

Cross-Insider Trading*

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Abstract

Insider-trading regulation focuses primarily on insiders' transactions in their own firms. We show that this firm-based approach leaves an important gap: insiders obtain material non-public information through professional networks and trade stocks of other firms, which we term "*cross-insider trading*." Using administrative transaction records from India matched to LinkedIn profiles, we find that insiders trade cross-linked firms more frequently than unlinked firms in the same industries, and that these trades predict future returns. Trading and return predictability are stronger when information exchange is reciprocal. The profitability advantage disappears after linked insiders leave the firm, while the likelihood of trading the linked firm remains unchanged. Following India's 2015 Prohibition of Insider Trading Regulations, insiders trade their own firms less and cross-linked firms more, and cross-insider trades become more informative. These findings reveal professional networks as a channel for informed trading across corporate boundaries and a limitation of regulation centered on own-firm transactions.

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1 Introduction

Insider trading, in which corporate insiders exploit confidential information for personal gain, undermines market fairness and efficiency and thus attracts considerable attention from regulators and academia. A large body of research has explored insiders’ trading in their own firms (e.g., [Lakonishok and Lee, 2001](#); [Cohen, Malloy, and Pomorski, 2012](#); [Ali and Hirshleifer, 2017](#); [Alldredge and Cicero, 2015](#); [Akbas, Jiang, and Koch, 2020](#); [Hvide and Nielsen, 2024](#)); however, little attention has been paid to their trades in other firms. This gap is particularly critical given that current regulations only require insiders to disclose trades in their own companies, leaving a large void in the oversight of trades involving other firms.¹ In this paper, we argue that insiders could access and even exchange material nonpublic information through professional networks and conduct cross-stock trading to exploit this information, which we term “*cross-insider trading*”. Such *cross-insider trading* constitutes a distinct form of insider trading and can help insiders circumvent regulations, thereby challenging market fairness and efficiency.²

There are two empirical challenges in testing our argument. First, since regulations do not require insiders to disclose trades in other firms, we lack comprehensive insider cross-firm trading data. Second, we lack data on insiders’ professional networks. We leverage two novel datasets to address these two challenges. The first is a comprehensive administrative dataset from India’s Na-

¹Recent studies provide suggestive evidence that insiders circumvent insider trading regulations by trading economically linked firms ([Mehta, Reeb, and Zhao, 2021](#)) or peer firms ([Deuskar, Khatri, and Sunder, 2025](#)).

²The *Raj Rajaratnam/Galleon Group, Anil Kumar, and Rajat Gupta insider trading cases* are among the most well-known examples of insider trading through professional networks and support our argument. The case involves a vast network across technology and finance firms such as Intel, Google, and Hilton. Within these networks, corporate executives exploited their professional relationships to exchange private information and evade direct trading restrictions. For instance, Rajat Gupta, while serving on the boards of Goldman Sachs and Procter & Gamble, shared sensitive boardroom information with Rajaratnam, who then traded on this privileged intelligence to generate substantial illicit profits. This illustrates how cross-insider trading can be enabled by informal professional ties, which pose significant challenges for traditional regulatory compliance.

tional Stock Exchange (NSE), which allows us to observe insiders' complete transaction records. The second contains detailed professional background information from LinkedIn, which enables us to identify the professional network among insiders.

Using NSE transaction-level data and LinkedIn data, we test our central argument: insiders can access and even exchange material nonpublic information through professional networks and exploit it via cross-stock trading. This argument yields two testable predictions. First, insiders are more likely to trade stocks of firms connected through their networks. Second, this “*cross-insider trading*” contains information and can predict future returns. We conduct the following empirical analysis to test these two predictions.

We start by examining how professional networks affect insiders' trading. Specifically, we first define a pair of insiders as *linked insiders* if they had overlapping work experience at the same firm in the past, as documented on LinkedIn. Then we construct an insider-stock-month sample as follows. First, for each insider, the sample includes the stocks of firms where at least one linked insider currently works, as well as other stocks in the same industry as these firms. Note that including stocks in the same industry helps isolate the effect of the professional networks from other confounding factors (e.g., industry familiarity, [Ben-David, Birru, and Rossi, 2019](#); [Deuskar et al., 2025](#)). To rule out the possibility that insiders have their own inside information about the stocks, we exclude firms from our sample for which focal insiders worked during our entire sample period.

The analysis of this sample provides strong evidence for our first prediction. That is, insiders are more likely to trade stocks of firms where their linked insiders currently work. The effect is highly statistically significant even after controlling for insider and industry-time fixed effects.

The magnitude of the effect is also economically important. For example, having a linked insider increases the likelihood of trading by 66 bps, compared with the sample mean of just 24 bps.³

We further corroborate our evidence by zooming in on earnings announcements. We find that insiders are not only more likely to trade stocks with linked insiders before earnings announcements, but they also trade in the same direction as the upcoming earnings news. This result provides compelling evidence that *cross-insider trading* does indeed have information motives.

We turn to examine whether *cross-insider trading* contains information (the second prediction of our argument). To this end, we aggregate *cross-insider trading* at the stock level. Specifically, for each stock j in week t , we calculate the net buy dollar amount (the net buy share amount) of all *cross-insider trading* of this particular stock and scale it by the total trading dollar amount (the total trading share amount) of all insider trades. We term these scaled *cross-insider trading* as *cross-insider order flows*. We find that *cross-insider order flows* can significantly predict stock returns next week. In contrast, insiders' trading on other stocks in the same industry does not exhibit any return predictability. This sharp contrast highlights that the return predictability of *cross-insider trading* does not arise solely from the industry expertise.⁴

We further conduct additional analyses to strengthen the argument. First, we examine the long-run return predictability of *cross-insider order flows* and find that the return predictability lasts for

³We find similar results when we consider the fraction of trading amount as the dependent variable. Furthermore, this trading pattern becomes even more pronounced when using refined definitions of professional links (e.g., requiring longer overlap or senior positions). We also explored refining links using shared locations and surnames, but limited location detail in LinkedIn working histories and the rarity of shared surnames in our linked-pair sample make these approaches infeasible.

⁴We intentionally avoid scaling order flows by firm market capitalization or market trading volume. Because insider trading is inherently sparse, scaling by market capitalization would cause the variation in our measure to be driven by fluctuations in the denominator. To overcome this limitation and evaluate the information content, we follow [Seasholes and Zhu \(2010\)](#) and [Ben-David et al. \(2019\)](#) to calculate calendar-time holding-based portfolio returns. We find that *cross-insider trading* significantly outperforms insiders' trades on other stocks in the same industry by about 4 bps per day in terms of risk-adjusted returns (e.g., controlling for the Carhart four factors).

eight weeks without reversals, suggesting that the return predictability of *cross-insider order flows* is not driven by price pressure and is consistent with the information motive. Second, we exclude firms from the insider's own industry or firms located in the same city as the insider's firm and find that our main inference remains, suggesting that the return predictability of *cross-insider order flows* is not driven by industry information (e.g., [Ben-David et al., 2019](#); [Deuskar et al., 2025](#)) and local information (e.g., [Coval and Moskowitz, 1999](#); [Ivković and Weisbenner, 2005](#); [Massa and Simonov, 2006](#)).

We further argue that information sharing on professional networks is likely to be bilateral. That is, if insiders indeed exchange information within their networks, when linked insiders profit from the focal insider's information and the subsequent trading of the focal insider's own stock, they are more willing to share their own information with the focal insider as payback. As a result, we expect that when a linked insider has profited from trading the focal insider's own firm, the focal insider is more likely to trade stocks with this linked insider, and such *cross-insider trading* contains more information. We indeed find strong supporting evidence for this argument in the data. Specifically, after a linked insider earns positive profits, the focal insider's likelihood of trading in her firm more than doubles (from 58 bps to 136 bps). Meanwhile, the return predictability of these *cross-insider trading* is 3.4 times greater than that of other *cross-insider trading*.

We further examine whether network-related trading patterns extend beyond direct professional links. We define *indirect cross-insider trading* as trades in a firm whose insider *C* has no direct professional relationship with focal insider *A*, but is connected to *A* through their shared link to insider *B*. Focal insiders are significantly more likely to trade these indirectly linked firms than unlinked firms in the same industry, and the resulting order flows significantly predict future returns.

These findings broaden the scope of the network evidence.⁵

Thus far, we have provided strong evidence consistent with the argument that insiders can access and even exchange material nonpublic information through professional networks and exploit it through cross-stock trading. However, we are well aware that some unobservable factors might drive our results. To provide causal evidence that the return predictability of *cross-insider trading* originates from information sharing, we utilize plausible exogenous shocks—the “moving-out” of linked insiders from their firms—and conduct a difference-in-difference (*diff-in-diff*) analysis. These events are ideal for our identification, as the linked insiders’ “moving-out” disrupts the professional connections between the focal insiders and the corresponding firms. To control for unobservable factors (e.g., industry familiarity), we use firms in the same industry as the affected firms as the control group. We argue that if the return predictability of *cross-insider trading* arises mainly from information sharing, we expect *cross-insider trading* to have strong return predictabilities before these “moving-out” events, but such predictabilities diminish or disappear afterward. We find that this is indeed the case in the data. We also confirm that the reduction in return predictability does not stem from pre-event trends or changes of firm fundamentals. Overall, the evidence from the *diff-in-diff* analysis supports the argument that the return predictability of *cross-insider trading* originates from insiders’ professional networks.

Finally, we examine the regulatory relevance of this information channel using India’s 2015 Prohibition of Insider Trading Regulations (PITR). The reform substantially increased scrutiny of insiders’ trading in their own firms. We compare insiders’ trading in their own firms and *cross-linked* firms with their trading in same-industry peer firms before and after the reform. Following

⁵Figure 5 illustrates *indirect cross-insider trading*.

the PITR, insiders become less likely to trade their own firms but more likely to trade *cross-linked* firms. Cross-insider trades also become more informative, while the informativeness of own-firm trades remains largely unchanged. The pattern suggests that insiders respond to tighter regulation of own-firm trading by shifting their informed trading to the less-regulated professional-network channel. This evidence highlights that *cross-insider trading* could serve as a channel for insiders to circumvent regulation.

Related Literature - Our paper contributes to the vast literature on insider trading. Early work focuses mainly on examining whether insider trading has information content ([Lakonishok and Lee, 2001](#); [Ravina and Sapienza, 2010](#); [Jagolinzer, Larcker, Ormazabal, and Taylor, 2020](#); [Eckbo and Smith, 1998](#); [Jenter, 2005](#); [Eckbo and Ødegaard, 2020](#); [Hvide and Nielsen, 2024](#)) and how insiders trade around major corporate events such as earnings announcements and M&As ([Keown and Pinkerton, 1981](#); [Cohen et al., 2012](#); [Griffin, Shu, and Topaloglu, 2012](#); [Augustin, Brenner, and Subrahmanyam, 2019](#)). In response to regulatory constraints and declining profitability from trading in their own firms ([Seyhun, 1992](#); [Fishman and Hagerty, 1992](#); [Bhattacharya and Daouk, 2002](#); [Fernandes and Ferreira, 2009](#); [Jagolinzer, Larcker, and Taylor, 2011](#); [Ali and Hirshleifer, 2017](#); [White, 2020](#)), a growing strand of literature documents that insiders also trade profitably in other companies' stocks (e.g., [Tookes, 2008](#); [Chen and Jorgensen, 2022](#); [Ben-David et al., 2019](#); [Deuskar et al., 2025](#); [Mehta et al., 2021](#)).

Among the literature, the studies most closely related to ours is [Ben-David et al. \(2019\)](#), [Deuskar et al. \(2025\)](#), and [Mehta et al. \(2021\)](#). Specifically, [Ben-David et al. \(2019\)](#) merges the brokerage account data of the [Barber and Odean \(2000\)](#) and insider trading files and finds that insiders are more likely to trade stocks in the same industry as their own firms and also make

profits from these trades. They attribute the profitability of these trades to industry familiarity or fungible industry knowledge. [Deuskar et al. \(2025\)](#) uses a proprietary data set of all trades on the BSE (formerly the Bombay Stock Exchange) and finds that insiders utilize their private information about their own firms and trade peer stocks (e.g., stocks in the same industry) to circumvent insider trading regulations. In a similar spirit of [Deuskar et al. \(2025\)](#), [Mehta et al. \(2021\)](#) provides suggestive evidence that insiders trade in connected firms using information derived from their own firms. While we also focus on insiders' trading on firms beyond their own firms, the type of insider trading is conceptually different from prior studies. Particularly, we identify a distinct channel—professional network among insiders—through which insiders obtain and profit from the information about other firms. This type of insider trading can help insiders circumvent regulatory scrutiny and poses a big challenge to market fairness and efficiency.

Our study also contributes to the literature on communication in financial markets, focusing on the role of social networks in information transmission ([Shiller, 2017](#); [Hirshleifer, 2020](#); [Kuchler, Li, Peng, Stroebe, and Zhou, 2022](#)).⁶ Prior studies have identified several channels of information sharing, including educational networks that link market participants via shared educational experiences ([Cohen, Frazzini, and Malloy, 2008, 2010](#)), geographic neighboring with which neighbors or colleagues could share information ([Ivković and Weisbenner, 2007](#); [Hvide and Östberg, 2015](#); [Huang, Hwang, and Lou, 2021a](#); [Hirshleifer, Peng, and Wang, 2025](#)), and the role of financial intermediaries such as brokers in disseminating information about large trades or upcoming analyst reports ([Irvine, Lipson, and Puckett, 2007](#); [Barbon, Di Maggio, Franzoni, and Landier, 2019](#); [Di Maggio, Franzoni, Kermani, and Sommovilla, 2019](#)). This body of work demonstrates that

⁶Models of person-to-person diffusion show that propagation depends on discrete network paths([Sadler, 2020](#)), while recent network theory distinguishes broad diffusion from information targeted toward particular recipients ([Bramoullé and Genicot, 2024](#)).

such communication can confer significant informational advantages—though these are occasionally tempered by regulatory measures. Additional evidence indicates that information leakage also manifests in insider trading, where brokers enable affiliates to benefit from nonpublic information (Li, Mukherjee, and Sen, 2021).

We extend the literature on communication by exploring a previously underexplored channel: information sharing among corporate insiders. In contrast to the prevalent focus on information flowing from insiders to outsiders, we uncover that professional ties among insiders themselves form a potent conduit for material nonpublic information. Thus, our study contributes twofold. First, it identifies insider networks as a distinct mechanism of information transmission. Second, by focusing on sharing within insider networks, it offers new insights into how these networks can undermine standard regulatory efforts against insider trading.

One closely related paper is Berkman, Koch, and Westerholm (2020), who merge Euroclear account-level holdings with the Finnish insider register and board-network data. They find that directors earn abnormal returns on *board-interlocked* stocks, but not on their own inside or unconnected holdings, consistent with profiting from information accessed through board networks. Our study extends theirs in four key ways. First, the network ties we study are far broader: links formed through insiders’ *prior shared employment* extend well beyond boardrooms and persist long after the common employment ends. Second, we document information flowing *between two insiders*, where information is exchanged bilaterally, rather than a single director’s unilateral access. Third, our empirical evidence is causal rather than associational; by exploiting the “moving-out” of linked insiders as exogenous shocks, we show that return predictability originates from the network link itself. Fourth and most importantly, we show that *cross-insider trading* serves as a

channel to circumvent regulations on own-firm insider trading, highlighting the policy importance of our findings.

The remainder of the paper is organized as follows. Section 2 describes data and empirical methodologies, Section 3 presents main empirical results, Section 4 presents additional analysis and the identification strategy, and Section 5 concludes.

2 Data and Sample Construction

In this section, we provide detailed descriptions of our data and discuss how we construct our sample. Section 2.1 introduces the transaction, disclosure, employment-history, and firm-level data. Section 2.2 explains how we link anonymized trading accounts to disclosed insiders and extract insider professional background information from LinkedIn. Section 2.3 describes how we construct professional networks of insiders and define *cross-insider trading*.

2.1 Data

This subsection describes the data used in our study. Our data include comprehensive administrative transaction data from the National Stock Exchange of India (NSE), Indian insider trading filings from NSE-Inforbase, and LinkedIn. In what follows, we provide more detailed descriptions of each data set.

NSE transactions. Our primary dataset is a proprietary administrative transaction dataset from the National Stock Exchange of India (NSE). Our main analysis sample covers January 2007 through June 2020 because identifiable network-linked trades are sparse prior to 2007. Compared

to the data sets used in prior literature, our data has at least two advantages. First, our data set is comprehensive, as the NSE is the leading exchange in India, based in Mumbai, and the fifth-largest stock exchange in the world by total market capitalization, exceeding \$5 trillion in May 2024. Second, in this transaction-level data set, we can observe the investor ID—the anonymized Permanent Account Number (PAN), the transaction date, the security ticker, the number of shares bought or sold, and the execution prices. The granularity and completeness of our data enable us to observe insiders’ complete trading records and systematically analyze how insiders trade in other firms in addition to their own firms. To ensure data consistency, we only include transactions associated with domestic common stocks listed in the Prowess Database maintained by the Centre for Monitoring Indian Economy (CMIE) and exclude exchange-traded funds and foreign securities.

Insider-trading disclosures. Our second data source is Primeinfobase, India’s first and only database that compiles filed insider trades at both the shareholder and company level. Under Indian insider trading regulations, directors, officers, and significant shareholders (holding more than 5% of voting rights) in a listed Indian company must disclose any changes in their shareholdings to the relevant stock exchange(s). The insider trading filings include the transaction date, insider’s name, stock identifier, acquisition or sale method, and the number of shares involved. They may also cover transactions that are not executed on an exchange, such as share allotments under employee stock ownership plans, off-market transactions, and the exercise of employee stock options. For our analysis, we use only disclosed secondary-market transactions when linking named insiders to anonymized NSE accounts and track their trading (see details in Section 2.2).

Employment histories. To identify the professional networks of insiders, we use the self-reported LinkedIn profiles, which typically include details about individuals’ past employers, po-

sitions, and employment dates. For the majority of profiles, these employment histories are clearly defined. We discard entries with ambiguous or incomplete descriptions to ensure the reliability of the professional network construction. These refined data are crucial for constructing accurate employment histories and establishing professional connections among insiders, which form the basis of our professional network.

We do not use overlapping educational experiences, such as shared college or MBA alma maters, as a measure of connections among CEOs and executives, because elite education in India is highly concentrated in a handful of institutions due to national policies favoring centralized excellence (i.e., Institutes of Technology Act, 1961; Indian Institutes of Management Act, 2017). For example, premier management education is dominated by the 21 Indian Institutes of Management (IIMs), which collectively offer approximately 5,500 MBA seats annually⁷, while top-tier science and technology programs are channeled through the 23 branches of the Indian Institute of Technology (IITs), providing around 18,000 undergraduate seats per year⁸. This structure results in dominance of IIT and IIM in shaping India's corporate leadership: at its peak, nearly half of CEOs and Managing Directors (MDs) in India's top 200 companies held IIT/IIM degrees in 2009⁹, the middle of our sample period.

Firm characteristics and events. Finally, we complement our regulatory data with publicly available data. First, we obtain the information on stock prices, market capitalization, book-to-market ratios, and adjustment factors from the Prowess Database, maintained by the Centre for Monitoring Indian Economy (CMIE). Second, we obtain the data on earnings announcements and industry classifications from Refinitiv.

⁷<https://www.shiksha.com/mba/articles/total-seats-in-iim-for-mba-pgdm-blogId-156489>

⁸<https://www.shiksha.com/engineering/articles/total-number-of-seats-in-iits-blogId-215828>

⁹<https://emapartners.in/media-insight.html>

2.2 Identifying Insiders and Sample Construction

This subsection describes how we identify insiders from the NSE data and extract their professional background information from LinkedIn.

Matching insiders to anonymized accounts. As the NSE data only provides the anonymized Permanent Account Number (PAN) of traders, we first need to identify insiders. To this end, we start with the insider trading filings from the Primeinfobase and match them with the NSE transaction-level data. Given that reported insider trades in the Primeinfobase may differ in reporting granularity from administrative NSE records (e.g., due to bundled reporting), we first construct candidate trades by aggregating reported transactions at the daily level.

We then adopt a multi-stage matching algorithm to link anonymous accounts with insiders. We start by strictly matching insiders to trade accounts using non-round-lot trades. We retain only mutual best unique matches, where the account is the strongest unique match for the insider, and vice versa, and require the matched account to contain at least one additional transaction with the same date and quantity as a disclosed trade. To maximize coverage without compromising accuracy, we further extend the identification to pairs that exhibit strong cumulative evidence of identity across multiple transactions. Specifically, rather than relying solely on a single unique odd-lot trade, we evaluate the aggregate consistency of multiple matched trades and retain a pair only when the cumulative matching evidence yields a unique account-insider assignment. In total, we identify 10,362 insiders in the NSE data.

The insider trading filings from Primeinfobase also report transactions by legal entities (e.g., corporations). Thus, we exclude filings attributable to legal entities rather than natural

persons. We also exclude insiders who execute more than 1,500 trades in a single month (unlikely to be managed personally by the insider) to ensure data quality. Following [Ben-David et al. \(2019\)](#), we exclude small trades below 10,000 Rupees (approximately 107 USD as of March 2026) to reduce noise.

Verifying via LinkedIn. To identify the professional network, we manually collect LinkedIn profiles for matched insiders. A big challenge in collecting LinkedIn profiles of insiders is the prevalence of common names in India. To tackle this challenge, we employ a three-step verification rule: 1) *Name Matching*: the LinkedIn profile name must correspond to the insider under common Indian naming conventions; 2) *Company Matching*: the insider’s work history must include the specific firm with which they are associated as an insider; 3) *Position Matching*: the individual must have held a managerial or officer-level role at the corresponding company.

The above process yields a final sample of 5,640 corporate insiders with identified LinkedIn profiles, which contain 4,520,424 trades during the sample period from January 2007 to June 2020.

2.3 Identifying Linked Insiders and Cross-Insider Trading

This subsection presents the core of our empirical design: defining linked insiders through professional networks and then identifying the trades most likely motivated by this channel, which we term “*cross-insider trading*.”

Professional links. We classify insiders *A* and *B* as *linked insiders* if their LinkedIn employment histories show at least 12 months of overlapping employment at the same company. The link becomes effective after they have worked together for 12 months; we denote this date as the *link*

establishment time (t_m). Once established, the link remains active for the rest of the sample. The one-year requirement provides a conservative threshold for establishing a meaningful professional relationship. Our results are robust to stricter definitions that require a longer employment overlap or both insiders to have held senior positions during their shared tenure. Figure 1 illustrates this definition.

Insider validity periods. Merely having a connection at a firm is not sufficient; that contact must also be in a position to access the firm’s material nonpublic information. For each insider-firm pair, we use regulatory disclosures to construct a transaction-based proxy for the period during which the individual is likely to hold insider status. The *validity period* begins six months before the first reported trade and ends six months after the last disclosed trade. We deliberately use this conservative approach to restrict the validity period as it provides a consistently observed window anchored in regulatory filings.

One alternative to define validity periods is using LinkedIn employment histories. The problem is that, these histories could be ambiguous for identifying whether an insider has active access to material nonpublic information, particularly during the early stages of their tenure. In the “*moving out*” analysis, however, we use LinkedIn employment end dates because they provide a more direct measure of departure timing.

Cross-insider trading. We next define *cross-insider trading*. Conceptually, it occurs when an identified insider trades the stock of another firm where at least one linked former colleague is a valid insider, while the trader has never served as an insider of that firm. Formally, consider an identified trader A , a target firm Y , and $[t_1^B, t_2^B]$ denote linked insider B ’s validity period at firm Y . We classify a trade by insider A in stock Y during month t as *cross-insider trading* if:

1. A and B are linked insiders and the link is active by month t , i.e., $t \geq t_m$;
2. B is a valid insider of firm Y in month t , i.e., $t \in [t_1^B, t_2^B]$; and
3. A never serves as an insider of firm Y during our sample period.

These first two conditions combine a prior professional relationship with the linked insider's contemporaneous potential access to firm information, while the third condition excludes trades that could reflect A 's own insider role at the target firm. Figure 2 illustrates the definition.

Same-industry comparison. Professional links may be correlated with industry familiarity (Ben-David et al., 2019; Deuskar et al., 2025), thus insider A may trade stock Y because of being familiar with its industry rather than information obtained through insider B . To isolate the effect of the professional network, in subsequent analyses, we compare *cross-insider trading* with the same insider's trades in *other stocks* from the same industries as her actively *cross-linked* stocks. Specifically, a trade by insider A in firm Z is classified as *other trading* if (1) it operates in the same industry as at least one of A 's actively linked firms; (2) A has no linked insider at firm Z at the time of the trade; and (3) A has never served as an insider of firm Z . The last condition excludes trades that may reflect A 's own firm-specific information or expertise. Figure 3 illustrates the sample construction.¹⁰

Stock-level order flows. Our ultimate goal is to study whether *cross-insider trading* is information-driven. Thus, to examine whether the trades predict returns, we aggregate *cross-insider trading* and *other trading* at the stock-week level. Specifically, for each stock j in week t , *cross-insider order flows* equals the net buy dollar amount (the net buy share amount) of all *cross-insider trading* of

¹⁰As additional robustness tests, we exclude trades in industries in which focal insiders currently work or previously worked (e.g., Ben-David et al., 2019; Deuskar et al., 2025), and trades in firms located in cities where focal insiders currently work (e.g., Coval and Moskowitz, 1999; Ivković and Weisbenner, 2005; Seasholes and Zhu, 2010; Massa and Simonov, 2006). The results remain unchanged.

this particular stock, scaled by the total trading dollar amount (the total trading share amount) of all insider trades in the stock-week. Similarly, we aggregate *other trading* to obtain *other order flows* measures.

3 Main Empirical Results

This section presents our two main results. Section 3.1 tests whether insiders are more likely to trade on firms with linked insiders and whether these trades are directionally aligned with the earnings news (our first hypothesis). Section 3.2 examines whether network-linked order flow predicts stock return (our second hypothesis).

3.1 Professional Networks and Insiders’ Trading Across Stocks

As our first hypothesis states, if insiders obtain information through professional networks, they should trade those connected firms (with former colleagues acting as current insiders) more often than otherwise comparable firms.

To formally test this prediction, we first construct a sample at the level of insider-stock-month. As described in Section 2.3, for an insider at each month, our sample includes two types of candidate stocks. The first type consists of firms where at least one of the focal insider’s former colleagues currently holds an insider position, which we term “*cross-linked stocks*”. The second type includes peer firms that operate in the same industry as these linked firms but have no such linked insiders, which we term “*other stocks*”. Note that *other stocks* serve as a control group to account for insiders’ familiarity (e.g., Ben-David et al., 2019). We further exclude any firm at

which the focal insider has ever held an insider position at any point in our entire sample period.

We estimate the following linear-probability specification, where the unit of observation is an insider-stock-month:

$$\mathbf{I}_{trade,i,j,t} = \alpha + \beta \mathbf{I}_{linked,i,j,t} + \gamma X_{j,t-1} + \mu_i + \lambda_{k,t} + \varepsilon_{i,j,t}, \quad (1)$$

where $\mathbf{I}_{trade,i,j,t}$ is a dummy equal to one if insider i trades firm j during month t , and zero otherwise. for ease of interpreting the coefficient, we multiply the dependent variable by 100. The key explanatory variable, $\mathbf{I}_{linked,i,j,t}$, is a dummy variable equal to one if at least one of insider i 's former colleagues is an insider of firm j in that month, and zero otherwise. The vector $X_{j,t-1}$ include lagged firm-level characteristics as control variables, including firm size, book-to-market, momentum, and past returns. We also consider insider, μ_i , and industry-time, $\lambda_{k,t}$, fixed effects. Standard errors are clustered at the insider, firm, and time levels.

Table 2 reports the results. As we can see, the coefficients on $\mathbf{I}_{linked,i,j,t}$ are positive and highly significant across all specifications, strongly supporting our first hypothesis. For example, in the preferred specification in Column (4) with the full set of controls and fixed effects, the coefficient is 0.661 (t -statistic = 4.613). It suggests that the presence of linked insiders increases the likelihood of trading by roughly 66 bps. The coefficient is economically large and approximately 2.8 times the unconditional mean of the dependent variable (0.24 percentage points).

This trading tilt is also visually evident in Figure 6, which compares the actual and hypothetical random trading likelihoods in *cross-linked stocks*¹¹. Furthermore, Table A.1 further shows that

¹¹Its denominator differs from that of the insider-stock-month regression, so the figure should not be read as a graphical version of the estimate in Table 2.

this effect is significantly stronger for ties with more than three years of overlapping employment and for ties involving senior insiders. For example, the implied linked-firm coefficient is 0.940 percentage points for longer-overlap ties, 1.020 percentage points for senior ties, and 1.273 percentage points when both conditions are present. These patterns are consistent with the notion where tie intensity strengthens the trading tilt.¹²

Evidence around earnings announcements. As key information events, earnings announcements have a clear timing and information content, allowing us to better identify informed trading (e.g., [Huang, O’Hara, and Zhong, 2021b](#)). We further corroborate our evidence by zooming in on earnings announcements. We use the same regression specifications in Equation 1, but restricts the trading window to -10 through -2 trading days relative to an earnings announcement. As reported in Panel A of Table 3, we find that insiders are more likely to trade on “*Cross-linked stocks*” before earnings announcements, consistent with the results in Table 2.

We next examine whether insiders trade correctly before the earnings announcements. To this end, we first measure the information content of the earnings announcements using the cumulative abnormal return adjusted for DGTW ([Daniel, Grinblatt, Titman, and Wermers, 1997](#)) around the announcement dates and estimate the following regression:

$$\mathbf{I}_{net\ buy,i,j,t} = \alpha + \beta_1 \mathbf{I}_{linked,i,j,t} + \beta_2 CAR_{3,j,t} + \beta_3 CAR_{3,j,t} \times \mathbf{I}_{linked,i,j,t} + \gamma X_{j,t-1} + \mu_i + \lambda_{k,t} + \varepsilon_{i,j,t}, \quad (2)$$

where $\mathbf{I}_{net\ buy,i,j,t}$ is an indicator that equals one if insider i is a net buyer of stock j over days -10 through -2 relative to the earnings announcement in quarter t , and zero otherwise. $CAR_{3,j,t}$ is the

¹²For robustness, we repeat the regression in Equation 1 but consider the fraction of trading dollar amount as the dependent variable. As shown in Appendix Table A.2.

3-day DGTW-adjusted cumulative abnormal return around the announcements.

Panel B of Table 3 reports the results. The coefficient estimate, β_3 , is significantly positive in all specifications, suggesting that insiders indeed trade in the same direction as the upcoming news on linked firms than the same-industry comparison firms. This result provides compelling evidence consistent with the information-based interpretation of *cross-insider trading*: focal insider received material nonpublic information or direct tip.

In sum, this section uncovers *cross-insider trading*: insiders are more likely to trade in firms with linked insiders, and their trades are directionally aligned with forthcoming earnings news. It is worth noting that such *cross-insider trading* is not driven by industry familiarity or expertise, as we use stocks in the same industry as the linked firms for comparison.

3.2 Cross-Insider Trading Performance

Having established the fact that insiders are more likely to trade on connected firms, we turn to examine the return predictability of these trades. As described in Section 2.3, we aggregate *cross-insider trading* at the stock level and construct *cross-insider order flows*. Specifically, for each stock j in week t , we calculate the net buy dollar amount (the net buy share amount) of all cross-insider trading of this particular stock and scale it by the total trading dollar amount (the total trading share amount) of all insider trades. We term these scaled *cross-insider trading* as *cross-insider order flows*. *Other order flows* is constructed similarly from *other trading*. We then follow Greenwood and Hanson (2012) and estimate the following specification:

$$Return_{j,w+1} = \alpha + \beta Order\ flow_{j,w} + \gamma X_{j,w} + FE + \varepsilon_{j,w} \quad (3)$$

where $Return_{j,w+1}$ is the return of stock j at week $w + 1$, $Order\ flow_{j,w}$ is either *cross-insider order flows* or *other order flows* for stock j at week w . We also control for firm-level characteristics, including firm size, book-to-market, momentum, and past returns, measured using information available by the end of week w . We also include time-fixed effects to examine the cross-sectional predictability of order flows (see the argument in [Greenwood and Hanson, 2012](#)). To ensure sufficient cross-sectional variation, we keep only weeks with at least 15 observations. Standard errors are clustered by firm and week.

As shown in Columns (1) and (2) of Table 4, both the dollar-amount-based and share-amount-based *cross-insider order flows* exhibit a statistically significant positive return predictability. In contrast, the corresponding coefficients for other order flow in Columns (3) and (4) are small and not statistically distinguishable from zero. The difference may stem from the fact that the cross-insider and other-order-flow columns use different estimation samples, so we conduct a formal test of coefficient equality: a stacked interaction test confirms that *cross-insider order flow*_{share} predicts returns significantly more strongly than *other order flow*_{share} (difference = 0.23, t value = 2.18). This stark contrast underscores that *cross-insider order flows* contains information, and this information does not arise from industry-level expertise.

We conduct a series of robustness checks. First, we confirm that the results are robust using Fama-MacBeth regressions ([Fama and MacBeth, 1973](#)) (see Appendix Table A.3). Second, we show that the results are robust to sample filters about the minimum number of valid observations per week (see Appendix Table A.4). Third, to address potential concerns regarding sample comparability, we unify the analysis by aligning sample of *cross-insider* and *other order flows*. The return predictability remains robust (see Appendix Table A.5). Fourth, we address a potential

methodological limitation regarding our variable construction. Because insider trading is inherently sparse, scaling order flows by firm market capitalization or market trading volume could cause the variation to be spuriously driven by fluctuations in the denominator. To overcome this limitation and evaluate the information content, we follow [Seasholes and Zhu \(2010\)](#) and [Ben-David et al. \(2019\)](#) and consider holding-based calendar returns for insiders' trading on *cross-linked firms* and *other firms* (the peer firms in the same industry as the linked firms). As shown in Appendix Table [A.6](#), *cross-insider trading* significantly outperforms *other trading*, after adjusting for common risk factors ([Fama and French, 1996](#); [Daniel et al., 1997](#); [Carhart, 1997](#)).¹³

Some may be concerned that the return predictability of *cross-insider trading* arises from the temporary price pressure. This is very unlikely, as insiders' trades account for only a small proportion of market trading. Nevertheless, we conduct formal exercises to address this concern. Specifically, we examine the long-run return predictability of *cross-insider trading*. Appendix Table [A.7](#) and Figure [7](#) show that the *cross-insider order flow* shows no reversals up to eight weeks, suggesting that the return predictability of *cross-insider trading* is not driven by temporary price pressure.

Finally, we explore how the return predictability of *cross-insider trading* varies with firm sizes. Specifically, each week, we classify firms in the bottom size quartile as small firms and classify the remaining firms as large firms. We then repeat the exercise of Table [4](#) and report the results in Appendix Table [A.8](#). We find that the return predictability of *cross-insider order flows* is more pronounced among small firms, consistent with the notion that the market for small stocks is less efficient, and informed trading exhibits stronger return predictability in such stocks ([Bhushan,](#)

¹³In untabulated results, we conduct a transaction-based performance analysis and find similar results.

1989; Hong, Lim, and Stein, 2000; Piotroski and Roulstone, 2004; Hou and Moskowitz, 2005).

Overall, *cross-insider trading* can significantly forecast future returns without reversals. In contrast, other order flow has no such return predictability, highlighting that the return predictability of *cross-insider trading*. This evidence is consistent with an information-based interpretation rather than broad industry expertise.

4 Additional Evidence and Identification

In this section, we conduct further studies to corroborate our evidence. Specifically, Section 4.1 presents additional evidence to assess whether the return predictability of cross-insider trading arises from industry or local information. Sections 4.2 and 4.3 show patterns consistent with bilateral information sharing and how it varies with the structure and history of professional links. In Section 4.4 and 4.5, we also present two identification tests to establish causality by exploring changes in cross-trading around the departure of a linked insider and the 2015 regulatory reform.

4.1 Robustness Checks: The Role of Industry and Local Information

Thus far, we have documented that insiders are more likely to trade their linked firms' stocks, and that these trades contain information, which is consistent with the notion that insiders can access and exploit material nonpublic information through professional networks. However, the baseline relation between professional links and trading could reflect expertise about an industry (Ben-David et al., 2019; Deuskar et al., 2025) or familiarity with local firms (Coval and Moskowitz, 1999; Ivković and Weisbenner, 2005; Seasholes and Zhu, 2010). In this section, we conduct a

series of rigorous robustness tests to formally rule out these possibilities.

We first confirm that the cross-trading pattern, a higher likelihood that insiders trade in linked firms, is not driven by industry or local familiarity. To address industry familiarity, we exclude insider–stock observations in which the stock belongs to an industry where the insider has worked or currently works at any point during the sample period, and then repeat the tests in Table 2. To address local familiarity, we exclude insider–stock observations in which the firm’s headquarters are located in the same city where the insider currently works, and again repeat the tests. Panel A of Table 5 reports results after excluding stocks in insiders’ industries, while Panel B reports results after excluding stocks in insiders’ cities.

As we can see, the linked-firm coefficient on *cross-linked* dummy I_{linked} remains positive in both restricted samples. In the preferred specifications in column (4), the estimated differences are 0.381 percentage points when own-industry firms are excluded ($t = 4.543$) and 0.427 percentage points when same-city firms are excluded ($t = 5.049$). These findings therefore reduce the concern that the baseline trading-incidence result is driven entirely by broad industry or local familiarity.

We then turn to show that the return predictability of *cross-insider trading* is not driven by the industry or local familiarity. Specifically, we focus on the same restricted samples in Table 5 (excluding stocks in insiders’ industries or excluding stocks in insiders’ cities) and reconstruct *cross-insider order flow* as Table 4. Table 6 reports the results and confirms that the return predictabilities of *cross-insider order flow* are robust after controlling for industry or local familiarity.

4.2 Reciprocal Cross-Insider Trading

In this section, we extend our analysis by examining whether information sharing within professional networks is reciprocal. If insiders exchange private information through their professional networks, a linked insider who profits from trading on information about the focal insider’s firm may have an incentive to reciprocate by sharing information about the linked insider’s own firm. The focal insider should therefore be more likely to trade the stock of the linked insider’s firm after the linked insider has profited from trading shares of the focal insider’s firm.

Moreover, such *reciprocal cross-insider trades* should be more informative than nonreciprocal cross-insider trades. This arrangement may be particularly difficult to detect under current insider trading regulations: connected insiders can potentially trade one another’s stocks rather than their own, creating a less visible channel through which they can circumvent legal constraints and capitalize on private information.

To test for this bilateral pattern, we define *reciprocal cross-insider trading* as a subset of *cross-insider trading*. Specifically, a trade by insider i in firm j at time t is classified as reciprocal if (i) insider i is linked to at least one insider of firm j and (ii), before time t , at least one such linked insider has profited from trading shares of insider i ’s own firm during i ’s tenure as an insider. We estimate the following regression, which is similar to the specification used in Table 2:

$$\mathbf{I}_{trade,i,j,t} = \alpha + \beta_1 \mathbf{I}_{linked,i,j,t} + \beta_2 \mathbf{I}_{linked,i,j,t} \times \mathbf{I}_{reciprocal,i,j,t} + \gamma Controls + FE + \varepsilon_{i,j,t}, \quad (4)$$

where $\mathbf{I}_{trade,i,j,t}$ and $\mathbf{I}_{linked,i,j,t}$ are defined as Equation 1. The key indicator, $\mathbf{I}_{reciprocal,i,j,t}$, equals one if, before time t , at least one insider of firm j who is linked to insider i has profited from trading

shares of insider i 's own firm during i 's tenure as an insider, and zero otherwise. Accordingly, β_2 captures the incremental likelihood that insider i trades the stock of firm j when the relationship has a history of reciprocal trading, relative to other cross-linked but nonreciprocal relationships.

Table 7 reports the results. The coefficients on the interaction term between $\mathbf{I}_{linked,i,j,t}$ and $\mathbf{I}_{reciprocal,i,j,t}$ are positive and highly statistically significant, suggesting the existence of *reciprocal cross-insider trading*. The magnitude is economically large. For example, in Column (4), reciprocity increases the likelihood of trading by an additional 136 basis points, more than twice the 58-basis-point increase associated with a cross-link alone.¹⁴

We next examine the return predictability of *reciprocal cross-insider trading* following the procedure used in Table 4. Table 8 reports the relation between reciprocal cross-insider trading and the following week's stock return. *Reciprocal cross-insider trading* strongly predicts future returns: its predictive coefficient is 3.4 times as large as that of *nonreciprocal cross-insider trading*. A stacked interaction test further shows that the coefficients on *reciprocal cross-insider tradingshare* and *nonreciprocal cross-insider tradingshare* differ statistically at the 10% level (difference = 0.814, $t = 1.67$).

Taken together, these findings provide strong evidence consistent with bilateral information exchange and the resulting *reciprocal cross-insider trading*. They not only reinforce our main argument but also point to a potentially important regulatory gap. Because existing disclosure requirements generally focus on insiders' trades in their own companies, they may fail to capture arrangements in which connected insiders exchange firm-specific information and trade one another's stocks. Insiders may therefore be able to capitalize on their private information without

¹⁴We consider the fraction of trading dollar amount as the dependent variable, and find insiders put a larger fraction of capital on cross-linked firms as well. (see Appendix Table A.9).

trading directly in the shares of their own firms.

4.3 Indirect Cross-linked Insider Trading

Having established that direct professional links facilitate information sharing, a natural next step is to examine whether information can propagate indirectly. To explore this, we introduce a measure of *indirect cross-insider trading*, tracing multi-hop information diffusion, and examine whether the trading pattern extends to second-degree professional connections. As illustrated in Figure 5, focal insider A is indirectly linked to terminal insider C at month t when there is an intermediary B such that the A–B and B–C links are both active at t , while A and C are never directly linked during the sample. An *indirect cross-insider trade* occurs when A trades firm F, where C is an active insider.

We begin by examining the likelihood of trading on an indirect link. Following Section 3.1, we carefully construct the regression sample. The treatment group comprises *indirectly cross-linked* firms, which have at least one indirectly linked active insider and no directly linked active insider. The control firms operate in the same industry but have neither a direct nor an indirect link to the focal insider. We also exclude firms where the focal insider has ever served to rule out the possibility that they are trading on their own information. We then estimate a similar specification as Equation 1 by replacing the direct-link indicator ($\mathbf{I}_{linked,i,j,t}$) with the indirect-link dummy ($\mathbf{I}_{indirect_linked,i,j,t}$).

Table A.15 reports the results. The coefficients on $\mathbf{I}_{indirect_linked,i,j,t}$ across all specifications are positive and statistically significant, suggesting that insiders trade on *indirect cross-linked* firm excessively as well. In Column (4), our most rigorous specification, the coefficient is 0.216 (t -statistic = 4.141), suggesting that insiders do trade on *indirect cross-insider linked* firms compared

to their industry peers.

We further examine the information content of *indirect cross-insider trading* following Section 3.2 to construct *indirect cross-insider order flow* measure. Table A.16 reports the results, where we follow Equation 3 to examine the return predictability of *indirect cross-insider order flows*. The coefficients of *indirect cross-insider order flow* are positive and statistically significant for both dollar-based and share-based measures. In Column (2), the coefficient of dollar-amount *indirect cross-insider order flow* is 0.204 (t -statistic = 3.021), indicating that *indirect cross-insider trading* contains value-relevant information.

In summary, we find that there is an information diffusion along professional network, and insiders trade on the indirect links and those trades are profitable. The results in this section further supports the network nature of information sharing of *cross-insider trading*.

4.4 Identification: Linked-Insider Departures

Our results so far are broadly consistent with the interpretation that insiders obtain material non-public information through their professional networks. However, unobserved firm–insider affinity may jointly explain trading choices and subsequent performance. To address this concern, we use linked insiders’ “*moving out*” events as plausibly exogenous disruptions to firm-specific information channels. A departure can reduce the focal insider’s access to firm-specific information, but departures may also be related to firm conditions, career changes, or compliance concerns. We implement a difference-in-differences (DiD) design and compare trades in the affected linked firm with the same focal insider’s trades in never-linked firms from the same industry. If the return predictability of *cross-insider trading* originates from information sharing, the profitability difference

should decline after the last linked insider leaves the firm.

We identify linked insiders' departures from their LinkedIn employment histories using their final employment records at the linked firms. To ensure timing accuracy, we retain only month-precise records and exclude end dates reported only annually. We also require the LinkedIn departure not to materially precede the linked insider's last reported regulatory trade. We discard substantially earlier departures because they may not cleanly disrupt the information channel if the individual remains an insider through continuing shareholding or another formal affiliation.

Having identified these departures, we define "*moving out*" events at the focal-insider–departure event level. First, when a focal insider is connected to the same firm through multiple linked insiders, we date the event by the departure of the last remaining linked insider; no event is defined while another linked insider remains at the firm. Second, we examine trades within up to two years before and after the event and require at least two years of potential sample coverage on both sides of the departure. Third, because compliance concerns may weaken information access before the formal departure, we exclude the six-month transition period ending in the departure month. Fourth, to ensure that the information channel is well established before this transition, we require the link to have become effective at least 12 months before the transition period begins. Finally, if the available pre-event history is shorter than two years, we shorten both sides of the event window to the same horizon to preserve a symmetric comparison.

For each "*moving out*" event, the treatment group consists of the focal insider's trades in the affected linked firm, while controls are the same insider's trades in firms from the same industry that are never linked to that insider. Firms at which the focal insider has served are excluded. This design holds the focal insider fixed while accounting for her familiarity with the treated firm's

industry. We estimate:

$$CAR_{20,i,j,t,e} = \alpha + \beta_1 Post_{e,t} + \beta_2 \mathbf{I}_{linked,i,j,e} \times Post_{e,t} + \gamma Controls + FE + \varepsilon_{i,j,t,e}, \quad (5)$$

where e indexes a “moving out” event. The dependent variable is the signed 20-day-ahead DGTW-adjusted cumulative return: sell returns are multiplied by -1 , so positive values indicate profitable trades. $\mathbf{I}_{linked,i,j,e}$ equals one for the affected linked firm, and $Post_{e,t}$ equals one after the departure. The coefficient β_2 therefore captures the change in the profitability of trading the linked firm after the departure. The specification includes insider-by-stock fixed effects and industry-by-month fixed effects.¹⁵ Standard errors are clustered by stock.

Table 9 presents the results. In Column (2), the coefficient on $\mathbf{I}_{linked} \times Post$ is -2.559 ($t = -4.263$), indicating a sharp decline in the return predictability of cross-insider trading after linked insiders move out. More importantly, this decline is not driven by differential pre-event trends as the pre-period interactions in Columns (3) and (4) are statistically insignificant.¹⁶

Appendix Table A.11 complements the DiD estimates by separately recovering profitability before and after the departure. In column (4), trades in linked firms outperform trades in control firms by 1.557 percentage points before the event, whereas the post-event difference falls to 0.124 percentage points and is statistically insignificant. Thus, the departure largely eliminates the pre-existing informational advantage.

We conduct two additional tests to assess whether the decline reflects a contemporaneous firm-

¹⁵In a standard stacked *DiD* setting, one would control for cohort-by-unit and cohort-by-time fixed effects. In our setting, however, insider trading is too sparse along the time dimension, making control for cohort-time fixed effects infeasible.

¹⁶Appendix Table A.10 extends the event window to three years and obtains similar results.

level shock. First, Figure 8 shows no significant change in either the linked firm’s DGTW-adjusted returns or standardized earnings surprises around the departure. Second, Table A.12 repeats the DiD around the same “moving-out” events used in Table 9, but for placebo insiders. These placebo insiders have links to the treated firm that are active before the event and remain supported by at least one linked insider throughout the event window, with no link interruption within six months of the event. For these placebo insiders, the profitability of trades in the treated firm does not decline after the event: the estimated interaction is positive and insignificant (0.514, with a t -statistic of 1.227).

We next examine whether the departure changes focal insiders’ propensity to trade the linked firm. Figure 9 shows that focal insiders’ event-equal likelihood of trading the linked firm remains stable after the departure (9.63% before versus 9.29% afterward; difference = -0.34 percentage points, $t = -0.22$). This suggests that attention to the linked firm persists while the informational advantage disappears, helping rule out attention alone as the source of return predictability.

Move-in: As complementary evidence, we also consider linked insiders’ “move-in” events as an alternative identification. Consistent with the insider validity rule in Section 2.2, We define the “move-in” timing as six months before the linked insider’s first reported regulatory trade. We do so because LinkedIn working histories are not suitable for finding the exact onset of an individual’s access to material nonpublic information. To rule out other confounding factors driving focal insider trading, we restrict our analysis to stocks that the focal insider did not trade before the linked insider’s “move-in.” The rationale is that if a focal insider had already traded a stock before the linked insider “move-in”, the linked insider’s arrival is unlikely to represent a shock to the focal insider’s attention or trading incentives. We find that focal insiders are indeed more likely to

trade in firms where the linked insider serves as an insider than in firms in the same industry (see Appendix Table A.13), and these trades have stronger return predictability than trades in same-industry firms (see Appendix Table A.14).

4.5 Trading around the 2015 Insider Trading Reform

The empirical results presented above suggest that insiders leverage their professional networks to acquire private information. We next examine whether *cross-insider trading* becomes more prevalent when tighter regulation raises the cost of trading in one's own firm's stock. Specifically, we exploit India's 2015 Prohibition of Insider Trading Regulations (PITR) as a quasi-natural experiment. If insiders substitute toward linked firms to avoid the heightened scrutiny applied to own-firm trades, the reform should reduce own-firm trading while increasing both the incidence and informativeness of *cross-insider trading*.

PITR 2015, notified by SEBI on January 15, 2015, significantly strengthened insider trading rules in India. Among other changes, the reform shifted the burden of proof toward insiders for own-firm trades, broadened the definition of connected persons, strengthened firms' internal monitoring responsibilities, and enhanced SEBI's enforcement powers (Deuskar et al., 2025). Prior to this, the Companies Act of 2013 introduced the foundational statutory prohibition on insider trading. While Deuskar et al. (2025) define the transition period from the Act's effective date in September 2013, we choose a more conservative approach and exclude the period starting from December 18, 2012, when the Act was passed by the Lok Sabha (the House of the People). The passage by the Lok Sabha served as a powerful signal of impending regulatory tightening, allowing insiders to anticipate the upcoming restrictions and gradually adjust their trading behavior.

To prevent this prolonged regulatory uncertainty from confounding our before-and-after comparison, our baseline difference-in-differences (DiD) analysis excludes the transition period from December 18, 2012, through January 14, 2015. We define the pre-reform period as January 1, 2011, through December 17, 2012, and the post-reform period as January 15, 2015, through December 31, 2016. These windows are approximately equal in length. Our main findings are robust to retaining the transition-period observations.

We first examine whether insiders change their trading behavior in *cross-linked* firms following the reform. Following Table 2, we construct, for each insider, a sample consisting of her *cross-linked* firms and the firms in the same industry as these *cross-linked* firms to which the insider is not linked. To avoid changes in network composition around the reform, we require each cross-insider link to remain in place throughout the full sample period, from January 1, 2011, to December 31, 2016. We then stack these insider-specific choice sets to form an insider–stock–month panel. We construct the own-firm sample analogously, replacing each insider’s *cross-linked* firms with her own firm. We estimate the following specification:

$$\mathbf{I}_{trade,i,j,t} = \alpha + \beta \mathbf{I}_{link,i,j,t} \times Post_{2015,t} + \gamma Controls + FE + \varepsilon_{i,j,t}. \quad (6)$$

where $\mathbf{I}_{trade,i,j,t}$, equals one if the insider trades the stock in month m and zero otherwise, multiplied by 100. For *cross-insider trading* regression, $\mathbf{I}_{link,i,j,t} = \mathbf{I}_{linked,i,j,t}$, which is one if in month t , stock j is *cross-linked* to insider i , and zero otherwise. For *own-firm trading* regression, $\mathbf{I}_{link,i,j,t} = \mathbf{I}_{own,i,j,t}$, which similarly is one if insider i is a valid insider in firm j in month t , and zero otherwise. $Post_{2015,t}$ is one if it is after the regulation adoption, and zero otherwise.

Table 10 Panel A shows that, after the reform, the probability that an insider trades a cross-linked firm increases relative to that of trading an unlinked firm in the same industry. In the most saturated specification, the estimated differential increase is 0.616 percentage points ($t = 2.99$) reports the results. Panel B shows the opposite pattern for own-firm trading: the post-reform change in trading probability is 3.514 percentage points lower for an insider's own firm than for the corresponding control firms. The pre-trend leads $\mathbf{I}_{link} \times Pre_{2011}$ are statistically insignificant in both panels, indicating no differential pre-reform trends. Together, these results are consistent with insiders substituting away from own-firm trades and toward cross-linked firms following the regulatory tightening.

We next turn to examine if the *informativeness* of *cross-insider trading* also increases after the reform. Following Table 9, we measure the informativeness using the signed DGTW-adjusted cumulative abnormal return over the 20 trading days following a trade. Returns enter positively for purchases and negatively for sales and are multiplied by 100; we denote it by CAR_{20} . We drop the transition at the daily trade date: we exclude trades dated December 18, 2012 through January 14, 2015 and classify trades executed on or after January 15, 2015, as post-reform trades. For each insider, the sample includes her trades in cross-linked firms and in unlinked firms from the same industries, again restricting the analysis to links that remain in place throughout the sample period.

$$CAR_{20,i,j,t} = \alpha + \beta \mathbf{I}_{linked,i,j,t} \times Post_{2015,t} + \gamma Controls + FE + \epsilon_{i,j,t}, \quad (7)$$

where notations follow Equation 6 except that the dependent variable is changed to signed DGTW-adjusted cumulative abnormal return over the 20 trading days following a trade.

Table 11 reports the results. After the reform, *cross-insider trading* becomes significantly more informative: the coefficient on $\mathbf{I}_{linked} \times Post_{2015}$ is positive and statistically significant across all four specifications (e.g., $\beta = 1.263, t = 2.16$, in Column 4), and the pre-trend lead $\mathbf{I}_{linked} \times Pre_{2011}$ is insignificant. By contrast, we find no corresponding increase in the informativeness of insiders' own-firm trades: the difference between the average signed CAR_{20} of own-firm trades before and after the reform is small and statistically insignificant ($0.09, t = 0.15$), consistent with the findings in (Deuskar et al., 2025). Taken together, the decline in own-firm trading and the simultaneous increases in both the incidence and informativeness of cross-insider trading are consistent with insiders reallocating their informed trading toward cross-linked firms when their own-firm trades become subject to tighter regulatory constraints.

5 Conclusion

Insider-trading regulation is mostly implemented firm by firm, but information does not necessarily remain within firm boundaries. This paper studies whether professional networks allow individuals to obtain and trade on information about firms at which they have never served. We use two novel datasets—a proprietary administrative trading data from NSE and LinkedIn profiles—to uncover a distinct form of insider trading, which we term cross-insider trading. Specifically, We show that insiders indeed exploit material nonpublic information accessed through their professional networks by trading in each other's stocks. Signed order flow from these trades also predicts next-week returns.

More importantly, we present several pieces of evidence consistent with toward network-

mediated access to firm-specific information. First, trading in linked firms increases before earnings announcements, and its direction covaries with the news subsequently released. Second, we use the exogenous "moving-out" of linked insiders as quasi-exogenous shocks to information sharing and conduct a *diff-in-diff* analysis to establish causality: The incidence and informativeness of *cross-insider* weakens sharply after the last linked insider leaves the target firm. Third, after India's 2015 Prohibition of Insider Trading Regulations, insiders became less likely to trade their own firms and more likely to trade professionally linked firms. Such trades also became more informative.

Our study not only contributes to the literature on insider trading, particularly on insider trading in other stocks, but also uncovers a significant regulatory gap that challenges market fairness and efficiency. The prevailing frameworks require insiders to disclose trades in their own firm's stock, yet they fail to cover *cross-insider trading*, thereby creating a vast and unmonitored arena for potential exploitation. These findings also draw broader implication beyond insider trading. Regulations commonly use the firm as the unit of disclosure and compliance because firms are observable and easy to track. But that misses how information and incentives actually move. They may move through networks that cross those formal boundaries. Effective policy design must therefore account not only for the firm in whose securities a transaction occurs, but also for the network through which the informational opportunity travels.

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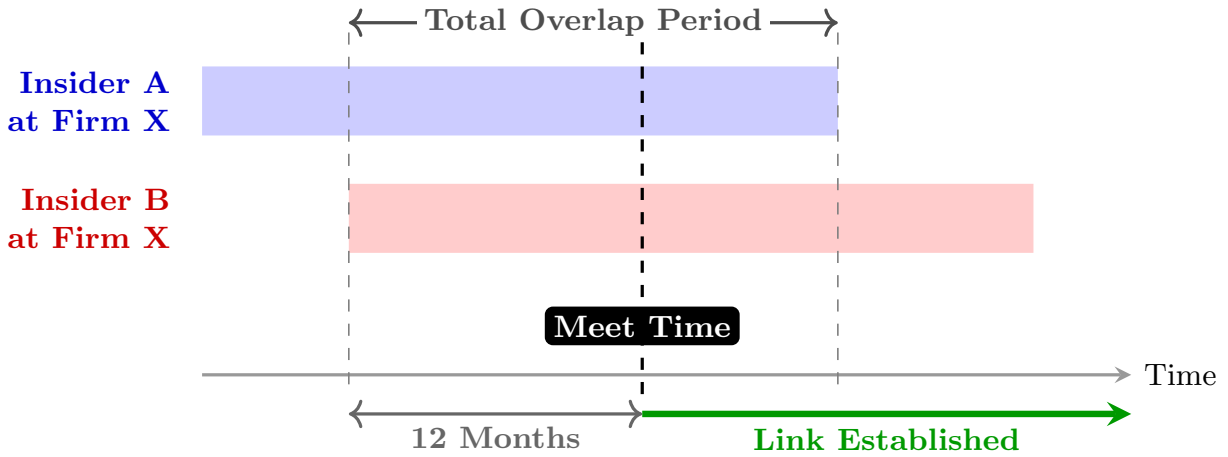


Figure 1: Timeline for the Identification of *Cross-Linked Insiders*.

This figure illustrates the temporal criteria used to define a professional link between two individuals. Insider A and Insider B are considered linked if they have a minimum overlapping employment period of 12 months at the same institution. The “*Meet Time*” represents the point at which this 12-month threshold is surpassed. From this point forward, the professional link is formally established.

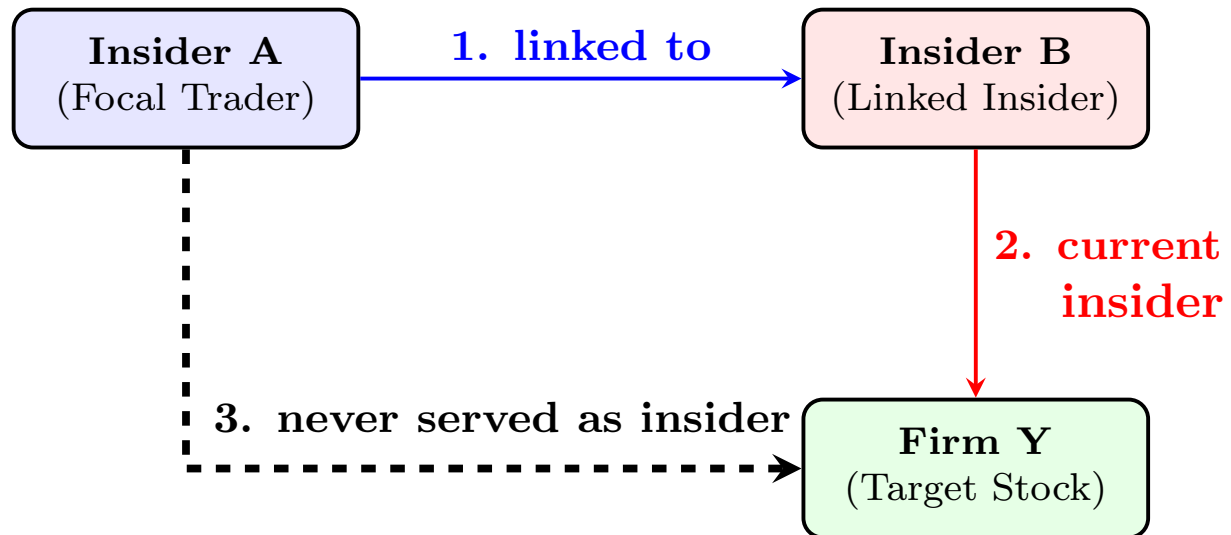


Figure 2: Definition of *Cross-Insider Trading*.

This figure depicts the conceptual framework for defining a *Cross-Insider Trade*. The focal trader, Insider A, is professionally linked to Insider B through historical co-employment. While Insider B currently serves as an insider at Firm Y, Insider A has never held any official position at Firm Y.

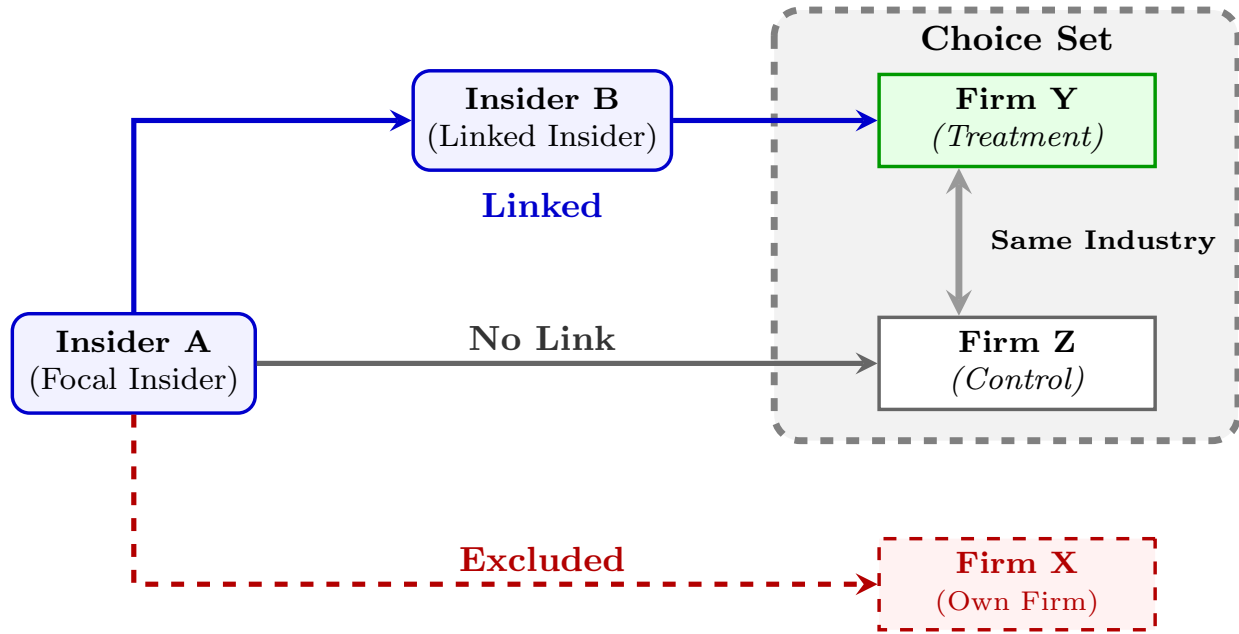


Figure 3: Regression Sample: Treatment and Control Assignment.

This figure depicts the classification of trades in our empirical design. The sample compares trades in *Treatment Firms* (linked to the focal insider through professional ties) and *Control Firms* (in same industry but no documented ties). Trades in the focal insider's own employer (*Firm X*) are removed.

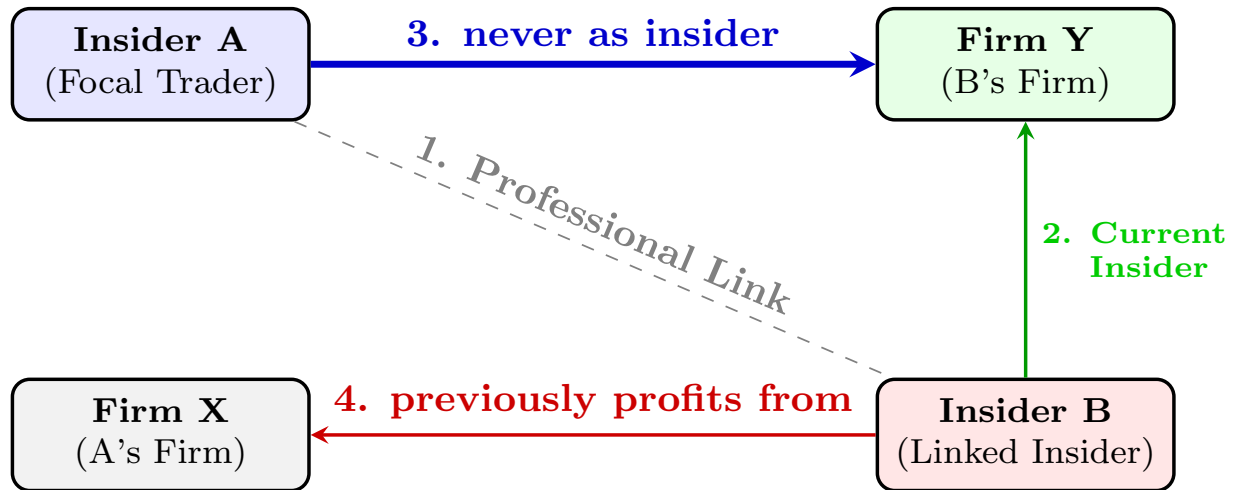


Figure 4: Definition of *Reciprocal Cross-Insider Trading*.

This figure depicts the conceptual framework for defining a *Reciprocal Cross-Insider Trade*. The focal trader, Insider A, is professionally linked to Insider B through historical co-employment. While Insider B currently serves as an insider at Firm Y, Insider A has never held any official position at Firm Y. Additionally, Insider B used to make profits from trading on Insider A's Firm X.

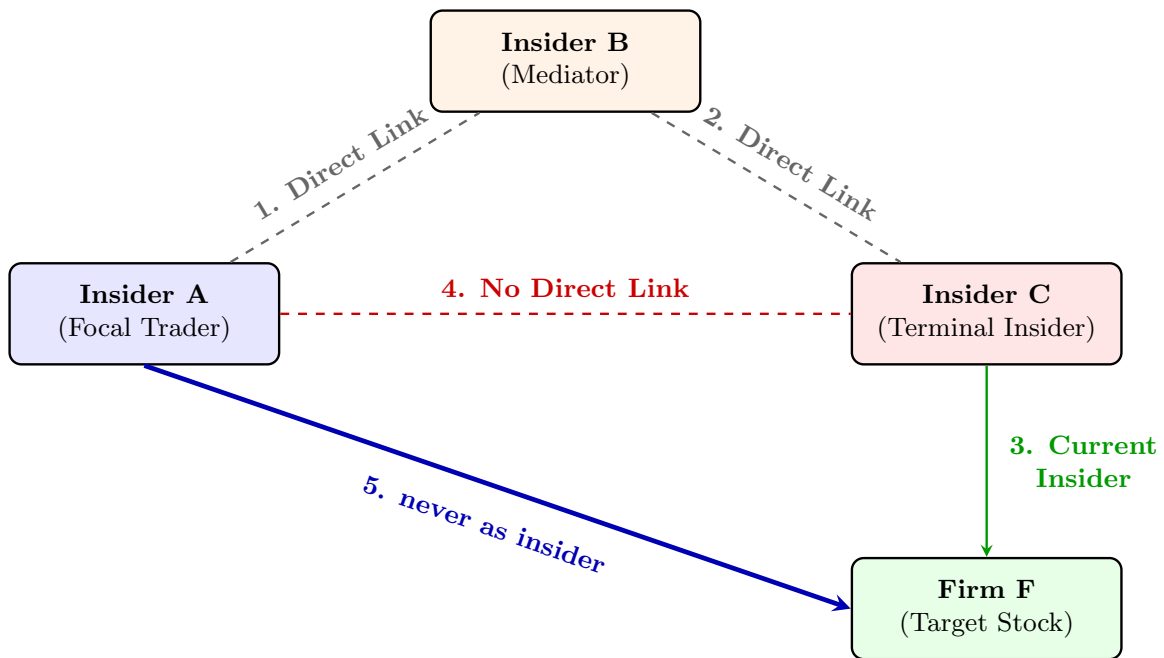


Figure 5: Definition of *Indirect Cross-Insider Trading*.

This figure depicts the conceptual framework for defining a *Indirect Cross-Insider Trade*. The focal trader, Insider A, is connected to the target firm F's insider, Insider C, strictly through a mediating mutual connection, Insider B. While Insider B and Insider C currently hold active professional links, Insider A has no direct connection to Insider C. Furthermore, Insider A has never served as an insider at Firm F.

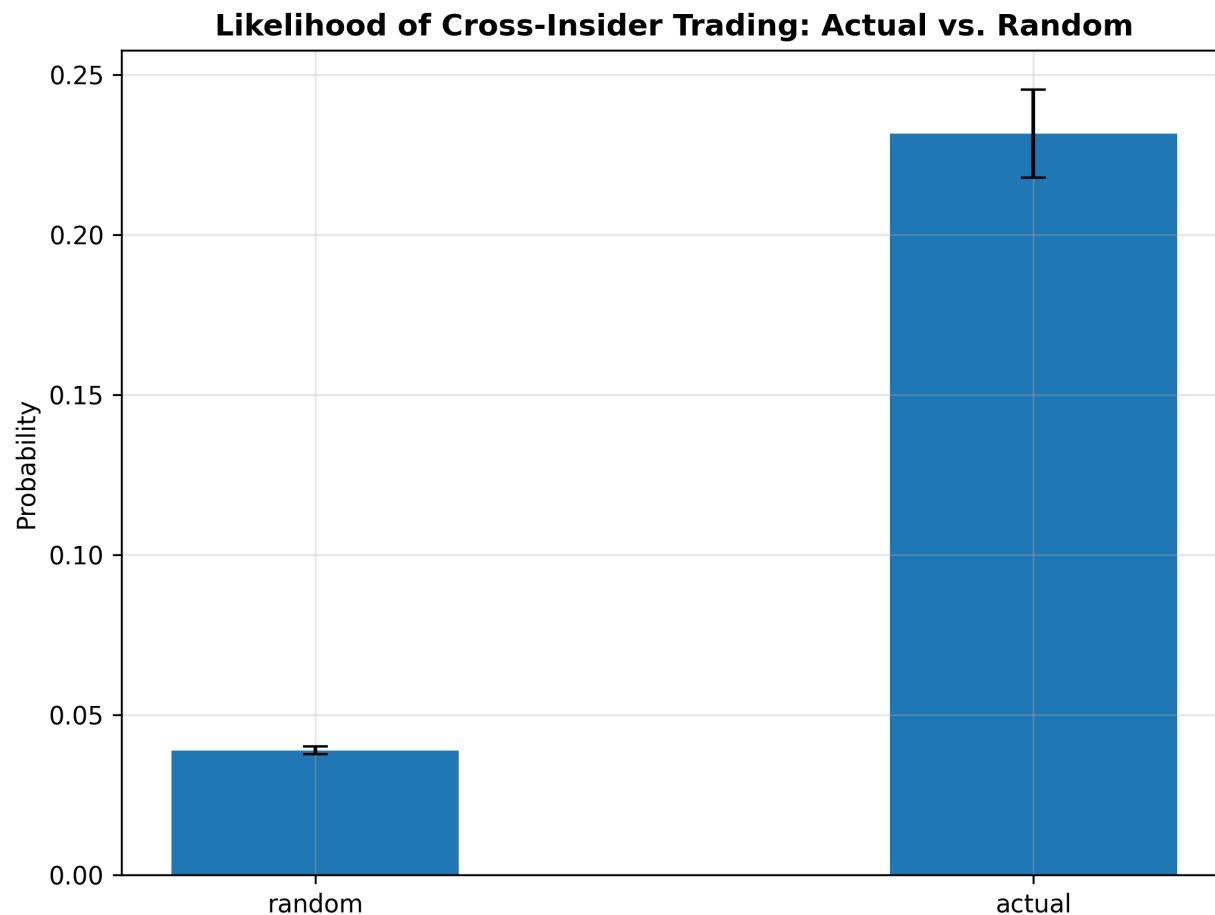


Figure 6: Likelihood of *Cross-Insider Trading*: Actual vs. Random Benchmark.

This figure compares the observed probability of *cross-insider trading* against a random benchmark. Specifically, the “random” probability refers to the likelihood of selecting the *cross-linked* firm from a set of all firms in its industry (excluding insider’s own firm), each of which has an equal probability of being chosen. The “actual” bar shows the average observed probability of trading a *cross-linked* firms across insiders. The “random” bar shows the average “random” probability of trading *cross-linked* firms across insiders. Error bars indicate 99% confidence intervals.

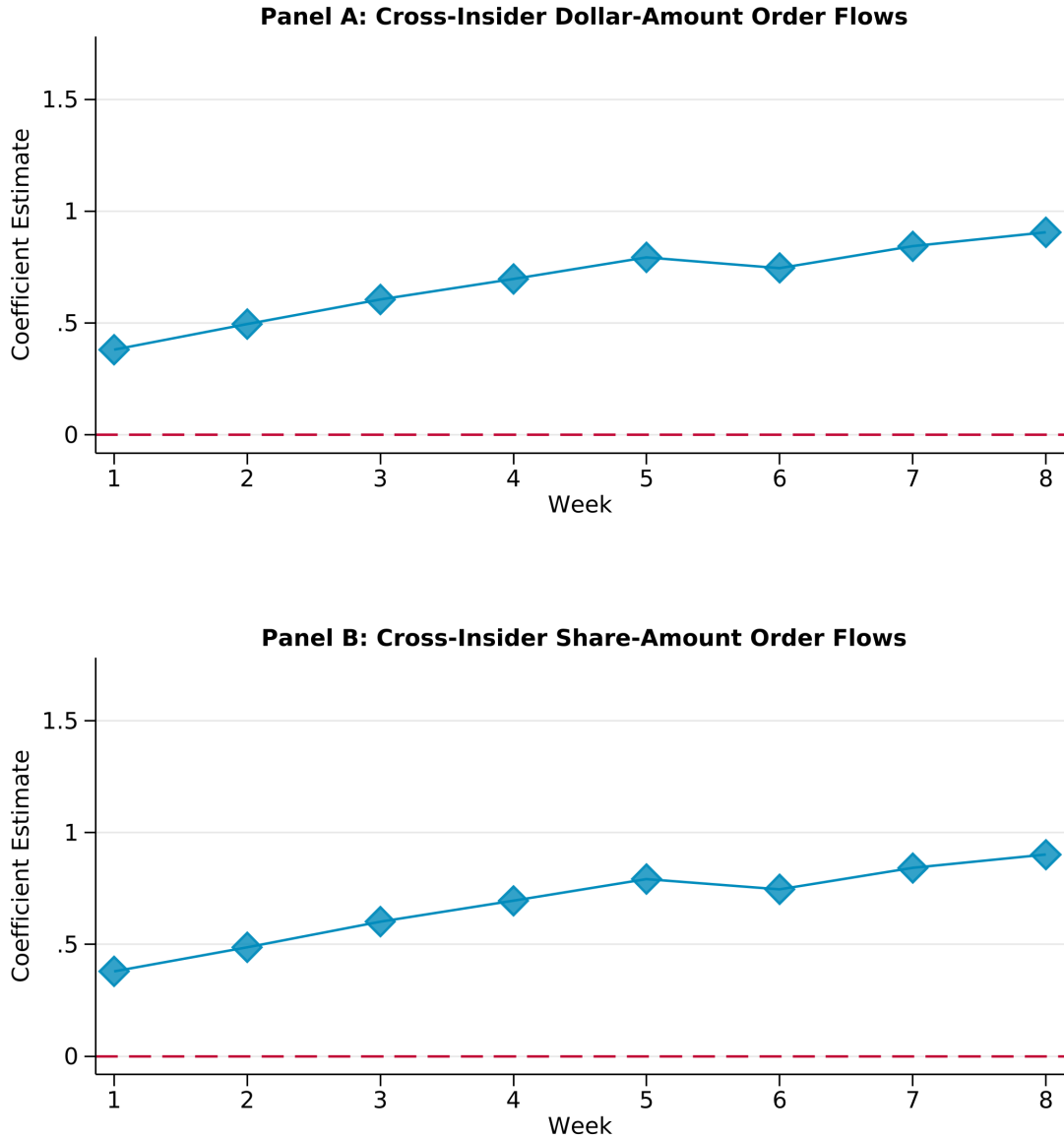


Figure 7: **Dynamics of Return Predictability of *Cross-Insider Order Flows*.** This figure plots the coefficient estimates from panel regressions of future cumulative returns on insider order flow. The x-axis represents the future return horizon in weeks, from future one week to eight weeks. The top panel shows results for the dollar-amount *cross-insider order flows*, and the bottom panel shows results for the share-amount *cross-insider order flows*. All regressions control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns, and include time fixed effects. Standard errors are clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations.

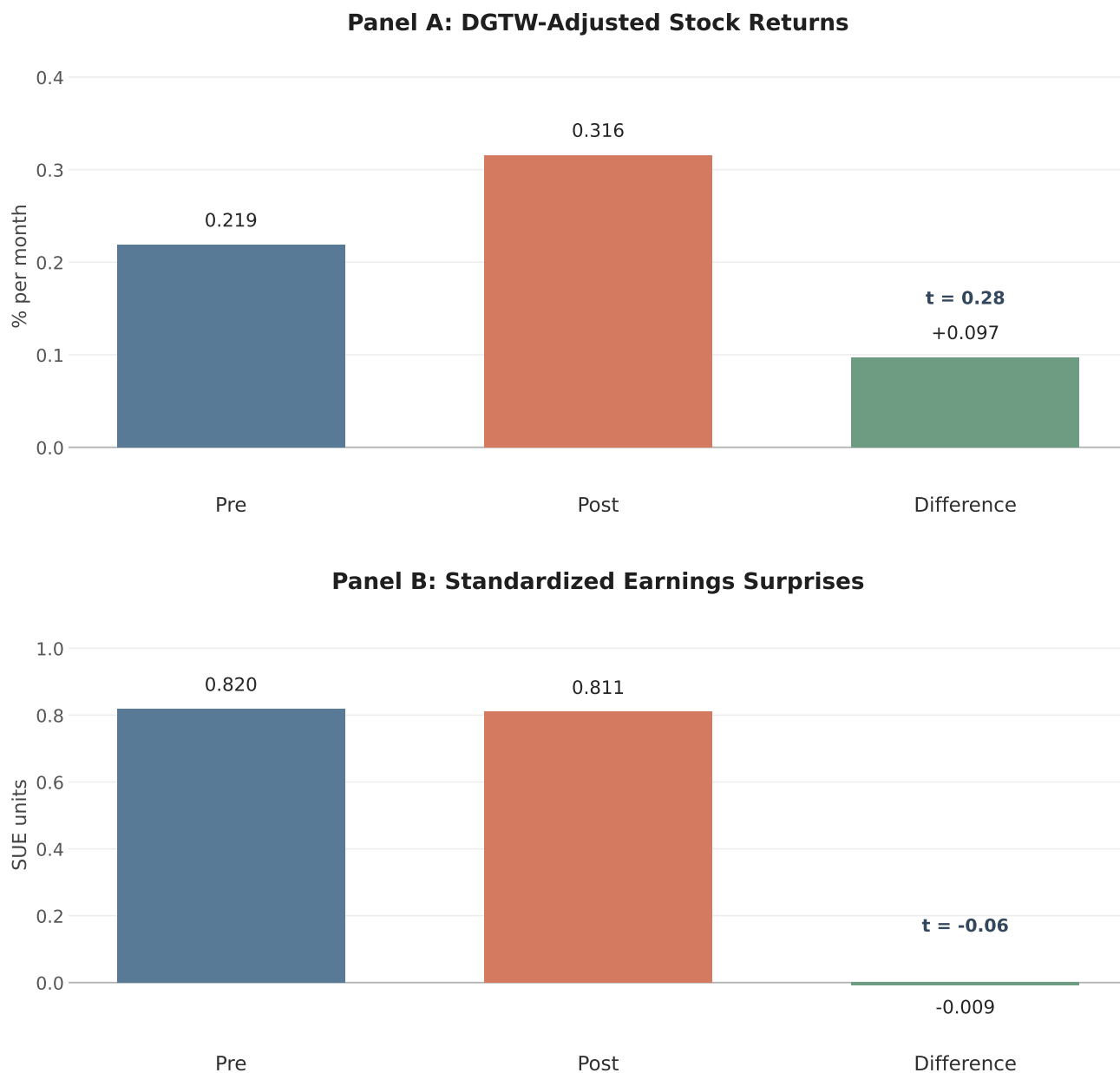


Figure 8: Firm Outcomes Before and After Linked Insider *Moving Out*. This figure examines whether linked insiders’ “*moving out*” events coincide with changes in the outcomes of the linked firms. Panel A reports DGTW-adjusted stock returns, and Panel B reports standardized earnings surprises. For each move-out event in the 24-month main sample, we compare the linked firm’s average outcome over the eight quarters before the departure with its average outcome from the departure quarter through the following eight quarters. Insider-level events involving the same stock and departure quarter are collapsed into one firm-level episode. The bars report the event-equal pre-event mean, post-event mean, and *Post-minus-Pre* difference. The *t*-value shown above the Difference bar is based on standard errors clustered by stock. Neither outcome changes significantly around the departure, suggesting that the move-out events are not accompanied by changes in the linked firms’ performance or earnings fundamentals.

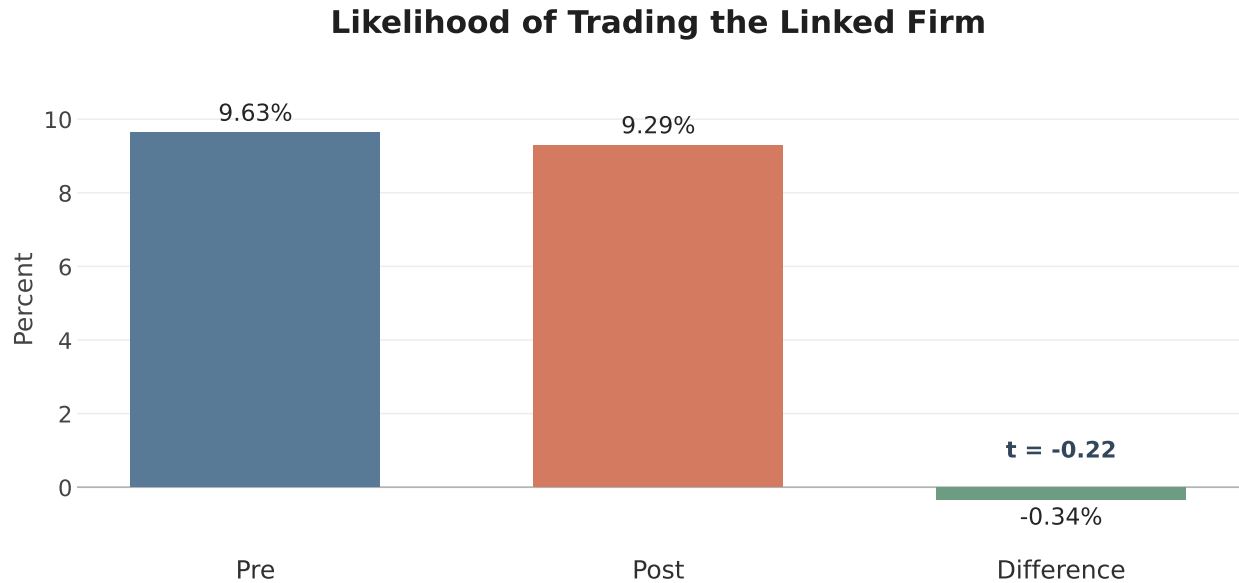


Figure 9: **Likelihood of Trading the Linked Firm Before and After Linked Insider *Moving Out***. This figure examines whether focal insiders become more likely to trade the linked firm after the linked insider’s “*moving out*” event. For each event in the 24-month main sample, we calculate the fraction of the focal insider’s traded firm-months to the linked firm, relative to the linked firm and same-industry control firms, separately before and after the departure. Control firms are firms that the focal insider neither owns nor is ever cross-linked to. Considering the compliance concerns, we exclude the transition period (6 months before the departure month) from the sample. We weight each de-duplicated event equally. The bars report the event-equal pre-event probability, post-event probability, and *Post-minus-Pre* difference. The *t*-value shown above the Difference bar is based on standard errors clustered by focal insider.

Table 1 Summary Statistics. This table presents summary statistics on our sample, covering the period from January 2007 to June 2020. *cross-insider order flows*_{\$} (*cross-insider order flows*_{share}) are stock-week level aggregated net buy dollar amount (net buy share amount) of all *cross-insider trading*, scaled by total trading dollar amount (total trading share amount). Similarly, *other order flows*_{\$} and *other order flows*_{share} are scaled and aggregated order flows of *other trading*. Firm characteristics include *BM* (book-to-market ratio), *lnME* (log of market capitalization), *MOM* (momentum), and *Return*_{*m*-1} (past 1-month return). For each variable, we report the number of observations, mean, standard deviation, and key percentiles.

	No. Obs.	Mean	SD	25th	50th	75th
<i>Cross-insider order flow</i> _{\$}	10544	-0.053	0.507	-0.207	-0.006	0.076
<i>Other order flow</i> _{\$}	54583	0.013	0.788	-0.987	0.000	1.000
<i>Cross-insider order flow</i> _{share}	10544	-0.052	0.507	-0.207	-0.005	0.078
<i>Other order flow</i> _{share}	54583	0.013	0.788	-0.987	0.000	1.000
<i>BM</i>	55739	0.607	0.709	0.192	0.363	0.780
<i>lnME</i>	58087	11.697	1.765	10.534	11.675	12.949
<i>MOM</i>	56688	0.195	0.588	-0.152	0.098	0.404
<i>Return</i> _{<i>m</i>-1}	58129	0.015	0.137	-0.056	0.007	0.077

Table 2 Likelihood of Cross-Insider Trading. This table examines whether insiders are more likely to trade *cross-linked* firms. For each insider, we include *cross-linked* firms and firms in the same industries as the linked firms in our sample but exclude firms where the focal insider works at any point in our sample period. The dependent variable is an trade dummy equal to one if insider trades on the firm in the month, and zero otherwise. For the interpretation of the coefficient, we multiply the dependent variable by 100. The key independent variable, $\mathbf{I}_{linked}$, is an indicator equal to one if the firm is *cross-linked* to the insider, and zero otherwise. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1)	(2)	(3)	(4)
	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$
$\mathbf{I}_{linked,m}$	1.003*** (5.581)	0.679*** (4.466)	0.674*** (4.520)	0.661*** (4.613)
$\ln ME_{m-1}$		0.114*** (7.784)	0.109*** (7.751)	0.104*** (7.675)
BM_{m-1}		0.019*** (4.260)	0.018*** (4.310)	0.013*** (3.839)
MOM_{m-1}		-0.023*** (-2.852)	-0.022*** (-2.765)	-0.012* (-1.820)
$Return_{m-1}$		-0.059* (-1.905)	-0.055* (-1.780)	-0.040* (-1.866)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	50,463,879	44,867,769	44,867,769	44,867,769
Adj. R^2	0.002	0.004	0.025	0.025

Table 3 **Cross-Insider Trading and Earnings Announcements.** This table investigates *cross-insider trading* before earnings announcements. Panel A examines if insiders are more likely to trade *cross-linked* firms. Sample includes both *cross-linked* firms and *other* firms and exclude insiders' own firms. The dependent variable is an indicator equal to one if the insider trades the stock between 10 to 2 trading days before the announcement. For interpretability, we multiply the dependent variable by 100. The independent variable, I_{linked} , is an indicator equal to one if the firm is *cross-linked* to the insider, and zero otherwise. Panel B tests if *cross-insider trading* is in the same direction as the earnings news. The dependent variable is an indicator equal to one if the insider is a net buyer from 10 to 2 trading days before the announcement, and zero otherwise, we also multiply the dependent variable by 100 for coefficient. CAR_3 is the DGTW-adjusted cumulative abnormal return from one trading day before to the one trading day after the announcement date. T-values are in parentheses, based on standard errors clustered at insider, firm, and time levels. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. The sample period is from January 2007 to June 2020. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Panel A: Likelihood of Cross-Insider Trading				
	(1)	(2)	(3)	(4)
	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$
$I_{linked,m}$	0.510*** (5.290)	0.344*** (4.203)	0.341*** (4.292)	0.334*** (4.389)
$lnME_{m-1}$		0.065*** (7.007)	0.062*** (6.919)	0.059*** (6.843)
BM_{m-1}		0.013*** (3.790)	0.012*** (3.826)	0.008*** (3.384)
MOM_{m-1}		-0.012** (-2.120)	-0.010* (-1.910)	-0.008* (-1.729)
$Return_{m-1}$		0.003 (0.133)	0.006 (0.271)	0.017 (0.946)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	15,452,803	14,012,313	14,012,313	14,012,313
Adj. R^2	0.001	0.002	0.018	0.018

Table 3 Continued

Panel B: Cross-Insider Trading and Direction of Earnings News				
	(1)	(2)	(3)	(4)
	$I_{net\ buy,m}$	$I_{net\ buy,m}$	$I_{net\ buy,m}$	$I_{net\ buy,m}$
CAR_3	0.070 (0.925)	0.088 (1.180)	0.087 (1.169)	0.081 (1.042)
$I_{linked,m}$	0.249*** (5.207)	0.156*** (4.164)	0.156*** (4.280)	0.152*** (4.410)
$CAR_3 \times I_{linked,m}$	0.502** (2.443)	0.482** (2.176)	0.479** (2.179)	0.478** (2.590)
$lnME_{m-1}$		0.037*** (6.816)	0.036*** (6.690)	0.034*** (6.604)
BM_{m-1}		0.007*** (3.820)	0.007*** (3.873)	0.005*** (3.464)
MOM_{m-1}		-0.005* (-1.735)	-0.005 (-1.601)	-0.004 (-1.168)
$Return_{m-1}$		-0.027* (-1.666)	-0.026 (-1.572)	-0.006 (-0.505)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	13,608,394	13,515,123	13,515,123	13,515,123
Adj. R^2	0.000	0.001	0.009	0.010

Table 4 Return Predictability of Cross-Insider Order Flows. This table presents results of panel regressions of one-week-ahead stock returns on the current week's *cross-insider* and *other order flows*. For Columns (1) and (2), the main independent variables are dollar-amount and share-amount *cross-insider order flows*. For Column (3) and (4), the main independent variables are dollar-amount and share-amount *other order flows*. All regressions include time fixed effects, and control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) <i>Return</i> _{w+1}	(2) <i>Return</i> _{w+1}	(3) <i>Return</i> _{w+1}	(4) <i>Return</i> _{w+1}
<i>cross-insider order flows</i> _{\$,w}	0.380*** (2.853)			
<i>cross-insider order flows</i> _{share,w}		0.380*** (2.823)		
<i>other order flows</i> _{\$,w}			0.051 (1.573)	
<i>other order flows</i> _{share,w}				0.050 (1.543)
<i>lnME</i> _w	-0.075 (-1.241)	-0.075 (-1.241)	-0.003 (-0.109)	-0.003 (-0.110)
<i>MOM</i> _w	0.191 (0.768)	0.191 (0.767)	0.239*** (3.146)	0.239*** (3.147)
<i>BM</i> _w	0.059 (0.221)	0.059 (0.222)	-0.153** (-2.236)	-0.153** (-2.236)
<i>Return</i> _{m-1}	-0.735 (-0.616)	-0.734 (-0.615)	-0.469 (-1.206)	-0.470 (-1.206)
Time FE	Yes	Yes	Yes	Yes
# Obs.	7,217	7,217	51,284	51,284
Adj. <i>R</i> ²	0.236	0.236	0.266	0.266

Table 5 Likelihood of Cross-Insider Trading: Excluding Firms in Insider's Industry or City.

This table examines if insiders are more likely to trade *cross-linked* firms even after excluding firms in insider's own industry or city. For Panel A, we use the sample from Table 2 but exclude firms in insider's own industry. For Panel B, we use the sample from Table 2 but exclude firms in insider's own city. The dependent variable is an indicator equal to one if insider trades the firm in that month, and zero otherwise. We multiply the dependent variable by 100 for coefficient interpretability. The independent variable, I_{linked} , is an indicator equal to one if the firm is *cross-linked* firm to the insider, and zero otherwise. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Panel A: Exclude Firms in Insider's Industry				
	(1)	(2)	(3)	(4)
	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$
$I_{linked,m}$	0.648*** (5.725)	0.374*** (4.226)	0.373*** (4.314)	0.381*** (4.543)
$lnME_{m-1}$		0.098*** (8.244)	0.096*** (8.256)	0.090*** (8.158)
BM_{m-1}		0.016*** (4.518)	0.015*** (4.642)	0.011*** (4.421)
MOM_{m-1}		-0.020*** (-2.978)	-0.020*** (-3.034)	-0.013** (-2.358)
$Return_{m-1}$		-0.052** (-2.019)	-0.049* (-1.897)	-0.037** (-2.229)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	38,773,238	34,013,944	34,013,944	34,013,944
Adj. R^2	0.001	0.003	0.018	0.019

Table 5 Continued

Panel B: Exclude Firms in Insider's City				
	(1)	(2)	(3)	(4)
	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$
$I_{linked,m}$	0.662*** (6.134)	0.430*** (4.919)	0.423*** (5.029)	0.427*** (5.049)
$lnME_{m-1}$		0.091*** (8.844)	0.089*** (8.600)	0.089*** (7.936)
BM_{m-1}		0.013*** (4.478)	0.013*** (4.539)	0.011*** (4.214)
MOM_{m-1}		-0.016*** (-2.640)	-0.017*** (-2.692)	-0.016** (-2.449)
$Return_{m-1}$		-0.031 (-1.413)	-0.030 (-1.387)	-0.041** (-2.514)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	40,369,431	35,736,466	35,736,466	35,736,466
Adj. R^2	0.001	0.003	0.019	0.020

Table 6 **Return Predictability of Cross-Insider Order Flows: Excluding Firms in Insider's Industry or City.** This table examines the return predictability of *cross-insider order flows* after excluding trades in firms which are in insider's industry or city. The regressions are based on Table 4 but for Columns (1) and (2), we exclude insiders' trades on firms which are in insider's own industry, and for Columns (3) and (4), we exclude insiders' trades on firms whose headquarters are located in the same city as insider's own city. All regressions include time fixed effects and control for last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Exclude Firms in Insider's Industry		Exclude Firms in Insider's City	
	(1) $Return_{w+1}$	(2) $Return_{w+1}$	(3) $Return_{w+1}$	(4) $Return_{w+1}$
$cross-insider\ order\ flows_{\$,w}$	0.375** (2.255)		0.417*** (2.835)	
$cross-insider\ order\ flows_{share,w}$		0.374** (2.252)		0.411*** (2.771)
$lnME_w$	-0.050 (-0.764)	-0.050 (-0.763)	-0.043 (-0.609)	-0.043 (-0.609)
MOM_w	0.238 (0.886)	0.238 (0.886)	0.218 (0.709)	0.218 (0.709)
BM_w	-0.084 (-0.306)	-0.083 (-0.304)	0.363 (1.065)	0.363 (1.066)
$Return_{m-1}$	-0.835 (-0.663)	-0.834 (-0.662)	-1.291 (-1.006)	-1.291 (-1.006)
Time FE	Yes	Yes	Yes	Yes
#Obs.	5,400	5,400	5,238	5,238
Adj. R^2	0.276	0.276	0.265	0.265

Table 7 Likelihood of Reciprocal Cross-insider Trading This table examines whether insiders are more likely to trade firms of linked insiders who have profited from trading focal insider's firm. We use the same sample as Table 2. The dependent variable is an indicator equal to one if the insider trades the firm in that month, and zero otherwise. We multiply the dependent variable by 100 for coefficient interpretability. The independent variable, $\mathbf{I}_{linked}$, is an indicator equal to one if the firm is *cross-linked* to the insider, and zero otherwise. $\mathbf{I}_{reciprocal}$ is an indicator equal to one if at least one of linked insider in the firm has profited from trading the focal insider's firm, and zero otherwise. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1)	(2)	(3)	(4)
	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$
$\mathbf{I}_{linked,m}$	0.910*** (5.555)	0.594*** (4.309)	0.590*** (4.367)	0.581*** (4.457)
$\mathbf{I}_{linked,m} \times \mathbf{I}_{reciprocal,m}$	0.867*** (3.898)	0.821*** (3.810)	0.819*** (3.938)	0.778*** (3.827)
$\ln ME_{m-1}$		0.113*** (7.792)	0.109*** (7.753)	0.104*** (7.680)
BM_{m-1}		0.019*** (4.257)	0.018*** (4.304)	0.013*** (3.854)
MOM_{m-1}		-0.023*** (-2.840)	-0.022*** (-2.757)	-0.012* (-1.867)
$Return_{m-1}$		-0.058* (-1.888)	-0.054* (-1.764)	-0.041* (-1.886)
Insider FE	No	No	Yes	Yes
Industry $\times$ TimeFE	No	No	No	Yes
#Obs.	50,463,879	44,867,769	44,867,769	44,867,769
Adj. R^2	0.002	0.004	0.025	0.025

Table 8 Return Predictability of Reciprocal Cross-Insider Order Flows. This table presents results of panel regressions of one-week-ahead stock returns on the current week's *reciprocal cross-insider order flows*. For Columns (1) and (2), the main independent variables are dollar-amount *reciprocal cross-insider order flows* and *non-reciprocal cross-insider order flows*. The definition of *reciprocal cross-insider order flows* is in Section 4.2, and *non-reciprocal cross-insider order flows* are the scaled aggregated order flows of the remaining *cross-insider trading*. For Columns (3) and (4), independent variables are share-amount *reciprocal cross-insider order flows* and *non-reciprocal cross-insider order flows*. All regressions include time fixed effects, and control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) $Return_{w+1}$	(2) $Return_{w+1}$	(3) $Return_{w+1}$	(4) $Return_{w+1}$
<i>reciprocal cross-insider order flows</i> $_{\$,w}$	1.105** (2.270)			
<i>non-reciprocal cross-insider order flows</i> $_{\$,w}$		0.325** (2.328)		
<i>reciprocal cross-insider order flows</i> $_{share,w}$			1.110** (2.285)	
<i>non-reciprocal cross-insider order flows</i> $_{share,w}$				0.324** (2.305)
$lnME_w$	0.100 (1.066)	-0.089 (-1.479)	0.100 (1.065)	-0.089 (-1.479)
MOM_w	0.351 (1.119)	0.169 (0.633)	0.351 (1.120)	0.169 (0.633)
BM_w	1.109 (1.457)	0.000 (0.001)	1.109 (1.456)	0.000 (0.001)
$Return_{m-1}$	-1.947 (-1.136)	-0.546 (-0.459)	-1.946 (-1.136)	-0.545 (-0.459)
Time FE	Yes	Yes	Yes	Yes
# Obs.	2,366	6,942	2,366	6,942
Adj. R^2	0.262	0.232	0.262	0.232

Table 9 **Profitability of Cross-Insider Trading and Linked Insider Moving Out.** This table examines, using a difference-in-differences (DID) setting, whether the profitability of *cross-insider trading* declines after a linked insider’s “moving out” event (formally defined in Section 4.4). The move-out month is identified from the linked insider’s LinkedIn employment history. The sample includes trades in the treated firm and in same-industry control firms within a symmetric window of up to 24 months around the event. Considering the compliance concerns, we exclude the transition period (6 months before the departure date) from the sample. The dependent variable is the future 20-day DGTW-adjusted cumulative abnormal return, multiplied by +1 for buy trades and −1 for sell trades and winsorized at the 1st and 99th percentiles across unique trades. The independent variable $\mathbf{I}_{linked}$ is the *cross-linked* dummy as before. *Post* is an indicator equal to one if the trade is after the “moving out”. *Pre* is an indicator for the first half of the pre-event period. All models include insider-stock and industry-time fixed effects, and columns (2) and (4) control for the previous month-end firm size, book-to-market ratio, momentum, and past one-month return. T-values are in parentheses, with standard errors clustered at the stock level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) CAR_{20}	(2) CAR_{20}	(3) CAR_{20}	(4) CAR_{20}
$\mathbf{I}_{linked,m} \times Post_m$	-2.173*** (-2.662)	-2.559*** (-4.263)	-2.058*** (-3.127)	-2.535*** (-3.317)
$\mathbf{I}_{linked,m} \times Pre_m$			0.246 (0.145)	0.050 (0.032)
$Post_m$	0.382 (0.713)	0.346 (0.659)	0.375 (0.712)	0.344 (0.665)
Pre_m			0.259 (0.407)	0.135 (0.214)
$lnME_{m-1}$		-3.286*** (-4.321)		-3.277*** (-4.299)
BM_{m-1}		0.258 (0.339)		0.260 (0.340)
MOM_{m-1}		0.892 (1.401)		0.887 (1.385)
$Return_{m-1}$		2.107 (1.246)		2.099 (1.241)
Insider $\times$ Stock FE	Yes	Yes	Yes	Yes
Industry $\times$ Time FE	Yes	Yes	Yes	Yes
# Obs.	9,734	9,734	9,734	9,734
Adj. R^2	0.067	0.070	0.067	0.070

Table 10 Likelihood of Cross-Insider Trading and Insider Own-Firm Trading around the 2015 PITR. This table examines whether India's 2015 Prohibition of Insider Trading Regulations (PITR) led insiders to trade less on insiders' own firm and to trade more on *cross-linked* firms. Panel A studies trading in *cross-linked* firms and Panel B trading in the insider's own firm. In both panels, same-industry firms the insider trades but is not linked to are the control. The dependent variable is an indicator equal to one if the insider trades the firm in that month, and zero otherwise. We multiply the dependent variable by 100 for coefficient interpretability. The key variable is $\mathbf{I}_{linked,m} \times Post_{2015}$ (Panel A) or $\mathbf{I}_{own,m} \times Post_{2015}$ (Panel B), the post-regulation change in the propensity to trade *cross-linked* / own firms relative to the control; $\mathbf{I}_{linked,m} \times Pre_{2011}$ ($\mathbf{I}_{own,m} \times Pre_{2011}$) is a pre-trend lead. The sample is a balanced panel of links and own-firm relations present over the entire 2011m1-2016m12 window; it drops the legislative-transition window between the Companies Bill (December 2012) and the PITR adoption (January 2015), so the clean pre-period ends in 2012 and the post-period begins in February 2015. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider and firm levels, are reported in parentheses. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Panel A: Likelihood of Cross-Insider Trading				
	(1)	(2)	(3)	(4)
	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$	$\mathbf{I}_{trade,m}$
$\mathbf{I}_{linked,m} \times Post_{2015}$	0.627*** (2.935)	0.605*** (2.822)	0.642*** (3.098)	0.616*** (2.990)
$\mathbf{I}_{linked,m} \times Pre_{2011}$			0.030 (0.520)	0.022 (0.358)
$\ln ME_{m-1}$		0.034** (2.366)		0.034** (2.371)
BM_{m-1}		-0.002 (-0.494)		-0.002 (-0.503)
MOM_{m-1}		0.002 (0.255)		0.002 (0.261)
$Return_{m-1}$		-0.010 (-0.527)		-0.010 (-0.524)
Insider $\times$ Stock FE	Yes	Yes	Yes	Yes
Industry $\times$ Time FE	Yes	Yes	Yes	Yes
# Obs.	4,615,516	4,349,812	4,615,516	4,349,812
Adj. R^2	0.178	0.177	0.178	0.177

Table 10 Continued

Panel B: Likelihood of <i>Insider Own-Firm Trading</i>				
	(1)	(2)	(3)	(4)
	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$	$I_{trade,m}$
$I_{own,m} \times Post_{2015}$	-2.813* (-1.929)	-2.805* (-1.821)	-3.418** (-2.094)	-3.514** (-2.144)
$I_{own,m} \times Pre_{2011}$			-1.163 (-0.875)	-1.393 (-1.134)
$lnME_{m-1}$		0.108*** (2.837)		0.106*** (2.779)
BM_{m-1}		0.013 (1.547)		0.013 (1.591)
MOM_{m-1}		0.005 (0.252)		0.005 (0.246)
$Return_{m-1}$		0.090 (1.277)		0.090 (1.270)
Insider $\times$ Stock FE	Yes	Yes	Yes	Yes
Industry $\times$ Time FE	Yes	Yes	Yes	Yes
# Obs.	844,005	774,963	844,005	774,963
Adj. R^2	0.190	0.191	0.190	0.191

Table 11 **Profitability of Cross-Insider Trading around the 2015 PITR.** This table examines whether *cross-insider* trading became more informed after India's 2015 Prohibition of Insider Trading Regulations (PITR). The dependent variable is the signed DGTW characteristic-adjusted cumulative abnormal return over the 20 trading days following a trade (+1 for buys, −1 for sells), multiplied by 100. $Post_{2015}$ equals one from January 2015. The key variable, $I_{linked,m} \times Post_{2015}$, is the post-regulation change in the performance of *cross*-firm trades relative to the insider's trades in same-industry non-linked (control) firms; $I_{linked,m} \times Pre_{2011}$ is a pre-trend lead. The sample includes trades in *cross-linked* firms and control firms whose relations are present over the entire 2011m1–2016m12 window. Trades in the legislative transition period, from 18 December 2012 (the Lok-Sabha passage of the Companies Bill 2012) through 14 January 2015, are dropped, and a trade is classified as post if it occurs on or after 15 January 2015. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. All columns include insider $\times$ stock fixed effects. Columns (1) and (3) include time fixed effects, whereas Columns (2) and (4) include industry $\times$ time fixed effects. T-statistics, based on standard errors clustered at insider and firm levels, are reported in parentheses. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1) CAR_{20}	(2) CAR_{20}	(3) CAR_{20}	(4) CAR_{20}
$I_{linked,m} \times Post_{2015}$	1.333*** (2.802)	1.122** (2.311)	1.455*** (2.729)	1.263** (2.161)
$I_{linked,m} \times Pre_{2011}$			0.266 (0.662)	0.318 (0.738)
$lnME_{m-1}$	-1.150* (-1.913)	-0.883 (-1.541)	-1.138* (-1.885)	-0.868 (-1.513)
BM_{m-1}	-0.095 (-0.287)	-0.070 (-0.214)	-0.097 (-0.294)	-0.072 (-0.222)
MOM_{m-1}	-0.097 (-0.197)	-0.195 (-0.336)	-0.083 (-0.169)	-0.177 (-0.304)
$Return_{m-1}$	-1.392 (-1.052)	-1.413 (-1.047)	-1.387 (-1.048)	-1.408 (-1.043)
Time FE	Yes	No	Yes	No
Industry $\times$ Time FE	No	Yes	No	Yes
Insider $\times$ Stock FE	Yes	Yes	Yes	Yes
# Obs.	20,651	20,551	20,651	20,551
Adj. R^2	0.023	0.031	0.023	0.031

APPENDIX

A Additional Results and Robustness Tests

Table A.1 **Likelihood of Refined Cross-Insider Trading.** This table examines whether insiders are more likely to trade stocks of *cross-linked* firms, and how the characteristics of these links affect the trading likelihood. The sample is the same as Table 2. The dependent variable, $I_{trade,m}$, is an indicator variable equal to one if the insider trades the firm's stock in a given month, and zero otherwise. The main independent variable, $I_{linked,m}$, is an indicator equal to one if the firm is *cross-linked* to the insider. Columns (2) to (5) further explore the impact of link characteristics: $I_{longer,m}$ is an indicator equal to one if the link overlap exceeds three years; $I_{senior,m}$ is an indicator equal to one if the linked insider pair were both in senior positions when they met; and $I_{combine,m}$ is an indicator equal to one if the link satisfies both the longer overlap and seniority conditions. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. All specifications include insider and industry-time fixed effects. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1) $I_{trade,m}$	(2) $I_{trade,m}$	(3) $I_{trade,m}$	(4) $I_{trade,m}$	(5) $I_{trade,m}$
$I_{linked,m}$	0.661*** (4.613)	0.406*** (3.659)	0.444*** (3.521)	0.296*** (2.720)	0.457*** (3.713)
$I_{longer,m}$		0.534*** (5.164)		0.406*** (4.606)	
$I_{senior,m}$			0.576*** (3.884)	0.454*** (3.284)	
$I_{combine,m}$					0.816*** (5.105)
$lnME_{m-1}$	0.104*** (7.675)	0.104*** (7.679)	0.105*** (7.691)	0.105*** (7.693)	0.105*** (7.689)
BM_{m-1}	0.013*** (3.839)	0.013*** (3.860)	0.013*** (3.877)	0.013*** (3.886)	0.013*** (3.894)
MOM_{m-1}	-0.012* (-1.820)	-0.012* (-1.844)	-0.012* (-1.849)	-0.012* (-1.862)	-0.013* (-1.868)
$Return_{m-1}$	-0.040* (-1.866)	-0.041* (-1.881)	-0.041* (-1.887)	-0.041* (-1.893)	-0.041* (-1.895)
Insider FE	Yes	Yes	Yes	Yes	Yes
Industry $\times$ Time FE	Yes	Yes	Yes	Yes	Yes
# Obs.	44,867,769	44,867,769	44,867,769	44,867,769	44,867,769
Adj. R^2	0.025	0.025	0.025	0.025	0.025

Table A.2 **Fraction of Trading Amount of Cross-Insider Trading.** This table examines whether the fraction of trading amount in the *cross-linked* firms are higher. The sample is the same as Table 2. The dependent variable is the insider's dollar trading amount in the firm divided by the total dollar trading amount of the insider in that month. We multiply the dependent variable by 100 for coefficient interpretability. The independent variable, I_{linked} , is an indicator equal to one if the firm is *cross-linked* to the insider. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1) <i>trading fraction</i> _{\$_{,m}\$}	(2) <i>trading fraction</i> _{\$_{,m}\$}	(3) <i>trading fraction</i> _{\$_{,m}\$}	(4) <i>trading fraction</i> _{\$_{,m}\$}
$I_{linked,m}$	0.287*** (4.764)	0.231*** (4.214)	0.231*** (4.221)	0.230*** (4.244)
$lnME_{m-1}$		0.020*** (6.146)	0.020*** (6.310)	0.019*** (6.139)
BM_{m-1}		0.003*** (3.703)	0.003*** (3.708)	0.002*** (3.211)
MOM_{m-1}		-0.005** (-2.251)	-0.004** (-2.162)	-0.003* (-1.667)
$Return_{m-1}$		-0.015** (-2.173)	-0.014** (-2.124)	-0.011* (-1.808)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	50,463,879	44,867,769	44,867,769	44,867,769
Adj. R^2	0.001	0.002	0.004	0.004

Table A.3 Return Predictability of Cross-Insider Trading: Fama-Macbeth Regression This table presents results of Fama-Macbeth regressions of one-week-ahead stock returns on the current week's *cross-insider* and *other order flows*. For Columns (1) and (2), the main independent variables are dollar-amount and share-amount *cross-insider order flows*. For Column (3) and (4), the main independent variables are dollar-amount and share-amount *other order flows*. All regressions control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors adjusted for autocorrelation using the Newey-West procedure with 6 lags. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) $Return_{w+1}$	(2) $Return_{w+1}$	(3) $Return_{w+1}$	(4) $Return_{w+1}$
<i>Cross-insider order flow</i> _{\$,w}	0.351** (2.341)			
<i>Cross-insider order flow</i> _{share,w}		0.351** (2.361)		
<i>Other order flow</i> _{\$,w}			0.072 (1.634)	
<i>Other order flow</i> _{share,w}				0.071 (1.613)
$lnME_w$	-0.135** (-2.533)	-0.136** (-2.546)	-0.014 (-0.562)	-0.014 (-0.567)
MOM_w	0.471* (1.760)	0.469* (1.756)	0.280 (1.496)	0.280 (1.495)
BM_w	0.213 (0.580)	0.216 (0.590)	-0.068 (-0.521)	-0.068 (-0.525)
$Return_{m-1}$	-0.065 (-0.067)	-0.058 (-0.060)	-0.488 (-1.187)	-0.489 (-1.188)
# Obs.	7,217	7,217	51,284	51,284
Adj. R^2	0.152	0.152	0.106	0.106

Table A.4 Return Predictability of Cross-Insider Trading with Alternative Sample Filters. This table presents results of panel regressions of one-week-ahead stock returns on the current week's *cross-insider* and *other order flows* with different sample filters. Specifically, for Columns (1) and (2), we adopt the filter keeping weeks with at least 10 firms with valid observations, and the threshold for Columns (3) and (4) is 12, and that for Columns (5) and (6) is 12, and that for Columns (7) and (8) is 20. The main independent variables are dollar-amount and share-amount *cross-insider order flows*. All regressions include time fixed effects, and control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	nobs ≥ 10		nobs ≥ 12		nobs ≥ 18		nobs ≥ 20	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Return_{w+1}$	$Return_{w+1}$	$Return_{w+1}$	$Return_{w+1}$	$Return_{w+1}$	$Return_{w+1}$	$Return_{w+1}$	$Return_{w+1}$
<i>Cross-insider order flow</i> _{\$,w}	0.317*** (2.669)		0.324** (2.578)		0.386*** (2.730)		0.309** (2.262)	
<i>Cross-insider order flow</i> _{share,w}		0.316*** (2.640)		0.323** (2.546)		0.385*** (2.700)		0.310** (2.238)
<i>lnME</i> _w	-0.076 (-1.314)	-0.076 (-1.313)	-0.080 (-1.316)	-0.080 (-1.316)	-0.060 (-0.998)	-0.060 (-0.998)	-0.034 (-0.585)	-0.034 (-0.585)
<i>MOM</i> _w	0.070 (0.309)	0.070 (0.309)	0.063 (0.270)	0.063 (0.269)	0.151 (0.578)	0.151 (0.578)	0.099 (0.356)	0.099 (0.356)
<i>BM</i> _w	-0.101 (-0.401)	-0.101 (-0.400)	-0.015 (-0.058)	-0.015 (-0.058)	-0.114 (-0.501)	-0.114 (-0.500)	-0.136 (-0.663)	-0.136 (-0.662)
<i>Return</i> _{m-1}	-0.415 (-0.368)	-0.415 (-0.368)	-0.427 (-0.366)	-0.427 (-0.366)	-0.741 (-0.600)	-0.741 (-0.600)	-0.537 (-0.409)	-0.536 (-0.408)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	8,152	8,152	7,641	7,641	6,741	6,741	6,141	6,141
Adj. R^2	0.240	0.240	0.239	0.239	0.230	0.230	0.228	0.228

Table A.5 Return Predictability of Cross-Insider Order Flows: Combined Sample. This table presents panel regression results of one-week-ahead stock returns on the current week's *cross-insider* and *other order flows*. To address potential concerns regarding sample comparability in our baseline results, we unify the analysis by using the union of the cross-insider and other-insider samples. Columns (1) and (2) examine the return predictability of dollar-amount and share-amount *cross-insider order flows*, respectively. Columns (3) and (4) examine *other order flows*. Columns (5) and (6) include both types of order flows jointly and report the test for the difference between their coefficients. All regressions include time fixed effects and control for the previous month's firm size, book-to-market ratio, momentum, and past one-month return. T-statistics, reported in parentheses, are based on standard errors clustered at the firm and time levels. The sample period is from January 2007 to June 2020, and the sample is limited to weeks with at least 15 valid firm observations. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1) $Return_{w+1}$	(2) $Return_{w+1}$	(3) $Return_{w+1}$	(4) $Return_{w+1}$	(5) $Return_{w+1}$	(6) $Return_{w+1}$
<i>Cross-insider order flows</i> $_{\$,w}$	0.266*** (2.657)				0.266*** (2.655)	
<i>Cross-insider order flows</i> $_{share,w}$		0.266*** (2.642)				0.265*** (2.641)
<i>Other order flows</i> $_{\$,w}$			0.052 (1.609)		0.052 (1.607)	
<i>Other order flows</i> $_{share,w}$				0.051 (1.577)		0.051 (1.575)
$\ln ME_w$	-0.009 (-0.358)	-0.009 (-0.358)	-0.009 (-0.342)	-0.009 (-0.342)	-0.009 (-0.356)	-0.009 (-0.356)
MOM_w	0.225*** (3.003)	0.225*** (3.003)	0.225*** (2.990)	0.225*** (2.990)	0.224*** (2.987)	0.224*** (2.987)
BM_w	-0.166** (-2.440)	-0.166** (-2.440)	-0.164** (-2.404)	-0.164** (-2.404)	-0.165** (-2.427)	-0.165** (-2.427)
$Return_{m-1}$	-0.382 (-1.007)	-0.382 (-1.007)	-0.386 (-1.014)	-0.386 (-1.014)	-0.375 (-0.984)	-0.375 (-0.985)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	55,134	55,134	55,134	55,134	55,134	55,134
Adj. R^2	0.266	0.266	0.266	0.266	0.266	0.266

Table A.6 **Holdings-based Calendar-time Performance of *Cross-Insider Trading*** This table reports the performance of holdings-based calendar-time portfolios of *cross-insider trading*, *other trading* and their difference. The daily holding positions are inferred from calendar-time purchase and sale transaction records, and returns are holdings-value-weighted and expressed in basis points. Following Ben-David et al. (2019), when portfolios are insufficiently diversified (fewer than 20 stocks), we instead use equal weighting to mitigate extreme weights. The rows “DGTW”, and “Excess” report the DGTW-adjusted return, and excess return, respectively, while “CAPM”, “FF3”, and “FFC4” report α under CAPM, Fama-French 3-factors, and Fama-French 3-factors augmented by an additional momentum factor, respectively. T-values of the difference, reported in parentheses in the last column, are adjusted for autocorrelation using the Newey-West procedure with 6 lags. The Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	Cross	Others	Diff.
DGTW	3.381*** (2.946)	−0.192 (−0.193)	3.573*** (3.152)
CAPM	4.192*** (2.707)	0.601 (0.425)	3.591*** (2.671)
FF3	4.310*** (2.794)	0.637 (0.457)	3.673*** (2.751)
FFC4	4.964*** (3.203)	1.652 (1.200)	3.312** (2.448)

Table A.7 Return Predictability of *Cross-Insider Order Flows* over Different Horizons. This table shows the results of panel regressions of future two to eight week returns on *cross-insider order flows*. For Columns (1) to (4), the main independent variables are dollar-amount *cross-insider order flows*. For Columns (5) to (8), the main independent variables are share-amount *cross-insider order flows*. All specifications include time fixed effects, and control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Return_{w+2}</i>	<i>Return_{w+4}</i>	<i>Return_{w+6}</i>	<i>Return_{w+8}</i>	<i>Return_{w+2}</i>	<i>Return_{w+4}</i>	<i>Return_{w+6}</i>	<i>Return_{w+8}</i>
<i>Cross-insider order flows_{\$,w}</i>	0.119 (1.023)	0.106 (0.917)	-0.089 (-0.825)	0.069 (0.750)				
<i>Cross-insider order flows_{share,w}</i>					0.115 (0.988)	0.106 (0.919)	-0.088 (-0.821)	0.067 (0.727)
<i>lnME_w</i>	-0.028 (-0.542)	-0.063 (-1.088)	0.014 (0.291)	0.022 (0.467)	-0.028 (-0.540)	-0.063 (-1.088)	0.014 (0.290)	0.022 (0.468)
<i>MOM_w</i>	0.379** (2.016)	0.222 (0.986)	0.378* (1.669)	0.225 (1.030)	0.379** (2.016)	0.222 (0.986)	0.378* (1.669)	0.225 (1.030)
<i>BM_w</i>	-0.265 (-1.100)	-0.473 (-1.312)	-0.199 (-0.748)	-0.250 (-1.000)	-0.265 (-1.101)	-0.473 (-1.312)	-0.199 (-0.748)	-0.250 (-1.000)
<i>Return_{m-1}</i>	0.097 (0.080)	0.338 (0.312)	-0.323 (-0.249)	-1.062 (-1.037)	0.096 (0.079)	0.339 (0.312)	-0.323 (-0.249)	-1.063 (-1.037)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	7,217	7,213	7,212	7,209	7,217	7,213	7,212	7,209
Adj. <i>R</i> ²	0.249	0.250	0.252	0.262	0.249	0.250	0.252	0.262

Table A.8 Return Predictability of Cross-Insider Order Flows: Subsample Analysis. This table presents results of panel regressions of one-week-ahead stock returns on the current week's *cross-insider order flows* in small firms and large firms subsample. Each week, we classify firms in the bottom size quartile into the group of small firms and classify the remaining firms into the group of large firms. Columns (1) and (2) report results for small firm sample, and Columns (3) and (4) for large firm sample. All regressions include time fixed effects and control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 valid observations in the full sample. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sample with Small Firms		Sample with Large Firms	
	(1) $Return_{w+1}$	(2) $Return_{w+1}$	(3) $Return_{w+1}$	(4) $Return_{w+1}$
<i>Cross-insider order flows</i> _{\$,w}	0.787*** (3.141)		0.197 (1.288)	
<i>Cross-insider order flows</i> _{share,w}		0.781*** (3.098)		0.202 (1.310)
$lnME_w$	-0.533** (-2.425)	-0.532** (-2.419)	0.045 (0.546)	0.045 (0.547)
MOM_w	-0.066 (-0.131)	-0.066 (-0.130)	0.351 (1.172)	0.351 (1.171)
BM_w	0.839 (1.267)	0.841 (1.268)	-0.269 (-0.660)	-0.269 (-0.659)
$Return_{m-1}$	-0.769 (-0.521)	-0.770 (-0.522)	-0.857 (-0.649)	-0.856 (-0.648)
Time FE	Yes	Yes	Yes	Yes
# Obs.	1,813	1,813	5,404	5,404
Adj. R^2	0.193	0.193	0.291	0.291

Table A.9 Fraction of Trading Amount of *Reciprocal Cross-Insider Trading*. This table examines whether the fraction of trading amount in the *reciprocal cross-linked* firms are higher. The sample is the same as Table 7. The dependent variable is the insider's dollar trading amount in the firm divided by the total dollar trading amount of the insider in that month. We multiply the dependent variable by 100 for coefficient interpretability. The independent variable, I_{linked} , is an indicator equal to one if the firm is *cross-linked* to the insider. $I_{reciprocal}$ is an indicator equal to one if at least one linked insider of the firm has profited from trading on the focal insider's own firm. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1) <i>trading fraction</i> _{\$,m}	(2) <i>trading fraction</i> _{\$,m}	(3) <i>trading fraction</i> _{\$,m}	(4) <i>trading fraction</i> _{\$,m}
$I_{linked,m}$	0.266*** (4.917)	0.212*** (4.319)	0.212*** (4.319)	0.211*** (4.346)
$I_{linked,m} \times I_{reciprocal,m}$	0.193** (2.297)	0.184** (2.191)	0.187** (2.254)	0.186** (2.249)
$lnME_{m-1}$		0.020*** (6.174)	0.019*** (6.328)	0.019*** (6.147)
BM_{m-1}		0.003*** (3.709)	0.003*** (3.706)	0.002*** (3.225)
MOM_{m-1}		-0.004** (-2.242)	-0.004** (-2.155)	-0.003* (-1.711)
$Return_{m-1}$		-0.015** (-2.163)	-0.014** (-2.114)	-0.011* (-1.823)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
#Obs.	50,463,879	44,867,769	44,867,769	44,867,769
Adj. R^2	0.001	0.002	0.004	0.004

Table A.10 **Profitability of Cross-Insider Trading and Linked Insider Moving Out: Alternative Window Length.** This table extends the window length of Table 9 to three years. Specifically, We extend the event window symmetrically as far as link history and post-event return coverage allow, up to 36 months on each side. Considering the compliance concerns, we exclude the transition period (6 months before the departure date) from the sample. The sample includes trades in the treated firm and in same-industry control firms. The dependent variable is the future 20-day DGTW-adjusted cumulative abnormal return, multiplied by +1 for buy trades and -1 for sell trades and winsorized at the 1st and 99th percentiles across unique trades. All variable definitions follow Table 9. Pre_1 indicates the earliest year of the pre-event period, while Pre_2 corresponds to the Pre period defined in Table 9. All models include insider-stock and industry-time fixed effects, and columns (2) and (4) control for the previous month-end firm size, book-to-market ratio, momentum, and past one-month return. T-values are in parentheses, with standard errors clustered at the stock level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) CAR_{20}	(2) CAR_{20}	(3) CAR_{20}	(4) CAR_{20}
$I_{linked,m} \times Post_m$	-1.777** (-2.432)	-2.176*** (-3.947)	-1.784*** (-2.614)	-2.211*** (-3.060)
$I_{linked,m} \times Pre_{1,m}$			-1.041 (-0.589)	-0.984 (-0.544)
$I_{linked,m} \times Pre_{2,m}$			0.332 (0.214)	0.256 (0.176)
$Post_m$	-0.417 (-0.741)	-0.476 (-0.835)	-0.423 (-0.729)	-0.492 (-0.840)
$Pre_{1,m}$			-0.576 (-0.724)	-0.618 (-0.791)
$Pre_{2,m}$			-0.320 (-0.571)	-0.386 (-0.694)
$lnME_{m-1}$		-3.098*** (-3.988)		-3.103*** (-4.013)
BM_{m-1}		0.200 (0.274)		0.197 (0.269)
MOM_{m-1}		0.643 (1.316)		0.640 (1.309)
$Return_{m-1}$		2.089 (1.436)		2.103 (1.448)
Insider $\times$ Stock FE	Yes	Yes	Yes	Yes
Industry $\times$ Time FE	Yes	Yes	Yes	Yes
# Obs.	12,831	12,831	12,831	12,831
Adj. R^2	0.066	0.071	0.066	0.070

Table A.11 **Profitability of Cross-Insider Trading Before and After Linked Insider Moving Out.** This table complements the difference-in-differences evidence in Table 9 by examining the profitability difference between trades in linked firms and same-industry control firms before and after the linked insider’s “moving out” event. The move-out month is identified from the linked insider’s LinkedIn employment history. The sample includes trades in the treated firm and in same-industry control firms within a symmetric window of up to 24 months around the event. Considering the compliance concerns, we exclude the transition period (6 months before the departure date) from the sample. The dependent variable is the future 20-day DGTW-adjusted cumulative abnormal return, multiplied by +1 for buy trades and –1 for sell trades and winsorized at the 1st and 99th percentiles across unique trades. The coefficient on $\mathbf{I}_{linked}$ captures the pre-event profitability difference, the coefficient on $\mathbf{I}_{linked} \times Post$ captures the change following the event, and their sum represents the post-event profitability difference. All models include event fixed effects, defined at the focal-insider–linked-firm level. Columns (1) and (3) include time fixed effects, while columns (2) and (4) include industry-time fixed effects. Columns (3) and (4) control for the previous month-end firm size, book-to-market ratio, momentum, and past one-month return. T-values are in parentheses, with standard errors clustered at the stock level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) CAR_{20}	(2) CAR_{20}	(3) CAR_{20}	(4) CAR_{20}
$\mathbf{I}_{linked,m}$	1.274** (2.396)	1.530** (2.520)	1.291** (2.339)	1.557** (2.496)
$\mathbf{I}_{linked,m} \times Post_m$	-1.210* (-1.940)	-1.408** (-2.012)	-1.233* (-1.948)	-1.433** (-2.042)
$Post_m$	-0.087 (-0.173)	-0.905 (-1.409)	-0.115 (-0.227)	-0.904 (-1.404)
$lnME_{m-1}$			0.045 (0.616)	0.026 (0.358)
BM_{m-1}			0.155 (0.577)	0.115 (0.413)
MOM_{m-1}			0.154 (0.387)	0.015 (0.033)
$Return_{m-1}$			-0.293 (-0.227)	0.610 (0.467)
Event FE	Yes	Yes	Yes	Yes
Time FE	Yes	No	Yes	No
Industry $\times$ Time FE	No	Yes	No	Yes
# Obs.	10,284	10,180	10,284	10,180
Adj. R^2	0.026	0.042	0.026	0.041

Table A.12 Profitability of Cross-Insider Trading and Linked Insider Moving Out: Placebo Test. This table uses trades by focal insiders whose links to the treated firm remain intact to provide a placebo test for the move-out events examined in Table 9. Specifically, for each move-out event, we identify focal insiders who remain connected to the same firm through another linked insider and re-estimate the DID using the same departure month. The sample includes trades in the treated firm and in same-industry control firms within a symmetric window of up to 24 months around the event. Considering the compliance concerns, we exclude the transition period (6 months before the departure date) from the sample. The dependent variable is the future 20-day DGTW-adjusted cumulative abnormal return, multiplied by +1 for buy trades and -1 for sell trades and winsorized at the 1st and 99th percentiles across unique trades. The independent variable I_{linked} is the *cross-linked* dummy as before. *Post* is an indicator equal to one if the trade is after the placebo event. *Pre* is an indicator for the first half of the pre-event period. All models include insider-stock and industry-time fixed effects, and columns (2) and (4) control for the previous month-end firm size, book-to-market ratio, momentum, and past one-month return. T-values are in parentheses, with standard errors clustered at the stock level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) CAR_{20}	(2) CAR_{20}	(3) CAR_{20}	(4) CAR_{20}
$I_{linked,m} \times Post_m$	-0.239 (-0.605)	0.514 (1.227)	-0.176 (-0.395)	0.484 (0.997)
$I_{linked,m} \times Pre_m$			0.125 (0.438)	-0.069 (-0.202)
$Post_m$	-0.043 (-0.313)	-0.147 (-1.013)	-0.112 (-0.683)	-0.201 (-1.166)
Pre_m			-0.243 (-1.452)	-0.221 (-1.353)
$lnME_{m-1}$		-2.563*** (-4.778)		-2.569*** (-4.695)
BM_{m-1}		-0.561 (-1.432)		-0.564 (-1.431)
MOM_{m-1}		0.564 (0.916)		0.567 (0.909)
$Return_{m-1}$		0.119 (0.049)		0.113 (0.046)
Insider $\times$ Stock FE	Yes	Yes	Yes	Yes
Industry $\times$ Time FE	Yes	Yes	Yes	Yes
# Obs.	41,743	41,743	41,743	41,743
Adj. R^2	0.076	0.079	0.076	0.079

Table A.13 **Likelihood of Cross-Insider Trading and Linked Insider Moving In.** This table examines whether the likelihood of *cross-insider trading* increases after a linked insider’s “*moving in*” event. We only include *cross-insider links* where the focal insider is linked to the linked firm through one linked insider, and define the “*moving in month*” as the month when the linked insider starts serving as an insider in the linked firm. We also drop stocks that the focal insider traded before the event to rule out confounding factors. If the treated stock was traded by the focal insider before the event, then the event is dropped. The sample includes the treated firm and same-industry control firms from the event month to 36 months later. The dependent variable is a trade dummy equal to one if the insider trades the firm in the month, and zero otherwise. For the interpretation of coefficients, we multiply the dependent variable by 100. The independent variable I_{linked} is the *cross-linked* dummy as before. Models progressively include event and industry-time fixed effects, and control for the previous month-end firm size, book-to-market ratio, momentum, and past one-month return. Event fixed effects are defined at the focal-insider–linked-firm level. T-values are reported in parentheses, with standard errors clustered at the stock level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) $I_{trade,m}$	(2) $I_{trade,m}$	(3) $I_{trade,m}$	(4) $I_{trade,m}$
$I_{linked,m}$	0.175*** (5.176)	0.061* (1.910)	0.074** (2.495)	0.070** (2.256)
$lnME_{m-1}$		0.055*** (8.304)	0.050*** (8.239)	0.051*** (8.334)
BM_{m-1}		0.011*** (4.936)	0.008*** (4.371)	0.008*** (4.467)
MOM_{m-1}		-0.008*** (-2.984)	-0.008*** (-2.849)	-0.008*** (-2.937)
$Return_{m-1}$		-0.016 (-1.335)	-0.025** (-2.006)	-0.026** (-2.031)
Event FE	No	No	No	Yes
Industry $\times$ Time FE	No	No	Yes	Yes
# Obs.	5,146,167	5,146,167	5,146,167	5,146,167
Adj. R^2	0.000	0.001	0.002	0.026

Table A.14 **Profitability of Cross-Insider Trading and Linked Insider Moving In.** This table examines whether the profitability of *cross-insider trading* increases after a linked insider’s “*moving in*” event. We only include *cross-insider links* where the focal insider is linked to the linked firm through one linked insider, and define the “*moving in month*” as the month when the linked insider starts serving as an insider in the linked firm. We also drop stocks that the focal insider traded before the event to rule out confounding factors. If the treated stock was traded by the focal insider before the event, then the event is dropped. The sample includes trades in the treated firm and same-industry control firms from the event month to 36 months later. The dependent variable is the future 20-day DGTW-adjusted cumulative abnormal return, multiplied by +1 for buy trades and −1 for sell trades and winsorized at the 1st and 99th percentiles across unique trades. The independent variable I_{linked} is the *cross-linked* dummy as before. Models progressively include industry-time and event fixed effects, and control for the previous month-end firm size, book-to-market ratio, momentum, and past one-month return. Event fixed effects are defined at the focal-insider–linked-firm level. T-values are reported in parentheses, with standard errors clustered at the stock level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) CAR_{20}	(2) CAR_{20}	(3) CAR_{20}	(4) CAR_{20}
$I_{linked,m}$	0.892** (2.279)	0.818** (2.046)	0.945** (2.260)	1.133** (1.973)
$lnME_{m-1}$		-0.040 (-0.738)	-0.034 (-0.498)	-0.057 (-0.645)
BM_{m-1}		-0.217* (-1.908)	-0.195 (-1.288)	-0.272* (-1.656)
MOM_{m-1}		-0.299 (-1.194)	-0.348 (-0.965)	-0.698 (-1.579)
$Return_{m-1}$		-0.529 (-0.513)	-0.770 (-0.564)	-0.463 (-0.317)
Event FE	No	No	No	Yes
Industry $\times$ Time FE	No	No	Yes	Yes
# Obs.	8,831	8,831	8,627	8,348
Adj. R^2	0.000	0.000	0.031	0.028

Table A.15 **Likelihood of Indirect Cross-Insider Trading.** This table examines whether insiders are more likely to trade *indirect cross-linked* firms. For more details about the definition of *indirect cross-linked*, please see Section 4.3. The dependent variable is a trade dummy equal to one if the insider trades on the firm in the month, and zero otherwise. For the interpretation of the coefficient, we multiply the dependent variable by 100. The key independent variable, $I_{indirect_linked,m}$, is an indicator equal to one if the firm is *indirectly cross-linked* to the insider, and zero otherwise. Other control variables include firm size, book-to-market ratio, momentum, and past 1-month return. T-statistics, based on standard errors clustered at insider, firm, and time levels, are reported in parentheses. The sample period is from January 2007 to June 2020. Statistical significance: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

	(1) $I_{trade,m}$	(2) $I_{trade,m}$	(3) $I_{trade,m}$	(4) $I_{trade,m}$
$I_{indirect_linked,m}$	0.436*** (6.421)	0.219*** (4.095)	0.221*** (4.105)	0.216*** (4.141)
$lnME_{m-1}$		0.078*** (9.485)	0.078*** (9.416)	0.073*** (9.136)
BM_{m-1}		0.013*** (5.807)	0.013*** (5.821)	0.010*** (5.796)
MOM_{m-1}		-0.014*** (-3.872)	-0.014*** (-3.844)	-0.014*** (-3.756)
$Return_{m-1}$		-0.022* (-1.658)	-0.022 (-1.644)	-0.032*** (-3.187)
Insider FE	No	No	Yes	Yes
Industry $\times$ Time FE	No	No	No	Yes
# Obs.	207,386,712	184,491,335	184,491,335	184,491,335
Adj. R^2	0.001	0.003	0.014	0.014

Table A.16 **Return Predictability of Indirect Cross-insider Order Flows.** This table presents results of panel regressions of one-week-ahead stock returns on the current week's *indirect cross-insider order flows*. For the detailed definition of *indirect cross-insider order flows*, please see Section 4.3. For Columns (1) and (2), the main independent variable is the dollar-amount order flows. For Column (3) and (4), the main independent variable is the share-amount order flows. All regressions include time fixed effects, and control for the last month end's firm size, book-to-market ratio, momentum, and past one-month returns. T-values, reported in parentheses, are based on standard errors clustered at firm and time levels. The sample is from January 2007 to June 2020, and is limited to weeks with at least 15 firms with valid observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) $Return_{w+1}$	(2) $Return_{w+1}$	(3) $Return_{w+1}$	(4) $Return_{w+1}$
<i>Indirect cross-insider order flows</i> $_{\$,w}$	0.228*** (3.376)	0.204*** (3.021)		
<i>Indirect cross-insider order flows</i> $_{share,w}$			0.227*** (3.372)	0.203*** (3.015)
$lnME_w$		-0.025 (-0.804)		-0.025 (-0.804)
MOM_w		0.009 (0.084)		0.009 (0.084)
BM_w		-0.065 (-0.370)		-0.065 (-0.370)
$Return_{m-1}$		-0.786 (-1.115)		-0.785 (-1.115)
Time FE	Yes	Yes	Yes	Yes
# Obs.	22,697	21,717	22,697	21,717
Adj. R^2	0.240	0.241	0.240	0.241