting Office. Its enforcement resources have remained limited while its workload has increased due to the capital market expansion (USGAO, 2002a). Accordingly, while the SEC is charged with regulating financial misreporting, it can inevitably sanction only a few committed cases. By necessity, it forgoes numerous securities class action lawsuits (SCALs), even though they were settled in the courts.

The SEC is generally ambivalent about the existence of settled SCALs against which it did not file cases (hereafter SCALs, unless otherwise specified). On the one hand, it admits that private litigation is a “necessary supplement” to public enforcement, which is bounded by limited resources. On the other hand, it argues that some SCALs include frivolous cases that lack sufficient grounds for causation (Poser, 2008).ⁱ

However, coupled with a series of regulatory failures (e.g., Sidak, 2003), the existence of committed but non-enforced misreporting cases casts doubt on SEC enforcement actions. From the perspective of accounting research, the incomplete coverage of misreporting cases in Accounting and Auditing Enforcement Releases (AAERs) undermines their reliability as an accounting fraud database (Karpoff et al., 2017). Moreover, the literature has identified various geographic and political biases in SEC enforcement that only strengthen this suspicion (e.g., Kedia and Rajgopal, 2011; Yu and Yu, 2011; and Correia, 2014).

Against this backdrop, this paper compares the characteristics of AAERs with those of SCALs,ⁱⁱ and examines why certain misreporting cases are not sanctioned by the SEC. This point is important for several reasons. Most importantly, despite the controversial debate over the role of public enforcement and private litigation, there is only limited research thus far comparing their enforcement outcomes. Empirical analysis results can thus provide a basis for ongoing discussions (e.g., Bratton and Wachter, 2011; and Schantl and Wagenhofer, 2020). In

addition, the public cannot have confidence in SEC enforcement if it believes the SEC sanctions firms arbitrarily.

To the extent that capital market investors also have strong incentives to detect fraudulent misreporting in order to obtain damage compensation, the non-enforcement of settled SCALs does not appear justifiable. Proponents for private litigation argue that modern securities class action lawsuits perform virtually the same function as the SEC in detecting and deterring financial misreporting (e.g., Cox, 1997). In this respect, settled SCALs are at least as likely to include material misreporting as AAERs, and thus it seems the SEC should have imposed sanctions.ⁱⁱⁱ Moreover, non-enforced misreporting may lead to corporate and financial system failures, and consequently increase social costs (Schroeder, 2002; and Frantz and Instefjord, 2018).

However, as per the theory of the economics of crime, not all misconduct needs to be sanctioned, and public enforcement and private litigation should strategically interact. A recent study by Schantl and Wagenhofer (2020) argues that the two institutions optimize their decisions. Most importantly, the SEC makes investigation decisions based on “the cost of negligence,” which is larger when misreporting missed by the SEC will be settled later in court. Courts’ settlement is affected by investors’ litigation decisions, which critically depend on the information leaked (or released) from the SEC. In a similar vein, Becker (1968) explains that society tolerates a certain level of offenses by choosing an optimal conviction rate. This is because eliminating all crimes would be overly costly. Thus, society determines the acceptable allocation of resources necessary for enforcement agencies to attain their required conviction rates.

In this enforcement system, a rational agency will effectively target the most egregious cases with the strongest evidence, and “condone” the others. Therefore, if the SEC can optimize its detection rate, we should observe systematic differences between AAERs and SCALs.

Given the information advantage of the SEC as a federal agency, which is staffed with skilled professionals and endowed with due authority to conduct its regulatory role^{iv} (e.g., Choi et al., 2009; and Schantl and Wagenhofer, 2020), I hypothesize positive (negative) associations between the egregiousness of misreporting cases and SEC enforcements (non-enforcements).

More specifically, SEC investigations consist of two stages. First, analysts in the Division of Corporate Finance review financial statements “for violations of routine screening criteria” (Feroz et al., 1991). This process ends if no issues were identified or if issues were resolved by exchanging comment letters between the division and firms (Dechow et al., 2016). Second, if the division becomes aware of securities law violations that were not resolved through the comment letter process, it makes enforcement referrals to the Division of Enforcement (USGAO, 2002b). Enforcement staff in the Division of Enforcement, headquartered in Washington, DC, with eleven regional offices (hereafter the Division, unless otherwise specified), then conduct in-depth investigations to acquire harder evidence from firms and witnesses (Bremser et al., 1991). The Division investigates cases referred from the Division of Corporate Finance or obtained through its surveillance activities or whistleblowers (Blackburne et al., 2020). This two-stage process suggests the two divisions have different focuses, and what we observe from AAERs are the results of these combined processes. Given the nature of each investigation stage, we would naturally expect the Division of Corporate Finance to focus more on the *ex ante* symptoms of misreporting, since it mainly analyzes financial statements or SEC filings. In contrast, the Division would likely seek more direct and *ex post* evidence to build cases successfully.

As proxies for the material misreporting that may be of interest to the Division of Corporate Finance, I use the magnitude of accruals prior to the incidence of misreporting, and the levels of financing needs and insider trading during manipulation periods. I call these *ex ante egregiousness measures*. They are the potential instruments of and incentives for

misreporting.^v I also adopt *expected misreporting amount* and *misreporting period* as proxies for material misreporting. I call these *ex post egregiousness measures* because they can only be determined after in-depth investigations and become the grounds for SEC sanctions.

The first part of my empirical analyses uses the merged file of Compustat, AAERs, and SCALs regarding financial misreporting.^{vi} This *pooled sample* consists of 92,785 firm-years for the 1995-2012 period. To highlight any systematic difference in *enforcement outcomes* of the two misreporting detection channels (i.e., AAERs and SCALs), I construct three subsamples: 1) a pooled sample excluding SCAL firms (*AAER sample*), 2) a pooled sample excluding AAER firms (*SCAL sample*), and 3) a combined sample of the AAER and SCAL firms (*combined sample*). Using the AAER and SCAL samples, I regress AAERs and SCALs on the three *ex ante* egregiousness measures and controls, respectively. Using the combined sample, I then compare the AAER and SCAL firms more directly by regressing AAERs on the same variables.

The second part of the analyses uses a more focused sample of SEC non-enforcements (*non-enforcement sample*), where the pooled sample is merged with the SEC's *investigation target* data (hereafter Freedom of Information Act (FOIA) dataset). The FOIA dataset includes all the Division investigations closed between January 1, 2000, and August 2, 2017. I obtain raw data from Blackburne et al. (2020). The merging process results in 354 misreporting firm-years for 1995-2011 (i.e., the years of misconduct and the class period specified in AAERs and SCALs, respectively) that were investigated by the SEC, and related class action lawsuits could be reasonably assumed to have been settled in the courts (see subsection 2.1.2 for more details). Using this focused sample of investigation targets, I regress the SEC non-enforcement dummy (i.e., non-enforcement vs. enforcement) on the two *ex post* egregiousness measures and controls.

Analysis of the non-enforcement sample has several benefits. Most importantly, it provides an opportunity to examine why the Division does not sanction certain cases referred from the Division of Corporate Finance or reported by whistleblowers after duly conducting investigations. By comparing the analysis results of *enforcement outcomes* (i.e., AAERs) and *investigation targets* (i.e., the Division's investigation), I can also examine how the AAER characteristics were formed through the two-stage investigation process.

Using the AAER sample, I find positive and significant associations between AAERs and the three ex ante egregiousness measures. In contrast, I do not find consistent and significant associations between SCALs and the three measures in the SCAL sample. In the combined sample, I again find positive and significant associations between AAERs and the three variables. These results reveal the egregious nature of AAERs, which should be compelling grounds for the Division of Corporate Finance to make enforcement referrals to the Division.

I then examine the characteristics of the Division's investigation targets using the non-enforcement sample. The results consistently indicate that, compared to non-enforced cases, enforced cases represent clearer evidence of ex post egregiousness. Specifically, I find negative and significant associations between SEC non-enforcements and the two ex post egregiousness measures after controlling for resource constraints. In contrast, the *expected settlement amount* of class actions is significantly and positively associated with court-only cases (equivalently, SEC non-enforcements).^{vii}

Further analyses indicate that case backlogs in the Division allow marginally egregious cases to go undetected. However, they do not appear to critically undermine SEC investigations to detect more material misreporting. Specifically, I find that the Division's case backlogs are positively and significantly associated with SEC non-enforcements, signaling a greater chance of sanction in the absence of resource constraints. Nevertheless, misreporting amounts and periods are significantly and negatively associated with SEC non-enforcements. This

correlation is greater when the level of case backlogs is higher. In contrast, plaintiff investors appear to go forum shopping to earn greater settlement proceeds from class actions against which the SEC did not file cases. I find that SCAL backlogs, rather than total court backlogs, are positively and significantly associated with court-only cases (i.e., SEC non-enforcements). Among the variables available in the non-enforcement sample, misreporting amount and the SCAL backlogs, respectively, explain around 5% of variations in SEC non-enforcements (or court-only cases).

On the other hand, despite the negative and significant correlation between the magnitude of accruals and SEC non-enforcements, I find no significantly moderating effect of accruals (or of the other ex ante egregiousness measures) on the relationship between the ex post egregiousness measures and SEC non-enforcements. These results imply that the Division does not necessarily rely on the ex ante symptoms of earnings management when conducting in-depth investigations. Rather, it is able to acquire stronger direct evidence from firms and witnesses.

Overall, the results consistently indicate systematic differences between AAERs and SCALs. These differences appear to be driven by the SEC's optimization of their detection rate by applying a more stringent standard for material misreporting than the courts. Given resource constraints, this optimization suggests that the SEC's non-enforcement of certain SCALs is reasonably justified.

I examine two additional issues that may support this inference. First, Choi et al. (2009) and Schantl and Wagenhofer (2020) commonly assume the SEC has an information advantage in detecting material misreporting. If we assume this information advantage hypothesis explains the characteristics of AAERs, we should see that class action cases filed *before* the initiation of SEC investigations (where plaintiff investors are less likely to have access to the hard evidence leaked from the SEC and firms) should represent less egregiousness.

Second, Pritchard and Sale (2005) find that district and appellate courts in the Second and the Ninth Circuits have systematically different dismissal rates. They posit this is due to differing interpretations of the pleading standard to prove the defendant's scienter regarding Rule 10b-5. Specifically, the Ninth Circuit is the most stringent; the Second Circuit is the least.

Note that these are court examples. Nevertheless, as the SEC argues (Poser, 2008; see Endnote (i)), if a more stringent screening standard leads to the detection of more egregious cases, we may find that class action lawsuits settled in the most stringent Ninth Circuit represent more egregious misreporting than those settled in the least stringent Second Circuit. As predicted, I find some evidence supporting the SEC's information advantage, and the role of screening standards in forming the characteristics of detected misreporting cases.

Finally, I examine the consequences of SEC sanctions using both non-matched and matched samples, as well as a difference-in-differences (DiD) framework. Relative to non-enforced firms, I find that enforced firms experienced significant drops in operating performance, analyst following, and CEO tenure around SEC sanctions. Due to the pre-trend in outcome variables, this analysis does not guarantee a causal impact of SEC sanctions. But the results from the DiD matching estimation highlight the systematic differences between enforced and non-enforced cases around (but not necessarily "subsequent to") SEC sanctions.

The main contributions of this study are as follows. First, this paper is among the limited body of research that explores public enforcement and private litigation. There has been a controversial debate over their advantages and disadvantages as a deterrent against fraud. On the one hand, Rose (2008) and Bratton and Wachter (2011) support strengthening public enforcement by casting doubt on the justifications for private litigation (i.e., damage compensation and fraud deterrence). On the other hand, Schantl and Wagenhofer (2020) argue that strengthening private enforcement improves deterrence, while strengthening public enforcement does not necessarily do the same (see also Seligman, 1994; and Cox, 1997).

Despite this ongoing debate, only limited literature explores the two institutions jointly or compares their enforcement outcomes. For example, Choi and Pritchard (2016) examine differences between SEC-only and class action-only misreporting cases. However, their focus is on the opposing side of this study: Why do investors sue firms that have been forgone by the SEC? Contrary to Choi and Pritchard (2016), this study poses the opposite question: Why doesn't the SEC sanction certain settled class action lawsuits (even after duly conducted investigations)? This study also adds to Schantl and Wagenhofer (2020) by revealing the factors that affect the SEC's investigation and sanction decisions.

Second, this study is the first to examine the SEC's two-stage investigation process. While several studies have explored the Division of Enforcement's investigation mechanisms (e.g., Bonsall et al., 2019), no studies have yet highlighted the distinct focuses of the Division of Corporate Finance and the Division of Enforcement.

Third, this study complements Karpoff et al.'s (2017) finding that each misreporting database captures only a narrow and limited selection of cases. While the authors focus mainly on the different *coverages* of misreporting databases, this study explicitly examines their distinct characteristics. The results indicate that the egregious nature of AAERs may better serve the accounting studies' purpose of exploring the determinants and mechanisms of intentional misreporting.

The remainder of this paper is structured as follows. Section 2 summarizes my data and research method, while sections 3 and 4 present my empirical results. Section 5 concludes.

2. DATA AND METHOD

This study examines differences between AAERs and SCALs along various dimensions. The sample selection processes are described below.

2.1 Sample and data

2.1.1 Pooled sample

The pooled sample consists of the merged file of Compustat and two misreporting databases: AAERs compiled by Dechow et al. (2011), and settled SCALs (related to financial misreporting) collected from Securities Class Action Clearinghouse. After excluding observations in financial industries (62,209)^{viii} or with missing values (87,840),^{ix} I am left with 92,785 firm-years for the 1995–2012 period. This sample includes 797 misreporting years of AAERs (or 3,820 firm-years of AAER firms) and 838 misreporting years of settled SCALs (or 5,637 firm-years of SCAL firms) against which the SEC did not file cases. Unlike most prior studies, which do not analyze frivolous lawsuits, I include all settled SCALs, because my goal is to gain a deeper understanding of their characteristics.

The sample begins in 1995 with the passage of the Public Securities Litigation Reform Act (PSLRA), which aimed to reduce the filing of frivolous private lawsuits. I chose the terminal year 2012 to ensure the SEC and plaintiff investors had enough time to detect misreporting, and to mitigate the burden of manual collection of data (e.g., misreporting amount and class action lawsuits). The detailed process is summarized in Panel A of Table 1.

From the pooled sample, I construct three subsamples designed to highlight the differences in firm characteristics of the AAER and SCAL firms: 1) the pooled sample excluding SCAL firms (*AAER sample*; $N = 87,148$), where AAER firms can be compared with non-misreporting Compustat firms; 2) the pooled sample excluding AAER firms (*SCAL sample*; $N = 88,965$), where SCAL firms can be compared with non-misreporting Compustat firms; and 3) the combined sample of AAER and SCAL firms (*combined sample*; $N = 9,457$), where the two are compared directly.

When using binary models (e.g., a probit regression model), I analyze fewer observations because of the loss of observations through maximum likelihood estimation, i.e., 79,264 for

the AAER sample, 84,918 for the SCAL sample, and 9,138 for the combined sample. When using the insider trading variable, I analyze even fewer observations because of the additional loss when merging with Thomson Reuters and CRSP, i.e., 39,619 AAERs, 42,623 SCALs, and 7,212 for the combined sample. To avoid the loss of misreporting cases, I do not require firms to be listed on CRSP or Thomson Reuters in some models. However, the results are not qualitatively altered even with the reduced sample size.

[Insert Table 1 here]

2.1.2 Non-enforcement sample

The non-enforcement sample consists of the merged file of the pooled sample and the SEC's FOIA dataset (Blackburne et al., 2020). Note that SCALs were not necessarily investigated by the SEC in the pooled sample. But this focused sample only includes misreporting firm-years investigated by the Division, and related class action lawsuits could be reasonably assumed to have been settled in the courts (i.e., *investigated and settled*). This specification enables me to explicitly observe the characteristics of misreporting cases "condoned" by the SEC after duly conducted investigations. The sample also ensures that cases have more equal chances of SEC non-enforcements (or sanctions).

The FOIA dataset, which includes 3,391 investigation cases closed between 2000 and 2017 in the Division after excluding cases of financial firms (557), cannot be directly merged with the pooled sample. This is because the FOIA dataset does not specify the firm-years that the Division actually investigated. It only notes when the investigations began and ended. It is also not matched with related class actions. The merging process thus requires some assumptions, which must be strict enough to eliminate the potential of classifying non-investigations as investigations or non-settlements as settlements (i.e., Type I error). I acknowledge that the validity of my inference rests on that of these assumptions.

I first require that the absolute value of the difference between the opening date of SEC investigations and the class action filing date does not exceed one year (i.e., two years). These two dates were when the SEC and the courts began their in-depth investigation and discovery processes, respectively. This requirement increases the chance of matching more relevant SEC investigations and class actions. Second, I assume the Division at least investigated the misreporting years specified in AAERs and SCALs (i.e., from the first to the final misreporting years). Although it may also have investigated other firm-years before and after the misreporting periods, this requirement removes the aforementioned Type I error. Finally, the first misreporting date should be earlier than the opening date of the investigation because the SEC cannot investigate future misreporting.

As a result of this process, I identify 361 *investigated and settled* cases (850 misreporting firm-years). After excluding observations outside the sample period (57) and those with missing values (439), I am left with 354 misreporting firm-years for 1995-2011. This means that, conservatively, approximately 42.2% of the settled SCALs in the pooled sample (i.e., 838 misreporting years) were investigated by the SEC. I believe that this level reasonably attenuates the aforementioned validity concern, which ultimately affects the generalization of my inference to the original population distribution and the construction of some variables using expected values. The sample selection process is summarized in Panel B of Table 1.

2.2 Empirical specifications

2.2.1 Misreporting model

Next, to compare the AAER and SCAL firms using the pooled sample ($N = 92,785$) and its three subsamples, I construct a misreporting model as in Eq. (1). This model is largely consistent with Dechow et al.'s (2011) misreporting prediction model, and revised to accommodate additional controls suggested by prior literature (e.g., Dechow et al., 1996; and

Khanna et al., 2015). The dependent variable is financial misreporting, which I capture using *AAERs* for the AAER and combined samples, and *SCALs* for the SCAL sample. *AAERs* is an indicator variable that equals 1 for misreporting firm-years against which the SEC filed cases, and 0 otherwise. Similarly, *SCALs* is an indicator variable for the misreporting firm-years against which plaintiff investors filed class actions but the SEC did not, and 0 otherwise.

The main variables of interest are three *ex ante egregiousness measures*: accruals, the actual issuance of stocks and debts (*Financing needs (chg)*), and insider trading (*Insider trading*). The larger magnitude of accruals represents a greater likelihood of misreporting, because accruals are used as an instrument to manipulate earnings. Specifically, I use the three-year accumulation of accruals to capture their reversal effect on the violations of Generally Accepted Accounting Principles (GAAP).

According to Ettredge et al. (2010), the frequent and aggressive adoption of accruals prior to an incidence of misreporting decreases a firm's capacity to manage earnings within legitimate boundaries. Consequently, this can cross into a GAAP violation. Dechow et al. (2011) also observe systematically higher accruals prior to incidences of misreporting. To consider distinct characteristics of various accruals measures (Christensen et al. 2021), I use five alternatives: the modified Jones accruals (*MJONES*), modified Jones accruals with current-year ROA (*PMJOES*), forward-looking modified Jones accruals (*FMJONES*), working capital accruals (*WC*), and total accruals (*TA*).

To maintain sufficient misreporting cases in the sample, I minimize the data requirements. Among the five accruals measures, I require firms to have full observations for *MJONES* only; in the robustness tests using alternative accruals measures, the others have fewer than or equal to 92,785 observations (i.e., ranging from 81,892 to 92,785). In the analyses with reduced observations, my results largely remain unchanged.

Financing needs (chg) and *Insider trading* are contemporaneous variables that capture the two traditional incentives to misreport, i.e., external financing needs and managerial equity-based portfolios. *Financing needs (chg)* is the sum of the change in common stocks, preferred stocks, and total liabilities, all deflated by total assets. *Insider trading* is the monetary value of stock sales.

Control variables are selected on the grounds that firms misreport to inflate earnings (*ROA (chg)*) using accounts receivable (*Rec (chg)*), soft assets (*S_assets*), and even cash sales (*C_sales (chg)*). To accommodate misreporting firms' optimistic prospects, I also add inventory increase rate (*Inv (chg)*). Firm age (*F_age*) is included because young growth firms are more likely to misreport due to their strong external financing needs (Beneish, 1999).

Additionally, the model includes asset size (*Assets*), Fortune 500 membership (*Fortune*), and stock markets (*Stock_mkt*), because more stringent monitoring tends to be imposed on larger firms – in terms of assets and revenues (e.g., Correia, 2014) – and those listed on major stock markets (Hope et al., 2013). On the other hand, *Stock_mkt* may be associated with firms' opportunistic motivation to obtain financing from stock markets (McTier and Wald, 2011). Similarly, leverage (*Lev*) has ambiguous implications. While highly leveraged firms are more likely to be tightly monitored by creditors, they may also have a greater motivation to misreport to avoid debt covenant violations (Dechow et al., 1996). Finally, I add year dummies to control for economic conditions that may affect misreporting over time, and industry dummies to address time-invariant heterogeneities.^x

To highlight any systematic differences between the AAER and SCAL firms, I run Eq. (1) using the AAER and SCAL samples, respectively. I then compare the size and statistical significance of the coefficients on the three ex ante egregiousness measures. Using the combined sample and *AAERs* as the dependent variable, I re-run Eq. (1) to compare the AAER and SCAL firms more directly.

$$\begin{aligned}
Pr(\text{Misreporting})_t = & \alpha_0 + \alpha_1 \text{Accruals} + \alpha_2 \text{Financing needs (chg)}_t \\
& + \alpha_3 \text{Insider trading}_t + \alpha_4 \text{Rec (chg)}_{t-1} + \alpha_5 \text{Inv (chg)}_{t-1} \\
& + \alpha_6 \text{S_assets (chg)}_{t-1} + \alpha_7 \text{C_sales (chg)}_{t-1} + \alpha_8 \text{ROA (chg)}_{t-1} \\
& + \alpha_9 \text{F_age}_{t-1} + \alpha_{10} \text{Assets}_{t-1} + \alpha_{11} \text{Fortune}_{t-1} + \alpha_{12} \text{Lev}_{t-1} \\
& + \alpha_{13} \text{Stock_mkt}_{t-1} + \sum \text{Year fixed effect} + \sum \text{Industry fixed effect} + \varepsilon_t \quad (1)
\end{aligned}$$

2.2.2 Non-enforcement model

I also construct a non-enforcement model as in Eq. (2) to be analyzed using the non-enforcement sample ($N = 354$). As noted, this research design enables me to explicitly explore the SEC's non-enforcement (or sanction) decisions even after duly conducting investigations. Accordingly, this model includes variables related to SEC investigations and judicial reviews in addition to all the variables in Eq. (1) (i.e., $\sum \text{Controls}$).

The dependent variable is SEC non-enforcements (*Non-enforcement*), which is set to 1 for firm-years against which class action lawsuits were filed and settled, but for which the SEC did not file cases. As the main determinant of SEC non-enforcements, I include two *ex post egregiousness measures*: $E(\text{Mis_amt})$ and Mis_prd . $E(\text{Mis_amt})$ is the expected misreporting amount that the Division may have reasonably identified at the time of investigation closure. This measure is the one-line summary of SEC investigations, and constitutes the compelling basis for SEC sanctions. More specifically, it is the natural logarithm of the *actual* misreporting amount specified in AAERs or the restated amount, divided by the number of misreporting years.^{xi} Given that the non-enforcement sample includes only misreporting firm-years, sample firms should have restated misreporting identified through the SEC investigations or court discovery. All remaining missing values are then replaced with the *expected* misreporting amount estimated using the three *ex ante* egregiousness measures and controls, as well as year and firm fixed effects (Appendix 1). While this variable specification is appropriate to explain the Division's decision to sanction a certain misreporting firm-year, I alternatively use the total – rather than average – expected misreporting amount of each case ($E(\text{Mis_amt_alt})$) during

the entire misreporting period. The results are robust to the adoption of this alternative specification.

Mis_prd is the natural logarithm of the difference between the first and final misreporting dates. The misreporting dates are acquired from AAERs, and, for missing values, from Audit Analytics (restatements) and settled class actions in order.

The case backlogs in regional offices (*Backlog_SEC*) are also included because they may limit the Division's ability to investigate even material misreporting cases (Bonsall et al., 2019).^{xiii} *Backlog_SEC* is the natural logarithm of the annual average of open cases that a regional office was investigating over the investigation period. Additionally, I include the investigation period of each case (*Invest_prd*) to control for the Division's time and effort (Heese et al., 2020).

I also add variables to capture potential geographic (e.g., Kedia and Rajgopal, 2011) and political (e.g., Correia, 2014; and Yu and Yu, 2011) biases in SEC enforcement: distance from a firm's headquarters and closest SEC offices (*Distance*), and firms' contributions to Political Action Committees (*Political_cont*), respectively.

In the non-enforcement sample, the flip side of SEC non-enforcements is court-only settlement. I therefore include four variables that are likely to affect courts' settlement decisions. According to Schantl and Wagenhofer (2020), investors will have weaker incentives to file lawsuits if the SEC does not sanction them. This implies that investors must have sufficient incentives to offset legal costs in order to sue against non-enforced cases. The case backlogs in judicial circuits (*Backlog_lawsuits*) are thus added because the chances of settlement may be higher if the private litigation regime is not working effectively due to an increased workload. *Backlog_lawsuits* is the natural logarithm of the annual average of open lawsuits, including federal, civil, criminal, bankruptcy, and appellate court cases, that a judicial circuit was reviewing during the proceeding. Bid-ask spread (*BA_sprd*) is included because investors are

more likely to file cases when they find more information asymmetries in the case firms (Choi and Pritchard, 2016).

As the main incentive for plaintiff investors to sue, the expected settlement amount per share ($E(\text{Settle_amt})$) is also included. $E(\text{Settle_amt})$ is the *actual* settlement amount from SCALs settled before the SEC closed its investigations and the *expected* settlement amount for missing values, all divided by the number of shares. Here, I use expected values, because the SEC can only consider actual settlement amounts when class actions have been settled *before* the closure of investigations. The expected settlement amount is also estimated using the same approach as $E(\text{Mis_amt})$ (Appendix 1). The results are qualitatively similar even when actual settlement amounts are used for all observations (untabulated). Additionally, the judicial review period (Rev_prd) is added to control for the time spent by each case in the courts.

Finally, the model includes several fixed effects to control for unobservable characteristics in different industries, years, regional offices, and judicial circuits.

$$\begin{aligned}
Pr(\text{Non-enforcement})_i = & \beta_0 + \beta_1 E(\text{Mis_amt})_i + \beta_2 \text{Mis_prd}_i + \beta_3 \text{Backlog_SEC}_i \\
& + \beta_4 \text{Invest_prd}_i + \beta_5 \text{BA_sprd}_i + \beta_6 E(\text{Settle_amt})_i + \beta_7 \text{Backlog_court}_i \\
& + \beta_8 \text{Rev_prd}_i + \beta_9 \text{Distance}_i + \beta_{10} \text{Political_cont}_i + \sum \text{Controls} \\
& + \sum \text{Year fixed effect} + \sum \text{Industry fixed effect} + \sum \text{Regional office fixed effect} \\
& + \sum \text{Judicial circuit fixed effect} + \zeta_i
\end{aligned} \tag{2}$$

2.2.3 DiD matching estimator

To examine the consequences of SEC sanctions, I employ the DiD model shown in Eq. (3). As outcome variables, I employ the following: sales (Sales), operating ROA (Op_ROA), number of analysts following (Analyst), and CEO tenure (CEO_ten). Sales and Op_ROA are analyzed because SEC sanctions are likely to undermine firms' product market reputations, and thus degrade operating performance (Leng et al. 2011; and Chakravarthy et al., 2014). Analyst is examined because analysts may lose confidence in sanctioned firms and consequently leave them (Dechow et al., 1996). I also analyze CEO_ten because firms tend to dismiss culpable CEOs after SEC sanctions (Karpoff et al., 2008; and Beneish et al., 2017).

In this model, the treatment is SEC sanctions (SEC_sanc), which I capture using the filing of AAERs. At this time, SEC sanctions are virtually confirmed, because 99.4% of filed AAERs ultimately reach a settlement (Ramphal, 2007). Specifically, SEC_sanc represents *all* firm-years of firms for which the SEC filed AAERs at least once during the sample period, and 0 otherwise. $Post$ equals 1 for all firm-years since the year of SEC sanction for enforced firms and the year of class action filing for non-enforced firms, and 0 otherwise. The coefficient on $SEC_sanc \times Post$ thus captures the effect of SEC sanctions on the four outcome variables. Finally, I include year fixed effects because treatment years differ for misreporting cases. I add firm fixed effects to control for time-invariant unobservable firm heterogeneity.

First, I estimate the model by using a non-matched sample extended from the non-enforcement sample by including misreporting and non-misreporting firm-years. This extension is necessary to capture any dynamic change in outcome variables around the treatment. After excluding firms that only existed before or after the treatment (20), and those that fall outside the five-year sampling window (5), I am left with 132 firms, or 977 firm-years. The sampling process is summarized in Panel A of Appendix 2.

I then construct a propensity score-matched (PSM) sample from the non-matched sample to mitigate endogeneity concerns. For example, in the non-matched sample, sanctioned cases may have inherently different chances of SEC sanctions (or non-enforcements) if the two groups of firms are unbalanced in the covariates associated with the dynamics of the outcome variables. Figure 1 shows that the four outcome variables have pre-trends before the filing of AAERs (solid line) or class actions (dashed line), i.e., $Year = 0$. Moreover, here, it is not clear *exactly when* class action lawsuits were “condoned” by the SEC. Therefore, the years of the AAER and SCAL filings may not be comparable, because they have different chances of settlement (Ramphal, 2007).

While the PSM does not fundamentally improve the pre-trend, it still enables researchers to identify matched pairs whose ex ante probability of SEC non-enforcements is the most similar, but whose actual non-enforcement decisions were the opposite. Moreover, matching attenuates the concern that the non-enforced years for class actions are not clear. In the PSM sample, the treatment is the year of AAER filing for enforced firms and the *matched year* for non-enforced firms. Accordingly, *Post* equals 1 for all firm-years since the matched year for the non-enforced firms, and 0 otherwise.

Overall, I believe the DiD matching estimation is less contaminated by endogeneity concerns, and can at least highlight any systematic differences between enforced and non-enforced cases *around* (but not necessarily “subsequent to”) SEC sanctions. Despite these differences, the results of the analysis are qualitatively similar for the non-matched and PSM samples.

More specifically, to construct the PSM sample, I identify matched pairs of enforced and non-enforced cases as of the filing year of AAERs (i.e., enforcements) based on the ex ante probability of SEC sanctions. The propensity scores are estimated using the PSM model in Eq. (4), which includes key variables from Eq. (2) that were significantly associated with SEC non-enforcements and that are relevant to SEC investigations and court discovery (i.e., $E(\text{Mis_amt})$, Mis_prd , Backlog_SEC , Invest_prd , $E(\text{Settle_amt})$, and Backlog_court). Total assets (Assets) are then included to control for the size effect. Here, I adopt a reduced version of Eq. (2) as the PSM model, because the maximum likelihood estimation of Eq. (2) does not converge due to the small sample size in each year. Based on the propensity scores, matches are chosen without replacement for the propensity score caliper of 0.05 ($N = 140$). The matching process and covariate balance are summarized in Panels B and C of Appendix 2, respectively.

$$\text{Outcome}_t = \gamma_0 + \gamma_1 \text{SEC_sanc}_t + \gamma_2 \text{Post}_t + \gamma_3 \text{SEC_sanc}_t \times \text{Post}_t + \sum \text{Year fixed effect} + \sum \text{Firm fixed effect} + \eta_t \quad (3)$$

$$Pr(\text{Non-enforcement})_t = \delta_0 + \delta_1 E(\text{Mis_amt})_t + \delta_2 \text{Mis_prd}_t + \delta_3 \text{Backlog_SEC}_t + \delta_4 \text{Invest_prd}_t + \delta_5 E(\text{Mis_amt})_t + \delta_6 \text{Backlog_court}_t + \delta_7 \text{Assets}_t + \theta_t \quad (4)$$

[Insert Figure 1 here]

3. RESULTS

3.1 Descriptive statistics: Pooled sample

Table 2 summarizes the variables in the pooled sample ($N = 92,785$), along with their mean values, differences in mean values, and p -values from t -tests denoted by asterisks.

In Panel A, columns (6) and (7) report the mean differences between the Compustat firms and the AAER and SCAL firms, respectively. Compared to the Compustat firms, both the AAER and SCAL firms exhibit higher magnitudes of three ex ante egregiousness measures. Those firms also have larger asset sizes (*Assets*). In contrast, the differences between the AAER and SCAL firms in column (5) are not highly salient. For example, the AAER firms exhibit greater *Insider trading* than the SCAL firms, while the magnitude of accruals (e.g., *MJONES*) is generally not significantly different between them. *Financing needs (chg)* is rather significantly greater for SCAL firms, and *Assets* is not significantly different.

These results do not consistently support the notion that AAER firms are systematically different and more likely to represent material misreporting. This may be because the statistics in columns (1) and (2) include both misreporting and non-misreporting firm-years. But the firm characteristics before and during the misreporting periods may be different, as shown by Dechow et al. (2011). Moreover, univariate analyses do not hold other conditions equal when comparing a specific firm characteristic. Therefore, in the following subsections, I illustrate the trend of accruals around the first misreporting year and adopt the multivariate models.

In Panel C, the Spearman correlation results are largely consistent with those reported in Panel A. Specifically, both *AAERs* and *SCALs* are positively correlated with the three ex ante egregiousness measures. Note also that there is a highly significant and negative correlation

between *AAERs* and *SCALs* (-0.009 with a p -value < 0.01). This is natural because *SCALs* are, by definition, misreporting cases that were settled in the courts but *not* sanctioned by the SEC.

[Insert Table 2 here]

3.2 Graphical analysis

The four graphs in Figure 2 illustrate the trends of the three-year accumulation of accruals (i.e., *MJONES*, *PMJOMES*, *FMJONES*, and *WC*) around the first misreporting year (i.e., $Time = 0$). Using the pooled sample ($N = 92,785$), the graphs consistently exhibit that the four different accruals gradually increase over the years prior to the misreporting, but those trends are reversed during the misreporting years. Recall that the overall accruals levels were not significantly different between the *AAER* and *SCAL* firms over the entire sample period (column (5) of Panel A in Table 2). Nevertheless, the magnitudes of accruals of the *AAER* firms (the solid line) are consistently larger than those of the *SCAL* firms (the dashed line) during misreporting periods. These patterns are consistent with those found in Ettredge et al. (2010) and Dechow et al. (2011), and suggest that aggressive earnings management prior to misreporting leads to GAAP violations.

[Insert Figure 2 here]

3.3 Ex ante egregiousness

3.3.1 Indirect comparison

Table 3 presents the probit estimation results of Eq. (1), where I regress financial misreporting on the three ex ante egregiousness measures and controls. For this analysis, I run the regressions using the *AAER* (39,619-79,264 observations in columns (1)-(4)) and *SCAL* (42,623-84,918 observations in columns (6)-(9)) samples. In columns (5) and (10), where large firms are used as a control group, I analyze 19,408-20,034 observations. Despite the decrease in sample size, the results remain largely unchanged.

In column (1), using the AAER sample, I first regress *AAERs* on *MJONES*, with year and industry fixed effects only. The focus here is to compare the AAER firms with non-misreporting firms in Compustat after excluding the SCAL firms. The positive and significant coefficient on *MJONES* (0.118; p -value < 0.01) indicates that the AAER firms adopt a significantly greater magnitude of accruals prior to the incidence of misreporting. The positive coefficient on *MJONES* remains significant even after controlling for all variables included in Eq. (1), except *Financing needs (chg)* and *Insider trading* (column (2)).

In column (3), the sign and size of the coefficient on *MJONES* remain qualitatively unchanged even after adding *Financing needs (chg)* to column (2). The positive and significant coefficient on *Financing needs (chg)* (0.174; p -value < 0.01) indicates that the AAER firms have greater financing needs than the Compustat firms during misreporting periods.

In column (4), despite the inclusion of *Insider trading*, the coefficients on *MJONES* and *Financing needs (chg)* remain qualitatively unchanged. *Insider trading* exhibits a positive and significant incremental effect (0.009; p -value < 0.05) on the probability of detected misreporting. This implies that CEOs in AAER firms have stronger incentives for insider trading during misreporting periods than those in the non-misreporting firms in Compustat.

Untabulated results confirm that the results are robust to the adoption of Dechow et al.'s (2011) misreporting prediction model, where *contemporaneous total accruals* and *an indicator variable of financing needs* are used with the logistic regression estimation (Dechow et al., 2011, Model 1, Table 7).

Next, using the SCAL sample, I conduct the same analysis with a new aim of comparing SCAL firms with non-misreporting firms in Compustat after excluding AAER firms. In contrast to my results for the AAER sample in columns (1)-(4), columns (6)-(9) reveal that the ex ante egregiousness measures are not significantly associated with *SCALs*.

In columns (5) and (10), I use large firms, with assets exceeding \$500 million, as an alternative control group to test the robustness of my results above. Large firms are suitable because both public and private enforcement targets tend to be large in asset size. In the AAER sample (column (5)), the results remain largely unchanged except those for *Insider trading*: Relative to large firms, AAER firms still exhibit greater accruals and financing needs. Likewise, in the SCAL sample (column (10)), although the findings are mixed, I do not find consistent evidence that SCAL firms show greater ex ante egregiousness than large firms. Specifically, *MJONES* is positively and significantly associated with *SCALs*; *Financing needs (chg)* and *Insider trading* are rather negatively and significantly associated with *SCALs*. Untabulated results using another control group whose asset size exceeds \$750 million are also qualitatively similar.

[Insert Table 3 here]

3.3.2 Direct comparison

Table 4 reports the estimation results for Eq. (1) using a more direct method. Given that I used the same control group for both AAER and SCAL firms in Table 3, a direct comparison should generate the same results. Indeed, using the *combined sample*, which includes only AAER and SCAL firms, I find results consistent with those reported in Table 3. Again, due to maximum likelihood estimation, and the merging with Thomson Reuters and CRSP, I analyze from 4,337 to 9,138 observations in this analysis.

The probit estimation results in column (1) of Panel A show that, even in the combined sample, *MJONES* is positively and significantly associated with *AAERs* with year and industry fixed effects only (0.230; p -value < 0.01). Columns (2) and (3) further report that the coefficients on *Financing needs (chg)* (0.237; p -value < 0.01) and *Insider trading* (0.084; p -value < 0.01) are also highly significant and positive with the same controls.

The results remain the same even when I include all controls in Eq. (1) (columns (4)-(6)), or adopt the linear probability model (LPM; column (7)). Untabulated results indicate that, in the full model reported in column (6), the marginal effects of *MJONES*, *Financing needs (chg)*, and *Insider trading* are associated with an increase in the probability of detected misreporting, by 1.76, 5.21, and 0.21 percent points, respectively, holding other variables at their means.

Columns (8)-(11) present the estimation results of Eq. (1) using a simple hazard model that accounts for both the occurrence of misreporting and the time spent without misreporting (Shumway, 2001). This model addresses any potential right censoring bias arising from the fact that misreporting firms may not have been detected yet by the SEC at the end of the sampling period. The positive and significant coefficients on *MJONES* and *Financing needs (chg)* affirm that the results estimated using the standard probit model and LPM are largely robust to adopting this alternative estimation model.

Columns (1)-(4) of Panel B then confirm the robustness of the main results (i.e., column (6) of Panel A) regarding the adoption of alternative accruals measures: *PMJONES*, *FMJONES*, *WC*, and *TA* (see Appendix 1 for more details). Overall, the results consistently suggest that, relative to SCALs, AAERs represent more of the material misreporting captured by the three ex ante egregiousness measures.

[Insert Table 4 here]

4. FURTHER ANALYSIS

The tests in the previous section establish that AAERs exhibit distinct characteristics compared to SCALs. However, the results do not reveal the hidden investigation mechanisms within the SEC. For example, consider the well-known positive association between the magnitude of accruals and misreporting (e.g., Jones et al., 2008). This correlation does not necessarily imply that, despite their means to acquire more direct evidence from firms and witnesses (by, e.g.,

issuing subpoenas), the Division of Enforcement will still rely on the preliminary evidence of fraud (e.g., accruals) throughout its investigations. Thus, in this section, I focus on identifying the factors the Division may consider when making enforcement (non-enforcement) decisions.

4.1 Ex post egregiousness

4.1.1 Descriptive statistics: Non-enforcement sample

Panels A and B in Table 5 report the summary statistics for the variables in the non-enforcement sample ($N = 354$). Panel A compares the enforced and non-enforced firms. Column (1) includes misreporting firm-years investigated and sanctioned by the SEC, of which related class action lawsuits were settled in the courts (*Enforced*; $N = 128$). In contrast, column (2) includes misreporting firm-years investigated but *not* sanctioned by the SEC, despite the settlement of related class actions in the courts (*Non-enforced*; $N = 226$).

The comparison in column (3) (*Diff.*) shows that the mean values of two *ex post egregiousness measures* are different. Specifically, enforced firms exhibit greater expected misreporting amounts ($E(\text{Mis_amt})$; $p\text{-value} < 0.01$) and longer misreporting periods (Mis_prd ; $p\text{-value} < 0.01$) than non-enforced firms. In contrast, non-enforced firms show significantly greater expected settlement amounts ($E(\text{Settle_amt})$; $p\text{-value} < 0.01$). I cannot draw any conclusive interpretations from these univariate analyses, but it appears that firm evidence of misreporting is a potential determinant of SEC sanctions. Monetary incentives from settlements appear to relate to court-only cases.

Case backlogs in the SEC and courts are typically greater for non-enforced firms (Backlog_SEC and Backlog_lawsuits , with $p\text{-value} < 0.05$ and $p\text{-value} < 0.01$, respectively). This signals a potential decrease in SEC non-enforcements and court-only settlements in the absence of resource constraints.

On the other hand, I do not observe consistent and significant differences in the three ex ante egregiousness measures in the non-enforcement sample. Specifically, *MJONES* and *Financing needs (chg)* are not significantly different, and *Insider trading* is marginally greater in the non-enforced group (p -value < 0.10). The Spearman correlation results in Panel C further reveal that, despite the negative and significant correlation between *Non-enforcement* and *MJONES* (-0.139 with a p -value < 0.01), *MJONES* is not significantly correlated with most of the ex post egregiousness measures (e.g., $E(\text{Mis_amt})$). I discuss these unexpected results in the following subsection.

[Insert Table 5 here]

4.1.2 Regression analysis

Using the non-enforcement sample, Table 6 reports the LPM estimation results of Eq. (2). Here, I adopt the LPM because of the frequent convergence failures (Agresti and Kateri, 2011) when using the probit model with a relatively small sample size. However, the adoption of probit estimation with a reduced model indicates that the results are largely robust to the alternative estimation method (untabulated).

SEC INVESTIGATION Column (1) reports negative and significant coefficients for $E(\text{Mis_amt})$ (-0.019 with a p -value < 0.01) and Mis_prd (-0.041 with a p -value < 0.01) with all variables included in Eq. (1). The negative coefficients indicate that cases with more ex post evidence of material misreporting are less likely to be unsanctioned (i.e., more likely to be sanctioned) by the Division. In contrast, the three ex ante egregiousness measures (i.e., *MJONES*, *Financing needs (chg)*, and *Insider trading*) are not significantly associated with *Non-enforcement* in this specification (coefficients range from -0.077 to -0.001 , with p -values > 0.10). A plausible explanation is that the Division seeks more direct and stronger evidence of misreporting through its in-depth investigations, rather than simply relying on preliminary

evidence (e.g., accruals).

Column (2) includes *Backlog_SEC* and *Invest_prd*. The positive and significant coefficient on *Backlog_SEC* (0.150; p -value < 0.01) suggests that resource constraints limit the Division's ability to investigate misreporting cases. In contrast, the negative but insignificant coefficient on *Invest_prd* (-0.017; p -value = 0.144) implies that SEC non-enforcements decrease with the time spent by the Division to investigate cases after controlling for case backlogs.^{xiii} Nevertheless, the effects of ex post egregiousness measures (i.e., $E(\text{Mis_amt})$ and *Mis_prd*) are significantly incremental to the resource constraints.

COURT DISCOVERY Court-only settlement is the flip side of SEC non-enforcement. Column (3) reports a positive and significant coefficient on *BA_sprd* (7.964 with a p -value < 0.05), with all the variables included in Eq. (1) but without year and industry fixed effects. This implies that the cases in which stock traders perceive more information asymmetry were settled more often in the courts even without SEC enforcements. However, it loses statistical significance when year and industry fixed effects are included in column (4) (-5.421 with a p -value > 0.10). Column (5) adds $E(\text{Settle_amt})$, *Rev_prd*, and *Backlog_lawsuits*, all of which are not statistically significant. In column (6), I split *Backlog_lawsuits* into *Backlog_SCALs* and *Backlog_lawsuits_less*. Among these two variables, only *Backlog_SCALs* is statistically significant (0.111 with a p -value < 0.01), signaling the potential for exploitation of private litigation by plaintiffs' attorneys to reach higher settlements (e.g., forum shopping).

FULL MODEL In columns (7) and (8), I note that including all the aforementioned variables and two potential SEC enforcement biases (i.e., *Distance* and *Political_cont*) largely does not alter the results. However, with the additions of regional office and judicial circuit fixed effects in column (9), *Invest_prd* and $E(\text{Settle_amt})$ gain statistical significance (-0.039 and 0.273 with p -value < 0.05 and p -value < 0.01 , respectively). Taken together, in this full model, $E(\text{Mis_amt})$, *Mis_prd*, and *Invest_prd* appear to be the facilitating factors of SEC

enforcements, while *Backlog_SEC* is a mitigating factor. From the perspective of private litigation, both $E(\text{Settle_amt})$ and *Backlog_SCALs* appear to promote court-only settlements. On the other hand, among the three ex ante egregiousness measures, only *MJONES* is significantly and negatively associated with SEC non-enforcements (-0.107 with a p -value < 0.05). These results are robust to the adoption of an alternative misreporting amount measure ($E(\text{Mis_amt_alt})$ in column (10).

Column (11) reports the R^2 s of the regression models, where *Non-enforcement* is regressed on each variable in Eq. (2) without any controls. Thus, this represents how much of the variation in *Non-enforcement* is explained by each variable. Among the variables significantly associated with *Non-enforcement* in columns (7) and (8), *Invest_prd* explains the greatest proportion of the variation (10.17%). Note, however, that *Invest_prd* is a relatively noisy variable compounded by various factors such as resource constraints in the Division and the egregiousness of cases. More importantly, $E(\text{Mis_amt})$ and *Backlog_SCALs* explain 5.48% and 5.51% of the variation, respectively. This suggests that stronger evidence of misreporting ($E(\text{Mis_amt})$) and any exploitation of the private litigation regime (*Backlog_SCALs*) are the main determinants of SEC enforcements and court-only settlements, respectively.

In contrast, the relatively less substantial influence of *MJONES* on non-enforcement (1.93%) suggests that the Division's investigations add value to the SEC's detection of material misreporting. It is able to pursue more direct evidence of material misreporting than simply relying on the preliminary analysis of financial statements (e.g., accruals).

[Insert Table 6 here]

4.2 Mechanism of non-enforcement

Next, I aim to understand the non-enforcement mechanism (equivalently, court-only settlements) in the Division (in the courts) in more detail. I conduct subsample analyses that

examine: 1) the impact of case backlogs in the Division and judicial circuits on their enforcement actions, 2) the interaction between misreporting and settlement amounts, and 3) the role of ex ante evidence of egregious misreporting throughout Division investigations.

Table 7 reports the LPM estimation results of Eq. (2) using subsamples constructed by dividing the non-enforcement sample at the median values of *Backlog_SEC*, *Backlog_lawsuits*, *E(Settle_amt)*, and *MJONES*. Again, I adopt the LPM due to the convergence failure of maximum likelihood estimation.

First, I analyze the impact of *Backlog_SEC* on the relation between ex post egregiousness measures (e.g., *E(Mis_amt)*) and SEC non-enforcement. In general, case backlogs tend to hinder SEC enforcement (Bonsall et al., 2019). In Table 6, I also find that *Backlog_SEC* is incrementally and positively associated with SEC non-enforcement. In this regard, Table 7, columns (1) and (2), reveal that the negative coefficients on *E(Mis_amt)* are significantly greater when the level of *Backlog_SEC* is higher (-0.025 vs. -0.005; *p*-value for the difference < 0.01). This implies that the Division is more likely to file cases with firmer evidence of material misreporting when their enforcement capacity is constrained. I posit that, by doing so, the Division may increase the chance of winning cases even when it is unable to use its best efforts. This may explain the SEC's low dismissal rate of public lawsuits (i.e., 0.04% in Ramphal, 2007). Thus, it appears that the case backlogs in the Division do not critically undermine its investigations to detect more material misreporting.

Resource constraints may allow marginally egregious cases to go unsanctioned. One solution could be for the Division to increase its investigation periods. However, while the coefficient on *Invest_prd* is greater when the level of *Backlog_SEC* is higher, the difference is not statistically significant (-0.051 vs. -0.038; *p*-value for the difference > 0.10).

In columns (3) and (4), I do not find that the relationship between *E(Mis_amt)* and *Non-enforcement* differs significantly depending on the level of *Backlog_lawsuits*. Specifically,

regardless of the levels of case backlogs, the positive coefficients on $E(\text{Settle_amt})$ are not different at the conventional level (0.436 vs. 0.314; p -value for the difference = 0.392).

Second, I examine the interaction between $E(\text{Mis_amt})$ and $E(\text{Settle_amt})$ to explain their opposing coefficients reported in column (9) of Table 6 (i.e., -0.017 and 0.273, respectively). The results are inconsistent with the positive and significant correlation between the two (i.e., 0.117 with a p -value < 0.01). In columns (5) and (6), the difference in $E(\text{Mis_amt})$ is not statistically significant at the conventional level (p -value = 0.186), but the negative coefficient on $E(\text{Mis_amt})$ is greater and significant when $E(\text{Settle_amt})$ is also greater (-0.021; p -value < 0.01 in column (5)). This means that misreporting cases with greater $E(\text{Mis_amt})$ and $E(\text{Settle_amt})$ are sanctioned at higher rates than they are missed by the SEC.

Third, I examine whether the Division uses the preliminary evidence of material misreporting acquired from the analysis of financial statements (e.g., accruals) in conjunction with ex post evidence (e.g., misreporting amount). In columns (7) and (8), different levels of *MJONES* do not exhibit any significant impact on the relation between ex post egregiousness measures and SEC non-enforcements. Untabulated results are qualitatively similar for other ex ante egregiousness measures.

These results are consistent with those in Table 6, where ex ante egregiousness measures, except for accruals, were not significantly and consistently associated with SEC non-enforcements. As the two-stage investigation process implies, this may be because the Division does not necessarily rely on the preliminary evidence of misreporting. It is able to obtain more direct evidence from firms and witnesses (by, e.g., issuing subpoenas). If that is the case, the greater ex ante egregiousness in AAERs reported in Table 4 may be driven by the screening role of the Division of Corporate Finance; the ex post egregiousness reported in Table 6 may be driven more by the in-depth investigations in the Division of Enforcement. The distinct focus of each division thus explains how the SEC builds cases successfully despite limited

resources.

[Insert Table 7 here]

4.3 Timing and the judicial court of class actions

4.3.1 Timing of class action filing

Choi et al. (2009) argue that SEC enforcement actions provide stronger evidence of fraud for plaintiff investors because of its information advantage in detecting material misreporting. In a similar vein, Schantl and Wagenhofer (2020) also assume that SEC enforcement is timelier than private litigation. Thus, the timing of lawsuit filings may affect the characteristics of the evidence included in class action lawsuits. If this is the case, plaintiff investors who sued *even before* the initiation of the SEC investigation are less likely to be able to use the hard evidence leaked from the SEC and firms.

If this hypothesis holds, and can thus explain the egregiousness of AAERs, a natural prediction is that class action lawsuits that could not rely on the SEC's information advantage will exhibit less egregiousness. To test this prediction, I adopt a timing dummy (*Early_file*), an indicator variable that equals 1 for misreporting firm-years against which class actions were filed *before* the SEC began an investigation, and 0 otherwise.

Table 8, columns (1) and (2), report the subsample analysis results using Eq. (2), where the non-enforcement sample is split based on *Early_file*. Here, I split *Backlog_lawsuits* into *Backlog_SCALs* and *Backlog_lawsuits_less* to observe their potentially heterogeneous impact. Columns (1) and (2) show that the negative coefficients on $E(\text{Mis_amt})$ are significantly smaller when class actions were filed before the initiation of an SEC investigation (-0.019 vs. -0.035; p -value for the difference = 0.095). Furthermore, although the coefficients are not significant at the conventional level, there is some evidence that the positive coefficients on $E(\text{Settle_amt})$ are greater in column (2) (0.328 vs. 0.491; p -value for the difference = 0.125).

This suggests that plaintiff investors garnered higher settlement amounts from the class actions filed after the initiation of an SEC investigation. Together, these results are consistent with the information advantage hypothesis.

4.3.2 *Judicial circuit*

Pritchard and Sale (2005) find that district and appellate courts in the Second and Ninth Circuits, two leading circuits for filing securities class actions, have systematically different dismissal rates. They posit this is because each interprets the pleading standard to prove defendants' scienter regarding Rule 10b-5 differently: the Ninth Circuit is the most stringent; the Second Circuit is the least so.

According to Poser (2008), the SEC has urged the Court to interpret securities laws such as the loss causation requirement more strictly. The SEC's position suggests that its strict screening standards may explain the egregiousness of AAERs. In such a case, (although these are court examples) class actions settled in the most stringent Ninth Circuit should also be more egregious than those settled in the least stringent Second Circuits.

Consistent with this prediction, columns (3) and (4) show that $E(\text{Mis_amt})$ and Mis_prd are significantly and negatively associated with *Non-enforcement* only in the Ninth Circuit subsample (coefficients are -0.034 and -0.058, respectively). And the differences in these coefficients are statistically significant (p -values for the difference < 0.01). Interestingly, in the Second Circuit subsample, Backlog_SCALs is positively and significantly associated with *Non-enforcement* (i.e., court-only settlement; 0.279 with a p -value < 0.01). The difference in the coefficients on Backlog_SCALs is also statistically significant (p -value < 0.10). This suggests that plaintiff investors may be exploiting the private litigation regime when the courts apply looser pleading standards. Together, these results suggest that more stringent screening standards lead to the detection of more material misreporting cases, and to the non-enforcement

of less egregious ones.

[Insert Table 8 here]

4.4 Consequences of SEC sanctions

Table 9 reports the DiD estimation results of Eq. (3). Four outcome variables (i.e., *Sales*, *Op_ROA*, *Analyst*, and *CEO_ten*) are regressed on treatment (*SEC_sanc*), the post-treatment dummy (*Post*), and their interaction ($SEC_sanc \times Post$). In Panels A and B, I use the non-matched ($N = 977$) and PSM ($N = 140$) samples, respectively. Both include the firm-years of enforced and non-enforced cases that fall within the five-year window before and after SEC sanctions. In Panel C, the sampling window is reduced to three years.

Panels A and B show consistently significant and negative coefficients on $SEC_sanc \times Post$ (ranging from -0.349 to -1.319 in Panel A, and from -0.238 to -2.090 in Panel B). This should indicate that, relative to non-enforced firms, enforced firms experienced a significant decrease in *Sales*, *Op_ROA*, *Analyst*, and *CEO_ten* subsequent to SEC sanctions. However, due to the pre-trend shown in Figure 1, it is not conclusive whether SEC sanctions caused the changes or whether the SEC chose such cases. Nevertheless, the results confirm that the two groups are systematically different in terms of the four outcome variables *around* (but not necessarily “subsequent to”) SEC sanctions.

In Panel C, the results remain largely unchanged even when the sampling window becomes narrower, except for *CEO_ten*. Although the coefficients on *CEO_ten* are not significant at a conventional level, they are significant at the 20% level (p -values are 0.166 and 0.193 in rows (1) and (2), respectively).

[Insert Table 9 here]

4.5 Short-term market reaction

Choi and Pritchard (2016) find greater negative market reactions to SCALs than to AAERs.

They thus argue that the greater loss of credibility in SCAL firms leads to the filing of private lawsuits even when the SEC does not sanction those cases.

However, while they measure cumulative abnormal returns (CARs) as of the first date of investigation for AAERs and the filing date for SCALs, only about 2% of firms disclose the first date of investigation to the public (Blackburne et al., 2020). Moreover, the measurement of short-term market reaction may be susceptible to the choice of CAR measurement timing. For example, the SEC investigations and class action filings may be triggered by existing misreporting disclosures made by alternative misreporting detection channels (e.g., restatements; Dechow et al., 1996).

Accordingly, I extend Choi and Pritchard's (2016) analysis by measuring CARs^{xiv} at alternative timings as of 1) the filing date of AAERs and SCALs, 2) the earliest date of AAER, SCAL, and restatement filing dates, and 3) the filing date of AAERs and the settlement date of SCALs. Here, I test cases (2) and (3) to explore any existing misreporting disclosure and the two groups' different chances of settlement at the time of case filings, respectively. Again, while SEC sanctions are virtually confirmed upon the filing date of AAERs, more than 35% of class actions are ultimately dismissed (Ramphal, 2007).

Table 10, the first row of columns (1) and (2), reports 545 CARs around the filing date of AAERs and SCALs that could be calculated using CRSP data. Alternatively, in the second row of the same columns, I use 615 CARs around the earliest date of AAER, SCAL, and restatement filings. In the third row of the same columns, 412 CARs are analyzed around the filing date of AAERs and the settlement date of SCALs. Finally, in all rows of columns (4) and (5), I use 383 CARs that are common to all the analyses in columns (1) and (2).

Consistent with Choi and Pritchard (2016), the first row of columns (1) and (2) show that, on the date of AAER and SCAL filings, the firms earned three-day CARs, with significant averages of -1.41% and -4.79%, respectively. The difference in CARs is statistically significant

(p -value = 0.011 in column (3)).

However, in the second and third rows, I observe opposing results when I measure CARs as of the earliest date of AAER, SCAL, and restatement filings, or as of the filing date of AAERs and the settlement date of SCALs. Specifically, the AAER and SCAL firms earned CARs, with significant averages of -11.18% (-1.41%) and -6.71% (0.30%) in the first (second) row, respectively. The difference in CARs is statistically significant (p -values = 0.002-0.020 in column (3)), and the results remain qualitatively the same even when I analyze the 383 common observations used in all three analyses (columns (4)-(6)).

These results indicate that the analysis of short-term market reactions to misreporting announcements is susceptible to the choice of the CAR measurement timing. Market reactions may be greater to AAERs than to SCALs if we explicitly consider existing misreporting disclosures made by alternative detection channels or the chance of an ultimate settlement of public and private lawsuit filings.

[Insert Table 10 here]

5. CONCLUSION

Using both enforcement outcomes and investigation targets, this paper compares the characteristics of AAERs with those of SCALs, and examines why the SEC does not sanction certain SCALs even after duly conducting investigations. The results consistently support the SEC's optimization of detection rates under resource constraints and strategic interaction between SEC enforcement and private litigation. Stronger evidence of misreporting in AAERs appears to help the SEC build successful cases using constrained resources, reflected in the low dismissal rate of AAERs. Further analysis indicates that the SEC's detection of more material misreporting is not critically undermined by resource constraints, although its low detection rate would have increased in the absence of such constraints. On the other hand, plaintiff

investors appear to go forum shopping to reach higher settlements from non-enforced cases.

The two-stage investigation process explains how the egregious characteristics of AAERs were formed in the SEC. Here, the Division of Corporate Finance makes enforcement referrals to the Division of Enforcement after screening investigation targets, based mainly on financial statement analysis. The Division of Enforcement then conducts in-depth investigations to acquire more direct evidence of fraud from firms and witnesses. This process seems to facilitate the SEC's detection of material misreporting cases without having to give all equal weight.

As with most research of this type, there are several caveats. In particular, the results should not be generalized to suggest that SEC enforcement is more effective than private litigation. As Leuz and Wysocki (2016) indicate, studies exploring government regulation's overall efficacy are usually more susceptible to endogeneity issues. The relatively egregious characteristics of AAERs do not necessarily translate to the SEC's efficacy in detecting or deterring misreporting.

With this in mind, this study provides valuable insights into the role of the two institutions. In particular, the results support the SEC's effective and efficient role as a federal agency regulating financial misreporting. They suggest that the cases missed by the SEC find their way to settlements in the private litigation regime. While this does not necessarily mean those cases should not have been settled in the courts (because the SEC may be relying on this supplement for less egregious cases), at least some plaintiff investors seem to be exploiting private litigation to extract monetary proceeds from less evident misreporting. I believe this study clarifies how the two institutions detect misreporting cases, which should be a basis for the ongoing debate over the U.S. enforcement system.

APPENDIX 1 VARIABLE DEFINITIONS

Variables	Definitions
Dependent variable	
AAERs	Indicator variable that equals 1 for misreporting firm-years against which the SEC filed cases, and 0 otherwise (Dechow et al., 2011).
SCALs	Indicator variable that equals 1 for misreporting firm-years against which plaintiff investors filed class action lawsuits but the SEC did not file cases, and 0 otherwise (http://securities.stanford.edu/).
Non-enforcement	Indicator variable that equals 1 for misreporting firm-years against which securities class action lawsuits were filed and settled, <i>but</i> the SEC did not file cases <i>even after</i> duly conducting investigations, and 0 otherwise (Blackburne et al., 2020).
Ex ante egregiousness	
MJONES	Three-year accumulation of residuals (ε) from $\Delta WC = \alpha_0 + \alpha_1 (1/\text{total assets } (at_{t-1})) + \alpha_2 (\Delta \text{sale } (sale) - \Delta \text{receivables } (rect)) + \alpha_3 (\Delta \text{PP\&E } (ppeg)) + \varepsilon$, estimated for each two-digit SIC year grouping. $WC = \text{current assets } (act) - \text{cash } (che) - \text{current liabilities } (lct) + \text{debt in current liabilities } (dlc) + \text{tax payable } (tap)$. All variables except $1/\text{total assets } (at_{t-1})$ are deflated by total assets (at_{t-1}).
PMJONES	Three-year accumulation of residuals (ε) from $\Delta WC = \alpha_0 + \alpha_1 (1/\text{total assets } (at_{t-1})) + \alpha_2 (\Delta \text{sale } (sale) - \Delta \text{receivables } (rect)) + \alpha_3 (\Delta \text{PP\&E } (ppeg)) + ROA + \varepsilon$, estimated for each two-digit SIC year grouping. All variables except $1/\text{total assets } (at_{t-1})$ are deflated by total assets (at_{t-1}).
FMJONES	Three-year accumulation of residuals (ε) from $\Delta WC = \alpha_0 + \alpha_1 [(1+k)\Delta \text{sale } (sale) - \Delta \text{receivables } (rect)] + \alpha_2 (\Delta \text{PP\&E } (ppeg)) + \alpha_3 (\Delta WC_{t-1}) + \alpha_4 (\Delta \text{sale } / \text{sale}_{t-1}) + \varepsilon$, estimated for each two-digit SIC year grouping. $\Delta \text{receivables } (rect) = \beta_0 + k\Delta \text{sales } (sale) + \gamma$. All variables except $\Delta \text{sale } / \text{sale}_{t-1}$ are deflated by Total assets (at_{t-1}).
WC	Three-year accumulation of change in working capital (ΔWC) deflated by total assets (at_{t-1}).
TA	Three-year accumulation of sum of ΔWC (working capital), ΔNCO (non-current operating assets), and ΔFIN (financial assets). $NOC = \text{total assets } (at) - \text{total current assets } (act) - \text{investment and advances } (ivao) - \text{total liabilities } (lt) + \text{total current liabilities } (lct) + \text{long-term debt } (dltt)$. $FIN = \text{short-term investments } (ivst) + \text{investment and advances } (ivao) - \text{long-term debt } (dltt) - \text{debt in current liabilities } (dlc) - \text{preferred stock } (pstk)$. All variables are deflated by total assets (at_{t-1}).
Financing needs (chg)	Sum of change in common stocks (ceq), preferred stocks ($pstk$), and total liabilities (lt), all divided by total assets (at).
Insider trading	Natural logarithm of $(1 + \text{stock sales } (shares) \times \text{transaction price } (tprice))$ (Thomson Reuters – Insiders Data).
Controls	
Rec (chg)	$\Delta \text{receivables } (rect) / \text{average total assets } (at)$.
Inv (chg)	$\Delta \text{inventory } (invt) / \text{average total assets } (at)$.

S_assets	(total assets (<i>at</i>) – net PP&E (<i>ppent</i>) – cash (<i>che</i>)) / total assets (<i>at</i>).
C_sales (chg)	Percentage change in (sales (<i>sale</i>) – Δreceivables (<i>rect</i>)).
ROA (chg)	Difference between income before extraordinary items (<i>ib</i>) / average total assets (<i>at</i>) and income before extraordinary items (<i>ib_{t-1}</i>) / average total assets (<i>at_{t-1}</i>).
F_age	Natural logarithm of the number of firm-years from the first Compustat year.
Assets	Natural logarithm of total assets (<i>at</i>).
Fortune	Indicator variable that equals 1 for firms on the Fortune 500 list, and 0 otherwise.
Lev	Long-term debt (<i>dltt</i>) / total assets (<i>at</i>).
Stock_mkt	Indicator variable that equals 1 for firms listed on major stock markets such as NYSE, AMEX, and NASDAQ, and 0 otherwise.
Ex post egregiousness and AAER-related variables	
E(Mis_amt)	Natural logarithm of the actual misreporting amount specified in AAERs or restatement amount for missing values, divided by the number of misreporting years (<i>Mis_amt</i>). For the remaining missing values, it is the fitted value of $Mis_amt = \alpha_0 + \alpha_1 MJONES + \alpha_2 Financing\ needs\ (chg) + \alpha_3 Insider\ trading + \alpha_4 Asset\ size + \alpha_5 Asset\ growth + \alpha_6 ROA + \Sigma Year\ fixed\ effect + \Sigma Firm\ fixed\ effect + \varepsilon$.
E(Mis_amt_alt)	Natural logarithm of the actual misreporting amount specified in AAERs or restatement amount for missing values (<i>Mis_amt_alt</i>). For the remaining missing values, it is the fitted value of $Mis_amt_alt = \alpha_0 + \alpha_1 MJONES + \alpha_2 Financing\ needs\ (chg) + \alpha_3 Insider\ trading + \alpha_4 Asset\ size + \alpha_5 Asset\ growth + \alpha_6 ROA + \Sigma Year\ fixed\ effect + \Sigma Firm\ fixed\ effect + \varepsilon$.
Mis_prd	Natural logarithm of the difference between the first and final misreporting dates. The misreporting dates are acquired from AAERs, and, for missing values, from Audit Analytics (restatements) and settled class actions in order.
Backlog_SEC	Natural logarithm of the annual average of open cases that a regional office was investigating during the investigation period (i.e., from the date a case was opened to the date it was closed) (Blackburne et al. 2020).
Invest_prd	Natural logarithm of the difference between the dates the SEC opened and closed an investigation (Blackburne et al., 2020).
Regional office	Dummy variable for each regional SEC office (Blackburne et al., 2020).
Distance	Natural logarithm of the distance between a firm's headquarters and the closest SEC offices in Washington, New York, Miami, Chicago, Denver, Los Angeles, Boston, Philadelphia, Atlanta, Fort Worth, Salt Lake City, and San Francisco. The latitude and longitude of the offices come from the U.S. Census Bureau Gazetteer. The distance is then calculated using the Haversine formula.
Political_cont	Five-year accumulation of Political Action Committee contributions by a firm, divided by average total assets (www.fec.gov).

SCAL-related variables

BA_sprd	Yearly median of the difference between the bid and ask prices divided by their midpoint price.
$E(\text{Settle_amt})$	Natural logarithm of the actual settlement amount from SCALs settled before the SEC closed its investigations, and the expected settlement amount for missing values, all divided by the number of shares (<i>Settlement_per_share</i>). The expected settlement amount is the fitted value of $\text{Settlement_per_share} = \alpha_0 + \alpha_1 \text{MJONES} + \alpha_2 \text{Financing needs (chg)} + \alpha_3 \text{Insider trading} + \alpha_4 \text{Asset size} + \alpha_5 \text{Asset growth} + \alpha_6 \text{ROA} + \Sigma \text{Year fixed effect} + \Sigma \text{Firm fixed effect} + \varepsilon$.
Backlog_SCALs	Natural logarithm of the annual average of open SCALs that a judicial circuit was reviewing during the proceeding (i.e., from the date a case was filed to the date it was settled).
Backlog_lawsuits	Natural logarithm of the annual average of open lawsuits, including all federal, civil, criminal, bankruptcy, and appellate court cases, that a judicial circuit was reviewing during the proceeding (Federal Judicial Center).
Backlog_lawsuits_less Rev_prd	Natural logarithm of the annual average of open lawsuits less SCALs. Natural logarithm of the difference between the dates a class action lawsuit was filed and settled.
Judicial circuit	Dummy variable for each judicial circuit.
Early_file	Indicator variable that equals 1 for firm-years against which SCALs were filed before the SEC initiated investigations, and 0 otherwise.

Outcome variables

Sales	Natural logarithm of sales (<i>sale</i>).
Op_ROA	Operating income (<i>oibdp</i>) / average total assets (<i>at</i>).
Analyst	Natural logarithm of the number of analysts following a firm (<i>numest</i>).
CEO_ten	Natural logarithm of CEO tenure.

Market reaction

CAR	Three-day market-adjusted returns for each firm over the interval extending from one trading day before through one trading day after the event date. CAR is estimated with a window of 100 days. Abnormal returns are defined in excess of predicted normal returns from the actual returns for each day in the three-day event window.
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APPENDIX 2 SAMPLE SELECTION FOR DiD ESTIMATION

Panel A: Non-matched ($N = 977$)			
	Enforced	Non-enforced	Total
Firms in the non-enforcement sample (1995-2011)	43	109	152
<i>less</i> firms that existed only either before or after the treatment	(5)	(15)	(20)
<i>less</i> firms outside the five-year window	(1)	(4)	(5)
Non-matched misreporting firms as of the filing date	37	90	132
(All matched firm-years)	316	661	977
Panel B: Propensity score matching (PSM; $N = 134$)			
	Enforced	Non-enforced	Total
Non-matched misreporting firms as of the filing date	37	90	132
<i>less</i> firms without matched pairs	(29)	(82)	(111)
Matched misreporting firms as of the filing date	8	8	16
(All matched firm-years)	66	74	140
Panel C: Covariate balance (PSM)			
Variables	Enforced	Non-enforced	<i>Diff.</i> (mean)
$E(\text{Mis_amt})$	9.116	10.34	-1.226
Mis_prd	6.398	5.547	0.851
Backlog_SEC	1.578	1.455	0.123
Invest_prd	6.303	7.334	-1.031
$E(\text{Settle_amt})$	0.238	0.629	-0.391
Backlog_court	4.777	4.661	0.116
Assets	20.39	19.56	0.826
Propensity score	0.542	0.531	0.011

This table reports the selection process of the non-matched and propensity score-matched (PSM) samples used in the difference-in-differences (DiD) analysis. The base sample to construct the non-matched (Panel A) and PSM (Panel B) samples are the non-enforcement sample, of which the selection process is reported in Table 5. The non-enforcement sample includes only misreporting firm-years, while the non-matched and PSM samples contain both misreporting and non-misreporting firm-years to capture any dynamic change in firm characteristics around SEC sanctions. Sanctioned firms are the misreporting firms against which the SEC filed AAERs. Non-enforced firms are the misreporting firms against which plaintiff investors filed class actions but the SEC did not file cases. The sample period is 1995-2011. See Appendix 1 for variable definitions. In Panel B, matched pairs are identified based on the ex ante probability of SEC non-enforcements, estimated using Eq. (4). To run Eq. (4), I use the average of each variable for missing values because the misreporting amount, for example, is available only for misreporting years. Panel C reports the covariate balance of the matched pairs.

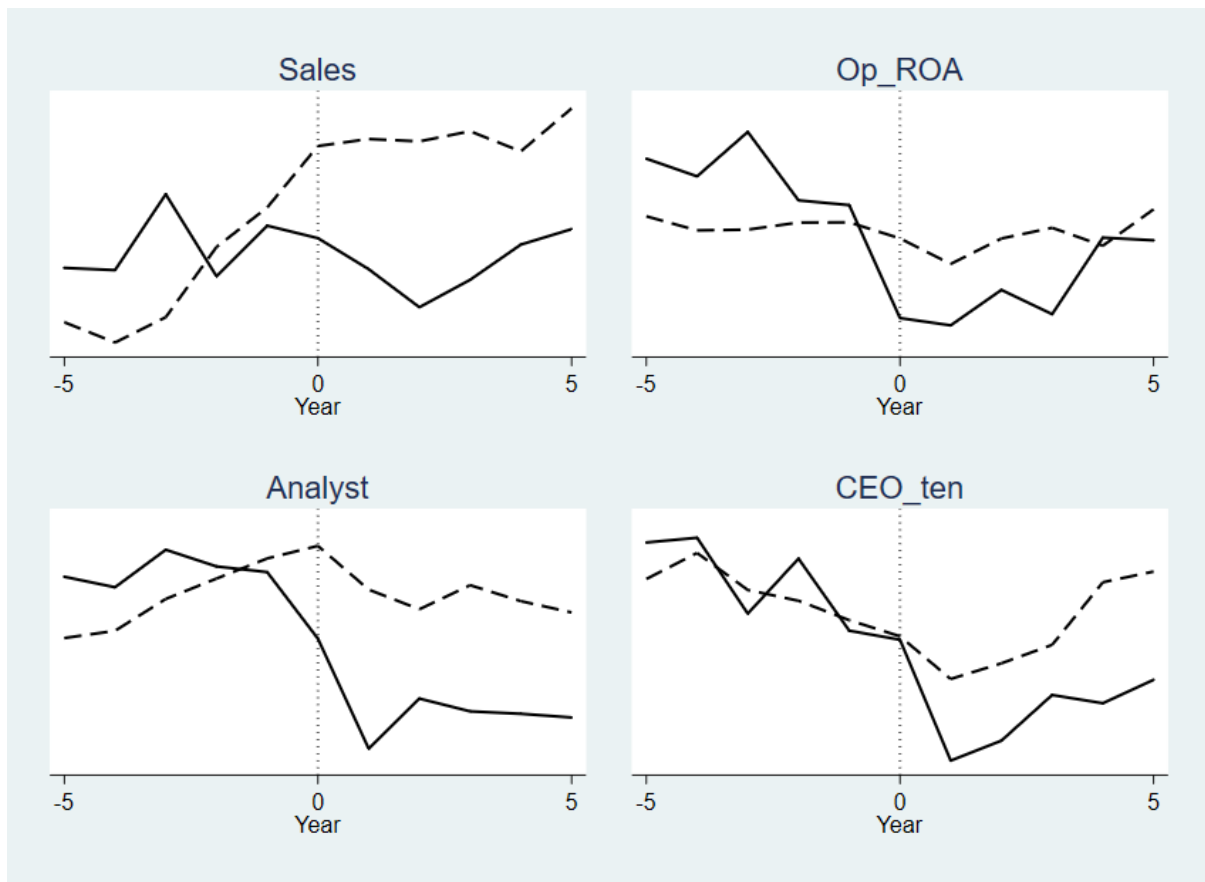
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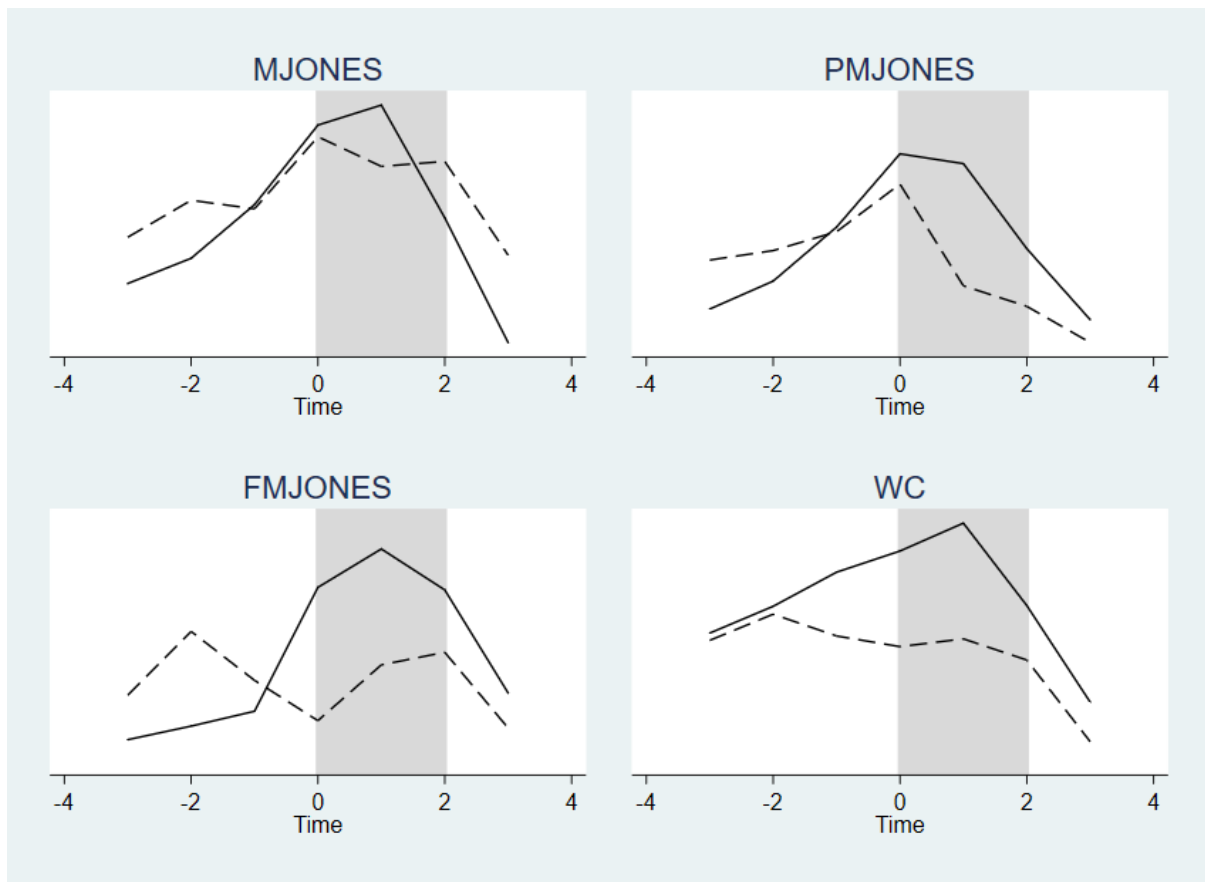
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FIGURE 1 TREND OF OUTCOME VARIABLES



This figure illustrates the trend of sales (*Sales*), operating ROA (*Op_ROA*), analyst following (*Analyst*), and CEO tenure (*CEO_ten*). The solid (enforced firms) and dashed (non-enforced firms) lines represent the four variables (y-axis) over ten years relative to the filing year of AAERs or SCALs (x-axis; *Year* = 0). Sanctioned firms are the misreporting firms against which the SEC filed AAERs. Non-enforced firms are the misreporting firms against which plaintiff investors filed class actions but the SEC did not file cases. Here, I use the non-matched sample of 977 firm-years for the 1995-2011 period. See Appendix 1 for variable definitions.

FIGURE 2 TREND OF ACCRUALS



This figure illustrates the trend of accruals measured by the modified Jones accruals (*MJONES*), modified Jones accruals with current-year ROA (*PMJONES*), forward-looking modified Jones accruals (*FMJONES*), and working capital accruals (*WC*). The solid (*AAERs*) and dashed (*SCALs*) lines represent four different types of accruals (y-axis) over the years relative to the first misreporting year (x-axis; *Time* = 0). Here, I use the pooled sample of 92,785 firm-years for the 1995-2012 period. See Appendix 1 for variable definitions.

TABLE 1 SAMPLE SELECTION

Panel A: Pooled sample			
	AAERs	SCALs	Compustat
AAERs compiled by Dechow et al. (2011)	1,961		
Settled SCALs		4,540	
Compustat			242,834
<i>less</i> obs. without identifier	467	2,473	
<i>less</i> obs. in financial industries	246	353	62,209
<i>less</i> duplications with AAERs		218	
<i>less</i> obs. with missing values	451	658	87,840
Misreporting firm-years	797	838	
Total firm-years (1995-2012)	3,820	5,637	92,785
Panel B: Non-enforcement sample			
FOIA cases with <i>permno</i> (closed between 2000-2017)		3,948 cases	
<i>less</i> cases in financial industries		(-557)	
<i>less</i> cases not settled in the courts		(-3,030)	
Investigated and settled firms		361 cases	
Investigated and settled firm-years		850 firm-years	
<i>less</i> firm-years outside of sample period (1995-2012)		(-57)	
<i>less</i> firm-years with missing values		(-439)	
Non-enforcement sample (1995-2011)		354 firm-years	

This table reports the selection process for the pooled sample, which includes 92,785 firm-years for 1995-2012. In Panel A, *AAERs* comprise 797 misreporting firm-years, or 3,820 firm-years. (Unsanctioned) *SCALs* consist of 838 misreporting firm-years, or 5,637 firm-years. In Panel B, the non-enforcement sample is the merged file of the pooled sample and the SEC's Freedom of Information Act (FOIA) dataset, which I acquired from Blackburne et al. (2020). The non-enforcement sample includes 354 misreporting firm-years for the 1995-2011 period. This sample includes misreporting firm-years that the SEC investigated, and related class action lawsuits could be reasonably assumed to have been settled in the courts.

TABLE 2 DESCRIPTIVE STATISTICS: POOLED SAMPLE

Panel A: Covariate balance							
Variables	AAERs	SCALs	Compustat		(1) – (2)	(1) – (3)	Diff.
	Mean	Mean	Mean	N			(2) – (3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MJONES	0.038	0.053	0.037	92,785	-0.015	0.001	0.016**
PMJONES	-0.058	-0.064	-0.042	92,331	0.007	-0.016*	-0.022***
FMJONES	0.024	0.021	0.011	81,892	0.003	0.013**	0.010**
WC	0.027	0.026	0.006	92,785	0.001	0.022***	0.020***
TA	0.110	0.160	0.073	83,785	-0.050***	0.039***	0.087***
Financing needs (chg)	-0.005	0.013	-0.020	92,785	-0.019**	0.015**	0.033***
Insider trading	2.982	2.358	1.987	47,670	0.624***	0.995***	0.371***
Rec (chg)	0.013	0.013	0.010	92,785	0.000	0.003**	0.003***
Inv (chg)	0.008	0.008	0.006	92,785	-0.001	0.002**	0.002***
S_assets	0.617	0.508	0.518	92,785	0.109***	0.099***	-0.010
C_sales (chg)	0.180	0.212	0.171	92,785	-0.032*	0.009	0.041***
ROA (chg)	-0.007	-0.001	-0.006	92,785	-0.006	-0.001	0.005
Lev	0.184	0.149	0.186	92,785	0.034***	-0.002	-0.037***
Assets	19.817	19.890	18.967	92,785	-0.073	0.850***	0.923***
Stock_mkt	0.717	0.913	0.583	92,785	-0.195***	0.134***	0.330***
Fortune	0.177	0.124	0.059	92,785	0.053***	0.118***	0.065***
F_age	2.804	2.667	2.679	92,785	0.137***	0.125***	-0.012
Max. observations	3,820	5,637	92,785				
Misreporting years	797	838	1,635				

Panel B: Summary statistics							
Variable	N	Mean	Std. Dev.	Q1	Median	Q3	
AAERs	92,785	0.009	0.092	0.000	0.000	0.000	
SCALs	92,785	0.009	0.095	0.000	0.000	0.000	
MJONES	92,785	0.037	0.561	-0.117	0.000	0.113	
Financing needs (chg)	92,785	-0.020	0.441	-0.073	0.036	0.150	
Insider trading	47,670	1.987	4.038	0.000	0.000	0.000	

Panel C: Spearman correlation matrix (N = 92,785)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) AAERs	1.000							
(2) SCALs	-0.009	1.000						
(3) MJONES	0.008	0.010	1.000					
(4) Financing needs (chg)	0.016	0.013	0.005	1.000				
(5) Insider trading	0.016	0.010	-0.012	0.049	1.000			
(6) ROA (chg)	0.002	0.005	0.017	0.038	0.005	1.000		
(7) Lev	-0.007	-0.010	0.004	0.003	-0.002	-0.024	1.000	
(8) Assets	0.044	0.046	-0.037	0.144	0.362	0.049	0.178	1.000

This table reports descriptive statistics for the pooled sample: *AAERs*, *SCALs*, and *Compustat* for the 1995-2012 period (columns (1)-(3), respectively). Panel A reports the mean differences of variables and *p*-values for the *t*-tests of the differences (columns (5)-(7)). *p*-values are denoted by asterisks, where *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively. Panel B reports the summary statistics for selected variables (e.g., mean, median, and standard deviation). Panel C examines Spearman correlations for key variables. Correlations with a *p*-value of less than 10% are in bold. All continuous variables are winsorized at the 1% and 99% levels. See Appendix 1 for variable definitions.

TABLE 3 EX ANTE EGREGIOUSNESS: INDIRECT COMPARISON

Control group:	<i>DV</i> =									
	AAERs					SCALs				
	Enforced or non-enforced (<i>N</i> = 79,624)					Settled or unsettled (<i>N</i> = 84,918)				
	Compustat			Large firms	Compustat			Large firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ex ante egregiousness										
MJONES	0.118*** (0.020)	0.145*** (0.030)	0.143*** (0.030)	0.094** (0.043)	0.150*** (0.057)	0.034 (0.022)	0.051 (0.034)	0.051 (0.034)	0.043 (0.036)	0.138*** (0.041)
Financing needs (chg)			0.174*** (0.058)	0.290*** (0.075)	0.232*** (0.083)			0.053 (0.054)	0.051 (0.067)	-0.149** (0.073)
Insider trading				0.009** (0.004)	0.005 (0.005)				-0.007 (0.004)	-0.013*** (0.005)
Controls										
F_age		-0.132*** (0.024)	-0.132*** (0.024)	-0.200*** (0.029)	-0.171*** (0.032)		-0.129*** (0.024)	-0.129*** (0.024)	-0.107*** (0.028)	-0.103*** (0.030)
Fortune		0.194*** (0.051)	0.198*** (0.052)	0.055 (0.057)	0.368*** (0.062)		0.179*** (0.053)	0.180*** (0.053)	0.117** (0.057)	0.810*** (0.069)
Assets		0.137*** (0.008)	0.136*** (0.008)	0.166*** (0.013)	-0.157*** (0.021)		0.091*** (0.009)	0.091*** (0.009)	0.093*** (0.012)	-0.399*** (0.029)
Rec (chg)		1.130*** (0.187)	1.050*** (0.191)	0.954*** (0.249)	1.397*** (0.345)		1.169*** (0.241)	1.140*** (0.242)	1.407*** (0.282)	1.546*** (0.401)
Inv (chg)		0.643** (0.303)	0.588* (0.306)	0.518 (0.406)	0.420 (0.504)		0.852** (0.341)	0.843** (0.342)	0.816* (0.430)	0.813 (0.524)
S_assets		0.477*** (0.077)	0.497*** (0.077)	0.610*** (0.094)	0.629*** (0.112)		-0.080 (0.075)	-0.078 (0.076)	-0.072 (0.083)	-0.112 (0.102)
C_sales (chg)		0.026** (0.012)	0.029** (0.013)	0.040*** (0.015)	0.065** (0.027)		0.032*** (0.012)	0.032*** (0.012)	0.038** (0.015)	0.088*** (0.024)
ROA (chg)		-0.026 (0.059)	-0.043 (0.061)	-0.066 (0.102)	-0.170 (0.155)		0.007 (0.096)	0.001 (0.096)	0.034 (0.103)	0.144 (0.167)
Lev		-0.270*** (0.072)	-0.266*** (0.072)	-0.310*** (0.102)	-0.697*** (0.120)		-0.113 (0.087)	-0.111 (0.087)	-0.150 (0.102)	-0.716*** (0.115)
Stock_mkt		0.064* (0.034)	0.045 (0.034)	-0.205*** (0.047)	-0.236*** (0.056)		0.510*** (0.041)	0.504*** (0.041)	0.300*** (0.062)	0.406*** (0.073)
Year dummy	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Industry dummy	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included

Constant	-2.574*** (0.125)	-4.935*** (0.203)	-4.902*** (0.204)	-5.345*** (0.297)	1.899*** (0.477)	-3.247*** (0.188)	-4.623*** (0.260)	-4.607*** (0.261)	-4.435*** (0.320)	6.061*** (0.661)
Observations	79,264	79,264	79,264	39,619	19,408	84,918	84,918	84,918	42,623	20,034
Pseudo R ²	0.080	0.146	0.148	0.153	0.212	0.033	0.102	0.102	0.061	0.186

This table reports probit estimation results of Eq. (1), where misreporting (i.e., *AAERs* in columns (1)-(5) and *SCALs* in columns (6)-(10)) is regressed on three ex ante egregiousness measures (*MJONES*, *Financing needs (chg)*, and *Insider trading*) and controls, using the *AAER* (columns (1)-(5)) and *SCAL* (columns (6)-(10)) samples. The *AAER sample* is the pooled sample excluding *SCAL* firms, and comprises 87,148 observations for the 1995-2012 period. The *SCAL sample* is the pooled sample excluding *AAER* firms, and consists of 88,965 observations for the same sample period. However, I mainly analyze 79,264 observations of the *AAER sample* and 84,918 observations of the *SCAL sample* due to the loss of observations through maximum likelihood estimation. In columns (4) and (9), I analyze 39,619 observations of the *AAER sample* and 42,623 observations of the *SCAL sample* due to the additional loss of observations from merging with Thomson Reuters and CRSP. In columns (5) and (10), I analyze 19,408 observations of the *AAER sample* and 20,034 observations of the *SCAL sample* by adopting large firms whose total assets exceed \$500 million as a control group. See Appendix 1 for variable definitions. Two values are reported for each variable: the coefficient estimate, and robust standard errors (in parentheses). *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

TABLE 4 EX ANTE EGREGIOUSNESS: DIRECT COMPARISON

Panel A: Direct comparison											
DV:	AAERs (vs. SCALs)										
Sample:	Enforced or settled (<i>N</i> = 9,457)										
Model:	(1)	(2)	(3)	(4)	(5)	Probit (6)	LPM (7)	(8)	(9)	Simple hazard (10)	(11)
Ex ante egregiousness											
MJONES	0.230*** (0.041)			0.167*** (0.046)	0.165*** (0.047)	0.133** (0.062)	0.024*** (0.007)	0.173*** (0.066)			0.235** (0.098)
Financing needs (chg)		0.237*** (0.061)			0.284*** (0.064)	0.393*** (0.081)	0.052*** (0.011)		0.452*** (0.134)		0.470*** (0.152)
Insider trading			0.084*** (0.021)			0.016*** (0.006)	0.002** (0.001)			0.004 (0.011)	0.001 (0.011)
Other controls				Included	Included	Included	Included	Included	Included	Included	Included
Year dummy	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Industry dummy	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Duration dummy								Included	Included	Included	Included
Constant	-1.259*** (0.189)	-1.201*** (0.188)	-1.122*** (0.245)	-3.664*** (0.312)	-3.639*** (0.317)	-3.604*** (0.456)	-0.140** (0.055)	-2.443*** (0.659)	-2.345*** (0.670)	-2.093** (0.855)	-2.445*** (0.857)
Observations	9,138	9,138	7,212	9,138	9,138	7,212	7,454	5,310	5,310	4,337	4,337
Pseudo R ²	0.118	0.117	0.140	0.171	0.176	0.200		0.165	0.171	0.186	0.197
Adjusted R ²							0.111				

Panel B: Alternative accruals measures				
<i>DV:</i>	AAERs (vs. SCALs)			
Model:	Probit			
Accruals:	PMJONES	FMJONES	WC	TA
	(1)	(2)	(3)	(4)
Accruals	0.131** (0.067)	0.299*** (0.100)	0.505*** (0.140)	0.171*** (0.055)
Financing needs (chg)	0.405*** (0.082)	0.373*** (0.093)	0.375*** (0.082)	0.424*** (0.087)
Insider trading	0.016*** (0.006)	0.016*** (0.006)	0.015*** (0.006)	0.015** (0.006)
Other controls	Included	Included	Included	Included
Year dummy	Included	Included	Included	Included
Industry dummy	Included	Included	Included	Included
Constant	-3.593*** (0.459)	-3.790*** (0.481)	-3.672*** (0.460)	-3.166*** (0.487)
Observations	7,196	6,538	7,212	6,340
Pseudo R ²	0.201	0.209	0.201	0.204

This table reports the probit, LPM, and simple hazard estimation results of Eq. (1), where *AAERs* is regressed on three ex ante egregiousness measures (*accruals*, *Financing needs (chg)*, and *Insider trading*) and controls, using the *combined sample*. All other control variables are the same as those used in column (4) in Table 3. The *combined sample* comprises 9,457 observations for the 1995-2012 period. However, in several model specifications, I mainly analyze 9,138 observations due to the loss of observations through maximum likelihood estimation. Furthermore, in columns (3), (6), (7), (10), and (11) of Panel A, and in Panel B, I analyze even fewer observations due to the additional loss when merging with Thomson Reuters and CRSP. Consistent with Shumway (2001), in columns (8)-(11) of Panel A, I add the duration dummy to Eq. (1). The duration dummy represents the number of years spent without misreporting. Duration is calculated from the year firms were listed on Compustat if they did not misreport during the sample period. For firms with prior misreporting experience before the sample period, I calculate duration from the actual misreporting year. Except for some exceptional cases that may have had misreporting before 1971, the duration dummy represents exactly the years during which a firm did not misreport before committing a new misreporting. In Panel B, I replace *MJONES* with four alternative accruals measures. See Appendix 1 for variable definitions. Two values are reported for each variable: the coefficient estimate, and robust standard errors (in parentheses). *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

TABLE 5 DESCRIPTIVE STATISTICS: NON-ENFORCEMENT SAMPLE

Panel A: Covariate balance						
Variables	Enforced (1)	Non-enforced (2)	Diff. (3)			
Non-enforcement	0.000	1.000	-1.000***			
<i>E</i> (Mis_amt)	7.917	4.595	3.322***			
<i>E</i> (Mis_amt_alt)	11.029	8.442	2.587***			
Mis_prd	5.154	4.149	1.005***			
Backlog_SEC	5.674	5.849	-0.175**			
Invest_prd	4.883	3.055	1.827***			
BA_sprd	-0.010	-0.005	-0.007***			
<i>E</i> (Settle_amt)	0.160	0.261	-0.101***			
Backlog_SCALs	4.395	4.845	-0.450***			
Backlog_lawsuits	11.734	12.295	-0.560***			
Distance	7.728	7.507	0.222			
Political_cont	0.043	0.042	0.001			
MJONES	0.184	0.032	0.152			
Financing needs (chg)	0.043	0.087	-0.043			
Insider trading	1.392	2.153	-0.761*			
Early_file	0.563	0.575	-0.013			
Observations	128 (36.2%)	226 (63.8%)				

Panel B: Summary statistics						
Variable	<i>N</i>	Mean	Std. Dev.	Q1	Median	Q3
Non-enforcement	354	0.638	0.481	0.000	1.000	1.000
<i>E</i> (Mis_amt)	354	5.796	6.825	0.000	1.060	13.536
Mis_prd	354	4.513	2.425	1.792	5.549	6.596
Backlog_SEC	354	5.784	0.655	5.302	5.587	6.405
Invest_prd	354	3.716	2.757	1.633	2.959	5.447
BA_sprd	354	-0.007	0.010	-0.009	-0.004	-0.001
<i>E</i> (Settle_amt)	354	0.225	0.334	0.003	0.110	0.269
Backlog_SCALs	354	4.682	0.922	3.850	5.115	5.291
Backlog_lawsuits	354	12.092	1.187	11.269	12.219	12.909

Panel C: Spearman correlation matrix (<i>N</i> = 354)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) Non-enforcement	1.000								
(2) <i>E</i> (Mis_amt)	-0.234	1.000							
(3) Mis_prd	-0.199	-0.165	1.000						
(4) Backlog_SEC	0.127	0.088	-0.057	1.000					
(5) Invest_prd	-0.319	0.131	0.216	0.108	1.000				
(6) <i>E</i> (Settle_amt)	0.145	0.116	0.105	-0.081	0.109	1.000			
(7) Rev_prd	0.073	-0.157	0.094	0.094	-0.159	0.105	1.000		
(8) Backlog_lawsuits	0.227	0.117	-0.147	0.159	-0.065	0.194	0.050	1.000	
(9) MJONES	-0.139	0.062	-0.056	0.013	0.148	0.016	0.001	0.180	1.000

This table reports descriptive statistics for the *non-enforcement sample* ($N = 354$) for the 1995-2011 period. Sanctioned firms are misreporting firms against which the SEC filed AAERs. Non-enforced firms are misreporting firms against which plaintiff investors filed class actions but the SEC did not file cases. The sample period is 1995-2011. Panel A reports the mean differences of variables and p -values for the t -tests of the differences (column (3)). p -values are denoted by asterisks, where *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively. Panel B reports the summary statistics for selected variables (e.g., mean, median, and standard deviation). Panel C examines Spearman correlations for key variables. Correlations with a p -value of less than 10% are in bold. All continuous variables are winsorized at the 1% and 99% levels. See Appendix 1 for variable definitions.

TABLE 6 EX POST EGREGIOUSNESS

DV =	Non-enforcement Investigated and settled (N = 354)										Explained variation (R ² ; %)
Sample:											LPM
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
SEC Investigation											
<i>E</i> (Mis_amt)	-0.019*** (0.005)	-0.019*** (0.005)					-0.021*** (0.004)	-0.021*** (0.004)	-0.017*** (0.004)		5.48
<i>E</i> (Mis_amt_alt)										-0.006* (0.003)	2.44
Mis_prd	-0.041*** (0.009)	-0.035*** (0.010)					-0.036*** (0.011)	-0.035*** (0.010)	-0.035*** (0.013)	-0.025** (0.012)	3.98
Backlog_SEC		0.150*** (0.045)					0.138*** (0.046)	0.122** (0.050)	0.402*** (0.087)	0.400*** (0.078)	1.61
Invest_prd		-0.017 (0.011)					-0.030** (0.012)	-0.022 (0.014)	-0.039** (0.015)	-0.043*** (0.016)	10.17
Court Discovery											
BA_sprd			7.964** (3.631)	-5.421 (4.467)	-5.009 (4.473)	-4.494 (4.201)	-6.268 (3.940)	-6.156 (3.840)	-5.223 (4.362)	-4.954 (4.728)	7.23
<i>E</i> (Settle_amt)					0.027 (0.139)	0.057 (0.141)	0.147 (0.122)	0.158 (0.118)	0.273*** (0.083)	0.220*** (0.081)	2.11
Rev_prd					0.055 (0.068)	0.068 (0.065)	0.019 (0.080)	0.032 (0.078)	-0.013 (0.067)	0.006 (0.064)	0.53
Backlog_lawsuits					0.027 (0.036)		0.070 (0.042)				5.15
Backlog_SCALs						0.111*** (0.037)		0.069* (0.039)	0.196** (0.086)	0.195** (0.080)	5.51
Backlog_lawsuits_less						0.001 (0.038)		0.046 (0.042)	0.024 (0.056)	0.017 (0.059)	5.14
SEC enforcement biases											
Distance							-0.007 (0.012)	-0.005 (0.012)	0.001 (0.011)	0.001 (0.014)	0.18
Political_cont							-0.434 (0.340)	-0.351 (0.347)	-0.348 (0.309)	-0.524* (0.294)	0.00

Ex ante egregiousness											
MJONES	-0.077	-0.059	-0.069**	-0.094*	-0.098*	-0.101**	-0.081	-0.085*	-0.107**	-0.121***	1.93
	(0.054)	(0.054)	(0.032)	(0.054)	(0.052)	(0.049)	(0.049)	(0.047)	(0.042)	(0.039)	
Financing needs (chg)	-0.011	-0.010	-0.026	-0.018	-0.020	-0.022	0.019	0.021	-0.097	-0.113	0.39
	(0.082)	(0.080)	(0.086)	(0.093)	(0.094)	(0.092)	(0.084)	(0.085)	(0.072)	(0.078)	
Insider trading	-0.001	-0.002	-0.000	0.004	0.002	0.003	-0.006	-0.005	-0.002	-0.003	0.82
	(0.006)	(0.005)	(0.006)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.005)	
Other controls	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included	
Regional office dummy									Included	Included	11.08
Judicial circuit dummy									Included	Included	9.67
Year dummy	Included	Included		Included	Included	Included	Included	Included	Included	Included	13.68
Industry dummy	Included	Included		Included	Included	Included	Included	Included	Included	Included	19.42
Constant	0.757	0.266	0.217	0.201	-0.347	-0.421	-0.869	-0.863	-1.210	-1.484	
	(0.520)	(0.526)	(0.433)	(0.604)	(0.833)	(0.765)	(0.905)	(0.865)	(1.172)	(1.083)	
Observations	354	354	354	354	354	354	354	354	354	354	
Adjusted R ²	0.383	0.415	0.209	0.298	0.297	0.329	0.436	0.445	0.571	0.539	

This table reports the LPM estimation results of Eq. (2), where *Non-enforcement* is regressed on two ex post egregiousness measures (*E(Mis_amt)* and *Mis_prd*) and controls, using the *non-enforcement sample*. *Non-enforcement* is an indicator variable that equals 1 for misreporting firms against which securities class action lawsuits were filed and settled, but the SEC did not file cases even after duly conducted investigations, and 0 otherwise. All other control variables are the same as those used in column (6) of Panel A in Table 4. In column (11), I report the R^2 s of the regression models, where *Non-enforcement* is regressed on each variable in Eq. (2) without any control. See Appendix 1 for variable definitions. Two values are reported for each variable: the coefficient estimate, and standard errors clustered by regional offices and judicial circuits (in parentheses). *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

TABLE 7 MECHANISM OF ENFORCEMENT

Subsample:	Backlog_SEC		Non-enforcement Investigated and settled (N = 354)				MJONES	
	High	Low	Backlog_lawsuits		E(Settle_amt)		High	Low
	(1)	(2)	High	Low	High	Low	(7)	(8)
SEC Investigation								
E(Mis_amt)	-0.025*** (0.008)	-0.005 (0.004)	-0.021*** (0.008)	-0.027*** (0.008)	-0.021*** (0.006)	-0.013 (0.009)	-0.015*** (0.005)	-0.021*** (0.004)
Backlog_SEC			0.935*** (0.219)	0.253 (0.200)	0.050 (0.266)	0.638** (0.279)	0.509*** (0.114)	0.318 (0.278)
Mis_prd	-0.058*** (0.011)	-0.046*** (0.010)	-0.062*** (0.014)	-0.027** (0.013)	-0.024 (0.016)	-0.044** (0.018)	-0.050** (0.024)	-0.033** (0.013)
Invest_prd	-0.051* (0.025)	-0.038** (0.014)	-0.010 (0.024)	-0.025 (0.019)	-0.025 (0.021)	-0.041 (0.031)	-0.045*** (0.013)	-0.044** (0.021)
Court Discovery								
BA_sprd	1.732 (3.876)	-11.827 (8.587)	-40.172** (15.807)	-0.921 (5.874)	-6.177 (4.477)	-10.823 (8.786)	-16.160 (11.419)	-12.620** (5.022)
E(Settle_amt)	0.063 (0.219)	0.351*** (0.110)	0.436*** (0.082)	0.314** (0.136)			0.128 (0.144)	0.198** (0.079)
Backlog_lawsuits	0.088 (0.056)	0.042 (0.077)			0.054 (0.079)	0.081 (0.065)	0.208*** (0.069)	0.026 (0.093)
Rev_prd	-0.280** (0.105)	0.193** (0.079)	-0.085 (0.072)	0.162 (0.105)	0.118 (0.097)	-0.107 (0.100)	-0.281*** (0.093)	0.133 (0.121)
Ex ante egregiousness								
MJONES	-0.098 (0.084)	-0.202*** (0.045)	-0.125 (0.139)	-0.084 (0.095)	-0.090 (0.103)	-0.257*** (0.074)		
All other controls and fixed effects								
Constant	3.582*** (1.080)	-1.754 (1.097)	-5.631*** (1.760)	0.214 (1.378)	-0.750 (1.636)	0.034 (2.064)	-0.039 (1.254)	-4.386*** (1.439)
Chi² statistic for Diff. (Prob > chi²):								
E(Mis_amt)	8.30 (0.004)		0.41 (0.521)		1.75 (0.186)		1.68 (0.195)	
Backlog_SEC			5.57 (0.018)		9.07 (0.003)		0.70 (0.403)	
Mis_prd	1.06 (0.304)		5.14 (0.023)		1.34 (0.247)		0.65 (0.420)	
Invest_prd	0.38 (0.536)		0.73 (0.392)		0.12 (0.730)		0.01 (0.928)	

BA_sprd	3.47 (0.062)		14.23 (0.000)		0.70 (0.403)		0.16 (0.685)	
E(Settle_amt)	2.84 (0.092)		0.87 (0.351)				0.33 (0.566)	
Backlog_court	0.37 (0.544)				0.24 (0.623)		7.69 (0.006)	
Rev_prd	23.90 (0.000)		4.73 (0.030)		5.24 (0.022)		16.31 (0.000)	
MJONES	1.85 (0.173)		0.21 (0.644)		3.84 (0.050)			
Observations	169	185	171	183	177	177	177	177
Adjusted R ²	0.644	0.645	0.692	0.665	0.370	0.683	0.680	0.572

This table reports the LPM estimation results of Eq. (2), where *Non-enforcement* is regressed on two ex post egregiousness measures (*E(Mis_amt)* and *Mis_prd*) and controls, using subsamples of the *non-enforcement sample*. All other control variables and fixed effects are the same as those used in column (7) in Table 6. Subsamples are constructed by dividing the non-enforcement model at the median values of *Backlog_SEC* (columns (1) and (2)), *Backlog_lawsuits* (columns (3) and (4)), *E(Settle_amt)* (columns (5) and (6)), and *MJONES* (columns (7) and (8)). Two values are reported for each variable: the coefficient estimate, and standard errors clustered by regional offices and judicial circuits (in parentheses). Wald test results, *Chi*² statistics, and *p*-values (in parentheses) for the difference in the parameters in each pair of models are reported at the bottom of the table. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

TABLE 8 TIMING AND JUDICIAL CIRCUIT OF CLASS ACTIONS

<i>DV</i> =	Non-enforcement			
	Investigated and settled (<i>N</i> = 354)			
Sample:	Early_file = Yes	Early_file = No	Circuit 9	Circuit 2
Subsample:	(1)	(2)	(3)	(4)
SEC Investigation				
<i>E</i> (Mis_amt)	-0.019*** (0.005)	-0.035*** (0.012)	-0.034*** (0.004)	0.038 (0.037)
Mis_prd	-0.033*** (0.012)	-0.047** (0.020)	-0.058*** (0.014)	0.023 (0.051)
Backlog_SEC	0.681*** (0.201)	0.436* (0.221)	0.628** (0.210)	-4.877 (39.729)
Invest_prd	-0.016 (0.019)	-0.008 (0.035)	-0.025 (0.050)	-0.207 (2.433)
Court discovery				
BA_sprd	-5.500 (4.010)	-18.310* (10.721)	-3.318* (1.558)	14.086 (15.965)
<i>E</i> (Settle_amt)	0.328*** (0.098)	0.491*** (0.116)	0.527** (0.133)	-9.409 (83.377)
Backlog_SCALs	0.182* (0.102)	0.291* (0.163)	-1.039 (1.123)	0.279*** (0.057)
Backlog_lawsuits_less	-0.059 (0.060)	0.049 (0.148)	0.068 (0.041)	-0.006 (7.543)
Rev_prd	0.030 (0.051)	-0.158 (0.272)	-0.206* (0.087)	-0.235 (2.379)
All other controls and fixed effects	Included	Included	Included	Included
Constant	-4.450*** (1.292)	-3.108 (2.412)	3.786 (5.291)	34.301 (127.478)
Chi ² statistic for <i>Diff.</i> (<i>Prob</i> > chi ²):				
<i>E</i> (Mis_amt)	2.79 (0.095)		51.19 (0.000)	
Mis_prd	0.59 (0.441)		23.94 (0.000)	
Backlog_SEC	0.87 (0.350)		0.26 (0.610)	
Invest_prd	0.06 (0.800)		0.10 (0.753)	
BA_sprd	3.09 (0.079)		16.19 (0.000)	
<i>E</i> (Settle_amt)	2.35 (0.125)		0.21 (0.649)	
Backlog_SCALs	0.72 (0.395)		2.71 (0.099)	
Backlog_lawsuits_less	0.78 (0.378)		0.00 (0.963)	
Rev_prd	0.95 (0.330)		0.01 (0.941)	
Observations	202	152	134	57
Adjusted R ²	0.726	0.645	0.577	0.986

This table reports the LPM estimation results of Eq. (2), where *Non-enforcement* is regressed on two ex post egregiousness measures (*E*(Mis_amt) and *Mis_prd*) and controls, using subsamples of the non-enforcement sample. All other control variables and fixed effects are the same as those used in column (7) in Table 6. *Early_file* is an indicator variable that equals 1 for firm-years against which securities class action lawsuits were filed before the SEC began its investigations, and 0 otherwise. See Appendix 1 for variable definitions. Two values are reported for each variable: the coefficient estimate, and standard errors clustered by regional offices and judicial circuits (in parentheses). Wald test results, *Chi*² statistics, and *p*-values (in parentheses) for the difference in the parameters in each pair of models are reported at the bottom of the table. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

TABLE 9 CHANGES AROUND SEC SANCTIONS

Panel A: Non-matched (Five-year window; $N = 977$)				
$DV =$	Sales	Op_ROA	Analyst	CEO_ten
	(1)	(2)	(3)	(4)
SEC_sanc	-5.773*** (0.133)	-0.211*** (0.053)	-0.412 (0.333)	0.701** (0.304)
Post	0.192*** (0.058)	-0.037*** (0.013)	0.019 (0.122)	-0.285** (0.124)
SEC_sanc \times Post	-0.557*** (0.102)	-0.041*** (0.016)	-1.319*** (0.156)	-0.349** (0.168)
Year dummy	Included	Included	Included	Included
Firm dummy	Included	Included	Included	Included
Constant	9.114*** (0.211)	0.177*** (0.027)	4.454*** (0.234)	0.799** (0.385)
Observations	977	977	977	509
Adjusted R ²	0.937	0.658	0.651	0.494
Panel B: PSM (Five-year window; $N = 140$)				
SEC_sanc \times Post	-1.255*** (0.249)	-0.238*** (0.054)	-2.090*** (0.410)	-0.789** (0.351)
Observations	140	140	140	85
Adjusted R ²	0.943	0.637	0.635	0.347
Panel C: Three-year window				
(1) Non-matched				
SEC_sanc \times Post	-0.439*** (0.110)	-0.039** (0.017)	-1.188*** (0.169)	-0.268 (0.193)
Observations	700	700	700	362
Adjusted R ²	0.952	0.727	0.667	0.535
(2) PSM				
SEC_sanc \times Post	-1.076*** (0.279)	-0.250*** (0.071)	-2.325*** (0.513)	-0.547 (0.412)
Observations	101	101	101	58
Adjusted R ²	0.949	0.605	0.608	0.253

This table reports the difference-in-differences (DiD) estimation results of Eq. (3), where four outcome variables are regressed on the treatment (SEC_sanc), post-treatment dummy ($Post$), and their interaction ($SEC_sanc \times Post$) using the non-matched (Panel A: $N = 977$) and propensity score-matched (Panel B: PSM; $N = 140$) samples. Outcome variables are sales ($Sales$), operating ROA (Op_ROA), number of analysts following ($Analyst$), and CEO tenure (CEO_ten). SEC_sanc represents all firm-years of firms for which the SEC filed AAERs at least once during the sample period, and 0 otherwise. $Post$ equals 1 for all firm-years since the year of SEC sanction for sanctioned firms and the year of class action filing for non-enforced firms, and 0 otherwise. $Post$ is 1 for all firm-years since the matched year for the non-enforced firms in the PSM sample. See Appendix 1 for variable definitions. In Panel C, a shorter window of three years before and after treatment is adopted. Two values are reported for each variable: the coefficient estimate, and robust standard errors (in parentheses). *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

TABLE 10 SHORT-TERM MARKET REACTION

	AAERs (unsanctioned) (1)	SCALs (unsanctioned) (2)	<i>Diff.</i> (1) – (2) (3)	AAERs (unsanctioned) (4)	SCALs (unsanctioned) (5)	<i>Diff.</i> (4) – (5) (6)
(1) Event date:	Filing date	Filing date		Filing date	Filing date	
CAR	-0.014* (0.007)	-0.048*** (0.008)	0.034** (0.013)	-0.014*** (0.008)	-0.045*** (0.010)	0.031** (0.015)
<i>N</i>	156	389	545	142	241	383
(2) Event date:	Earliest disclosure	Earliest disclosure		Earliest disclosure	Earliest disclosure	
CAR	-0.112*** (0.013)	-0.067*** (0.008)	-0.045*** (0.007)	-0.091*** (0.015)	-0.059*** (0.010)	-0.032* (0.017)
<i>N</i>	231	384	615	142	241	383
(3) Event date:	Filing date	Settled date		Filing date	Settled date	
CAR	-0.014* (0.007)	0.003 (0.004)	-0.003** (0.004)	-0.014* (0.008)	0.002 (0.004)	-0.016** (0.008)
<i>N</i>	156	256	412	142	241	383

This table reports the abnormal returns associated with the announcement of misreporting at three alternative timings: 1) the filing date of AAERs and SCALs (the first row of columns (1) and (2)), 2) the earliest date of AAER, SCAL, and restatement filing dates (the second row of the same columns), and 3) the filing date of AAERs and the settlement date of SCALs (the third row of the same columns). In columns (1) and (2), I use 412-545 observations, of which three-day cumulative abnormal returns (CARs) are available as of the aforementioned alternative timings. In columns (4) and (5), I use 383 observations that are common in (1)-(3). CAR is a three-day market-adjusted return for each firm over the interval *from* the one trading day before *through* the one trading day after the event date. I estimate the CAR with a 100-day window. Abnormal returns are defined in excess of the predicted normal returns from the actual returns for each day in the three-day event window. The values in columns (1), (2), (4), and (5) are the coefficients on the constant terms of the regression, where the three-day CAR is regressed on the constant. The values in columns (3) and (6) are the differences between columns (1) and (2), and (4) and (5), respectively. *p*-values denoted by the number of asterisks are reported for the statistics that test whether the CAR equals 0 (columns (1), (2), (4), and (5)), or whether the CARs of AAERs and SCALs differ (columns (3) and (6)). In particular, *p*-values in columns (3) and (6) are for the unpaired *t*-tests. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

ENDNOTES

ⁱ “Private actions under rule 10b-5 are an *essential supplement* to Commission enforcement of the Exchange Act, and the Commission has a strong interest in seeing that the principles applied in such actions promote the purposes of the securities laws” (p. 305) vs. “the SEC has used its influence to persuade the Court...to tighten pleading requirements in securities fraud cases, and to dismiss investors’ claims on the ground of *failure to prove causation*” (p. 304).

ⁱⁱ SCALs provide an opportunity to study committed *but* unsanctioned misreporting cases, because committed misreporting is not observed until detected by either the SEC or the courts.

ⁱⁱⁱ See Bratton and Wachter (2011) for an opposing view that private enforcement has failed in terms of both damage compensation and deterrence.

^{iv} For example, the Division of Enforcement is composed skilled accountants and attorneys with at least two to five years of prior experience, and is supported by other divisions in the SEC’s headquarters and regional offices staffed with thousands of full-time employees. Moreover, the SEC’s legal rights to investigate cases and collect evidence from firms and witnesses compensate its resource constraints and differentiate it from other institutions (e.g., Bremser et al., 1991; and Shleifer, 2005).

^v Managers may use accruals to manipulate earnings, which is well established in the literature through the positive correlation between the magnitude of accruals and the probability of detected misreporting (Jones et al., 2008). Likewise, financing needs and insider trading are two traditional incentives for managers to misreport (Dechow et al., 1996; and Beneish, 1999). Managers may be tempted to inflate earnings to attract external financing at low cost, or to maximize the monetary value of their equity portfolio (Agrawal and Cooper, 2015). Note that insider trading itself is not a manifestation of misconduct, because insiders’ stock trading becomes *illegal* only when they utilize inside information. This can be confirmed only through further investigations.

^{vi} In contrast, Choi et al. (2019) examine SEC enforcement regarding non-accounting issues and the relation between non-accounting and accounting violations.

^{vii} While the amounts of misreporting and settlement are positively correlated each other, their respective association with SEC non-enforcements is the opposite. Nonetheless, I find that the cases with greater amounts of both misreporting and settlement tend to be sanctioned more often than unsanctioned by the SEC.

^{viii} I exclude financial firms because their main accruals type (i.e., loan loss provision) is different from that of manufacturing firms.

^{ix} Separately, accruals measures were estimated using firms with at least ten years of industry observations. I set the minimum requirement at ten years of industry observations to obtain sufficient data for the estimation, following Jones et al. (2008).

^x In Eq. (1), I do not include firm fixed effects. As in Khanna et al. (2015), misreporting models generally do not include firm fixed effects because their use in the binary estimation generates biases when the number of firm observations is small (see Blackburne, 2014).

^{xi} Although some firms restated misreporting after the SEC closed investigations, it seems reasonable to assume that the SEC was able to estimate the amount to be restated at the time of investigation closure as a result of its in-depth investigations.

^{xii} The main finding of the authors is that high case backlogs significantly reduce the likelihood of opening new cases.

^{xiii} See also Heese et al. (2020) for a similar case for the Department of Justice.

^{xiv} I calculate abnormal returns using the market-adjusted model with an estimation window of 100 days. Specifically, abnormal returns are defined in excess of the predicted normal returns from the actual return for the three-day window around event dates.