Gider, Jasmin

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Do SEC Detections Deter Insider Trading? Evidence from Earnings Announcements

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Abstract

The paper explores the consequences of SEC detection of illegal insider trading on subsequent insider trading activities. We hypothesize that individuals with private information update their subjective probabilities of getting caught and are less likely to exploit material, non-public information about pending earnings announcements. The hypothesis is tested using data on earnings announcements of a unique hand-collected sample of 398 insider trading episodes publicly detected by the Securities and Exchange Commission (SEC). Based on a difference-in-differences analysis where we compare firms with a detection event, their industry peers and firms in remote industries, we document a statistically and economically significant deterrence effect: In the vicinity of the detection target post detection the run-ups prior to earnings announcements are significantly reduced by 0.7%.

JEL classification: G38, G18
Keywords: insider trading, regulatory enforcement, economics of crime

1 Introduction

What are the consequences of the detection of illegal insider trading by the Securities and Exchange Commission (SEC)? We hypothesize that detection has an impact on future insider trading: Individuals with access to material, non-public information update their subjective probabilities of getting caught when they observe a detection event in their vicinity and are less likely to exploit private information. Using a unique hand-collected dataset of 398 insider trading episodes detected by the SEC between 1995 and 2011 this paper analyzes whether the detection of insider trading by the SEC deters insider trading activities in the same stock and stocks of industry peers. Insider trading is difficult to measure because it is not directly observable. In order to measure trading ahead of price-sensitive announcements we look at abnormal runups prior to earnings announcements. This measure is likely to be monotonically linked to variations in the 'true' level of insider trading. We analyze a firm-quarter panel of 43,646 earnings announcements of detection targets, their industry peers and control firms in remote industries using difference-in-differences approach.

The analysis focuses on illegal insider trading, i.e., the trading on material, non-public information in violation of Rule 10b-5 of the Securities and Exchange Act of 1934. Illegal trading is to be distinguished from 'legal' insider trades by corporate insiders which have to be registered with the SEC. Legal insider trades are disclosed to the public and are not necessarily based on material, non-public information. Most developed countries prohibit trading on material, non-public information based on the premise that it harms other investors.[1] Uninformed market participants lose when trading against market participants with superior information because the uninformed buy at prices which are too high and sell at prices which are too low. This idea is, e.g., analyzed in the model by Glosten and Milgrom (1985). In their model, a specialist, the provider of liquidity, anticipates these cost, so called adverse selection cost, caused by the presence of informed traders. The specialist increases the bid-ask spread accordingly to offset these losses, i.e., he sells at higher prices and buys at lower ones. Uninformed market participants in turn ask for compensation of these costs which results in higher required returns.[2] Bhattacharya

[1] There is an academic debate of whether insider trading is beneficial or detrimental to welfare. See, e.g., Bainbridge (2001) for a discussion of the arguments. Welfare implications of insider trading are not addressed in the current paper.

[2] See Amihud and Mendelson (1986) for a theoretical model of this link and Brennan and Subrahmanyam (1996) for empirical evidence. Easley et al. (2002) find evidence that more trading on private information as indicated by the probability of informed trading is associated with higher returns.

and Daouk (2002) show that the introduction of insider trading rules reduces the cost of equity and thus provide additional support for the notion that insider trading is costly for uninformed market participants and ultimately to shareholders. Cumming et al. (2011) study the impact of exchange trading rules on liquidity and find that insider trading rules reduce bid-ask spreads.

The prevalence of insider trading is actually readily identifiable when there were suspicious trading activities prior to important announcements. However, from a practical law-making perspective, insider trading is very difficult to detect and prosecute. It is difficult to identify the individuals which have caused the suspicious price movements because of the great number of traders in the market and market anonymity. Individuals who want to trade on material, non-public information are also likely to cover up their trading, by, e.g., trading through accounts of remote friends or relatives. Further, even if it is possible to identify the traders, it is difficult to prove that a defendant has known the private information and has also been aware that it is non-public. As a result, there are only very few cases of insider trading publicly detected and brought to justice. U.S. prosecutors have announced to fight insider trading more aggressively, by employing techniques previously uncommon in the control of white collar crime, such as search warrants or wiretaps.[3]

Despite of the substantial public interest in fighting insider trading, little is known of its systematic determinants. There is empirical evidence on the impact of legislation: E.g., Bris (2005) or Ackerman et al. (2008) study empirically how insider trading activity varies with the enactment and first enforcement of insider trading rules at the country level. The existing evidence at the country level suggests that insider trading laws and their enforcement have an impact on the magnitude of insider trading. But how does the insider trading activity vary within a given legal context? Little is known so far about the effects detection has on subsequent insider trading activity. In particular, it is unclear whether SEC actions have spillover effects and thereby lead to reduced insider trading in neighboring firms. Our paper aims at filling this gap.

The present paper contributes to the existing literature on insider trading and regulation. It is the first paper to analyze the consequences of the detection of insider trading and is, hence, complementary to the international studies which analyze the impact of regulation at the country level. Moreover, the paper contributes to the literature on the

[3]See, e.g., the speech by U.S. Attorney Preet Bharara to the New York City Bar Association on October 20, 2010.

economics of crime: We investigate the extent to which experiences in the vicinity of individuals affect their subjective probabilities of detection. Thereby, we contribute to the debate on the effect of risk perceptions about the detection on criminal behavior.

Earnings announcements offer an attractive case to study trading ahead of price-sensitive announcements: There are less individuals involved as compared to M&A transactions and the informed individuals are typically more closely related to the firm which allows for a more representative measurement of the general level of insider trading in a given stock. Moreover, quarterly earnings announcements are regular and frequent. This allows for a comparison of the insider trading activity before and after the detection event. Further, earnings announcements are available for a large sample of firms. As a consequence, the sample is not likely to suffer from a selection bias which could be induced by looking at announcement events which are endogenously chosen such as M&A announcements.[4] Lastly, the magnitude of the surprise component can be quantified.

We test whether runups prior to earnings announcements are lower after there has been a detection event in the firm while controlling for alternative determinants. We use a difference-in-differences approach and compare runup changes in the post detection period of detection targets, their industry peers and firms in remote industries. A detection event is the first day on which the market has learned from the detection of the case by the SEC. In a nutshell, we obtain the following results: SEC detection significantly reduces the runup prior to earnings announcements for firms with a detection event and their industry peers. More concretely, the runup over the time window of t-5 to t-1 around the earnings announcement is reduced by 0.7%. The effect on industry peers, the spillover effect, is strong and statistically indistinguishable from the effect on the detection target itself. We find weak evidence that the deterrent effect for the detection targets is slightly more pronounced for episodes which involve information leakage from within of the firm. These findings remain robust to an alternative measure of insider trading, over different time windows for the runup calculation and over different subsamples. One may object that the observed effect is mechanical, because the pre detection runups for treatment firms include the runup of the insider trading episode. We rule out that the observed relationship is purely mechanical. We also discuss the limitations of the present analysis. In sum, the empirical findings lend support to the notion that detection discourages the

[4] See, e.g., Hasbrouck (1985), Palepu (1986) or Ambrose and Megginson (1992) for evidence which suggests that the likelihood to be acquired is endogenous or, e.g., Harford (1999) or Paliwal (2008) for evidence that the likelihood to acquire is endogenous.

exploitation of inside information in the vicinity of the detection target.

The paper is structured as follows: Section 2 introduces the main hypothesis and relates the paper to the existing literature on the economics of crime and insider trading. Section 3 describes the construction of the dataset and report summary statistics. Section 4 explains the empirical approach. Section 5 presents the empirical findings, robustness checks are reported in Section 6 and potential caveats and limitations are discussed in Section 7. Section 8 concludes.

2 Hypothesis and related literature

Our paper is related to several strands of literature: First, it is related to the literature on the economics of crime which deals with the determinants of the subjective probability of detection. Second, our paper is related to the existing work on the effects of insider trading legislation. Third, it is related to the empirical literature which seeks to construct measures which capture illegal insider trading activities. The present section summarizes the related literature and sets out how the present paper relates to the existing work.

2.1 Probability of detection and economics of crime

The objective of the paper is to test the following hypothesis: The detection of insider trading in a given stock reduces illegal insider trading activities. Existing work on the economics of crime provide a theoretical underpinning for this hypothesis. The core of the argument consists of three parts: First, the propensity of an individual to commit a crime depends on the probability of detection. Second, when the probability of detection is not common knowledge, the individual forms beliefs about it based on past experience. Third, individuals use information from their vicinity when forming and updating expectations about the probability of detection.

The idea of crime as the outcome of rational choice is mainly shaped by Becker (1968). According to this cost-benefit view of crime, a rational individual chooses between criminal and non-criminal behavior by trading off expected benefits against expected costs. The costs are determined by the severity and the likelihood of punishment. Increases in the probability of detection and punishment lead to higher expected costs and, hence, to a lower propensity to commit crime. The hypothesis that variations in the perceived likelihood of detection and punishment lead to changes in the observed crime rate has also been empirically tested. E.g., Bar-Ilan and Sacerdote (2001) analyze traffic data from a series of experiments. The authors investigate an exogenous variation in the likelihood of getting caught. The introduction of a camera enforcement program to monitor driving through red light provides for an exogenous increase in the likelihood of getting caught. Analyzing the consequences of this increase in the probability of detection, the authors find that individuals are substantially less likely to cross red lights.

In the model by Becker (1968) the probability and severity of punishment is common knowledge. However, the real world often exhibits great uncertainty with respect to the true frequencies of crime and detection. This observation applies to insider trading in particular. Detection and enforcement events of insider trading are infrequent. In addition, information about the true crime rate are also very opaque, because they are not directly observable. Further, the SEC investigates secretly, so not much is known about the sources and methods of investigation. Hence, apart from the notion that the likelihood of detection is probably small, not much is known about its determinants. Rather than the actual 'true' probability of detection, individual behavior is more likely to respond to the *perceived* probability of detection. Several existing papers study how these perceptions shape the propensity to commit crime. E.g., Ben-Sahar (1997) argues that individuals are imperfectly informed about detection efforts and the true rate of detection. Enforcement events contribute to learning, because they help individuals to form more precise expectations about the detection rate. Lochner (2007) provides empirical evidence based on longitudinal survey data with information about reported perceptions about the probability of arrest. His results show that personal experiences with enforcement actions shape individual perceptions about the likelihood of detection and sanction.

Information is costly and difficult to obtain. As a result, individuals use information from their close environment rather than 'global' information. When forming and updating subjective probabilities of detection, individuals are likely to use personal experiences or experiences by their vicinity.[5]

Sah (1991) theoretically analyzes the evolution of the probability of detection over time as it is shaped by past experiences. His model explains differences across groups which otherwise face identical economic fundamentals. As argued by Sah, individuals use information from their personal experiences and the experiences of their vicinity to form

[5]The 'neighborhood effect' is analyzed in various alternative economic and social contexts, e.g., by Aneshensel and Sucoff (1996), Clampet-Lundquist and Massey (2008), Grinblatt et al. (2008), or Kuhn et al. (2011).

and update their subjective probabilities of detection. Rincke and Traxler (2011) analyze data on the compliance of Austrian households with paying TV license fees. They find that enforcement actions increase the propensity of neighboring households to comply and pay the fees. Thereby, the empirical evidence in their paper supports the vicinity effect of detection and enforcement actions.

In addition to updating beliefs about the probability of detection, learning about social stigma might present an alternative channel over which individuals are affected by detection and enforcement actions in their neighborhood. Social stigma refers to the reluctance to interact socially and economically with convicted criminals. Thereby, being convicted of a criminal act involves costs which go beyond legal penalties such as monetary fines or imprisonment. The idea is introduced and theoretically analyzed by Rasmusen (1996). By observing how convicted individuals are stigmatized, individuals learn about the costs involved with criminal actions such as illegal insider trading which makes them less likely to trade on private information.

We hypothesize that the detection of illegal insider trading affects the perceptions of individuals in the vicinity of the defendant, namely other individuals who are associated with the firm. Further, we hypothesize that a detection event also impacts insider trading activities at neighboring firms, i.e., industry peers. When trading off expected costs and benefits of exploiting private information for profitable trading, individuals with access to private information take their updated subjective detection probability into account and are less likely to trade.

2.2 Legislation and insider trading

Based on an international sample of 103 countries, Bhattacharya and Daouk (2002) analyze the impact of regulation and enforcement of insider trading laws on the cost of equity. Their test is actually a test of two joint hypotheses: First, insider trading regulation reduces insider trading activity. Second, more insider trading activity leads to higher cost of equity. According to the authors, insider trading is costly to uninformed investors because they lose against informed traders. As a result, investors require compensation for these anticipated losses and ask for a higher rate of return which should be ultimately reflected in greater cost of equity. The cost of equity do not change significantly after the introduction of the prohibition of insider trading. However, they decrease after the first prosecution of insider trading. This suggests that insider trading activity is sensitive with respect to enforcement.[6] Bris (2005) also analyzes a country cross section. However, he takes an alternative approach by more directly investigating the impact of regulation on insider trading activity. His sample consists of 4,541 acquisitions from 52 countries between 1990 and 1999. He uses abnormal returns and volumes in the days prior to takeover announcements as a measure of suspicious trading. His study documents that insider trading enforcement increases both the incidence and the profitability of insider trading. Bris further finds that tougher laws reduce the incidence of illegal trading activity.

Similar to the study by Bris (2005), Ackerman et al. (2008) study the run ups prior to the announcements of approximately 19,000 acquisitions in 48 countries from 1990 to 2003. They find that the enactment of insider trading laws significantly reduces the extent to which the information about the pending acquisition is released prior the announcement. However, in contrast to Bris (2005) they find that the effect of the initial enforcement is small and statistically insignificant.

2.3 Measuring insider trading

How does 'true' insider trading affect returns and volume? Meulbroek (1992) addresses this question by looking at illegal insider trading data from the SEC. Her sample consists of 183 insider trading episodes where individuals were charged with insider trading by the SEC between 1980 and 1989. She finds that insider trades move prices substantially. There are abnormal returns of 3% on average on insider trading days. Meulbroek controls for a potential selection bias. The detection of illegal insider trading may be biased, as unusual stock behavior might catch the regulator's interest in the first place. This is why the sample of detected insider trading cases could be biased in favor of insider trading cases which significantly affect stock behavior. However, Meulbroek argues many cases are detected on the basis of referrals which are unrelated to the magnitude of the price impact, such as referrals by brokers, former employees, fellow conspirators or ex-wives. These cases are unlikely to suffer from the selection bias as the likelihood of detection in such cases is independent of the actual price and volume impact of the insider trade. When analyzing this subsample, the results remain robust.

[6] In a more recent paper Bhattacharya and Daouk (2009) show that the introduction of a law can lead to higher cost of equity if the law is not enforced. According to the authors, the underlying mechanism is the following: If there are some individuals who follow the law when is merely enacted and not enforced, the individuals who are not following the law will violate the law with even greater intensity. However, this result only applies to environments with a low general level of enforcement, i.e., emerging markets and not to developed and highly regulated markets such as the U.S.

Keown and Pinkerton (1981) are among the first to empirically analyze runups prior to price-sensitive announcement. Based on unusual daily returns and weekly volume data on 192 takeover announcements between 1975 and 1978 they identify suspicious trading activities in the targets' stock [7] prior to takeover announcements. According to their results, 40 to 50% of the price impact of an acquisition announcement is already incorporated before the public disclosure.

Jarrell and Poulsen (1989) analyze abnormal return and volume behavior prior to 172 M&A announcements between 1981 and 1985. They draw attention to the problem that runups prior to price-sensitive announcements could also stem from rational market anticipation rather than insider trading. They find that unusual behavior prior to M&A announcement can be partially explained by observable information such as M&A rumors in the press and the foothold acquisition of the bidder. Their paper highlights the need to control for the extent to which the announcement is already anticipated by the market, in order to obtain a more accurate measure of insider trading.

Acharya and Johnson (2010) analyze a link between insider trading and the number of individuals who have access to inside information. They argue that there could either be a positive or a negative effect of the number of insiders on insider trading intensity: More insiders can lead to a higher likelihood of detection and punishment which could lead to less exploitation of inside information. Hence, there will be a negative relationship. However, if there is no connection between the likelihood of detection and the number of insiders, more insiders will lead to more insider activity. They empirically test the hypotheses with data on insider activity prior to bid announcements of private equity buyouts. As a number of insiders they use the number of financing participants of the transaction. As a measure of insider trading they use unusual behavior in return and volume 5 days prior to the announcement. They find that more equity participants are associated with suspicious stock and options trading activities.

To our best knowledge there is only one other paper which analyzes earnings announcements from the angle of information leakages. Cai et al. (2011) investigate whether Wall Street connections of the firm are associated with increased selective pre-disclosure of material, non-public information. They analyze whether Wall Street connections lead to earning announcements being less informative, where they measure the informativeness of an earnings announcement by the market reactions around the earnings announce-

[7] Cumming and Li (2011) also document significant pre-announcement runups in the stock of acquirers.

ments in the window of t-1 to t+1 around the announcement at t. The main hypothesis of Wall Street connections leading to more insider trading also implies that there will be higher runups prior to earnings announcements. The study of the runup as opposed to the market reaction - the approach we take in the present paper - would even present a more direct test of their hypothesis. However, it is unclear why the authors do not test this implication in their paper.

3 Data

3.1 Construction of the dataset

Detection sample: The detection sample is hand-collected from the archive of litigation releases from the website of the SEC.[8] Litigation releases are available from this database starting September 28, 1995. We collect insider trading litigation releases up to December 31, 2011. We search all litigation releases for cases of insider trading, i.e., trading on material, non-public information which constitutes a violation of SEC Rule 10b-5. A litigation release typically includes the following information: date of filing of the release, names of the defendants, the stock in which the defendant has traded, type of information on which the defendant has allegedly traded (e.g., takeover by another firm), date of the announcement of the public disclosure of the information (e.g., date of the takeover announcement), the association of the defendants with the firm, how the defendants learned from the information and whether the information has been tipped to others. In several cases, there is also information on the trading dates, the amount of shares or options traded and trading profits. Where available, we also collect information based on complaints[9].

For the purpose of our empirical analysis, we are interested in the date where the detection of the insider trading incidence by the SEC becomes publicly available for the first time. As a default approach, we use the filing date of the litigation release as the detection date. The SEC investigates secretly and does not disclose any information on ongoing investigations. This is why the filing date of the litigation release usually coincides with the first disclosure of the insider trading case on behalf of the SEC. We run a press search using Factiva to search for a potential earlier release of the detection.

[8] http://www.sec.gov/litigation/litreleases.shtml.

[9] In some cases, the SEC publishes the complaints for the insider trading episodes on their website. A complaint is a legal document which presents the facts and legal reasons for the litigation.

Only for a few cases, we identify an earlier date. One may object that individuals associated with the firm are aware of the SEC investigations, because the firm is often asked to provide internal documents, e.g., information about all employees who have been involved in a takeover. In those cases, some employees receive earlier notice about the insider trading detection. In optimum, we would have to include the first date on which firm individuals become aware of the detection, i.e., the date at which the SEC informs individuals in the firm about the ongoing investigations in the matter of the insider trade. However, this information is private to the SEC. Further, it is likely that only a small circle of individuals in a firm will be informed about the SEC investigations and this information is likely to spread within the firm in unknown cascades. Hence, we choose the date on which the information becomes publicly available, although we are aware that there might be some individuals who have received notice of the SEC investigations in advance. In some cases, there are several insider trading episodes in the same firm. Only the first insider trading episode which is detected by the SEC is included in our sample.

Based on the information manually collected from the SEC website, we classify the insider trading cases to the following event categories which reveal the type of information on which the defendants have traded: M&A, earnings, fraud (accounting or financial fraud), announcements by the U.S. Food and Drug Administration, corporate finance measures (e.g., issue or repurchase of shares), analyst revisions, and other. The category other includes general business announcements or changes in personnel.

In many cases the stock of the firm is not listed anymore when the insider trading incidence is detected by the SEC. In several cases this is due to a takeover of the firm or other reasons for delisting (e.g., such as bankruptcy). If the firm has been taken over by another firm in the meantime, we include the acquiring firm in the sample. We investigate whether this adversely affects our results. In other cases of delisting, we exclude the observations from the sample.

Further, we classify the observations according to the source of leakage. Has the information been stolen, has the information leaked or is the source unknown? In cases of stolen information, we require that the defendant did not have access to the inside information through his job or did not receive any tips but has expropriated the inside information by illegally gaining access to it, e.g., by hacking into computers. Observations with an unknown source of leakage refer to the cases where the SEC has frozen accounts subsequent to extra-ordinarily large trading volumes in shares or options by one account prior to the announcement of price-sensitive information. The SEC has done so only in a small number of cases (see descriptive statistics below).

For the cases of information leakage, we classify the source further. How is the defendant related to the firm? The defendant has either traded on the information himself or he has tipped the information to a third party. The source of leakage may be identical to the individual that also traded on the information. Alternatively, the source of leakage has tipped the piece of material, non-public information to another party. In many cases, insider information is tipped to family members of friends, in an attempt to conceal the insider trading. We differentiate between the following categories: inside leakage, shareholder, director, professional service firm or other. The inside leakage category includes individuals which are directly employed by the firm, e.g., CEO, top executive (other executive members of the board except for the CEO), officer and executive not part of the management board, or employee. The category of professional service firm consists of individuals who have a fiduciary duty to the firm and its shareholders by advising the firm as a management consultant, investment bank or legal advisor. The category other includes individuals who have received the confidential information due to their employment with a supplier or a client. Furthermore, we classify the observations as crime of opportunity or organized crime. An insider trading episode is considered as organized crime if the defendants have traded in at least two episodes in different stocks. Otherwise, the observations are classified as crime of opportunity.

Firm characteristics and stock data: COMPUSTAT is used to collect information about the total assets, market capitalization, return on assets, leverage, Tobin's Q, book-to-market ratio, R&D expenditures and SIC code. Returns and trading volumes are from CRSP.

Sample of earnings announcements: We collect information on quarterly earnings announcements for the firms and corresponding analyst forecasts from IBES from 1993 to 2011. The following data are collected: date of the actual earnings announcement, number of analyst forecasts, actual earnings announced, mean and median of the analyst forecast consensus and the standard deviation of the analyst forecasts.

Control samples: Our objective is to compare the post detection change in insider trading with respect to detection targets with the effects on industry peers and remote firms. To this end, we select two control samples: a control sample of industry peers and a control sample of firms in industries in which there was no detection event over the sample period. We identify the matched sample of industry peers as follows: From the universe of COMPUSTAT firms without any insider trading episodes we delete the ones for which there are no available data on CRSP and where we do not have IBES data on 8 or more earnings announcements. We use the following matching algorithm: For each insider trading episode in our sample, we select the firms with the same first 3 digits in terms of SIC code as the detection target. We then choose the 10 firms which are closest in terms of size in the month of the filing of the SEC detection. Among those, the firm that minimizes the distance in terms of book-to-market ratio is chosen as the matched control firm.[10] For each matched industry peer we also construct variables which describe the insider trading episode, such as the filing date, the event type of the insider trading case and the source of leakage by setting the values equal to the corresponding matched detection firm.

For the construction of the sample of firms in remote industries, we proceed as follows: Similar to the approach above, from the universe of COMPUSTAT firms without any insider trading episodes we delete the ones for which there is no available data on CRSP and where we do not have 8 or more earnings announcement data on IBES. As a next step, we identify the firms which are in 2-digit SIC code industries which have not experienced any detection event over the sample period. To each insider trading episode in our sample, we randomly assign a control firm from this set of firms. The values of variables describing the insider trading episode are set equal to the value of the matched episodes.

In sum, we have three different groups: the group of firms with an insider trading episode, a group of matched industry peers and a group of randomly assigned matched firms in remote industries. In the following analyses we refer to two samples which are defined as follows: The *treatment* sample consists of firms with an insider trading episode only. The *vicinity treatment* sample consists of firms in the neighborhood of the detection firm, that is the detection firm itself and industry peers.

In total, many observations from the detection sample are lost due to missing data from COMPUSTAT, CRSP or IBES. In total we count 1,165 insider trading episodes detected by the SEC between 1995 and 2011. Restricting attention to the first announcement of insider trading within one firm, sufficient available information on the

[10] Later in this paper, we control for a slightly different matching algorithm and more than one matched control firm.

episode in the litigation release and the availability of CRSP data, our sample reduces to 625. The availability of COMPUSTAT data further reduces the sample to 573. The required availability of coverage by the IBES database further reduces the sample to 398 cases of insider trading episodes.

3.2 Descriptive statistics

Table 1 breaks down the insider trading episodes in our sample by time and type of information events. Exploiting foreknowledge of pending M&A announcements is the most common type of insider trading (35.9%). Earnings announcements represent the second most common type (23.1%), followed by announcements of the firm regarding corporate financing (15.3%). The average number of days between the event date (the date on which the information on which the defendants traded was disclosed) and the detection date (filing date of the SEC litigation release or if available earlier disclosure in the press) is 877.12 days (median 791.5). Of the 398 insider trading episodes, 298 observations include information on the original firm and stock in which the insider trading occurred, while in 100 cases, the firm has been acquired. In these cases, we use data on the acquiring firm.[11] Table 2 displays how the events are distributed across types of leakage sources. Source of leakage denotes the association between the tipper or trader accused of illegal insider trading and the firm. In 38 cases, the material, non-public information has been stolen. In 14 cases, the source of leakage is unknown at the time of detection by the SEC. There is information leakage in 346 cases. 121 of those include leakage of information by employees of the firm. 57 include leakage of information by shareholders and 21 of directors. In 123 cases, employees of professional service firms traded on non-material, public information or passed the information along to family or friends. Of the 398 insider trading episodes, 200 are classified as crime of opportunity and 198 as organized crime.

Table 3 shows the summary statistics of the firm characteristics for the treatment sample, (Panel A), the sample of matched industry peers (Panel B) and the sample of randomly assigned control firms in remote industries (Panel C). A firm with an insider trading episode has a mean size of total assets of 8,097 million and a market capitalization of USD 7,374 million. The median values are much lower with 1,403 million and 1,589 million which indicates that the sample is skewed to the right. The mean Tobin's Q is

[11] We explicitly address potential differences with respect to the subsample of acquired firms in Section 6.

Table 1: Distribution of detection events

	M&A	Earnings	Fraud	FDA	Finance	Analyst revision	Other	Sum
1994	1	0	0	0	0	0	1	2
1995	2	1	0	0	0	0	0	3
1996	3	4	1	0	0	0	0	8
1997	1	2	1	3	0	0	1	8
1998	13	7	0	1	0	0	4	25
1999	14	0	1	0	0	0	9	24
2000	21	4	0	0	0	0	7	32
2001	9	4	2	0	0	0	1	16
2002	7	5	1	2	0	0	3	18
2003	8	6	2	1	0	0	1	18
2004	13	5	4	0	2	0	1	25
2005	5	17	1	5	7	0	11	46
2006	8	2	1	3	40	0	1	55
2007	7	14	0	1	11	16	0	49
2008	10	3	0	1	1	0	0	15
2009	15	8	1	2	0	0	7	33
2010	3	10	1	2	0	0	1	17
2011	3	0	0	0	0	0	1	4
Sum	143	92	16	21	61	16	49	398
In percent	35.9%	23.1%	4.0%	5.3%	15.3%	4.0%	12.3%	100.0%

The table reports the distribution of detection events over years and over different types of events on which material, non-public information has been misused. M&A denotes announcements of mergers and acquisitions, earnings denotes the announcement of quarterly or annual earnings, fraud denotes the announcement of financial or accounting fraud, FDA denotes announcements by the U.S. Food and Drug Administration, Finance denotes corporate financing policies (e.g., share buyback, capital increase). Analyst revisions refer to the update of analysts regarding buy or sell recommendations.

Table 2: Distribution of the source of leakage

Source of leakage			Number of cases
unknown			14
stolen			38
leakage			346
	inside leakage		121
		CEO	10
		top executive	10
		officer	57
		employee	44
	shareholder		57
	director		21
	professional service firm		123
		consultant	29
		legal advisor	21
		investment bank	73
	Other		24
Crime of opportunity			200
Organized crime			198

The table reports the sources of leakage of the material, non-public information which was exploited for insider trading. The source of leakage is labeled *unknown* if the SEC does not have information on how the defendants received access to the material, non-public information. Cases are classified as *stolen* if the defendant has illegally gained access to the private information. *leakage* denotes cases in which the defendants, i.e., the trader or the tipper, received the information as part of their employment. *inside leakage* denotes cases in which the trader or tipper is directly employed by the firm. This category is further broken down: *CEO* denotes Chief Executive Officer, *top executive* other executive board members excluding the CEO, *officer* includes executives below the executive board, and *employees*. *shareholder* denotes individuals with a substantial stake in the firm (at least 5%). *director* denotes non-executive members of the board. The category *professional service firm* includes employees of service firms with a fiduciary duty to the firm with the insider trading episode: *Consultant* stands for management consultants, *investment bank* for employees of investment banks who are advising the firm in matters of financing or mergers and acquisitions and *legal advisor* denote employees of law firms. An insider trading episode is classified as *crime of opportunity* if the defendants have exploited inside information with respect to one firm only. If they have committed insider trading in at least two different stocks, we classify the episode as *organized crime*.

2.396, while the mean book-to-market ratio is 0.472. On average, the firms have a mean return on assets of 8.7% and R&D expenditures of 6.8%.

The table also reports the differences in means between the sample. Despite of the matching procedure, firms in the industry peer group are significantly smaller as compared to the treatment sample. They industry peers have a smaller Tobin's Q, a greater book-to-market ratio, are less profitable and have less R&D expenditures. There are two potential explanations for these differences: First, firms with certain characteristics could more likely to be involved with illegal insider trading activities. Second, if one assumes that illegal insider trading activities are equally distributed among all types of firms, it could be the case that the SEC does not treat all firms equally when deciding on the investigation and prosecution of insider trading episodes, but is biased towards focusing on certain types of firms. The resources to fund investigation and enforcement are constraint. [12] Investigating the reasons for the observed differences would go beyond the scope of this paper. In the following analyses, we control for the differences by using firm characteristics as control variables, including firm-fixed effects in the model and explicitly test how firm characteristics affect the impact of detection.

Table 4 shows summary statistics of the earnings information. On average, there are 10.8 analysts who issue quarterly earnings forecasts for one stock in the treatment sample. The mean standard deviation of the estimate is 3.1%. The mean absolute surprise ((actual earnings minus median of the forecasts)/share price) is 0.6%. If the actual earnings exceed the forecast (positive surprise), the surprise is 0.4%, whereas the surprise is -0.8% if the actual earnings fall short of the forecast consensus (negative surprise). There are significantly more analysts covering stocks in the treatment sample as opposed to stocks of industry peers or firms in remote industries. This difference can be attributed to the differences in size as shown in Table 3, because the number of analysts typically increases with firm size. The dispersion of analysts as measured by the standard deviation of the estimates is larger for treatment firms as opposed to their industry peers. When we compare the treatment firms or the industry peers to the firms in remote industries we find that firms in remote industries seem to have a slightly smaller standard deviation of analyst estimates.

[12] Kedia and Rajgopal (2011) find empirical evidence which points towards SEC enforcement preferences. According to their results, SEC enforcement is more likely with respect to firms which are geographically close to SEC offices.

Table 3: Firm characteristics

Variable	Mean	St. Dev.	Median	95% quantile	5% quantile
Panel A: treatment sample					
Total assets (in USDm)	8,097.306	14,720.860	1,403.257	53,747.250	42.131
Market capitalization (in USDm)	7,373.833	12,155.290	1,588.920	42,380.780	70.814
Tobin's Q	2.396	1.773	1.719	6.612	0.945
Book-to-market ratio	0.472	0.369	0.378	1.215	0.058
Return on assets	0.087	0.166	0.114	0.302	-0.267
R&D	0.068	0.101	0.023	0.287	0.000
Panel B: industry peer control sample					
Total assets (in USDm)	3,990.630	9,994.488	587.018	23,043.000	41.858
Market capitalization (in USDm)	3,527.729	7,691.392	746.515	17,966.620	60.998
Tobin's Q	2.292	1.664	1.715	5.973	0.917
Book-to-market ratio	0.486	0.367	0.400	1.192	0.076
Return on assets	0.080	0.167	0.115	0.272	-0.283
R&D	0.067	0.098	0.022	0.273	0.000
Panel C: remote firms control sample					
Total assets (in USDm)	1,791.924	3,565.729	626.571	7,872.000	62.777
Market capitalization (in USDm)	1,428.141	2,856.751	521.743	5,897.250	51.195
Tobin's Q	1.782	1.224	1.401	4.093	0.861
Book-to-market ratio	0.622	0.445	0.523	1.632	0.087
Return on assets	0.136	0.093	0.131	0.298	0.001
R&D	0.004	0.022	0.000	0.016	0.000
Difference in means:	A vs. B	A vs. C	B vs. C		
Total assets (in USDm)	4106.676***	6305.382***	2198.706***		
Market capitalization (in USDm)	3846.104***	5945.692***	2099.588***		
Tobin's Q	0.104***	0.614***	0.511***		
Book-to-market ratio	-0.014***	-0.150***	-0.136***		
Return on assets	0.008***	-0.049***	-0.056***		
R&D	0.002***	0.064***	0.062***		

The table reports summary statistics of firm characteristics of the detection group, the group of industry peers and the group of firms in remote industries. $Total\,assets$ is the total book value of the firm's assets. $Market\,capitalization$ is the market value of equity. $Tobin's\,Q$ denotes the market value of equity and debt divided by the book value of the firm's assets. $Return\,on\,assets$ denotes earnings before taxes and interest before depreciation divided by the book value of total assets. $R\&D$ is R&,D expenditures scaled by the book value of total assets. The table also indicates the differences in means between the detection group and the industry peers (A vs. B), the detection group and the group of remote firms (A vs. C) and the industry peer group and the group of remote firms (B vs. C). *, ** and *** indicate statistical significance at the 10%, 5% and 1% level based on a t-test with unequal variances.

Table 4: Summary of earnings announcements

Variable	Mean	St. Dev.	Median	95% quantile	5% quantile
Panel A: treatment sample					
Number of analyst estimates	10.796	8.022	9	26	1
Analyst dispersion	0.031	0.043	0.02	0.12	0
Surprise	0.006	0.015	0.001	0.029	0
Positive surprise	0.004	0.007	0.001	0.020	0.000
Negative surprise	-0.008	0.014	-0.002	0.000	-0.055
Panel B: industry peer control sample					
Number of analyst estimates	7.852	6.620	6	22	1
Analyst dispersion	0.028	0.041	0.01	0.11	0
Surprise	0.006	0.015	0.001	0.028	0
Positive surprise	0.004	0.006	0.001	0.020	0.000
Negative surprise	-0.009	0.014	-0.003	0.000	-0.055
Panel C: remote firms control sample					
Number of analyst estimates	5.533	4.504	4	16	1
Analyst dispersion	0.039	0.049	0.020	0.14	0
Surprise	0.007	0.015	0.002	0.034	0
Positive surprise	0.004	0.006	0.002	0.020	0.000
Negative surprise	-0.010	0.015	-0.004	0.000	-0.060
Difference in means:	A vs. B	A vs. C	B vs. C		
Number of analyst estimates	2.944***	5.263***	2.319***		
Analyst dispersion	0.003***	-0.008***	-0.011***		
Surprise	0.000	-0.001***	-0.001***		
Positive surprise	0.000	0.000***	0.000***		
Negative surprise	0.000	0.002***	0.001***		

The table reports summary statistics of the analyst forecasts of earnings announcements for the detection group, the group of industry peers and the group of firms in remote industries, the industry. *Number of analyst estimates* is the number of analysts who issue a forecast for a quarterly earnings announcement. *Analyst dispersion* is the standard deviation of the analyst forecasts with respect to one earnings announcement. *Surprise* is the absolute value of the difference between the actual earnings minus the median of analyst forecast scaled by the share price. *Positive surprise* (*negative surprise*) is the difference between the actual earnings minus the median of analyst forecasts scaled by the share price if the difference is strictly positive (negative). The table also indicates the differences in means between the detection group and the industry peers (A vs. B), the detection group and the group of remote firms (A vs. C) and the industry peer group and the group of remote firms (B vs. C). *, ** and *** indicate statistical significance at the 10%, 5% and 1% level based on a t-test with unequal variances.

4 Empirical strategy

4.1 Identification of insider trading

The goal of the present analysis is to analyze the effect of detection on subsequent exploitation of inside information in the neighborhood of the detection target, i.e., the detection firm itself and industry peers. Measuring insider trading activity directly is nearly impossible as this would require knowledge of the information set and transactions of all traders in a specific stock as pointed out by Acharya and Johnson (2010). Hence, empirical studies which aim at capturing insider trading activities have to rely on measures which are known to be monotonically related to insider trading. In the literature section above we have argued that unusual stock return behavior qualifies as a measure of suspicious trading activities.

For the purpose of testing the hypothesis that observed detection reduces insider trading activity, we choose a variable which is likely to be monotonically linked to the 'true' level of insider trading. We choose abnormal runups prior to quarterly earnings announcements. Part of the existing literature which analyzes the effects of regulation on insider trading (e.g., Ackerman et al. (2008) or Bris (2005)) uses abnormal returns prior to M&A announcements. For the purpose of the present paper, studying earnings announcements as opposed to M&A transactions is more appropriate for the following reasons: First, we seek to analyze the effect of detection *at the firm level.* Information about earnings announcements are available for many firms. Looking at M&A events, we would encounter a selection bias because the likelihood of undertaking or undergoing an M&A event is not exogenous. Second, earnings announcements occur on a quarterly basis, i.e., frequently and regularly. M&A events are less frequent and regular. In addition, the acquired firms are often delisted after the acquisition. Hence, we are able to compare insider trading activity before and after the detection event. Third, many different parties are involved in and have thus foreknowledge of M&A transactions. In addition to usual firm insiders, there are individuals associated with the acquirer, alternative bidders, alternative targets, consultants, investment banks, law firms etc. involved. Thus, it is less likely the case that the source of leakage stems from within the firms as opposed to earnings announcements, where the foreknowledge is more concentrated among individuals directly associated with the firm.

Further, the study by Jarrell and Poulsen (1989) argues that runups can both be caused by market anticipation and insider trading. With respect to earnings announcements we can account for market anticipation by controlling for the analyst forecast consensus. Thereby we can measure the surprise component of the announcement more precisely.

Similar to the approaches taken by Acharya and Johnson (2010) or Ackerman et al. (2008) we use runups prior to the announcement date, i.e., abnormal returns as a measure of insider trading. Cumulative abnormal returns (CAR) are constructed in a period before the earnings announcement day t. First, we estimate the parameters of the market model based on the daily returns in the estimation period from t-140 to t-30 by regressing daily returns on the market premium and a constant.[13] The parameters are then used to calculate expected daily returns for the event period t-20 to t+2. The daily abnormal return is obtained by subtracting the expected return from the actual return. To obtain CAR, we sum up the daily abnormal returns over the corresponding event window, where we use the window from t-5 to t-1 as the standard window of our analysis.

4.2 Regression

Our objective is to measure the effect of detection, our treatment, on runups as a measure of insider trading. To this end, we use a difference-in-differences method. According to this method, the effect of detection on insider trading is computed as a double difference: From the difference between the post and pre detection runups of the treatment sample we subtract the difference between the post and pre detection difference in runups of the control sample. This approach combines a cross-sectional and a time series perspective. Thereby, we avoid the potential problem of an omitted time trend which is affecting the groups simultaneously and sidestep the issue of unobserved heterogeneity between the groups. Since we would like to differentiate between the treatment effects on the close versus farther neighborhood of SEC detection, we look at two double differences and compare the pre-post differences of (1) the treatment group and the control group of remote firms and (2) the vicinity treatment group and the control group of remote firms. We run the following regression using data on the treatment sample and the two matched control samples:

$$\begin{aligned} runup_{i,t} = \beta_0 &+ \beta_1 \cdot vicinity\,treatment_i + \beta_2 \cdot treatment_i + \beta_3 \cdot post\,detection_{i,t} \\ &+ \beta_4 \cdot post\,detection_{i,t} \cdot vicinity\,treatment_i + \beta_5 \cdot post\,detection_{i,t} \\ &\cdot treatment_i + \beta_6 \cdot surprise_{i,t} + \beta_7 \cdot analyst\,dispersion_{i,t} + \beta_8 \cdot X_{i,t}, \end{aligned} \quad (1)$$

[13] The factors are obtained from the Data Library of Kenneth French's website.

where $runup_{i,t}$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement of firm i in quarter t. It aims at capturing the extent of trading ahead of the upcoming earnings announcement. For the observations in which the analyst forecasts fall short of the actual earnings (negative surprises), we reverse the sign of the abnormal return. The dummy variable *vicinity treatment* is equal to 1 if the firm was subject to a detection event or if the firm is a matched industry peer. In other words, it is equal to 0 for the matched control firms in remote industries and 1 otherwise. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. We control for various other factors which are expected to affect the runup prior to an earnings announcement. The size of the runup is likely to depend on the information content of the earnings announcement. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable controls for market anticipation and provides for an event-specific measure of asymmetric information with respect to the information content of the earnings announcement. We also control for the dispersion of analyst forecasts. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. The higher the dispersion, the more uncertainty is there in the market regarding the upcoming earnings announcement. X stands for the vector of firm- and time-specific control variables. As control variables we use firm size (natural logarithm of market capitalization), Tobin's Q, R&D expenditures and return on assets. We also add firm-fixed effects to the model later. Thereby, we measure how the regression coefficients are driven by the variation over time within a given firm. When including firm-fixed effects we have to drop the variables *vicinity treatment* and *treatment* from the model specification because those variables are perfectly collinear to the firm intercepts. Standard errors are clustered at the firm level. Except for dummy variables, all independent variables are standardized.

Table 5 displays the conditionals mean estimates from the difference-in-differences regression model. The sum $\beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5$ denotes the average runup for firms in the treatment group (i.e., detection targets) in the post detection period, while $\beta_0 + \beta_1 + \beta_2$ is the average runup for this group in the pre detection period. Hence, the pre-post difference between those average runups is $\beta_3 + \beta_4 + \beta_5$. The equivalent pre-post difference for the control group is β_3 $(= \beta_0 + \beta_3 - \beta_0)$. The double difference for the treatment effect of SEC detection is the difference between those pre-post differences, i.e., $\beta_4 + \beta_5$. In an analogous manner, the double difference with respect to the treatment effect on the farther vicinity of the detection target is β_4. The main coefficients of interest are, hence, β_4 and β_5. They test our hypothesis that a detection event deters insider trading in the neighborhood of the firm. The coefficient β_4 captures the extent to which insider trading is deterred at the detection firm and peers in the same industry. However, the coefficient β_5 tests whether there is an additional deterrent effect at the level of the firm with the detection event. If there are spillover effects on industry peers, we expect β_4 to be negative and significant. If the deterrent effect for the detection firm is more pronounced as compared to the effect on industry peers, we expect a negative and significant β_5. If a detection event only impacts subsequent insider trading at the firm level, we expect β_4 to be insignificant and β_5 to be negative and significant.

Table 5: Conditional mean estimates from the difference-in-differences regression model

	Post detection	Pre detection	Difference
Treatment	$\beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5$	$\beta_0 + \beta_1 + \beta_2$	$\beta_3 + \beta_4 + \beta_5$
Vicinity treatment	$\beta_0 + \beta_1 + \beta_3 + \beta_4$	$\beta_0 + \beta_1$	$\beta_3 + \beta_4$
Control	$\beta_0 + \beta_3$	β_0	β_3
Difference treatment - control	$\beta_2 + \beta_4 + \beta_5$	β_2	$\beta_4 + \beta_5$
Difference vicinity treatment - control	$\beta_1 + \beta_4$	β_1	β_4

This table illustrates the conditional mean estimates of the difference-in-difference regression. This table is based on Roberts and Whited (2011), p. 39.

5 Empirical results

5.1 Impact of detection

Table 6 shows the results for the difference-in-differences regressions. Model (1) regresses runups on the difference-in-differences indicators, the earnings surprise and the dispersion of analyst forecasts. Model (2) adds control variables, whereas Model (3) additionally includes firm-fixed effects.

The results of Models (1), (2) and (3) show that a detection event reduces runups only with respect to treatment firms or firms in their neighborhood, but not with respect to remote firms. The coefficient of *post detection* is insignificant, while the one of *post detection* $*$ *vicinity treatment* is negative and statistically significant at the 1% level. The effect is also economically significant: It varies from -0.6% to -0.7%. However, the insignificance of the variable *post detection* $*$ *treatment* indicates that the detection effect on treatment firms does not differ from the effect on peers. These findings suggest that a detection event significantly reduces insider trading in the vicinity of the detection target. The spillover effect on industry peers is apparently strong, as this effect is statistically indistinguishable from the immediate effect in the same firm. The positive and significant coefficient of the dummy variable *vicinity treatment* indicates, that firms with a detection event and their peers have higher runups on average. However, the insignificant coefficient of *treatment* suggests that there is not any significant difference between the detection targets and their peers regarding the overall level of runups.

In Models (4) and (5) we look at positive and negative earnings surprises separately. A positive (negative) earnings surprise is an earnings announcement which beats (falls short of) the analyst forecast consensus which is defined as the median of the forecasts.[14] The runups for negative surprises have been multiplied with (-1), so we expect the independent variables to affect the runups in the same direction as compared to positive surprises. We observe that there is a significant deterrent effect with respect to positive surprises, but there is no effect on negative surprises. The positive coefficient of *post detection* in Model (4) indicates that runups of positive surprises are slightly higher for all three groups in the post detection period. The deterring effect of detection on detection targets and their peers is -0.9%. According to the results in Model (5) a detection event does not significantly affect insider trading prior to negative earnings announcements.

Models (6) and (7) show the results with respect to two subsets of the entire sample: Model (6) tests for differences in the treatment effect of a detection event between the group of detection targets and their industry firms. Model (7) compares the treatment effect between the group of detection targets and firms in remote industries. The results of Model (6) confirm the finding stated earlier that there is no statistically significant difference in the deterrent effect of detection between detection targets and their industry peers. Model (7) shows that compared to matched controls in remote industries, a detection event leads to significantly smaller runups in detection targets.

The magnitude of the runup is positively related to the variable *surprise*. The variable *size* is inversely related to the magnitude of the runup. A one standard-deviation increase in the natural log of firm size decreases the runup by between 0.8% and 1.2%.

[14] The number of observations from (4) and (5) do not add up to the number of observations in Model (3) because we do not include observations where the variable surprise is equal to zero. In addition, we use several alternative thresholds to define positive and negative surprises. The results remain robust, when we use 0.0001 (-0.0001), 0.001 (-0.001), or 0.005 (-0.005) as alternative thresholds to define positive (negative) surprises.

The coefficient of $Tobin's\,Q$ is positively associated with the magnitude of the runup. A one standard deviation increase leads to an increase in runups by between 0.5% to 0.7%. These findings are consistent with the notion that runups are higher in firms with higher asymmetric information. The coefficient of the variable *R&D* is only statistically significant at the 10% level in Model (3). In all other 5 specifications, the coefficient fails to be significant at conventional levels. *Return on assets* is inversely related to runups.

5.2 Source of leakage

The strength of the deterrent effect may be affected by the source of leakage. Trading ahead of earnings announcements is likely to stem from individuals who are closely related to the firm and have regular access to material, non-public information. We hypothesize that the deterrent effect is stronger when the caught individuals are closely related to the firm in which the insider trading occurred. This argument is based on the notion that individuals use experiences from their vicinity when updating their subjective probabilities of detection (see arguments in Section 2). Moreover, shareholders might be pressing to implement measures prohibiting future information leakages when they learn that there was an internal source of leakage. The regressions in Table 7 aim at testing whether the deterrent effect depends on the source of leakage. We build interaction terms between the dummy variable for the source of leakage and the variables *post detection*, $post\,detection * vicinity\,treatment$ and $post\,detection * treatment$.

We include the same control variables as in the previous regressions, however, for the sake of brevity we do not report the control variables in Table 7. Model (1) tests for differences between the cases where information has leaked as opposed to stolen information. All coefficients of the interaction terms with *stolen* are insignificant. Apparently, there is no significant difference between cases of stolen versus leaked information with respect to the deterrent effect. Model (2) tests whether there is a difference with respect to insider trading cases based on leakage from individuals who are employed by the firm (managers or employees). The deterrent effect on the stock of a detection target is significantly stronger for cases of information leakage from the inside. The size of the incremental effect is -0.5% as suggested by the coefficient of the variable $post\,detection * treatment * stolen$. In total, the deterrent effect on detection targets is -1.3% as opposed to -0.8% for industry peers. However, this difference is only statistically

Table 6: Impact on runups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Basic	Control variables	Firm-fixed effects	Positive surprise	Negative surprise	No distant controls	No close controls
Dep. var.: runup (-5,-1)	b/t	b/t	b/t	b/t	b/t	b/t	b/t
Post detection (d)	-0.001	0.001	0.001	0.003*	-0.001	-0.006***	0.001
	(-0.73)	(0.75)	(0.84)	(1.69)	(-0.46)	(-5.39)	(0.68)
Postdetection*vicinity treatment	-0.007***	-0.006***	-0.007***	-0.012***	-0.001		
	(-4.21)	(-3.92)	(-4.51)	(-5.78)	(-0.19)		
Postdetection*treatment	-0.000	0.001	0.001	0.002	-0.003	0.001	-0.007***
	(-0.10)	(0.68)	(0.37)	(1.17)	(-1.04)	(0.37)	(-4.19)
Vicinity treatment (d)	0.006***	0.005***					
	(3.83)	(3.40)					
Treatment (d)	-0.001	0.002					
	(-0.40)	(1.29)					
Surprise	0.009***	0.006***	0.005***	0.005***	0.005***	0.004***	0.005***
	(17.06)	(10.83)	(8.41)	(5.69)	(7.31)	(6.01)	(7.96)
Analyst dispersion	-0.001***	0.000	0.000	-0.000	0.000	0.001	-0.000
	(-3.02)	(0.21)	(0.34)	(-0.10)	(0.35)	(1.47)	(-0.26)
Size		-0.008***	-0.010***	-0.012***	-0.008***	-0.010***	-0.011***
		(-19.68)	(-11.34)	(-9.90)	(-5.27)	(-10.11)	(-9.31)
Tobin's Q		0.005***	0.007***	0.007***	0.006***	0.007***	0.006***
		(10.90)	(11.28)	(9.76)	(5.91)	(10.97)	(8.47)
R&D		0.001	-0.002*	-0.002	-0.002	-0.001	-0.001
		(1.14)	(-1.70)	(-1.32)	(-1.20)	(-1.26)	(-1.07)
Return on assets		-0.003***	-0.004***	-0.004***	-0.003**	-0.004***	-0.004***
		(-5.29)	(-4.66)	(-4.10)	(-2.22)	(-4.69)	(-4.21)
Obs	44304	43646	43646	23157	15361	29370	31377
R-squared	0.035	0.078	0.149	0.177	0.195	0.155	0.145
Adj. R-squared			0.131	0.145	0.146	0.135	0.128

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. Models (3) to (7) include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable *vicinity treatment* is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. *Size* is the natural logarithm of the market value of equity. *Tobin's Q* denotes the market value of equity and debt divided by the book value of the firm's assets. *R&D* is R&D expenditures scaled by the book value of total assets. *Return on assets* denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

significant at the 10% level.[15]

Model (3) tests the deterrent effect of cases involving professional service firms. We do not find any significant impact of the involvement of professional service firm employees on the deterrent effect with respect to detection targets or their peers. Model (4) tests whether there are differences between cases of crime of opportunity versus organized crime, i.e., cases where there has been trading on inside information with at least three different firms). The findings suggest that there are no significant differences among these two sources. In sum, there is only weak evidence that the source of leakage has an impact on the magnitude of the deterrent effect. The deterrent effect on the detection target seems to be slightly larger for episodes involving inside leakage.

5.3 Impact on firm cross-section

The deterrent effect is likely to be stronger for firms with a high ex ante risk of insider trading. The opportunity to make profits by trading ahead of price-sensitive announcements is larger for firms with high asymmetric information. Tables 8, 9 and 10 test whether the deterrent effect differs with respect to firm characteristics which are likely to be associated with asymmetric information, i.e., $size$, *R&D* and $Tobin's\,Q$.

First, we test whether there is an impact of firm size on the deterrent effect. In Model (1) of Table 8 we interact the difference-in-difference indicators with $size$, the natural logarithm of a firm's market value of equity. Model (2) interacts the indicators with dummy variables for small and large firms. We construct these dummies as follows: We sort the observations into terciles with respect to $size$. The results of Model (1) suggests that there is not any significant linear impact of the natural logarithm of size on the deterrent effect. The findings of Model (2) indicate that the runups of small firms are slightly larger in the post detection period for all firms in the sample, as suggested by the coefficient of $post\,detection * small$. However, the remaining interaction terms with the dummies for small and large firms are all insignificant.

Second, we analyze a potential impact of R&D on the deterrent effect. In an analogous manner to the procedure above, we interact a continuous measure for R&D, the R&D expenditures scaled by total assets, with the difference-in-differences indicators as well as binary variables for low and high R&D firms. To this end, we also sort firms into terciles

[15]In non-reported results, we break down the source of inside leakage further. We do not find any significant differences between the impact of employees, top managers, shareholders or directors as the source of leakage on the deterrent effect.

Table 7: Source of information leakage and the impact of runups

Dep. var.: runup (-5,-1)	(1) b/t	(2) b/t	(3) b/t	(4) b/t
post detection (d)	0.001	0.004*	-0.001	0.002
	(0.98)	(1.92)	(-0.48)	(1.29)
Post detection*vicinity treatment	-0.007***	-0.008**	-0.007***	-0.007***
	(-4.23)	(-2.56)	(-3.41)	(-3.08)
Postdetection*treatment	0.001	0.001	0.001	-0.000
	(0.45)	(0.55)	(0.29)	(-0.01)
Post detection*stolen	-0.002	-0.005	0.000	
	(-0.45)	(-1.06)	(0.00)	
Post detection*vicinity treatment*stolen	0.001	0.001	0.001	
	(0.16)	(0.18)	(0.13)	
Post detection*treatment*stolen	-0.002	-0.003	-0.002	
	(-0.48)	(-0.57)	(-0.42)	
Post detection*inside leakage		-0.005*		
		(-1.80)		
Post detection*vicinity treatment*inside leakage		0.000		
		(0.02)		
Post detection*treatment		-0.001		
		(-0.24)		
Post detection*profservice			0.007**	
			(2.42)	
Post detection*vicinity treatment*profservice			-0.001	
			(-0.32)	
Post detection*treatment*profservice			0.000	
			(0.02)	
Post detection*crime of opportunity				-0.002
				(-1.01)
Post detection*vicinity treatment*crime of opportunity				-0.001
				(-0.23)
Post detection*treatment*crime of opportunity				0.002
				(0.50)
Obs	46646	46646	46646	46646
R-squared	0.149	0.149	0.149	0.149
Adj. R-squared	0.131	0.131	0.131	0.131

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. All regressions include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable $vicinity\,treatment$ is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable $treatment$ denotes a subset of $vicinity\,treatment$. It is set to 1 only if there is a detection event in the firm. The dummy variable $post\,detection$ is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. Cases are classified as $stolen$ if the defendant has illegally gained access to the private information. $insideleakage$ denotes cases in which the trader or tipper is directly employed by the firm. $professional\,service\,firm$ includes employees of service firms with a fiduciary duty to the firm with the insider trading episode (management consulting firm, investment bank or law firm).$crime\,of\,opportunity$ if the defendants have exploited inside information with respect to one firm only. We also include the following control variables. For brevity, we do not report the variables in the table. $surprise$ is the absolute value of the difference between the actual earnings minus the median of analyst forecast scaled by the share price. $size$ is the natural logarithm of the market value of equity. $Tobin's\,Q$ denotes the market value of equity and debt divided by the book value of the firm's assets. $R\&D$ is R&D expenditures scaled by the book value of total assets. $return\,on\,assets$ denotes earnings before taxes and interest before depreciation divided by the book value of total assets. $Surprise$ is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable $analyst\,dispersion$ is equal to the standard deviation of analyst forecasts. $Size$ is the natural logarithm of the market value of equity. $Tobin's\,Q$ denotes the market value of equity and debt divided by the book value of the firm's assets. $R\&D$ is R&D expenditures scaled by the book value of total assets. $Return\,on\,assets$ denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

according to their R&D expenditures. When we include the R&D variables in the analysis, the treatment effect of detection on detection targets and their peers disappears. In Model (1) of Table 9, the post detection runups are significantly smaller for all firms, i.e., for detection firms, their peers and firms in remote industry. This effect seems to increase with the R&D intensity, as indicated by the interaction variable $post\,detection * R\&D$. In Model (2), we do not observe any significant impact of the difference-in-differences indicators or their interactions with R&D. However, in both models the effect of detection on the post detection runups of firms in the *vicinity treatment* group disappears when we include interactions with $R\&D$.

Third, we investigate whether $Tobin's\,Q$ affects the deterrent effect. In Model (1) of Table 10 we interact $Tobin's\,Q$ with the difference-in-differences indicators. We do not observe any evidence for a linear impact of this variable on the deterrent effect. Again, we sort firms into terciles according to their $Tobin's\,Q$ and form interaction variables of the top and bottom tercile with the difference-in-differences indicators. According to the results of Model (2), the deterrent effect is less pronounced for firms with a low value of $Tobin's\,Q$. This effect applies to the group of vicinity firms, i.e., the detection targets and their industry peers. Typically those firms have small growth prospects and, as a result, a rather low degree of information asymmetry. Apparently, the deterrent effect is smaller in environments with a rather low level of asymmetric information, because the opportunities for exploiting asymmetric information are smaller as compared to firms with medium or high levels of information asymmetry.[16]

6 Robustness

6.1 Fraction as an alternative measure

We use an alternative approach to measure insider trading prior to earnings announcements. As an alternative measure for information leakage prior to the public announcement we calculate the variable $fraction$ as the runup prior to the announcement (CAR(-10,-1)/CAR(-10,2)), similar to the approach by Ackerman et al. (2008). This variable seeks to measure the fraction of information which has been impounded into prices before the announcement relative to the total price effect of the earnings announcements. When fraction is smaller than 0, we set it to 0 and when it is larger than 1, we set it equal to 1.

[16] When we repeat the analyses above by sorting into quintiles instead of terciles, we find similar results. The results are not reported here but available upon request.

Table 8: Firm size and the impact of runups

Dep. var.: runup (-5,-1)	(1) runup (-5,-1) b/t	(2) runup (-5,-1) b/t
Post detection (d)	0.001	-0.001
	(0.69)	(-0.70)
Postdetection*vicinity treatment	-0.007***	-0.006**
	(-4.38)	(-2.56)
Postdetection*treatment	-0.000	0.000
	(-0.26)	(0.13)
Post detection*size	-0.002	
	(-1.20)	
Post detection*vicinity treatment*size	0.003	
	(1.30)	
Post detection*treatment*size	0.001	
	(0.90)	
Post detection*small		0.005*
		(1.84)
Post detection*vicinity treatment*small		-0.005
		(-1.23)
Post detection*treatment*small		-0.001
		(-0.24)
Post detection*large		0.002
		(0.98)
Post detection*vicinity treatment*large		0.000
		(0.06)
Post detection*treatment*large		0.000
		(0.07)
Surprise	0.005***	0.005***
	(8.46)	(8.39)
Analyst dispersion	0.000	0.000
	(0.33)	(0.35)
Size	-0.010***	-0.010***
	(-11.38)	(-11.38)
Tobin's Q	0.007***	0.007***
	(11.27)	(11.20)
R&D	-0.002	-0.002
	(-1.56)	(-1.57)
Return on assets	-0.004***	-0.004***
	(-4.64)	(-4.67)
Obs	43646	43646
R-squared	0.149	0.149
Adj. R-squared	0.131	0.131

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. All Models include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable *vicinity treatment* is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. *Size* is the natural logarithm of the market value of equity. *Small* (*large*) is a dummy variable which is set to 1 if the firm is in the bottom (top) tercile in terms of size. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. $Tobin's\,Q$ denotes the market value of equity and debt divided by the book value of the firm's assets. *R&D* is R&D expenditures scaled by the book value of total assets. *Return on assets* denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

Table 9: R&D and the impact of runups

Dep. var.: runup (-5,-1)	(1) runup (-5,-1) b/t	(2) runup (-5,-1) b/t
Post detection (d)	-0.004**	-0.001
	(-2.23)	(-0.42)
Postdetection*vicinity treatment	-0.002	-0.004
	(-1.10)	(-1.14)
Postdetection*treatment	0.001	0.002
	(0.62)	(0.85)
Post detection*R&D	-0.009***	
	(-3.55)	
Post detection*vicinity treatment*R&D	0.004	
	(1.33)	
Post detection*treatment*R&D	0.001	
	(0.30)	
Post detection*low R&D		0.003
		(0.88)
Post detection*vicinity treatment*low R&D		0.001
		(0.19)
Post detection*treatment*low R&D		-0.002
		(-0.49)
Post detection*high R&D		-0.005
		(-0.81)
Post detection*vicinity treatment*high R&D		-0.001
		(-0.14)
Post detection*treatment*high R&D		-0.003
		(-0.67)
Surprise	0.005***	0.005***
	(8.54)	(8.40)
Analyst dispersion	0.000	0.000
	(0.28)	(0.27)
Size	-0.010***	-0.010***
	(-11.17)	(-11.24)
Tobin's Q	0.007***	0.007***
	(11.08)	(10.82)
R&D	0.000	-0.001
	(0.03)	(-1.44)
Return on assets	-0.004***	-0.004***
	(-4.48)	(-4.47)
Obs	43646	43646
R-squared	0.150	0.150
Adj. R-squared	0.132	0.132

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. All Models include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable *vicinity treatment* is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. *R&D* is R&D expenditures scaled by the book value of total assets. *High R&D* (*low R&D*) is a dummy variable which is set to 1 if the firm is in the top (bottom) tercile in terms of R&D expenditures. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. *Size* is the natural logarithm of the market value of equity. *Tobin's Q* denotes the market value of equity and debt divided by the book value of the firm's assets. *Return on assets* denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

Table 10: Tobin's Q and the impact of runups

Dep. var.: runup (-5,-1)	(1) runup (-5,-1) b/t	(2) runup (-5,-1) b/t
post detection (d)	0.000	0.001
	(0.04)	(0.82)
Postdetection*vicinity treatment	-0.007***	-0.010***
	(-3.76)	(-4.79)
Postdetection*treatment	0.001	0.002
	(0.40)	(0.81)
Post detection*Tobin's Q	-0.003	
	(-1.62)	
Post detection*vicinity treatment*Tobin's Q	-0.000	
	(-0.13)	
Post detection*treatment*Tobin's Q	-0.001	
	(-0.57)	
Post detection*low Q		-0.000
		(-0.22)
Post detection*vicinity treatment*low Q		0.007**
		(2.38)
Post detection*treatment*low Q		-0.002
		(-0.57)
Post detection*high Q		-0.000
		(-0.03)
Post detection*vicinity treatment*high Q		-0.001
		(-0.33)
Post detection*treatment*high Q		-0.001
		(-0.52)
Surprise	0.005***	0.005***
	(8.39)	(8.29)
Analyst dispersion	0.000	0.000
	(0.35)	(0.35)
Size	-0.010***	-0.010***
	(-10.88)	(-10.74)
Tobin's Q	0.008***	0.007***
	(11.96)	(11.28)
R&D	-0.002	-0.002
	(-1.48)	(-1.53)
Return on assets	-0.004***	-0.004***
	(-4.48)	(-4.42)
Obs	43646	43646
R-squared	0.149	0.149
Adj. R-squared	0.132	0.131

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. All Models include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable $vicinity\,treatment$ is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable $treatment$ denotes a subset of $vicinity\,treatment$. It is set to 1 only if there is a detection event in the firm. The dummy variable $post\,detection$ is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. $Tobin's\,Q$ denotes the market value of equity and debt divided by the book value of the firm's assets. $High\,Tobin's\,Q$ ($low\,Tobin's\,Q$) is a dummy variable which is set to 1 if the firm is in the top (bottom) tercile in terms of $Tobin's\,Q$. $Surprise$ is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable $analyst\,dispersion$ is equal to the standard deviation of analyst forecasts. $Size$ is the natural logarithm of the market value of equity. $R\&D$ is R&D expenditures scaled by the book value of total assets. $Return\,on\,assets$ denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

The rationale for this robustness check stems from the critique that the observed deterrent effect may not be due to less insider trading activity, but due to a smaller information content of earnings announcements in general. Scaling the runup by a measure for the overall information content, which in our case is approximated by the abnormal return in the time window from -10 to 2 days around the earnings announcement, addresses this objections.

Table 11 shows the results from the regressions. We use the same model specifications as before with the difference-in-differences indicators, characteristics of the earnings announcement and firm characteristics as independent variables. We merely exchange the dependent variable and use $fraction$ instead of the runup. The coefficient of the variable $post\,detection * vicinity$ is negative and statistically significant. The effect remains robust to the inclusion of control variables in Model (2) and the inclusion of firm-fixed effects in Model (3). Post detection the fraction of the market response impounded already before the announcement is reduced by between -3.6% and -3.1%. This effect prevails for the stock of the detection firm as well as the stocks of firms in the direct neighborhood, that is industry peers. There is no significant difference between the direct effect on the detection and the indirect spillover effect on peers.

6.2 Alternative pre-event windows

We use alternative event windows prior to the earnings announcement day to calculate the runup: (-10,-1), (-3,-1) and (-2,-1). Table 12 reports the results of the main regression with firm-fixed effects for these alternative event windows. In sum, the results remain robust to alternative pre-event windows. The coefficient of the variable $post\,detection * vicinity\,treatment$ is statistically significant at the 1% level in all six regressions. The economic magnitude ranges from -1.1% for the (-10,-1) window to -0.5% for the (-3,-1) window.

6.3 Excluding acquired firms

In several cases, the firm on whose stock the insider trading occurred is acquired by another firm in between the insider trading event and the detection. Earnings announcement runups cannot be calculated if the stock has been delisted. Instead, in the analyses above we include the acquiring firm in the sample and compare their earnings announcement runups before and after the detection event. In Model (1) of Table 13 we exclude

Table 11: Information impounded before the announcement

Dep. var.: Fraction of runup	(1) Pooled b/t	(2) Pooled b/t	(3) Pooled b/t
post detection (d)	-0.053***	-0.053***	-0.041***
	(-5.43)	(-5.43)	(-3.71)
postdetection*vicinity treatment	-0.036***	-0.031**	-0.031**
	(-2.59)	(-2.22)	(-2.17)
postdetection*treatment	0.004	0.002	0.016
	(0.29)	(0.13)	(1.20)
vicinity treatment (d)	0.023**	0.025**	
	(2.39)	(2.46)	
treatment (d)	0.004	0.013	
	(0.46)	(1.37)	
Surprise	0.018***	0.009***	-0.005
	(5.32)	(2.67)	(-1.59)
Analyst dispersion	-0.003	-0.002	-0.002
	(-1.49)	(-0.72)	(-0.94)
Size		-0.019***	-0.101***
		(-5.30)	(-13.56)
Tobin's Q		-0.004	0.025***
		(-1.39)	(6.38)
R&D		-0.004	-0.017**
		(-0.90)	(-2.40)
Return on assets		-0.015***	0.004
		(-3.85)	(0.74)
Obs	46492	45783	45783
R-squared	0.010	0.014	0.079
Adj. R-squared			0.060

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. Model (3) includes firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. *fraction* is defined as CAR(-10,-1) divided by CAR(-10,2). The dummy variable *vicinity treatment* is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. *Size* is the natural logarithm of the market value of equity. *Tobin's Q* denotes the market value of equity and debt divided by the book value of the firm's assets. *R&D* is R&D expenditures scaled by the book value of total assets. *Return on assets* denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

Table 12: Alternative pre-event windows

Dep. var.: runup	(1) (-10,-1) b/t	(2) (-3,-1) b/t	(3) (-2,-1) b/t
Post detection (d)	0.002	-0.000	0.001
	(1.22)	(-0.09)	(0.80)
Postdetection*vicinity treatment	-0.011***	-0.005***	-0.006***
	(-4.74)	(-3.93)	(-5.09)
Postdetection*treatment	0.003	0.001	0.002**
	(1.18)	(0.49)	(2.05)
Surprise	0.008***	0.004***	0.003***
	(10.80)	(8.67)	(8.30)
Analyst dispersion	-0.000	0.000	-0.000
	(-0.48)	(0.01)	(-0.46)
Size	-0.013***	-0.009***	-0.008***
	(-9.97)	(-11.49)	(-12.93)
Tobin's Q	0.009***	0.006***	0.005***
	(10.81)	(12.87)	(13.42)
R&D	-0.003**	-0.002**	-0.002***
	(-2.04)	(-2.14)	(-3.45)
Return on assets	-0.007***	-0.003***	-0.003***
	(-5.43)	(-4.95)	(-5.85)
Obs	43645	43649	43650
R-squared	0.153	0.148	0.146
Adj. R-squared	0.135	0.130	0.128

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. All Models include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable *runup* is the abnormal return of the firm's stock prior to the announcement of the earnings announcement over different windows prior to the announcement date at t: from t-10 to t-1, t-3 to t-1 and t-2 to t-1. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable *vicinity treatment* is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. *Size* is the natural logarithm of the market value of equity. *Tobin's Q* denotes the market value of equity and debt divided by the book value of the firm's assets. *R&D* is R&D expenditures scaled by the book value of total assets. *Return on assets* denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

the cases of acquired firms. The results are very similar to the basic results. The coefficient of the dummy $post\,detection * vicinity\,treatment$ is -0.7% and is statistically significant at the 1% level.

6.4 Excluding M&A or fraud

In Model (2) of 13 we exclude the cases from the sample where the insider trade took place in advance of the announcement of M&A events. Model (3) excludes cases of trading ahead of the announcement of corporate fraud. The deterrent effect of detection on detection targets and their peers remains robust to these subsamples.

6.5 Alternative control sample

In the basic specifications, we only include one matched control firm for each treatment firm. In Model (4) we include up to 3 control firms from the same industry (according to the 3-digit SIC code). From the 10 firms in the same 3-digit SIC code, we pick up to 3 firms (if available) which are closest in terms of book-to-market ratio. The results remain robust to this alternative specification. In Model (5) we select the matched sample of industry peers using a slightly altered matching algorithm. We select the closest firm in terms of book-to-market ratio from the five firms which are closes in terms of size. The main effect also prevails in this sample.

6.6 Minimum number of analysts

Lastly, Model (6) of Table 13 only includes earnings announcements in the sample where there are at least 5 analyst forecasts for the quarterly earnings announcement available. The precision of analyst consensus as a proxy for market expectations is likely to increase with the number of analysts who are analyzing the firm and issuing forecasts. The results remain robust to this subsample. The results also remain robust to different thresholds which are not reported here, such as 3 or 7 analyst forecasts.

7 Discussion

7.1 Mechanical relationship

One may object that the observed drop in the post detection runups is purely mechanical for the following reason: It could be argued that the pre detection runups are greater

Table 13: Alternative robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Excl. acquired	Excl. fraud	Excl. M&A	Up to 3 controls	Alternative matching	Min. 5 analysts
Dep. var.: runup (-5,-1)	b/t	b/t	b/t	b/t	b/t	b/t
Post detection (d)	0.001	0.001	0.001	0.001	0.001	-0.002
	(0.68)	(0.71)	(0.69)	(0.86)	(0.63)	(-0.84)
Postdetection*vicinity treatment	-0.007***	-0.007***	-0.009***	-0.007***	-0.009***	-0.006*
	(-4.07)	(-4.21)	(-4.61)	(-4.75)	(-4.25)	(-1.73)
Postdetection*treatment	-0.002	0.001	0.001	0.000	0.002	0.002
	(-0.88)	(0.45)	(0.49)	(0.09)	(1.06)	(0.72)
Surprise	0.005***	0.005***	0.004***	0.005***	0.005***	0.007***
	(7.03)	(7.78)	(5.70)	(10.13)	(9.22)	(6.39)
Analyst dispersion	0.000	0.000	0.000	0.000	0.000	0.000
	(0.45)	(0.55)	(0.44)	(0.03)	(0.04)	(0.91)
Size	-0.011***	-0.010***	-0.012***	-0.011***	-0.010***	-0.008***
	(-10.32)	(-11.19)	(-10.34)	(-13.67)	(-9.69)	(-4.92)
Tobin's Q	0.007***	0.007***	0.007***	0.007***	0.006***	0.006***
	(10.30)	(11.26)	(9.09)	(13.66)	(8.68)	(6.96)
R&D	-0.002	-0.002	-0.002	-0.003***	-0.001	-0.001
	(-1.45)	(-1.56)	(-1.60)	(-3.14)	(-0.90)	(-0.34)
Return on assets	-0.003***	-0.004***	-0.003***	-0.004***	-0.004***	-0.004***
	(-3.45)	(-4.74)	(-3.57)	(-5.58)	(-4.29)	(-3.49)
Obs	35379	43698	29994	64378	40316	22758
R-squared	0.149	0.148	0.146	0.147	0.150	0.156
Adj. R-squared	0.130	0.130	0.127	0.129	0.132	0.131

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. All Models include firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), we reverse the sign of the abnormal return. The dummy variable *vicinity treatment* is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable *treatment* denotes a subset of *vicinity treatment*. It is set to 1 only if there is a detection event in the firm. The dummy variable *post detection* is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. *Surprise* is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable *analyst dispersion* is equal to the standard deviation of analyst forecasts. *Size* is the natural logarithm of the market value of equity. *Tobin's Q* denotes the market value of equity and debt divided by the book value of the firm's assets. *R&D* is R&D expenditures scaled by the book value of total assets. *Return on assets* denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

simply because they include the insider trading episode. First, a pure mechanical relationship is unable to explain why we observe a significant drop in the runups of industry peers. Second, if this criticism holds and the observed relationship was purely mechanical, then the effect would disappear if we exclude the runups from insider trading episodes from our sample. Only 23.1% of the insider trading episodes in our sample include earnings announcements as the event on which material, non-public information has been exploited. In Table 14 we report the results of the difference-in-differences regression in which we exclude all insider trading episodes involving earnings announcements.[17] The results suggest that the treatment effect on detection targets and their industry peers persists in this subsample. This finding casts doubt on the objection that the treatment effect is purely mechanical.

7.2 Alternative explanations

The main hypothesis developed in Section 2 is that a reduction in trading ahead of earnings announcements is caused by the update of subjective detection probabilities of individuals with access to private information. One could argue that there are alternative factors which drive the main findings. SEC detection could raise the shareholders' concerns about adverse selection costs. As a consequence, they could exert pressure on the management to improve efforts to prevent the exploitation of inside information. There are two ways in which firms could respond to those demands: improve the dissemination of information to the market or improve internal compliance mechanisms.

After a detection event firms may improve their disclosure of information to the market which leads to more accurate prices to begin with. Under this assumption, the decrease in the price runups is due to more informative prices. First, although the channel through which SEC detection affects insider trading is different, this alternative explanation is still consistent with the interpretation of less insider trading activity. Second, improved dissemination of information to the market implies that analysts should make more accurate forecasts. This implication is empirically testable. We compare analyst dispersion and accuracy of analyst forecasts for the vicinity treatment sample (detection targets and their industry peers) pre and post detection in Table 15. Post

[17] It would actually be sufficient to exclude the runups for the quarterly earnings announcement which were involved in a detected insider trading episode. However, the SEC litigation releases do not always provide precise information about the respective quarter. As an alternative approach, we therefore eliminate all observations (detection firms and also their matched control firms) for which the respective insider event type is earnings announcements.

Table 14: Impact on runups excluding earnings announcements

Dep. var.: runup (-5,-1)	(1) b/t
Post detection (d)	0.001
	(0.58)
Postdetection*vicinity treatment	-0.006***
	(-3.42)
Postdetection*treatment	0.000
	(0.13)
Surprise	0.005***
	(7.78)
Analyst dispersion	0.000
	(0.32)
Size	-0.010***
	(-9.83)
Tobin's Q	0.006***
	(8.99)
R&D	-0.001
	(-1.12)
Return on assets	-0.004***
	(-3.96)
Obs	32446
R-squared	

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the results of the difference-in-differences regression. The Model includes firm-fixed effects. Standard errors are clustered at the firm level. All variables except for dummy variables are winsorized at the 2% level and standardized. The dependent variable $runup\,(-5,-1)$ is the abnormal return of the firm's stock prior to the announcement of the earnings announcement from t-5 to t-1 prior to the announcement date at t. For the observations in which the earnings fall short of the analyst forecasts (negative surprise), I reverse the sign of the abnormal return. The dummy variable $vicinity\,treatment$ is equal to 1 if there is a detection event in the firm or if the firm is a matched industry peer of a firm with a detection event. The dummy variable $treatment$ denotes a subset of $vicinity\,treatment$. It is set to 1 only if there is a detection event in the firm. The dummy variable $post\,detection$ is set to 1 if the earnings announcement occurs after a detection event in the stock or 0 otherwise. $Surprise$ is the absolute difference between the actual earnings and the median forecast scaled by the share price. The variable $analyst\,dispersion$ is equal to the standard deviation of analyst forecasts. $Size$ is the natural logarithm of the market value of equity. $Tobin's\,Q$ denotes the market value of equity and debt divided by the book value of the firm's assets. $R\&D$ is R&D expenditures scaled by the book value of total assets. $Return\,on\,assets$ denotes earnings before taxes and interest before depreciation divided by the book value of total assets.

detection we observe an increase of the average number of analyst by 1.3. The difference is statistically significant at the 1%. We observe a greater dispersion among analysts' opinions after a detection event (difference in means: 1.2%). Further, analyst forecasts seem to be less accurate post detection as suggested by a significant increase in the magnitude of surprises (difference in means: 0.3%).[18] These observations cast doubt on the notion that firms have improved their communication with the market after a detection event.

Table 15: Analyst estimates pre and post detection

Variable	Mean	St. Dev.	Median	95% quantile	5% quantile
Panel A: vicinity treatment pre detection					
Number of analyst estimates	7.466	6.628	5	21	1
Std. dev. of estimate	0.026	0.042	0.010	0.1	0
Surprise	0.006	0.016	0.001	0.027	0
Positive surprise	0.004	0.008	0.001	0.019	0.000
Negative surprise	-0.008	0.015	-0.002	0.000	-0.052
Panel B: vicinity treatment post detection					
Number of analyst estimates	8.814	7.136	7	23	1
Std. dev. of estimate	0.038	0.051	0.020	0.15	0
Surprise	0.009	0.020	0.002	0.048	0
Positive surprise	0.006	0.009	0.002	0.030	0.000
Negative surprise	-0.012	0.018	-0.004	0.000	-0.055
Difference in means					
Number of analyst estimates	1.347***				
Std. dev. of estimate	0.012***				
Surprise	0.003***				
Positive surprise	0.002***				
Negative surprise	-0.004***				

This table shows descriptive statistics of analyst estimates of quarterly earnings announcement for the sample of firms with detection events before and after the detection event. *Number of analyst estimates* is the number of analysts who issue a forecast for a quarterly earnings announcement. *Analyst dispersion* is the standard deviation of the analyst forecasts with respect to one earnings announcement. *surprise* is the absolute value of the difference between the actual earnings minus the median of analyst forecast scaled by the share price. *Positive surprise* (*negative surprise*) is the difference between the actual earnings minus the median of analyst forecasts scaled by the share price if the difference is strictly positive (negative). *, **, *** denote statistical significance at the 10%, 5% or 1% level respectively based on a t-test with unequal variances.

Moreover, the firm subject to a detection event could improve efforts to prevent insider trading through internal policies. Examples for better prevention are limiting the access to confidential information, requiring pre clearance of any transaction by the compliance department or increasing awareness by issuing a code of conduct. These actions are likely to lead to reductions in insider trading activities. The consequences of an up-

[18]When we compare the figures for the treatment group, we find comparable results: The number of analysts increases by 1.4, the standard deviation of the analyst forecasts increases by 1.2% and the magnitude of surprises increases by 0.3%.

date of the subjective detection probability and improved internal control mechanisms are observationally equivalent. We could disentangle these two explanations if we had information on changes in internal compliance mechanisms subsequent to the detection event. This information is not publicly available and costly to collect. Based on the analysis in this paper, we cannot disentangle an update in probabilities from improved compliance mechanisms.

7.3 Representativeness

One may object that trading ahead of earnings announcements is not representative for the general amount of insider trading in a given stock. We do not account for a complete picture of all insider trading activities in a given firm. Insider trading can take place well in advance of the announcement of price-sensitive information and it also can take place with respect to many different types of news events. A comprehensive analysis of insider trading based on an exhaustive set of news announcements is difficult because of the high frequency of news production. With respect to many types of those news (such as personnel changes or investment activities) it is difficult to assess the value implications ex ante and to construct a proxy for market expectations. Therefore, it is difficult to jointly analyze all different types of news announcements. Our analysis just captures a small aspect by focusing on the trading ahead in the 10 days prior to an earnings announcement.

Further, one can criticize that post detection individuals with private information trade smarter in such a way that they reduce the market impact of their transactions. E.g., the individuals who seek to exploit private information place their trades in smaller chunks or over a larger period of time. The observation of smaller runups before earnings announcement after a detection event may not be due to actual decreases insider trading but differences in the strategies to exploit private information. This potential change in insider trading strategies cannot be observed. Our results are, hence, limited to the finding SEC detection reduces insider trading activities prior to earnings announcements.

8 Conclusion

The present paper explores the consequences of the public detection of insider trading by the SEC. Using runups prior to earnings announcement as a measure of insider trading activity, we compare insider trading before and after the event of SEC detection using a difference-in-differences approach. We hypothesize that SEC detection leads to an update of the subjective probabilities of getting caught among individuals in the vicinity of the defendants. As a consequence of this update, expected cost of insider trading increase which reduces the observed level of insider trading.

In sum, we find that post detection earnings announcement runups prior are significantly lower. Subsequent to detection, runups over the time window of t-5 to t-1 around the earnings announcement are reduced by 0.7% for firms with a detection event and their industry peers. We do not find any significant difference between the impact on the detection targets and impact on their peers. There is weak empirical support that the effect on the detection targets is stronger for cases of inside leakage. This finding corroborates the hypothesis that individuals base their subjective probabilities on experiences gained by themselves and their close vicinity. The empirical results support the hypothesis that individuals with the ability to trade on private information update their subjective detection probabilities after they observe an SEC detection event in their vicinity. As a result, they are less likely to exploit their possession of material, non-public information. Our results portray a positive picture of SEC enforcement by indicating that enforcement actions against insider trading are effective in reducing future insider trading activities.

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