

# Shadow Trading

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## ***Abstract***

Private information held by firm insiders is likely to be value-relevant for business partners and competitors. We investigate whether firm insiders attempt to exploit their stakeholder-relevant private information in order to circumvent regulatory scrutiny against conventional insider trading. Empirical tests show that prior to a given firm's ("source firm") release of private information, business partners or competitors ("linked firms") experience increases in symptoms of informed trading activity, an activity we characterize as "shadow trading." We use a plausibly exogenous shock to firm insider incentives to engage in conventional insider trading to document that the mechanism underlying shadow trading is consistent with information leakage from source firm employees rather than alternative explanations. Additional analyses show that linked firms display fewer symptoms of shadow trading when source firms have explicit restrictions against shadow trading. Our study is relevant for legislators and regulators seeking to promote confidence in the integrity of capital markets.

*Key Words:* Business Partners; Informed Trading; Private Information; Supply Chain

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## 1. Introduction

Firms are linked to each other through both explicit and implicit relationships. Because of these relationships, information from a given firm is likely to be relevant for linked firms. A growing body of research examines the mechanisms that facilitate inter-firm information transfers such as the strength of the economic bonds (Pandit et al., 2011), the amount of investor attention (Cohen and Frazzini, 2008; Menzly and Ozbas, 2010), financial statement comparability (Wang, 2014), and trade flows (Aobdia et al., 2014). A common theme across much of this research is the focus on information transfers *subsequent* to the disclosure of private information.

Our study focuses on information transfers that occur *prior* to the disclosure of private information and in particular, a specific mechanism that facilitates these information transfers: leaks by employees. Because employees face substantial regulatory scrutiny regarding their trading or leaking activities with respect to their *own* firms,<sup>1</sup> we argue that they can leak information about economically-linked firms to exploit their private information while circumventing regulatory scrutiny. We label this activity “shadow trading”. The legality of shadow trading appears to be relatively untested because of the lack of a clear breach of fiduciary responsibility. Consistent with this argument, there are few cases of prosecutions against shadow trading.<sup>2</sup>

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<sup>1</sup> Employees or their tippees face substantial penalties for trading on inside information. Unlike other offences, U.S. lawmakers have not explicitly defined illegal insider trading. Rather, U.S. Supreme Court rulings have drawn a narrow, common law-based definition of illegal insider trading (Pritchard, 2016). For instance, United States v. O’Hagan (1997) clarifies that trading using private information is legal but constitutes an illegal activity when there is a breach of fiduciary responsibility.

<sup>2</sup> In a rare case against shadow trading, in 2011 the SEC brought action against employees of Flextronics for leaking information about business partner component orders (including Apple Inc. and Research in Motion) to hedge fund managers. In a related case, the SEC and DOJ initiated enforcement actions against mid-level managers for leaking information about their business partners to Primary Global Research LLC, an competitive intelligence firm. Two associated indictments against insider trading based on the use of confidential business partner data include actions against executives at Advanced Micro Devices (NASDAQ: AMD) and Taiwan Semiconductor Manufacturing Company (NYSE: TSM) (<http://www.sec.gov/news/press/2011/2011-38.htm>). In this context, sophisticated investors appear to experience limited government scrutiny or repercussions from shadow trading.

We examine whether shadow trading represents a material channel through which information transfers occur, investigate the source of shadow trading, and consider how firms can limit shadow trading. Throughout the paper, we refer to a “source firm” as a company from which private information potentially emerges and a “linked firm” as a business partner or competitor for whom the information could be price-relevant.

Using multiple measures of informed trading from the literature to measure shadow trading, we find evidence that trading in linked firms is statistically and economically related to the magnitude of multiple types of *subsequent* information shocks from source firms, including earnings announcements, merger and acquisition (M&A) announcements and new product announcements. First, for earnings announcement, we find that a one standard deviation increase in the magnitude of a source firm’s earnings announcement surprise is associated with a 6.4%-19.2% increase in shadow trading activity in the linked firm during the 30 days prior to the source firm’s earnings announcement.

Next, we also find evidence of shadow trading leading up to M&A and new product announcements. In economic terms, a one standard deviation increase in the magnitude of a source firm’s M&A (new product) announcement surprise is associated with a 3.1%-12% (4%-10.5%) increase in business partner linked firms’ trading activity in the 30 days prior to the source firm’s M&A (new product) announcement.

To further assess whether shadow trading is actually profitable, we examine whether shadow trading is associated with linked firm subsequent stock returns. We document that across our proxies, shadow trading is statistically associated with linked firm stock returns after controlling for various measures of linked firm mispricing.

Next, we examine the source of shadow trading. We argue that shadow trading is attributable to source firm employees who leak linked firm-relevant private information they

possess to professional investors or competitive intelligence firms who compile and sell this information to investors. An alternative (and less nefarious) possibility is that our results simply capture trades by sophisticated investors who use proprietary methods to acquire private information about firms in anticipation of information events (e.g., McNichols and Trueman, 1994; Kim and Verrecchia, 1997; Bushee and Goodman, 2007; Drake et al., 2012). Yet another possibility is that shadow trading simply represents unobserved market structure effects such as short-selling constraints (e.g., Khan and Lu, 2013; Akbas et al., 2015)

To differentiate among these explanations, we create a novel insider trading enforcement index to exploit unanticipated spikes in regulatory and media attention over *conventional* insider trading. Following these events, source firm insiders likely have incentives to switch from conventional insider trading to shadow trading to avoid prosecution but continue to monetize their private information. This is consistent with arguments that extensive media coverage and perceived scrutiny against insider trading including jail terms appear to act as an effective deterrent against insider trading (Bobelian, 2012). However, the events should not have any effects on sophisticated investors' non-shadow trading related information gathering efforts or market frictions.

Empirical findings indicate significant increases in shadow trading following periods of increased media attention to insider trading. In economic terms, shadow trading increases by 5.1% - 36.1% in the three-month window following increased media attention on insider trading relative to the three-month window prior to the increased media attention.

To further establish that insiders are the mechanism underlying shadow trading, we undertake a second test that exploit a different series of events: state-level adoptions or rejections of a legal doctrine known as the inevitable disclosure doctrine (IDD). The IDD affects the ability of in-state firms to legally prevent employees that are privy to trade secrets from seeking

employment with competitor firms. A shift in the IDD therefore affects source firm employees' incentives to engage in shadow trading because of changes in the opportunities to use their human capital to obtain lucrative employment contracts with competitor firms. Empirical findings from difference-in-differences tests indicate that in the year after source firms encounter an IDD shock, linked firms experience an 8.6%-20% incremental change in shadow trading relative to linked firms with unaffected source firms.

In sum, the findings from both the insider trading attention and IDD tests are consistent with the explanation that shadow trading is attributable to leakage by source firm employees rather than other explanations such as superior information processing by sophisticated investors or market structure characteristics. Under the latter explanations, we would not expect to observe any systematic changes in shadow trading around the shocks.

We next examine whether firms can self-police shadow trading. Firms have incentives to prevent their employees from engaging in shadow trading if the public revelation of such activities adversely affects their business relationships. For a subsample of our data for which we can observe corporate policy manuals, we investigate and find significant heterogeneity in ethics policies against shadow trading. Empirical tests indicate that firm-mandated prohibitions against shadow trading are effective: the magnitude of shadow trading activity in linked firms is significantly larger when source firms *do not* explicitly prohibit employees from engaging in shadow trading relative to when the source firms do explicitly prohibit shadow trading.<sup>3</sup> Although largely untested in U.S. courts, such company regulations arguably create a fiduciary responsibility for employees to not exploit their private information.

Finally, we examine characteristics that are likely to affect the prevalence of shadow

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<sup>3</sup> As the public disclosure of corporate rules potentially represents a conscious choice made by a firm, this test does not allow us to draw inferences about the veracity of such rules but only the decision to create such a rule.

trading. First, we examine and find that shadow trading in linked firms is negatively associated with source firm corporate governance quality and insider ownership. Second, we find that shadow trading is positively associated with the source firm's proximity to a major urban center. Finally, we find that shadow trading is pronounced (dampened) when source firms have higher (lower) transitional (long-term) investor ownership.

Our paper makes a number of contributions. The first is to a literature examining the mechanisms used to facilitate information transfers across firms. We document some of the first evidence about a largely unexplored channel for information transfers that occurs *prior* to the information being publicly disclosed: employees. In this vein, our study is an important addition to prior research that largely focuses on information transfers *following* the release of private information (see Section 2 for more details). Furthermore, our findings highlight the importance of considering private communication when examining information transfers.

Second, our study shows that information transfers that occur prior to the release of private information have asset pricing effects for *other* linked firms. We highlight that in the absence of shadow trading, sophisticated investors are still likely to discover value relevant information about linked firms. However, our findings suggest that shadow trading increases the speed and depth at which value relevant information is impounded into linked firms' stock prices. In this context, shadow trading potentially captures an important mechanism through which sophisticated investors obtain private information to help investment decisions.

Third, our study is also relevant for both regulators and academic literature on insider trading.<sup>4</sup> Regulations and enforcement against illegal insider trading are designed to ensure

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<sup>4</sup> An incomplete list include Bushman and Indjejikian (1995), Aboody and Lev (2000), Bettis et al. (2000), Ke et al. (2003), Roulstone (2003), Cheng and Lo (2006), Huddart et al. (2007), Jagolinzer (2009), Jagolinzer et al. (2011), and Khan and Lu (2013).

confidence in the integrity of capital markets (e.g., see Bushman et al., 2005). Accordingly, SEC regulators note that “Insider trading is legally forbidden. It is morally wrong. And it is economically dangerous.” and that the SEC has “...zero tolerance for the crime of insider trading” (Levitt, 1998). In light of these regulations against illegal insider trading, many academic studies examine how employees with private information opportunistically leak or use that information to trade in their *own* firms. Our study complements this literature by documenting that employees with private information profitably exploit their private value-relevant information to facilitate trading in *other* firms. Given that most insider trading prosecution efforts focus on own-firm insider trading (Alldredge and Cicero, 2015) or trading by finance professionals (Acharya and Johnson, 2010), our results suggest a need for further evaluation of the completeness of insider trading regulations and the allocation of prosecution resources.

The rest of this paper proceeds as follows. Section 2 present a literature review. Section 3 outlines the sample, empirical methodology, and descriptive statistics. In Section 4 we present multivariate results and robustness tests and in Section 5 we present tests to identify the mechanism for shadow trading. In Section 6, we consider how shadow trading is affected by firms’ corporate policies. We conclude in Section 7.

## **2. Literature Review on Information Transfers**

A growing cross-disciplinary literature examines information transfers across firms. Our study is especially relevant to research that examines the sources of information or events that result in information transfers and factors that affect the speed of information transfers.

Early research examining information transfers focuses on the spillover effects of expected and unexpected earnings announcements on the stock prices of other firms in the same industry (e.g., Foster, 1981; Clinch and Sinclair, 1987; Han and Wild, 1990), management forecasts on industry firm stock prices (Baginski, 1987; Pyo and Lungarten, 1990) and the effects of sales

announcements on vertically connected firms (Olsen and Dietrich, 1985). Recent studies examine whether other sources or events result in information spillovers. Hertz et al. (2008) find that suppliers of firms that file for bankruptcy experience large negative stock price effects. Gleason et al. (2008) and Kravet and Shevlin (2010) examine spillover effects of accounting restatement announcements. They find evidence of intra-industry information transfers following accounting restatements. Wang (2014) finds that accounting statement harmonization and increased comparability across firms improves cross-country information transfers and Benveniste et al. (2003) find evidence of information transfers between banks around contemporaneous IPO offerings. Slovin et al. (1991) show that going-private bid announcements results in positive stock returns for industry rivals. A common thread among these studies is that they focus on ex post effects of the public release of private information. Our study complements these studies by examining the effect of information transfers that occur *prior* to the disclosure of private information.

Another stream of research examines factors that create frictions or facilitate information transfers. Pownall and Waymire (1989) finds that firms enjoy benefits of information transfers from other firms' earnings announcements only when the former don't provide their own earnings forecasts. Research also suggests that the extent of cross-industry information transfers is affected by the centrality of an industry's location within an economy (Ahern, 2013; Aobdia et al., 2014). Ramnath (2002) finds evidence that investors do not immediately process the information relevance from the first announcers in an industry for the other industry members based on the predictability of short window stock returns, but Hilary and Shen (2013) find analyst coverage of firms that issue management forecasts result in faster information transfers to other non-issuing firms in the same industry. In related work, studies find that limited investor attention (Cohen and Frazzini, 2008; Menzly and Ozbas, 2010) and firm complexity (Cohen and Lou, 2012) also affect



the speed of information transfers across firms. Our study is also relevant to this stream of the literature on information transfers because we show that it is important to consider the time period used to identify information transfers and the implications of shadow trading for the price discovery process.

### **3. Sample, Methodology, and Descriptive Statistics**

#### **3.1 Sample Selection**

Our main research question centers on evaluating whether material shadow insider trading takes place in U.S. capital markets. Intuitively, if the source firm's news announcement contains stakeholder-relevant information, source firm employees who are privy to the information can trade on or share the information with outsiders. This can result in abnormal trading activity in the linked firm *prior to* the source firm's announcement.

We begin with all non-utility, non-financial U.S. stock exchange listed firm observations (source firms) in Compustat for which we can also collect data about publicly listed U.S. stakeholders with U.S. headquarter locations (linked firms). We link source firms with their competitors based on the Hoberg and Phillips (2010, 2016) methodology and suppliers and customers using the Ellis et al. (2012) methodology that relies on the financial statements disclosures of material customers from the Compustat Segment files.

We also rely on other data sources. Daily short sale data is from NYSE, NASDAQ, and FINRA, option trading volume data is from OptionMetrics, order imbalance data is via institutional trading from Ancerno Ltd, institutional holdings data is from Thomson Reuters 13F filings, institutional investor classification is from Professor Brian Bushee (<http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html>), and reported insider trades are from Thomson Reuters Insider Filing. We obtain stock price data from the Center for Research in Security Prices (CRSP), firm-specific financial data from Compustat, board information from the

RiskMetrics and BoardEx databases, and firm proxy statements from the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system. We obtain M&A data from Thomson Reuters SDC database and new product announcements from LexisNexis. Finally, we procure insider ownership data from firm proxy statements, ExecuComp, RiskMetrics, Capital IQ, and BoardEx. We delete observations for which we cannot obtain all required data.

We use three types of news announcements to capture informational shocks in the source firms and examine the corresponding insider trading activity in the linked firms: earnings announcements, mergers and acquisition announcements, and new product announcements. Our sample size for empirical tests varies based on the setting and measure of shadow trading. Specifically, our sample for earnings announcement tests range between of 1,129 and 5,228 observations.<sup>5</sup> The sample for the M&A (product announcement) tests range between 1,694 and 12,164 observations (215,601 - 1,300,637).

For tests using earnings announcements, we require that for each source firm's earnings announcement at day  $t$ , none of the affiliated linked firm's other stakeholders release an earnings announcement during the 30-day window around the source firm's earnings announcement (i.e., from  $t-30$  to  $t+30$  days). This restriction alleviates the possibility that abnormal trading in linked firms occurs for reasons unrelated to information from the source firm's earnings announcement.<sup>6</sup>

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<sup>5</sup> For tests using short sales, the sample consists of 745 unique linked firms and 598 unique source firms, which represents 3,111 source firm-linked firm quarter observations. Of those observations 1,129 represent negative earnings shocks and 1,982 represent positive shocks. For tests using Option/Stock Ratio, the sample consists of 1,472 unique linked firms and 1,185 unique source firms, which represents 7,794 source firm-linked firm quarter observations. Of those cases, 2,882 represent negative earnings shocks and 4,912 represent positive shocks. Finally, for tests using Order Imbalance, the sample consists of 1,555 unique linked firms and 1,258 unique source firms, which represents 8,454 source firm-linked firm quarter observations. Of those cases, 3,226 represent negative earnings shocks and 5,228 represent positive shocks.

<sup>6</sup> A possible concern with this restriction is that we may lose observations if earnings announcements are typically clustered by industry and around the same dates as many U.S. firms have fiscal years ending December 31. To alleviate this concern, we repeat our tests using an alternate methodology that relies on non-earnings announcement related stock price shocks (discussed in Section 4). The primary empirical results are robust to this alternate methodology.

We also require that a linked firm's earnings announcement does not also occur within the same  $t-30$  to  $t+30$  window. This requirement allows us to isolate the informed trading in the linked firm from trading because of the linked firm's own informational leakage.<sup>7</sup> Figure 1 is a graphical illustration of the timeline and these two conditions. In sensitivity tests discussed below, we consider the possibility that a source firm's news can have either positive or negative implications for competitors.

For tests using mergers and acquisition announcements, we obtain M&A details from Thomson Reuters. We include all announced M&A transactions during the sample period and remove cases where 1) the acquirer does not obtain 100% ownership of the target following the merger; 2) either the acquirer and target are not publicly traded; 3) the merger attempt is dropped prior to the completion of an antitrust review; and 4) either the acquirer or target is a non-U.S. firm. We also exclude recapitalizations, self-tender offers, exchange offers, repurchases, acquisitions of remaining interest, or privatizations.

For tests using new product announcements, we search LexisNexis for new product announcements by sample source firms. We search for corporate news releases that are tagged under the subject "new products" and the headlines of which contain any keywords, or roots of words, of "launch", "product", "introduce", "begin", or "unveil".

### 3.2 Empirical Methodology

Our main test specification is as follows:

$$ShadowTrading = \beta_1 * Business\ Partner\ CAR + \beta_2 * Competitor\ CAR + \beta_x * Controls_x + \varepsilon \quad (1)$$

*ShadowTrading* is measured using one of three proxies from prior research that capture

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<sup>7</sup> While these restrictions improve the identification between source firm shocks and linked firm insider trading activities, it affects the power and generalizability of our findings. In untabulated analyses, we drop the two restrictions and perform tests on the full sample. The results yield similar inference with the larger sample.

abnormal trading: *Abnormal Short Sales*, *Option/Stock Ratio*, or *Order Imbalance*. First, following prior work (Desai et al., 2002; Christophe et al., 2004; Diether et al., 2009; Anderson et al., 2012), we measure *Abnormal Short Sales* for each linked firm in our earnings announcement news tests as the following firm-specific measure: ([linked firm  $i$ 's average daily short sales for the 30-day window prior to source firm  $A$ 's quarterly earnings announcement and divided by linked firm  $i$ 's average daily short sales for the year outside of  $A$ 's earnings announcement windows]  $- 1$ ). Our results are qualitatively similar if we use linked firm  $i$ 's earnings announcement dates to determine the non-event window. Daily short sales are measured as daily short sale volume divided by daily share trading volume.

We measure *Abnormal Short Sales* for each linked firm in our M&A and new product announcement tests as the following firm-specific measure: ([linked firm  $i$ 's average daily short sales for the 30-day window prior to source firm  $A$ 's M&A (new product) announcement at  $t$  and divided by linked firm  $i$ 's average daily short sales for the  $t-365$  to  $t-30$  day window]  $- 1$ ).

Second, *Option/Stock Ratio* is based on the ratio of option trading volume relative to stock trading volume (Roll et al., 2010; Johnson and So, 2012). The measure for each linked firm is calculated as the average of the linked firm's daily option trading volume scaled by the stock trading volume for the 30-day window prior to the source firm's news announcement. Prior research finds that the measure contains significant information about decreases in future stock prices (Johnson and So, 2012).

Third, *Order Imbalance* is based on the institutional investor buy-sell order imbalance as recent evidence indicates that institutional trading activity is predictive of future stock returns (Griffin et al., 2003; Campbell et al., 2009; Puckett and Yan, 2011). For each of our news announcement events, *Order Imbalance* is the average of the linked firm's daily order imbalance for the 30-day window prior to the source firm's news announcement. Daily order imbalance is

calculated as buyer-initiated institutional trading minus seller-initiated institutional trading, scaled by the total number of shares traded by institutions (Puckett and Yan, 2011).

We conduct tests using *Abnormal Short Sales* for the period from 2005 to 2011 and conduct tests using *Option/Stock Ratio* and *Order Imbalance* for the period from 1997 to 2011 because these are the periods for which data to calculate each dependent variable are available. Thus, the sample sizes vary across the tests. In untabulated tests, we find that our results are qualitatively similar if the tests using *Option/Stock Ratio* or *Order Imbalance* are restricted to the same period for which we can calculate *Abnormal Short Sales* (i.e., the period between 2005 to 2011).

Our primary independent variables are *Business Partner CAR* and *Competitor CAR*. These variables capture the three-day ( $t-1$  to  $t+1$ ) cumulative abnormal return (CAR) around the source firm news announcement at day  $t$ , when the source firm is a linked firm's business partner or competitor, respectively.<sup>8</sup> Abnormal return is the raw return minus the equally-weighted market return. For the earnings announcement tests, we use a market-based measure rather than an earnings surprise model because the former allows us to precisely capture the extent to which the earnings announcement is informative to market participants.<sup>9</sup> Note that for expositional purposes, we use the absolute CAR in multivariate tests but explicitly differentiate between whether the source firm news results in a positive or negative market reaction around the earnings announcement.

The variable *Controls* captures both linked firm and source firm characteristics that

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<sup>8</sup> Untabulated tests indicate that the results are very similar when we use alternative CAR windows (e.g.,  $t-5$  to  $t+5$ ,  $t-3$  to  $t+3$ ,  $t$  to  $t+1$ ,  $t$  to  $t+3$ , or  $t$  to  $t+5$ ), or value-weighted market returns.

<sup>9</sup> In untabulated robustness tests, we find that our results are qualitatively similar if we instead use the residual from an earnings surprise-based model:  $EPS_{A,q} = \alpha + \beta_1 EPS_{A,q-1} + \beta_2 EPS_{A,q-4} + \beta_3 EPS_{A,q-8} + \varepsilon_{A,q}$ , where  $EPS_{A,q}$  is reported earnings per share (EPS) in announcement quarter  $q$ , and historical EPS is reported earnings per share in the prior quarter ( $q-1$ ), four quarters ago ( $q-4$ ), and eight quarters ago ( $q-8$ ) using quarterly earnings announcements from I/B/E/S. A third approach is to use the difference between the consensus analyst quarterly earnings forecast and the corresponding reported quarterly earnings. We find these alternative measures yield similar results.

correlated with informed trading and returns. Following Drake et al. (2011), we control for firm size (*Firm Size*), book to market ratio (*Book-to-Market*), changes in analyst forecasts/recommendations (*FREV*), past stock returns (*Past Return*), total accruals (*TACC*), earnings price ratio (*E/P*), stock turnover (*Turnover*), growth in sales (*Sales Growth*) and long-term earnings (*LTG*), and momentum (*Momentum*). In addition, we include *MISP* for both the source firm and the linked firm documented by Stambaugh, Yu, and Yuan (2015) to capture the degree of the stock being mispriced. To differentiate between control variables related to the source and linked firm pair, we add the suffix “Source” to variable names to indicate these are controls for the source firm. Finally, we cluster standard errors by firm and year and also include year and Fama-French industry fixed effects. We define all variables in Table 1.

### 3.3 Descriptive Statistics

We present summary statistics for the sample used for earnings announcement tests in Table 2.<sup>10</sup> Panel A (Panel B) presents sample source firm (linked firm) summary statistics. Panel C presents industry membership details for source and linked firms. In Panel A, business partner source firms experience average negative (positive) CARs of -3.5% (2.1%) in the 3-day window around negative (positive) earnings announcement shocks. Competitor source firms report average negative (positive) CARs of -3.3% (2.3%) over the 3-day window around negative (positive) earnings announcement shocks. *T*-tests indicate that both positive and negative CARs are significantly different from zero, which suggests that earnings announcements provide information to market participants. Source firms have average total assets of \$6.1 billion and the average book-to-market is 0.818. Average *FREV* is -1.8% and the average total accruals is -2.7% of total assets. The average earnings price ratio for source firms is -1.2% and the average stock turnover is roughly

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<sup>10</sup> We tabulate summary statistics for the samples used for M&A announcements and new product announcements in the online Appendix.

0.52. The average annual sales growth for source firms is 2.4% and the long-term growth is 13.5%. Stock momentum on average is 0.09 for the source firms and the mispricing factor is 54.57 on average. The average past stock return is 2.7% for the source firms.

For linked firms, the results in Panel B of Table 2 indicate that in the 30-day window leading up to source firm earnings announcements, linked firms negative (positive) CARs for *Abnormal Short Sales* are 0.079 (-0.036). In other words, linked firms experience a 7.9% increase (3.6% decrease) in short selling activity prior to source firm earnings announcements (relative to the average short selling activity in the linked firms during nonevent windows). The *Option/Stock Ratio* is 2.336 in the 30-day window prior to a source firm negative earnings announcement CAR and 1.496 prior to a positive earnings announcement CAR. As a higher ratio indicates negative news, the evidence is consistent with shadow trading. The mean *Order Imbalance* is -0.032 (0.025) in the 30-day window leading up negative (positive) source firm earnings announcement CARs. *T*-tests of differences in means indicate that the values of all three measures are significantly different from zero.

Linked firms in our sample have average total assets of \$4.4 billion (lower quartile = \$335 million; upper quartile = \$2.53 billion). The mean book-to-market 0.782 and the mean FREV is -1.2%. The average total accruals is -2.3% and the mean earnings price ratio is -0.007. The average stock turnover is 0.522 and the average sales growth is 1.7%. Average stock momentum is 0.085 and the mean mispricing factor is 53.22. Lastly, the average past stock return is 2.2% for the linked firms.

Panel C in Table 2 presents the proportion of linked and source firms for the top ten Fama-French industries represented in the respective groups. Both groups of firms represent a large distribution of industries, with no single industry representing more than 9%-10% of the total sample. These top ten industries represent about 55% of the entire sample.

## 4. Multivariate Results

In Section 4.1, we present the findings from each of our three news announcement events. Section 4.2 presents results from tests examining whether shadow trading predicts linked firm returns. In Section 4.3 we discuss additional analyses and robustness checks.

### 4.1 Main Findings

Table 3 presents the results from tests of equation (1) for earnings announcements news events. The overall findings are consistent with the presence of shadow trading. In column 1 (2), when the dependent variable is *Abnormal Short Sales*, the coefficient on *Business Partner CAR* is positive (negative) and is statistically significant at the 5% level. In other words, as the magnitude of a business partner source firm's negative (positive) earnings announcement CAR increases, abnormal short sales for linked stakeholder firms increases (decreases). The coefficient on *Business Partner CAR* is positive in column 1 because we use the absolute value of a negative earnings announcement CAR in multivariate tests.

In economic terms, a one standard deviation increase in the negative (positive) CAR around a source firm earnings announcement is associated with a 10.7% (13.5%) increase (decrease) in *Abnormal Short Sales* for the business partner linked firms in the 30-day window prior to the source firm's earnings announcement.<sup>11</sup> We find parallel results for the coefficients on *Competitor CAR* across both positive and negative earnings shocks, although the magnitudes are slightly smaller. A one standard deviation change in CAR for a negative (positive) earnings announcement CAR results in a 9% (12.7%) increase (decrease) in *Abnormal Short Sales* for a linked competitor firm in the 30-day window prior to the source firm's earnings announcement.<sup>12</sup>

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<sup>11</sup> We calculate the economic significance as the coefficient divided by the mean abnormal short sales leading up to negative (positive) CARs, all multiplied by the standard deviation of positive (negative) CARs around earnings announcements. Thus, for business partner results in column 1 in Table 3:  $(0.033/0.079) * 0.256 = 10.7\%$ .

<sup>12</sup> A related working paper by Akbas et al. (2015) finds that a firm's short interest positively predicts the future returns



Columns 3 and 4 in Table 3 present coefficient estimates when *Option/Stock Ratio* is the dependent variable. Greater values of the ratio indicate higher information asymmetry and are consistent with more severe shadow trading. The coefficients on the variables of interest (*Business Partner CAR* and *Competitor CAR*) are positive and statistically significant across positive and negative source firm earnings announcement CAR groups. On average, a one standard deviation change in a negative (positive) earnings announcement CAR for a source firm is associated with a 7.7% (9.1%) increase in the *Option/Stock Ratio* for a business partner-linked firm in the prior 30-day window. The effects are similar when the linked is a source firm's competitor. On average, a one standard deviation change in a negative (positive) earnings announcement CAR for a source firm is associated with a 6% (8.9%) increase in a linked competitor firm's *Option/Stock Ratio* in the prior 30-day window.

Columns 5 and 6 in Table 3 present results when the dependent variable is set to *Order Imbalance* in the regression. The results are consistent with the findings in Columns 1-4. Economically, a one standard deviation change in a business partner source firm's negative (positive) earnings announcement CAR is associated with a 7.9% (5.5%) increase in *Order Imbalance* for the linked firm in the prior 30-day window. Similarly, a one standard deviation change in a competitor source firm's negative (positive) earnings announcement CAR is associated with a 6.4% (7.3%) increase in the linked firm's *Order Imbalance* in the prior 30-day window.

In additional analyses, *F*-tests of differences between coefficients indicate that there is no

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of its peer firms. This conflicting finding is likely to reflect differences in sample construction choices. Their tests focus solely on (1) firm and closest competitor pairs; and (2) settings in which short selling constraints exist. In contrast, our sample focuses on firms and all material competitors and we do not restrict the sample to settings with short selling constraints. Furthermore, in sensitivity tests described in online Appendix C Panel A and B show that depending on the nature of the source firm news event and whether the event has a negative or positive impact for the industry, shadow trading can occur in a manner consistent with the Akbas et al. (2015) findings.

significant difference between the effects on shadow trading for business partners versus competitors. Importantly, our results are robust to the inclusion of controls for both source and linked firm characteristics that are associated with informed trading activity.

In sum, our evidence is consistent with the argument that shadow trading in linked firms occurs in the lead up to a source firm's earnings shocks, and that shadow trading is increasing in the magnitude of the earnings shock. The results hold across multiple proxies for shadow trading.

To demonstrate that our findings are not concentrated in earnings announcements, we use two other informational events that are presumably only known to source firm insiders, i.e., M&A announcements and new product announcement. Similar to the approach used for earnings announcement tests, we present results from estimations of equation (1) for each of the three previously defined shadow trading measures for M&A announcements (Panel A) and new product announcements (Panel B).

The results are largely consistent with the findings for earnings announcement shocks presented in Table 3. First, in Table 4 Panel A, we find that prior to source firm M&A announcements, linked business partner firms experience statistically significant increases in shadow trading across all our measures of shadow trading. We also find some evidence of shadow trading in linked competitor firms. A possible explanation for the weaker results for competitors is that mergers have heterogeneous effects on competitors based on the effects of the merger on market structure characteristics.

Second, in Table 4 Panel B, the empirical results for new product announcements provide corroboratory evidence in support of shadow trading. For instance, for a source firm product announcement with a positive CAR, linked business partner firms experience lower abnormal short sales in the lead up to the announcement. This is consistent with positive new product announcements by a source firm having spillover benefits for linked firm business partners. In

addition, competitors experience lower abnormal short sales in the lead up to the announcement which suggests that on average, investors view source firm new product announcements as having negative implications for competitors. On the other hand, when a new product announcement receives a more lukewarm market reaction, we find no evidence of shadow trading in business partners, and less abnormal short selling in competitors. We find consistent evidence for tests using the other proxies for shadow trading.

To provide further evidence, we undertake a test using stock price shocks to source firms as a proxy for news events. The methodology and findings are outlined in the online Appendix and provide corroborating evidence of shadow trading leading up to information events.

In sum, the findings indicate that linked firms experience significant informed trading leading up to their stakeholders' news announcements. These findings are robust to multiple proxies to measure shadow trading and different types of news events.

#### **4.2 Does Shadow Trading Predict Future Returns?**

Our tests above document the presence of shadow trading but do not speak to whether it is profitable. In this section, we examine whether our shadow trading predicts linked firm returns. We regress future stock returns of the linked firms on each of the three measures of shadow trading, where future returns are measured as the linked firm short window abnormal stock returns between  $t-1$  and  $t+1$  around the source firm earnings announcement at day  $t$ . Untabulated analyses indicate that the results are not sensitive to the choice of the window around the earnings announcement. We also control for firm size, prior stock returns, book-to-market, momentum, and mispricing factors for the linked firms. The empirical results show that future returns for linked firms (around the source firm earnings announcement) are negatively and statistically associated with *Abnormal Short Sales*, and positively and statistically associated with the Option/Stock ratio and Order Imbalance, suggesting that shadow trading is profitable.

### **4.3 Additional Analyses and Robustness Checks**

In this section, we first validate our findings using a fourth proxy to measure shadow trading. Second, we examine whether cross-sectional differences in either the size of the source firm earnings announcement CAR or the relation between firms and their business partners affect shadow trading. Finally, we consider whether our competitor effects are sensitive to whether the source firm shock has negative or positive spillover effects.

#### ***4.3.1 Alternative Measure of Shadow Trading***

In untabulated analyses, we check whether our findings are robust to the use of a fourth alternative proxy to measure shadow trading: linked firm abnormal trading volume (*AbTradeVol*). The measure is defined as: ([linked firm *i*'s average daily trading volume prior to source firm A's quarterly earnings announcement (day *t*-30 to *t*-1) divided by linked firm *i*'s average daily trading volume for the year outside of A's earnings announcement windows] - 1). We re-estimate equation (1) after setting the dependent variable to be *AbTradeVol* and we obtain similar inferences to those from our main findings. In economic terms, a one standard deviation increase in a source firm's earnings announcement CAR results in a 5%-8.8% increase in the linked firm's *AbTradeVol* over the 30-day window prior to the source firm's earnings announcement.

#### ***4.3.2 Cross-Sectional Variation in the Magnitude of the Earnings Announcement CAR***

We next examine whether the prevalence of shadow trading is predictably and positively related to the magnitude of source firm's earnings announcement CARs. In untabulated analyses, we partition our sample into three tercile groups based on the magnitude of the source firm earnings announcement CAR. The results indicate that the sensitivity of shadow trading to the magnitude of the earnings announcement CAR more than doubles from the lowest to the highest tercile groups.

#### ***4.3.3 Strength of the Relationship between the Source Firm and the Linked Firm***

Next, we exploit a unique element of the Compustat Segment data that allows us to identify the strength between firms and their material customers. Firms are required to disclose these data under U.S. financial accounting disclosure requirements. We collect these data and partition our sample into two groups based on the median value of the magnitude of the relation between a source firm and a business partner linked firm. We use the proportion of a firm's sales to each customer to measure the magnitude of a relationship. Untabulated results show that informed trading in source firm customers is significantly greater for firms with above-median supplier-customer relationships relative to the below-median group.

#### ***4.3.4 Industry-Wide versus Firm-Specific Competitor Source Firm Shocks***

We examine whether the effect of source firm shocks on competitor-linked firms varies with the nature of the shock. Competitors of a source firm that experience a negative earnings shock may be positively or negatively affected by the source firm's performance. On the one hand, if the source firm's negative earnings shock represents decreased sales because of a transfer of market share, then competitor firms that have captured the source firm's lost market share are likely to experience positive stock returns. Alternatively, if the source firm's negative earnings shock represents an industry-wide effect (such as an aggregate reduction in demand for the industry's products or services), competitors are likely to also experience negative stock returns. The implication of this conflicting effect is that the coefficient on *Competitor CAR* in our main results is likely to be understated.

To evaluate this possibility, we explicitly differentiate between source firm earnings shocks (negative or positive) at  $t$  based on whether they are expected to result in positive or negative spillover effects for competitors. We determine the direction of the effect for linked competitors by examining the sign of the  $t-1$  to  $t+1$  market-weighted CAR for all competitor firms in the same three-digit SIC industry. We then use both the CAR for the source firm and the source firm's

industry to reverse engineer the expected direction of shadow trading effect for the linked competitor.<sup>13</sup> Untabulated results indicate that the results are qualitatively similar to the findings in Tables 3 and 4 but of slightly larger magnitude, consistent with our expectations.

#### ***4.3.5 Can Information Transfers Via Director Networks Explain Shadow Trading?***

It is possible that our results simply capture information flows that occur through board of director networks (e.g., Larker et al., 2013; Akbas et al., 2016). We follow the Akbas et al. (2016) methodology to measure director networks. In untabulated analyses, we find that our main results are qualitatively similar after including a control variable to measure the connectedness of the linked firm's director network.<sup>14</sup>

We highlight that our results should not be viewed as providing counter-evidence to the board network literature. Rather, our findings suggest that the nature of information transferred across board networks may not be short-term information related to upcoming news announcements that can be used to engage in short-run profitable trading/leaking opportunities. Instead, information transfers via board networks may reflect other types of news such as long-term, macro, or strategic information.

### **5. Who Undertakes Shadow Trading?**

The findings in Section 4 confirm the existence of shadow trading but do not allow us to identify the mechanism underlying shadow trading. There are at least three possibilities. First, it is

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<sup>13</sup> The methodology is better understood with an example. Take a source firm that experiences a negative CAR between  $t-1$  and  $t+1$  around an earnings announcement at  $t$ . We determine whether the average CAR between  $t-1$  and  $t+1$  for the source firm's same three-digit SIC code competitors is positive or negative. A positive CAR is consistent with the explanation that the source firm's competitors have increased their market share at the expense of the source firm. This suggests that abnormal short sales should *decrease* for the linked competitor firm prior to the source firm's earnings announcement. Conversely, a positive CAR for industry peers is consistent with the explanation that investors believe the source firm's bad performance is likely to reflect an industry-wide effect. This suggests that abnormal short sales should also *decrease* for the linked competitor firm prior to the source firm's earnings announcement.

<sup>14</sup> Our results are robust to the use of an alternate measure of director connectedness from Intintoli et al. (2018).

possible that source firm insiders share their stakeholder relevant private information with sophisticated investors or with competitive intelligence firms who sell this information to sophisticated investors. Second, shadow trading could reflect trading by sophisticated market participants such as hedge funds who use proprietary methods to independently collect and process information about upcoming news events. Third, it is possible that market structure characteristics such as short sale restrictions explain our results (Khan and Lu, 2013). If it is the case that the sample source firms systematically face short-selling constraints, then short sale activity may shift to stakeholder firms (Akbas et al., 2015). In the subsections below, we conduct three tests to help identify the source(s) of shadow trading.

### **5.1 Identification Using Increased Attention to Conventional Illegal Insider Trading**

Our first test exploits unexpected public attention to conventional insider trading. The intuition for this test is that increased public attention can reduce insiders' incentives to engage in illegal insider trading because of the higher perceived risk of prosecution. Indeed, empirical studies document reductions in illegal insider trading over time due to greater regulatory scrutiny by the SEC and Department of Justice (Brochet, 2010; Anderson et al., 2013; Del Guercio et al., 2013).

In order to continue to monetize their private information, insiders may switch to exploiting stakeholder-relevant private information because of lower perceived risks of prosecution under the current U.S. insider trading rules. In particular, the legality of shadow trading imposes a hurdle on prosecutors because it requires an assessment as to whether the trading creates a breach of fiduciary responsibility. Supreme Court decisions, such as *United States v. James Herman O'Hagan* (1997), define own-firm insider trading rules and regulations but do not explicitly speak to shadow trading. Furthermore, legal definitions provide limited guidance about how a firm's boundaries are defined, and therefore the legality with which employees can use own-firm private information that affects stakeholders.

We develop a time-varying measure of public attention to illegal insider trading by creating an “insider trading enforcement index”. This index is in the spirit of the economic policy uncertainty measure developed by Baker, Bloom, and Davis (2016). We follow their methodology and search major U.S. newspapers during our sample period to obtain a count of articles that contain the term “insider trading”.<sup>15</sup> We aggregate the count by month across all the major newspaper reports that include the term “insider trading” during our sample period. A graphical illustration of this count is attached in Figure 1. Four spikes are clearly visible. The first spike is in 2002 during the passage of the Sarbanes Oxley Act. We do not include this spike in our tests because of the large number of regulatory and disclosure changes occurring around this period that limit our ability to isolate changes in source firm employee incentives to engage in shadow trading that are driven by increased media attention to insider trading. The second, third, and the fourth spikes occur in June 2003, June 2006, and October 2009, respectively.<sup>16</sup>

We compare shadow trading prior to earnings announcements during the 3-month period before and after each of these three spike events.<sup>17</sup> We apply the same procedure and restrictions used for our primary sample to identify source and linked firms to generate a sample of 6,298 (6,474) observations for the *Option/Stock Ratio (Order Imbalance)* measures.

Figure 2 illustrates the insider trading activities of the source firm and the shadow trading of the linked firms around the spikes of media attention on conventional insider trading. We calculate own-firm insider trading during the four 30-day windows prior to earnings

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<sup>15</sup> The newspapers are USA Today, Miami Herald, Chicago Tribune, Washington Post, Los Angeles Times, Boston Globe, San Francisco Chronicle, Dallas Morning News, New York Times, and Wall Street Journal.

<sup>16</sup> The June 2003 spike is primarily related to the SEC’s securities fraud charges against Martha Stewart and the June 2006 spike is primarily related to the SEC’s securities fraud charges against individuals related to Blue Rhino Corp’s merger and jury verdicts against Enron executives for insider trading. The October 2009 spike is primarily related to the SEC’s securities fraud charges against Raj Rajaratnam and hedge fund advisory firm Galleon Management LP.

<sup>17</sup> Note that in all the cases, spikes last for several months. We use the first month of each spike to identify the spike month because insider trading incentives are likely to be curbed immediately after the first spike in media attention.



announcements in a year and then take the average for each year across firms. The evidence in Panels B to D show that own-firm insider trading in source firms *decreases* after spikes in media attention to insider trading, whereas shadow trading *increases* significantly after the spikes across each of our three measures of shadow trading. These figures are consistent with the idea that source firm insiders attach greater costs to conventional insider trading after media attention to SEC prosecutions against insider trading and that shadow trading appears to at least partially substitute for insider trading. Next, we examine shadow trading empirically by estimating the following equation:

$$\text{Shadow Trading} = \beta_1 * \text{Business Partner CAR} + \beta_2 * \text{Competitor CAR} + \beta_3 * \text{Post} + \beta_4 * \text{Business Partner CAR} * \text{Post} + \beta_5 * \text{Competitor CAR} * \text{Post} + \beta_x * \text{Controls}_x + \xi. \quad (3)$$

where *Post* is an indicator variable set to one for the 3-month period following one of the three insider trading news spikes noted above, and zero for the 3-month before the spike. *Post\*Business Partner CAR* (*Post\*Competitor CAR*) captures whether the relation between the magnitude of a business partner (competitor) source firm earnings shock CAR affects shadow trading differentially in the post-spike period. We use the same control variables as specified in the discussion of equation (1) and include year and industry fixed effects.

We present coefficient estimates in Table 6. The results indicate that shadow trading increases in the post-spike windows and the effect is increasing in the magnitude of the earnings announcement CAR. The coefficients on *Post \* Business Partner CAR* and *Post \* Competitor CAR* across all three measures are statistically significant at the 5% level or better in 10 out of 12 cases. In economic terms, shadow trading in linked firms becomes approximately two times more sensitive to business partner (competitor) earnings announcement CARs in the post-spike period relative to the pre-spike period.

A possible concern with this test is that our results are simply attributable to increased

investor effort to undertake independent information gathering and processing efforts in light of decreased supply of private information from insiders after insider trading news shocks. To address this issue, we exploit a sample of firms for which we have corporate policies (See Section 6 below for details about these policies). These policies allow us to identify whether firms explicitly prohibit trading in or sharing information about stakeholders. If our results are attributable to increased investor effort to obtain information other than from source firm insiders, we should observe no differences between shadow trading between the two groups after insider trading news shocks. In Figure 3 we graphically show that following insider trading news shocks, the increase in shadow trading across all three proxies is more pronounced when source firms do not explicitly prohibit shadow trading relative to when source firms explicitly prohibit shadow trading. This finding is inconsistent with an increased investor effort explanation.

In sum, our findings suggest that increased media attention to conventional insider trading increases the incentives for source firm employees to engage in shadow trading.

## **5.2 Identification Using Shocks To Employee Mobility**

The findings above are consistent with the argument that shadow trading is attributable to information from source firm insiders. In this section we provide additional evidence about the source of shadow trading by examining an alternative identification strategy. In particular, we exploit a plausibly exogenous staggered shock to state laws pertaining to the ability of a firm's employees with trade secret knowledge to obtain external employment opportunity. The changes in the laws, widely known as the inevitable disclosure doctrine (IDD), occur via precedent setting cases. A state's adoption or the rejection (following a previous adoption) of the IDD affects the ability of employees of the firms headquartered in those states to profit from their firm-specific knowledge by joining competitor firms. In a similar vein, Huddart and Lang (2003) examine and find evidence that both senior and lower ranking employees exploit private information in their

stock option exercise decisions. Thus, the adoption (rejection) of an IDD is thus likely to increase (decrease) the incentives of employees looking for opportunities to use their firm-specific human capital or knowledge to accrue wealth, which in turn affects their incentives to engage in shadow trading.

We predict that the adoption (rejection) of the IDD results in an increase (decrease) in shadow trading when source firms are in the state that experiences the IDD shock, relative to source firms in other states. We begin by identifying all U.S. states affected by an IDD shock via case precedent and relevant dates as described in Klasa et al. (2018).<sup>18</sup> Our analyses rely on the sample over the period for which we can obtain data (1997-2011). For each treatment source firm in a state that experiences an IDD shock, we identify a propensity score-matched control source firm from a state that does not experience an IDD shock. The matched control source firm is identified based on characteristics in the year prior to the treatment firm's IDD shock, including industry, firm size, leverage, market-to-book, volatility, ROA, firm age, analyst following, institutional ownership, number of competitors, and number of customers/suppliers, with no replacement, and a caliper of 0.1%. The process yields a sample of 508 treatment source firms and 508 matched control source firms. We estimate the following OLS specification:

$$Shadow\ Trading = \beta_1 * Business\ Partner\ CAR + \beta_2 * Competitor\ CAR + \beta_3 * IDDSHock + \beta_4 * Business\ Partner\ CAR * IDDSHock + \beta_5 * Competitor\ CAR * IDDSHock + \beta_x * Controls_x + \xi. \quad (3)$$

*IDDSHock* is an indicator variable. For all treatment source firms that experience the adoption (rejection) of the IDD in the past two years, *IDDSHock* is set to one (zero) following the adoption (rejection), and zero (one) for the pre-shock observation. The interaction term *Business Partner CAR \* IDDSHock* (*Competitor CAR \* IDDSHock*) captures the incremental effect of

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<sup>18</sup> These cases reflect IDD shocks for the following states: Missouri and Ohio adopt in 2000, Florida rejects in 2001, Michigan rejects in 2002, Texas rejects in 2003, and Kansas adopts in 2006. We require at least two years of data prior to and following the shock so we do not use cases that occur in 1998 and 1999.

business partner (competitor) source firm earnings shocks on shadow trading in linked firms in the post-IDD shock period. We use control variables as specified in the discussion of equation (1) and include year and industry fixed effects.

We present regression coefficients in Table 7.<sup>19</sup> The coefficients on *IDDShock* bear positive signs and are statistically significant across specifications using either *Option/Stock Ratio* or *Order Imbalance* as the dependent variable. This suggests that the imposition (removal) of restrictions that prohibit a firm's employees from working for competitors is positively (negatively) associated with shadow trading. Second, the coefficients on *Business Partner CAR \* IDDShock* (*Competitor CAR \* IDDShock*) bear positive and statistically significant signs at the 5% level or better in six out of eight cases (and significant at the 10% level in the remaining two cases). This finding suggests that for source firms in states impacted by IDD shocks, the shock is associated with significant changes in linked firm shadow trading. In economic terms, for source firms that experience an IDD shock, a one standard deviation increase in a source firm earnings announcement negative CAR is associated with an incremental 3.3% (2.8%) increase in a business partner (competitor) linked firm's *Option/Stock Ratio*, relative to linked firms associated with source firms that do not experience an IDD shock. We find similar inferences for tests using *Order Imbalance* as the dependent variable. A one standard deviation increase in business partner (competitor) source firm CAR around a negative earnings announcement is associated with an incremental 4% (2.9%) increase in linked firms' *Order Imbalance*, relative to linked firms associated with source firms that do not experience an IDD shock.

The evidence in Table 7 is consistent with the argument that shadow trading is attributable to information leaks from source firm employees rather than because of proprietary information

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<sup>19</sup> Daily short sales data is only available for a brief time period during our sample period. As such, we cannot use short sales as a proxy for shadow trading.

gathering by sophisticated investors or because of market frictions. Thus, leaking stakeholder-relevant information to competitive intelligence firms or information intermediaries appears to potentially offer a viable mechanism for source firm employees to exploit their private information when faced with adverse shocks to the set of potential employment opportunities.

We caveat that changes to employment opportunities around the IDD shocks may not arise in a vacuum and possibly also impact changes in competition across firms. For example, changes in competition among firms for employees may also affect the economic linkages across firms. To provide evidence to rule out this possibility, we examine *t*-tests of differences between the values of control variables in the year prior to and following each shock. To the extent that there are changes in economic linkages between firms, we would expect differences in the treatment firm control variable means in the year around the IDD shock. Untabulated results indicate no significant differences between any control variable values in the pre and post periods for treatment firms.

## **6. The Effects of Firm-Specific Policies against Shadow Trading**

In this section, we examine whether firms can influence shadow trading by explicitly having corporate policies prohibiting employees from leaking or sharing stakeholder-relevant information. Bettis, Coles, and Lemmon (2000) find that firms with corporate policies to curtail insider-trading exhibit lower instances of such activity. Their findings imply that if these policies act as an effective deterrent for employees and all else equal, linked firms should display reduced levels of shadow trading when they are associated with source firms that explicitly restrict employees from sharing stakeholder-relevant information.

Using a sample window between 2010 and 2013 for which firms are available in Standard and Poor's Capital IQ database, we manually collect each firm's Code of Ethics statement or Employee Professional Conduct manual to determine each source firm's insider trading policy.

We are able to identify policies for 267 unique firms and classify firms based on whether their policy explicitly prohibits insiders from: i) trading in or sharing information about the firm and its business partners and/or competitors; or ii) exclusively focuses on restricting conventional own-firm trading. Other sample firms likely have policies that reflect one of these two classifications do not publicly disclose this information. These firms typically refer readers to a corporate intranet site or employee handbook. Approximately 53% of the sample firms for which we can obtain policy details explicitly prohibit employees from using private information to trade in their own firms or in stakeholders. The remaining 47% only explicitly prohibit employees from using private information to trade in their own firms. An untabulated comparison of the two groups of firms (explicit policy against shadow trading vs. no explicit policy against shadow trading) shows that source firms with explicit prohibitions against shadow trading are significantly smaller in size, have higher volatility and bid-ask spreads, as well as lower institutional ownership and trading volume. However, we also find no pattern of statistical difference in other control variables across the two groups.

In Figure 4, we present graphical depictions of shadow trading in competitor and business partner linked firms during the  $t-25$  to  $t+5$  trading-day window around a source firm's earnings announcement date for the sample cases for which we can identify source firm insider trading policies. Panels A and B display patterns for *Abnormal Short Sales* in linked firms for which the source firm is a competitor (business partner). We present values separately for cases in which source firms explicitly prohibit and do not explicitly prohibit shadow trading. Panels C to F present parallel trading patterns in linked firms for the other two metrics: the *Option/Stock Ratio* and *Order Imbalance*. All the figures show that trading activity in competitor and business partner linked firms is much greater when affiliated source firm trading policies *do not* explicitly prohibit insiders from trading in or sharing private information about stakeholders, relative to source firms that

explicitly prohibit shadow trading.

Table 8 Panel A presents results from multivariate tests of equation (1) after adding interaction terms to capture differences in linked firm shadow trading between affiliated source firms that explicitly prohibit shadow trading and those that do not. *Prohibit* is an indicator variable set to one if the source firm has an explicit restriction against shadow trading, and set to zero otherwise. We also interact *Prohibit* with *Business Partner CAR* and *Competitor CAR*

First, the coefficient on *Prohibit* is statistically significant in all specifications. The coefficient signs suggest that shadow trading in linked firms is muted on average when source firms explicitly prohibit shadow trading. Second, coefficients on *Business Partner CAR* and *Competitor CAR* are statistically significant across both positive and negative earnings shock CAR partitions and for all three measures of shadow trading. In economic terms, a one standard deviation increase in a negative (positive) CAR around a source firm's earnings announcement is associated with a 6.6% (6.1%) increase (decrease) in abnormal short selling in the linked firm for the 30-day window prior to the business partner source firm's earnings announcement.<sup>20</sup> Coefficients on *Competitor CAR* across both positive and negative earnings shocks suggest similar economic interpretations. A one standard deviation increase in business partner (competitor) source firm CAR around an earnings announcement is associated with a 9.1% (6.7% - 7.2%) increase in the linked firm's *Option/Stock Ratio*. We find similar inferences using *Order Imbalance* as the dependent variable in columns 5 and 6. Untabulated *F*-tests indicate no significant differences between coefficients on Business Partner CAR and Competitor CAR across all the specifications. The interaction terms between *Prohibit* and *Business Partner CAR* and

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<sup>20</sup> We calculate the economic significance in column 1 as:  $(0.019/0.069) * 0.221 = 6.1\%$ , where 0.019 is the coefficient, 0.069 is the mean abnormal short sales with negative shocks, and 0.221 is the standard deviation of *Business Partner CAR* (negative shock sample). Note that these statistics are different from the main sample statistics described in Table 2.

*Competitor CAR* are significant across all specifications. This finding suggest that shadow trading is incrementally sensitive to source firm inside information based on whether source firms have a policy that explicitly prohibits shadow trading. In sum, our findings suggest that shadow trading can be influenced by source firm-specific policies that impose prohibitions on shadow trading.

## **7. Additional Analyses**

In this section we consider source firm characteristics that can affect the intensity of shadow trading activity. These include variation in the type of institutional investors, corporate governance characteristics, and firm geographic location.

### **7.1 Source Firm Institutional Investors**

First, we examine source firm institutional investor characteristics. Data from Ancerno allows us to identify two types of institutional investors: pension funds and mutual funds. Pension funds are typically long-term buy-and-hold investors whereas mutual funds are more likely to actively trade. It follows that mutual funds are more likely to seek to find opportunities to engage in illicit shadow trading.

We collect detailed institutional trading activity from Ancerno and follow the Hu et al. (2018) approach to identify institutional investor type as mutual funds or pension funds. The approach relies on linking Ancerno's institution identifier details with that in the Spectrum 13F database. We then classify source firms as having high or low mutual fund and pension fund investment based on the median level of investment for each of the two investor groups in our entire sample. We focus on the quadrants in which source firms are categorized as having high mutual fund investment and low pension fund investments and vice versa. The sample cases in these two quadrants represent 42% of our total sample.

The overall results in online Appendix D Panels A and B indicate that while both types of



institutional investors in source firms are positively associated with shadow trading, the economic magnitudes are larger for mutual funds than for pension funds.<sup>21</sup>

## 7.2 Source Firm Corporate Governance

We examine cross-sectional variation in source firm corporate governance using G-Index, a firm-level measure of governance from Gompers et al. (2003). Firms with higher governance quality are more likely to have internal controls in place to curb shadow trading (and insider trading). Accordingly, it is possible that insiders in these firms are less likely to leak or sell stakeholder-relevant information, which suggests lower levels of shadow trading in linked firms. We create an indicator variable (*High Governance*) that is set to one for firms with above median values of G-Index and interact this variable with *Business Partner CAR* and *Competitor CAR*. The untabulated empirical results indicate that shadow trading is positively associated with source firm corporate governance quality. This finding is consistent with the evidence in Jagolinzer et al. (2011).

## 7.3 Source Firm Insider Ownership

In this section, we consider whether source firm insider ownership stakes affect shadow trading. Higher insider ownership reflects greater levels of managerial bonding with the firm's interests. If the revelation of shadow trading can adversely affect firms' relationships with stakeholders, then insiders with higher ownership levels have greater incentives to monitor and prohibit shadow trading. We measure insider ownership as the aggregate equity ownership percentage of top executives and directors, based on data from BoardEx. We use an indicator variable, *High Inside Own*, set to one for source firms with insider ownership above the sample

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<sup>21</sup> We also find similar results when using an alternative approach to classify institutional investors based on Chen (2007) and partitioning source firms by ownership of dedicated and non-dedicated institutional investors. We create two subsamples of source firms with high (low) levels of dedicated institutional investor ownership and low (high) levels of non-dedicated institutional investor ownership. Untabulated results indicate that the presence of non-dedicated institutional investors is positively associated with shadow trading.

median. The untabulated results show that for both business partner and competitors, shadow trading is significantly and economically dampened for source firm with relatively higher levels of insider ownership. These results complement prior literature that documents the benefits of concentrated ownership (e.g., Anderson and Reeb, 2003).

#### **7.4 Source Firm Location**

Next, we examine whether source firm geographic location affects shadow trading. Firms located further away from major metropolitan locations are expected to be less prone to shadow trading because of fewer opportunities for insiders to develop connections to monetize their private information. We use the distance between source firm headquarters and the nearest major airport as a proxy for urban location following John et al. (2011). We create an indicator variable, *Urban*, set to one for firms below the median distance to the nearest major airport. The untabulated empirical indicate that shadow trading is more pronounced for source firms located closer to urban areas. This finding also indirectly provides support for the argument that our main results are driven by source firm insiders leaking information rather than trading in the information themselves. If the latter, it is unclear why we would observe systematic variation in shadow trading based on source firm geographic location.

#### **8. Concluding Remarks and Policy Implications**

We provide evidence about the effects of information transfers that occur prior to the release of earnings announcements. In contrast, most prior research focuses on information transfers that occurs *following* the release of private information. We document evidence consistent with the idea that firm employees' use and/or leak private information that is relevant for linked firms prior to the public release of that information and the leaked information affects trading activity in those linked firms. We label this scrutiny avoidance activity as "shadow trading."

Shadow trading is, by its nature, aimed at circumventing regulations against conventional insider trading. In the US, the explicit actions that make insider trading a criminal offense are not explicitly defined in legislation. Instead, the definition has been left to court decisions (and somewhat to guidance by the SEC which can be overturned by courts) which explicitly require a breach of fiduciary responsibility. Against this backdrop, we describe how shadow trading constitutes a gray area in trading on material, non-public information, which we posit that insiders and sophisticated investors seek to exploit.

We undertake analyses to help rule out the possibility that shadow trading is attributable to other explanations, such as market structure constraints or trading by sophisticated investors from other information sources. Firm employees appear to engage in shadow trading to avoid exposure to penalties associated with illegal insider trading. We also find that firms can curb shadow trading by formally imposing restrictions on employee use of private information in company manuals.

A potentially important avenue for future research centers on the magnitude and sources of payment to source firm employees for their shadow information. Competitive intelligence firms appear widespread and arguably serve as intermediaries between professional investors and firm insiders. One viable source of information centers on pension and mutual funds and the magnitude of their payments to obtain data from competitive intelligence firms. We also note that our data does not allow us to evaluate whether firm insiders with stakeholder relevant private information directly undertake shadow trading; this is an interesting opportunity for future research.

In sum, our paper highlights a wealth transfer effect of information spillovers and contributes to a literature in accounting and financial economics. Our findings have an important policy implication. The vast majority of regulatory (and academic) attention on insider trading has centered on managers who use private information to trade in their *own* firms as opposed to other firms. Our findings suggest a need for increased legislative and regulatory attention towards firm

employee ability to trade in or leak information about business partners and competitors.

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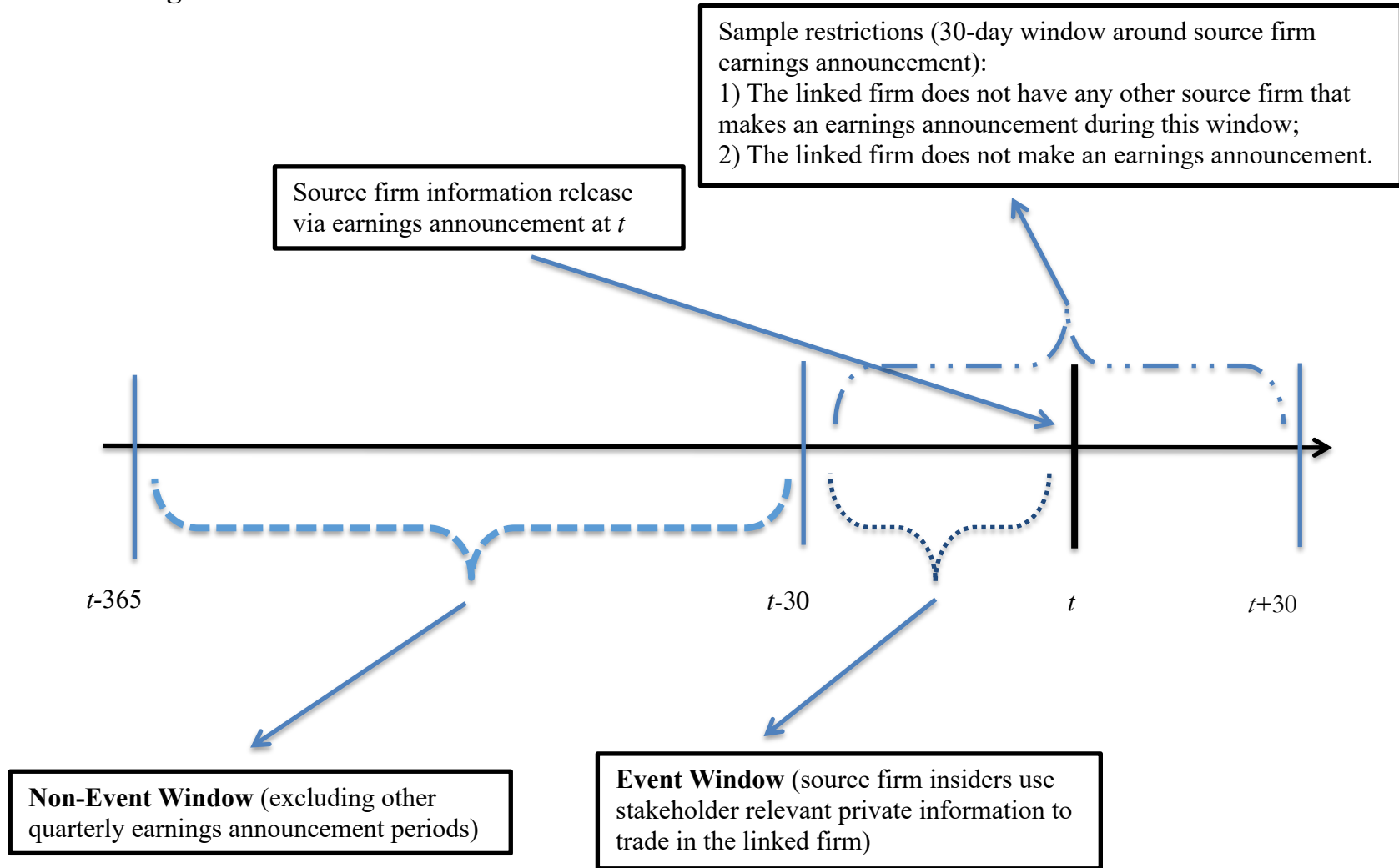
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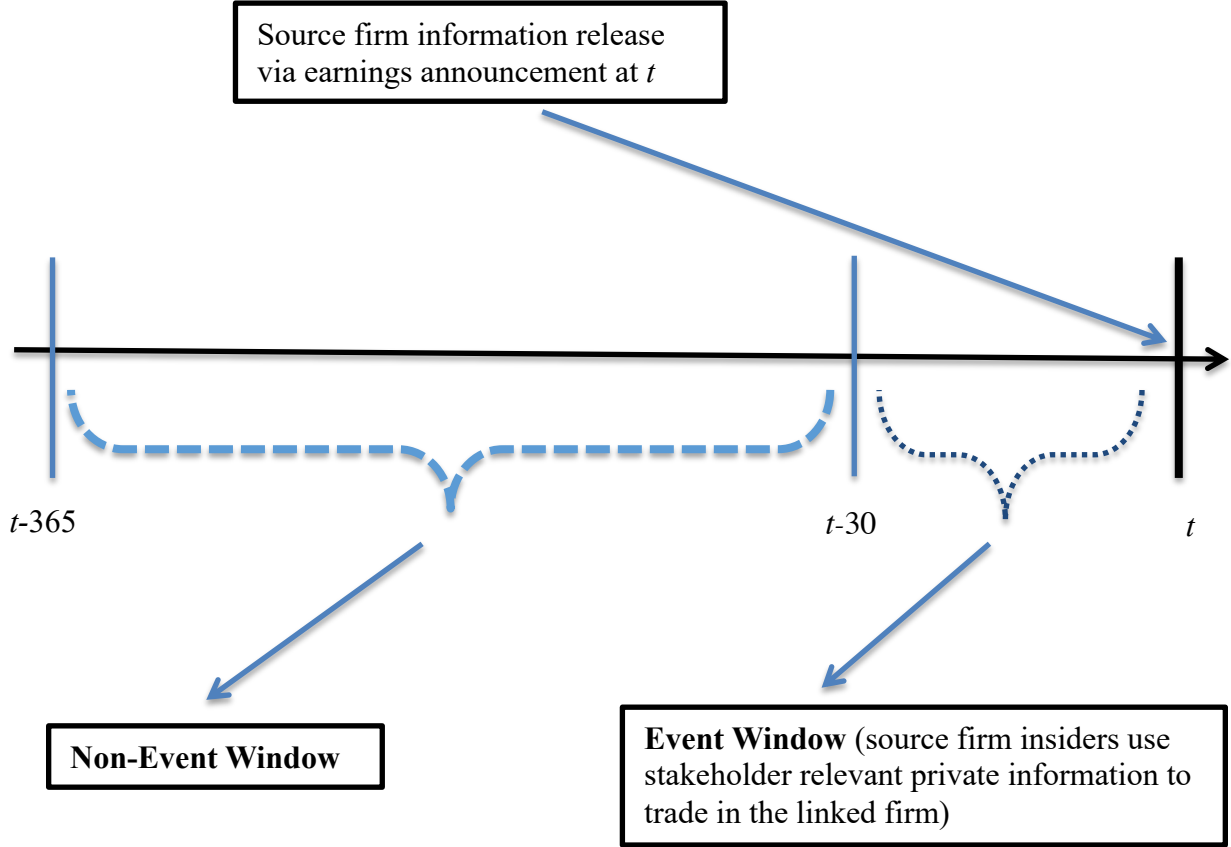
### Figure 1. Timeline of Events for News Announcement Tests

Panel A (Panel B) presents a timeline of the event window used to determine Earnings (M&A and New Product) news announcements.

#### Panel A: Earnings News Announcements



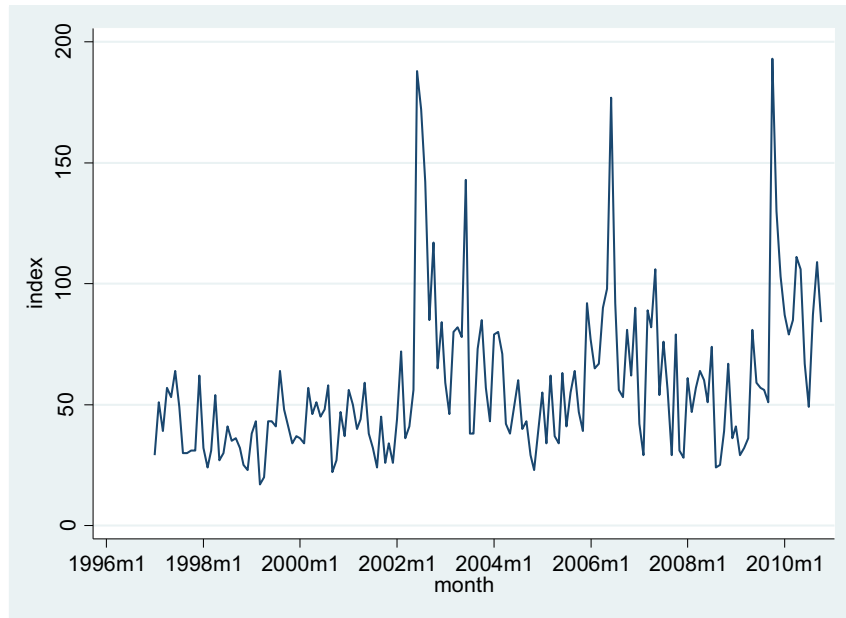
**Panel B: M&A and New Product News Announcements**



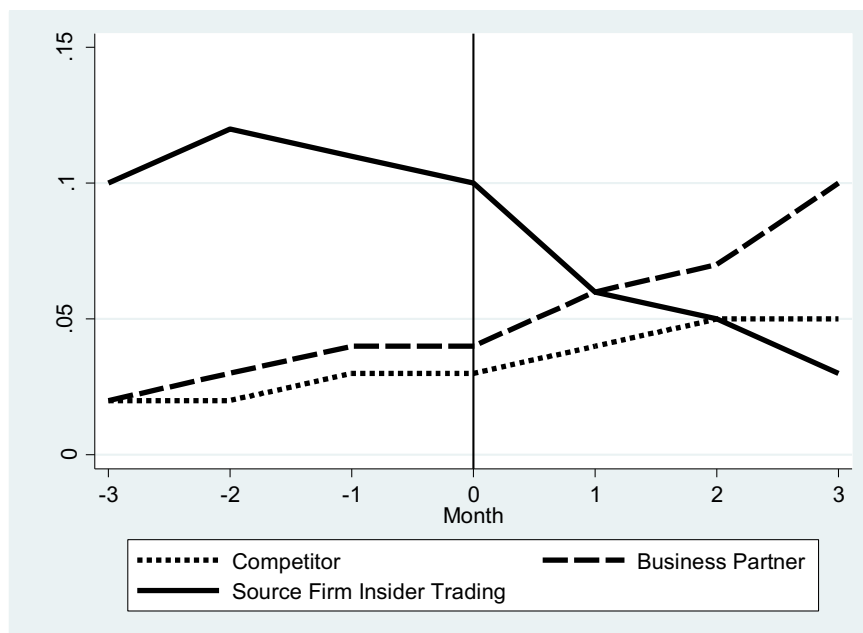
## Figure 2. Media Attention On Insider Trading

Panel A shows the time series of the insider trading index via media attention on insider trading. The first month of the four largest spikes are June 2002, June 2003, June 2006, and October 2009. Panel B, C, and D present graphical time series of linked firm *Abnormal Short Sales*, *Option/Stock Ratio*, and *Order Imbalance* respectively around the spikes in the insider trading index, respectively. The changes in values are presented separately for competitor and business partner linked firms. Panels B, C, and D also present changes in source firm conventional insider trading during the same period.

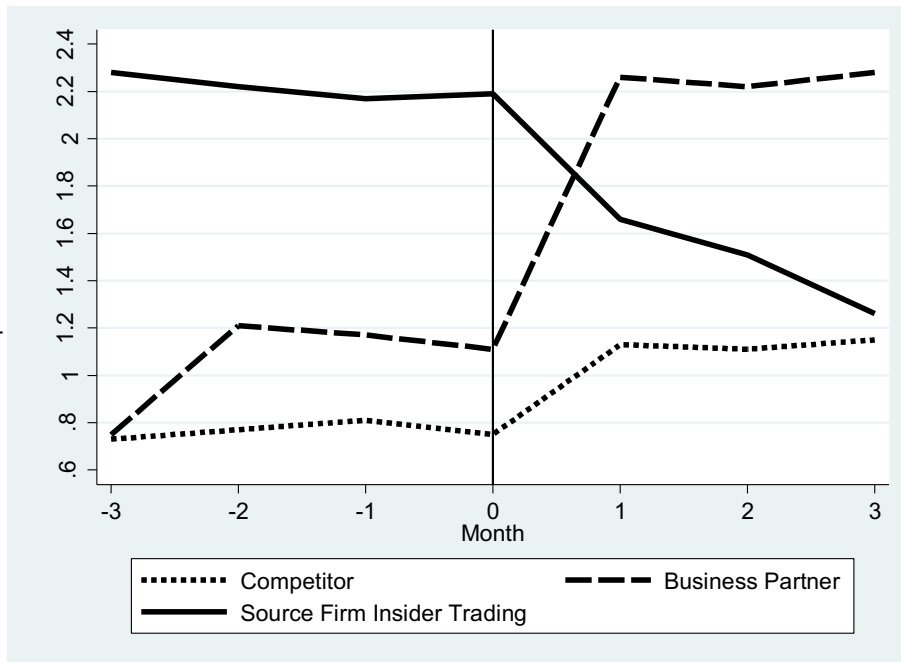
### Panel A: Insider Trading Index



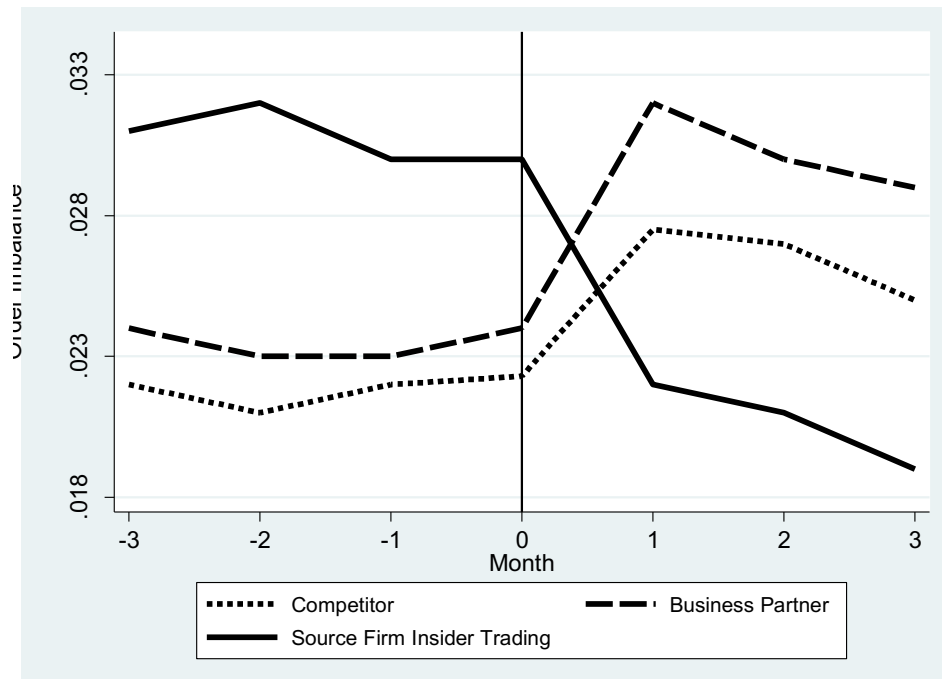
### Panel B: Abnormal Short Sales



**Panel C: Option/Stock Ratio**



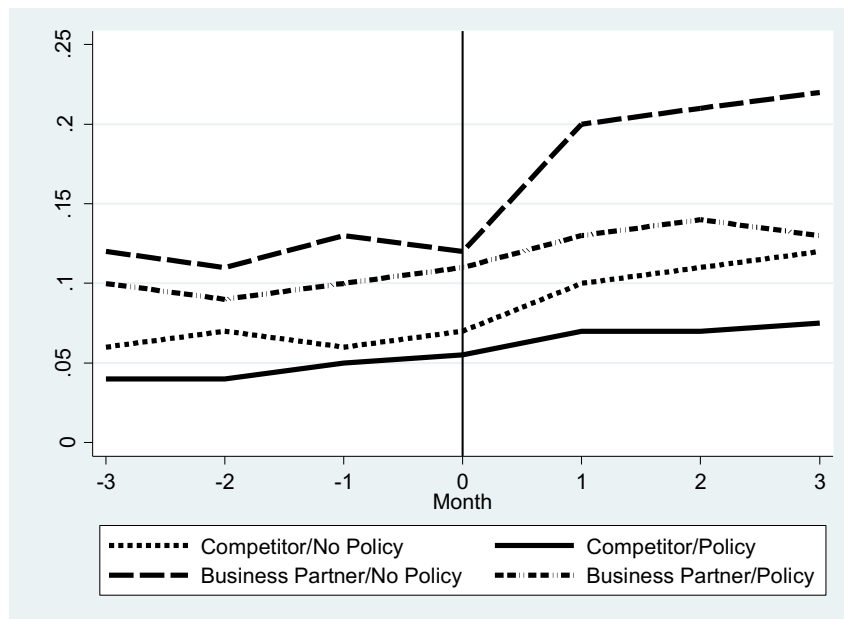
**Panel D: Order Imbalance**



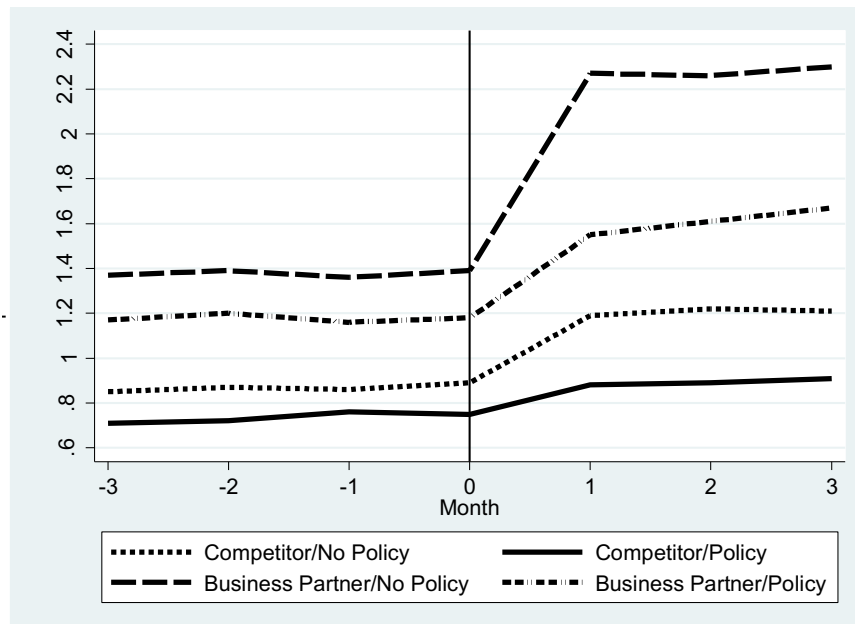
### Figure 3. Insider Trading Index Shock: Source Firms With vs. Without Shadow Trading Policies

Panels A to C present graphical time series of shadow trading in linked firms for the three-month window around business partner or competitor source firm earnings announcements. The graphs are centered around insider trading enforcement index shocks (Month = 0). Panel A (Panel B) [Panel C] displays a time series of shadow trading for linked firm abnormal short sales (option/stock ratio) [order imbalance].

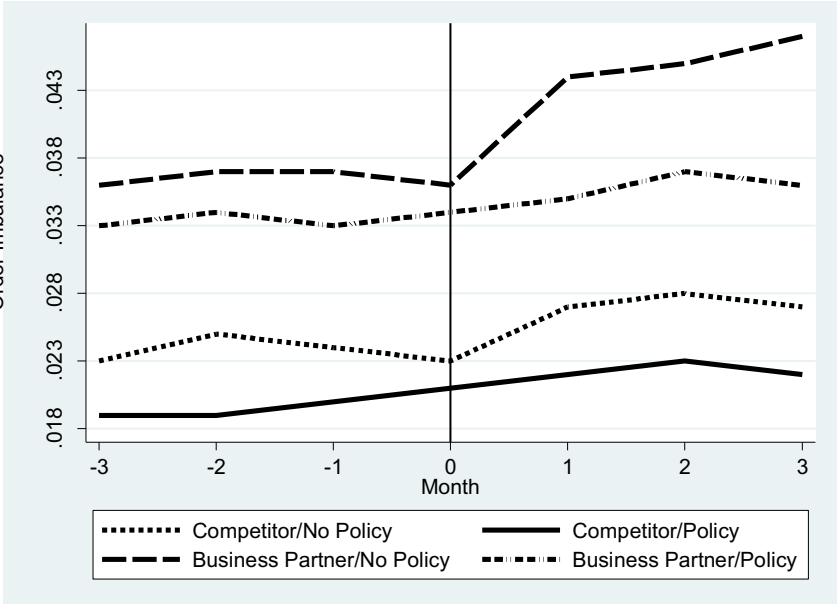
#### Panel A: Abnormal Short Sales in Source Firm Stakeholders



#### Panel B: Option/Stock Ratio in Source Firm Stakeholders



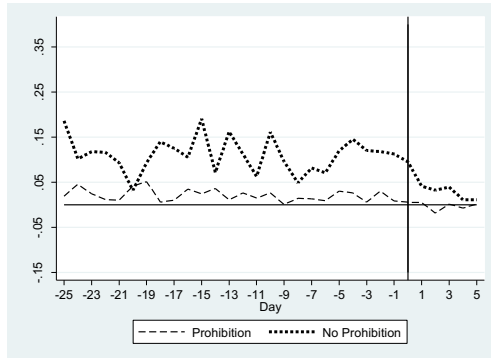
Panel C: Order Imbalance in Source Firm Stakeholders



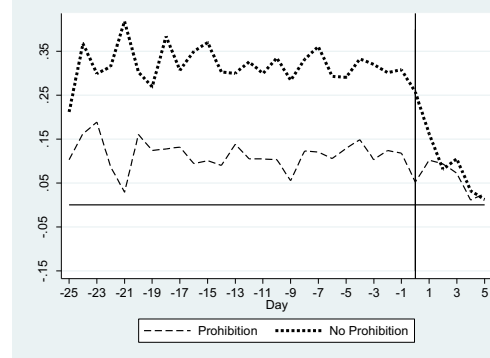
## Figure 4. Source Firms With vs. Without Shadow Trading Policies

Panels A-F present graphical time series of shadow trading proxies in linked firms around business partner or competitor source firm earnings announcements centered at day 0. Each panel presents the proxies for groups of firms based on whether they prohibit shadow trading or do not prohibit shadow trading. Panels A and B display a time series of linked firm abnormal short sales; Panels C and D display a time series of linked firm option/stock ratio values; and Panels E and F display a time series of linked firm order imbalance values.

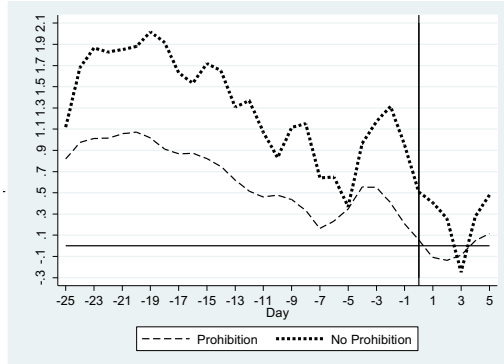
**Panel A: Abnormal Short Sales  
In Source Firm Competitors**  
(Negative Earnings Shock sample;  
Day 0 = Source Firm Earnings Announcement)



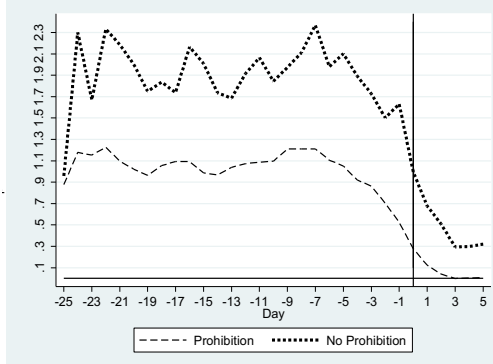
**Panel B: Abnormal Short Sales  
In Source Firm Business Partners**  
(Negative Earnings Shock sample;  
Day 0 = Source Firm Earnings Announcement)



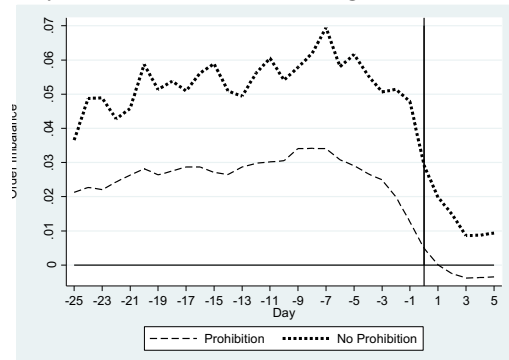
**Panel C: Option/Stock Ratio  
In Source Firm Competitors**  
Day 0 = Source Firm Earnings Announcement



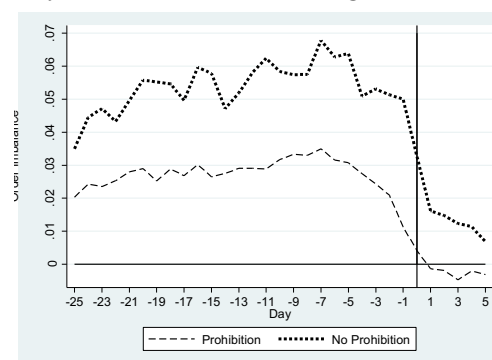
**Panel D: Option/Stock Ratio  
In Source Firm Business Partners**  
Day 0 = Source Firm Earnings Announcement



**Panel E: Order Imbalance  
In Source Firm Competitors**  
Day 0 = Source Firm Earnings Announcement



**Panel F: Order Imbalance  
In Source Firm Business Partners**  
Day 0 = Source Firm Earnings Announcement



## Table 1. Variable Definitions

### *Dependent Variables*

Short Sales: Daily short sale volume divided by total stock trading volume.

Abnormal Short Sales: ([linked firm  $i$ 's average daily short sales prior to a source firm's quarterly earnings announcement (day  $t-30$  to  $t-1$ ) divided by linked firm  $i$ 's average daily short sales for the year outside of the source firm's earnings announcement windows] - 1).

Option/Stock Ratio: Average daily option/stock trading volume for linked firm  $i$  during the 30 days prior to source firm A's quarterly earnings announcement date.

Order Imbalance: Daily institutional buys minus institutional sales scaled by total trading volume for linked firm  $i$  during the 30 days prior to source firm A's quarterly earnings announcement date.

### *Primary Independent Variables*

CAR: Cumulative abnormal return, calculated as the sum of the daily abnormal returns. Daily abnormal return is the raw return minus the market return.

Business Partner CAR: Business partner source firm CAR from day  $t-1$  to  $t+1$  where  $t$  is the earnings announcement date.

Competitor CAR: Competitor source firm CAR from day  $t-1$  to  $t+1$  where  $t$  is the earnings announcement date.

Business Partner StockShock: The absolute CAR of the business partner linked firm between day  $t+1$  and day  $t+5$ , where  $t$  represents the day of the source firm shock.

Competitor StockShock: The absolute CAR of the competitor-linked firm between day  $t+1$  and day  $t+5$ , where  $t$  represents the day of the source firm shock.

Post: An indicator variable set to one if the year is after 2002 and set to zero otherwise.

Prohibit: An indicator variable set to one if the source firm has an explicit policy prohibiting its employees from engaging in shadow trading, and set to zero otherwise.

IDDSHOCK: An indicator variable set to one (zero) following the adoption (rejection), and zero (one) for the pre-shock observation for the source firms in states that adopt (reject) the IDD.

### *Source Firm Control Variables*

Firm Size Source: Log of total assets.

Book-to-Market Source: log of book value of common equity plus deferred taxes scaled by market value of common equity at the end of fiscal quarter.

FREV Source: Rolling sum of the preceding 30-day earnings forecast revisions scaled by stock price.

TACC Source: Total accruals scaled by average assets measured at the end of quarter.

Past Return Source: Market-adjusted return during the 30-day window prior to earnings announcement.

E/P Source: Ratio of the rolling sum of earnings over the preceding four quarters to price at the end of quarter.



Turnover Source: Average daily volume turnover ratio measured as the exchange-specific, percentile rank, based on the six-month period prior to calendar quarter.

Sales Growth Source: Rolling sum of sales growth over the preceding four fiscal quarters.

LTG Source: Mean consensus long-term earnings growth forecast at the end of calendar quarter t.

Momentum Source: Price momentum, measured as the six-month raw return ending one month prior to the end of the fiscal quarter.

MISP Source: the mispricing measure of the prior month as developed by Stambaugh, Yu, and Yuan (2015).

### ***Linked Firm Control Variables***

Firm Size: Log of total assets.

FREV: Rolling sum of the preceding 30-day earnings forecast revisions scaled by stock price.

TACC: Total accruals scaled by average assets measured at the end of quarter.

Past Return: Market-adjusted return during the 30-day window prior to earnings announcement.

E/P: Ratio of the rolling sum of earnings over the preceding four quarters to price at the end of quarter (Drake et al., 2011).

Sales Growth: Rolling sum of sales growth over the preceding four fiscal quarters (Drake et al., 2011).

LTG: Mean consensus long-term earnings growth forecast at the end of calendar quarter.

Momentum: Price momentum, measured as the six-month raw return ending one month prior to the end of the fiscal quarter.

MISP: the mispricing measure of the prior month as developed by Stambaugh, Yu, and Yuan (2015).

Book-to-Market: log of book value of common equity plus deferred taxes scaled by market value of common equity at the end of fiscal quarter.

Turnover: Average daily volume turnover ratio measured as the exchange-specific, percentile rank, based on the six-month period prior to the calendar quarter.

**Table 2. Summary Statistics**

Table 2 presents summary statistics for sample source firms and linked firms. Panel A displays summary statistics for source firms. Panel B presents descriptive data for sample linked firms. Panel C presents the top ten industries represented for sample source and linked firms. All variables are defined in Table 1.

**Panel A: Source Firm Characteristics**

	Mean	Median	Std. Dev.	Lower Quartile	Upper Quartile
<i>CAR - Negative Earnings Shock (business partner)</i>	-0.035	-0.028	0.256	-0.323	0.062
<i>CAR - Negative Earnings Shock (competitor)</i>	-0.033	-0.030	0.229	-0.292	0.068
<i>CAR - Positive Earnings Shock (business partner)</i>	0.021	0.011	0.219	-0.040	0.278
<i>CAR - Positive Earnings Shock (competitor)</i>	0.023	0.012	0.228	-0.043	0.235
<i>Total Assets (\$million)</i>	6,137	1,229	9,162	287	7,263
<i>Book-to-Market</i>	0.818	0.778	0.517	0.477	1.113
<i>FREV</i>	-0.018	0.002	0.102	-0.039	0.025
<i>Total Accruals</i>	-0.027	-0.012	0.072	-0.047	0.003
<i>E/P</i>	-0.012	0.004	0.080	-0.021	0.015
<i>Turnover</i>	0.517	0.525	0.267	0.281	0.748
<i>Sales Growth</i>	0.024	0.011	0.086	-0.014	0.040
<i>LTG</i>	13.490	12.000	9.912	7.050	17.500
<i>Momentum</i>	0.090	0.047	0.233	-0.052	0.172
<i>MISP</i>	54.566	55.238	11.655	46.179	62.784
<i>Past Return</i>	0.027	0.030	0.592	-0.089	0.111

**Panel B: Linked Firm Descriptive Statistics**

	Mean	Median	Std. Dev.	Lower Quartile	Upper Quartile
Linked Firm Abnormal Short Sales prior to Source Firm:					
<i>Negative Earnings Announcement CAR</i>	0.079	0.055	0.279	-0.088	0.225
<i>Positive Earnings Announcement CAR</i>	-0.036	-0.029	0.332	-0.193	0.122
Linked Firm Option/Stock Ratio prior to Source Firm:					
<i>Negative Earnings Announcement CAR</i>	2.336	1.527	3.221	0.420	4.293
<i>Positive Earnings Announcement CAR</i>	1.496	0.972	2.558	0.176	3.056
Linked Firm Order Imbalance prior to Source Firm:					
<i>Negative Earnings Announcement CAR</i>	-0.032	-0.037	0.072	-0.962	0.928
<i>Positive Earnings Announcement CAR</i>	0.025	0.032	0.075	-0.919	0.992
<i>Total Assets (\$million)</i>	4,434	814	16,040	335	2,530
<i>Book-to-Market</i>	0.782	0.750	0.509	0.435	1.003
<i>FREV</i>	-0.012	0.003	0.095	-0.029	0.033
<i>Total Accruals</i>	-0.023	-0.009	0.070	-0.050	0.003
<i>E/P</i>	-0.007	0.005	0.076	-0.017	0.014

<i>Turnover</i>	0.522	0.530	0.273	0.280	0.755
<i>Sales Growth</i>	0.017	0.010	0.056	-0.002	0.037
<i>LTG</i>	15.552	13.000	9.156	8.273	19.108
<i>Momentum</i>	0.085	0.050	0.267	-0.060	0.182
<i>MISP</i>	53.223	54.902	10.985	48.192	61.007
<i>Past Return</i>	0.022	0.025	0.617	-0.099	0.126

**Panel C: Industry Representation**

Industry Name	% of Source Firms	Industry Name	% of Linked Firms
<i>Drugs</i>	10.02	<i>Drugs</i>	8.91
<i>Business Services</i>	9.71	<i>Energy</i>	8.51
<i>Energy</i>	7.27	<i>Business Services</i>	8.24
<i>Chemicals</i>	5.25	<i>Meals</i>	6.68
<i>Telecommunications</i>	4.34	<i>Machinery</i>	5.74
<i>Retail</i>	4.22	<i>Health</i>	3.85
<i>Machinery</i>	3.73	<i>Building Materials</i>	3.71
<i>Health</i>	3.60	<i>Retail</i>	3.51
<i>Electric Equipment</i>	3.54	<i>Transportation</i>	3.51
<i>Building Materials</i>	3.48	<i>Wholesale</i>	3.31
Total	55.16	Total	55.97

### Table 3. Earnings Shocks and Shadow Trading

This table presents coefficients from regressions testing whether a source firm's competitors and business partners experience abnormal trading in the 30-day window prior to the source firm's earnings announcement. We measure abnormal trading using three proxies: (1) *Abnormal Short Sales*; (2) *Option/Stock Ratio*; and (3) *Order Imbalance*. The samples used for each regression are partitioned based on the sign (*Negative* or *Positive*) of the source firm Cumulative Abnormal Return (CAR) from day  $t-1$  to  $t+1$  where  $t$  is the source firm's earnings announcement date. All variables are defined in Table 1.  $t$ -values are reported in parentheses based on standard errors clustered by firm and year. All specifications include year and Fama-French industry fixed effects. Statistical significance at the 1%, 5%, and 10% level is denoted by \*\*\*, \*\*, and \*, respectively.

	<i>Abnormal Short Sales</i>		<i>Option/Stock Ratio</i>		<i>Order Imbalance</i>	
	Negative	Positive	Negative	Positive	Negative	Positive
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Business Partner CAR</i> ( $\beta_1$ )	0.033** (2.55)	-0.019** (-2.11)	0.699** (2.33)	0.621** (2.49)	0.011** (2.41)	0.008** (2.26)
<i>Competitor CAR</i> ( $\beta_2$ )	0.031** (2.25)	-0.020** (-2.20)	0.611** (2.09)	0.581** (2.17)	0.009* (1.72)	0.008** (2.23)
<i>Firm Size</i>	-0.019 (-1.22)	0.007 (0.60)	-0.300 (-1.17)	-0.089 (-1.03)	0.010 (1.11)	0.009 (1.22)
<i>Past Return</i>	0.132 (1.31)	-0.092 (-0.88)	2.356 (1.30)	1.722 (1.25)	0.018 (0.78)	0.013 (0.73)
<i>FREV</i>	0.056 (1.31)	0.033 (1.46)	1.002* (1.90)	0.523* (1.92)	0.012* (1.90)	0.011** (2.01)
<i>Book-to-Market</i>	0.017 (0.47)	0.042 (0.97)	0.287 (1.22)	0.722 (1.45)	0.005 (0.66)	0.013 (0.23)
<i>Total Accruals</i>	0.316* (1.90)	0.143* (1.95)	1.034* (1.69)	2.322* (1.80)	0.078* (1.74)	0.055 (1.56)
<i>E/P</i>	-0.120 (-0.53)	0.143 (0.67)	-1.902 (-0.89)	-2.009 (-1.33)	0.056 (0.89)	0.045 (0.88)
<i>Turnover</i>	-0.026 (-1.13)	-0.014 (-1.53)	-0.293 (-1.52)	-0.200 (-1.32)	-0.005 (-1.55)	-0.007 (-1.38)
<i>Sales Growth</i>	0.308 (1.48)	0.158 (1.33)	2.126* (1.67)	0.788* (1.90)	0.089 (1.33)	0.022 (1.36)
<i>LTG</i>	-0.001 (-0.67)	-0.003** (-2.13)	-0.013 (-1.23)	-0.021* (-1.80)	-0.001 (-0.44)	-0.001 (-0.27)
<i>Momentum</i>	-0.082 (-0.88)	0.079 (1.13)	-1.221 (-1.09)	-0.936 (-1.23)	-0.033 (-1.00)	-0.020 (-1.33)
<i>MISP</i>	0.002 (1.20)	0.002 (1.52)	0.027 (1.11)	0.023 (1.20)	0.002 (1.38)	0.003 (1.62)
<i>Firm Size Source</i>	0.011 (1.22)	-0.005 (-1.29)	0.187 (1.39)	0.079 (1.27)	0.003 (1.11)	0.002 (1.26)
<i>Past Return Source</i>	-0.035 (-1.20)	0.040 (1.37)	-0.556* (-1.76)	-0.366 (-1.60)	-0.009 (-1.60)	-0.012* (-1.77)
<i>FREV Source</i>	0.022 (1.50)	-0.017 (-1.09)	0.401* (1.79)	0.282 (1.52)	0.007 (1.28)	0.005 (0.80)
<i>Book-to-Market Source</i>	-0.018 (-0.82)	-0.037 (-1.09)	-0.322 (-1.10)	-0.562 (-0.99)	-0.005 (-0.90)	-0.009 (-1.20)
<i>Total Accruals Source</i>	0.022 (0.70)	0.050 (0.66)	0.322 (0.78)	0.722 (1.27)	0.005 (1.12)	0.012 (1.43)
<i>E/P Source</i>	0.070 (1.26)	-0.111 (-1.38)	1.110 (1.51)	1.667* (1.89)	0.022 (1.23)	-0.033 (-1.54)
<i>Turnover Source</i>	0.025 (0.23)	0.011 (0.80)	0.422 (0.56)	0.156 (1.02)	0.010 (0.67)	0.003 (0.72)
<i>Sales Growth Source</i>	0.156 (1.00)	-0.022 (-0.92)	2.443 (1.38)	0.311 (1.24)	0.067 (0.78)	0.020 (1.45)
<i>LTG Source</i>	-0.002* (-1.88)	0.006* (1.77)	-0.025* (-1.69)	-0.011* (-1.90)	-0.001 (-0.56)	0.002 (1.35)
<i>Momentum Source</i>	0.056 (1.39)	-0.067 (-1.45)	1.009 (1.58)	1.022 (1.32)	0.021 (1.01)	0.024 (1.12)

<i>MISP Source</i>	0.003 (1.39)	0.002 (1.32)	0.035 (1.52)	0.031 (1.60)	0.002 (0.78)	0.002 (0.87)
<i>F-test: <math>\beta_1 = \beta_2</math></i>	0.21	0.65	0.82	0.57	0.84	0.95
Year & Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,129	1,982	2,882	4,912	3,226	5,228
Adjusted R <sup>2</sup>	0.189	0.180	0.142	0.127	0.113	0.115

**Table 4. Shadow Trading Using Alternative Information Shocks**

This table presents coefficients from regressions testing whether a source firm's competitors and business partners experience abnormal trading in the 30-day window prior to the source firm's M&A announcement (Panel A) and new product announcement (Panel B). We measure abnormal trading using three proxies: (1) Abnormal Short Sales; (2) Option/Stock Ratio; and (3) Order Imbalance. The samples used for each regression are partitioned based on the sign (Negative or Positive) of the source firm Cumulative Abnormal Return (CAR) from day  $t-1$  to  $t+1$  where  $t$  is the source firm's news announcement date. All variables are defined in Table 1.  $t$ -values are reported in parentheses based on standard errors clustered by firm and year. All specifications include year and Fama-French industry fixed effects. Statistical significance at the 1%, 5%, and 10% level is denoted by \*\*\*, \*\*, and \*, respectively.

**Panel A: Shadow trading prior to M&A news announcements**

	Abnormal Short Sale		Option/Stock Ratio		Order Imbalance	
	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(6)	(7)
<i>Business Partner CAR</i> ( $\beta_1$ )	0.012*** (2.95)	0.018*** (3.60)	0.348* (1.80)	0.238*** (4.50)	0.065** (2.31)	0.117*** (3.83)
<i>Competitor CAR</i> ( $\beta_2$ )	0.021 (1.21)	0.011 (0.59)	0.027 (0.45)	0.027 (0.72)	0.128 (1.62)	-0.115 (-1.26)
<i>Firm Size</i>	0.001*** (3.30)	0.002*** (8.98)	0.034 (0.13)	-0.248 (-1.36)	-0.009 (-0.22)	-0.039 (-0.90)
<i>FREV</i>	-0.005 (-1.61)	0.001 (0.30)	-10.374 (-1.16)	-0.367 (-0.15)	0.159 (0.30)	0.057 (0.07)
<i>Book-to-Market</i>	0.001 (1.17)	0.005*** (3.62)	3.276 (0.99)	-1.057 (-1.00)	-0.124 (-0.47)	-0.051 (-0.18)
<i>Total Accruals</i>	0.004 (1.48)	0.009*** (2.88)	0.396 (0.19)	-1.157 (-0.86)	0.193 (0.35)	0.089 (0.26)
<i>E/P</i>	0.017*** (3.71)	-0.014 (-1.26)	75.954 (1.08)	4.821 (0.52)	-2.937 (-1.11)	-4.587* (-1.89)
<i>Turnover</i>	-0.000*** (-7.61)	-0.000*** (-9.67)	-0.026 (-0.78)	-0.021 (-1.57)	0.001 (0.43)	-0.004 (-1.28)
<i>Sales Growth</i>	0.004 (0.82)	0.010* (1.88)	5.994 (0.49)	-0.151 (-0.07)	0.080 (0.09)	1.180 (1.21)
<i>LTG</i>	-0.000*** (-2.71)	-0.000*** (-6.64)	-0.058 (-0.65)	0.027 (0.67)	-0.003 (-0.33)	-0.004 (-0.34)
<i>Momentum</i>	0.002* (1.85)	0.003** (2.18)	0.784 (0.41)	0.536 (0.42)	0.070 (0.23)	-0.616* (-1.79)
<i>MISP</i>	0.000*** (3.03)	0.000*** (8.13)	0.029 (0.86)	-0.016 (-0.70)	0.004 (0.84)	0.003 (0.66)
<i>Firm Size Source</i>	-0.005*** (-18.11)	-0.004*** (-24.92)	1.279** (2.38)	1.520*** (11.32)	-0.017 (-0.48)	-0.052 (-1.50)
<i>FREV Source</i>	0.002 (0.55)	-0.006 (-1.64)	14.802 (1.09)	-0.937 (-0.38)	0.622 (1.05)	-0.601 (-1.13)
<i>Book-to-Market Source</i>	0.019*** (8.37)	0.018*** (9.96)	-7.198*** (-2.70)	-2.712*** (-3.31)	0.215 (0.82)	0.402 (1.55)
<i>Total Accruals Source</i>	0.001 (0.27)	-0.042*** (-12.09)	-5.094 (-1.58)	-1.665 (-0.93)	-0.903* (-1.90)	-0.075 (-0.17)
<i>E/P Source</i>	0.415*** (8.66)	0.015 (0.40)	3.921 (0.54)	5.236 (1.29)	2.072 (1.12)	0.232 (0.14)
<i>Turnover Source</i>	0.000*** (11.23)	0.001*** (31.76)	0.097** (1.98)	0.067*** (4.81)	0.000 (0.01)	-0.000 (-0.13)
<i>Sales Growth Source</i>	0.036*** (4.15)	0.042*** (6.80)	1.463 (0.22)	-4.976** (-2.31)	-0.232 (-0.32)	-0.726 (-0.90)
<i>LTG Source</i>	0.000*** (3.91)	0.000 (0.31)	0.022 (0.20)	0.113*** (3.07)	-0.004 (-0.50)	-0.011 (-1.56)
<i>Momentum Source</i>	-0.008*** (-3.89)	0.001 (0.51)	-0.134 (-0.11)	0.499 (0.80)	0.260 (1.05)	0.747*** (2.66)
<i>MISP Source</i>	0.001*** (21.67)	0.001*** (24.40)	-0.014 (-0.52)	-0.018 (-0.65)	-0.000 (-0.02)	0.005 (0.94)
<i>F-test: <math>\beta_1 = \beta_2</math></i>	0.61	0.73	0.16	0.00***	0.55	0.01**

Year & Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,596	12,164	2,048	1,979	1,758	1,694
Adjusted R <sup>2</sup>	0.514	0.430	0.072	0.365	0.025	0.041

### Panel B Shadow trading prior to new product announcements

	Abnormal Short Sale		Option/Stock Ratio		Order Imbalance	
	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Business Partner CAR (<math>\beta_1</math>)</i>	-0.009*** (-5.04)	0.003 (1.29)	0.006*** (5.06)	0.003** (2.17)	0.124*** (8.99)	0.126*** (8.78)
<i>Competitor CAR (<math>\beta_2</math>)</i>	0.029*** (3.68)	-0.038*** (-4.55)	-0.001 (-1.60)	0.001 (1.42)	0.069 (0.69)	0.071 (1.20)
<i>Firm Size</i>	-0.005*** (-52.26)	-0.005*** (-53.54)	-0.000 (-0.37)	0.000 (0.19)	0.004 (1.00)	0.005 (1.08)
<i>FREV</i>	-0.003* (-1.73)	0.000 (0.12)	-0.003 (-1.31)	-0.002 (-0.53)	-0.255** (-2.02)	-0.277** (-2.54)
<i>Book-to-Market</i>	0.005*** (5.89)	0.004*** (4.37)	-0.001** (-2.19)	-0.001** (-2.01)	-0.005 (-0.13)	0.004 (0.10)
<i>Total Accruals</i>	0.002 (1.00)	0.001 (0.44)	0.001 (0.93)	0.001 (1.19)	0.024 (0.58)	0.010 (0.25)
<i>E/P</i>	-0.049*** (-6.10)	-0.042*** (-4.78)	0.001 (0.29)	0.001 (0.24)	-0.240 (-1.31)	-0.171 (-0.91)
<i>Turnover</i>	0.000*** (46.98)	0.000*** (46.64)	0.000 (0.04)	0.000 (1.01)	-0.001 (-1.34)	-0.000 (-0.68)
<i>Sales Growth</i>	0.046*** (10.03)	0.043*** (9.82)	-0.002 (-0.93)	-0.000 (-0.06)	0.253** (2.07)	0.370*** (2.94)
<i>LTG</i>	0.000*** (2.64)	0.000* (1.90)	-0.000 (-1.19)	-0.000 (-0.31)	0.002 (1.30)	0.001 (0.56)
<i>Momentum</i>	0.010*** (11.49)	0.007*** (8.30)	0.000 (0.78)	0.001 (0.99)	-0.070** (-2.47)	-0.105*** (-3.52)
<i>MISP</i>	0.001*** (48.08)	0.001*** (49.74)	0.000 (1.40)	0.000 (0.36)	-0.001 (-0.80)	-0.001 (-0.69)
<i>Firm Size Source</i>	0.001*** (7.39)	0.001*** (8.50)	-0.000*** (-5.14)	-0.000** (-2.37)	-0.021*** (-4.51)	-0.019*** (-3.54)
<i>FREV Source</i>	0.002 (0.99)	0.002 (1.01)	0.008** (2.33)	0.009** (2.01)	0.946*** (6.99)	0.965*** (6.21)
<i>Book-to-Market Source</i>	-0.000 (-0.55)	-0.001* (-1.87)	0.001 (1.35)	0.001 (0.94)	0.266*** (6.93)	0.290*** (6.99)
<i>Total Accruals Source</i>	0.003** (2.02)	0.002 (1.41)	0.012*** (5.53)	0.012*** (4.45)	-0.747*** (-7.14)	-0.777*** (-6.33)
<i>E/P Source</i>	-0.002 (-0.50)	-0.003 (-0.80)	-0.010 (-0.89)	0.003 (0.29)	1.683*** (5.41)	1.791*** (7.42)
<i>Turnover Source</i>	-0.000*** (-13.11)	-0.000*** (-14.03)	0.000 (0.91)	0.000* (1.90)	-0.001 (-1.06)	-0.001 (-1.13)
<i>Sales Growth Source</i>	0.014*** (4.87)	0.014*** (4.70)	0.047*** (9.21)	0.046*** (8.39)	-0.266 (-1.30)	-0.219 (-0.97)
<i>LTG Source</i>	-0.000*** (-7.93)	-0.000*** (-7.10)	-0.000*** (-5.66)	-0.000*** (-4.10)	-0.006*** (-3.84)	-0.004*** (-2.84)
<i>Momentum Source</i>	-0.001 (-1.56)	-0.001 (-1.46)	0.027*** (21.73)	0.027*** (20.83)	0.523*** (13.58)	0.535*** (10.45)
<i>MISP Source</i>	0.000*** (9.17)	0.000*** (10.32)	0.000*** (9.55)	0.000*** (7.98)	0.007*** (8.72)	0.006*** (8.21)
<i>F-test: <math>\beta_1 = \beta_2</math></i>	21.44***	21.65***	6.59***	3.09*	5.66**	4.57**
Year & Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	215,601	217,864	1,300,637	1,241,346	970,139	929,610
Adjusted R <sup>2</sup>	0.419	0.422	0.017	0.017	0.035	0.035

**Table 5. Future Returns and Shadow Trading in Linked Firms**

This table presents regressions of future returns for linked firms on informed trading in linked firms prior to source firm earnings shocks. We control for linked firm and source firm characteristics. The future return is the abnormal return of the linked firm ( $t+1, t+5$ ) around the source firm earnings announcement at day  $t$ . The independent variable is the informed trading measures of the linked firms in the 30-day window prior to source firm earnings announcements.  $t$ -values are reported in parentheses based on standard errors clustered by firm and year. All specifications include year and Fama-French industry fixed effects. Statistical significance at the 1%, 5%, and 10% level is denoted by \*\*\*, \*\*, and \*, respectively.

	Future Return		
	(1)	(2)	(3)
<i>Abnormal Short Sales</i>	-0.311** (-2.52)	-	-
<i>Option/Stock Ratio</i>	-	0.261** (2.40)	-
<i>Order Imbalance</i>	-	-	0.229*** (3.33)
<i>Firm Size</i>	-0.070** (-2.52)	-0.121** (-2.33)	-0.111** (-2.15)
<i>Past Return</i>	0.109* (1.88)	0.132* (1.78)	0.120* (1.90)
<i>Book-to-Market</i>	-0.027 (-1.60)	-0.025 (-1.60)	-0.022 (-1.32)
<i>Momentum</i>	0.041* (1.70)	0.039* (1.88)	0.030* (1.80)
<i>MISP</i>	0.003 (1.35)	0.003 (1.28)	0.003 (1.21)
Year & Industry Fixed Effects	Yes	Yes	Yes
Observations	3,111	7,794	8,454
Adjusted R <sup>2</sup>	0.282	0.233	0.220



**Table 6. Shadow Trading Following Increased Attention to Conventional Insider Trading**

This table presents coefficients from regressions testing whether the magnitude of shadow trading in source firms' competitors and business partners increases around spikes in an insider trading index. The insider trading index is a monthly count of the number of articles in major U.S. newspapers that include the term "insider trading". *Post* is an indicator variable set to one for all three-month periods following insider trading index spikes. We measure abnormal trading using one of three proxies: *Abnormal Short Sales*, *Option/Stock Ratio*, or *Order Imbalance*. Sample observations are partitioned based on the sign (Negative or Positive) of the source firm's Cumulative Abnormal Return (CAR) from day  $t-1$  to  $t+1$  where  $t$  is the source firm's earnings announcement date. All variables are defined in Table 1.  $t$ -values are reported in parentheses based on standard errors clustered by firm and year. All specifications include year and Fama-French industry fixed effects. Statistical significance at the 1%, 5%, and 10% level is denoted by \*\*\*, \*\*, and \*, respectively.

	<i>Abnormal Short Sales</i>		<i>Option/Stock Ratio</i>		<i>Order Imbalance</i>	
	Negative (1)	Positive (2)	Negative (3)	Positive (4)	Negative (5)	Positive (6)
<i>Business Partner CAR</i>	0.017* (1.91)	-0.013* (-1.92)	0.227** (2.21)	0.192** (2.15)	0.008** (2.28)	0.008* (1.85)
<i>Competitor CAR</i>	0.016* (1.81)	-0.010* (-1.88)	0.192** (2.25)	0.221** (2.11)	0.007* (1.82)	0.005* (1.79)
<i>Post</i>	0.011 (1.40)	0.007 (1.15)	0.117** (2.03)	0.102** (2.11)	0.006* (1.90)	0.004* (1.83)
<i>Post * Business Partner CAR</i>	0.019*** (2.60)	-0.013** (-2.50)	0.191*** (2.72)	0.178*** (2.59)	0.007*** (2.71)	0.006** (2.55)
<i>Post * Competitor CAR</i>	0.013** (2.22)	-0.012* (-1.82)	0.120** (2.19)	0.125** (2.36)	0.004* (1.82)	0.004** (2.12)
<i>Firm Size</i>	-0.015 (-1.09)	0.006 (0.77)	-0.202 (-1.20)	-0.076 (-0.95)	0.011 (1.26)	0.010 (1.21)
<i>Past Return</i>	0.111 (1.17)	-0.072 (-0.80)	1.782 (1.20)	1.883 (1.51)	0.022 (1.20)	0.011 (0.79)
<i>FREV</i>	0.060 (1.31)	0.035 (1.42)	0.832* (1.77)	0.572* (1.88)	0.011* (1.82)	0.011* (1.90)
<i>Book-to-Market</i>	0.011 (0.49)	0.042 (1.15)	0.312 (1.55)	0.627 (1.30)	0.005 (1.02)	0.012 (0.70)
<i>Total Accruals</i>	0.290* (1.90)	0.149** (2.11)	0.930* (1.92)	2.109* (1.88)	0.085* (1.88)	0.072* (1.82)
<i>E/P</i>	-0.111 (-0.95)	0.129 (0.75)	-1.267 (-0.66)	-1.732 (-1.20)	0.068 (1.25)	0.055 (1.19)
<i>Turnover</i>	-0.028 (-1.30)	-0.016 (-1.46)	-0.242 (-1.51)	-0.222 (-1.46)	-0.004 (-1.25)	-0.007 (-1.31)
<i>Sales Growth</i>	0.251 (1.38)	0.130 (1.40)	1.437 (1.50)	0.509 (1.50)	0.082 (1.30)	0.028 (1.33)
<i>LTG</i>	-0.001 (-0.60)	-0.003* (-1.90)	-0.009 (-1.29)	-0.012 (-1.31)	-0.002 (-0.87)	-0.002 (-0.66)
<i>Momentum</i>	-0.072 (-0.78)	0.074 (1.11)	-0.660 (-0.90)	-0.801 (-1.22)	-0.030 (-0.99)	-0.022 (-1.35)
<i>MISP</i>	0.002 (1.02)	0.002 (1.28)	0.024 (1.22)	0.029 (1.36)	0.002 (1.06)	0.003 (1.45)
<i>Firm Size Source</i>	0.010 (1.15)	-0.005 (-1.22)	0.121 (0.81)	0.080 (1.05)	0.003 (0.99)	0.003 (1.02)
<i>Past Return Source</i>	-0.032 (-1.20)	0.034 (1.26)	-0.378 (-1.28)	-0.241 (-1.50)	-0.009 (-1.21)	-0.010 (-1.36)
<i>FREV Source</i>	0.020 (1.26)	-0.019 (-1.26)	0.225 (1.26)	0.220 (1.35)	0.008 (1.39)	0.006 (0.97)
<i>Book-to-Market Source</i>	-0.027 (-1.11)	-0.039 (-1.20)	-0.337 (-1.02)	-0.556 (-1.28)	-0.008 (-1.33)	-0.010 (-1.21)

<i>Total Accruals Source</i>	0.026 (0.89)	0.049 (0.90)	0.115 (0.69)	0.326 (0.88)	0.005 (0.87)	0.007 (1.11)
<i>E/P Source</i>	0.071 (1.42)	-0.104 (-1.41)	1.255* (1.90)	1.828* (1.92)	0.025 (1.50)	0.024 (1.42)
<i>Turnover Source</i>	0.022 (0.30)	0.012 (0.85)	0.302 (0.90)	0.126 (1.20)	0.011 (0.82)	0.005 (1.25)
<i>Sales Growth Source</i>	0.160 (0.99)	-0.020 (-0.91)	1.298 (1.00)	0.678 (1.33)	0.062 (0.92)	0.055 (1.01)
<i>LTG Source</i>	-0.003* (-1.80)	0.006* (1.91)	-0.022* (-1.77)	-0.015** (-2.01)	-0.002 (-1.25)	-0.002 (-1.52)
<i>Momentum Source</i>	0.053 (1.22)	-0.062 (-1.35)	0.778 (1.50)	0.764 (1.20)	0.023 (1.18)	0.022 (1.20)
<i>MISP Source</i>	0.003 (1.22)	0.002 (1.05)	0.025 (1.33)	0.032 (1.50)	0.003 (1.07)	0.003 (1.03)
Year & Industry Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Observations	220	305	519	742	582	720
Adjusted R <sup>2</sup>	0.180	0.171	0.179	0.158	0.146	0.139

**Table 7. IDD Legal Case Shock and Shadow Trading**

This table presents coefficients from regressions testing whether shadow trading in competitors and business partners varies when source firms experience an IDD legal case shock. This shock affects the ability of firm insiders with knowledge of the firm's trade secrets to obtain employments with competitors. All variables are defined in Table 1. We measure shadow trading using two proxies: (1) *Option/Stock Ratio*; and (2) *Order Imbalance*. The samples used for each regression are partitioned based on the sign (*Negative* or *Positive*) of the source firm's Cumulative Abnormal Return (CAR) from day  $t-1$  to  $t+1$  where  $t$  is the source firm's earnings announcement date. We measure shadow trading using Option/Stock Ratio and Order Imbalance.  $t$ -values are reported in parentheses based on standard errors clustered by firm and year. All specifications include year and Fama-French industry fixed effects. Statistical significance at the 1%, 5%, and 10% level is denoted by \*\*\*, \*\*, and \*, respectively.

	<i>Option/Stock Ratio</i>		<i>Order Imbalance</i>	
	Negative	Positive	Negative	Positive
	(1)	(2)	(3)	(4)
<i>Business Partner CAR</i>	0.456** (2.25)	0.432** (2.11)	0.008** (2.20)	0.005* (1.80)
<i>Competitor CAR</i>	0.410** (2.15)	0.401* (1.89)	0.006* (1.85)	0.005* (1.82)
<i>IDDShock</i>	0.111** (1.99)	0.121** (2.20)	0.005* (1.89)	0.005* (1.86)
<i>Business Partner CAR * IDDShock</i>	0.257*** (2.72)	0.222** (2.27)	0.005** (2.16)	0.005** (2.52)
<i>Competitor CAR * IDDShock</i>	0.202** (2.21)	0.198** (1.99)	0.004* (1.87)	0.004* (1.79)
<i>Firm Size</i>	-0.289 (-1.24)	-0.092 (-1.11)	0.009 (1.02)	0.010 (1.35)
<i>Past Return</i>	2.190 (1.31)	1.755 (1.33)	0.017 (0.79)	0.015 (0.89)
<i>FREV</i>	0.896* (1.88)	0.511* (1.90)	0.011* (1.78)	0.010* (1.92)
<i>Book-to-Market</i>	0.303 (1.30)	0.656 (1.23)	0.007 (0.78)	0.012 (0.35)
<i>Total Accruals</i>	1.190* (1.77)	2.109* (1.86)	0.075* (1.80)	0.052 (1.46)
<i>E/P</i>	-1.782 (-0.86)	-2.229 (-1.50)	0.053 (0.82)	0.049 (0.95)
<i>Turnover</i>	-0.260 (-1.42)	-0.178 (-1.22)	-0.005 (-1.46)	-0.006 (-1.32)
<i>Sales Growth</i>	2.001 (1.57)	0.821* (1.88)	0.076 (1.20)	0.026 (1.50)
<i>LTG</i>	-0.011 (-1.09)	-0.023* (-1.88)	-0.001 (-0.50)	-0.001 (-0.33)
<i>Momentum</i>	-1.156 (-1.02)	-0.882 (-1.20)	-0.031 (-1.01)	-0.022 (-1.35)
<i>MISP</i>	0.030 (1.18)	0.021 (1.15)	0.002 (1.44)	0.003 (1.56)
<i>Firm Size Source</i>	0.171 (1.25)	0.083 (1.36)	0.003 (1.02)	0.003 (1.40)
<i>Past Return Source</i>	-0.515* (-1.70)	-0.392* (-1.78)	-0.008 (-1.36)	-0.013* (-1.84)
<i>FREV Source</i>	0.366* (1.77)	0.266 (1.32)	0.007 (1.25)	0.004 (0.89)
<i>Book-to-Market Source</i>	-0.300 (-1.00)	-0.535 (-1.23)	-0.006 (-0.99)	-0.010 (-1.30)
<i>Total Accruals Source</i>	0.335 (0.90)	0.702 (1.35)	0.005 (1.22)	0.011 (1.23)
<i>E/P Source</i>	1.023 (1.33)	1.522 (1.60)	0.025 (1.48)	0.020 (1.24)
<i>Turnover Source</i>	0.367 (0.60)	0.144 (0.90)	0.011 (0.72)	0.003 (0.78)
<i>Sales Growth Source</i>	1.892	0.356	0.062	0.023

	(1.02)	(1.27)	(0.85)	(1.50)
<i>LTG Source</i>	-0.022*	-0.013*	-0.001	-0.001
	(-1.71)	(-1.92)	(-0.50)	(-0.66)
<i>Momentum Source</i>	1.132	0.923	0.020	0.021
	(1.55)	(1.40)	(0.92)	(1.00)
<i>MISP Source</i>	0.030	0.026	0.002	0.002
	(1.43)	(1.47)	(0.83)	(0.82)
Year & Industry Indicators	Yes	Yes	Yes	Yes
Observations	1,422	1,682	1,556	1,726
Adjusted R <sup>2</sup>	0.140	0.125	0.114	0.115

**Table 8. Source Firm Policies on Shadow Trading**

This table presents regressions examining whether source firm policies against shadow trading affect shadow trading during the 30-day window prior to the source firm's earnings announcement. We measure shadow trading using three proxies: (1) *Abnormal Short Sales*; (2) *Option/Stock Ratio*; and (3) *Order Imbalance*. The samples used for each regression are partitioned based on the sign (*Negative* or *Positive*) of the source firm Cumulative Abnormal Return (CAR) from day  $t-1$  to  $t+1$  where  $t$  is the source firm's earnings announcement date. All variables are defined in Table 1. t-values are reported in parentheses based on standard errors clustered by firm and year. All specifications include year and Fama-French industry fixed effects. Statistical significance at the 1%, 5%, and 10% level is denoted by \*\*\*, \*\*, and \*, respectively.

	<i>Abnormal Short Sales</i>		<i>Option/Stock Ratio</i>		<i>Order Imbalance</i>	
	Negative	Positive	Negative	Positive	Negative	Positive
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Prohibit</i>	-0.013** (-2.16)	0.009* (1.90)	-0.723** (-2.23)	-0.432** (-2.28)	-0.006** (-2.22)	-0.005* (-1.88)
<i>Business Partner CAR</i>	0.020** (2.23)	-0.009* (-1.76)	0.660* (1.90)	0.609** (2.11)	0.011* (1.88)	0.004* (1.83)
<i>Prohibit * Business Partner CAR</i>	-0.025** (-2.23)	0.018** (2.45)	-0.834** (2.34)	-0.562** (-2.39)	-0.011** (-2.19)	-0.005** (-2.28)
<i>Competitor CAR</i>	0.017* (1.89)	-0.010* (-1.88)	0.670** (2.11)	0.449* (1.91)	0.008* (1.80)	0.005** (2.09)
<i>Prohibit * Competitor CAR</i>	-0.015* (-1.82)	0.009* (1.85)	0.478* (1.70)	0.423* (1.88)	0.010* (1.75)	0.003* (1.82)
<i>Firm Size</i>	-0.020 (-1.25)	0.011 (0.67)	-0.278 (-1.20)	-0.082 (-0.90)	0.011 (1.02)	0.010 (1.29)
<i>Past Return</i>	0.126 (1.25)	-0.090 (-0.90)	2.172 (1.22)	1.562 (1.22)	0.020 (0.72)	0.015 (0.79)
<i>FREV</i>	0.060 (1.39)	0.037 (1.50)	0.982* (1.81)	0.545* (1.82)	0.015* (1.82)	0.013** (2.25)
<i>Book-to-Market</i>	0.015 (0.40)	0.040 (0.92)	0.262 (1.20)	0.711 (1.40)	0.005 (0.79)	0.015 (0.20)
<i>Total Accruals</i>	0.322* (1.88)	0.150* (1.92)	1.119* (1.89)	2.182* (1.81)	0.082* (1.77)	0.051 (1.51)
<i>E/P</i>	-0.110 (-0.50)	0.123 (0.72)	-1.878 (-0.93)	-2.276 (-1.60)	0.052 (0.76)	0.049 (0.95)
<i>Turnover</i>	-0.023 (-1.10)	-0.012 (-1.50)	-0.287 (-1.43)	-0.221 (-1.50)	-0.005 (-1.45)	-0.007 (-1.32)
<i>Sales Growth</i>	0.323 (1.50)	0.160 (1.30)	2.092* (1.69)	0.825* (1.92)	0.082 (1.21)	0.020 (1.30)
<i>LTG</i>	-0.001 (-0.62)	-0.003** (-2.00)	-0.016 (-1.49)	-0.019* (-1.77)	-0.001 (-0.40)	-0.001 (-0.22)
<i>Momentum</i>	-0.089 (-0.82)	0.082 (1.20)	-1.012 (-1.11)	-0.967 (-1.35)	-0.027 (-0.82)	-0.017 (-1.28)
<i>MISP</i>	0.002 (1.11)	0.002 (1.42)	0.022 (1.10)	0.028 (1.25)	0.002 (1.48)	0.003 (1.50)
<i>Firm Size Source</i>	0.010 (1.16)	-0.005 (-1.33)	0.166 (1.20)	0.082 (1.33)	0.004 (1.30)	0.002 (1.30)
<i>Past Return Source</i>	-0.032 (-1.25)	0.041 (1.46)	-0.540* (-1.78)	-0.332 (-1.59)	-0.010 (-1.52)	-0.011* (-1.72)
<i>FREV Source</i>	0.020 (1.40)	-0.018 (-1.12)	0.387* (1.88)	0.250 (1.32)	0.008 (1.38)	0.005 (0.88)
<i>Book-to-Market Source</i>	-0.020 (-0.99)	-0.044 (-1.34)	-0.309 (-1.20)	-0.532 (-0.72)	-0.005 (-0.91)	-0.008 (-1.11)
<i>Total Accruals Source</i>	0.020 (0.77)	0.056 (0.89)	0.310 (0.72)	0.756 (1.39)	0.006 (1.30)	0.013 (1.50)
<i>E/P Source</i>	0.072	-0.101	1.002	1.578*	0.028	-0.031

	(1.30)	(-1.25)	(1.35)	(1.82)	(1.50)	(-1.50)
<i>Turnover Source</i>	0.021	0.013	0.402	0.133	0.015	0.004
	(0.20)	(0.87)	(0.78)	(0.87)	(0.80)	(0.99)
<i>Sales Growth Source</i>	0.137	-0.027	2.190	0.335	0.056	0.017
	(0.89)	(-1.32)	(1.21)	(1.56)	(0.80)	(1.22)
<i>LTG Source</i>	-0.002*	0.006*	-0.022*	-0.013*	-0.001	0.002
	(-1.82)	(1.72)	(-1.76)	(-1.92)	(-0.66)	(1.31)
<i>Momentum Source</i>	0.052	-0.069	1.120	1.293	0.023	0.020
	(1.45)	(-1.55)	(1.50)	(1.55)	(1.22)	(1.01)
<i>MISP Source</i>	0.004	0.002	0.032	0.028	0.002	0.002
	(1.40)	(1.21)	(1.32)	(1.50)	(0.85)	(0.85)
<i>Year &amp; Industry Fixed Effects</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Observations</i>	586	611	674	1,192	1,258	2,164
<i>Adjusted R<sup>2</sup></i>	0.230	0.321	0.151	0.139	0.118	0.120