The Deterrent Effect of Insider Trading Enforcement Actions

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ABSTRACT

We analyze whether exposure to an SEC insider trading enforcement action affects how insiders trade. We find that following an insider trading enforcement action at one firm, exposed insiders earn significantly lower abnormal profits from their trades at other firms compared to non-exposed insiders. The deterrent effect is stronger when a fellow insider is convicted and is similarly significant both pre- and post-SOX. Following the enforcement event, exposed insiders do not trade less frequently, but do trade significantly fewer shares per trade. Insiders who have witnessed an enforcement action have a lower probability for future conviction than their unexposed peers.

Keywords: insider trading, inside information, deterrence, enforcement, salience

JEL Classification: G14, G40, K42.

Data Availability: Data are available from the public sources cited in the text.

I. INTRODUCTION

Does exposure to an enforcement action for illegal insider trading exhibit a deterrent effect on corporate insiders' future trading? Existing rules and regulations discourage insiders from the use of proprietary information for private benefit. Yet many of them continue to trade opportunistically, which raises questions about the effectiveness of existing regulations (Bhattacharya and Marshall 2012; Soltes 2016; Amiram, Huang, and Rajgopal 2020). A better understanding of what deters informed insider trading could provide insight into the underlying motivations behind informed trading and have important implications for firms and regulators seeking to constrain opportunistic executive behavior.

In this paper, we study whether the enforcement of insider trading laws at one firm affects affiliated corporate insiders' trading behavior in other firms based on a dataset of all insider trading

enforcement actions by the SEC from 1995 to 2017.[1] More specifically, we define our "treatment group" as all (non-convicted) insiders present at a given firm at the time of an enforcement event (enforcement firms) who also trade as insiders in another firm before and after the event (sample firms).[2] We define our "control group" as all insiders in sample firms who did not witness an enforcement event and trade before and after the associated event. To test for a deterrent effect, we estimate a staggered difference-in-differences model with firm-year fixed effects of changes of several characteristics of insider trades across the treatment and control group. The evaluation of the behavior of insiders away from the enforcement firm and the inclusion of firm-year fixed effects alleviate concerns that our results are attributable to omitted factors related to the enforcement firm, sample firm-year characteristics, or confounding time trends.

We find that the profitability of insiders' trades decreases significantly following exposure to an enforcement event. The effect is statistically significant for both purchases and sales and is economically meaningful. For example, we estimate that after the enforcement event treated insiders earn 7.9% lower abnormal returns over the 180 days following purchases than do control insiders in the same firm. Although exposed insiders do not trade less frequently, they trade fewer shares per trade following the event, suggesting that exposure to an enforcement action reduces net insider trading activity.

We consider two channels through which enforcement actions could deter insiders – *information* and *salience*. Witnessing an enforcement action could provide information about the actual probability for regulatory investigation and enforcement moving forward. For example, the

[1] Throughout the paper, we use the terms enforcement action and enforcement event interchangeably.
[2] The term 'insider' refers to Section 16 officers, who are generally executives, non-senior managers, directors, and individuals owning more than 10% of the firm's outstanding stock. They are required to file trading reports under Section 16(a) of the Securities and Exchange Act of 1934.

event may signal an increased level of enforcement of insider trading laws due to stronger regulatory focus. Trades made by insiders who have worked at firms whose stock was illegally traded in the past may also face increased regulatory scrutiny in the future, even when they trade other securities. We refer to this possibility as an information effect.

Witnessing an enforcement event could also prompt insiders to react to the event and change their behavior even if enforcement risk is unchanged. A growing body of research has shown that individuals tend to overweight salient information when making decisions. For example, salience has been found to bias a series of consumption (Bordalo, Gennaioli, and Shleifer 2013), management (Dessaint and Matray 2017), and judicial decisions (Guthrie, Rachlinski, and Wistrich 2001; Viscusi 1999). We refer to this possibility as a salience effect.

Ex ante, it is not clear whether a deterrent effect of enforcement on insider trading occurs through the information channel or the salience channel. However, our research design and several of our tests suggest that the information channel alone is unlikely to explain the totality of our results. Enforcement quality largely reflects the resources and enforcement priorities of regulators (Glaeser, Johnson, and Shleifer 2001; Kedia and Rajgopal 2011). We compare the behavior of insiders at the same firm and the same time using a staggered difference-in-differences design with firm-year fixed effects that effectively controls for systematic regulatory factors as well as a wide range of time-varying covariates correlated with enforcement activity.

We indirectly evaluate the significance of the information channel by conditioning our analysis on whether a Section 16 insider was convicted for insider trading. 85 percent of insider trading cases are against non-Section 16 insiders. In these cases, individuals who are *not* officers of the firm (e.g. investment bankers, consultants, and even therapists) obtain proprietary information by a matter of chance and trade based on this information. As a result, these insider

trading cases are expected to contain limited information about the degree of compliance within the firm. Our results remain significant when examining the effect of exposure to enforcement events involving non-Section 16 insiders, suggesting that the behavioral response we document is unlikely to only reflect information related to increased regulatory scrutiny in the future. As expected, the profitability of insider trades also declines significantly when a fellow Section 16 insider is convicted and the effect here is stronger in the case of insider purchases.[3]

Next, we examine the effect of exposure to an enforcement action on the profitability of insider trades conditional on the distance between the sample and enforcement firms. Enforcement actions are situated within enforcement firms and their salience does not depend on where sample firms are located. Their information content, however, is expected to weaken with the distance between sample and litigation firms given that the probability for an insider trading enforcement action is expected to reflect local regulatory and perhaps cultural factors. We find that the exposure effect exhibits similar statistical and economic significance across sample firms that are located relatively close or far from the enforcement firm. Finally, we examine the deterrent effect before and after the enactment of the Sarbanes-Oxley Act (SOX). We fail to find a significant difference in deterrence across the pre-SOX and post-SOX periods, despite the fact that SOX intensified the public scrutiny of insider trades.

In sum, we show that the effect of exposure to enforcement of insider trading laws does not vary with variables proxying for the information content of the enforcement event. Thus, our results are broadly consistent with the idea that the deterrent effect of enforcement actions occurs in part due to the salience of directly witnessing enforcement and not necessarily due to the

[3] This result is consistent with the information and the salience channel given that the conviction of a fellow insider could be both more informative and salient.

information it provides. Our tests do not rule out the importance of the information channel, however, as it may still play a significant role in some cases.

Finally, we examine whether exposure to an enforcement event is associated with future illegal insider trading. We find that the future conviction rate of insiders exposed to an enforcement event is substantially lower than the conviction rate of unexposed insiders, suggesting that enforcement may deter illegal insider trading. Of the 4,544 insiders present at firms at the time of an enforcement action, only one is convicted of illegal trading in the future.

The paper contributes to the literature studying the costs and benefits of financial misconduct. Although empirical evidence from the 1990s shows that perpetrators incur significant costs (Desai, Hogan, and Wilkins 2006; Karpoff, Lee, and Martin 2008; Hennes, Leone, and Miller 2008), more recent research suggests that corporate misconduct can pay off. For example, Amiram et al. (2020) find that more than half of the perpetrators in recent years would benefit from engaging in financial reporting misconduct. Jagolinzer, Larcker, Ormazabal, and Taylor (2020) document a significant relation between political connections and informed trading by corporate insiders at financial institutions during the period in which Troubled Asset Relief Program (TARP) funds were disbursed in the 2008 financial crisis. Our study presents evidence of a deterrent effect of insider trading enforcement actions for insiders who directly witness enforcement, suggesting that insiders are engaging in some form of cost-benefit consideration prior to trading; otherwise, revisions in beliefs about detection would not affect their decision making. We also find that this consideration does not necessarily reflect the objective probability for enforcement.

II. DETERRENCE AND ENFORCEMENT ACTIONS

Deterrence is a theory of compliance with legal norms in which potential offenders balance the benefits and costs associated with deviant behavior. The benefits can be pecuniary, as well as

psychological (e.g., thrill seeking). The potential costs for violators include loss of reputation (Klein and Leffler 1981; Levin 2003); a sense of guilt or shame (Charness and Dufwenberg 2006); and third-party sanctions, such as penalties, fines, and prison sentences.

While the origin of most theories of deterrence can be traced to the legal philosophers of the 17th century, the first modern formalization of the deterrence process was provided by Becker (1968).[4] According to Becker, potential offenders respond to both the probability of detection and the severity of punishment. Prior research on deterrence is non-conclusive. For example, Weisburd, Einat, and Kowalski (2008) find that would-be offenders are affected by both the probability of detection and the expected penalty. However, Braithwaite and Makkai (1991) and Walters and Geyer (2004) contend that deterrence exhibits a more limited effect on behavior because criminals have a disposition that leads to crime. The evidence also suggests that the efficacy of deterrence varies depending on the severity of the crime (Dölling, Entorf, Herrmann, and Rupp 2009) and that the probability of detection is a stronger deterrent than the magnitude of the penalty ensuing from apprehension (Nagin 2013).

In the corporate setting, Delis, Staikouras, and Tsoumas (2016) find that banks subject to enforcement actions subsequently reduce their risk exposure, suggesting that deterrence may occur at a micro level with those subject to enforcement changing their behavior. However, Nguyen (2021) finds evidence that white-collar crime increased following the 9/11 terrorist attack and subsequent shift of enforcement priorities to counter-terrorism cases, and Huang, Roychowdhury, and Sletten (2020) show that when litigation risk decreased for ninth-circuit firms, real earnings management increased for those firms, suggesting deterrence may occur at a macro level with a market broadly responding to shifts in enforcement.

[4] See Nagin (2013) for a detailed review of the deterrence literature.

We investigate whether direct exposure to an insider trading enforcement action exhibits a deterrent effect on insiders' opportunistic trading. We consider two possible channels through which enforcement could affect individual behavior – *information* and *salience*. Enforcement actions may contain information about both the probability of future enforcement and the expected penalty. For example, enforcement actions could reveal information about the resources and enforcement priorities of regulators. Enforcement could also raise individual awareness about existing laws and regulations. As a result, exposure to enforcement actions could prompt individuals to update their priors about both the probability and the expected costs of enforcement.

However, witnessing enforcement could change the behavior of exposed insiders even when it provides no additional information about enforcement risk. Existing research in psychology, sociology, and economics suggests that the "salience" of a characteristic, or its tendency to stand out, significantly affects behavior. For example, consumers are more likely to tilt their consumption towards goods and services with more noticeable attributes (Bordalo et al. 2013). With this in mind, marketing strategists routinely enhance product attributes that are more advantageous to consumers and obscure attributes that are less desirable (Gabaix and Laibson 2006). Further, even highly trained professionals such as judges and executives frequently overweight the more salient aspects of their environment when making decisions (Bordalo, Gennaioli, and Shleifer 2015; Dessaint and Matray 2017).

If salience affects individual behavior broadly, then it could also influence one's propensity for compliance with insider trading laws. In particular, factors that make the benefits of deviant behavior more salient are expected to promote such behavior and factors that make the costs of deviant behavior more salient are expected to deter such behavior. The salience effect could be

further strengthened by the fact that enforcement is an infrequent event and people exhibit limited ability to comprehend low probabilities (Kahneman and Tversky 1979).

The information and salience channels are not mutually exclusive and could jointly affect insider trading behavior. As an example, the SEC could require an enforcement firm to hire an Independent Compliance Consultant to ensure the firm's improvement with internal compliance procedures. Insiders who go through mandatory compliance training could change their behavior because they learn new information related to enforcement risk or because the training made the enforcement event more salient even if there was no change in enforcement risk. The case is analogous to studying why an airline crash reduces travel. On the one hand, airline crashes could reduce travel by legitimately altering assessments of flight risk. For example, the recent Boeing 737-MAX disasters uncovered a broad range of issues with internal Boeing processes and triggered negative public response to the events (e.g., Cioroianu, Corbet, and Larkin 2021). On the other hand, airline crashes could reduce travel through salience alone without providing any information per se about the future probability of a crash. Indeed, there is evidence that individuals routinely overreact to fearsome risks (Sunstein and Zeckhauser 2011).

III. EMPIRICAL DESIGN

Methodology

To test the existence of a deterrent effect of enforcement on insider trading, we search all SEC Litigation Releases (LR) for variants of the phrase 'insider trading' (including 'inside trade', 'inside information', 'insider trade', 'illegal trade', and 'inside trading') from September 20th, 1995 through December 26th, 2017. The LR series, available from the SEC starting in 1995 at https://www.sec.gov/litigation/litreleases.shtml, provides summaries of civil litigation the SEC

brought against individuals for regulatory violations, including cases of illegal insider trading.[5] Next, we read through every flagged Litigation Release to determine if the case is related to illegal insider trading and identify the firm(s) whose stock was illegally traded, the convicted individuals and their association to the firm, and the Litigation Release date. All cases involve a publicly traded firm and at least one individual convicted for insider trading. After merging the firms with Thomson Reuters' insider filing data and CRSP, we are left with 923 firms whose stock was illegally traded. We define these firms as *enforcement firms*.

To identify a treatment group of insiders exposed to enforcement, we start with all Section 16 insiders present at an enforcement firm at the time of the enforcement event and remove all insiders who were convicted for illegal insider trading (*convicted insiders*). We define all remaining insiders as exposed to enforcement (*exposed insiders*) if they trade both before and after the enforcement event, defined as the date the SEC Litigation Release was issued, or if they are listed in the firm's DEF 14A filing as an officer of the firm during the year the Litigation Release is issued.[6] This definition will all but ensure the insider was directly exposed to the enforcement event, but will exclude exposed insiders who are not directors or top 5 paid executives or did not trade before and after the enforcement event.

Next, we define *treated insiders* as exposed insiders who are also Section 16 insiders at another (non-enforcement) firm and trade the stock of this other firm both before and after their exposure to an enforcement action. We evaluate the trading behavior of treated insiders in these

[5] The LR series does not include investigations that did not lead to prosecution, and at least in the releases we read does not include cases that the SEC lost.

[6] In some cases, an insider may become exposed to enforcement before the Litigation Release date. However, objectively and systemically defining when this occurs is difficult. The SEC can investigate suspicious trades without informing even the individual who made the trade, much less employees of the firm whose stock was traded. And, as mentioned in the introduction, 85 percent of cases do not involve Section 16 insiders. The firm's officers need not know about investigations or enforcement events until they are made public.

non-enforcement firms relative to the trading behavior of other insiders in these same firms who were never exposed to an enforcement event. These other insiders enter our sample as *control insiders* if they execute trades both before and after the associated enforcement event. Because we analyze purchases separately from sales, we require all insiders in models analyzing purchases (sales) to purchase (sell) shares before and after the enforcement event. For example, an insider who only purchases shares pre-exposure and only sells shares post-exposure does not enter our final sample. Firms that have treated and control insiders who trade before and after the enforcement event enter our sample as *sample firms*. Focusing on firms that were never subject to an SEC enforcement action allows us to control for potential confounding factors that could affect insider trading through other channels unrelated to perceptions of enforcement risk.

Let us use Litigation Release 19549 as an illustration. LR 19549 details insider trading at Citigroup Inc. by senior executive Victor Menezes. In this example, Citigroup is the enforcement firm and Menezes is a convicted insider. Exposed insiders are those at Citigroup at the time the Litigation Release was issued (1/31/2006). For example, Alain Belda was an independent director at Citigroup on the LR date. Belda was also an insider at Arconic Inc., where he served as Chief Executive Officer. Belda enters our sample as a treated insider because he sold Arconic stock before and after 1/31/2006. Our control insiders are those who also sell Arconic stock before and after 1/31/2006 but have no association with Citigroup and were therefore not exposed to enforcement. For example, Joseph Gorman was an independent director at Arconic Inc. and sold Arconic stock before and after 1/31/2006. In our model, we compare the abnormal returns from sales of Arconic stock made by Belda and Gorman before and after the associated enforcement event. If some insiders trade opportunistically and exposure to insider trading enforcement serves as a deterrent, then we expect that trades made by Belda in the post-enforcement period will earn

significantly lower abnormal returns than trades he made in the pre-enforcement period and that this decline in abnormal returns will be significantly larger than any decline in abnormal returns earned by trades made by Gorman in the post compared to the pre-enforcement period.

Formally, we estimate variants of the following regression model:

$$Y_{i,j,t} = \alpha + \beta_1 Treated\ insider_{i,j,t} + \beta_2 Post_{i,j,t} + \beta_3 Treated\ insider_{i,j,t} * Post_{i,j,t} + Firm - Year\ fixed\ effects_{j,t} + \epsilon_{i,j,t}, \quad (1)$$

where the dependent variables $Y_{i,j,t}$ measure various characteristics of insider *i*'s trades in firm *j*'s stock at time *t*, such as trade profitability, frequency, and size. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for all trades taking place after the Litigation Release date, and equal to 0 for all trades taking place before the Litigation Release date (all variable definitions are provided in the Appendix). We also include the difference-in-differences term, the interaction term *Treated insider* * *Post*. All insiders in the analysis execute at least one purchase (or one sale) in a sample firm both before and after the LR date, so that we compare the trading behavior of the same insiders in the same firms in both periods.

The model includes firm-year fixed effects because insider trading has been shown to vary systematically across firms and over time (Arif, Kepler, Schroeder and Taylor 2020; Blackburne, Kepler, Quinn and Taylor 2020).[7] This fixed effects structure measures within firm-year variation

[7] Both Arif et al. (2020) and Blackburne et al. (2020) analyze trades within firm-year-quarter. While this research design even more strongly controls for factors that may vary within firm over time, our sample construction leaves us with an average of 6 insiders per sample firm who make between 1.2 and 2.2 trades a year. Many firm-year-quarters have only 1 trade or only have trades by treated or control insiders, not both, resulting in little variation to exploit. Including firm-year fixed effects results in a loss of just 4 percent of our possible sample due to singleton groups of observations.

in insider trading characteristics, controlling for possible temporal shifts in factors correlated with enforcement actions. Although we cannot rule out a correlated omitted variable, such a variable would need to vary within the firm in a way that is correlated with the timing of treated and control insiders' trades within the year, the sample firm's future abnormal stock returns, and with the associated enforcement event, which is not directly linked to the sample firm and occurs at 475 different points in time across our sample firms.[8]

Characterizing Insider Trades

Illegal insider trading is formally defined as "buying or selling a security, in breach of a fiduciary duty or other relationship of trust and confidence, on the basis of material, nonpublic information about the security."[9] This definition implies that the identification and successful prosecution of illegal insider trading is challenging. Corporate insiders are routinely awarded company stock and regularly trade the stock to meet liquidity needs or for portfolio rebalancing, making it difficult to separate information-based trades from non-information-based trades. Further, regulators have limited resources to monitor insiders and enforce the law in capital markets that encompass thousands of different firms and securities. It can also be difficult to prove in court that someone traded *on the basis* of material, non-public information.

Yet, a large body of research suggests that insiders trade on privileged information that outside investors do not possess (Lin and Howe 1990; Huddart, Ke, and Petroni 2003; Huddart, Ke, and Shi 2007). First, consistent with informed trade, the stocks that insiders trade realize significant abnormal returns following the transaction (Lakonishok and Lee 2001). Second, insider

[8] We also estimate this model including insider-year fixed effects instead of firm-year fixed effects; the results are of similar statistical significance. Only 4 treated insiders are present at more than one sample firm, precluding the inclusion of insider and firm fixed effects.

[9] https://www.investor.gov/additional-resources/general-resources/glossary/insider-trading

purchases (sales) are associated with significant positive (negative) abnormal stock returns around earnings announcements (Hillier and Marshall 2002; Marin and Olivier 2008). Finally, the SEC regularly prosecutes over 100 individuals for insider trading each year, suggesting that illegal insider trading is prevalent despite enforcement efforts.

In light of the above evidence, we evaluate the opportunistic component of insider trades by estimating abnormal returns over the 180 trading days following the transaction. Non-informed trades should not be associated with significant abnormal stock return performance. Informed trades, on the other hand, are expected to be profitable. We note that our approach allows us to identify opportunistic behavior in aggregate and does not present evidence for illegal insider trading by any particular insider. We also characterize insiders in terms of their trade frequency and trade size and investigate whether exposure to enforcement alters these aspects of insiders' trading behavior.

Data and Summary Statistics

All data related to the enforcement event are collected from SEC Litigation Releases. In particular, we identify the firm whose stock was illegally traded, the individuals who execute the trades or pass on inside information and their association to the firm, and the date the Litigation Release was issued. We use the term "insider" to refer to officers of a firm who are required to file trading reports under Section 16(a) of the Securities and Exchange Act of 1934. We collect all information about insiders' trades from Thomson Reuters' insiders filing feed including: the trade date, whether the trade was a purchase or a sale, the number of shares traded, the number of shares the insider held following the trade, and the role of the insider (e.g., CEO, director).

Table 1 presents the distribution of the sample over time. It reports the number of enforcement firms, the number of exposed and treated insiders in enforcement firms, the number

of non-enforcement firms with a treated insider (sample firms), and the total number of non-treated insiders in all sample firms who trade before and after the event (control insiders) for each year of the sample period. We identify 923 different enforcement actions that can be matched to firms in Thomson Reuters. Among the 4,544 insiders exposed to the enforcement event, 478 individuals are also insiders in 480 sample firms and trade before and after the enforcement event in sample firms. We verify that sample firms were never exposed to an SEC enforcement event during the sample period (related to insider trading or otherwise). Our sample includes all insiders associated with these firms – 478 treated insiders and 2,407 control insiders.

Table 2 reports the percentage of insider trading cases in which individuals with certain associations to the firm are convicted for illegal insider trading. Since cases often involve multiple people, we categorize cases as follows. First, all cases involving a Section 16 insider are listed as such. Second, for cases not involving a Section 16 insider, we list the association to the firm of the individual who received the largest penalty from the SEC. Third, when the individual who received the highest penalty was a friend or family member of an associated individual who passed on private information, we take the association of that individual. For example, if an auditor obtained private information and gave that information to his brother, we treat this case as an event involving an auditor even if the unaffiliated brother received a larger penalty.[10] We present data for the 923 firms identified in releases that we can match to Thomson Reuters and the 475 enforcement firms (with 478 treated insiders) used to construct our sample.

The table shows that Section 16 insiders are convicted in 15 percent of cases (11 percent in our sample). It is possible that Section 16 insiders are convicted in a seemingly small percentage

[10] This could happen because the brother earned significantly larger illegal profits than did the auditor who shared the information. In a majority of cases, the SEC imposes penalties equal to the illegal profits earned from the trades.

of cases because it is difficult to establish in court that a Section 16 insider traded *on the basis* of private information or because Section 16 insiders are less likely to illegally exploit private information than others. We observe that most convicted individuals gain access to inside information through their role in another company with a contractual relationship with the firm, including those employed at consulting firms, suppliers, customers, law firms, investment banks, or audit firms.

Our sample construction methodology and empirical execution should yield results with relatively strong internal validity. However, our sample firms and insiders are not representative of the Thomson Reuters/CRSP/Compustat population which could raise questions about the external validity of our findings. In Table 3, we report firm-level (Panel A) and insider-level (Panel B) summary statistics for sample firms and insiders compared with the population across the Thomson Reuters, CRSP, and Compustat databases.

In Panel A, we observe that sample and population firms have similar annual stock returns and that sample firms have slightly lower stock return volatility and higher operating cash flows (significant at the mean). Additionally, sample firms are significantly larger, whether measured by total assets or market capitalization, and have significantly higher executive compensation.[11] Given that we analyze treated and control insiders in the same firm, these differences will not affect our results. However, it is possible that our results will not generalize to the population due to these differences and our results should be interpreted with this in mind.

In Panel B, we observe that sample insiders earn similar abnormal returns from purchases and lower abnormal returns from sales. They also execute slightly larger trades. We require that

[11] We also compare the distribution of sample firms across industry based on the Fama-French 17 industry classification. We find that sample firms are more often in the financial services industry but are otherwise similar to the population.

treated insiders hold at least two concurrent Section 16 insider positions. When analyzing all trades in the Thomson database, we find that insiders who do not hold multiple insider roles at the same time actually earn statistically larger abnormal returns than insiders who do, but the economic significance is trivial (approximately 0.2 basis points per day or less than 1 percent of the standard deviation). One-firm insiders generally execute smaller trades, but as a percentage of their holdings the difference is negligible.

Our sample contains 5,984 purchases and 46,774 sales.[12] Following Jagolinzer, Larcker, and Taylor (2011), we estimate abnormal insider trading returns from purchases (sales) as the α (-α) from the four factor Fama-French (1993) and Carhart (1997) model estimated over the 180 days following the transaction:

$$\left(R_{j,t} - R_{f,t}\right) = \alpha + \beta_1\left(R_{mkt,t} - R_{f,t}\right) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 UMD_t + \epsilon_t, \qquad (2)$$

where on a given day, $R_{j,t}$ is the daily return to firm *j*'s equity, $R_{f,t}$ is the daily risk-free interest rate, $R_{mkt,t}$ is the daily CRSP value-weighted market return, and SMB_t, HML_t, and UMD_t are the daily size, book-to-market, and momentum factors (Fama and French 1993; Carhart 1997). The estimated intercept term α (-α) is the average daily risk-adjusted return to a net purchase (sale) during the 180 days following the trade (*Insider abnormal returns)*.

Table 4 reports descriptive statistics for the variables used in our analysis. *Insider abnormal returns* has a mean value of 0.072 for purchases and -0.037 for sales.[13] These coefficients can be interpreted as daily basis points following the trade and are consistent with estimates from the prior literature – for example, using the same estimation methodology Jagolinzer et al. (2011) report

[12] We treat multiple purchases or sales by the same individual on the same day in the same firm as one transaction.
[13] These values are slightly different than those reported in Table 3 because all trades by sample insiders in any firm they are an insider at and made at any time are included in the comparison with the population.

mean values of 0.06 and -0.02 for purchases and sales, respectively. Insiders make about 1.226 purchases and 2.246 sales per year, and the average trade size is just over 13 thousand shares for purchases and 21 thousand shares for sales. The values for *Treated insider* indicate that 31.0% of purchases and 14.7% of sales are made by treated insiders. We observe that 52.0% of purchases and 62.8% of sales are made after the enforcement event. 18.7% (15.1%) of purchases (sales) are associated with an enforcement event in which a Section 16 insider was convicted.

An implicit assumption of a difference-in-differences estimation is that there is no unobserved time-varying confounding effect to the treatment. One commonly used diagnostic for confounding effects is a parallel trend in the values of the dependent variable across the treatment and control groups pre-treatment. It requires that in the absence of treatment, the difference between the treatment and control groups is constant over time. While a staggered difference-in-differences design with firm-year fixed effects should reduce concerns over confounding effects, we plot insider abnormal returns across treated insiders and control insiders in event time for purchases and sales, respectively, in Figures 1.A and 1.B. As exposure to enforcement occurs on a specific day in the event year, we measure the time from year t-1 to year t0 as the 365-day period leading up to the enforcement event (the Litigation Release date) and time from year t0 to year t+1 as the 365-day period following the LR date (and so forth for other event years). We observe that the abnormal returns for the two groups of insiders tend to exhibit parallel trends prior to enforcement, and do not trend towards the condition we document post treatment.

IV. RESULTS

Baseline Results – Profitability of Insider Trades

Table 5 reports results from estimates of our baseline model, equation (1). The dependent variable is *Insider abnormal returns*, as defined in equation (2). We estimate equation (1) for our

full sample of insiders, as well as the sub-samples associated with enforcement events involving illegal trading by Section 16 insiders and by non-Section 16 insiders. For each sample, regressions analyzing purchases are presented first.

Our primary variable of interest, the interaction term *Treated insider * Post,* is negative and significant at the 0.01 level in the full sample for purchases and sales. The results are economically significant. For example, results presented for purchases in the first column indicate that the difference-in-differences in abnormal returns of exposed insiders post-treatment is 4.4 basis points lower per day, which accounts for over half of the sample mean of 7.2 basis points. This translates to reduced abnormal returns of approximately 7.9% over the 180 days following the trade. We note though, that the 95 percent confidence interval around this coefficient estimate is large, ranging from -0.076 to -0.012. So, while the results strongly suggest a significant decline in abnormal returns following treatment, the coefficient estimate is not precise.

The coefficients on both *Treated insider* and *Post* are not significantly different from 0, indicating that abnormal returns in the pre-enforcement period are not different across treated and control insiders and that there is no difference in abnormal returns from trades made by control insiders in the pre- and post-enforcement periods.[14] The literature documents that insider sales exhibit relatively weaker information content than insider purchases, possibly because sales are more likely to be liquidity-driven (Cohen, Malloy, and Pomorski 2012) or because sales have higher litigation risk (Cheng and Lo 2006). The results in Table 5 are highly significant for sales and again, economically meaningful. We estimate a 2.1 basis point (column 2) differential reduction in abnormal returns from sales with a 95 percent confidence interval of -0.034 to -0.009.

[14] We re-estimate the regressions estimating abnormal returns from insider trades based on the Fama and French (1992) 3-factor model. The interaction term remains statistically significant at the 0.05 level or better in all cases.

For comparison, the sample mean is 3.7 basis points. The results suggest that exposure to an enforcement action could deter the exploitation of private information.

The last four columns of Table 5 present results when splitting the sample conditional on whether Section 16 insiders or only non-Section 16 insiders are convicted for illegal trading. As noted earlier, the group of Section 16 insiders consists of all officers, directors, and beneficial owners of more than 10% of the company's stock. If enforcement actions exhibit a deterrent effect on corporate insiders, then we would expect this effect to be stronger when one of their peers, another Section 16 insider, was convicted for illegal trading. Both the information and the salience channels predict a stronger deterrent effect here. The results provide evidence that the exposure effect is stronger for events involving Section 16 insiders. The estimated coefficients on the interaction term in the Section 16 sample (columns 3 and 4) are approximately twice as large in magnitude as the coefficients in the non-Section 16 sample (columns 5 and 6). The difference is statistically significant at the 0.05 level in the case of insider purchases. However, the interaction term remains statistically significant in the sample of purchases and sales associated with events that do not involve Section 16 insiders, suggesting that our full sample results are not entirely attributed to the Section 16 insider sample events. [15]

Our treated insiders are not representative of the Thomson Reuters population because we require that they are insiders in at least two firms at the same time. In practice, this means that treated insiders are more likely to be senior executives and directors than control insiders. Indeed,

[15] We provide an Internet Appendix listing the Litigation Release number, the enforcement firm, the convicted Section 16 insider and their role within the firm. Less than one percent of cases involve an employee who is a non-Section 16 insider and do not also involve at least one employee who is a Section 16 insider. Trades associated with these events are included in the non-Section 16 sample; their inclusion in the Section 16 sample does not influence our results. Also, in one enforcement firm the only convicted Section 16 insiders were listed as beneficial owners and were not employed by the firm. Excluding trades associated with this event from the Section 16 sample does not influence our results.

we confirm that while senior executives and directors account for 92 percent of trades by treated insiders, this percentage among control insiders is 66 percent.[16] It is not clear whether exposure to enforcement is a stronger deterrent for an executive, director, or other Section 16 insider. However, it is possible that senior executives (and directors) have more valuable private information, so any change in their behavior exhibits a larger effect on abnormal returns.

To address the above concern, we perform two robustness tests. First, we re-estimate the baseline model within the subsample of senior officers and directors. The results are presented in Table 6. We observe that the difference-in-differences term remains statistically significant in all regressions with similar coefficient estimates to those reported in Table 5. Second, similar to treated insiders, we require that control insiders be Section 16 insiders in at least two firms at the same time. The results are presented in Table 7. The difference-in-differences term remains significant at the 0.05 level or better in all cases.[17]

Conditional Analysis

We consider two channels through which enforcement could deter informed insider trading – information and salience. The information channel is active when the enforcement event triggers a Bayesian updating of either the actual probability for enforcement, the associated penalties, or both. The salience channel is active when the enforcement event changes the perceived risk of enforcement without necessarily affecting the actual risk. As discussed in Section 2, there is evidence in the literature that both channels could significantly influence behavior.

[16] We consider the following Thomson role codes as representing either senior executives or directors: CEO, CFO, CI, CO, CT, P, EVP, SVP, D, DO, CB, H, OD, and V).

[17] The deterrent effect of enforcement could also vary depending on the insider's role in the enforcement firm. For example, the CEO of a firm that had its stock traded illegally may more significantly change their behavior than would a director of that firm. 80 percent of our treated insiders were independent directors in the enforcement firm at the time of the event. Our results remain statistically significant at the 0.05 level or better when estimating regressions only including treated insiders who were serving as independent directors at the enforcement firm.

Our research design controls for a wide range of factors that could influence behavior through the information channel. Several important determinants of enforcement quality are on the regulatory side. Regulators exhibit limited resources and ability to enforce the law (e.g., Kedia and Rajgopal 2011) and their relative enforcement priorities vary through time. Our identification strategy compares the behavior of insiders at the same firm and trades made in the same year, which controls for the potential impact of macro factors on insider trading. However, it is still possible that insiders at a firm whose stock was illegally traded are subject to higher regulatory scrutiny in the future, even when they trade other securities. To assess this possibility, here we introduce three additional tests.

First, we condition our analysis on whether a Section 16 insider was convicted in the associated enforcement event. As discussed earlier in Section 4, we expect that any observed change in behavior will be greater with enforcement events involving a Section 16 insider. When a fellow Section 16 insider is convicted the event could provide more relevant information about future enforcement risk and it could also be a more salient event for the insider absent any change in underlying risk. Akerlof and Kranton (2000) contend that individuals can be influenced by people they identify with and ignore the behavior of people they do not identify with. In this regard, insiders could react more strongly to the prosecution of their peers because they identify more strongly with these individuals. However, insiders may also suspect that their own trades will face higher levels of regulatory scrutiny when a fellow senior executive or board member is convicted.

When an unaffiliated individual (non-Section 16 insider) is convicted there is less reason to expect that exposed insiders will face additional regulatory scrutiny going forward. For example, Litigation Release 14731 describes insider trading at Medstat Group Inc. by former CFO William Rauwerdink. Contrast that with Litigation release 14754, in which a therapist gained inside

information while providing counseling services to a Lockheed Corporation executive and traded illegally using that information. In the first case, illegal trading was done by a prominent executive and could indicate troubling aspects of the corporate culture within the firm such as weak controls, an absence of policies regarding insider trading, or poor oversight. In the second case, the illegal trading is completely independent of the firm and it appears there is little the firm could have done to prevent a licensed therapist from abusing doctor-patient confidentiality. As a result, we argue that any response by insiders exposed to enforcement events involving unaffiliated individuals is more likely due to the salience of the event.

Our baseline tests in Tables 5, 6, and 7 condition the analysis on whether a Section 16 insider (middle two columns) or only non-Section 16 insiders (last two columns) were convicted in the associated enforcement event. We find that exposed insiders earn significantly lower abnormal returns from their trades following *both* types of enforcement actions. We interpret the fact that our results remain significant in cases in which no Section 16 insider was convicted as evidence that part of the deterrent effect of enforcement occurs through salience.

While the results appear stronger for enforcement events involving Section 16 insiders, only 11 percent of our treated insiders are associated with these events. In our next two cross-sectional tests we examine sub-samples that often contain less than 50 percent of our initial sample of trades. When further conditioning on the role of the convicted insider we are left with less than 5 percent of our initial sample in some cases. Because of this, and because we observe significant associations between abnormal returns and treatment even when the enforcement event does not involve a Section 16 insider, we present results from analysis of our full sample moving forward.

First, we examine the effect of exposure to an enforcement action on the profitability of insider trades conditional on the distance between the sample and enforcement firms. Enforcement

actions concern enforcement firms and, as a result, the salience of the event is not expected to depend on the geographic location of sample firms. The information content of an enforcement event, however, may weaken with distance to the event because enforcement quality could reflect regional factors such as regulatory resources or enforcement priorities. To examine the implications of distance for the deterrent effect of insider trading enforcement actions, we partition our sample firms based on whether they are within 75 miles of the enforcement firm and present the results in Table 8. Consistent with the salience channel, we find that the exposure effect remains statistically significant at the 0.05 level or better in both subsamples and that the coefficient estimates are not significantly different across these samples.[18]

Second, we examine the deterrent effect of enforcement in the periods before and after the enactment of the Sarbanes-Oxley Act (SOX) of 2002. Post SOX, insiders are required to report Form 4 detailing inside trades to the SEC within two business day of the trade. Previously, insiders had until the 15th day of the month following the trade to report. While it is not clear whether SOX was associated with an increase in insider trading investigations or enforcement, it did represent a significant increase in public scrutiny around the time of insider trades.[19]

To assess whether the deterrent effect of enforcement is significantly different post-SOX, we examine sample firms for which both pre- and post-enforcement trades occur in the pre-SOX period and firms with both such trades in the post-SOX period.[20] The results are presented in Table

[18] The 75-mile cutoff was chosen because it creates samples in which the sample and enforcement firms are either within or outside of the same Core Based Statistical Area. However, using a 37.5-mile cutoff shifts fewer than 10 percent of our observations across groups and the results remain similarly significant.

[19] From Table 1 we see an increase in insider trading convictions in the years following the great recession, but no strong trend over longer periods of time. It is possible that investigations increased post-SOX and also possible that the true level of illegal trade decreased in response to SOX.

[20] We separate the samples in this way to allow for a cleaner interpretation of any effect of SOX. A SOX term in the full-sample regression will only have within firm-year variation in year 2002 and we have sample firms for which all trades occur before or after 2002. This also allows us to avoid attributing an effect of specific cases of enforcement to

9. The first two columns analyze firms with pre- and post-enforcement trades in the pre-SOX period, while the last two columns analyze firms with pre- and post-enforcement trades in the post-SOX period. The interaction term *Treated insider * Post* remains negative and significant in all four models. Insiders may have changed their trading behavior in multiple ways in response to SOX, but we do not find evidence that it influenced the deterrent effect of being present at a firm when enforcement occurs.

Additional Results – Trade Frequency and Trade Size

The insider trading literature has focused on abnormal returns earned from trades. Yet, insiders could change other characteristics of their trading as well. We next consider whether insiders change the frequency or the size of their trades after exposure to an enforcement event. The results are presented in Table 10.

The first model of Table 10 (columns 1 and 2) reports estimates from an OLS regression evaluating trade frequency. The dependent variable, *Trade frequency*, is the number of trades an insider makes in either the 365 days before or after the Litigation Release date. We measure trade frequency in this manner because we often do not know the year an insider's tenure started or ended at the firm, so the further we are from the enforcement event, the more likely that the absence of trades in a given year is because the insider was not at the firm. Because each insider has only one observation before and after the LR date there is no within firm-year variation, therefore we include firm fixed effects instead of firm-year fixed effects in this regression. We fail to find evidence that treated insiders trade at different frequency following exposure.[21]

SOX and vice versa in cases where pre-enforcement trades occur pre-SOX and post-enforcement trades occur post-SOX.

[21] The results remain statistically insignificant if we consider longer time ranges from the Litigation Release date.

The dependent variable in the middle two columns of Table 10 is *Trade size scaled*, the number of shares traded scaled by shares held and normalized using the sample average and standard deviation, and in the last two columns is *Trade size*, the number of shares traded scaled by 1,000. Scaling shares traded by holdings addresses the fact that insiders can only sell shares they already own (insiders can generally not sell short). But shares held is only provided for 62 percent of trades in our sample, therefore we include raw shares traded to document that the results are not associated with the inclusion of shares held in Thomson-Reuters.

We find that following exposure to an insider trading enforcement action, insiders significantly reduce the size of their trades. The results are statistically significant for both purchases and sales at the 0.05 level for both measures of trade size. Given the average trade size, the effect appears larger for purchases. This is reasonable because some percentage of sales serve to meet liquidity needs and trade size in those cases will be affected by the needs of the insider. The fact that exposed insiders trade in smaller quantities following enforcement (relative to control insiders) and at the same frequency, implies that exposure to SEC enforcement actions induces insiders to trade less. This result is consistent with the notion that exposure to enforcement of insider trading laws acts as a deterrent against informed trading.

Following exposure to an enforcement action, insiders may seek to change other aspects of their trading behavior to reduce regulatory scrutiny. Formally, insiders could trade pursuant to Rule 10b5-1 trading plans. The SEC enacted Rule 10b5-1 in October 2000 to provide insiders an affirmative defense against charges of insider trading. The rule allows insiders to pre-announce trades in advance under the premise that an insider would not have material non-public information around the time of the trade when the pre-announcement was made. Informally, insiders may seek to make non-informed trades following routine patterns based on reasonably estimable liquidity or

diversification needs. Cohen et al. (2012) classify insiders and trades as routine (versus opportunistic) based on whether their trades occur in the same month for three consecutive years. Our sample includes 1,918 Rule 10b5-1 trades and only 34 of our treated insiders can be classified as routine based on the timing of their trades, making a robust estimate of changes in behavior related to routine forms of trade difficult. Instead, we verify that our results remain significant when excluding Rule 10b5-1 trades and routine traders from the sample.[22]

Exposure to SEC Enforcement Actions and the Probability for Future Violations

If exposure to enforcement actions significantly reduces the propensity of insiders to engage in informed trading, then exposed insiders should exhibit lower future conviction rates than unexposed insiders. A robust estimation of the conviction rate of insiders exposed to SEC enforcement actions is challenging given the small number of enforcement events and the smaller number of events involving Section 16 insiders.

In Table 11, we provide a parsimonious estimate of these probabilities by calculating the conviction rates across three types of insiders: insiders who witnessed an enforcement action (exposed insiders who were not convicted in this specific case); insiders who did not witness an enforcement action but are colleagues in another firm with an insider exposed to SEC enforcement actions (our control insiders); and the remaining insiders in the Thomson Reuters database (other insiders). We observe that the conviction rate of exposed insiders is indeed significantly less than the conviction rate of unexposed insiders. This finding must be interpreted with the small number of convictions in mind, but of the 4,544 insiders exposed to enforcement, only 1 is ever convicted

[22] Jagolinzer (2009) identified 10 percent of firms and 3 percent of insiders as having 10b5-1 trading plans over years in which most of our sample trades occur. While disclosure of such plans is common now, evidence in Jagolinzer (2009) suggests such plans were often not publicly disclosed in the early 2000s. Cohen et al. (2012) are able to classify approximately one-third of trades as routine or opportunistic. We are able to categorize a higher percentage of trades using their methodology, but a relatively small proportion of insiders in our sample are classified as routine traders.

of illegal insider trading in the future. The small number of convicted Section 16 insiders prevents strong inferences, but if exposed insiders are subject to higher regulatory scrutiny following exposure, then we would expect higher future conviction rates absent a more than offsetting change in trading behavior.

V. CONCLUSION

We study the deterrent effect of the enforcement of insider trading laws on insider trading by compiling a dataset with all SEC enforcement actions of illegal insider trading from 1995-2017. To construct our treatment group, we start with all insiders present at enforcement firms when the enforcement event occurred. Afterwards, we identify within this group the subset of insiders who are also insiders at other firms and contrast their trading behavior in these firms to the behavior of other insiders. This approach controls for firm- and time-specific confounding effects associated with enforcement firms that could affect insider trading and allows for stronger identification of a possible deterrent effect of enforcement on insider trading.

We find that exposure to an enforcement action significantly affects insider trading behavior – the abnormal returns from trades made by exposed insiders decreases following an SEC enforcement action relative to the abnormal returns from trades made by control insiders. Exposure to an enforcement action also reduces insiders' trading activity, as reflected in reduced trade size. Our results suggest that insiders strategically assess the costs and benefits of their trades. We consider two non-mutually exclusive channels through which enforcement could deter informed trade – information and salience. Our results provide evidence that enforcement is a salient event that can deter informed insider trading even when it provides limited information about enforcement risk.

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Appendix - Variable Definitions

Variable	Definition and Data Source
Enforcement firm	Firm whose stock is traded illegally. *Source: SEC Litigation Releases*
Convicted insider	Insider who is convicted for illegal insider trading. *Source: SEC Litigation Releases*
Exposed insider	Non-convicted Section 16 insider at an enforcement firm as of the Litigation Release date. *Source: SEC Litigation Releases, Thomson Reuters, DEF 14A filings*
Treated insider	Exposed insider who also trades before and after the Litigation Release date in a sample (non-enforcement) firm. *Source: SEC Litigation Releases, Thomson Reuters*
Sample firm	Non-enforcement firm that includes a treated insider. *Source: SEC Litigation Releases, Thomson Reuters*
Control insider	Non-treated insider who trades before and after the associated Litigation Release date in a sample firm. *Source: SEC Litigation Releases, Thomson Reuters*
Insider abnormal returns	Abnormal returns from purchases (sales) calculated as the Alpha (-Alpha) from the four factor Carhart model (Carhart 1997) estimated over the 180 days following the transaction. *Source: CRSP, Thomson Reuters, Fama-French factors*
Post	An indicator variable equal to 1 for trades occurring after the associated Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. *Source: SEC Litigation Releases, Thomson Reuters*
Trade frequency	The number of trades made in the 365 days preceding or following the associated Litigation Release date. *Source: SEC Litigation Releases, Thomson Reuters*
Trade size scaled	The number of shares traded scaled by shares held and normalized using the sample average and standard deviation. *Source: Thomson Reuters*
Trade size	The number of shares traded in thousands. *Source: Thomson Reuters*
Annual Return	The firm's annual stock return. Source: CRSP
Return volatility	The standard deviation of the firm's last 12 monthly stock returns. *Source: CRSP*
Operating cash flows	Net operating cash flows scaled by total assets. *Source: Compustat*
Total assets	The natural logarithm of the firm's total assets. *Source: Compustat*
Market capitalization	The natural logarithm of the firm's market capitalization. *Source: Compustat*
Total compensation	The natural logarithm of the average total compensation paid to the firm's top 5 paid executives. *Source: Execucomp*
Equity compensation	The natural logarithm of the average equity compensation paid to the firm's top 5 paid executives. *Source: Execucomp*

Figure 1.A. Parallel Trends in Abnormal Returns from Purchases

This figure displays the average annual abnormal returns earned from purchases by treated insiders and control insiders in event time with the enforcement event occurring at time t = 0.

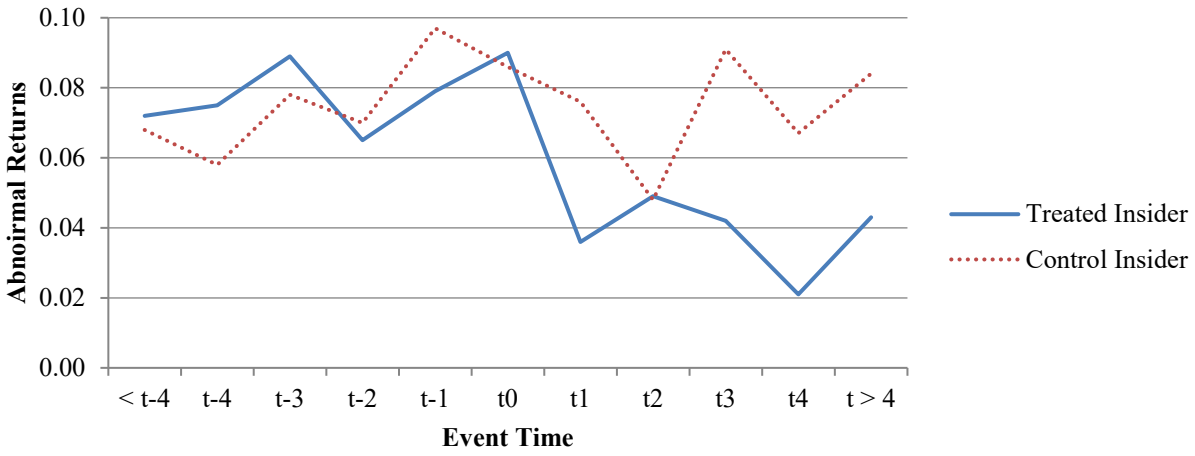

Figure 1.B. Parallel Trends in Abnormal Returns from Sales

This figure displays the average annual abnormal returns earned from sales by treated insiders and control insiders in event time with the enforcement event occurring at time t = 0.

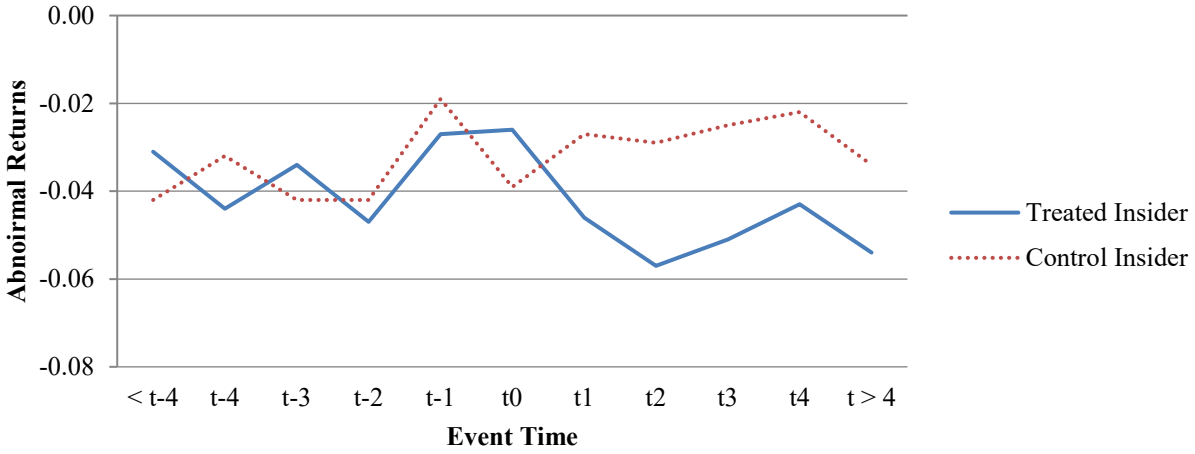

Table 1
Sample Composition over Time

This table reports the number of firms whose stock is traded illegally using inside information (enforcement firms); the number of Section 16 insiders in enforcement firms as of the SEC Litigation Release date (exposed insiders); the subset of exposed insiders who trade before and after the associated Litigation Release date in at least one non-enforcement firm (treated insiders); the number of non-enforcement firms with a treated insider (sample firms); and the total number of non-treated insiders in all sample firms who trade before and after the associated Litigation Release date (control insiders) for each year of the sample period.

Year	Enforcement Firms	Exposed Insiders	Treated Insiders	Sample Firms	Control Insiders
(1)	(2)	(3)	(4)	(5)	(6)
1995	18	36	0	0	0
1996	34	142	10	10	51
1997	23	84	9	9	36
1998	54	195	32	34	164
1999	34	198	32	32	142
2000	60	303	27	29	179
2001	46	217	22	22	79
2002	41	127	6	6	45
2003	29	185	25	25	136
2004	32	137	13	13	61
2005	29	295	29	29	171
2006	28	224	29	29	168
2007	29	181	17	17	142
2008	60	283	33	33	158
2009	97	550	57	56	266
2010	67	326	28	27	139
2011	76	320	27	27	102
2012	33	127	18	18	76
2013	23	101	18	18	79
2014	49	233	19	19	78
2015	19	73	6	6	35
2016	31	151	16	16	94
2017	13	59	5	5	6
Total	923	4,544	478	480	2,407

Table 2
Who is Convicted for Insider Trading?

This table reports the percentage of individuals convicted for illegal insider trading based on their association to the enforcement firm. All cases involving a Section 16 insider are listed as such. For cases not involving Section 16 insiders we list the association to the firm of the individual who received the largest penalty from the SEC. When the largest penalty was given to a friend or family member of the individual who passed on inside information, we list the association of the individual who provided the information. We present data for the 923 firms identified in SEC Litigation Releases matched by CUSIP to Thomson Reuters and the 475 enforcement firms used to construct our sample.

	923 enforcement firms with CUSIPs in Thomson Reuters	475 enforcement firms with a treated insider
	(1)	(2)
Section 16 insider	15%	11%
External consultant	17%	18%
Employee at another firm (supplier, customer, potential merger target)	15%	15%
External lawyer	12%	13%
Investment banker	12%	13%
Security analyst	9%	8%
Professional investor	8%	7%
Hedge fund manager	7%	9%
Auditor	3%	4%
Non-Section 16 insider employee	1%	1%
Therapist	1%	1%

Table 3
Panel A: Firm-Level Sample Statistics Comparison to the Population

This table reports firm-level summary statistics for sample firms compared with the population of firms across the Thomson Reuters, CRSP, and Compustat databases. *Annual return* is the firm's stock return over the previous 12 months; *Return volatility* is the standard deviation of the firm's last 12 monthly stock returns; *Operating cash flows* is net operating cash flows scaled by total assets; *Total assets* is the natural logarithm of the firm's total assets; *Market capitalization* is the natural logarithm of the firm's market capitalization; *Total compensation* is the natural logarithm of the average total compensation paid to the firm's top 5 paid executives; *Equity compensation* is the natural logarithm of the average equity compensation paid to the firm's top 5 paid executives. (***), (**), and (*) indicate statistically significant differences between the means and medians of firms in our sample and firms in the Thompson Reuters/CRSP/Compustat universe at the 0.01, 0.05 and 0.10 level, respectively.

	Sample firms			Thomson Reuters/CRSP/Compustat population		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Annual return	0.17	0.09	0.58	0.16	0.06	0.68
Return volatility	0.11**	0.09*	0.08	0.13	0.11	0.10
Operating cash flows	0.05**	0.08	0.19	0.02	0.06	0.19
Total assets	8.09***	8.43***	2.14	6.03	6.02	2.07
Market capitalization	8.08***	8.39***	1.91	5.72	5.67	1.96
Total compensation	7.78***	7.80***	0.93	7.11	7.07	0.87
Equity compensation	6.85***	7.25***	1.79	5.70	6.21	2.17

Table 3
Panel B: Insider-Level Sample Statistics Comparison to the Population

This table reports insider-level summary statistics for sample insiders compared with the population of insiders across the Thomson Reuters and CRSP databases. *Insider abnormal returns* from purchases (sales) is calculated as the Alpha (-Alpha) from the four factor Carhart model estimated over the 180 days following the transaction (Carhart 1997). *Trade size* is the number of shares traded in thousands. (***), (**), and (*) indicate statistically significant differences between the means and medians of insiders in our sample and insiders in the Thompson Reuters/CRSP universe at the 0.01, 0.05 and 0.10 level, respectively.

	Sample insiders			Thomson Reuters/CRSP population		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Purchases:						
Insider abnormal returns	0.07	0.05	0.27	0.07	0.05	0.29
Trade size	20.77**	2.40*	73.87	18.55	1.52	77.71
Sales:						
Insider abnormal returns	-0.04***	-0.03**	0.21	-0.02	-0.02	0.25
Trade size	27.41**	5.00	94.93	26.10	5.00	87.85

Table 4
Trade-Level Sample Summary Statistics

This table reports summary statistics for sample variables and categorizations associated with insider purchases and sales. *Insider abnormal returns* from purchases (sales) is calculated as the Alpha (-Alpha) from the four factor Carhart model estimated over the 180 days following the transaction (Carhart 1997). *Trade frequency* is the number of trades made by an insider over the 365 days preceding or following the associated Litigation Release date. *Trade size scaled* is the number of shares traded scaled by shares held and normalized using the sample average and standard deviation. *Trade size* is the number of shares traded in thousands. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. Section 16 insider convicted trades are trades made by insiders associated with an enforcement event in which a Section 16 insider is convicted. Senior executive/director trades are trades made by insiders with the following Thomson Reuters insider role codes: CEO; CFO; CI; CO; C; P CT; EVP; P; SVP; CB; D; DO; H; MC; OD; VC. Multiple-role insider trades are trades made by insiders who have served as a Section 16 insider concurrently in multiple firms. Close firm trades are trades made in sample firms that are within 75 miles of the associated enforcement firm.

	Purchases		Sales	
	Max. Observations (5,984)		Max. Observations (46,774)	
	Mean	St. Dev	Mean	St. Dev
	(1)	(2)	(3)	(4)
Mean value:				
Insider abnormal returns	0.072	0.271	-0.037	0.199
Trade frequency	1.226	2.072	2.246	3.748
Trade size scaled	0.000	1.000	0.000	1.000
Trade size	13.527	46.598	21.295	54.532
Percentage of trades:				
Treated insider	31.0%	46.4	14.7%	30.9
Post	52.0%	46.1	62.8%	48.3
Treated insider * Post	24.0%	39.1	11.6%	26.6
Section 16 insider convicted trades	18.7%	40.7	15.1%	35.4
Trades by senior executives and directors	72.1%	33.4	64.4%	46.9
Trades by multiple-role insiders	60.6%	47.4	41.8%	49.7
Close firm trades	43.1%	49.6	41.4%	48.9

Table 5
Insider Abnormal Returns and Exposure to an Enforcement Action

This table reports coefficient estimates from OLS regressions of insider abnormal returns on exposure to an SEC insider trading enforcement action. Our sample consists of insiders at firms that were never subject to an SEC enforcement action but have an insider who was exposed to an SEC enforcement action at another firm. All insiders in the sample trade both before and after this enforcement action. *Insider abnormal returns* from purchases (sales) is calculated as the Alpha (-Alpha) from the four factor Carhart model (Carhart 1997) estimated over the 180 days following the transaction. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. The first two columns present results using the entire sample; the middle two columns present results for trades made by insiders associated with an enforcement event in which a Section 16 insider was convicted; the last two columns present results for trades made by insiders associated with an enforcement event in which only non-Section 16 insiders were convicted. All models include firm-year fixed effects. Standard errors in all models are adjusted for clustering at the firm-level. (***), (**), and (*) indicate statistical significance at the 0.01, 0.05 and 0.10 level, respectively.

	Full Sample		Section 16 Insider Convicted		Non-Section 16 Insider Convicted	
	Purchases	Sales	Purchases	Sales	Purchases	Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Treated insider	0.010	0.009	0.039	0.022	0.015	0.008
	(0.85)	(1.26)	(1.28)	(1.35)	(1.46)	(0.99)
Post	-0.035	0.004	0.225	-0.020	-0.039	0.007
	(-0.57)	(0.20)	(1.49)	(-0.94)	(-0.99)	(0.35)
Treated insider * Post	-0.044***	-0.021***	-0.078***	-0.038**	-0.036**	-0.020**
	(-2.70)	(-2.76)	(-2.97)	(-2.37)	(-2.39)	(-2.24)
Intercept	0.096***	-0.039***	-0.038	-0.007	0.089***	-0.044***
	(2.86)	(-3.33)	(-0.37)	(-0.51)	(4.11)	(-3.35)
Coefficient: Treated insider * Post S16 convicted less S16 not convicted			-0.042**	-0.018		
P-value: Treated insider * Post S16 convicted ^= S16 not convicted			(0.04)	(0.38)		
Firm-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,984	46,774	1,119	7,042	4,865	39,732
Adjusted R-squared	0.58	0.57	0.67	0.50	0.58	0.58

Table 6
Insider Abnormal Returns and Exposure to an Enforcement Action:
Trades Made by Senior Executives and Directors

This table reports coefficient estimates from OLS regressions of insider abnormal returns on exposure to an SEC insider trading enforcement action. All insiders in these regressions are senior executives are directors. Senior executive/directors are insiders with the following Thomson Reuters insider role codes: CEO; CFO; CI; CO; C; P CT; EVP; P; SVP; CB; D; DO; H; MC; OD; VC. Our sample consists of insiders at firms that were never subject to an SEC enforcement action but have an insider who was exposed to an SEC enforcement action at another firm. All insiders in the sample trade both before and after this enforcement action. *Insider abnormal returns* from purchases (sales) is calculated as the Alpha (-Alpha) from the four factor Carhart model (Carhart 1997) estimated over the 180 days following the transaction. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. The first two columns present results using the entire sample; the middle two columns present results for trades made by insiders associated with an enforcement event in which a Section 16 insider was convicted; the last two columns present results for trades made by insiders associated with an enforcement event in which only non-Section 16 insiders were convicted. All models include firm-year fixed effects. Standard errors in all models are adjusted for clustering at the firm-level. (***), (**), and (*) indicate statistical significance at the 0.01, 0.05 and 0.10 level, respectively.

	Full Sample		Section 16 Insider Convicted		Non-Section 16 Insider Convicted	
	Purchases	Sales	Purchases	Sales	Purchases	Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Treated insider	0.008	0.008	0.031	0.038	0.014	0.005
	(0.53)	(1.04)	(1.01)	(1.64)	(0.94)	(0.69)
Post	-0.012	0.009	0.209	-0.031	-0.078	0.015
	(-0.16)	(0.37)	(1.42)	(-1.02)	(-1.22)	(0.57)
Treated insider * Post	-0.054***	-0.020**	-0.079***	-0.046*	-0.044**	-0.017**
	(-2.75)	(-2.46)	(-2.73)	(-1.94)	(-2.14)	(-2.04)
Intercept	0.095**	-0.043***	-0.026	0.001	0.123***	-0.051***
	(2.32)	(-3.00)	(-0.26)	(0.04)	(3.48)	(-3.33)
Firm-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,312	30,124	891	4,277	3,421	24,975
Adjusted R-squared	0.59	0.58	0.72	0.49	0.57	0.58

Table 7
Insider Abnormal Returns and Exposure to an Enforcement Action:
Trades Made by Insiders Holding Section 16 Insider Roles in Multiple Firms

This table reports coefficient estimates from OLS regressions of insider abnormal returns on exposure to an SEC insider trading enforcement action. All insiders in these regressions served as Section 16 insiders in at least two firms concurrently. Our sample consists of insiders at firms that were never subject to an SEC enforcement action but have an insider who was exposed to an SEC enforcement action at another firm. All insiders in the sample trade both before and after this enforcement action. Insider abnormal returns from purchases (sales) is calculated as the Alpha (-Alpha) from the four factor Carhart model (Carhart 1997) estimated over the 180 days following the transaction. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. The first two columns present results using the entire sample; the middle two columns present results for trades made by insiders associated with an enforcement event in which a Section 16 insider was convicted; the last two columns present results for trades made by insiders associated with an enforcement event in which only non-Section 16 insiders were convicted. All models include firm-year fixed effects. Standard errors in all models are adjusted for clustering at the firm-level. (***), (**), and (*) indicate statistical significance at the 0.01, 0.05 and 0.10 level, respectively.

	Full Sample		Section 16 Insider Convicted		Non-Section 16 Insider Convicted	
	Purchases	Sales	Purchases	Sales	Purchases	Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Treated insider	0.010	0.009	0.058*	0.019	0.005	0.010
	(0.66)	(1.02)	(1.69)	(1.06)	(0.31)	(1.11)
Post	0.013	0.008	0.312**	-0.029	-0.067	0.010
	(0.13)	(0.35)	(2.28)	(-1.01)	(-0.71)	(0.43)
Treated insider * Post	-0.069***	-0.023**	-0.091***	-0.043**	-0.056**	-0.020**
	(-2.92)	(-2.38)	(-2.75)	(-2.11)	(-2.16)	(-2.09)
Intercept	0.088*	-0.039***	-0.085	0.004	0.126**	-0.044***
	(1.70)	(-2.68)	(-0.92)	(0.20)	(2.49)	(-2.88)
Firm-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,626	19,543	697	3,411	2,929	15,936
Adjusted R-squared	0.60	0.55	0.78	0.50	0.55	0.55

Table 8
Insider Abnormal Returns and Exposure to an Enforcement Action:
Conditioning on the Distance between Sample and Enforcement Firms

This table reports coefficient estimates from OLS regressions of insider abnormal returns on exposure to an SEC insider trading enforcement action. Our sample consists of insiders at firms that were never subject to an SEC enforcement action but have an insider who was exposed to an SEC enforcement action at another firm. All insiders in the sample trade both before and after this enforcement action. *Insider abnormal returns* from purchases (sales) is calculated as the Alpha (-Alpha) from the four factor Carhart model (Carhart 1997) estimated over the 180 days following the transaction. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. The first two columns include trades in sample firms that are located within 75 miles of the associated enforcement firm, while the last two columns include trades in sample firms that are located more than 75 miles away from the associated enforcement firm. The rows below the intercept report the difference and P-values from a pairwise test of differences of Treated insider * Post in regressions in sample firms within and beyond 75 miles of the enforcement firm. All models include firm-year fixed effects. Standard errors in all models are adjusted for clustering at the firm-level. (***), (**), and (*) indicate statistical significance at the 0.01, 0.05 and 0.10 level, respectively.

	Sample Firm and Enforcement Firm within 75 miles of each other		Sample Firm and Enforcement Firm not within 75 miles of each other	
	Purchases	Sales	Purchase	Sales
	(1)	(2)	(3)	(4)
Treated insider	0.023	0.012	0.014	0.013
	(1.34)	(1.29)	(1.21)	(1.32)
Post	-0.023	-0.003	-0.047	0.026
	(-0.27)	(-0.17)	(-0.64)	(1.15)
Treated insider * Post	-0.063**	-0.023**	-0.041***	-0.026**
	(-2.24)	(-2.39)	(-3.03)	(-2.52)
Intercept	0.089*	-0.033***	0.102***	-0.053***
	(1.81)	(-3.73)	(2.77)	(-3.59)
Coefficient: Treated insider * Post within 75 miles less beyond 75 miles	-0.022	0.003		
P-value: Treated insider * Post within 75 miles less beyond 75 miles	(0.24)	(0.94)		
Firm-Year fixed effects	Yes	Yes	Yes	Yes
Observations	2,580	19,382	3,404	27,392
Adjusted R-squared	0.61	0.56	0.58	0.56

Table 9

Insider Abnormal Returns and Exposure to an Enforcement Action: Pre and Post Sarbanes Oxley

This table reports coefficient estimates from OLS regressions of insider abnormal returns on exposure to an SEC insider trading enforcement action. Our sample consists of insiders at firms that were never subject to an SEC enforcement action but have an insider who was exposed to an SEC enforcement action at another firm. All insiders in the sample trade both before and after this enforcement action. *Insider abnormal returns* from purchases (sales) are calculated as the Alpha (-Alpha) from the four factor Carhart model (Carhart 1997) estimated over the 180 days following the transaction. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. The first two columns analyze sample firms with pre- and post-enforcement event trades in the pre-SOX period, while the last two columns analyze sample firms with pre- and post-enforcement event trades in the post-SOX period. The rows below the intercept report the difference and P-values from a pairwise test of differences of Treated insider * Post in regressions in sample firms with pre- and post-enforcement trades in the pre-SOX period and those within the post SOX period. All models include firm-year fixed effects. Standard errors in all models are adjusted for clustering at the firm-level. (***), (**), and (*) indicate statistical significance at the 0.01, 0.05 and 0.10 level, respectively.

	Firms with pre- and post-enforcement event trades in the pre-SOX period		Firms with pre- and post-enforcement event trades in the post-SOX period	
	Purchases	Sales	Purchases	Sales
	(1)	(2)	(3)	(4)
Treated insider	0.028	-0.001	0.011	0.014
	(1.05)	(-0.06)	(0.91)	(1.29)
Post	-0.063	0.016	-0.003	-0.009
	(-0.61)	(0.44)	(-0.04)	(-0.42)
Treated insider * Post	-0.049*	-0.032**	-0.039**	-0.029**
	(-1.90)	(-2.03)	(-2.24)	(-2.20)
Intercept	0.115*	-0.089***	0.069*	-0.014
	(1.94)	(-5.43)	(1.92)	(-1.12)
Coefficient: Treated insider * Post Pre-SOX less Post-SOX	-0.010	-0.003		
P-value: Treated insider * Post Pre-SOX ^= Post-SOX	(0.79)	(0.95)		
Firm-Year fixed effects	Yes	Yes	Yes	Yes
Observations	905	6,175	2,547	16,202
Adjusted R-squared	0.48	0.55	0.58	0.52

Table 10
Trade Frequency, Trade Size, and Exposure to an Enforcement Action

This table reports coefficient estimates from OLS regressions of trade frequency and trade size on exposure to an SEC insider trading enforcement action. Our sample consists of insiders at firms that were never subject to an SEC enforcement action but have an insider who was exposed to an SEC enforcement action at another firm. All insiders in the sample trade both before and after this enforcement action. *Trade frequency* is the number of trades an insider makes in during the 365 days before or after the associated Litigation Release date. *Trade size scaled* is the number of shares traded scaled by shares held and normalized using the sample average and standard deviation. *Trade size* is the number of shares traded in thousands. *Treated insider* is an indicator variable equal to 1 for treated insiders (those exposed to an enforcement event) and equal to 0 for control insiders (those never exposed to an enforcement event). *Post* is an indicator variable equal to 1 for trades occurring after the Litigation Release date and equal to 0 for trades occurring before the Litigation Release date. Standard errors in all models are adjusted for clustering at the firm-level. The last two rows report the total number of observations and the adjusted R-squared. (***), (**), and (*) indicate statistical significance at the 0.01, 0.05 and 0.10 level, respectively.

	Trade Frequency		Trade Size Scaled		Trade Size	
	Purchases	Sales	Purchases	Sales	Purchases	Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Treated insider	0.018	-0.302	0.141	0.039	7.869	3.448
	(0.11)	(-1.32)	(1.42)	(0.88)	(1.41)	(1.18)
Post	0.021	0.123	-0.067	-0.042	-1.917	2.342
	(0.13)	(1.02)	(-0.71)	(-1.05)	(-0.25)	(0.89)
Treated insider * Post	-0.023	-0.316	-0.206**	-0.122**	-9.682**	-6.602**
	(-0.12)	(-1.29)	(-2.18)	(-2.20)	(-2.28)	(-2.35)
Intercept	1.223***	2.251***	-0.034	0.029	13.458***	19.940***
	(12.49)	(34.58)	(-0.64)	(1.13)	(4.46)	(12.15)
Firm-Year fixed effects	No	No	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	No	No	No	No
Observations	1,340	5,778	3,900	28,585	5,984	46,774
Adjusted R-squared	0.19	0.23	0.43	0.28	0.34	0.39

Table 11

Exposure to an Enforcement Action and the Likelihood for Future Indictment

This table reports the number of convicted insiders, the number of non-convicted insiders, and the conviction rate across three types of insiders: non-convicted insiders who were present at an enforcement firm on the Litigation Release date (exposed insiders); insiders who did not witness an enforcement action but are colleagues at another firm with an insider who was exposed to an SEC enforcement action (control insiders); and all remaining Section 16 insiders in Thomson Reuters (all other insiders). The last row reports P-values from a pairwise test of differences of the conviction rates of exposed and non-exposed insiders.

	Exposed insiders	Non-exposed insiders (control insiders)	Non-exposed insiders (all other insiders)
	(1)	(2)	(3)
Convicted of insider trading	1	4	216
Not convicted	4,543	2,403	259,320
Conviction rate	0.022	0.166	0.083
P-value (conviction rate of exposed insider ≠ conviction rate of non-exposed insider)		0.03	0.09