Do Executives Behave Better When Dishonesty is More Salient?

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Abstract

In behavioral experiments, individuals are less likely to cheat at a task when the saliency of dishonesty is increased [Mazar, Amir, and Ariely (2008), Gino, Ayal, and Ariely (2009)]. We test a similar hypothesis in a real world setting by treating news about high-profile political scandals as shocks to the salience of unethical/illegal behavior and its consequences. We find that local corporate insiders engage in fewer suspect behaviors in the year after a political scandal is revealed. Their stock sales are less profitable and they are less likely to sell stock ahead of large price declines, suggesting less illegal insider trading. These patterns vary predictably with the level of media attention to scandal-related events during the scandal years. Locally headquartered firms also appear to engage in less earnings management following the revelation of a political scandal. However, these changes in executives' behaviors appear to be largely transitory and the evidence of suspect behaviors resumes in following years.

I. Introduction

One cannot go long without learning of new instances of business executives engaging in unethical or illegal behaviors. Recent examples include the large-scale frauds perpetrated by public companies during the stock market run-up of the late 1990s, the alleged misbehaviors of bankers who securitized and sold mortgage-backed securities, and corporate insiders who either traded on private information or passed it along to their outside associates. Previous literature has shown that executives' wrongdoings are costly to both shareholders and society. Karpoff, Lee and Martin (2009) estimate a loss of $4.08 in reputational penalty for every dollar a company misleadingly inflates its market value. Kedia and Philippon (2009) show how earnings manipulations can amplify business cycles and cause the misallocation of resources. The ubiquity and costliness of illegal corporate activity has motivated a great deal of effort to understand and, hopefully, minimize these behaviors.

With this paper, we attempt to further our understanding of the factors that impact executive behaviors. Experimental studies in psychology show that people modify their actions when the ethical content of behaviors is more salient to them; i.e., when ethical considerations are made to stand out in an obvious way. Mazar, Amir, and Ariely (2008) showed that individuals are less likely to cheat on a task that could lead to a monetary reward when they were first asked to write down the Ten Commandments. Gino, Ayal, and Ariely (2009) find that when a person's attention is merely drawn to ethical considerations before playing a game they cheat less. In that case, an actor pretending to be one of the participants asked aloud before the game began, "So, is it OK to cheat?," to which the experimenter responded "You can do whatever you want." However, works like these are routinely confined to contrived

settings with small rewards at stake, and it is unclear the extent to which the results can help explain the actions of corporate executives in the business world.

We test whether executives in real world settings also appear to act more ethically/legally when their attention is drawn to examples of bad behavior and its consequences. To identify a quasi-experimental setting where this question can be evaluated, we focus on executives located in areas where a major political scandal is revealed publicly. We begin with a list of scandals identified by Puglisi and Snyder (2008), and, in order to conduct the cleanest difference-in-differences tests possible, limit our analysis to those states that experienced just one major scandal over the period 1997 to 2006. This resulted in a sample of ten scandals that occurred from 2001 to 2006. They are summarized in Table 1 and Figure 1, and include recognizable cases such as former House Majority Leader Tom Delay's relationship with corrupt lobbyist Jack Abramoff, and former Alabama Governor Don Siegelman's conviction for bribery and mail fraud in relation to alleged kickbacks from former HealthSouth CEO Richard Scrushy.[1]

Political scandals receive a great deal of attention in the news and it is our expectation that the amount of exposure to a scandal is increasing in geographic proximity to where it occurred. These high profile ethical and/or legal missteps are therefore more salient to local executives. According to the reasoning and evidence of Mazar et al. (2008) and Gino et al. (2009) that individuals behave more ethically when the ethical considerations are made more salient, we hypothesize that this will cause local executives to modify their behavior – for the better – to a

[1] Puglisi and Snyder (2008) identified 32 high profile political scandals in 18 states over the period 1997-2006. We also conduct all of our main analyses using all of the scandals that were both preceded and followed by two years without another local scandal, which resulted in 23 scandals in 18 states. All of the main results are qualitatively similar in this broader sample. This robustness analysis is included in the appendix tables.

greater extent than those living further away, whose attention is not grabbed as tightly by public discussion of the inappropriate acts of others.

To test this hypothesis, we focus on two suspect executive behaviors: insider trading and earnings management. The fact that actions are being brought against a politician for illegal acts associated with public office should not impact the amount of attention paid by authorities to these white collar crimes. New cases against politicians may therefore represent an exogenous shock to the saliency of illegal behaviors and their repercussions, but arguably do not impact the actual probability of corporate insiders being prosecuted. It is thus plausible to interpret any change in corporate executives' trading behavior or in earnings management as resulting from shocks to their level of attention to the illegal acts of others.

The first behavior examined is corporate insiders' stock trading activities. Insiders accumulate private information about their firms as they oversee its day-to-day operations, and, at the same time, they own significant amounts of their companies' stock. Thus, they have both the ability and incentive to trade on private information. Previous research provides evidence that insiders sometimes trade on private information and their trades often predict future abnormal return.[2] However, under U.S. securities laws, it is clearly illegal for anyone to trade a stock based on private information that is relevant to its value. Because corporate insiders trade often and there are reasonably straightforward methods for evaluating whether their trades are informed, we can test whether their behavior changes when the inappropriateness and negative consequences of breaking the law are made more salient.

[2] Previous research includes Lorie and Niederhoffer (1968); Jaffe (1974); Finnerty (1976); Seyhun (1986, 1992, 1998); Chowdhury, Howe, and Lin (1993); Bettis, Vickrey, and Vickery (1997); Lakonishok and Lee (2001); Jeng, Metrick, and Zeckhauser (2003); Agrawal and Cooper (2008); Agrawal and Nasser (2012); Cohen, Malloy, and Pomorski (2012); Alldredge and Cicero (2014), and Biggerstaff, Cicero and Wintoki (2015), among others.

We examine changes in insider trading in the twelve month periods beginning when a local political scandal is revealed publicly. We start by evaluating the profits generated by insiders' trades, which can be viewed as a proxy for informed trading. In a difference-in-differences setting, we find that the returns to insider stock sales declines after the revelation of a local political scandal, suggesting that they are less likely to be motivated by private information. In test that don't include fixed effects, we find that local insiders' sales are followed by average monthly abnormal returns that are approximately 1.90% more positive in the year following revelation of a political scandal. Controlling for year and industry fixed effects, we find that in scandal years the abnormal returns are 0.76% more positive following trades made by the full sample of insiders, and 1.62% more positive following the trades of top executives (one percent significance on both).

We also evaluate the likelihood that insiders trade when a profitable opportunity is presented. Again implementing difference-in-differences tests, we find that the odds an insider sells stock ahead of a large stock price decline is lower during the year following revelation of a local scandal. Similar results hold when we evaluate the number of shares sold. For example, compared to other years, during scandal years insiders sell less than half as many shares ahead of stock price declines of -10% or more.

Interestingly, we don't find similar results when evaluating insiders' stock purchases. In fact, we find some mixed evidence that local insiders' purchases are actually more profitable during the year following the revelation of a political scandal. We offer the following twofold explanation for this contrasting finding. For one, it is consistent with the general sentiment that it isn't as egregious for an insider to purchase their stock when they have information suggesting it is undervalued as it is for them to sell it when it is overvalued. Indeed, other

researchers argue that there is greater litigation risk is associated with selling stock than with purchasing it on private information (Skinner, 1994; Brochet, 2010; and Chen, Martin and Wang, 2012). Given the contrasting risks associated with informed purchases and sales, it is possible that insiders' increased awareness of the ethical and legal content of their actions has less of an impact on their informed stock purchasing activity. Second, it is also consistent with a desire to diversify away from their firms because they now feel more constrained from selling shares in the future ahead of price declines, which increases the costs of holding an undiversified portfolio. As such, they may not want to purchase shares and increase their holdings unless they are quite confident that it is a good investment. Consistent with these explanations, we find that during scandal years insiders are less likely to purchases shares ahead of price declines, but they are not more likely to purchase ahead of price increases. This pattern of behavior could cause more positive abnormal returns following purchases on average even though insiders were actually not more likely to buy their stock when in possession of private positive information about their firm. We find further that following the revelation of political scandals insiders indeed reduce their stock holdings in their firms by approximately 3% on average, suggesting the costs of holding a concentrated position are larger when profitable trading opportunities are restricted.

Further tests indicate that although insiders appear reluctant to sell their stock based on an informational advantage when unethical acts are more salient, the effect is largely temporary. This is demonstrated by regressions indicating that the evidence of restrained trading is apparent in the year that follows the initial revelation of local political scandals, but they are not evident in the second year following these events.

Unfortunately, it is difficult to identify a perfect setting to test social science hypotheses, so we must evaluate the extent to which our results are robust to alternative explanations. It is possible that corporate insiders may change their behavior in these settings in response to either a real or perceived increase in the probability of being caught engaging in illegal acts themselves. However, given that the authorities who investigate political corruption do not generally also investigate corporate white collar crimes, we would not expect for there to be an actual change in the likelihood of an insider trading or accounting fraud investigation.[3] Alternatively, it could be that the revelation of a political scandal and the attendant negative consequences causes corporate executives to become more acutely aware of the costs associated with wrongdoing. To the extent that this is the mechanism causing changes in observed behaviors, we would still classify it as a response to the increased salience of consequences.

To evaluate whether changes in insiders' trading behavior are in response to increases in the probability of being caught, we evaluate whether trading patterns during scandal years vary with the level of local media attention given to the scandal. In months with more local news articles about the scandals, insiders are both less likely to sell their stock and their trades are less profitable that sales in other months during the scandal year. During scandal revelation years, insiders are approximately 17% less likely to sell stock during months when local newspapers run an above median number of articles referencing the scandal. When they do sell stock in these months, their trades are followed by abnormal returns that on average are approximately 1.20% more positive than sales in other months during the scandal year. These

[3] Political corruption is normally investigated by the Department of Justice or congressional ethics committees, whereas financial fraud and insider trading cases are normally brought by the S.E.C. and/or private parties. To the extent that the Department of Justice also investigates financial frauds or insider trading these investigations are conducted by different divisions than those prosecuting political corruption.

results suggest that any changes in insider behavior is not in response to changes in the odds of being caught for wrongdoing since the level of law enforcement activities is unlikely to vary across such short time periods.

We turn next to whether insiders appear to also act more ethically on behalf of their firms when dishonesty is more salient. To do so, we focus on indicators of earnings management. Following prior literature, we focus on two different measures: the likelihood of just meeting or beating earnings expectations, and the use of discretionary accruals. Prior research finds that firms appear to opportunistically manage their earnings in order to just meet or beat analysts' forecasts in order to maximize their stock valuations (Hayn, 1995; Degeorge et al, 1999). We find less evidence of earnings management in the year following the revelation of a local political scandal using both measures. Firms are 13% less likely to report quarterly earnings that just meet or beat analysts' forecasts during scandal years (one percent sig.). They also use significantly fewer discretionary accruals when computing their reported earnings. These results are more persistent into the second year after a scandal is revealed than those for insider trading, but they also appear to decline to some degree, suggesting that individuals may revert to prior behaviors once ethical considerations have faded from memory.

Overall, our analyses provide evidence that the results found in the labs of Mazar et al. (2008) and Gino et al. (2009) carry over to the actions of corporate executives. This suggests that the individuals in control of our public companies can change their behavior, and that they appear to do so in response to certain stimuli. Even the suggestion that certain self-serving actions are unethical and illegal appears to cause some executives to choose more appropriate courses of action on behalf of themselves and the firms they run.

This work helps extend the literature on managerial misconduct. Much research aims to understand why executives misbehave. Seminal work by Becker (1968) proposed a theoretical model of rational crime where individuals are expected to commit illegal acts if the personal benefits of doing so outweigh the expected costs. Kedia et al. (2010) empirically examined contagion in corporate misconduct and argue that manager's assessment of the benefits and cost of cheating change when they observe others cheating. Parsons, Sulaeman and Titman (2014) find that financial misconduct by firms is clustered geographically.

Our work is also related to research that examines the deterrent effects of law enforcement activities. Several authors provide evidence that insider trading declines when countries begin enforcing their insider trading laws (Bhattacharya and Daouk, 2002; Bushman, Piotrioski, and Smith, 2005; DeFond, Hung and Trezevant, 2007; Fernandes and Ferreira, 2009). Recent work provides indirect evidence that informed insider trading declines when U.S. legal authorities allocate more resources to investigating insider trading (Del Guercio, Odders-White, and Ready, 2015). There is also evidence of spillover effects such that cases dealing with financial fraud or insider trading have a deterrent effect on the same type of behavior at other firms (Kedia and Rajgopal, 2011; Jennings, Kedia and Rajgopal, 2011; Cheng, Huang and Li, 2013). Kedia and Rajgopal (2011) also show that firms located closer to the SEC are less likely to restate earnings. In contrast to these other works, this paper considers how litigation for inappropriate acts in one context can have spillover effects in other contexts. The results suggest that when more attention is paid to politicians' inappropriate acts, the salience of dishonesty is increased and this has a positive effect on corporate executives.

II. Data and Methodology

II.a. *Methodologies*

Our main hypothesis predicts that insiders will engage in less negative behavior after being exposed to news of a local political scandal. To test this prediction, we employ a difference-in-difference methodology, comparing corporate insiders' actions in the years after a political scandal is revealed to their behavior in other years. We focus on the year following the first announcement of a scandal since the salience of illegal actions should be most acute during this period, although we also test whether any change in behavior is more permanent. Insiders at firms located in a scandal state during the years the scandal is revealed represent the treatment group while firms in the other scandal states during the same time-periods serve as a control group of observations, allowing for a well-specified difference-in-differences approach.

We examine two activities where corporate insiders may misbehave: insider trading and earnings manipulations. For the insider trading analysis, we evaluate the overall profitability of insiders' trades and the likelihood that insiders trade ahead of price swings. Similar to Daniel, Grinblatt, Titman, and Wermers (1997), we calculate abnormal returns as the excess of a firm's one month total return relative to the return on a portfolio of firms formed similar in size, market-to-book, and recent return momentum. Each month all U.S. firms in CRSP are categorized into 125 portfolios based on size and book-market quintiles using the Daniel et al. (1997) annual breakpoints, and quintiles of the rolling past 12-month returns. If the increased salience of dishonesty deters executives from trading on private information, we expect to find that their trades are less profitable during this period.

We next implement tests designed to identify whether insiders take advantage of profitable trading opportunities when they arise. To test for changes in behavior, we consider whether the

odds of trading prior to large price changes are lower following the public revelation of a political scandal. We first employ a logit model predicting the likelihood of trading as a function of the abnormal returns in the following month. We also conduct Negative Binomial regressions to determine whether the relationship between potential gains and the total number of shares traded by insiders is affected by the revelation of a political scandal.

To examine whether corporate executives behave more ethically on behalf of their firms when a local political scandal is revealed we test for whether there is less evidence of earnings management during this time period. Following prior literature, we evaluate two measures of earnings management: the likelihood of just meeting or beating earnings expectations, and the level of discretionary accruals in reported earnings. Executives have direct private incentives to meet or exceed the earnings expectations of analysts, since executive compensation is largely comprised of equity-based components and stock prices are sensitive to meeting analysts' forecasts (Murphy, 2003; Bartov, Givoly, and Hayn, 2002). Prior research finds that a disproportionately large number of firms just meet or beat analysts' forecasts (Hayn, 1995; Degeorge et al, 1999) and commonly interpret this as evidence that executives opportunistically manage earnings to attain these thresholds. We test whether firms are less likely to engage in this particular form of earnings management during the years when local political scandals are revealed by comparing firms' reported earnings to analysts' forecasts of earnings reported in the IBES unadjusted summary files (Kasznik and McNichols, 2002; McVay, Nagar, and Tang, 2006). To generate an expected earnings benchmark, we take the last analyst consensus mean or median earnings forecast prior to the earnings announcement.

One of the ways that firms can manage their earnings to meet analysts' expectations is by manipulating the discretionary component of their accruals (Bergstresser and Philippon, 2006; Jiang,

Petroni and Wang, 2010). We calculate quarterly discretionary accruals using the modified Jones (1991) model that includes an intercept term (the specifics of this methodology are discussed in Appendix A). We compare the use of discretionary accruals across scandal and non-scandal years by regressing the absolute value of level of discretionary accruals onto a variety of control variables and an indicator variable for whether the quarter fell in a scandal year.

We include several control variables in the earnings management tests. Following Summers and Sweeney (1998), we control for standard firm characteristics that could be related to the fraudulent misstatement of financial statements: *Size*, growth opportunities (*Market to Book*), *Leverage*, and profitability (*ROA).* We also control for channels of external monitoring, as previous literature has shown that outside monitoring affects earnings management. Yu (2008) finds that firms followed by more analysts manage their earning less. Institutional investors also play important roles in preventing suspect earnings manipulations (Shleifer and Vishny, 1986; McConnell and Servaes, 1990; Chung, Firth and Kim 2002). Therefore, we control for total shares of stock owned by institutional investors (Natural log of *Institutional Ownership*) and the number of analysts following the firm (*# Analyst Coverage*). Lastly, we control for extreme performance and cash flow volatility by including *Growth Rate of Assets* and *Cash flow volatility* (Dechow and Dichev 2002, Yu 2008).

II.b. *Data Sources*

We obtain a list of political scandals from Puglisi and Snyder (2008)'s paper "Media Coverage of Political Scandals." They collected data on high profile political scandals from 1994 to 2006 involving U.S. senators, congressmen, governors and high-ranking members of public administrations. These scandals involved various types of wrongful behaviors including bribery, money laundering and bank fraud. Each of the scandal was investigated by either

federal or state law enforcement agencies. They identify 35 scandals in 19 states. However, in order to have clearly identified shocks to executives' attention, it is important to exclude from the analysis states that had multiple scandals in a short period of time. We therefore focus our analysis on states that only had one scandal during the sample period. After applying this restriction, there are 10 scandals in 10 states over the time period 2001 to 2006. In the appendix tables, we conduct most of our analyses on an extended sample that includes all of the scandals from Puglisi and Snyder (2008), and confirm that the results are largely consistent.

-Table 1-

Table 1 provides a brief description of each of the scandals considered, including the position, state of origin, and political affiliation of the political figures who were implicated. Following Puglisi and Snyder (2008), we define the start date of a scandal as the first day of the month when it was revealed that an investigation was being conducted by a federal agency, a congressional ethics committee, or a state attorney general. A "Scandal Year" refers then to the 12-month period beginning with the start date of a scandal, and the other years are considered "Non-Scandal Years". In order to measure changes in insiders' trading behavior we include observations from three years prior to, and two years after, the scandal starting dates. As a result, the overall time period examined in this paper is from 1999 to 2008. Figure 1 presents a timeline of the begin dates for the scandals during this period.

-Figure 1-

The insider trading data comes from Thomson Reuters. Corporate insiders are required to report their transactions to the Securities and Exchange Commission (SEC) and their trading records are available to the public. These insiders include top executives who oversee day-to-

day operations of the firm, directors, and beneficial owners of 10% or more of a company's stock. Since we are interested in identifying evidence of illegal trading on private information, we only evaluate their open market stock purchases and sales.

To ensure that news about a political scandal will be salient, we only include insiders in the sample if they also live in the state where their firms are headquartered. Trades are aggregated at the firm-month, and trade months are classified as either sale or purchase months based on the aggregate change in insiders' positions. Following Biggerstaff, Cicero and Wintoki (2014), returns are evaluated following either an isolated trade month or the end of a sequence of trade months.[4] In particular, consecutive trading months are defined as a trade sequence, whereas a trading month with no trade occurring in the preceding or following month is considered an isolated trading month. For robustness, we confirm in Appendix Table A2 that our main results continue to hold when we do not control for trade sequences, but instead treat each trading month as an individual observations.

We obtain financial statement information and the addresses for firms' headquarters from Compustat, and return data from CRSP. In the final part of our analysis, we consider whether firms change their financial reporting practices in response to the revelation of a local political scandal. We use analysts' forecasts of expected earnings and actual reported earnings which are available in I/B/E/S for this analysis.

[4] Biggerstaff et al (2014) find that isolated trading months are followed by abnormal returns in the following months and that longer sequences of trading months are followed by abnormal returns in the month following sequence completion. They also show that although for sequences of trades the abnormal returns are not evident until the sequences end, there is also evidence of abnormal returns relative to when the sequences begin, although tests focusing on identifying abnormal returns following sequences and isolated trades are more powerful.

III. Empirical Results

III.A. Profitability of Insider Trades

We have hypothesized that corporate insiders will be less willing to trade on private information during the year a local political scandal comes to light publicly. In this section, we compare the abnormal returns following insiders' trades during the scandal years to those that are apparent in non-scandal years. Table 2 reports a univariate analysis of the one-month abnormal returns following insiders' trading months. On average, insider sales are followed by a -2.5% abnormal return in non-scandal years. In contrast, in the year following the announcements of local political scandals, insiders' sales are only followed by a -0.7% abnormal return (the difference is significant at the 1% level). We also contrast the returns following insiders' trades during scandal years to those at the same firms in the year after the scandal. In year *t+1* following scandals, insiders' sales are followed by abnormal returns of -1.40% which are significantly lower than those following trades in year *t* at the 5 percent level. These results indicate that insiders earn smaller abnormal returns in the year following the revelation of local political scandals, but that the contrast is less pronounced in the second year following the scandals.

A comparison of the abnormal returns following insiders' purchases tell a different story. Purchases are actually followed by negative abnormal returns during non-scandal years (-1.7% overall and -1.3% during year *t+1* following a scandal), but they are followed by positive abnormal returns of 0.6% during scandal years. These differences, which are significant at the 1 percent level, are not consistent with insiders being less likely to buy their

stock when they have private positive information when dishonesty is more salient. If anything, they suggest the opposite.[5]

-Table 2-

Our analysis continues with multivariate OLS regressions evaluating the returns following insiders' trade months in Table 3. To distinguish the treatment group for each scandal, we include the indicator variable *Scandal Year*, which equals one if the firm is headquartered in a scandal state (and the individual also resides in that state), and the transaction is in the year following the date the scandal is revealed. Since the sample period spans from 1999 to 2008 and includes firms in various industries, either a time trend or unobserved industry characteristics could impact the results. We therefore also include both industry and year fixed effects.

The multivariate regression results for insiders' sales are reported in Panel A. The regressions in Column 1 and 2 confirm the univariate results for the returns following insiders' sales, although the coefficient on *Scandal Year* goes down to about 65 to 75 basis points now that the tests include year and industry effects. The regression in Columns 2 also includes an additional dummy, *Scandal Year + 1*, that indicates trades executed in the same state as a scandal but in the second year following it's revelation. The coefficient on *Scandal Year + 1* is negative and insignificant, suggesting that any impact on executive behavior is limited to the year following the revelation of the local political scandal. In Columns 3 and 4 we conduct the regressions on just the sample of trades by top executives (CEO, CFO, COO, GC, President or

[5] Other authors have found that insiders' purchases are followed by positive abnormal returns, but this is not generally the case in our sample. In order to have confidence in our methodology, we apply it to purchase months by all insiders covered by Thomson during this timeperiod, and confirm that on average purchases are followed by positive monthly abnormal returns of 1.48%, consistent with prior literature.

Chairman of the Board), and find a stronger effect. The coefficient on *Scandal Year* is around 1.5% in these two regressions and each is significant at the 5% level. The larger magnitude of these results suggests that the saliency of dishonesty has the greatest impact on firms' top executives. The samples for the regressions in Columns 5 and 6 are limited to the trades of directors and officers other than the top executives, and the results are similar to those for the full sample.

Panel B presents a similar analysis of insiders' purchases. Once we control for year and industry effects, the coefficients on *Scandal Year* are insignificant in the regressions including the full sample of purchases (Columns 1 and 2) and those that evaluate the trades of directors and lower level officers (Columns 5 and 6). However, the coefficients on *Scandal Year* in the regressions evaluating stock sales by top executives are large and significant. It is 2.55% in the Column 3 regression, and 2.02% in Column 4. The coefficients on the variable *Scandal Year + 1* are insignificant across all specifications, again suggesting that any saliency effect is short-lived.

-Table 3-

Why might this be the case? A possible explanation may be related to insiders' incentives to diversify their portfolios if they perceive new limitations on their ability to trade profitably. Insiders maintain large, undiversified positions in their firms' stock and they also have large human capital investments in their firms. One factor in their willingness to maintain large equity stakes may be their ability to adjust that position downward when they receive information indicating they may face losses. If, once there is an increase in the saliency of dishonest behavior, they feel constrained in their willingness to sell shares when they have

negative information, then the costs of being undiversified will be higher, and they will have incentives to limit the size of their positions in their firms. We would therefore only expect them to purchase their stock when they are confident it will not decline in value, which could be identified empirically as less buying ahead of price declines.

There is also reason to believe that insiders may be less deterred from purchasing their shares based on private information when the salience of dishonesty is increased. Other researchers have argued both that there is greater litigation risk is associated with selling stock and it is easier to identify harm associated with insiders withholding bad news. If an insider withholds negative information and trades, other investors will have identifiable losses when they purchase at an inflated price and the stock subsequently declines in value. However, if an insider purchases stock on private information, the only harm is to the investors who sold their shares and therefore weren't able to enjoy the extra gains the would have realized if the stock had already reflected the positive information. Insider stock sales can also be used as evidence in a suit claiming fraudulent financial reporting. However, it is less likely that shareholders will bring a successful derivative lawsuit claiming insiders fraudulently withheld positive information because their losses are best described merely as opportunity costs (Skinner, 1994; Brochet, 2010; and Chen, Martin and Wang, 2012).

The next sets of tests we present focus on the relationship between the distribution of monthly returns and insiders' trading activity in prior months. This analysis could indicate whether during scandal years insiders are more likely to purchase shares ahead of price increases, or if they are merely more likely to avoid purchasing ahead of price declines as our reasoning above would predict. Following that, we test whether insiders change their level of

holdings in their firms following the revelation of a local political scandal. If they are more likely to reduce their holdings, this would further support our analysis above.

Regardless, it remains possible that an alternative explanation drives the similar abnormal return patterns that we find for both sales and purchases under these circumstances. This concern provides additional motivation for our analysis of earnings management practices. If those results are also in line with our expectations under the salience of dishonesty hypothesis, it will lend confidence to our interpretation of the insider trading results.

III.B. Likelihood of Trading

It is possible that the results from the prior section reflect differences in the monthly return distributions across years as opposed to changes in insiders' willingness to trade on private information. In this section, we present a supporting analysis that evaluates more directly whether insiders are less likely to trade when it would be profitable to do so.

We use logistic regressions to model the likelihood of trading in a given calendar month as a function of the abnormal return that is evident in the following month. Under the assumption that insiders have private information about the expected return distribution in the following month, we should find a greater likelihood of trading when the return in the following month is favorable to the trading strategy. This test should not be biased by changes in the distribution of returns across years because the prediction of trading is conditional on the magnitude of the returns to be realized.

Of course, there will be many monthly returns that insiders could not have predicted, and the prevalence of these realizations will introduce noise into any relationship between

returns that were predictable by insiders and their trading in the previous month, reducing the power of the test. It is also possible that the returns in months following trading actually reflect the market's response to insiders' trading signals. However, concerns over reverse causation are lessened because we implement this test in a difference-in-differences framework that contrasts the relation between trading and subsequent returns across scandal and non-scandal years.

All calendar firm-months during the full time period are included as observations in these regressions. We present Logit models where the dependent variable is a dummy variable *Trade*, which equals one if it is a trading month, and 0 otherwise. We also present Negative Binomial Models where the dependent variable is number of shares traded by firm insiders in the month. In order to determine whether insiders' behavior is different ahead of large price swings, we include the independent indicator variables *Positive 10% Month* and *Negative 10% Month*, which indicate that the return in the following month was above or below those thresholds. By interacting these variables with dummy variables indicating that the month was during a scandal year, we can determine whether insiders had a different propensity to trade ahead of these outcomes when dishonesty was more salient.

Table 4 reports these regressions, with the results for insiders' sales in Panel A, and those for purchases in Panel B. The insignificant coefficients on *Scandal Year*Positive 10% Return Months* in Columns 1, 2, 5, and 6 show no evidence of a change in insiders' selling activity ahead of large positive returns during scandal years. However, the consistently negative and significant coefficients on *Scandal Year*Negative 10% Return Months* in Columns 3, 4, 7, and 8 indicate less insider selling ahead of large price declines in scandal years. The coefficient of -0.289 in the Logit Regression in Column 3 indicates that insiders are

22% less likely to sell shares ahead of a -10% or greater monthly return that occurs during a scandal year (a reduction from selling ahead of 7.8% of large price declines to 6.1%). And the coefficient of -0.799 in the Negative Binomial Regression in Column 7 indicates that insiders sell 55% fewer shares ahead of large price declines in scandal years (an average of 948 shares relative to 2113 shares sold in non-scandal year months).

-Table 4-

These regressions also include interactions of *Scandal Year+1* with the large return variables. The coefficients indicate that in the second year after a scandal breaks local insiders are actually less likely to sell shares ahead of a large positive return, but the lower level of selling activity ahead of price declines is no longer apparent. These results are also consistent with those from the returns to trading analysis presented above suggesting that any shift away from bad behaviors during the scandal years do not persistent.

Panel B presents similar regressions evaluating insiders' purchases, and the patterns identified confirm our expectations discussed above. The insignificant coefficients on *Scandal Year*Positive 10%* Months across all specifications indicates that insiders are not purchasing more shares ahead of large positive returns. In contrast, the positive and significant coefficients on *Scandal Year*Negative 10% Months* in the Logit Regressions (Columns 3 and 4) indicate a decrease in purchasing activity ahead of large price declines in scandal years. In particular, the coefficient of -0.380 in Column 3 indicates a 27% reduction in the odds that insiders buy their stock ahead of a large price decline in a scandal year (the odds of a purchase go from 6.6% down to 4.8%). The coefficients of interest from the Negative Binomial Regressions

considering the number of shares purchased are also in the expected direction, but they are insignificant.

We next consider whether insiders adjust their ownership of their firms' stock following the revelation of a local political scandal. As discussed above, we expect that insiders will diversify away from their companies if they feel restricted in their ability to trade their stock profitably. Table 5 presents OLS regressions explaining of the natural log of the number of shares insiders hold (or the dollar value held) that include year and individual fixed effects. The observations for this test include the calendar year-end holdings for each insider based on their trading records and stock grants reported in the Thomson data. If insiders are diversifying, we expect their holdings following the scandal year to be lower than in other years[6] The variable of interest is therefore the indicator *Following Scandal Year*, which takes the value 1 if the observation corresponds to the calendar year end that follows the scandal year. The coefficient on this variable is negative and significant in both regressions. The coefficient of -0.0315 in the first regression indicates that insiders reduce the dollar value of their holdings by 3.1% during the year. Similarly, the coefficient of -0.0319 in the second specification indicates a 3.13% reduction in the number of shares they hold.

-Table 5-

[6] The level of holdings are established using the values reported by insiders on Forms 4 when they are granted shares or trade. The levels are measured as of calendar year ends due to the great amount of variation in the timing of trading across months during a year.

III.C. *Media Coverage of Scandals and Insider Trading Behavior*

The evidence thus far supports a conclusion that corporate insiders are less likely to sell stock based on private information when the saliency of dishonest behavior is higher. In this section, we evaluate whether insiders' trading patterns and returns during scandal years vary as a function of the level of media coverage of the scandals. This analysis can possibly help rule out the alternative explanation that local insiders are merely responding to an increase (either actual or perceived) in the odds of being caught for white collar crime during these time periods because of higher levels of attention by law enforcement.

We begin by counting the number of local newspaper articles referencing the scandals in each state. For example, Figure 2 plots the number of articles in Texas newspapers that reference the scandal involving Tom Delay in each of the twelve months beginning in April, 2005. As would be expected, a large number of articles reference the scandal when Delay's ties to lobbyist Jack Abramoff is first reported in April (about 400 articles), but the coverage in other months ranges from around 150 to over 500 articles on the topic. The highest volumes of articles came when he was indicted in October, 2005, and when he plead guilty in January, 2006.

-Figure 2-

Table 6 presents regressions evaluating whether local insiders behave differently when there is more news coverage of a political scandal. Panel A reports Logit regressions explaining the likelihood that insiders trade in months during scandal years as a function of the level of media coverage, and Panel B reports OLS regressions of the relationship between media coverage and the returns to insiders' trades. The logit regressions include as

observations each firm month (12) for each local company in the sample in the scandal year, and the dummy variable *High Coverage* indicates if insiders at that firm traded in that month. The Panel A regressions indicate that insiders are less likely to sell shares in months with above median media coverage of the local scandal. For example, the coefficient on *High Coverage* in column (1) indicates that insiders are 18% less likely to sell shares in a scandal year month with above median media coverage of the scandal. However, the result is insignificant with respect to stock purchases.

-Table 6 -

The return regressions in Panel B also include as observations each local firm month in the scandal years.[7] Abnormal returns are regressed onto a dummy *Trade* that indicates that insiders traded in that month, *High Coverage*, and the interaction term *Trade*High Coverage*. The regressions indicate that when insiders do trade in high media coverage months, the trades are not as profitable. This can be determined by the significant coefficients on the interaction term *Trade*High Coverage*, which is positive and significant in the regressions explaining the returns to insiders' sales, and negative and significant in the regressions for purchases. The returns following sales (purchases) are about 120 bps higher (200 bps lower) when media coverage is high. The negative and significant coefficient on *High Coverage* in the regressions analyzing insiders' sales also confirms that these months are generally bad for local firms, and confirm the importance of controlling for this condition separately.

[7] For the regressions explaining the returns following sales (purchases), firms are only included in the regressions if at least one of the months could be classified as a sale (purchase) month. This explains the different number of observations in these regressions.

The results discussed in this section indicate that corporate insiders at local firms are less likely to sell their stock in months when media coverage of scandal-related events is elevated. The evidence that earn smaller abnormal returns when they do trade their stock in months with high media coverage suggests that they are less likely to be trading on private information. These results help to rule out alternative explanations for the main results above based on expectations of elevated law enforcement because this type of activity would not likely vary from month to month within the scandal years. These results provide stronger evidence in favor of the salience of dishonesty hypothesis.

III.D. *Insider Trading Results Using an Extended Sample*

To conduct clean difference-in-differences tests, our main analyses focus on insiders' behavior around the ten political scandals that were the only ones identified in their states. We also conduct all of our analyses on a broader sample that considers changes in behavior around any scandal identified by Puglisi and Snyder (2008) so long as there wasn't another local scandal that came to light within two years before or after. The resulting sample includes 23 scandals across 18 states. Appendix Table A1 lists the full set of scandals identified by Puglisi and Snyder (2008) and those that were used in each of our analyses. We conduct our returns and likelihood of trading analyses using this broader sample and report the results in Appendix Tables A2 and A3. For both purchases and sales, we find similar results when evaluating the returns following trading in Table A2, although the level of statistical significance is lower. When evaluating the likelihood of trading in Table A3, we find that during scandal years insiders are less likely to sell shares ahead of both large price declines and increases. When evaluating the likelihood of purchases, the results are generally insignificant in this extended sample.

III.E. Earnings Management

Up to this point, we have shown that suspect insider trading behavior declines after the revelation of a local political scandal. Those results suggest that insiders modify their personal behavior in response to an increase in the saliency of dishonest behavior. In this section, we extend the analysis to consider whether corporate executives also change the way they act on behalf of their firms under similar circumstances. Our focus is on earnings management, which, as discussed in the introduction, is one of the more egregious ways that managers may mislead investors about firm performance and value.

We begin this section with a difference-in-differences analysis of firms' earnings surprises. As demonstrated by prior research, firms appear to manage their earnings in order to just meet or beat analysts' forecasts in order to either keep investors from pushing their stock price downward (if they manage earnings up to the threshold) or to reserve slack that can be used to attain thresholds in the future (when they manage earnings down to the threshold). Figure 3 plots the distribution of local firms' quarterly earnings surprises in scandal years versus other years. A strikingly smaller fraction of surprises in the scandal years either just meet analysts' expectations or exceed expectations by 1 penny. It is also evident that firms report more earnings that either miss expectations by up to 5 cents or exceed expectations by 3 or more cents. These patterns are consistent with prior research showing that firms manage their earnings either up or down to narrowly attain analysts' expected earnings.

In Panel A of Table 7 we evaluate the statistical significance of these patterns. In years when scandals are revealed, local firms just meet or beat the median earnings forecast 41.4% of the time compared to 35.9% of the time in other years (an economically-significant 15% change), and the difference is statistically significant at the 1 percent level. Similar results

obtain when considering surprises relative to the mean of analysts' forecast. The reversed pattern for earnings falling just outside of this window during scandal years is also significant. During scandal years, firms report earnings that either miss analysts' expectations by 1 to 5 cents or beat expectations by 2 to 6 cents approximately 5 percent more often (one percent significance). These results suggest that firms are less likely to manage earnings into the narrow range of just meeting or beating analysts' expectations when wrongdoing and its consequences are more salient.

-Figure 3-

We also compare firms' likelihood of just meeting or beating analysts' forecasts in scandal years to the likelihood in the following year. They are actually slightly less likely to report earnings in this narrow range in the second year after a local scandal is revealed, but the difference is not statistically significant.

A multivariate logit regression analysis of earnings surprises is presented in Table 8. We predict whether firms report earnings that meet or just beat earnings forecasts after controlling for common determinants of earnings surprises identified by prior researchers and discussed in Section 2. We also present specifications that include either year and industry fixed effects, or individual quarter and industry effects. The results are largely consistent with the univariate analysis of earnings surprises. The coefficients on *Scandal Year* are consistently negative and significant across all specifications, indicating that local firms are less likely to report earnings that fall into this narrow range in the year when a political scandal is first publicized. There is also some evidence that this effect holds in the year following the scandal year, as evidenced by the negative and significant coefficient on *Scandal Year + 1* in specifications that do not include

fixed effects; however, this result does not obtain when industry and year fixed effects are present.

The greater dispersion of earnings surprises in scandal years suggests that firms are managing their reported earnings towards targets less aggressively during these years. To evaluate this proposition further, we analyze firms' use of discretionary accruals to generate their reported earnings, a practice that prior researchers have argued may implicate inappropriate earnings manipulations.

-Tables 7 and 8-

We report OLS regressions evaluating firms' use of discretionary accruals in Table 9. The independent variable of interest is again *Scandal Year*. The regressions also include common determinants found to be related to the use of discretionary accruals along with different combinations of time, industry and firm fixed effects. The coefficients on *Scandal Year* are statistically significant with the exception of the regression in Column (3) which includes year and industry fixed effects. However, it remains significant when including either industry and quarter effects (Column 4), year and firm effects (Column 5), or firm and quarter effects (Column 6). The coefficient on *Scandal Year* in the regressions where it is significant range from -0.0665 when controlling for year and firm effects to -0.363 when only including the control variables but no fixed effects. This analysis provides substantial evidence that firms are less likely to manipulate earnings by using greater discretionary accruals in the year following public revelation of a local political scandal.

The regressions in Table 9 also include the dummy variable *Scandal Year+1*. The coefficient on this variable is consistently smaller than the coefficient on *Scandal Year*, and it is insignificant in all but one of the regressions controlling for fixed effects (the exception is

the regression in Column (4) that includes industry and quarter effects). This suggests that although firms may engage in significantly less earnings management in the year after a local political scandal breaks, this activity seems to pick back up in the following year.

III.F. *Earnings Management Results Using Extended Sample*

We also conduct our earnings management analysis using the extended sample of political scandals discussed in Section III.D. above. The results are reported in Appendix Tables A4 and A5, and the results are very similar to those that hold in the more restricted sample.

-Table 9-

IV. Conclusion

We present evidence that corporate insiders react to the public revelation of the unethical behaviors of others by acting more honestly themselves. We show that the saliency effect proposed and supported by Mazar, Amir, and Ariely (2008) and Gino, Ayal, and Ariely (2009) in their experimental work appears to hold in the real world as well. In particular, corporate insiders appear to execute fewer informed stock sales and to engage in less earnings management on behalf of their firms after the revelation of a local political scandal. However, the salience of dishonesty appears to have only a temporary effect on insiders, as the evidence of informed stock sales and earnings management pick back up in the second year after a political scandal has been revealed.

This paper furthers our understanding of the reasons why individuals engage in illegal or antisocial behaviors. It sheds light on whether and how the actions taken by business

professionals reflect the extent to which their attention is drawn to societal rules about the appropriateness of behavior and the consequences to engaging in illegal actions. It may also provide guidance on how to develop regulatory or legal regimes that more effectively deter unwanted behaviors in the business community. For example, it suggests that it be reasonable to use taxpayer funds to advertise public service announcements in city centers and around corporate headquarters that remind the public about acts that are illegal or inappropriate. This tactic -- or other similar alternatives -- may in fact serve as low-cost means of deterring unwanted and costly behaviors, and, in turn, reduce the cost of investigating and prosecuting such actions after they occur.

Appendix A

We estimate discretionary accruals at the firm-quarter level using a modified version of the Jones (1991) model with an intercept term. This model employs a regression of total accruals on changes in sales and property, plant, and equipment to obtain the estimated values.

First, we calculate total accruals ($TA_{i,t}$) as:

$$TA_{i,t} = \left(\Delta CA_{i,t} - \Delta CL_{i,t} - \Delta Cash_{i,t} + \Delta STD_{i,t} - Dep_{i,t}\right) \quad \text{(A1)}$$

Where *i* and *t* index the firm and quarter respectively, $\Delta CA_{i,t}$ is the change in current assets, $\Delta CL_{i,t}$ is the change in current liabilities, $\Delta Cash_{i,t}$ is the change in cash and cash equivalents, $\Delta STD_{i,t}$ is the change in debt included in current liabilities, and $Dep_{i,t}$ is the depreciation and amortization expense. Next, we run the following cross-sectional OLS regressions on subsets of firms formed by two-digit SIC codes.

$$\frac{TA_{i,t}}{Assets_{i,t-1}} = \widehat{\alpha_0} + \widehat{\alpha_1}\frac{1}{Assets_{i,t-1}} + \widehat{\alpha_2}\frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \widehat{\alpha_3}\frac{Net\ PPE_{i,t}}{Assets_{i,t-1}} + \varepsilon_{i,t} \quad \text{(A2)}$$

Where, $Assets_{i,t-1}$ are total assets, $\Delta Sales_{i,t}$ is the change in sales, $\Delta AR_{i,t}$ is the change in receivables, and $Net\ PPE_{i,t}$ is the property, plant, and equipment.

The coefficient estimates $\widehat{\alpha_0}, \widehat{\alpha_1}, \widehat{\alpha_2}, \widehat{\alpha_3}$ from A2 are then used to calculate non-discretionary accruals (NDA) as follows:

$$NDA_{i,t} = \widehat{\alpha_0} + \widehat{\alpha_1}\frac{1}{Assets_{i,t-1}} + \widehat{\alpha_2}\frac{\Delta Sales_{i,t} - \Delta AR_{i,t}}{Assets_{i,t-1}} + \widehat{\alpha_3}\frac{Net\ PPE_{i,t}}{Assets_{i,t-1}} \quad \text{(A3)}$$

We then derive our measure of discretionary accrual as the difference between the predicted non-discretionary accruals and the total actual accruals as follows:

$$DA_{i,t} = \frac{TA_{i,t}}{Assets_{i,t-1}} - NDA_{i,t}$$

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Table 1. Summary of Political Scandals

This table lists the political scandals evaluated in the paper. It includes the position, state of origin, and political affiliation of the persons involved. A brief description of each scandal is also included. The start date for a scandal year is the first day of the month during which an investigation by a federal agency, a congressional ethics committee, or a state attorney general is announced.

Start Date	Name	State	Party	Position	Scandal
10/1/2005	Don Siegelman	Alabama	D	Governor	Racketeering and extortion dealing with HealthSouth and doctor's boards
11/1/2003	John Rowland	Connecticut	R	Governor	Corruption and fraud stemming from work done on his weekend cottage, as well as dealings on a home in Washington
3/1/2001	Ed Mezvinsky	Iowa	D	House	Bank fraud, mail fraud, wire fraud
5/1/2005	Ernie Fletcher	Kentucky	R	Governor	Merit system related corruption
3/1/2005	Conrad Burns	Montana	R	Senate	Pay for play, accepting funds from Abramoff in turn for allocating money for Michigan Indian tribe
11/1/2006	Jim Gibbons	Nevada	R	House/Governor	Bribery (Gifts given for votes on Armed Services and Intelligence Committee)
11/1/2003	Frank Ballance	North Carolina	D	House	Money Laundering and mail fraud
4/1/2005	Tom DeLay	Texas	R	House	Illegal corporate donations through TRMPAC as part of redistricting plan, money laundering, aides and personal connections to Jack Abramoff investigation
12/1/2004	Jim McDermott	Washington	D	House	Eavesdropping on Gingrich/Boehner conversation
2/1/2006	Alan Mollohan	West Virginia	D	House	Misrepresentation of private assets, earmarking funds to an aide

Table 2. Univariate Comparison of Insider Trading Profits

This table reports average DGTW excess returns following insider trading months in scandal years versus other years in our sample. *Scandal Year* is defined as the twelve calendar month period beginning with the month an investigation is first announced. *Scandal Year +1* is the second year after the starting date of a scandal. Robust standard errors are presented in parentheses, and ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

	Sales	**Purchases**	**#Obs**	
	DGTW Excess Ret	**DGTW Excess Ret**	**Sales**	**Purchases**
Scandal Year	-0.007**	0.005	935	472
	(0.023)	(0.238)		
Scandal Year +1	-0.014***	-0.013***	944	493
	(0.000)	(0.007)		
All Other Years	-0.025***	-0.017***	7573	6240
	(0.000)	(0.000)		
Differences				
Scandal Year+1 - Scandal Year	-0.006**	-0.018***		
	(0.060)	(0.006)		
All Other Years - Scandal Year	-0.019***	-0.022***		
	(0.000)	(0.004)		

Table 3. Regression Analysis of Insider Trading Profits

This table reports OLS regressions of monthly abnormal returns following insider trading months in scandal and non-scandal years. The dependent variable is the DGTW excess return, which is the difference between firm monthly return and the return of a characteristic-based benchmarking portfolio. The key explanatory variable is a dummy variable *Scandal Year* that takes one if (i) the firm is headquartered in one of the scandal state, and (2) the transaction date of the trade is within one-year from the start date of a scandal. *Scandal Year+1* is a dummy variable equals one if the transaction is made within the second year of a scandal. Standard errors are clustered at the firm level. Robust standard errors are presented in parentheses. ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

Panel A: Sell						
	All Insiders		**Top Executives**		**Directors & Officers**	
	(1)	(2)	(3)	(4)	(5)	(6)
Scandal Year	0.00764**	0.00654*	0.0162**	0.0144**	0.00773**	0.00667*
	(0.00366)	(0.0039)	(0.00651)	(0.00701)	(0.00377)	(0.00400)
Scandal Year + 1		-0.00324		-0.00547		-0.00310
		(0.00369)		(0.00602)		(0.00373)
Observations	8,658	8,658	3,077	3,077	8,440	8,440
R-squared	0.05939	0.05944	0.05163	0.0882	0.05918	0.05922
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Purchase						
	All Insiders		**Top Executives**		**Directors & Officers**	
	(1)	(2)	(3)	(4)	(5)	(6)
Scandal Year	0.0083	0.00589	0.0255**	0.0202*	0.00844	0.00641
	(0.00543)	(0.00552)	(0.0105)	(0.0108)	(0.00547)	(0.00556)
Scandal Year + 1		-0.00772		-0.0154		-0.00649
		(0.00571)		(0.00953)		(0.00577)
Observations	6,581	6,581	2,728	2,728	6,525	6,525
R-squared	0.0224	0.02252	0.03163	0.03199	0.02193	0.02201
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 4. Predicting Trading Activity

This table presents logit models predicting the likelihood of insider trading and Negative Binomial Models explaining the number of shares traded by insiders in a trading month. All calendar months in our sample period are included for each firm. Months in which no trades were made are recorded as zero shares traded. The dependent variable is a dummy that equals 1 if firm insiders traded in that month, 0 otherwise *Positive 10% Months* is a dummy which equals 1 if the DGTW excess return in the following month is more than 10%. *Negative 10% Months* is a dummy which equals 1 if the DGTW excess return in the following month is less than -10%. *Scandal Year* Positive (Negative) 10% Months* is the interaction term between *Positive (Negative) 10% Months* dummy and *Scandal Year* dummy. *Scandal Year+1* Positive (Negative) 10% Months* is the interaction term between *Positive (Negative) 10% Months* dummy and *Scandal Year* +1 dummy. Other variables are defined in Table 3. Robust standard errors are presented in parentheses, and ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively. Standard errors are clustered at the firm level. Year and industry fixed effects are included in all regressions.

Panel A: Sell

	Likelihood of Trading (Logit Model)				**Number of Share Traded (Negative Binomial Model)**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Scandal Year	0.00357	0.000336	0.0284	0.0249	-0.0738	-0.137*	-0.00239	-0.0637
	(0.0452)	(0.0499)	(0.0466)	(0.0511)	(0.0697)	(0.0760)	(0.0705)	(0.0767)
Scandal Year +1		0.00273		-0.00383		-0.158*		-0.181**
		(0.0535)		(0.0551)		(0.0824)		(0.0863)
Positive 10% Return Months	-0.303***	-0.281***			-0.147**	-0.113*		
	(0.0399)	(0.0413)			(0.0664)	(0.0683)		
Scandal Year* Positive 10% Months	-0.111	-0.133			-0.0846	-0.112		
	(0.133)	(0.134)			(0.185)	(0.188)		
Scandal Year +1* Positive 10% Months		-0.255*				-0.634**		
		(0.153)				(0.258)		
Negative 10% Months			-0.180***	-0.174***			-0.110*	-0.106*
			(0.0381)	(0.0394)			(0.0564)	(0.0582)
Scandal Year* Negative 10% Months			-0.289**	-0.296**			-0.799***	-0.792***
			(0.124)	(0.125)			(0.172)	(0.174)
Scandal Year+1*Negative 10% Months				-0.0801				-0.0346
				(0.112)				(0.181)
Constant	-5.094***	-5.097***	-5.079***	-5.081***	3.547***	3.511***	3.411***	3.406***
	(0.0779)	(0.0779)	(0.0801)	(0.0800)	(0.160)	(0.160)	(0.145)	(0.144)
Observations	96,596	96,596	96,596	96,596	96,694	96,694	96,694	96,694

Panel B: Purchase

	Likelihood of Trading (Logit Model)				Number of Share Traded (Negative Binomial Model)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Scandal Year	-0.00291	-0.00369	0.0302	0.0259	-0.131	-0.171	-0.0808	-0.129
	(0.0616)	(0.0684)	(0.0606)	(0.0676)	(0.113)	(0.123)	(0.108)	(0.119)
Scandal Year +1		0.0311		-0.00418		-0.0958		-0.154
		(0.0704)		(0.0705)		(0.110)		(0.109)
Positive 10% Months	0.224***	0.252***			0.406***	0.442***		
	(0.0380)	(0.0389)			(0.0527)	(0.0536)		
Scandal Year* Positive 10% Months	-0.0720	-0.0997			0.155	0.128		
	(0.150)	(0.150)			(0.207)	(0.208)		
Scandal Year +1* Positive 10% Months		-0.534***				-0.600**		
		(0.185)				(0.268)		
Negative 10% Months			-0.0291	-0.0229			-0.00809	-0.00998
			(0.0374)	(0.0386)			(0.0529)	(0.0547)
Scandal Year* Negative 10% Months			-0.380**	-0.386**			-0.0674	-0.0641
			(0.155)	(0.155)			(0.228)	(0.229)
Scandal Year+1*Negative 10% Months				-0.117				0.00735
				(0.156)				(0.247)
Constant	-4.506***	-4.512***	-4.465***	-4.467***	-2.165*	-2.165*	-2.162*	-2.162*
	(1.316)	(1.316)	(1.316)	(1.316)	(1.299)	(1.299)	(1.296)	(1.295)
Observations	96,596	96,596	96,596	96,596	96,694	96,694	96,694	96,694

Table 5: Changes in Insiders' Stock Holding following Local Political Scandals

This table report fixed effects regressions explaining annual changes in insider's overall stock holding following local political scandals. The dependent variables are the natural log of the level of insiders' holdings in dollar value or number of share held. These variable are measured as of the end of each calendar year that the insider is in the sample. The variable of interest is an indicator *Following Scandal Year*, which takes the value 1 if the observation corresponds to the calendar year end that follows the end of the scandal year. Robust standard errors are presented in parentheses, and ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

	Holdings ($ value)	**Holdings (# shares)**
Following Scandal Year	-0.0315**	-0.0319***
	-0.0132	-0.0112
Constant		
Observations	47,127	51,046
R-squared	0.26742	0.14324
Year FE	Yes	Yes
Person FE	Yes	Yes

Table 6. Local Newspaper Coverage and Insider Trades

Panel A of this table reports logit regressions which predicting the insider trading as a function of local newspaper coverage of political scandals. The observations come from the scandal years only. Panel A presents logit models where the dependent variable is an indicator that insiders traded in a given month. The dependent variable in Panel B is the one-month DGTW excess return. *High Coverage* is a dummy that equals one if the number of article in that month is above the median. *Trade*High Coverage* is an interaction term between *Trade* and *High Coverage* Dummies. Standard errors are clustered at the firm level. Robust standard errors are presented in parentheses. ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

Panel A: Logit regs predicting trading	**Sell**		**Purchase**	
High Coverage	-0.188***	-0.179***	0.0629	0.0520
	(0.0572)	(0.0597)	(0.0712)	(0.0756)
Return	0.399	0.293	1.095***	1.247***
	(0.252)	(0.266)	(0.313)	(0.379)
Constant	-1.788***		-2.597***	
	(0.0668)		(0.0850)	
Observations	8,970	8,970	8,869	8,869
Year FE		Yes		Yes
Industry FE		Yes		Yes
Panel B: OLS regs predicting returns	**Sell**		**Purchase**	
Trade	-0.0114**	-0.0130***	0.0282***	0.0250***
	(0.00441)	(0.00451)	(0.00699)	(0.00687)
High Coverage	-0.00998***	-0.00990***	-0.00175	-0.00276
	(0.00345)	(0.00354)	(0.00431)	(0.00430)
Trade*High Coverage	0.0117*	0.0129**	-0.0207*	-0.0198*
	(0.00642)	(0.00656)	(0.0106)	(0.0102)
Constant	0.00451*		-0.0123***	
	(0.00245)		(0.00325)	
Observations	4,579	4,579	2,951	2,951
R-squared	0.00275	0.02433	0.00625	0.05887
Year FE		Yes		Yes
Industry FE		Yes		Yes

Table 7. Earnings Surprises and Discretionary Accruals

This table reports quarterly earnings surprises and discretionary accruals for sample firms headquartered in scandal states. We obtain earnings announcements and analysts' forecasts from the IBES detailed files. We take the last analyst consensus mean or median earnings forecast to benchmark earnings expectations. Our measure of earnings surprise is the actual earnings announced minus the mean or median analyst forecast from IBES. Discretionary accruals are calculated following Jones (1991) model modified to include an intercept. We evaluate the absolute value of discretionary accruals in each quarter. Robust standard errors are presented in parentheses. ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

Panel A: Earnings Surprises

	Meet/Beat (0 to 2 ¢)		**(-5 ¢ to -1 ¢ or +2 ¢ to +6 ¢)**		
	vs. Median forecast	vs. Mean forecast	vs. Median forecast	vs. Mean forecast	#obs
Scandal Years vs. All Other Years:					
Scandal Year	0.359	0.319	0.478	0.492	1,837
Other Years	0.414	0.376	0.431	0.435	15,847
Difference	-0.055***	-0.057***	0.048***	0.057***	
	(0.000)	(0.000)	(0.000)	(0.000)	
Scandal Years vs. Scandal Year+1:					
Scandal Year	0.359	0.319	0.478	0.492	1,837
Scandal Year +1	0.338	0.306	0.436	0.435	1,923
Difference	0.021	0.013	0.042***	0.057***	
	(0.183)	(0.382)	(0.009)	(0.000)	

Panel B: Discretionary Accruals

Scandal Years vs. All Other Years:		***Scandal Years vs. Scandal Year+1:***		#obs
Scandal Year	0.086	Scandal Year	0.006	1,207
Other Years	0.416	Scandal Year +1	0.032	1,293
Difference	-0.330*	*Difference*	0.047	
	(0.055)		(0.165)	

Table 8. Regression Analysis of Earnings Surprises

This Table presents logit regressions of *BEAT* (an indicator that a firm reported a quarterly earnings surprise of 0¢, 1¢ or 2¢) onto independent variables that control for firm characteristics. The sample of observation includes earnings announcements from 1999 to 2008. Earnings surprises are calculated as the difference between firm actual earnings and analysts' consensus median forecast. *Scandal Year* is an indicator variable equals one if observations are in the first four quarters (0, 1, 2, and 3) after revelation of a political scandal. *Scandal Year+1* equals one for the second four quarters (4, 5, 6, 7) after a scandal is first publicized. *Size* is the nature log of market cap. *Market to Book* is the natural log of market to book ratio. *ROA* is the return on assets. *# of analyst coverage* is the number of analyst covering the firm. *Institutional Ownership* is the natural log of total share of stock owned by institutional investor. *Leverage* is calculated as total liabilities scaled by total assets. *Growth rate of assets* is calculated by the change of assets scaled by lagged assets. *Cash flow volatility* is estimated by standard deviations of cash flow of a firm in the entire sample period, scaled by lagged assets. All non-binary independent variables are lagged one year. Robust standard errors in parentheses***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

	Relative to Mean Forecast				Relative to Median Forecast			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Scandal Year	-0.292***	-0.378***	-0.177***	-0.178**	-0.275***	-0.368***	-0.112*	-0.114*
	(0.0542)	(0.0558)	(0.0664)	(0.0718)	(0.0530)	(0.0543)	(0.0634)	(0.0675)
Scandal Year+1	-0.353***	-0.441***	-0.0172	-0.0254	-0.366***	-0.465***	-0.0314	-0.0511
	(0.0582)	(0.0612)	(0.0662)	(0.0719)	(0.0578)	(0.0608)	(0.0654)	(0.0711)
Size		-0.0370	-0.130***	-0.128***		-0.0499	-0.143***	-0.142***
		(0.0458)	(0.0489)	(0.0492)		(0.0439)	(0.0472)	(0.0475)
Market to Book		0.247***	0.347***	0.349***		0.238***	0.339***	0.340***
		(0.0481)	(0.0529)	(0.0532)		(0.0467)	(0.0510)	(0.0513)
ROA		7.333***	8.165***	8.237***		6.838***	7.706***	7.760***
		(0.734)	(0.829)	(0.831)		(0.699)	(0.767)	(0.770)
# Analyst Coverage		-0.0162**	-0.0192**	-0.0196***		-0.00483	-0.00538	-0.00572
		(0.00720)	(0.00745)	(0.00748)		(0.00698)	(0.00712)	(0.00715)
Institutional Ownership		0.115***	0.304***	0.304***		0.130***	0.314***	0.315***
		(0.0369)	(0.0416)	(0.0418)		(0.0359)	(0.0393)	(0.0395)
Leverage		0.0550	-0.637***	-0.643***		-0.0183	-0.647***	-0.654***
		(0.159)	(0.171)	(0.172)		(0.151)	(0.160)	(0.161)
Growth Rate of Assets		-0.681***	-0.715***	-0.712***		-0.595***	-0.622***	-0.618***
		(0.122)	(0.131)	(0.131)		(0.117)	(0.125)	(0.125)
Cash flow volatility		0.957***	1.257***	1.259***		0.765**	1.039***	1.037***
		(0.321)	(0.360)	(0.360)		(0.313)	(0.349)	(0.348)
Constant	-0.467***	-2.353***			-0.306***	-2.354***		
	(0.0407)	(0.460)			(0.0389)	(0.451)		
Observations	17,684	17,684	17,684	17,684	17,684	17,684	17,684	17,684
Year FE			Yes				Yes	
Industry FE			Yes	Yes			Yes	Yes
Quarter FE				Yes				Yes

Table 9. Regression Analysis of Discretionary Accruals

This Table presents OLS regressions of discretionary accruals onto independent variables that control for firm characteristics. The sample of observation includes earnings announcements for firm quarters from 1999 to 2008. Discretionary accruals are calculated following Jones (1991) modified model that includes an intercept. *Scandal Year* is an indicator variable equals one if observations are in the first four quarters (0,1,2,3) after a political scandal is first reported. *Scandal Year+1* equals one for the second four quarters (0,1,2,3) after a scandal is first publicized. *Size* is the nature log of market cap. *Market to Book* is the natural log of market to book ratio. *ROA* is the return on assets. *# of analyst coverage* is the number of analyst covering the firm. *Institutional Ownership* is the natural log of total share of stock owned by institutional investor. *Leverage* is calculated as total liabilities scaled by total assets. *Growth rate of assets* is calculated by the change of assets scaled by lagged assets. *Cash flow volatility* is estimated by standard deviations of cash flow of a firm in the entire sample period, scaled by lagged assets. All non-binary independent variables are lagged one year. Robust standard errors in parentheses***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

Dependent Variable : Absolute Value of Discretionary Accruals

	(1)	(2)	(3)	(4)	(5)	(6)
Scandal Year	-0.363***	-0.360***	-0.0423	-0.304***	-0.0665*	-0.216***
	(0.0674)	(0.0740)	(0.0293)	(0.0693)	(0.0390)	(0.0708)
Scandal Year+1	-0.329***	-0.294***	-0.0106	-0.228***	-0.0121	-0.0639
	(0.0792)	(0.0824)	(0.0394)	(0.0761)	(0.0431)	(0.0609)
Size		0.165	0.183	0.214*	0.0245	-0.0829
		(0.102)	(0.118)	(0.127)	(0.316)	(0.310)
Market to Book		0.269***	0.179*	0.161*	0.293	0.474*
		(0.0811)	(0.0933)	(0.0950)	(0.245)	(0.251)
ROA		-5.701**	-5.442**	-6.086**	2.371	1.335
		(2.475)	(2.700)	(2.759)	(2.008)	(1.992)
Leverage		-0.881***	-0.716**	-0.492	-0.550	-0.936
		(0.276)	(0.319)	(0.312)	(0.841)	(0.828)
Growth Rate of Assets		0.865	0.701	0.740	0.206	0.333
		(0.611)	(0.604)	(0.606)	(0.588)	(0.578)
Cash flow volatility		-0.265	-0.386	0.156	-1.046	-0.857
		(0.536)	(0.586)	(0.527)	(2.007)	(1.973)
# Analyst Coverage		0.00203	0.00264	0.00918	-0.0269	-0.0169
		(0.0118)	(0.0132)	(0.0133)	(0.0209)	(0.0191)
Institutional Ownership		-0.293***	-0.266***	-0.366***	-0.288	-0.394
		(0.0972)	(0.0994)	(0.117)	(0.270)	(0.278)
Constant	0.440***	4.520***				
	(0.0679)	(1.301)				
Observations	9,376	9,376	9,376	9,376	9,376	9,376
R-squared	0.00068	0.01026	0.04004	0.02937	0.00991	0.00537
Year FE			yes		yes	
Industry FE			yes	yes		
Firm FE					yes	yes
Quarter FE				yes		yes

Figure 1. Timeline of Political Scandals

This figure plots a timeline of the months when the political scandals used in this paper were first publicized.

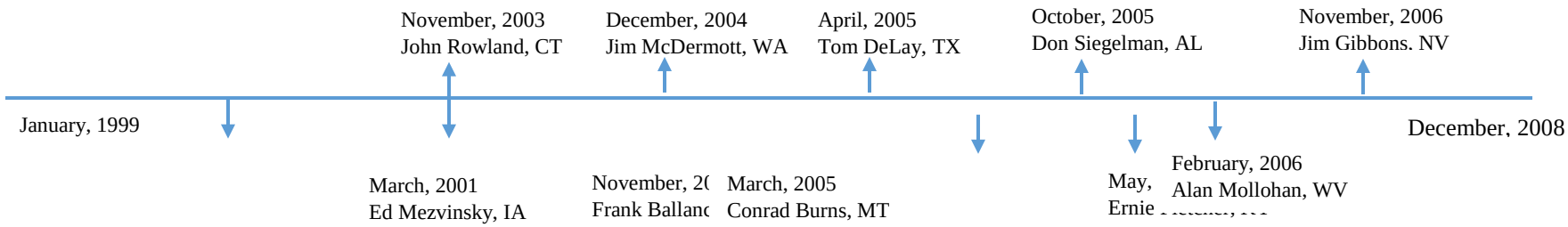

Figure 2. Local Media Coverage around Tom Delay Scandal

This figure shows the number of newspaper articles in each month during the scandal year.

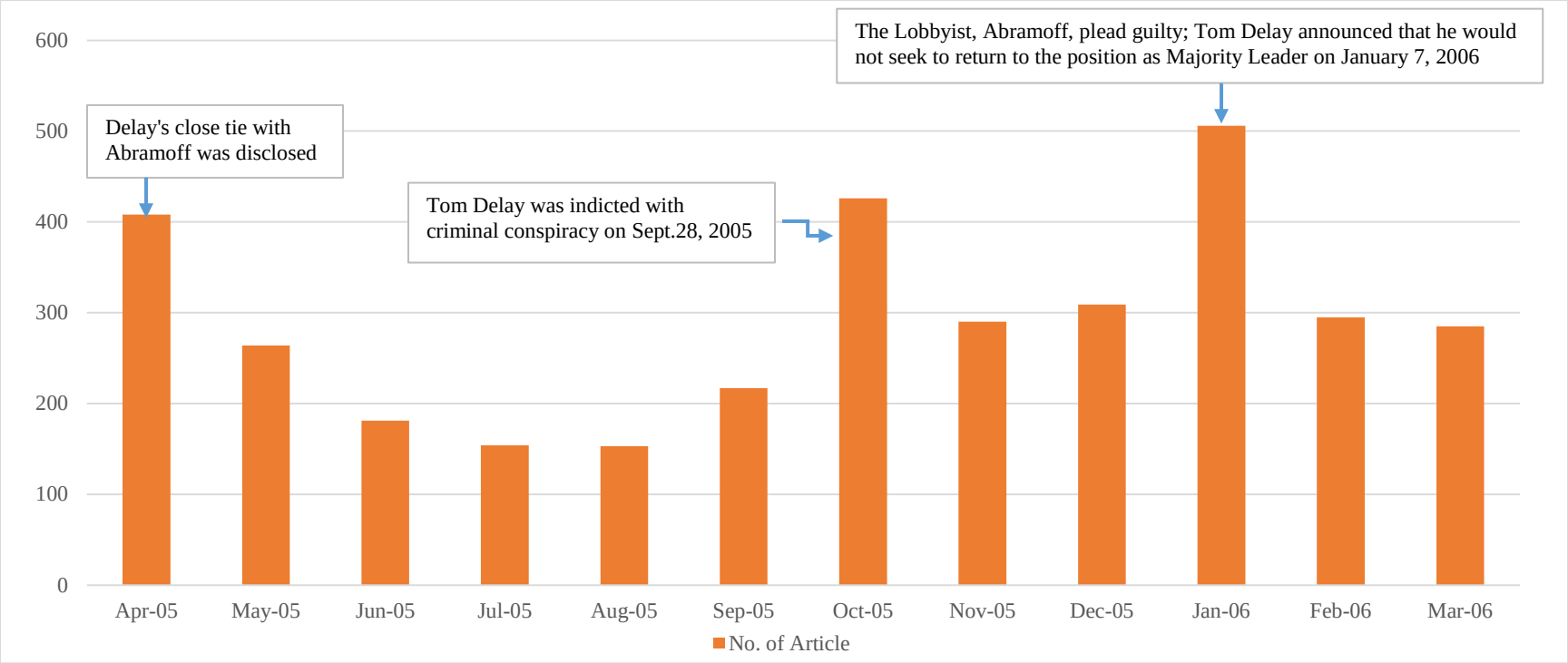

Figure 3. Earnings Surprises

This figure presents the frequency of earning surprises from -10¢ to 10¢ for quarterly earnings announcements during 1999 to 2008 sample period. Earnings surprises are calculated as the difference between actual reported quarterly earnings and analysts' consensus median forecast.

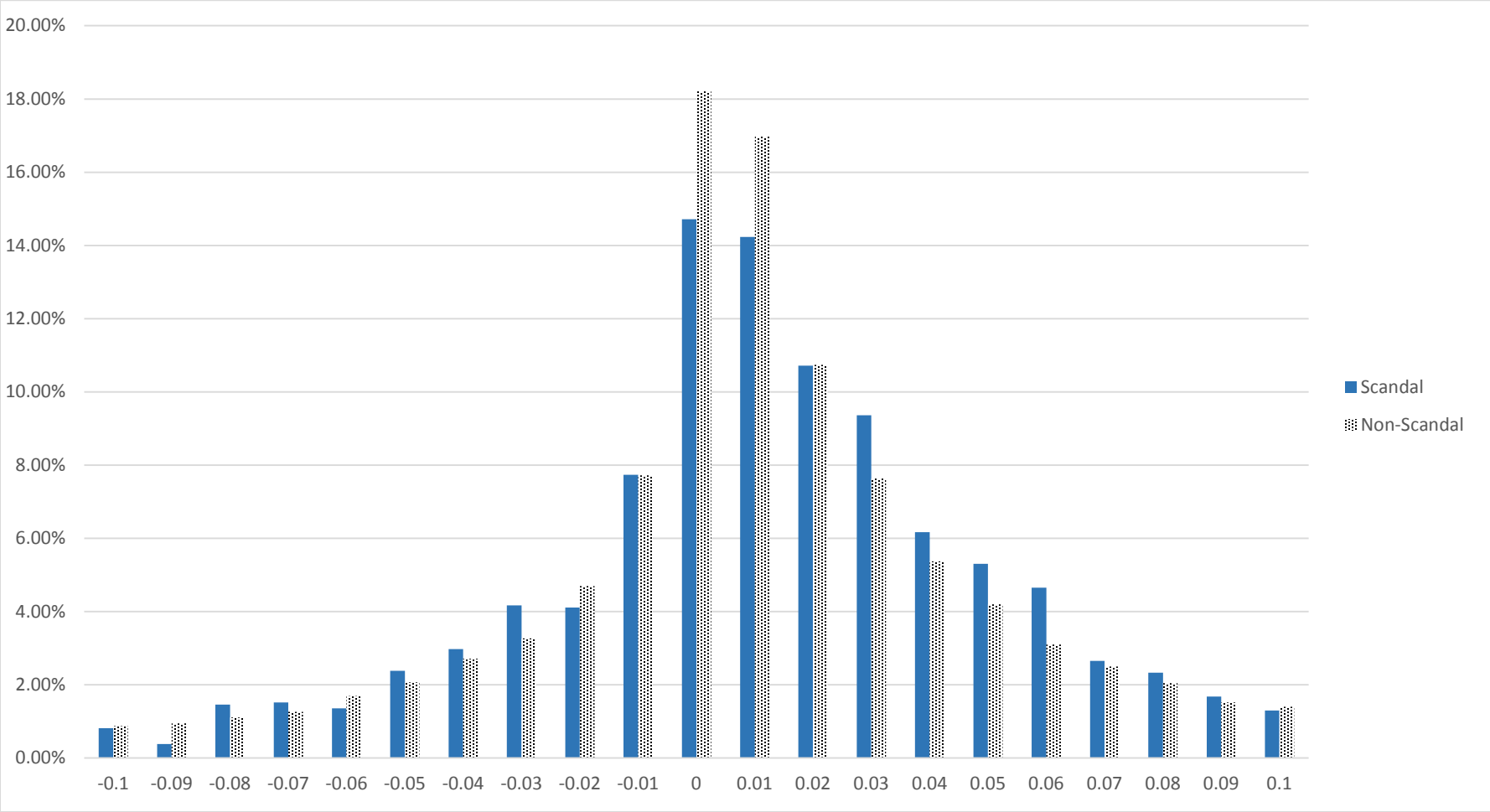

Table A1. : Full List of Political Scandals

This table lists all political scandals included in Puglisi and Snyder (2008) from 1997 to 2006. It indicates the cases used in the main tests of this paper and those used in the extended sample for the robustness tests.

Name	State	Party	Position	Start Date	Used in Main Sample	Used in Extended Sample
Don Siegelman	Alabama	D	Governor	10/1/2005	Yes	Yes
Jim Kolbe	Arizona	R	House	9/1/2006		Yes
Rick Renzi	Arizona	R	House	10/1/2006		
John Doolittle	California	R	House	12/1/2004		Yes
Randy Cunningham	California	R	House	5/1/2005		
Jerry Lewis	California	R	House	12/1/2005		
Jane Harman	California	D	House	10/1/2006		
Gary Miller	California	R	House	12/1/2006		
John Rowland	Connecticut	R	Governor	11/1/2003	Yes	Yes
Katherine Harris	Florida	R	House	6/1/2005		Yes
Mark Foley	Florida	R	House	9/1/2006		
George Ryan	Illinois	R	Governor	1/1/2000		Yes
Rod Blagojevich	Illinois	D	Governor	8/1/2005		Yes
Dennis Hastert	Illinois	R	House	5/1/2006		
Ed Mezvinsky	Iowa	D	House	3/1/2001	Yes	Yes
Ernie Fletcher	Kentucky	R	Governor	5/1/2005	Yes	Yes
Edwin Edwards	Louisiana	D	Governor	11/1/1998		Yes
William Jefferson	Louisiana	D	House	6/1/2005		Yes
Conrad Burns	Montana	R	Senate	3/1/2005	Yes	Yes
Jim Gibbons	Nevada	R	Governor	11/1/2006	Yes	Yes
Robert Torricelli	New Jersey	D	Senate	4/1/2001		Yes
Jim McGreevey	New Jersey	D	Governor	8/1/2004		Yes
Robert Menendez	New Jersey	D	Senate	8/1/2006		
Frank Ballance	North Carolina	D	House	11/1/2003	Yes	Yes
James Traficant	Ohio	D	House	1/1/1997		Yes
Robert Taft	Ohio	R	Governor	6/1/2005		Yes
Bob Ney	Ohio	R	House	10/1/2005		
Paul Kanjorski	Pennsylvania	D	House	2/1/2002		Yes
Curt Weldon	Pennsylvania	R	House	10/1/2006		Yes
Tom DeLay	Texas	R	House	4/1/2005	Yes	Yes
Jim McDermott	Washington	D	House	12/1/2004	Yes	Yes
Alan Mollohan	West Virginia	D	House	2/1/2006	Yes	Yes

Table A2. Regression Analysis of Insider Trading Profits, Different Samples

This table reports OLS regressions of monthly abnormal returns following insider trading months in scandal and non-scandal years. The dependent variable is the DGTW excess return, which is the difference between firm monthly return and the return of a characteristic-based benchmarking portfolio. The key explanatory variable is a dummy variable *Scandal Year* that takes one if (i) the firm is headquartered in one of the scandal state, and (2) the transaction date of the trade is within one-year from the start date of a scandal. *Scandal Year+1* is a dummy variable equals one if the transaction is made within the second year of a scandal. Standard errors are clustered at the firm level. Robust standard errors are presented in parentheses. ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

Panel A: Sell						
	Trader-Month, (without Sequences)		**Firm-Month (without Sequences)**		**Extended Sample**	
Scandal Year	0.0119***	0.0114**	0.00783**	0.00725**	0.00381*	0.00423*
	(0.00424)	(0.00445)	(0.00328)	(0.00350)	-0.00221	-0.00226
Scandal Year + 1		-0.00140		-0.00181		0.00203
		(0.00391)		(0.00343)		-0.00218
Observations	17,734	17,734	10,990	10,990	38,161	38,161
R-squared	0.05794	0.05795	0.05650	0.05652	0.04656	0.04657
Panel B: Purchase						
Scandal Year	0.00646	0.00707	0.00452	0.00349	0.00622*	0.00532
	(0.00466)	(0.00482)	(0.00483)	(0.00484)	-0.00326	-0.00332
Scandal Year + 1		0.00201		-0.00323		-0.00588*
		(0.00449)		(0.00441)		-0.00338
Observations	15,553	15,553	8,272	8,272	27,501	27,501
R-squared	0.02601	0.02602	0.02143	0.02146	0.01173	0.01181

Table A3. Predicting Trading Activity, Extended Sample

This table presents logit models predicting the likelihood of insider trading and Negative Binomial Models explaining the number of shares traded by insiders in a trading month. All calendar months in our sample period are included for each firm. Months in which no trades were made are recorded as zero shares traded. The dependent variable is a dummy that equals 1 if firm insiders traded in that month, 0 otherwise *Positive 10% Months* is a dummy which equals 1 if the DGTW excess return in the following month is more than 10%. *Negative 10% Months* is a dummy which equals 1 if the DGTW excess return in the following month is less than -10%. *Scandal Year* Positive (Negative) 10% Months* is the interaction term between *Positive (Negative) 10% Months* dummy and *Scandal Year* dummy. *Scandal Year+1* Positive (Negative) 10% Months* is the interaction term between *Positive (Negative) 10% Months* dummy and *Scandal Year* +1 dummy. Other variables are defined in Table 3. Robust standard errors are presented in parentheses, and ***, **, and * represent significance at the 1%, 5% and 10% levels, respectively. Standard errors are clustered at the firm level. Year and industry fixed effects are included in all regressions.

Panel A: Sell

	Likelihood of Trading (Logit Model)				Number of Share Traded (Negative Binomial Model)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Scandal Year	0.0207	0.0219	0.0340	0.0363	-0.0665	-0.0872**	-0.0399	-0.0591
	(0.0267)	(0.0292)	(0.0276)	(0.0301)	(0.0407)	(0.0445)	(0.0415)	(0.0453)
Scandal Year +1		0.0183		0.0265		-0.0803*		-0.0715
		(0.0294)		(0.0301)		(0.0448)		(0.0464)
Positive 10% Return Months	-0.260***	-0.243***			-0.163***	-0.146***		
	(0.0190)	(0.0192)			(0.0291)	(0.0294)		
Scandal Year* Positive 10% Months	-0.157**	-0.174**			-0.137	-0.148		
	(0.0697)	(0.0701)			(0.105)	(0.105)		
Scandal Year +1* Positive 10% Months		-0.241***				-0.306***		
		(0.0740)				(0.114)		
Negative 10% Months			-0.149***	-0.136***			-0.0805***	-0.0684***
			(0.0169)	(0.0174)			(0.0247)	(0.0254)
Scandal Year* Negative 10% Months			-0.180***	-0.193***			-0.245***	-0.251***
			(0.0547)	(0.0552)			(0.0841)	(0.0844)
Scandal Year+1*Negative 10% Months				-0.187***				-0.209**
				(0.0576)				(0.0872)
Constant	-2.666***	-2.668***	-2.666***	-2.668***	7.198***	7.207***	7.194***	7.193***
	(0.291)	(0.291)	(0.290)	(0.290)	(0.583)	(0.587)	(0.585)	(0.587)
Observations	417,271	417,271	417,271	417,271	417,271	417,271	417,271	417,271

Panel B: Purchase

	Likelihood of Trading (Logit Model)				Number of Share Traded (Negative Binomial Model)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Scandal Year	0.0132	0.0181	0.0277	0.0294	-0.103**	-0.115**	-0.0753	-0.0941*
	(0.0340)	(0.0361)	(0.0342)	(0.0362)	(0.0506)	(0.0525)	(0.0513)	(0.0529)
Scandal Year +1		0.0382		0.00205		-0.0589		-0.108**
		(0.0372)		(0.0375)		(0.0525)		(0.0527)
Positive 10% Return Months	0.201***	0.209***			0.375***	0.386***		
	(0.0195)	(0.0200)			(0.0251)	(0.0255)		
Scandal Year* Positive 10% Months	0.0609	0.0525			0.165*	0.157		
	(0.0704)	(0.0707)			(0.0962)	(0.0967)		
Scandal Year +1* Positive 10% Months		-0.137*				-0.180*		
		(0.0781)				(0.108)		
Negative 10% Months			-0.1000***	-0.106***			-0.00114	-0.00968
			(0.0175)	(0.0179)			(0.0241)	(0.0247)
Scandal Year* Negative 10% Months			-0.0562	-0.0510			0.0456	0.0567
			(0.0678)	(0.0681)			(0.0961)	(0.0963)
Scandal Year+1*Negative 10% Months				0.103				0.118
				(0.0697)				(0.101)
Constant	-3.089***	-3.090***	-3.048***	-3.047***	5.236***	5.235***	5.293***	5.298***
	(0.392)	(0.392)	(0.391)	(0.391)	(0.582)	(0.580)	(0.582)	(0.582)
Observations	417,271	417,271	417,271	417,271	417,271	417,271	417,271	417,271

Table A4. Regression Analysis of Earnings Surprises, Extended Sample

This Table presents logit regressions of *BEAT* (an indicator that a firm reported a quarterly earnings surprise of 0¢, 1¢ or 2¢) onto independent variables that control for firm characteristics. The sample of observation includes earnings announcements from 1999 to 2008. Earnings surprises are calculated as the difference between firm actual earnings and analysts' consensus median forecast. *Scandal Year* is an indicator variable equals one if observations are in the first four quarters (0, 1, 2, and 3) after revelation of a political scandal. *Scandal Year+1* equals one for the second four quarters (4, 5, 6, 7) after a scandal is first publicized. *Size* is the nature log of market cap. *Market to Book* is the natural log of market to book ratio. *ROA* is the return on assets. *# of analyst coverage* is the number of analyst covering the firm. *Institutional Ownership* is the natural log of total share of stock owned by institutional investor. *Leverage* is calculated as total liabilities scaled by total assets. *Growth rate of assets* is calculated by the change of assets scaled by lagged assets. *Cash flow volatility* is estimated by standard deviations of cash flow of a firm in the entire sample period, scaled by lagged assets. All non-binary independent variables are lagged one year. Robust standard errors in parentheses***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

	Relative to Mean Forecast				Relative to Median Forecast			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Scandal Year	-0.152***	-0.170***	-0.00433	-0.179***	-0.169***	-0.193***	-0.00120	-0.203***
	(0.0290)	(0.0295)	(0.0332)	(0.0304)	(0.0291)	(0.0295)	(0.0330)	(0.0303)
Scandal Year+1	-0.289***	-0.296***	-0.0160	-0.292***	-0.294***	-0.310***	-0.0145	-0.308***
	(0.0299)	(0.0308)	(0.0339)	(0.0317)	(0.0302)	(0.0311)	(0.0343)	(0.0320)
Size		0.0938***	0.0411*	0.125***		0.0714***	0.0164	0.105***
		(0.0222)	(0.0225)	(0.0232)		(0.0217)	(0.0221)	(0.0227)
Market to Book		0.0227***	0.0231***	0.0172***		0.0213***	0.0211***	0.0155***
		(0.00355)	(0.00364)	(0.00366)		(0.00351)	(0.00359)	(0.00360)
ROA		6.318***	6.165***	6.165***		6.292***	6.141***	6.075***
		(0.313)	(0.326)	(0.328)		(0.307)	(0.317)	(0.317)
# Analyst Coverage		-0.00932**	-0.0138***	-0.0179***		-0.000450	-0.00371	-0.00808*
		(0.00434)	(0.00433)	(0.00425)		(0.00433)	(0.00430)	(0.00426)
Institutional Ownership		-0.0133	0.154***	0.0654***		-0.000176	0.169***	0.0734***
		(0.0202)	(0.0214)	(0.0210)		(0.0198)	(0.0210)	(0.0205)
Leverage		-0.296***	-0.560***	-0.435***		-0.334***	-0.573***	-0.446***
		(0.0709)	(0.0841)	(0.0837)		(0.0699)	(0.0829)	(0.0827)
Growth Rate of Assets		-0.215***	-0.248***	-0.317***		-0.173***	-0.205***	-0.270***
		(0.0438)	(0.0452)	(0.0454)		(0.0429)	(0.0445)	(0.0445)
Cash flow volatility		1.091***	1.154***	1.411***		0.992***	1.066***	1.324***
		(0.127)	(0.137)	(0.140)		(0.123)	(0.133)	(0.137)
Constant	-0.373***	-0.724***	-2.828***	-2.504***	-0.326***	-0.765***	-2.815***	-2.416***
	(0.0208)	(0.251)	(0.305)	(0.793)	(0.0205)	(0.247)	(0.299)	(0.784)
Observations	73,260	73,260	73,260	73,257	73,260	73,260	73,260	73,259
Year FE			Yes				Yes	
Industry FE			Yes	Yes			Yes	Yes
Quarter FE				Yes				Yes

Table A5. Regression Analysis of Discretionary Accruals, Extended Sample

This Table presents OLS regressions of discretionary accruals onto independent variables that control for firm characteristics. The sample of observation includes earnings announcements for firm quarters from 1999 to 2008. Discretionary accruals are calculated following Jones (1991) modified model that includes an intercept. *Scandal Year* is an indicator variable equals one if observations are in the first four quarters (0,1,2,3) after a political scandal is first reported. *Scandal Year+1* equals one for the second four quarters (0,1,2,3) after a scandal is first publicized. *Size* is the nature log of market cap. *Market to Book* is the natural log of market to book ratio. *ROA* is the return on assets. *# of analyst coverage* is the number of analyst covering the firm. *Institutional Ownership* is the natural log of total share of stock owned by institutional investor. *Leverage* is calculated as total liabilities scaled by total assets. *Growth rate of assets* is calculated by the change of assets scaled by lagged assets. *Cash flow volatility* is estimated by standard deviations of cash flow of a firm in the entire sample period, scaled by lagged assets. All non-binary independent variables are lagged one year. Robust standard errors in parentheses***, **, and * represent significance at the 1%, 5% and 10% levels, respectively.

Dependent Variable : Absolute Value of Discretionary Accruals

	(1)	(2)	(3)	(4)	(5)	(6)
Scandal Year	-0.341***	-0.229***	-0.0335	-0.277***	0.0187	-0.174***
	(0.0496)	(0.0446)	(0.0484)	(0.0479)	(0.0543)	(0.0484)
Scandal Year+1	-0.328***	-0.200***	-0.00311	-0.213***	0.0226	-0.120***
	(0.0437)	(0.0357)	(0.0330)	(0.0375)	(0.0396)	(0.0381)
Size		0.118***	0.178***	0.190***	0.280***	0.236**
		(0.0419)	(0.0452)	(0.0460)	(0.0948)	(0.0916)
Market to Book		0.0501***	0.0310***	0.0345***	0.0219*	0.0280**
		(0.0114)	(0.0102)	(0.0104)	(0.0115)	(0.0116)
ROA		-4.031***	-3.856***	-3.857***	-0.263	-0.563
		(1.166)	(1.263)	(1.263)	(0.971)	(0.975)
Leverage		-0.524***	-0.314**	-0.0940	0.142	0.365
		(0.110)	(0.122)	(0.123)	(0.234)	(0.239)
Growth Rate of Assets		0.854***	0.677**	0.693**	-0.0812	-0.0353
		(0.284)	(0.279)	(0.281)	(0.297)	(0.297)
Cash flow volatility		-0.00307	0.0115	0.136	0.931	0.967*
		(0.203)	(0.210)	(0.204)	(0.569)	(0.568)
# Analyst Coverage		0.00411	-0.00412	0.000632	-0.0103	-0.00175
		(0.00699)	(0.00767)	(0.00762)	(0.0125)	(0.0124)
Institutional Ownership		-0.199***	-0.202***	-0.238***	-0.130	-0.192**
		(0.0398)	(0.0399)	(0.0421)	(0.104)	(0.0961)
Constant	0.465***	2.975***	2.409***	3.043***	0.278	1.491
	(0.0383)	(0.512)	(0.537)	(0.553)	(1.583)	(1.457)
Observations	37,422	37,422	37,422	37,422	37,422	37,422
R-squared	0.00041	0.00666	0.03616	0.02161	0.01340	0.00272
Year FE			yes		yes	
Industry FE			yes	yes		
Firm FE					yes	yes
Quarter FE				yes		yes