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Insider Trading and Information in Financial Markets

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Introduction

Insider trading often receives high attention from the public when it is uncovered. One prominent example is the former Chief Executive Officer (CEO) of Enron, Jeffrey Skilling, who was found guilty of insider trading for selling large amounts of shares while knowing about the company's impeding bankruptcy. Apart from the general public, insider trading is frequently discussed from a legal point of view. This is due to the fact that trading on material nonpublic information is illegal, but oftentimes difficult for prosecutors to prove.

But foremost, the occurrence, the effects, and the role of insider trading have been studied extensively by financial economists because insider trading touches upon fundamental concepts of the field. Trading in their own stock can increase managers' incentive to produce information. Through the trades, the information enters share prices, which increases the efficiency of financial markets. Disclosed trades provide an optimal setting to study how the market reacts to the release of new information, and how information disseminates between different market participants. Such analyses are at the center of all three essays of this thesis. The efficiency gain has to be weighted against concerns about fairness. In the words of Gary Gensler, the Chair of the Securities and Exchange Commission (SEC), "... few things undermine trust in the markets more than insiders abusing their positions for personal advantage, such as by trading using material nonpublic information.".[1] A loss of investors' confidence in the fairness of markets can have far-reaching consequences, including a decrease in access to capital for firms. Insider trading is also closely related to corporate governance issues. On one hand, stock-based compensation should reduce agency conflicts by aligning the interests of managers (agents) and shareholders (principals), a concept dating back to Jensen and Meckling (1976). On the other hand, companies are obligated to protect their shareholders from informed managerial trading, which is often done with the help of blackout windows which restrict trading at times of high information asymmetry. Such corporate governance considerations are crucial for this thesis' third essay.

The connection to major topics of financial economics such as market efficiency, information, or corporate governance makes insider trading an important field to study. With this thesis, my goal is to expand the knowledge in this area. In the following passages, I summarize the three essays which together constitute this thesis, and discuss their contribution to the literature before reaching an overarching conclusion.

[1] Remarks by Chair Gensler Before the American Bar Association, Harvard Law School Forum on Corporate Governance, accessed on 10 October 2024.

Essay 1: Loan Renegotiation and Information Diffusion: The Role of Insider Trading

The first essay is joint work with Philip Valta. We use data on loan renegotiations, insider trades, and stock returns from publicly traded U.S. firms to investigate the flow of private information. We argue that banks' information on their borrowers disseminates to managers during loan renegotiations. When managers trade in their firm's stock, the information reaches investors in financial markets and impacts share prices. We find that stock returns are higher (lower) following months with both informative insider purchases (sales) and loan amendments. Consistent with an information transfer, the effect is stronger when the involved lenders have a high quality, and for firms without credit rating, closer to default, and with more illiquid stocks.

This paper contributes to the literature by establishing insider trading as a new channel, through which financial markets receive private information from lenders. Insider trades are therefore helpful for market participants to interpret loan renegotiations. We are the first to look at the joint effect of insider trading and loan amendments on future stock returns. By documenting the additional effect on returns of this combination of events, we show that the market receives valuable information about borrowers' financial health. The effect on returns also contributes to the market efficiency literature as it shows that insider trades around loan amendments help keep security prices closer to fundamental values.

Essay 2: How Do Directors and Officers React to Insider Trading in Peer Firms?

The second essay is single-authored. I examine how officers and directors react to insider trading activities in closely related companies. My main finding is that probability, frequency, and profitability of trades increase when they are done after peer trades. The increase in frequency and probability tends to happen in the same direction as in the peer companies. The results are consistent with managers gathering valuable information from peer trades about their company's industry. In principle, managers could also increase trading for purely social reasons (peer effect). However, additional evidence from peer trades influenced by factors orthogonal to the focal firm, speaks against this approach.

This study contributes to the literature by providing evidence that insider trades are relevant not only for the valuation of the company whose shares are traded, but also for peer firm valuation. Higher profitability following peer trades can also be interpreted as constituting an additional channel through which insiders can monetize their advantage in processing publicly available information. Second, I add insider trades as a channel for intra-industry information spillovers. So far, this strand of literature has been more focused on earnings announcements. Third, I contribute to the literature on managerial learning from outside sources by showing that besides the well-established learning from stock prices, managers can gather relevant information from insider trades in peer companies.

Essay 3: How Do Founders and Venture Capitalists Sell Their Shares after the IPO?

The third essay is joint work with Rüdiger Fahlenbrach. We analyze shareholder protection in newly public firms using a hand-collected sample of U.S. venture-backed companies where founders are still active. Our main finding is that the current shareholder protection framework is not sufficient to prevent insiders from generating abnormal returns to their sales transactions. As weak spots we identify the generous limits set by Rule 144, the circumvention of the rule by Venture Capitalists (VCs) via in-kind distributions, and the fact that Form 144 can be filed concurrently with the actual transaction on Form 4, rendering the former useless.

This paper makes multiple contributions to the literature. First, we provide a detailed overview on the evolution of ownership by founders, financial and strategic VCs after going public. For our sample, we can link ownership and trades. We contribute to the insider trading literature by showing that in newly public firms, insider transactions are still highly profitable. Lastly, we provide an update to Gompers and Lerner (1998) which is due since the regulation on disclosure of in-kind distributions has changed. Our results indicate that increased disclosure has no notable effect on the profitability of these distributions.

Overall, my thesis contributes to the literature in the aforementioned areas of information in financial markets, corporate governance, and market efficiency by analyzing three aspects of insider trading. The first essay shows how information that managers obtain from lending banks enters share prices through insider trading. The second essay concentrates on the information flow between managers in companies operating in related product markets. Its contribution is to establish managerial learning from peer insider trading. The third essay looks at how insiders reduce their holdings in newly public firms. It expands the literature by revealing gaps in current shareholder protection laws in the U.S. and in young firms' corporate governance structures.

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Contents

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Acronyms

ADR American Depositary Receipt. 85

AM Amihud Illiquidity Measure. 117

AMEX American Stock Exchange. 85

B/M Ratio Book-to-Market Ratio. 3, 10, 12, 15–17, 19, 21, 24, 25, 27, 29, 31, 33, 34, 40–42, 47, 48, 52, 57, 58, 60, 63, 64, 66, 67, 69, 71, 72

CAR Cumulative Abnormal Return. 7, 48, 52–55, 57, 59–61, 63, 64, 66, 67, 69–72, 80, 106–112

CDS Credit Default Swap. 50

CEO Chief Executive Officer. vi, 80, 82, 83, 87

CIK Central Index Key. 87, 88

CMP Cohen, Malloy, and Pomorski. 47

CRSP Center for Research in Security Prices. 3, 9, 47, 50, 52, 54, 85, 88, 89, 105, 117

D&O Directors and Officers. 47, 49, 51, 57, 60, 63, 64, 66–69, 71, 72, 82, 83, 85, 87, 93, 95, 98

EDGAR Electronic Data Gathering, Analysis, and Retrieval. 84, 85, 87, 90

FDAAA Food and Drug Administration Amendments Act of 2007. 89

FHT Fong, Holden, and Trzcinka. 5, 26, 27, 117

GICS Global Industry Classification Standard. 103

I/B/E/S Institutional Brokers' Estimate System. 22, 51

IPO Initial Public Offering. 79–83, 85–90, 92, 93, 95, 97–104, 106, 113, 117, 118

M&A Mergers & Acquisitions. 92

NASDAQ National Association of Securities Dealers Automated Quotations. 9, 51, 85

NPR Net Purchase Ratio. 4, 10, 12, 18, 19, 40, 47–49, 52, 53, 56, 57, 65, 66, 69

NYSE New York Stock Exchange. 9, 51, 85

PE Private Equity. 87

PSU Performance Stock Unit. 90

QEA Quarterly Earnings Announcement. 22, 40, 42, 52, 57, 58, 60, 63, 64, 66, 67, 69, 71, 72

R&D Research and Development. 8

REIT Real Estate Investment Trust. 85

RSU Restricted Stock Unit. 90

S&L Savings and Loan Association. 85

S&P Standard & Poor's. 10, 12, 23, 24, 40

SDC Securities Data Company. 3, 4, 9, 11, 18, 21, 28, 30

SEC Securities and Exchange Commission. vi, 3, 7, 11, 20, 32, 47, 51, 79, 82, 84, 85, 90, 91, 95, 117

SEO Seasoned Equity Offering. 6, 8, 14, 87

SIC Standard Industrial Classification. 9, 47, 49, 51, 53, 62, 64, 70, 73

SUR Seemingly Unrelated Regressions. 23

TNIC Text-based Network Industrial Classification. 47, 49, 51, 54, 62, 64

USD U.S. dollar. 10, 15, 17–19, 21, 24, 25, 27–29, 31, 33, 34, 40–42, 46, 48, 52, 57, 60, 63, 64, 66, 67, 69, 71, 72, 80, 82, 85, 90, 92, 97, 105

VC Venture Capitalist. viii, 79, 84, 85, 93, 98, 99, 102–105, 111–113

WRDS Wharton Research Data Services. 105, 107, 108, 111

Loan Renegotiation and Information Diffusion: The Role of Insider Trading*

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Abstract

This paper analyzes the effects of insider trading around loan renegotiations on stock returns. Using a large sample of loan renegotiations for publicly traded U.S. firms, the paper shows that stock returns are 1.35% higher (0.85% lower) following months with both non-routine insider purchases (sales) and loan amendments. This effect is stronger when the involved lenders have a high quality, and for firms without credit rating, closer to default, and with more illiquid stocks. The findings suggest that insider trades are an important channel through which lenders' private information about the financial health of borrowers diffuses to market participants.

Keywords: Stock Returns, Loan Renegotiation, Insider Trading, Information Diffusion
JEL Classification Numbers: G14, G21, G32

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1 Introduction

Bank loans are the dominant source of finance for many firms (e.g., Berg et al. (2021)). A typical bank loan contains covenants and other contingencies as a way to allocate state contingent control rights ex ante (Aghion and Bolton (1992); Dewatripont and Tirole (1994)). Recent research shows that loan contracts are frequently renegotiated outside of bankruptcy along multiple dimensions, and that renegotiation is a way to allocate control rights and bargaining power ex post (e.g., Roberts and Sufi (2009); Roberts (2015)). The parties involved in a loan renegotiation are the borrower's management and the lenders. Banks are sophisticated market participants with private information about borrowers' financial health and future cash flow potential (e.g., Fama (1985); Sharpe (1990)). They produce information about the borrower either through research and screening (e.g., Campbell and Kracaw (1980)), or they have access to information through repeated interactions with the borrower's management (e.g., Bharath et al. (2011)). Hence, the private information that lenders produce about a borrower likely affects renegotiation outcomes and is relevant for asset prices. However, little is known how and through which channel the incremental information about the borrower revealed in a loan renegotiation diffuses to financial markets.

In this paper, we aim at closing this gap and analyze how information about the borrower diffuses from lenders to financial market participants. To do so, we investigate the trading of corporate insiders in the shares of their firm around a loan renegotiation event. Corporate insiders, such as the firm's top management, have privileged access to material information from the lender through the renegotiation process and can learn about the lender's assessment of the borrower's financial health. Therefore, the trades of corporate insiders in the shares of their firm around loan amendments can inform other market participants about the content of the loan amendment and whether it is value increasing or decreasing.

Building on theories of market efficiency (e.g., Fama (1970); Grossman and Stiglitz (1980)), where informed investors help keep security prices close to fundamental values, we hypothesize that because the top management and the directors are better informed about the content of the loan renegotiation compared to other market participants, their non-routine (i.e., opportunistic) insider trading behavior around loan amendments is informative about asset prices. Specifically, we predict that loan amendments that are accompanied by opportunistic inside purchases are value increasing and predict higher future returns. By contrast, loan amendments that are accompanied by opportunistic inside sales are value decreasing and predict lower future returns. In addition, because loan amendments of firms closer to default should be more relevant to market participants, we expect these effects to be more pronounced for firms with low credit quality and high default risk. Furthermore, if prices of stocks with low liquidity are less informative, we expect the incremental information from loan amendments to be more relevant for stocks with low liquidity. Finally, the information revealed in loan renegotiations is likely going to be more relevant when the involved lender has a high quality. Therefore, we expect the signal from insider trading to be stronger for loans renegotiated with lenders that have a high quality.

We test these hypotheses by analyzing stock returns after loan amendments when corporate insiders trade shares of their own firm. Our sample contains 5,239 loan amendments of 1,375 distinct U.S. companies between 2001 and 2020. We collect data on monthly stock returns from the Center for Research in Security Prices (CRSP), and annual firm fundamentals from Compustat. The data on loan amendments come from the Securities Data Company (SDC) Plantinum Syndicated Loan database. For all firms that can be matched to CRSP and Compustat, we identify all amendments to loans made during the sample period. Because amendments to multiple tranches in the same firm-month are frequent, we aggregate the sample to the firm-month level, resulting in 3,693 firm-months with at least one amendment. Using these data, we create a loan amendment indicator variable that equals one when a firm experiences a loan amendment in a given month, and zero otherwise. Data on insider trading is from the Thomson Reuters Insider Trading database. Following existing literature on insider trading, we only look at open market trades in ordinary shares that are published in Table 1 of SEC Form 4. This focus on open market trades implies proportionally more sales compared to purchases in our sample, because, while managers and directors regularly sell shares in the market, they typically receive them through their compensation plans (i.e., through non open market transactions). We classify insider trades following Cohen et al. (2012) and exclude all trades that are considered to be routine trades (regular trading in specific year-months). Insider trades are thus non-routine (or opportunistic) trades that likely reveal incremental information to market participants. We then measure insider trading activity with an indicator variable that equals one when insiders either purchase or sell stock in a given firm-month, and zero otherwise.

To analyze if the information revealed through loan amendments predicts stock returns through insider trading, we regress the stock return of the next month on the current month loan amendment indicator variable, the current month insider trading indicator variable, and the interaction between the two variables. In addition, we include the Book-to-Market Ratio (B/M Ratio), the market capitalization, the return in the current month, and the return in the previous year (from 12 months to one month before the month of the amendment) as firm-specific control variables, and month fixed effects to absorb aggregate shocks that affect all firms in a month. Standard errors are robust to heteroskedasticity and clustered at the firm-level.

Our results show that loan amendments by themselves do not predict stock returns. This result is consistent with the existing literature that finds mixed evidence on the cumulative abnormal short-run returns around the announcement of loan amendments (e.g., Godlewski (2015); Silaghi et al. (2022)). However, once we interact loan amendments with insider trading, we find significantly positive effects on next-month returns for months with insider purchases and loan amendments, and negative effects on next months returns when the amendment is accompanied by insider sales. The estimated effects are economically large. The monthly return following a month with insider purchases and a loan announcement is 1.68% higher compared to a month with none of these events. For months that are followed by a month with both amendments and insider sales, stock returns are 1.07% lower. The effects stem mostly from the interaction of amendments and trading for which we estimate coefficients of 1.35% for purchases and −0.85% for sales, respectively.

Our main result is robust to alternative specifications and estimation methods. Specifically, we estimate the main specification and include firm fixed effects, or double cluster standard errors by firm and month. Alternatively, we estimate the model with Fama-MacBeth, allowing for serial correlation in stock returns. The results remain quantitatively largely unchanged. Second, we use the Net Purchase Ratio (NPR) developed by Lakonishok and Lee (2001) that we calculate based on the transaction value and the number of transactions as an alternative measure of insider trading. This analysis complements our main results by establishing that our findings are not driven by small insider trades.

Third, we investigate the robustness to our definition of loan amendment that depends on the classification by SDC Platinum. Specifically, we complement the definition of loan amendment by announcements of amendments that we determine based on strings of notes in SDC. Alternatively, we use the hand-collected sample of loan renegotiations from Roberts (2015), match these data with our sample, and re-estimate the main specification. We confirm the main result that next month returns are positive after months with loan amendments and inside purchases, and negative after months with loan amendments and inside sales. Fourth, we address concerns related to potential confounding events and exclude all months from our sample that feature an earnings announcement. Our coefficient estimates remain largely unchanged.

So far, our results are consistent with the hypothesis that insider trades are a channel through which lenders' information about the future prospects of borrowers diffuses to financial markets. In a next step, we characterize this economic channel in more detail and provide further evidence that supports the interpretation of the results. First, we expect the results to be more pronounced for firms closer to financial distress. Therefore, we group firms based on their credit rating. We show that insider trading around loan amendments is particularly informative for firms with a low credit rating or for firms without a credit rating. These are exactly the firms for which lenders' incremental information (revealed through loan amendments) about the financial health is likely to be most relevant. Second, and similarly, we show that the effect is more pronounced for firms closer to default using a market-based distance to default measure. Third, we split firms based on stock market liquidity. We expect the prices of less liquid stocks to contain less information. Therefore, incremental information that is revealed through insider trading should have a larger impact on these stock prices. We confirm this prediction using the illiquidity measure of Fong et al. (2017) (FHT).

Next, we analyze the role of lender quality. Banks play a crucial role in the information production along the lending process by screening potential borrowers and by monitoring borrowers while the loan is outstanding. The heterogeneity in bank quality influences the amount and precision of information produced during the analysis of a borrower. We expect that higher quality banks are able to produce more valuable information. Building on Ross (2010) and measuring lender quality using rankings in league tables, we show that loan amendments, where high-quality lenders are involved, are more predictive for future returns. Further, existing literature shows that lenders gain additional information about borrowers over the duration of the lending process (e.g., Botsch and Vanasco (2019)). Similar to the effect that lender quality has on information production, we expect banks to be better informed about a borrower after repeated interactions. To test this prediction, we compare returns after insider trading around *new* loan originations with returns after insider trading around loan amendments (implying previous and repeated interactions). Consistent with this prediction, future stock returns are significant only after amendments accompanied by insider trades, but not after new loan originations.

Finally, we split firms into subsamples based on the day of the month on which an amendment is announced and insiders trade. Our analysis shows that the main effect is driven by amendments taking place in the second half (last five days) of the month. This finding suggests that it takes market participants some time to digest the news about the amendments. Neuhierl et al. (2013) find that stock prices reflect the information of earnings announcements within about three trading days. It seems reasonable to assume that loan amendments, which are less standardized and more difficult to interpret, take more time to be reflected in stock prices.

Our paper makes several contributions to the literature. First, the paper contributes to the literature on loan renegotiation. Existing research shows that loan renegotiations are consistent with incomplete contracting and occur frequently (e.g., Roberts and Sufi (2009); Garleanu and Zwiebel (2009); Roberts (2015)). More recent papers analyze short-term announcement returns to loan amendments (e.g., Godlewski (2015); Nikolaev (2018); Silaghi et al. (2022)). Our paper advances this literature by analyzing a distinct channel, insider trading, through which lenders' private information about the borrower diffuses to financial markets. While the incremental information contained in a loan renegotiation is hard to interpret for market participants, it can be inferred by observing the trades by insiders that are involved in the renegotiation process.

Second, our paper contributes to the literature on insider trading. Several papers examine the cross-sectional return forecasting ability of insider trades either aggregated at the firm (e.g., Lorie and Niederhoffer (1968); Jaffe (1974); Rozeff and Zaman (1988); Seyhun (1992); Lakonishok and Lee (2001); Akbas et al. (2020)) or the individual insider (e.g., Scott and Xu (2004); Jenter (2005); Cohen et al. (2012)). In addition, several papers explore insider trading around corporate events such as debt issues (Kahle (2000)), Seasoned Equity Offerings (SEOs) and share repurchases (Cziraki et al. (2021)), or takeovers (Agrawal and Nasser (2012)), and how they relate to returns. Our focus in this paper is on so far unexplored loan renegotiation events. These events occur frequently and contain important information about the financial situation of a firm. Our results show that the key role of non-routine insider trades around loan amendments is to provide valuable information to market participants about the borrower's financial health.

Finally, our results contribute more generally to the literature that examines how insider trading relates to the efficiency of financial markets (e.g., Seyhun (1986)). We show that the propagation of information about the borrower through the trades of insiders leads to an adjustment in stock prices and helps keep security prices closer to fundamental values.

The paper is organized as follows. Section 2 summarizes the relevant literature and develops the hypothesis. Section 3 presents the sample, the description of the variables and methodology, and summary statistics. Section 4 reports the main results and robustness tests. Section 5 analyzes the economic channel. Section 6 concludes.

2 Related Literature and Hypotheses

2.1 Loan Amendments

It is not possible for the parties in a loan agreement to contract upon all future states of the world. Moreover, there is an information gap between lender and borrower concerning the borrower's intentions. Maskin and Moore (1999) show in an incomplete contract setting that after a shock to the borrower, it is Pareto-optimal to renegotiate the contract instead of forcing compliance, as there is an ex-post bargaining surplus. Roberts and Sufi (2009) argue that new information on the borrower's credit quality or on the outside options to obtain financing should trigger renegotiation. Similarly, Garleanu and Zwiebel (2009) show that covenants in loan contracts are strict at the beginning to allow the lender to take control in certain states, and that these covenants are loosened as information asymmetry decreases.

The announcement of an amendment reveals information to the public that was private and only known to the lender and the borrower. Creditors' main concern is the borrower's ability to repay. Therefore, they primarily collect information helping them to assess the probability of default. They obtain this information through the ongoing relationship with the borrower that is not limited to the loan contract (Petersen (1999)). The public release of information about amendments should therefore lead to a significant stock price reaction (Nikolaev (2018)). Roberts (2015) argues that an update of the contract should in principle make the contract more efficient. Godlewski (2015) mentions the certification role of bank loans as they reflect a positive lending decision by the lending bank. This effect should become stronger with a decrease of information asymmetry over time. Finally, Silaghi et al. (2022) interpret the market reaction to loan amendments as reflecting a decrease in uncertainty that might lead to a wealth transfer from shareholders towards lenders. In sum, these different explanations suggest that a loan amendment can be positive or negative for the value of the borrower. The borrower could, for example, initiate a renegotiation to finance a growth opportunity, or a renegotiation might become necessary to prevent default.

Early research on loans finds positive abnormal returns to loan announcements (James (1987)) and renegotiations (Lummer and McConnell (1989)). However, Maskara and Mullineaux (2011) argue that these abnormal returns are an artifact of sample selection, due to small firms with greater informational asymmetry having a stronger incentive to publish announcements related to loans (see Diamond and Verrecchia (1991)). In the sample of Maskara and Mullineaux (2011), which reflects the universe of bank loans, the authors find no significant reaction to such events except for the smallest firms in the sample. More recently, several studies have produced mixed results for the short-term market reaction to bank loan amendments. Godlewski (2015) finds positive abnormal returns to covenant renegotiation, but negative reactions to less important amendments such as changes to definitions. Using a sample drawn from SEC filings, Nikolaev (2018) reports significantly positive Cumulative Abnormal Returns (CARs) for the five days surrounding the loan amendment, with the effect stemming largely from the announcement day. In contrast, Silaghi et al. (2022) find a negative, but not significant reaction both for a three- and a ten-day trading window around amendments. All three papers are concerned with short-term effects and do not consider long-term effects on returns.

2.2 Insider Trading

It is a well established fact in the literature that trading by corporate insiders such as officers, managers, board members, or investors that hold a large fraction of shares predicts future stock returns. In fact, Gao et al. (2022) provide evidence that not only insider trading, but also the absence thereof can predict returns. There are several channels through which insiders obtain the private information that gives their trades predictive power. Seyhun (1992) makes the distinction between insiders' ability to predict future cash flows faster than other market participants and their superior capacity to discover misvaluation. He finds that both effects have a positive impact on predictive power. Piotroski and Roulstone (2005) confirm that insiders act as contrarian investors and that they possess superior knowledge (i.e., their expectations differ from market beliefs) also at the firm-level, not only for the aggregated stock market.

With higher information asymmetry, the ability to predict cash flows can lead to a higher profitability of trades, as is shown in Research and Development (R&D) firms by Aboody and Lev (2000). With up to nine quarters into the future, insiders have a long forecasting horizon for their firm's cash flows (Ke et al. (2003)). Insiders' proficiency in spotting misvaluation is shown around events that are more likely to take place when a company's stock is seen as inadequately priced. Examples include SEOs (see, e.g., Lee (1997) or Clarke et al. (2001)) and share repurchases (Jenter (2005)). More recent work establishes that higher attention to publicly available information is a third potential channel through which insiders gain an information advantage that they can exploit. Alldredge and Cicero (2015) provide evidence for profitable informed trading on public information about the main customer. Chabakauri et al. (2022) investigate trading at the onset of activist investor involvement.

Lakonishok and Lee (2001) find that insider purchases are more predictive for future returns than sales. Other papers find that in numerous scenarios, purchases *and* sales predict the direction of returns, such as surprising trades (Cohen et al. (2012), Akbas et al. (2020)), and for trades by insiders, which have executed profitable trades in the past (Cline et al. (2017)) or shown opportunistic behavior (Ali and Hirshleifer (2017)). Jagolinzer et al. (2020) also find that the trade direction and subsequent stock performance coincide when insiders are politically well-connected. Biggerstaff et al. (2020) analyze the longevity of insiders' information advantage and confirm the previous finding. Finally, insider trading has been analyzed in connection to a variety of corporate events such as debt issues (Kahle (2000)), SEOs and share repurchases (Cziraki et al. (2021)), or takeovers (Agrawal and Nasser (2012)).

2.3 Hypotheses

Lenders play a crucial role in the information production along the lending process by screening potential borrowers and by monitoring borrowers while the loan is outstanding. We argue that during the loan renegotiation process, insiders infer private information from lenders about the borrower's financial health. This information then diffuses to financial markets when insiders trade shares of their own firm around the loan renegotiation events. We expect share price movements around loan amendments to be in the same direction as the insider trades surrounding them. In addition, we expect the incremental information about the borrower to be most valuable when the borrower has a low credit quality, high default risk, or low stock liquidity, and when the involved lender has a high quality. Before turning to the empirical analysis, we summarize the main testable hypotheses:

Hypotheses: *Non-routine inside stock purchases (sales) around loan amendments predict higher (lower) stock returns. This effect is more pronounced for firms with lower credit quality, higher default risk, lower liquidity, and for loans where the lender has a high quality.*

3 Data and Method

3.1 Sample

To construct our sample, we collect data from four different sources: SDC Platinum's Syndicated Loans database, Thomson Reuters Insider Trading database, and CRSP/Compustat for stock returns and balance sheet data. From the intersection of these four data sets, we restrict our sample to U.S. companies listed on the New York Stock Exchange (NYSE) or National Association of Securities Dealers Automated Quotations (NASDAQ). We exclude firms in the financial industry (Standard Industrial Classification (SIC) 6000-6999). Firms must have at least one amendment to their bank loan and some insider trading activities during our sample period. Since we investigate the impact of renegotiation, we only include firm-months in which the company has an outstanding loan (such that renegotiation can take place). Our sample has 1,375 distinct companies with a total of 179,651 firm-months between 2001 and 2020.

3.2 Variables and Descriptive Statistics

3.2.1 Loans and Amendments

Our sample contains 8,219 distinct loan packages that have been issued and active at some point during our sample period and 5,239 loan amendments. Because multiple tranches are often amended simultaneously, we aggregate our data to the firm-month level to prevent overestimating months with multiple amendments. This aggregation leads to 3,693 firm-months with one or multiple amendments. Our sample compares well to others used in the literature (e.g., Maskara and Mullineaux (2011)). This indicates that we are able to adequately represent the loan universe and that we are not affected by the selection bias towards small firms that is inherent in the early literature on loan announcements and amendments. By contrast, the average tranche amount in our sample tends to be larger than in Maskara and Mullineaux (2011). The number of banks in a syndicate varies from one to 25, and the variation in tranche amounts (see Table 1, Panel A) also reflects the diversity of loans in the sample. Table A1 in the Appendix contains definitions of variables used in the analysis.

Table 1: Descriptive Statistics

This table reports summary statistics. *Amendments per Firm* is the no. of amendments per firm during our sample period. *Amendments per Firm-month* is the no. of amendments per firm-month. *Spread* is the spread above the base rate for all sample loans. *Tranche Amount* is the amount in millions of U.S. dollar (USD) per loan. *No. of Tier 1 Agents* is the no. of (co-)agents per loan. *No. of Insider Purchases (Sales)* describes the no. of insider purchases (sales). *Net Purchase Ratio (Trx. Value) (No. of Trades)* summarizes the NPR based on transaction value (number of trades) as defined in Equation 1. *Monthly Return* summarizes the monthly return. *Ret. after Purchase (Sale)* summarizes the monthly return in the firm-month following a firm-month with only non-routine insider purchase(s) (sale(s)). *Ret. after Amend. & Purchase (Sale)* summarizes the monthly return in the firm-month following a firm-month with a loan amendment and non-routine insider purchase(s) (sale(s)). *Market Cap* denotes the market capitalization (common shares outstanding × share price) in millions of USD. *Book-to-Market Ratio* describes the B/M Ratio defined as (book value per share × common shares outst.) divided by market cap. *Distance to Default* uses the naïve approach of Bharath and Shumway (2008). *Rated* is an indicator for Standard & Poor's (S&P) rating. *Unique Companies* is the number of distinct firms in the sample. The sample period is Jan 2001 to Dec 2020.

Panel A: Loans & Amendments

	N	Mean	Median	Std. Dev.	5%	95%
Amendments per Firm	5,239	3.80	3.00	3.64	1	11
Amendments per Firm-month	5,239	0.03	0.00	0.23	0	0
Spread (bps)	7,529	169.67	150.00	111.09	30	362
Tranche Amount (millions of USD)	8,218	808.77	397.76	1,414.25	50	2,850
No. of Tier 1 Agents	8,181	4.30	4.00	2.81	1	10

Panel B: Trades

	N	Mean	Median	Std. Dev.	5%	95%
No. of Insider Purchases	179,651	0.27	0.00	4.10	0	1
No. of Insider Sales	179,651	3.12	0.00	27.37	0	11
Net Purchase Ratio (Trx. Value)	179,651	-0.27	0.00	0.54	-1	1
Net Purchase Ratio (No. of Trades)	179,651	-0.26	0.00	0.54	-1	0

Panel C: Returns

	N	Mean	Median	Std. Dev.	5%	95%
Monthly Return	179,651	1.16	1.07	11.24	-16.90	19.30
Ret. after Purchase	8,348	1.92	1.41	13.97	-21.05	26.41
Ret. after Sale	48,285	0.95	1.05	9.09	-13.58	15.11
Ret. after Amend. & Purchase	213	3.15	2.89	14.73	-22.28	34.28
Ret. after Amend. & Sale	1,020	0.55	0.77	9.59	-15.49	14.47

Panel D: Firms

	N	Mean	Median	Std. Dev.	5%	95%
Market Cap (millions of USD)	179,651	10,445.94	2,068.66	36,654.90	130.02	41,816.30
Book-to-Market Ratio	179,651	0.63	0.44	1.07	0.10	1.57
Distance to Default	163,270	9.05	7.81	6.65	1.00	21.19
Rated	176,191	0.60	1.00	0.49	0.00	1.00
Unique Companies	1,375					

We use a dummy variable to capture the occurrence of one or multiple amendments per firm-month. The variable is based on the variables *amended* and *amendedrestated* in SDC Platinum. These variables are themselves indicators denoting that an entry is either an amendment or an amended and restated loan. Roberts (2015) mentions that the economic interpretation of these events as well as for rollovers is similar because they all represent changes in the loan contract's terms. Because we can only clearly identify the former two categories, we focus on these categories.

A renegotiation can be initiated by either the borrower or the lender. Intuitively, one could think that lenders tend to pull the trigger to negotiate new terms when the borrower's financial situation worsens or after a covenant violation. In practice, however, the majority of amendments are induced by borrowers (see, e.g., Campbell et al. (2014) or Roberts (2015)). We do not include the outcome of the amendment in our analysis. One reason is that we only have this information for roughly half of the amendments in our sample.[1] The second reason is that the amendments do not provide information on whether or not the outcome is good or bad for the borrower. For example, even if the outcome is an increase in the committed amount, we cannot infer whether the involved parties and market participants expected a larger increase ahead of the negotiation.

3.2.2 Insider Trading

In the U.S., corporate insiders have to disclose their trading activities according to the Securities Exchange Act of 1934 (sec. 16a). This law mandates disclosure for directors, officers and beneficial owners of at least 10% of the company's shares. For our analysis we do not include the trades of large shareholders if they do not hold either a director or an officer position. In accordance with previous papers measuring insider trading, we only look at open market trades in ordinary shares that are shared with the public on Table 1 of SEC Form 4. The focus on open market trades explains the dominance of sales as described in Panel B of Table 1. While managers regularly sell shares in the open market, they mostly receive shares through their compensation plans (i.e., non open market transactions).

Trades by insiders can have a number of causes and not all of them have to do with the company's performance. For example, managers can sell shares to diversify their wealth or to satisfy their liquidity needs. For our analysis, however, we are interested in trades that reveal incremental information about the company. We employ the method of Cohen et al. (2012) to classify trades into routine and opportunistic trades. If an insider has traded in the same calendar month for three consecutive years, we label her as a routine trader.[2] Insiders that do not display such regular trading behavior are classified

[1] Of these, the majority are extensions to the maturity, changes in pricing, or changes in the commitment (mostly increases).

[2] We classify trades from 1997 onward to guarantee that all insiders can potentially be routine traders.

as opportunistic.[3] We then construct an insider trading indicator variable equal to one when an insider executes non-routine purchase(s) (sale(s)) in a given firm-month, and zero otherwise.

In a robustness analysis (Section 4.3), we use the NPR by Lakonishok and Lee (2001) as an alternative measure for insider trading. It is defined as follows:

$$\text{NPR} = \frac{\text{Value (No.) of purchases} - \text{Value (No.) of sales}}{\text{Value (No.) of purchases} + \text{Value (No.) of sales}} \tag{1}$$

The ratio is continuous and ranges from -1 for months with only sales to 1 for months with only purchases. A value of zero indicates either the same value (number) of purchase and sale transactions or no trading at all. We consider trades in the event month. We calculate the NPR based on transaction values and based on the number of transactions. This allows us to capture the intensive and the extensive margin of insider trades. The two ratios themselves as well as their impact on future returns correlate strongly, which makes us confident that we are not merely picking up noise from small transactions. Consistent with the higher amount of insider sales relative to purchases, the average NPR is negative in our sample.

3.2.3 Returns

We define the month in which we observe insider trading and amendments as t, and the month in which we measure the returns as $t+1$. We control for momentum and reversal using the event month's return and the return of the 12 months starting with month $t-12$ and ending with month $t-1$. Panel C of Table 1 presents descriptive statistics for the returns. As expected, returns are higher (lower) after non-routine insider purchases (sales) compared to average returns. Returns are even higher (lower) following months with an amendment *and* insider trading.

3.2.4 Firm-specific Variables

We control for firm size and the B/M Ratio, two variables commonly known to influence stock returns. Following Cohen et al. (2012), we use the natural logarithm of these variables (values in Panel D of Table 1 are not in log). Also, we treat B/M Ratios above 100 and below zero as mistakes in the data and set them as missing. The distance to default is calculated using Bharath and Shumway (2008)'s naïve approach. The sample firms have a S&P Domestic Long Term Issuer rating for roughly 60% of the

[3] We classify all insiders that do not have a three year consecutive trading history as opportunistic. Cohen et al. (2012) show that their results are robust to adding infrequently trading insiders to the routine or the opportunistic group. In our view, it makes sense to classify them as opportunistic, since they do not show any regular trading pattern.

firm-months, suggesting that the subsample of firms present in our data set is slightly biased towards larger, less opaque firms.

3.3 Empirical Method

To test our main hypothesis, we follow Cohen et al. (2012) and regress monthly stock returns on our measure for loan amendment, insider trading, and the interaction of the two variables. Specifically, the regression we estimate is as follows:

$$\begin{aligned}\text{Return}_{i,t+1} =& \alpha + \beta_1 \text{Amendment}_{i,t} + \beta_2 \text{Insider Purchase}_{i,t} + \beta_3 \text{Insider Sale}_{i,t} \\ &+ \beta_4 \text{Amendment} \times \text{Insider Purch.}_{i,t} + \beta_5 \text{Amendment} \times \text{Insider Sale}_{i,t} \\ &+ \gamma_1 \textit{Firm Controls}_{i,t} + \gamma_2 \textit{Past Returns}_{i,t} + \delta \text{Month Fixed Effects}_t + \epsilon\end{aligned} \tag{2}$$

$\textit{Amendment}_{i,t}$ is an indicator variable equal to one when when firm i experiences one or more loan amendments in month t, and $\textit{Insider Purchase}_{i,t}$ ($\textit{Sale}_{i,t}$) is an indicator variable equal to one when one or more insiders trade shares of their firm i in month t. $\textit{Firm Controls}_{i,t}$ is a vector of control variables that includes *ln(market capitalization)* and *ln(book-to-market)*, the vector $\textit{Past Returns}_{i,t}$ includes current month and previous year stock returns. To account for potential time effects, we include month fixed effects in our main specification, while also including firm fixed effects in robustness analyses to account for time-invariant firm heterogeneity. We cluster standard errors at the firm level, and in robustness analyses both at the firm and month level (see, e.g., Petersen (2009)). In additional analyses, we also estimate Fama and MacBeth (1973) regressions and use Newey and West (1987) standard errors with a six-month lag.

4 Insider Trading, Loan Amendments, and Returns

In this section, we first present the baseline results. Next, we demonstrate the robustness of the results to alternative estimation methods (Section 4.2), alternative measures of insider trading (Section 4.3), alternative definitions for loan amendments (Section 4.4), and potential confounding events (Section 4.5).

4.1 Main Results

In Table 2, we present evidence on the impact of insider trading around a loan renegotiation on future returns. In the first column, we regress monthly returns (in month $t + 1$) on indicator variables for loan amendments and non-routine insider trading, firm-level control variables (all in month t) and past returns. We show that insider sales predict negative stock returns (see, e.g., Cline et al. (2017), or Jagolinzer (2009) specifically for Rule 10b-5-1 trades), and insider purchases predict positive returns (see, e.g., Gao et al. (2022) or Cohen et al. (2012)). We attribute this effect to the fact that insiders, such as directors or officers, are generally better informed about their company compared to other market participants (e.g., Piotroski and Roulstone (2005)) and that their trades reveal relevant information about the firm. The effect is strengthened because we drop routine trades which are expected to be less informative.

In contrast to other corporate events such as share repurchases or SEOs, loan amendments are more opaque and harder to interpret for market participants. They follow the accrual of new information on the borrower's credit quality, investment opportunities, collateral, or macroeconomic fluctuations (e.g., Roberts and Sufi (2009)). The resulting shift in relative bargaining strength between lender and borrower can thus go both ways, depending on whether the information reflects a positive or a negative shock to the borrower. Hence, theory does not provide a clear prediction for the relation between loan amendments and future stock returns. In column 1 of Table 2, we find a positive but not statistically significant coefficient estimate on the loan amendment indicator variable.

In columns 2 to 4, we add the interaction terms of the insider trading and loan amendment indicator variables to the regression specifications. We predict a positive interaction coefficient for insider purchases and a negative interaction coefficient for insider sales. Table 2 reports significantly positive effects on next-month returns for months with insider purchases and loan amendments and significantly negative effects when the amendment is accompanied by insider sales. One interpretation is that insider trading helps classifying loan amendments into three groups. Amendments that are accompanied by purchases (sales) are interpreted as positive (negative) news for shareholders while those with no surrounding insider trading are seen as neutral. The effects are economically significant, especially for the interaction of insider trading and amendments. The monthly return following a month with both insider purchases and a loan announcement is 1.35% higher compared to a month with purchases only (column 4). For months followed by amendments and insider sales, returns are significantly lower with a value of -0.85%. The comparison of months with insider trading and amendments to those with none of these events shows a difference in subsequent returns of 1.68% for purchases and -1.07% for sales, respectively.

We argue that as a consequence of screening and monitoring, lenders are well informed about their borrowers. Similarly, we also expect officers and directors to have an informational advantage over market participants when it comes to their firm. In the lending process, lenders receive confidential information about the borrowing firm (Standard&Poor's (2011)). Since lenders in large corporate loan deals are sophisticated market participants such as banks, institutional investors, and other financial firms, they

Table 2: Insider Trading and Amendments

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) No interaction	(2) Purchases	(3) Sales	(4) Combined
Loan Amendment	0.120	0.015	0.383*	0.268
	(0.728)	(0.088)	(1.835)	(1.251)
Insider Purchase	0.360***	0.324***		0.328***
	(3.478)	(3.086)		(3.130)
Insider Sale	−0.177***		−0.156***	−0.159***
	(−3.867)		(−3.371)	(−3.437)
Ins. Purch. × Amendment		1.406**		1.351**
		(2.042)		(1.968)
Ins. Sale × Amendment			−0.881***	−0.848***
			(−2.735)	(−2.630)
Ln(Market Cap)	−0.107***	−0.114***	−0.109***	−0.108***
	(−6.484)	(−6.911)	(−6.563)	(−6.491)
Ln(Book-to-Market)	0.031	0.036	0.033	0.031
	(0.819)	(0.970)	(0.874)	(0.813)
Curr. Month Return	−0.009***	−0.010***	−0.009***	−0.009***
	(−2.801)	(−2.955)	(−2.890)	(−2.802)
Prev. Year Return	0.001	0.001	0.001	0.001
	(1.382)	(1.107)	(1.253)	(1.381)
Observations	179,651	179,651	179,651	179,651
Adjusted R^2	0.252	0.252	0.252	0.252
Month FE	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Stock return next month

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

have the ability to process the information they receive and form their proper opinion on the borrower's financial situation. This assessment will in turn influence the outcome of loan (re-)negotiations and provide managers with a well-informed outside opinion about their company, similar to information that is contained in share prices.[4] The information complementarity that we report could stem from the fact that lenders and managers focus on different aspects when collecting information about the company. To lenders for example, the most important aspect is the borrowing firm's ability to make loan payments. Therefore, managerial trading around loan amendments might reflect an update on their beliefs about the company's default probability. Since regular market participants cannot participate in the renegotiation, and outcomes of loan amendments are difficult to interpret, it seems plausible that market participants rely on additional sources to assess whether the outcome is positive or negative for firm value. We argue that insider trading around loan amendments is a channel through which information about the renegotiation flows from lenders to market participants.

The firm-specific control variables in all four specifications have the expected sign. Firms with higher market capitalization display lower future returns. Similarly, value firms (companies with a higher B/M Ratio) have on average higher returns than growth firms, although this result is not statistically significant. We also include past returns in the baseline model. The coefficient of the return of the month in which we observe trading and amendments is statistically significant and negative, indicating short term reversal. The effect of past-year returns in our sample is neither statistically nor economically meaningful.[5]

4.2 Robustness to Estimation Method

Our baseline model includes month fixed effects (to account for common shocks to all firms), and standard errors are clustered at the firm-level (to account for time-series dependence in the variables). In this section, we show that our results are robust to several variations of the baseline specification. First, we double-cluster standard errors by firm and months. Column 1 in Table 3 shows the results. We find that the standard errors are largely unchanged. The interaction terms for both purchases and sales remain statistically significant at the 95% level. In column 2, we estimate the baseline model and include firm fixed effects to account for time-invariant firm heterogeneity. Again, our baseline results remain unaffected. In the last two columns of Table 3, we estimate Fama and MacBeth (1973) regressions.

Column 3 uses non-adjusted standard errors, and in column 4 we use Newey and West (1987) stan-

[4]For more information on learning from share prices, see, e.g., Foucault and Frésard (2014).

[5]In the appendix (Table A2), we specify returns as in Cohen et al. (2012), using the return of the month preceding the event month and then the eleven months before. This change in specification has no material impact on any of our results (unreported for other specifications).

Table 3: Robustness to Estimation Method

This table reports regressions of monthly returns on indicators for non-routine insider trading (based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Column 1 presents the baseline specification with standard errors clustered by both firm and month. Column 2 includes firm and month fixed effects with standard errors clustered by firm. Columns 3 and 4 report Fama and MacBeth (1973) regressions. Column 4 reports Newey and West (1987) standard errors with a lag of six months.

	(1) Cluster	(2) Firm FE	(3) Fama-MacBeth	(4) FMB 6m lag
Loan Amendment	0.268	0.290	0.320	0.320
	(1.233)	(1.360)	(1.182)	(1.351)
Insider Purchase	0.328^{**}	0.294^{***}	0.346^{***}	0.346^{**}
	(2.231)	(2.738)	(2.922)	(2.385)
Insider Sale	-0.159^{***}	-0.194^{***}	-0.147^{**}	-0.147^{***}
	(−2.665)	(−3.896)	(−2.580)	(−2.855)
Ins. Purch. × Amendment	1.351^{**}	1.478^{**}	1.003^{**}	1.003^{**}
	(1.985)	(2.124)	(2.336)	(2.112)
Ins. Sale × Amendment	-0.848^{**}	-0.825^{**}	-0.805^{*}	-0.805^{*}
	(−2.582)	(−2.563)	(−1.823)	(−1.801)
Ln(Market Cap)	-0.108^{**}	-1.994^{***}	-0.110^{**}	-0.110^{**}
	(−2.169)	(−20.249)	(−2.251)	(−2.022)
Ln(Book-to-Market)	0.031	−0.016	0.021	0.021
	(0.385)	(−0.206)	(0.359)	(0.306)
Curr. Month Return	−0.009	-0.009^{***}	-0.012^{*}	-0.012^{**}
	(−0.943)	(−2.647)	(−1.676)	(−2.161)
Prev. Year Return	0.001	0.002^{**}	−0.001	−0.001
	(0.433)	(2.322)	(−0.315)	(−0.257)
Observations	179,651	179,649	179,651	179,651
Adjusted R^2	0.252	0.261		
Month FE	*Yes*	*Yes*	–	–
Firm FE	*No*	*Yes*	–	–

t statistics in parentheses

Dependent variable: Stock return next month

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

dard errors with a lag of six months. In both columns, we confirm the main result. The interaction term between amendments and insider purchases is positive and statistically significant, and the interaction term between amendments and insider sales is negative and statistically significant. Overall, our main result is robust to changes in the main regression specification and to alternative estimation methods.

4.3 Robustness to Insider Trading Measure

Our main results are based on an indicator variable that captures months with non-routine insider trading. A potential caveat of this approach is that we cannot rule out that our results might be driven by small transactions. In this section, we therefore complement the main results by using measures that incorporate the USD value of the transactions and the number of trades. Specifically, we show that our findings hold when we use Lakonishok and Lee (2001)'s NPR calculated with transaction value and number of trades (see Section 3.2.2 for the definition). In column 1 of Table 4, we use the continuous NPR based on transaction values. A higher NPR (i.e., higher net value of purchases) leads to significantly higher future returns. This relation intensifies for firm-months with a loan amendment. The effect is almost identical in strength and statistical significance for the net number of purchase transactions (see column 3). Overall, the findings of this analysis help to mitigate concerns that our results are driven by small transactions.

The NPR's downside is that it does not allow for the distinction of net purchase and net sale months. To alleviate this issue, we construct indicators for strict net purchase (sale) months (i.e., NPR above (below) zero). The results of this analysis are presented in columns 2 and 4 of Table 4. Returns are significantly lower after net sale months. This effect is even stronger for the interaction with loan amendments. Interestingly, when using this more granular approach, the coefficient for loan amendments is no longer significant, which coincides with our main results. In sum, this additional analysis confirms our finding of information complementarity between the announcement of bank loan amendments and insider trading. More purchases than sales during the period surrounding the loan amendment sends a stronger positive signal as compared to other months.

4.4 Robustness to Loan Events

Because we are using loan events that are classified by SDC Platinum, the results of our analysis depend on the classification's accuracy. We perform two tests to check the robustness of our results to alternative classifications. First, we complement our sample by announcements of amendments that we determine based on strings of notes in SDC.[6] Column 1 of Table 5 presents the results. Our main results remain

[6]We used the following list of terms in searching through SDC Platinum's *purposenotes* and *amendmentnotes*: Strings containing "AMENDS AND RESTATES", "A&R", "AMENDMENT AND RESTATEMENT",

Table 4: Robustness to Insider Trading Measure

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Net Purchase Ratio* is the ratio of the difference and the sum of the transaction value of purchases and sales (columns 1 & 2) or the number of purchase and sale transactions (columns 3 & 4) (see Formula 1). The division between the columns is the same for all NPR-related variables in this table. *Positive (Negative) Net Purchase Ratio* is an indicator with value one for firm-months with a strictly positive (negative) NPR, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) NPR value	(2) NPR value	(3) NPR trades	(4) NPR trades
Loan Amendment	0.380^{*}	0.254	0.369^{*}	0.249
	(1.924)	(1.110)	(1.873)	(1.096)
Net Purchase Ratio	0.174^{***}		0.173^{***}	
	(4.009)		(3.977)	
Positive Net Purchase Ratio		0.378^{***}		0.435^{***}
		(2.954)		(3.371)
Negative Net Purchase Ratio		-0.097^{**}		-0.094^{**}
		(-2.059)		(-1.998)
NPR × Amendment	0.959^{***}		0.932^{***}	
	(3.391)		(3.264)	
Positive NPR × Amendment		1.660^{**}		1.509^{*}
		(2.022)		(1.815)
Negative NPR × Amendment		-0.714^{**}		-0.675^{**}
		(-2.196)		(-2.073)
Ln(Market Cap)	-0.105^{***}	-0.108^{***}	-0.106^{***}	-0.108^{***}
	(-6.345)	(-6.511)	(-6.368)	(-6.503)
Ln(Book-to-Market)	0.029	0.031	0.029	0.031
	(0.762)	(0.809)	(0.767)	(0.809)
Curr. Month Return	-0.009^{***}	-0.009^{***}	-0.009^{***}	-0.009^{***}
	(-2.827)	(-2.845)	(-2.826)	(-2.828)
Prev. Year Return	0.001	0.001	0.001	0.001
	(1.370)	(1.332)	(1.359)	(1.331)
Observations	179,651	179,651	179,651	179,651
Adjusted R^2	0.252	0.252	0.252	0.252
Month FE	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Stock return next month

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

similar in magnitude and significance with this alternative classification.

Second, we use the publicly available sample of loans from Roberts (2015). Roberts (2015) uses data on loan origination, renegotiation, and termination for a sample of 114 firms that are present in the Compustat and Dealscan databases. The data are manually collected from SEC filings. We merge these data with our sample and end up with a considerably smaller sample because it only represents the months with an active loan by the intersection of Roberts' firms with the Thomson Reuters Insider Trading database. Furthermore, the sample period ends in 2011, thus excluding a large fraction of our original sample. We use events classified by Roberts either as *Amendment* or *Amended & Restated*. Column 2 of Table 5 presents the results. Insider sales do not significantly predict future returns in this smaller subsample, and the coefficient's sign switches. Our main focus, however, is on the interaction term with the loan amendment indicator variable. The interaction of the amendment indicator with insider sales is negative and statistically significant. The interaction of insider purchases and the amendment indicator is positive and similar to the baseline result in magnitude, but the effect is not statistically significant. The weaker statistical significance of the interaction between loan amendment and purchases is likely due to the smaller sample size. Overall, this subsection confirms our main result using alternative definitions of loan amendments.

4.5 Robustness to Confounding Events and Timing

A potential concern with our analysis so far could be that the results are either contaminated by other events or that renegotiation and insider trading are influenced by past stock return behavior. For example, firms could be more inclined to renegotiate a loan following an increase in the stock price, since it would strengthen their position in talks with the lenders.[7] We address these concerns by lagging all explanatory variables by one month and by controlling for year-month fixed effects and past returns. Hence, at the time when insider trading and amendments happen, the next month's return is unknown and can therefore not influence current behavior.

The lagging of the explanatory variables alleviates concerns of reverse causality. However, two other issues remain. First, since we observe amendments and trading in calendar month t and the returns in the following month $t+1$, we rely on the fact that market participants take some time to fully incorporate the information revealed by the combination of insider trading and loan amendments. If these events were incorporated into stock prices within hours or within a few days, they would only have an impact on

"AMENDED AND RESTATED", "AMEND" and "RESTATE" identify *amended & restated* loans. Strings containing "AMDT", "REPRICING", "AMEND" but not "RESTATE", and none of the above identify an *amended* loan.

[7] In unreported tests, we find that the distribution of returns in the year preceding loan amendments is identical to the returns before the other firm-months in the sample.

Table 5: Robustness to Loan Events

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. In column 1, loan amendments are identified based on strings of notes in SDC. In column 2, loan amendments are classified by Roberts (2015) as *Amendment* or *Amended & Restated*. The sample period is Jan 2001 to Dec 2020 in column 1 and Jan 2001 to Jul 2011 in column 2. Standard errors are clustered by firm.

	(1) Strings SDC	(2) Roberts (2015)
Loan Amendment	0.251	−0.061
	(1.182)	(−0.097)
Insider Purchase	0.330***	1.666**
	(3.141)	(2.043)
Insider Sale	−0.159***	0.351
	(−3.436)	(1.357)
Ins. Purch. × Amendment	1.241*	1.033
	(1.858)	(0.723)
Ins. Sale × Amendment	−0.817**	−2.154**
	(−2.556)	(−2.044)
Ln(Market Cap)	−0.108***	−0.364***
	(−6.495)	(−3.183)
Ln(Book-to-Market)	0.031	0.756***
	(0.813)	(2.949)
Curr. Month Return	−0.009***	−0.022
	(−2.801)	(−1.575)
Prev. Year Return	0.001	0.001
	(1.381)	(0.164)
Observations	179,651	6,642
Adjusted R^2	0.252	0.240
Month FE	*Yes*	*Yes*

t statistics in parentheses
Dependent variable: Stock return next month
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

next month's returns if they happen towards the end of the calendar month. We examine this question in more detail in Section 5.6.

Second, the lag does not exclude the possibility that confounding events, such as earning announcements, contaminate our results. To account for this possibility, we exclude all months in which a Quarterly Earnings Announcement (QEA) takes place.[8] In this analysis, we eliminate the possible contaminating effects of earnings announcements. We pick QEAs because they take place regularly and can cause significant stock price reactions. If these announcements were driving our results, we should not find significant effects after excluding announcement months. However, this is not the case. Our results in Table A3 of the appendix are largely unaffected in both statistical and economic significance. In addition, we conduct a simple placebo test by interacting our insider trading variables with a QEAs indicator variable (column 3 of Table A3). These interaction terms are unrelated with next month returns. While we cannot completely rule out the possibility that other categories of corporate events influence our results, we believe the odds to be small.

5 Analysis of the Economic Channel

So far, we have provided strong evidence that insider trading around loan amendments carries incremental and valuable information to market participants and affects firms' stock prices. However, this incremental information likely varies depending on firm, lender, or loan amendment characteristics. In this section, we perform additional analyses to dissect the economic mechanism and to provide further support for the interpretation of the results. First, we exploit the heterogeneity of firm characteristics in our sample. Specifically, we split the sample based on credit ratings (Section 5.1), the distance to default (Section 5.2), and stock illiquidity (Section 5.3). Second, we exploit the heterogeneity in lender quality (Section 5.4) and the relationship with the borrower (Section 5.5). Finally, we analyze the impact of the timing of loan amendment events within the calendar month (Section 5.6) and document the persistence of the effect on returns over time (Section 5.7).

5.1 Debt Rating

Given that banks care about firms' financial health, we expect that incremental information from loan amendments is especially important when default is a realistic scenario for the firm, or when the available information about firms' debt is relatively scarce. Credit ratings provide an easily observable measure of firms' default risk (see, e.g., Standard&Poor's (2011)). We therefore split our sample into two groups.

[8]The announcement dates are drawn from the Institutional Brokers' Estimate System (I/B/E/S).

The first group contains firms with an investment grade rating by S&P (i.e., credit ratings of BBB or higher). The second group contains firms with a rating below BBB or with no rating at all. We expect the interaction terms between loan amendments and insider trading to be especially significant for firms with a low rating or with no rating. Table 6 presents the results.

Consistent with our hypothesis, we find that insider trading around loan amendments is associated with significant stock returns only in the group of non-investment grade or non-rated firms (column 3). As in our baseline results (column 1), the negative effect of insider sales in amendment months on subsequent stock returns exceed the effect of sales in other months. We observe the same effect, but in the opposite direction, for insider purchases. For investment grade firms (column 2), the interaction terms are not statistically significant.

We use Seemingly Unrelated Regressions (SUR) to test whether the differences in the coefficient estimates between the two subsamples are statistically significant. For insider sales, the interaction terms between columns 2 and 3 are significantly different with a p-value of 0.017. For inside purchase, the null hypothesis of equal coefficient estimates cannot be rejected (p-value of 0.812). These findings suggest that for outside investors, insider trading around loan amendments can constitute an update on firms' credit risk, in particular when insiders sell company shares around the loan amendment. Inspecting the other coefficients, we observe that for non-investment grade firms, the announcement of the loan amendment itself has a positive effect on stock returns. In addition, insider trading only predicts future returns in the non-investment grade group. These firms tend to be smaller and are followed less closely by analysts and credit rating agencies.

5.2 Distance to Default

As an alternative measure for firms' financial health, we estimate the distance to default using Bharath and Shumway (2008)'s naïve approach. This measure has the additional advantage that it can be calculated for all firms in our sample, even those for which there is no rating available from S&P. We split the sample at the median of the distance to default. As for the analysis on credit ratings, we expect that the lenders' private information about the loan amendment is more valuable when it concerns firms that are closer to defaulting on their debt payments. Table 7 presents the results.

The results in columns 2 and 3 confirm this hypothesis and the results from Section 5.1. For firms that are closer to default (column 2, distance to default below or at the monthly sample median), loan amendments have a positive effect on stock return. Moreover, insider purchases predict future returns significantly, while the coefficient on insider sales is negative but not statistically significant. Importantly, however, the interaction term between insider sales and loan amendments is negative and

Table 6: Loan Amendments, Insider Trading, and Credit Ratings

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t + 1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample is split between firm-months with an investment grade rating by S&P, and firm-months with a rating below investment grade or without rating. Column 1 presents the benchmark specification from Table 2. Column 2 reports the results for investment grade firms. Column 3 shows the results non-investment grade firms. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Baseline	(2) Investment Grade	(3) Not Investment Grade
Loan Amendment	0.268	−0.326	0.471*
	(1.251)	(−1.382)	(1.673)
Insider Purchase	0.328***	−0.092	0.487***
	(3.130)	(−0.722)	(3.485)
Insider Sale	−0.159***	−0.056	−0.164**
	(−3.437)	(−1.038)	(−2.459)
Ins. Purch. × Amendment (i)	1.351**	1.262	1.564*
	(1.968)	(1.359)	(1.790)
Ins. Sale × Amendment (ii)	−0.848***	0.099	−1.294***
	(−2.630)	(0.268)	(−2.910)
Ln(Market Cap)	−0.108***	−0.135***	−0.143***
	(−6.491)	(−5.316)	(−4.676)
Ln(Book-to-Market)	0.031	−0.161***	0.082
	(0.813)	(−3.695)	(1.523)
Curr. Month Return	−0.009***	−0.034***	−0.005
	(−2.802)	(−6.026)	(−1.302)
Prev. Year Return	0.001	−0.001	0.002**
	(1.381)	(−0.725)	(2.267)
Observations	179,651	58,042	121,609
Adjusted R^2	0.252	0.300	0.254
Month FE	*Yes*	*Yes*	*Yes*
(i) P-value of diff. (2) & (3)	–	0.812	–
(ii) P-value of diff. (2) & (3)	–	0.017	–

t statistics in parentheses

Dependent variable: Stock return next month

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

Table 7: Loan Amendments, Insider Trading, and Distance to Default

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. Column 1 presents the benchmark specification from Table 2. Column 2 contains results for firm-months below or at the median of distance to default. Column 3 presents results for firm-months above the median. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Baseline	(2) Close to Default	(3) Far from Default
Loan Amendment	0.268	0.415	0.042
	(1.251)	(1.361)	(0.164)
Insider Purchase	0.328^{***}	0.363^{**}	0.138
	(3.130)	(2.446)	(1.191)
Insider Sale	-0.159^{***}	-0.127	-0.152^{***}
	(-3.437)	(-1.608)	(-2.968)
Ins. Purch. × Amendment (i)	1.351^{**}	1.384	1.262
	(1.968)	(1.622)	(1.241)
Ins. Sale × Amendment (ii)	-0.848^{***}	-1.429^{***}	-0.221
	(-2.630)	(-2.816)	(-0.624)
Ln(Market Cap)	-0.108^{***}	-0.122^{***}	-0.102^{***}
	(-6.491)	(-4.684)	(-5.245)
Ln(Book-to-Market)	0.031	0.114^{*}	-0.148^{***}
	(0.813)	(1.940)	(-3.892)
Curr. Month Return	-0.009^{***}	-0.006	-0.026^{***}
	(-2.802)	(-1.518)	(-5.638)
Prev. Year Return	0.001	0.003^{***}	-0.002
	(1.381)	(3.031)	(-1.428)
Observations	179,651	98,072	81,579
Adjusted R^2	0.252	0.269	0.268
Month FE	*Yes*	*Yes*	*Yes*
(i) P-value of diff. (2) & (3)	–	0.926	–
(ii) P-value of diff. (2) & (3)	–	0.045	–

t statistics in parentheses

Dependent variable: Stock return next month

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

statistically significant with a value of -1.429, supporting the interpretation that insider sales around loan amendments carry important information for market participants. The interaction term between insider purchases and loan amendments is positive but not statistically significant, suggesting that insider purchases are less important for market participants in this case. For firms far away from default, neither interaction term is statistically significant. The difference in the sales interaction between columns 2 and 3 is statistically significant with a p-value of 0.045.

Overall, Sections 5.1 and 5.2 provide evidence that debt-related events, such as a renegotiation, have a stronger effect when default is a possible scenario. In addition, insider trading around such events are especially informative when insiders sell company shares. These findings are consistent with the hypothesis that insider trades are a channel through which lenders' private information about the financial health of the firm affects asset prices.

5.3 Stock Illiquidity

A lower stock market liquidity is generally associated with less informative stock prices (see, e.g., Bai et al. (2016)). Hence, we expect the prices of less liquid stocks to contain less information about the financial health of a firm. We hypothesize that the private information that is revealed through insider trading around loan amendments has a larger impact on the prices of stocks that are less liquid. To test this hypothesis, we use the illiquidity measure introduced by Fong et al. (2017) (FHT), which measures the ratio of zero-return days to trading days per firm-month, as a proxy for stock liquidity. A higher value of the FHT-measure implies lower liquidity. Our sample contains quite liquid stocks, as the median firm-month does not contain any zero-return days. In a first step, we include the measure as an additional control variable in our baseline model to see if stock liquidity is systematically related to next month returns in our sample. Table 8 (column 1) shows that higher illiquidity is associated with higher future returns. The effect, however, is economically small and statistically not significant.

Next, we divide our sample into a group of firm-months with zero-return trading days (see column 2 of Table 8) and a group of firm-months without zero-return trading days (column 3). Strikingly, the effect of insider trading around loan amendments on stock returns is statistically and economically significant only in the group of relatively illiquid stocks. In column 2, the interaction term of insider purchases (sales) and loan amendments has a value of 3.254 (-3.268). The differences in the coefficient estimates between the two subsamples are statistically significant with p-values of 0.000 and 0.097 for the sales and the purchase interaction, respectively. Moreover, loan amendments are associated with significantly higher future returns in the group of illiquid stocks. While insider trading by itself has the expected predictive power across both groups, the effect is stronger when liquidity is lower. This

Table 8: Loan Amendments, Insider Trading, and Illiquidity

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Illiquidity Measure (FHT (2017))* is the illiquidity measure by Fong et al. (2017), calculated on a monthly basis. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. In column 2 (3), only firm-months with a positive (zero) FHT-illiquidity measure are used. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Illiquidity Measure	(2) Illiquid	(3) Liquid
Illiquidity Measure (FHT (2017))	0.099		
	(0.671)		
Loan Amendment	0.327	1.398***	−0.010
	(1.508)	(2.706)	(−0.043)
Insider Purchase	0.355***	0.802***	0.211*
	(3.332)	(3.602)	(1.811)
Insider Sale	−0.159***	−0.216**	−0.142***
	(−3.423)	(−1.989)	(−2.735)
Ins. Purch. × Amendment (*i*)	1.263*	3.254**	0.515
	(1.815)	(2.237)	(0.670)
Ins. Sale × Amendment (*ii*)	−0.921***	−3.268***	−0.232
	(−2.830)	(−4.197)	(−0.665)
Ln(Market Cap)	−0.099***	−0.053	−0.121***
	(−5.888)	(−1.505)	(−6.471)
Ln(Book-to-Market)	0.020	0.243***	−0.045
	(0.536)	(2.818)	(−1.108)
Curr. Month Return	−0.010***	−0.004	−0.013***
	(−2.973)	(−0.575)	(−3.366)
Prev. Year Return	0.001	0.005***	−0.000
	(1.203)	(2.651)	(−0.410)
Observations	173,953	39,697	134,256
Adjusted R^2	0.253	0.221	0.268
Month FE	*Yes*	*Yes*	*Yes*
(*i*) P-value of diff. (2) & (3)	–	0.097	–
(*ii*) P-value of diff. (2) & (3)	–	0.000	–

t statistics in parentheses
Dependent variable: Stock return next month
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

evidence is consistent with our expectation that incremental information about loan renegotiation has a stronger effect in stocks whose prices tend to contain less information prior to the renegotiation.

5.4 Lender Quality

Besides borrower and loan characteristics, the outcomes that we document may also depend on the lender. Banks play a crucial role in the information production along the lending process by screening potential borrowers and by monitoring borrowers while the loan is outstanding. The heterogeneity in bank quality influences the amount and precision of information produced during the analysis of a borrower. Specifically, higher-quality banks are able to produce more valuable information. According to Ross (2010, p.2731), the three biggest lenders in the U.S. syndicated loan market (JP Morgan, Bank of America, Citi) have "particularly good reputation for evaluating a borrower's underlying business and true risk for default, both initially (screening) and while the loan is outstanding (monitoring)". Therefore, we expect that insiders of borrowers can profit from the more insightful outside view on their company during loan (re-)negotiations. This, in turn, should make their insider trades more informative for market participants and lead to a stronger effect on stock returns.

Building on Ross (2010), we create a continuous measure of lender quality as follows. From SDC Platinum, we obtain annual league tables based on the USD amount lent for the top 50 lenders (covering 97.67 - 100% of loans in SDC, depending on the year). Based on these loans, we calculate for each amendment the average rank of all book runners. If this value is below (i.e. better) or at the median, the amendment is classified as high-ranking, otherwise as low-ranking [9]. Analogous to our main analysis, we only consider firm-months in which a loan is outstanding such that there can be an amendment.[10] We present the results of this analysis in Table 9.

We find that when book runners' mean league table ranking is better, the insider trades accompanying such amendments are indeed more predictive for future returns, which supports our suggested mechanism. When less successful lenders participate in the loan, the interaction term has the expected sign but is not statistically significant.

[9] As an example, consider a loan that was granted in 2020 by Bank of America (highest ranking) and JP Morgan (2[nd]) as sole book runners. This loan has an average score of 1.5 and is classified as high-ranking since the score is below median.

[10] Note that since a firm can have a high- and a low-lender loan outstanding at the same time, there is a certain overlap between the two subsamples.

Table 9: Loan Amendments, Insider Trading, and Lender Quality

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Amendment by higher-(lower-)ranking Banks* is an indicator variable equal to one for firm-months in which a loan amendment by lenders whose average league table rank is better than or at (worse than) the median of ranks takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. Column 1 contains results for amendments where the lenders on average rank better than or at the median. Column 2 presents results for amendments by lenders ranked worse on average. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) High-ranking Banks	(2) Low-ranking Banks
Amendment by higher-ranking Banks	0.196	
	(0.725)	
Amendment by lower-ranking Banks		0.345
		(0.952)
Insider Purchase	0.335^{***}	0.277^{**}
	(3.158)	(2.174)
Insider Sale	-0.168^{***}	-0.145^{**}
	(-3.586)	(-2.517)
Ins. Purch. × Amend. high-ranking (*i*)	1.593^{*}	
	(1.911)	
Ins. Sale × Amend. high-ranking (*ii*)	-0.882^{**}	
	(-2.294)	
Ins. Purch. × Amend. low-ranking (*i*)		0.922
		(0.794)
Ins. Sale × Amend. low-ranking (*ii*)		-0.736
		(-1.193)
Ln(Market Cap)	-0.118^{***}	-0.137^{***}
	(-6.990)	(-6.765)
Ln(Book-to-Market)	0.026	-0.002
	(0.672)	(-0.053)
Curr. Month Return	-0.009^{**}	-0.007^{*}
	(-2.533)	(-1.845)
Prev. Year Return	0.001	0.001
	(1.008)	(0.872)
Observations	169,171	118,249
Adjusted R^2	0.256	0.254
Month FE	*Yes*	*Yes*
(*i*) P-value of diff. (1) & (2)	0.637	–
(*ii*) P-value of diff. (1) & (2)	0.843	–

t statistics in parentheses

Dependent variable: Stock return next month

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

5.5 Loan Amendments versus Loan Originations

Existing literature shows that lenders gain additional information about borrowers over the duration of the lending process (see, e.g., Botsch and Vanasco (2019)[11]). Similar to the effect that lender quality has on the information production, we expect banks to be better informed about a borrower after repeated interactions. This improved information is likely to spill over to corporate insiders, such that insider trades transmit a higher amount of information to market participants.

To test this prediction, we compare insider trading around loan originations with insider trading around loan amendments. Amendments represent outcomes of negotiations during an existing lending relationship in which lenders and borrowers are familiar with each other, whereas in new loan originations, the lenders have acquired comparatively less private information about the borrower. To capture events with less informed banks, we only consider new loan originations and exclude loan refinancing because refinancing is based on previous interactions, similar to amendments. We construct an indicator variable for loan originations based on the variables *loanpurpose* and *purposenotes* from SDC Platinum.[12] The results of this analysis are documented in Table 10.

Consistent with our prediction, the market reacts significantly only to amendments accompanied by insider trades, but not to loan originations or the interaction of originations with trades. The interaction terms of amendments and originations with insider sales differ significantly with a p-value of 0.099. The direction of insider trades predicts the direction of the interaction effect for both categories of events, but for origination it is not statistically significant. This finding is consistent with managerial learning from banks in relationships with multiple interactions. Managers then pass this information on to market participants through insider trading, leading to significant changes in stock prices.

The distinction of events by borrower-lender-familiarity is a unique trait of our sample of loan events as compared to other corporate events that have been investigated in connection with insider trading in the existing literature. Thus, we are able to produce valuable new insights on the information flow from banks to insiders, and from insiders to the stock market.

5.6 Timing of Amendment

Measuring returns in the month following the amendments is a valid approach if it takes some time for the market to incorporate the amendments' full information. To the best of our knowledge, the time span it takes for loan amendments to be reflected in the stock price has not been examined, especially

[11] For more literature on this topic, check, e.g., Mester et al. (2007), Agarwal and Hauswald (2010), Norden and Weber (2010), Gustafson et al. (2021).

[12] We search for terms such as "Refinancing" and "Refin/Ret Bank Debt". If the notes contain these terms, the observation is classified as loan refinancing and is excluded from the loan origination variable.

Table 10: Comparison of Loan Amendments and Originations

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments (originations) in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment (Origination)* is an indicator variable equal to one for firm-months in which a loan amendment (origination) takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Amendments	(2) Originations
Loan Amendment	0.268	
	(1.251)	
Loan Origination		−0.308
		(−1.254)
Insider Purchase	0.328***	0.351***
	(3.130)	(3.382)
Insider Sale	−0.159***	−0.177***
	(−3.437)	(−3.827)
Ins. Purch. × Amendment (i)	1.351**	
	(1.968)	
Ins. Sale × Amendment (ii)	−0.848***	
	(−2.630)	
Purch. × Origination (i)		0.625
		(0.820)
Sale × Origination (ii)		−0.024
		(−0.064)
Ln(Market Cap)	−0.108***	−0.107***
	(−6.491)	(−6.435)
Ln(Book-to-Market)	0.031	0.031
	(0.813)	(0.824)
Curr. Month Return	−0.009***	−0.009***
	(−2.802)	(−2.803)
Prev. Year Return	0.001	0.001
	(1.381)	(1.380)
Observations	179,651	179,651
Adjusted R^2	0.252	0.252
Month FE	*Yes*	*Yes*
(i) P-value of diff. (1) & (2)	0.479	–
(ii) P-value of diff. (1) & (2)	0.099	–

t statistics in parentheses

Dependent variable: Stock return next month

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

when they are coupled with insider trading. However, there is some evidence related to other corporate events. Neuhierl et al. (2013), for example, find that the bulk of the effect of earnings announcements is incorporated into the prices of stocks after three trading days. For other events, it may take longer. Thus, it seems reasonable to assume that loan amendments, which are less standardized and more difficult to interpret, take more time to be incorporated compared to earnings announcements, which are standardized and more broadly discussed in the market.

Building on this evidence, we analyze the effects of loan amendments and insider trading on next month's stock that happen earlier or later during a month. Specifically, we compare amendments taking place in the first half of the month with those happening either in the second half or during the last few days of the month (i.e., those on or after the 25^{th} day). If the information is processed quickly, we should observe a significant effect for amendments happening in the last days of the month. Table 11 presents the results.

The results in Table 11 suggest that our main findings are mostly driven by amendments that are announced later in the calendar month. In column 1, we compare amendments published in the first half of the month with those published in the second half of the month. We find that the interactions with both sales and purchases are only statistically and economically significant for amendments published in the second half. In column 2, we show that the reaction is similar if the amendment is announced late in the month (i.e., after the 25^{th}). Finally, in column 3, we compare amendments happening up to the 25^{th} of a month to those happening in the subsequent days. The interaction terms for the "late" announcements are largely unchanged, but we additionally find a significantly negative interaction effect for earlier amendments for non-routine insider sales. These results suggest that it takes the market some time to digest the signal that insiders' trading activity around loan amendments sends.

Overall, the results confirm our main results and suggest non-routine insider trades provide valuable private information of lenders and insiders to market participants about the outcome of loan renegotiations and the future financial prospects of firms.

5.7 Long-term Effect on Returns

Our analyses up to this point suggest that the effect holds over the month following the amendment and insider trading and is therefore not merely a brief overreaction to an event. To further substantiate this finding, we expand the horizon over which we measure future returns. We show the results for three- and six-month returns in Table 12. We choose a six-month period because of the SEC's short-swing profit rule which stipulates that insiders have to forfeit profits based on purchase and sale trades within less than six months.

Table 11: Amendment Timing within Calendar Month

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Amend. in 1st (2nd) half of month* and *Amend. after 25th (bef. 26th)* are indicator variables equal to one for firm-months in which a loan amendment takes place in the respective period, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Early vs. late	(2) Early vs. end	(3) Cutoff at 25th
Amend. in 1st half of month	0.124 (0.384)	0.123 (0.379)	
Amend. in 2nd half of month	0.356 (1.268)		
Amend. bef. 26th			0.132 (0.539)
Amend. after 25th		0.571 (1.362)	0.572 (1.363)
Insider Purchase	0.328*** (3.131)	0.336*** (3.229)	0.328*** (3.129)
Insider Sale	−0.159*** (−3.440)	−0.166*** (−3.596)	−0.160*** (−3.452)
Purch. × Amend. bef. 16th	0.868 (0.818)	0.877 (0.827)	
Sale × Amend. bef. 16th	−0.422 (−0.855)	−0.420 (−0.850)	
Purch. × Amend. after 15th	1.780** (2.046)		
Sale × Amend. after 15th	−1.146*** (−2.727)		
Purch. × Amend. bef. 26th			0.970 (1.229)
Sale × Amend. bef. 26th			−0.618* (−1.656)
Purch. × Amend. after 25th		2.791** (2.159)	2.800** (2.165)
Sale × Amend. after 25th		−1.373** (−2.197)	−1.375** (−2.200)
Control Variables	*Yes*	*Yes*	*Yes*
	⋮	⋮	⋮
Observations	179,651	179,651	179,651
Adjusted R^2	0.252	0.252	0.252
Month FE	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Stock return next month

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

Table 12: Long-term Effects on Stock Returns

This table reports regressions of one-, three-, and six-month returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$ (from $t+1$ to $t+3$ (2) and $t+6$ (3)), expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Next month	(2) Next three months	(3) Next six months
Loan Amendment	0.268	−0.340	−0.377
	(1.251)	(−0.910)	(−0.681)
Insider Purchase	0.328***	0.749***	1.276***
	(3.130)	(3.750)	(3.931)
Insider Sale	−0.159***	−0.319***	−0.246
	(−3.437)	(−3.103)	(−1.395)
Ins. Purch. × Amendment	1.351**	2.229*	4.637**
	(1.968)	(1.823)	(2.558)
Ins. Sale × Amendment	−0.848***	−0.126	−1.013
	(−2.630)	(−0.207)	(−1.163)
Ln(Market Cap)	−0.108***	−0.394***	−0.889***
	(−6.491)	(−7.876)	(−8.678)
Ln(Book-to-Market)	0.031	0.086	0.347
	(0.813)	(0.762)	(1.495)
Curr. Month Return	−0.009***	−0.001	−0.013
	(−2.802)	(−0.175)	(−1.376)
Prev. Year Return	0.001	−0.002	−0.010**
	(1.381)	(−1.064)	(−2.380)
Observations	179,651	178,747	177,347
Adjusted R^2	0.252	0.266	0.280
Month FE	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Stock return next month (1) ; next three months (2) ; next six months (3)

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

Consistent with the portfolio analysis of Cohen et al. (2012), we find that the predictive ability of non-routine insider sales fades out over time. The effect of the interaction term weakens together with the sales and is no longer statistically significant after more than one month. For purchases, however, we can document a longer-lasting effect which is persistent over the three- and six-month period following the event month. This evidence is consistent with the hypothesis that private information about a renegotiation outcome has a long-lasting and economically meaningful effect on firm valuation.

6 Conclusion

We analyze how and through which channel relevant information about a borrower is revealed through loan renegotiations and affects stock returns. Specifically, banks are sophisticated financial market participants with private information about borrowers that they use in a renegotiation. Corporate insiders, such as the firm's top management, are involved in the renegotiation process and obtain superior knowledge about whether the loan amendment is positive or negative for the firm. Hence, we hypothesize that the trades of corporate insiders around loan amendments inform market participants about the firm's future prospects. Therefore, loan amendments that are accompanied by inside stock purchases (sales) predict higher (lower) stock returns. This effect should be more pronounced for firms closer to default, when stocks are more illiquid, and when the lender has a high quality.

We test these predictions using a large sample of loan amendments of U.S. firms between 2001 and 2020. We find that insider purchases (sales) executed in the same month as the loan amendment predict higher (lower) stock returns in the following month. This result is robust to the estimation method, alternative definitions of loan amendments and insider trading, and to confounding events such as earnings announcements. In further analyses, we show that the effect is stronger for firms with low credit rating, close to default, with illiquid stocks, or when a high quality lender is involved in the renegotiation. Our findings suggest that insider trades are a channel through which lenders' information about the financial health of a borrower affects asset prices.

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Appendix

Table A1: Definitions of variables

Variable name	Variable definition
Loan amendment	Indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise.
Stock return	Monthly stock return, expressed in percent.
Insider purchase	Indicator variable equal to one if there are any non-routine insider purchases happening in the firm-month, and zero otherwise.
Insider sale	Indicator variable equal to one if there are any non-routine insider sale happening in the firm-month, and zero otherwise.
Ln(Market Cap)	Natural logarithm of market capitalization in millions of USD
Ln(Book-to-Market)	Natural logarithm of the B/M Ratio, capped at zero and 100.
Curr. Month Return	Monthly return in month t in which trading and amendments are observed (event month t), expressed in percent.
Prev. Year Return	Return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent.
Net Purchase Ratio (NPR)	Difference of transaction values (numbers) of purchases and sales divided by the sum of transaction values (numbers) of purchases and sales
Positive (Negative) NPR	Indicator variable equal to one for firm-months with a strictly positive (negative) NPR, and zero otherwise.
Rated	Indicator variable equal to one if S&P domestic long term issuer credit rating available, and zero otherwise.
Distance to Default	Naïve distance to default following Bharath and Shumway (2008).
Spread	Loan spread above the base rate.
Tranche Amount	Loan amount in millions of USD.
Illiquidity Measure	Ratio of zero-return days to trading days per firm-month, following Fong et al. (2017).
Month with QEA	Indicator variable equal to one for firm-months with a QEA, and zero otherwise.
High-(low-)ranking Banks	Indicator variable equal to one for amendments with an average ranking of bookrunners at or below (above) the median rank.
Loan Origination	Indicator variable equal to one for firm-months in which a loan origination takes place, and zero otherwise.

Table A2: Measurement of Past Returns

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Prev. Month Return* is the monthly return in month $t-1$, the month before trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month, and ending after the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1)	(2) Purchases	(3) Sales	(4) Both
Loan Amendment	0.116	0.010	0.378*	0.263
	(0.700)	(0.060)	(1.813)	(1.229)
Insider Purchase	0.367***	0.330***		0.335***
	(3.533)	(3.138)		(3.187)
Insider Sale	−0.182***		−0.161***	−0.164***
	(−3.944)		(−3.446)	(−3.518)
Ins. Purch. × Amendment		1.406**		1.350**
		(2.043)		(1.969)
Ins. Sale × Amendment			−0.881***	−0.848***
			(−2.734)	(−2.629)
Ln(Market Cap)	−0.107***	−0.114***	−0.109***	−0.107***
	(−6.478)	(−6.919)	(−6.558)	(−6.485)
Ln(Book-to-Market)	0.029	0.036	0.032	0.029
	(0.773)	(0.945)	(0.833)	(0.767)
Prev. Month Return	0.001	0.000	0.000	0.001
	(0.218)	(0.033)	(0.047)	(0.221)
Prev. Year Return	0.000	0.000	0.000	0.000
	(0.247)	(0.024)	(0.159)	(0.245)
Observations	179,651	179,651	179,651	179,651
Adjusted R^2	0.252	0.252	0.252	0.252
Month FE	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Stock return next month

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

Table A3: Quarterly Earnings Announcements (QEA)

This table reports regressions of monthly returns on indicators for non-routine insider trading (classification based on Cohen et al. (2012)) and loan amendments in the previous month, and control variables. The dependent variable is the monthly return in $t+1$, expressed in percent. *Loan Amendment* is an indicator variable equal to one for firm-months in which a loan amendment takes place, and zero otherwise. *Month with QEA (QEA)* is an indicator variable equal to one for firm-months with a QEA, and zero otherwise. *Insider Purchase* (*Sale*) is an indicator variable equal to one if there are any non-routine insider purchases (sales) happening in the firm-month, and zero otherwise. *Ln(Market Cap)* is the natural logarithm of market capitalization in millions of USD. *Ln(Book-to-Market)* is the natural logarithm of the B/M Ratio, capped at zero and 100. *Curr. Month Return* is the monthly return in month t in which trading and amendments are observed (event month t), expressed in percent. *Prev. Year Return* is the return of the period starting twelve months before the event month and ending in the month preceding the event month, in percent. The sample period is Jan 2001 to Dec 2020. Standard errors are clustered by firm.

	(1) Baseline	(2) No QEA	(3) Placebo test
Loan Amendment	0.268	0.377	
	(1.251)	(1.447)	
Month with QEA			-0.005
			(-0.081)
Insider Purchase	0.328^{***}	0.340^{**}	0.370^{***}
	(3.130)	(2.455)	(2.704)
Insider Sale	-0.159^{***}	-0.141^{**}	-0.166^{***}
	(-3.437)	(-2.338)	(-2.818)
Ins. Purch. × Amendment	1.351^{**}	1.640^{*}	
	(1.968)	(1.960)	
Ins. Sale × Amendment	-0.848^{***}	-0.906^{**}	
	(-2.630)	(-2.288)	
Insider Purchase × QEA			-0.021
			(-0.104)
Insider Sale × QEA			-0.034
			(-0.371)
Control Variables	*Yes*	*Yes*	*Yes*
	⋮	⋮	⋮
Observations	179,651	123,642	179,651
Adjusted R^2	0.252	0.245	0.252
Month FE	*Yes*	*Yes*	*Yes*

t statistics in parentheses
Dependent variable: Stock return next month
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

How Do Directors and Officers React to Insider Trading in Peer Firms?*

Christian Steiner†

September 27, 2024

Abstract

Directors and officers increase their trading activity (probability and frequency) and the profitability of trades following published insider trades in product-market peer companies. Trading is more likely to happen in the same direction as in peers, which is consistent with a peer effect and with learning from peer trades about industry-wide information. Additional results based on a subsample of peer trades that are most likely influenced by factors orthogonal to the focal firm are consistent with the learning channel, but evidence for peer effects is limited. I find insiders' industry knowledge to be useful to close peer firms only, and that insiders in smaller firms profit from larger firms' information, but not vice versa.

Keywords: Insider Trading, Managerial Learning, Peer Effects, Intra-industry Information Spillovers
JEL Classification Numbers: G14, G32, G40, G50

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1 Introduction

Insider trades are not orthogonal to future returns. Insiders show the ability to sell ahead of downturns and to buy before run-ups more consistently than other investors. The literature documents such contrarian investment behavior extensively and finds that both insider purchases and sales are informative for future returns, especially when they are unexpected either in timing (Cohen et al. (2012)) or in direction (Akbas et al. (2020)). This strongly suggests that corporate insiders have an informational advantage vis-à-vis other protagonists in financial markets. Three questions arise: First, *what type* of information do insiders such as directors or officers possess? Second, given that they have an advantage, but are not perfectly informed, *from what external sources* can they get incremental information on their firm's perspectives? And *how does this information flow* between different agents?

On the first question, the literature shows that insiders are well informed about assets in place (e.g., Goldstein (2023)), but rely on outside information to forecast macroeconomic factors (e.g., Ye et al. (2023)). In between the two poles of the own firm and the entire economy, there is the industry in which the company operates. While Hutton et al. (2012) find industry knowledge of insiders similar to that of analysts covering a certain industry, Ben-David et al. (2019) find that experts such as managers which have been or are still employed in an industry display outperformance in their portfolios when investing in "their" industry. On the second question, most notably, Chen et al. (2007) establish the idea of managerial learning from stock prices. Related work expands this idea to other sources, such as Foucault and Frésard (2012) who find evidence for learning from cross-listings. However, to the best of my knowledge there is a gap in this literature in that the published trades by insiders in close peer firms have been ignored as an information source. Given the amount of information which is applicable industry-wide that these insiders have, it seems reasonable that such trades can be informative to managers in closely related firms. With this paper, my goal is to close this gap. Simultaneously, by investigating the effects of insider trading in peer firms, I attempt to add new insights on information dissemination. Berkman et al. (2020) use interlocked boards as a channel for the exchange of information between insiders. However, the transfer of information—intentional or not—through published trades in peer firms remains unexplored so far.

This paper analyzes the question of whether insider trades in product-market peers have an impact on the occurrence and the profitability of trades by directors and officers in the focal firm. There can be at least two explanations for such an effect. First, an endogenous effect as described by e.g., Manski (1993), where managers trade because their peers did so, and not due to common characteristics or exposure to the same exogenous shocks.[1] Second, a learning effect, where managers draw useful

[1] The endogenous effect is hereafter referred to as peer effect.

information from their peer firms' insider trading. Besides Chen et al. (2007), the idea of managerial learning from sources outside the company in a more general setting is based on Bond et al. (2012). Managers are not perfectly informed about their company and therefore rely on information generated by outsiders to some extent. An illustrative example for possible learning from peer trading took place in November 2021. On 11 November, the general counsel of ITT Inc., a manufacturer of hitech components for transportation, industrial, and energy markets sold company stock worth roughly USD 250'000. The trade was unexpected in that it was not part of a 10b5-1 plan, neither was there a similar selling pattern in the previous years. Around a week later, the chairman of Applied Industrial Technologies (AIT), a smaller competitor in the industrial and energy markets, in a similarly surprising move sold stock in the open market for around USD 500'000. In the end of October, AIT had confirmed its earnings and growth outlook, and I find no other events in this time period that could impact the trading behavior.

A peer effect tends to lead to reactions in the same direction, as in the example above. If there is an information spillover happening from the peer insider trades, it is less clear what reaction to expect. Foster (1981) discusses this issue in the context of earnings announcements. A favorable announcement in company i can in principle have a positive, negative, or no impact on peer firm j. Looking at management forecasts, Kim et al. (2008) refine this mechanism and suggest that within an industry there can be competitive effects—leading to an opposite reaction—or industry-wide effects, from which a reaction in the same direction can be expected. A release of information, no matter if it happens through a new management forecast or the publication of an insider trade, can contain both effects which offset each other.

Based on the mechanisms described above, I hypothesize that if there is a learning effect, insider trading in peer firms leads to an increase in insider trading in the same (opposite) direction in the focal firm if these trades are interpreted as containing more industry-wide applicable (peer-specific) information. Given a peer effect, I expect peer insider trading to lead to an increase in insider trading in the same direction in the focal firm. These reactions are independent of whether the peer trades are noise (e.g., for personal liquidity) or contain useful information. I base this conjecture on the findings of Dessaint et al. (2019) who find that managers reduce investment as a reaction to both fundamental and non-fundamental price drops in peers' stock prices.

I expect that a learning effect will also influence the profitability of focal firm insider trading. Informative insider trades in peer firms should increase this profitability, whereas there should be no effect from noise trades. I want to emphasize that the two channels are not mutually exclusive. For example, the finding of increased profitability following informative trades supports the learning effect. However, it is not evidence against peer effects, for which there is no clear prediction of an effect on profitability.

To confront my hypotheses with the data, I use a sample of U.S. companies from the CRSP/Compustat merged database, from which I also take stock prices and firm fundamentals. I impose that there are Form 4 open market trades available for the company in the Thomson Reuters Insider Trading database and that the company is classified in Hoberg and Phillips (2016)'s Text-based Network Industrial Classification (TNIC) database. Then, I exclude observations from financial and utility companies, and observations that predate the Sarbanes-Oxley Act. The resulting sample spans from August 2002 to December 2021 and contains 4'113 distinct companies. For each sample company, I annually determine the peer group using the TNIC. Based on the methodology developed by Cohen et al. (2012) (CMP), I classify for all firms the open market trades by insiders categorized as Directors and Officers (D&O). For the peer firms, I add bona fide gifts and "other transactions" (to capture in-kind distributions) as well as transactions by other entities which are obligated to file Form 4 with the SEC. For the main analysis, in focal firms I exclude transactions classified as routine[2], while for the peer groups I do not exclude any trades based on the CMP-categorization. Since the TNIC aims at achieving a granularity similar to the three-digit SIC, the median focal firm in my sample has 91 product-market peers. Each sample firm can in principle be included in the study as focal firm and in peer groups. Since corporate insiders normally receive shares directly from the issuer, but sell on the open market, sales outnumber purchases leading to a Net Purchase Ratio (NPR) (see Lakonishok and Lee (2001)) of -0.254 in the average firm-month.

To test the first hypothesis that insider trading in peer firms has an impact on focal firm insider trading, I regress three measures (NPR, trade indicator, log-number of trades) of one-month-ahead focal firm insider trading on the respective current-month peer group insider trading variable. Additionally, I control for market capitalization, B/M Ratio, a proxy for information asymmetry (based on Huddart and Ke (2007)), and an indicator for earnings announcements, all in the current month and in the focal firm. I also include current month and past year returns in the focal firm. The one-month lag between the outcome and the explanatory variables mitigates concerns of reverse causality. The control variables should cover the influence of the most important known determinants of insider trading. With the earnings announcement indicator, I make sure that the trading is not due to confounding events. I include month and focal firm fixed effects. Since in the TNIC, each company forms its own industry together with its peer group, these firm fixed effects should also control for unobserved characteristics of the peer group and common shocks. Month fixed effects control for seasonality effects. Standard errors in all baseline regressions are clustered at the level of the focal firm.

To test the second hypothesis of an effect on insider trading profitability, I use all non-routine insider trades by D&Os in the focal firms, and each day with trades is one observation. I regress six-month-

[2] i.e., I use opportunistic trades and those which cannot be classified because the entity or individual does not trade regularly.

ahead CARs starting from the trading day on indicators for focal and peer firm (group) purchases and sales, and the interactions thereof. I control for firm size, B/M Ratio, information asymmetry, and past returns. I use the same set of fixed effects and the same clustering level as in the first set of results.

First, I show a consistent effect of insider trading in the peer group on insider trading in the focal firm in the next month. The coefficient for the peer-NPR is positive and statistically significant at the 1% level. The magnitude indicates that a one unit change in the peer-NPR—corresponding to a move from an equal USD volume of purchases and sales to having only purchases—is associated with an economically meaningful change of 7% in the focal firm's NPR, relative to its sample mean. I find evidence for an effect on the extensive margin for purchases. Namely, peer purchases predict a higher probability of observing focal firm purchases in the next month. This is consistent with the findings of Fidrmuc et al. (2006). Given that there is trading, more trades in the peer group predict more (fewer) trades in the same (opposite) direction in the focal firm. This effect is statistically significant for all four combinations of peer and focal transactions. These results based on all peer trades support the notions of both a peer effect and learning about industry-wide information.

To help distinguish the two channels, I look at the effect on trading profitability. While finding more profitable insider trades following peer insider trades does not exclude peer effects, it certainly is what one would expect if managers draw new knowledge from such trades. I find a positive effect on abnormal returns if the focal firm insider trade is preceded by peer trading in the same direction. If the trades are in opposite directions, the effect tends to be negative. The effect that is persistently significant across all specifications is the one for the interaction of peer and focal firm purchases. The total effect of the combination of the two types of trades is sizeable at 9.9% over the six-month period. The negative effect of trading in opposite directions has no clear interpretation. It is statistically, but not economically significant. These results are consistent with the idea of managers learning from the information contained in peer insider trades and they indicate that such information is economically relevant for firm valuation. As the effect is measured over six months, it seems to be persistent, and not merely a short-term overreaction of the stock market.

I conduct additional analyses to substantiate my results, starting with firm size. Based on the peer effects literature (e.g., Leary and Roberts (2014)), I expect the results to be driven by insiders in smaller firms reacting to larger peers. The idea that small follows big should also be applicable to the learning channel. I split the sample into observations in which the focal firm has a higher market capitalization than the peers, and vice versa. Indeed, the results are largely driven by smaller focal firms' reactions to trades in bigger peers.

Second, I aim at a clearer characterization of the scope of insiders' knowledge. The literature assigns insiders knowledge on their industry, however there seem to be boundaries to it. I change the peer group from the TNIC product market peers, for which I assume that insiders' industry knowledge is more applicable, to the four-digit SIC peers (excluding those peers which are already in the TNIC group), which tend to differ more strongly from the focal firms in their business activities. I find only very limited evidence for an effect on focal firm insider trading when using this peer group.

Third, to better identify the endogenous effect, shocks which are orthogonal to focal firm trading, but have an impact on peer trading (such as trades for liquidity) can be used. If focal firm trading also increases after such trades, this would support the peer effects theory. As an approximation to such non-informative trades, I use trades classified as routine by Cohen et al. (2012)'s approach. The results provide only limited evidence for such a peer effect, namely the coefficient for the NPR is significant, as are the effects of peer sales on focal purchases. The routine trades do not influence abnormal returns, which is in line with the learning channel.

My findings are robust to changes in the methodology, namely the clustering of standard errors and the choice of fixed effects. By including SIC industry fixed effects, I can show that my results are not the result of common industry shocks. The selection of insider trades does not lead to material changes either. I show this by only including D&Os-trades for the peer groups.

My paper adds new insights to several strands of literature. First, it contributes to the insider trading literature. The positive relationship of insider purchases with future returns (e.g., Lakonishok and Lee (2001) or Fidrmuc et al. (2006)) is well established, as is the negative one for certain types of sales (e.g., Cohen et al. (2012), Cline et al. (2017), or Akbas et al. (2020)). By showing the effect that insider trading has on trading behavior and on abnormal returns in product-market peers, I provide evidence that such trades are not only relevant for the valuation of the company whose shares are traded, but also for peer firm valuation. The trades that I analyze are based on publicly available information (namely the Form 4 publications), which directors and officers seem to be able to interpret and learn from more efficiently than other market participants. Thus, my work is also related to the attentive insider trading literature (e.g., Alldredge and Cicero (2015) or Chabakauri et al. (2022)), to which I add a new channel.

Second, this paper expands the literature on information spillovers within industries and between insiders, in which to the best of my knowledge, insider trades have been overlooked as a channel through which information can disseminate. Related papers on industry spillovers often use earnings announcements (e.g., Hung et al. (2015), Baker et al. (2019), or Bergsma and Tayal (2020)) or managerial forecasts (Kim et al. (2008)). On information dissemination between insiders, I relate to e.g., Ahern (2017) who looks at illegal insider trading networks and how they are shaped by social ties, or Alldredge and Blank

(2019) who document the information flow between colleagues within the same company.

Third, my results can be interpreted as a new channel of managerial learning from outside sources. This complements the literature starting with Chen et al. (2007) and Bond et al. (2012). Other papers establish that corporate decision makers draw information from sources such as cross-listings (Foucault and Frésard (2012)), peers' stock prices (Foucault and Frésard (2014)), or Credit Default Swap (CDS) trading activity (Kim et al. (2023)). Simultaneously, I shed light on the extent to which managers have superior information about the industry that their employer operates in. On this topic, my paper is e.g., related to Ben-David et al. (2019) and Hutton et al. (2012) who attribute industry knowledge to directors, officers, and other experts.

Fourth, my work relates to the peer effects literature, where it is at the intersection of household- and corporate finance outcomes. Kuchler and Stroebel (2021) list several channels through which individual behavior can be influenced by the social environment. Namely, social learning, perception, or utility. Fracassi (2017) reports that companies where the managers have prior social connections display more similar capital investment behavior, which can be explained by the influence of the peer network. Also, papers by Shiller and Pound (1989) or Ivkovic and Weisbenner (2007) discuss how retail and institutional investors are influenced by their peers. In terms of corporate outcomes, e.g., firm characteristics (Leary and Roberts (2014)), dividends (Grennan (2019)), stock splits (Kaustia and Rantala (2015)) or measures of corporate social responsibility (Cao et al. (2019)) in peer firms are shown to have a positive impact on the respective outcomes in the focal firm. Even though I can only provide limited evidence for peer effects in insider trading, it is a mechanism that has not been covered so far.

This paper is structured as follows. Section 2 discusses the data, the sample and the methodological approach. In Section 3, I introduce and discuss the main findings on peer insider trading's impact on focal firm insider trading and the profitability of such trades. Section 4 elaborates further on the suggested channels, while Section 5 shows the robustness tests. Section 6 concludes the paper.

2 Data and Method

2.1 Data

I examine the effect that trading by insiders in the group of peer companies has on subsequent trading by directors and officers in the company of interest, hereafter called the focal firm. My sample for this analysis is based on firms in the CRSP/Compustat merged universe. From these companies, I include those for which insider trades are available in the Thomson Reuters Insider Trading database[3] and which

[3]If there are multiple share classes, I aggregate them by company.

are in the Hoberg and Phillips (2016) TNIC database. In this methodology, firms are classified based on business descriptions from 10-K filings. Compared to other established industry classifications, TNIC is more suitable for the identification of peer effects, because all company-pairs with a score can be thought of as competitors.[4] The SIC and other classifications on the other hand include companies that produce complementary products or do not interact in the product market at all. Additionally, I use I/B/E/S to identify earnings announcement dates.

In accordance with existing literature, I only consider common stock traded on NYSE or NASDAQ, and I exclude financial and utility firms by SIC code, namely companies in the ranges 4900 - 4999 and 6000 - 6999. The sample spans from September 2002 to December 2021. The factors limiting the sample period are the introduction of the Sarbanes-Oxley Act in August 2002 and the TNIC measure whose coverage ends with the year 2021. Among other things, Section 403 of the Sarbanes-Oxley Act mandates timely reporting of trades by corporate insiders. Before, insiders had to report trades within ten days after the end of the month in which the trade took place. Since the introduction of the act, trades have to be filed with the SEC electronically within two business days. The effects of this new regulation are twofold. On one hand, more timely reporting should increase the value of the contained information. On the other hand, more frequent reporting reduces a report's information content, because fewer trades are reported at once. Brochet (2010) finds that the first effect is dominant. To ensure comparability of the Form 4 filings that I use, I restrict my sample to the post-Sarbanes-Oxley-Act period.

2.2 Variables

From the insider trading data, I use two categories of trades. For focal firm trades, these are open market trades by D&Os. I only use trades filed in Table 1 of the SEC's Form 4. For peer trading measures, I additionally include bona fide gifts and "other transactions" (transaction code J) to capture the reaction to transactions such as in-kind distributions. The inclusion of these categories is justified by previous work which shows that they also signal private information to the market (see Avci et al. (2021) for gifts, or e.g., Gompers and Lerner (1998) for in-kind distributions).

To capture the insider trades in focal firms that are most likely to be based on valuable, new information about company value (as compared to e.g., liquidity trades), I classify trades according to the procedure of Cohen et al. (2012). I then only use opportunistic trades and those which are not classified[5], summing them up under the term non-routine trades. With these trades, I calculate the

[4] Furthermore, the TNIC database is purged for vertical relationships (see authors' notes)

[5] Unclassified trades are e.g., those of insiders with no three year trading history, i.e., not regular traders. Based on this information, it is reasonable to include such trades. Cohen et al. (2012) show that their results are robust to including unclassified trades.

Net Purchase Ratio (NPR) as in Lakonishok and Lee (2001). This measure ranges from -1 to 1 with the lower (upper) bound indicating a month with sales (purchases) only. A value of zero means either no trading, or more generally an equal value of purchases and sales. I calculate the ratio based on transactions' USD values in order to not overstate the impact of small trades. To measure the effect of the intensive and extensive margins of insider trading, I create an indicator for having open market, non-routine trades and also use the natural logarithm of the number of such trades. For peer firms, I use the same measures, but also include the other transactions as mentioned above. For the aggregation of single peer firm values to the combined peer group, I use the NPR's mean, the maximum value of the indicator variables and the sum of the number of individual peer transactions.

The six-month CARs are calculated using a market model[6], in which the CRSP Value-weighted market return proxies for the market performance. I use an estimation window of 100 trading days (imposing a minimum of 70 days) and a gap of ten trading days between estimation and event window. The return measurement starts at the date of the insider trade and ends at 126 trading days post-trade. Trades for which the return time series ends early are cleansed from the sample.

I control for a variety of firm characteristics which are known to have an impact on insider trading. As my dependent variable is always based on focal firm values, I also control for characteristics of said firm, except for the peer trading. To control for size and future growth opportunities, I use the natural logarithms of market capitalization and of the B/M Ratio. Following Cohen et al. (2012), I cap the B/M Ratio at zero and 100 to eliminate values which are most likely outliers. Among others, Aboody and Lev (2000) show that information asymmetry between corporate insiders and outsiders has an impact on insider trading. One way in which information asymmetry manifests is abnormal returns to earnings announcements. I therefore use the median of the magnitude of CARs to earnings announcements over a rolling five-year window as a proxy (see Huddart and Ke (2007)). To deter insiders from using their information advantage for private gains, most companies have introduced blackout windows during which no insider trading can take place. The length and timing of blackout windows are not publicly known and are difficult to measure exactly (for more information, see e.g., Bettis et al. (2000) or Jagolinzer et al. (2011)). Since trading for insiders is in most cases open for a time period following the QEA, I create an indicator variable for a QEA taking place in a certain month to control for the impact of blackout windows. Past stock returns also influence insider trading behavior. Seyhun (1992) shows that insiders tend to be contrarian investors when it comes to their own stock. I use the return in the month before measuring focal insider trading to control for momentum and the aggregate return over the eleven months before said month to measure the impact of a reversal in the long run.

[6] See, e.g., Mackinlay (1997) as a reference for event studies.

2.3 Sample

An overview on the resulting sample is presented in Table 1, where Panel A contains information on firm characteristics and control variables. Panel B lists the summary statistics for the insider trading variables. Table 1 numbers are all based on focal firm values. It is worth noting that any focal company can also be part of one or multiple peer groups and vice versa.
The proxy for information asymmetry compares well to the mean (median) of 3.485% (3.022%) reported by Huddart and Ke (2007). The number of peers per focal company is spread relatively widely, which is due to Hoberg and Phillips (2016)' attempt to achieve a granularity similar to three-digit SIC. Panel B shows a negative mean for the NPR and consistently higher numbers for the sale as compared to the purchase variables. This reflects the fact that corporate insiders usually obtain shares through stock-based compensation and warrants (i.e., not open market trades), while they are more likely to sell them via the open market.

In addition, I show an overview on the six-month CARs following non-routine, open market transactions by directors or officers in Table 2. A look at column (2) provides evidence supportive of the notion that insider purchases predict positive future returns, while insider sales are usually followed by worse performance (compare to e.g., Cohen et al. (2012)). The median outperformance as compared to the market is economically significant at almost 10% following the six months after a purchase, as is the underperformance of 7.2% following a sale. Insider trades in peer firms in the ten days before a focal firm purchase (sale) strengthen the positive (negative) effect. The differences in means reported in column (3) are $(-)1.7\%$ for the six-month period and are statistically significant on the 99% level, which is consistent with a learning effect.

2.4 Method

To capture different dimensions of focal firm trading outcomes, I estimate two main sets of regressions. First, I aim to describe the effect that trading in peer companies has on the ratio of purchases to sales, the probability and the number of trades in the focal firm. Second, to differentiate between peer effects in the spirit of Manski (1993) and a learning from peer trades, I look at the CARs to insider trades. I base the analysis on the same sample of companies over the same period of time. However, to capture the effects as precisely as possible, the samples are structured differently.
First, I use a panel with monthly observations of focal firms and aggregated values over the peer firm groups. I introduce a lag in the explanatory variables to check the extent to which peer trades can predict focal trades and to alleviate concerns over reverse causality.

For the CAR regressions, I use the full available sample of non-routine insider trades by directors and

Table 1: Summary Statistics - Firm Characteristics and Insider Trading

This table shows in Panel A an overview on the firm-specific characteristics in the sample on which the regressions are computed. *Market capitalization*, *Book-to-market ratio*, *Information asymmetry proxy*, *Monthly return*, and *Distinct companies* are based on focal firm values. Panel B presents the insider trading variables, also based on focal firm values. The sample contains the intersection of the CRSP/Compustat merged, Thomson Reuters Insider Trading, and Hoberg and Phillips (2016) TNIC databases. The sample period is Sep 2002 to Dec 2021.

Panel A: Firm Characteristics

	N	Mean	Median	SD	1%	99%
Market capitalization (millions of USD)	363,187	7,382	937	37,132	17.2	129,836
Book-to-market ratio	363,187	.55	.395	.707	.0223	2.84
Information asymmetry proxy	363,187	.0357	.0308	.0218	.00809	.109
Monthly return	363,187	.0151	.00933	.135	-.327	.462
Peers per focal company	4,113	176	91	198	2	819
Distinct companies	4,113					

Panel B: Insider Trading Characteristics

	N	Mean	Median	SD	1%	99%
NPR of D&O non-rout. open market trades	363,187	-.254	0	.531	-1	1
Dummy for D&O non-rout. o. m. purchases	363,187	.0639	0	.245	0	1
Dummy for D&O non-rout. o. m. sales	363,187	.31	0	.462	0	1
Number of D&O non-rout. o. m. purchases	363,187	.25	0	3.32	0	5
Number of D&O non-rout. o. m. sales	363,187	3.08	0	20.7	0	51

Table 2: Summary Statistics - Cumulative Abnormal Returns

This table shows an overview on the six-month CARs following insider trades. Column (1) presents CARs for focal trades preceded by peer trades in the 10 trading days before. Column (2) displays the statistics for focal trades not accompanied by peer trades. Column (3) lists the differences between the sample means and t-tests. The sample contains the intersection of the CRSP/Compustat merged, Thomson Reuters Insider Trading, and Hoberg and Phillips (2016) TNIC databases. The sample period is Sep 2002 to Dec 2021.

	(1) Has preceding peer trades				(2) No preceding peer trades				(3) Difference	
	N	Mean	Median	SD	N	Mean	Median	SD	Diff.	t-Test
6m-CAR after purchase	52,447	0.163	0.108	0.651	11,767	0.146	0.097	0.547	-0.017***	(-2.918)
6m-CAR after sale	268,599	-0.115	-0.085	0.450	36,462	-0.098	-0.072	0.377	0.017***	(8.091)

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

officers in focal firms. CARs are measured over the six months following each day with insider trading activity. The main explanatory variables for peer trading are measured over the ten days immediately preceding the focal firm trading day. All control variables for focal firm characteristics are based on the calendar month ahead of the trading day. The regressions that I estimate are specified as follows, first for the focal firm trading outcomes:

$$\begin{aligned}\text{Insider Trading}_{i,t+1} = \alpha &+ \beta_{1,2} \mathit{Insider\ Trading}_{j,t} + \beta_3 \ln(\text{Market Capitalization})_{i,t} \\ &+ \beta_4 \ln(\text{B/M-Ratio})_{i,t} + \beta_5 \text{Info. Asymmetry}_{i,t} + \beta_6 \text{QEA Dummy}_{i,t} \\ &+ \beta_7 \text{Return}_{i,t} + \beta_8 \text{Return}_{i,(t-1)-(t-12)} \\ &+ \gamma \text{Firm Fixed Effects}_i + \delta \text{Month Fixed Effects}_t + \epsilon\end{aligned} \tag{1}$$

, where i denotes the focal firm, j the aggregated peer group, and t the month. The second set of regressions focuses on abnormal returns:

$$\begin{aligned}\text{6m forward CAR}_{i,t} = \alpha &+ \beta_{1,2} \mathit{Insider\ Trading}_{i,t} + \beta_{3,4} 10d\ \mathit{ahead\ Insider\ Trading}_{j,t} \\ &+ \beta_{5-8} \mathit{Focal} \times \mathit{Peer\ Trading}_{i,j,t} + \beta_9 \ln(\text{prev. month Market Cap.})_{i,t} \\ &+ \beta_{10} \ln(\text{prev. m. B/M-Ratio})_{i,t} + \beta_{11} \text{prev. m. Info. Asymmetry}_{i,t} \\ &+ \beta_{12} \text{prev. m. Return}_{i,t} + \beta_{13} \text{prev. year Return}_{i,t} \\ &+ \gamma \text{Firm Fixed Effects}_i + \delta \text{Month Fixed Effects}_t + \epsilon\end{aligned} \tag{2}$$

, where i denotes the focal firm, j the aggregated peer group, and t the day. For both regression sets, I use focal firm and month fixed effects. Standard errors are clustered on focal firm level.

3 Main Results

To explore the effect that insider trading in close industry peers has on a company's own insiders' trading activity, I estimate two main sets of results. In Section 3.1, I examine the effect on focal firm trading directly. The results reported in Section 3.2 focus on the influence that the trading has on the return to focal firm stock.

3.1 Trading Activity

In this section, I look at how managers react to insider trades published in companies which are active in a similar or even the same line of business as they are. I utilize all open market trades, bona fide gifts and in-kind distributions in peer firms, because I assume that managers are not able to distinguish peer trading purely for liquidity from actually informative trades. My assumption bases on the finding of Dessaint et al. (2019), who show that companies reduce investment as a reaction to non-fundamental drops in peers' stock prices, which indicates that managers are unable to make the aforementioned distinction. For focal firm managerial insider trading—the outcome variables—however, I only use non-routine open market trades to ensure that I actually capture insider trades which have been made actively. In Panel A of Table 3, I report the coefficient estimates of the peer trading indicators without controlling for further variables. In Panel B, I add a selection of controls for focal firm characteristics. In both panels, I use month and focal firm fixed effects, also standard errors are clustered by focal firm.

The results indicate that managers tend to follow insiders in their peer companies. Namely, in column (1), the estimate for *NPR, peer* is positive and statistically significant, meaning that when there are more purchases relative to sales in peer companies, this tends to increase the relative number of purchases in the following month for the focal firm. A one-unit change in the peer-NPR (e.g., moving from an equal dollar-volume of purchases and sales to having only purchases) is associated with a 7% change in the NPR of the focal firm, relative to its sample mean in the specification with control variables. Turning to the probability of having insider trading in the focal firm, the results are less clear. Overall, the results in columns (2) and (3) support the ones from the NPR in that trading seems to go in the same direction (i.e., more purchases predict more purchases and less sales, and vice versa). However, the evidence is not as strong: while in Panel A, peer purchases predict both types of focal firm trades and peer sales predict more sales, in Panel B, only the coefficient for peer on focal purchases is significant. This finding is in line with the literature—such as Fidrmuc et al. (2006)—in that insider purchases are expected to send a stronger signal as compared to sales. In terms of magnitude, having peer purchases increases the likelihood of focal firm purchases by 3.58% relative to the sample mean. Given that there is trading, having more peer trades increases the number of trades in the same direction in the focal firm and decreases those in the opposite direction. All four effects are statistically significant in both specifications.

The results reported in Table 3 with peer trading having a significant impact on trading in related firms are in line with a social peer effect (or, put differently, an endogenous effect as described by Manski (1993)). Nevertheless, I do not interpret them as evidence for such an effect, since I cannot rule out exogenous or correlated effects. As for the managerial learning hypothesis, finding an effect in the same direction (i.e., peer sales lead to more focal sales and / or fewer purchases and vice versa) indicates that managers interpret their peers' trades as containing incremental information which is applicable to the entire peer group. It does not rule out the occurrence of company-specific information, but the peer-group wide effect seems to overlap in my sample.

Table 3: Trade Direction

This table reports regressions of insider trading measures in the focal firm on previous-month peer insider trading. The dependent variable in each column is the same trading measure as for the main explanatory variables, but for the focal firm and only considering non-routine, open market trades by D&Os. *NPR, peer* denotes the NPR of all peer insider trades. *Purchase (Sale) dummy, peer* is an indicator for having a purchase (sale) transaction in the group of peer firms. *Log(N) purchases (sales), peer* measures the natural logarithm of the number of peer purchase (sale) transactions. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *QEA dummy, focal* is an indicator for a QEA taking place in the month before the focal firm trading. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. In both panels, the same fixed effects are used, and standard errors are clustered by focal firm.

Panel A: Insider Trading Variables

	(1) Net Purchase Ratio	(2) Purch. Dummy	(3) Sale Dummy	(4) Log(N) Purch.	(5) Log(N) Sale
NPR, peer	0.041***				
	(9.359)				
Purchase dummy, peer		0.004***	−0.006**		
		(2.819)	(−2.436)		
Sale dummy, peer		−0.003	0.009***		
		(−1.642)	(2.867)		
Log(N) purchases, peer				0.003***	−0.011***
				(3.813)	(−4.946)
Log(N) sales, peer				−0.002***	0.016***
				(−3.356)	(7.753)
Observations	463,929	463,929	463,929	463,929	463,929
Adjusted R^2	0.199	0.068	0.226	0.068	0.239

Panel B: Insider Trading and Control Variables

	(1)	(2)	(3)	(4)	(5)
NPR, peer	0.013***				
	(2.910)				
Purchase dummy, peer		0.003*	−0.003		
		(1.916)	(−1.369)		
Sale dummy, peer		−0.002	−0.001		
		(−0.991)	(−0.234)		
Log(N) purchases, peer				0.002**	−0.007***
				(1.973)	(−3.024)
Log(N) sales, peer				−0.001*	0.006***
				(−1.653)	(2.618)
Log(Market Cap (millions of USD)), focal	−0.101***	−0.013***	0.085***	−0.023***	0.133***
	(−22.877)	(−7.894)	(21.883)	(−9.296)	(15.822)
Log(B/M-Ratio), focal	0.032***	0.004***	−0.026***	0.006***	−0.062***
	(7.768)	(2.700)	(−7.461)	(2.713)	(−7.722)
Information asymmetry proxy, focal	0.096	0.010	−0.081	0.006	−0.071
	(0.832)	(0.196)	(−0.831)	(0.076)	(−0.333)
QEA dummy, focal	−0.072***	0.022***	0.088***	0.024***	0.169***
	(−26.916)	(17.326)	(33.522)	(14.754)	(31.617)
Current month return, focal	−0.337***	−0.099***	0.232***	−0.139***	0.534***
	(−38.181)	(−23.943)	(31.597)	(−23.178)	(33.537)
Past 12m-return, excl. current month, focal	−0.061***	−0.006***	0.052***	−0.011***	0.127***
	(−18.801)	(−5.924)	(18.287)	(−6.935)	(18.578)
Observations	363,187	363,187	363,187	363,187	363,187
Adjusted R^2	0.229	0.074	0.245	0.077	0.261
Month FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: NPR of D&O non-rout. open-mkt. focal trades' volume (Col. (1)),
indicators for such purchases (2) (sales (3)), log number of such purchases (4) (sales (5))

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

Besides the insider trading variables, I control for firm characteristics. First, higher firm size leads to fewer purchases and more sales. Possible explanations for this finding are higher public attention towards the trades of managers in larger firms, which deters them from purchasing shares. Also in bigger firms, compensation packages are probably more likely to contain a higher number of shares and warrants, leading to managers obtaining more shares in non-open market transactions, which they then sell in the open market. In growth firms, where shares are traded at a strong premium compared to book value, insiders tend to purchase fewer and sell more shares. This is in line with the observed, contrarian behavior in Jenter (2005) and Seyhun (1992).

I expect that a higher information asymmetry increases all insider trading, but in particular insider purchases. The coefficients for Huddart and Ke (2007)'s proxy are not significant. I attribute this to the fact that firm size and the B/M Ratio also proxy for information asymmetry to some extent. Indeed, when I exclude these two variables, the effect of the proxy is positive and significant (results not reported). The impact of earnings announcements is not limited to proxying for unequal access to information. They also have a direct effect on trading, because the vast majority of public companies limits trading by insiders to a period following the QEAs (see, e.g., Bettis et al. (2000)). It is therefore not surprising that the indicator for having a QEA has a positive effect on the probability and the number of trades in the following month. Together with the use of month fixed effects, such a control variable alleviates concerns of finding an effect solely due to the bunching of trades during certain time periods. Lastly, I control for past stock returns. They support the hypothesis of contrarian insiders in that higher returns in the past month and year tend to decrease (increase) the number of buy (sale) transactions significantly.

When comparing the two panels, it is striking that the adjusted R^2 is only slightly increased by the additional control variables. The majority of the variance in the dependent variable that the model can explain, stems from the peer trading variables and the fixed effects. Congruent with this is the fact that the inclusion of controls only slightly changes the estimates of the peer trading, with the exception of the sale indicators discussed above. Overall, peer trading nudges subsequent trading in the focal firm in the same direction, which is consistent with a peer effect among managers and other insiders, but also with these trades sending a signal about industry prospects to close peer firms.

3.2 Trade Profitability

The results presented in the previous section lay the groundwork by establishing an effect of peer- on focal insider trading. They do not allow for a distinction between the two explanations of managers drawing useful information from peer insider trading and observing a purely social phenomenon of managers following peer firm insiders in their trading behavior. In this section, I report results on the returns to focal firm trades. While the occurrence of abnormal returns does not rule out a peer effect, it certainly supports the notion of managers gaining new insights from peer trades.

In the case of a learning effect, I expect that trades are more profitable when they are executed following a peer trade. Profitability in this case can be either an outperformance of the market following a purchase, or underperformance of the stock in the period after a sale. As discussed in Sec. 2.1, I calculate CARs over the six months immediately after the trade using a market model. Choosing a period of six months is justified by the short-swing profit rule under Section 16(b) of the Securities Exchange Act of 1934. The rule posits that any profit made by insiders through a purchase followed by a sale (or vice versa) within less than six months have to be forfeited. Also, if such an effect persists over at least this time period, it is likely to be more than a short-term overreaction of the market to the publication of the trade itself. To identify the effect, I check for each non-routine, open market trade by directors or officers in the focal firm if there has been any peer trade in the ten days before and classify them using an indicator variable. I then interact the indicator for the type of focal trade (i.e., purchase or sale) with the peer trade indicator. A significant interaction effect in the same direction as the focal trade's effect signals incremental information drawn from the peer trades.

Similar to the results reported in Sec. 3.1, the inclusion of control variables does not have a material impact on the estimates of the insider trading variables. However, the explanatory power of the model increases more sharply, the adjusted R^2 roughly doubles from 0.17 to 0.36. As expected, insider purchases in the focal firm predict positive future abnormal returns across specifications, while sales predict an underperformance vis-à-vis the market. For the peer trading variables, the expectation is less straightforward: in principle, if peer trades contain information specific to the peer companies, I would expect an effect on focal-firm CARs in the opposite direction as focal firm trades. If the contained information is applicable to the entire industry (i.e., the focal firm and its group of peers), the effect is more likely to be similar to the focal trades' one. The results in Table 4 support the second trajectory of industry-wide signals. The coefficient for peer purchases is positive across specifications, but not always significant. Similarly for sales, the effect tends to be negative. However, in column (6), where all control variables and interaction terms for all combinations of peer and insider trading are included, the peer trading variables lack statistical significance. Also, the magnitude of the peer trades' effect is smaller compared to focal firm trades. One explanation for the findings could be common industry shocks causing insider trading and concurrently having an impact on future abnormal returns. The fact that in the most detailed specification (column (6)), both peer trading variables become insignificant, is evidence against this conjecture. As is the evidence provided in Table 10, where I include industry fixed

Table 4: Trade Profitability

This table reports regressions of returns on focal and peer firm insider trading, and control variables. The dependent variable is the focal firm's six-month CAR, in decimals. *D&O non-rout. open market purch. (sale), focal* is an indicator equal to one for non-routine, open market purchases (sales) by D&Os in the focal firm, and zero otherwise (routine trade classification following Cohen et al. (2012)). *Purch. (sale), peer* is an indicator for purchases (sales) taking place in the peer firm during the ten days preceding the focal firm trade. *D&O n.-r. o. m. purch (sale), focal* × *purch. (sale), peer* denote the four interaction terms of the aforementioned trading indicators in focal and peer firms. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm.

	(1) Focal Purchases	(2) Focal Sales	(3) Both	(4) Focal Purchases	(5) Focal Sales	(6) Both
D&O non-rout. open market purch., focal	0.274^{***}		0.071^{***}	0.115^{***}		0.025
	(21.498)		(3.592)	(11.344)		(1.386)
D&O non-rout. open market sale, focal		-0.288^{***}	-0.219^{***}		-0.122^{***}	-0.098^{***}
		(−21.833)	(−10.008)		(−11.572)	(−5.020)
Purchase, peer	0.004	0.102^{***}	0.056^{**}	0.002	0.074^{***}	0.025
	(1.169)	(9.596)	(2.418)	(0.711)	(8.160)	(1.319)
Sale, peer	-0.012^{***}	-0.043^{***}	−0.038	0.004	-0.023^{**}	0.005
	(−3.043)	(−3.385)	(−1.366)	(0.991)	(−2.137)	(0.183)
D&O n.-r. o. m. purch., focal × purch., peer	0.096^{***}		0.047^{**}	0.071^{***}		0.049^{***}
	(9.051)		(2.284)	(7.966)		(2.892)
D&O n.-r. o. m. purch., focal × sale, peer	-0.033^{***}		−0.004	-0.029^{***}		−0.028
	(−2.603)		(−0.168)	(−2.725)		(−1.253)
D&O n.-r. o. m. sale, focal × purch., peer		-0.098^{***}	-0.052^{**}		-0.071^{***}	−0.023
		(−8.757)	(−2.241)		(−7.580)	(−1.204)
D&O n.-r. o. m. sale, focal × sale, peer		0.030^{**}	0.026		0.027^{**}	−0.001
		(2.282)	(0.917)		(2.384)	(−0.036)
Log(Market Cap (millions of USD)), focal				-0.153^{***}	-0.153^{***}	-0.152^{***}
				(−20.582)	(−20.549)	(−20.544)
Log(B/M-Ratio), focal				0.075^{***}	0.074^{***}	0.074^{***}
				(10.462)	(10.443)	(10.443)
Information asymmetry proxy, focal				-0.749^{***}	-0.749^{***}	-0.750^{***}
				(−3.688)	(−3.685)	(−3.688)
Current month return, focal				-0.929^{***}	-0.927^{***}	-0.926^{***}
				(−62.227)	(−62.191)	(−62.153)
Past 12m-return, excl. current month, focal				-0.171^{***}	-0.171^{***}	-0.171^{***}
				(−19.252)	(−19.250)	(−19.246)
Constant	-0.100^{***}	0.188^{***}	0.118^{***}	1.271^{***}	1.389^{***}	1.363^{***}
	(−25.099)	(15.377)	(5.472)	(22.841)	(25.238)	(23.594)
Observations	310,249	310,249	310,249	279,546	279,546	279,546
Adjusted R^2	0.171	0.173	0.173	0.359	0.359	0.359
Month FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses
Dependent variable: 6m-CAR
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

effects (see Sec. 5.2).

The focus of the analysis in this section is on the interaction of focal and peer trading. I find that there is a positive effect on abnormal returns if the focal firm insiders follow the direction of their peer group. Trading in opposite directions on the other hand tends to hamper future returns in the focal firm at first sight. The most persistent effect stems from purchases which are immediately preceded by peer purchases. The result suggests that focal insider purchases are a positive signal for firms operating in a closely related market. Ben-David et al. (2019) find that managers are experts in the field in which their employer operates. Thus, it is plausible that their trading activity is a credible indicator for directors and officers in similar companies. Similar to Sec. 3.1, the incremental information which is applicable to the entire peer group seems to outweigh any company-specific information from the peer trades. Finding the strongest effect for the combination of purchases is no surprise given existing findings. With 9.9% higher abnormal returns following focal and peer insider purchases, the effect is economically sizeable. The mean CAR over six months in the sample is −7.08%. While having such negative CARs across the sample might seem exceptional, it can be explained by the prevalence of insider sales.

With the other interaction effects, a pattern can be observed in that most statistically significant coefficients run in the opposite direction as the corresponding peer trading. One example is in column (3), where *purchase, peer* and *D&O n.-r. o. m. sale, focal* × *purch., peer* show values of 0.056 and −0.052, respectively. Therefore, while in some cases there is statistical significance, there is no economically significant effect because the two effects cancel each other out.

I control for a similar group of focal-firm specific variables as in the first set of regressions in Sec. 3.1. Higher firm size negatively impacts future abnormal returns, while value firms tend to produce higher returns. Higher information asymmetry has a negative effect, as do higher past returns in the shorter and longer term. In other words, there is some evidence for a reversal effect when comparing the twelve months before the trade to the six months after.

4 Additional Analyses

4.1 Effect of Firm Size

Existing literature on peer effects often shows that smaller companies follow their bigger peers, but not vice versa (e.g., Leary and Roberts (2014)). Also when considering learning effects, I would expect that actions of managers in small firms are less important to managers in larger companies. The opposite can be expected for small firms' managers' reaction to trades in their bigger peers. I confirm this finding for managerial trades in Table 5, where the sample is split by market capitalization. Columns (1) to (4) only use observations in which the peer firm is smaller than the focal firm. Columns (5) to (8) report results for peer firms which are bigger than the focal firm.

Because the sample is split along monthly market capitalization which is subject to fluctuations, the analysis can be arbitrary for similarly sized companies. This might help explain why smaller peers' trades have some predictive ability for trades in larger firms. In analyses not reported in this paper, I confirm that clearly smaller peers (e.g., those in the smallest size tercile or quartile) do not predict trading in bigger firms. Another way to investigate the impact of firm size is to compare similarly sized firms (e.g., peers which are at least three quarters and no more than 1.5 times the focal firms' size). In this group, the results are also stronger as compared to the small peers.

4.2 Scope of Insiders' Industry Knowledge

The literature attributes to managers knowledge about their industry (e.g., Ben-David et al. (2019)). Hutton et al. (2012) find that it is at a level similar to analysts. But it seems limited in that managers are more knowledgeable about assets in place in their firm and they can learn from increased outside information production, which as Ye et al. (2023) argue has a comparative advantage over managers when it comes to macroeconomic and industry factors.

Furthermore, Kim et al. (2008) find differential effects of management forecasts for direct competitors and non-rival industry peers. In similar vein, I expect that insider trades in product market competitors identified by the TNIC affect firms differently than trades in more distant industry peers. In my analysis, the distant peers are represented by firms in the same four-digit SIC industry which are not peers of the focal firm in TNIC. Specifically, I expect that insider trades have a stronger effect on trading in TNIC-peers as compared to SIC-peers. This is due to the fact that these companies operate in more similar markets and the information contained in such trades is thus more likely to be of interest for and applicable to the managers in the focal firm.

Table 5: Effect of Firm Size on Peer Trading

This table reports regressions of insider trading measures in the focal firm on previous-month peer insider trading. Columns (1) - (4) report results for peers which are smaller than the focal company. In columns (5) - (8), only peers with higher market capitalization than the focal firm are considered. The dependent variable in each column is the same trading measure as for the main explanatory variables, but for the focal firm and only considering non-routine, open market trades by D&Os. *Purchase (Sale) dummy, peer* is an indicator for having a purchase (sale) transaction in the group of peer firms. *Log(N) purchases (sales), peer* measures the natural logarithm of the number of peer purchase (sale) transactions. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *QEA dummy, focal* is an indicator for a QEA taking place in the month before the focal firm trading. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm.

	Small Peers				Big Peers			
	(1) Purch. 0/1	(2) Sale 0/1	(3) N Purch.	(4) N Sale	(5) Purch. 0/1	(6) Sale 0/1	(7) N Purch.	(8) N Sale
Purchase dummy, peer	0.000	−0.003			0.004***	−0.010***		
	(0.216)	(−1.381)			(3.269)	(−4.079)		
Sale dummy, peer	−0.004***	0.003			−0.003*	0.006**		
	(−2.657)	(1.022)			(−1.859)	(2.013)		
Log(N) purchases, peer			0.000	−0.006**			0.003***	−0.010***
			(0.249)	(−2.256)			(2.706)	(−3.961)
Log(N) sales, peer			−0.001	0.011***			−0.002***	0.003
			(−1.532)	(4.585)			(−2.634)	(1.057)
Log(Market Cap (millions of USD)), focal	−0.012***	0.090***	−0.022***	0.140***	−0.014***	0.087***	−0.023***	0.133***
	(−7.005)	(20.378)	(−8.351)	(14.494)	(−7.875)	(21.964)	(−8.926)	(15.599)
Log(B/M-Ratio), focal	0.004**	−0.028***	0.006**	−0.066***	0.004**	−0.027***	0.006**	−0.065***
	(2.560)	(−7.162)	(2.571)	(−7.355)	(2.394)	(−7.323)	(2.417)	(−7.672)
Information asymmetry proxy, focal	0.059	−0.077	0.075	−0.084	0.006	−0.074	−0.011	−0.097
	(1.065)	(−0.670)	(0.935)	(−0.334)	(0.127)	(−0.753)	(−0.139)	(−0.452)
QEA dummy, focal	0.021***	0.091***	0.022***	0.175***	0.022***	0.084***	0.024***	0.162***
	(15.612)	(31.634)	(12.887)	(29.994)	(16.969)	(32.470)	(14.113)	(30.654)
Current month return, focal	−0.103***	0.253***	−0.142***	0.583***	−0.096***	0.216***	−0.137***	0.504***
	(−22.873)	(31.090)	(−21.749)	(32.936)	(−22.834)	(29.501)	(−22.276)	(31.764)
Past 12m-return, excl. current month, focal	−0.007***	0.054***	−0.011***	0.132***	−0.006***	0.050***	−0.011***	0.122***
	(−5.810)	(17.126)	(−6.510)	(17.281)	(−5.684)	(17.247)	(−6.600)	(17.521)
Observations	315,282	315,282	315,282	315,282	332,069	332,069	332,069	332,069
Adjusted R^2	0.072	0.240	0.078	0.262	0.077	0.252	0.079	0.268
Month FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Indicator for D&O non-rout. open-mkt. focal purchases (Col. (1) & (5)) (sales (2) & (6)), log number of such purchases ((3) & (7)) (sales ((4) & (8)))

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

Table 6: Trading in SIC Four-Digit Industry Peers, Excluding Direct TNIC Competitors

This table reports regressions of insider trading measures in the focal firm on previous-month peer insider trading. The peer firms consist of SIC four-digit industry peers, excluding TNIC-peers. The dependent variable in each column is the same trading measure as for the main explanatory variables, but for the focal firm and only considering non-routine, open market trades by D&Os. *Purchase (Sale) dummy, peer* is an indicator for having a purchase (sale) transaction in the group of peer firms. *Log(N) purchases (sales), peer* measures the natural logarithm of the number of peer purchase (sale) transactions. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *QEA dummy, focal* is an indicator for a QEA taking place in the month before the focal firm trading. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm.

	(1) Purch. Dummy	(2) Sale Dummy	(3) Log(N) Purch.	(4) Log(N) Sale
Purchase dummy, peer	−0.002	−0.001		
	(−1.618)	(−0.315)		
Sale dummy, peer	−0.002	0.005*		
	(−1.105)	(1.846)		
Log(N) purchases, peer			−0.001	−0.003
			(−1.279)	(−1.147)
Log(N) sales, peer			−0.002**	0.003
			(−2.441)	(1.450)
Log(Market Cap (millions of USD)), focal	−0.013***	0.084***	−0.022***	0.133***
	(−7.895)	(21.154)	(−9.270)	(15.252)
Log(B/M-Ratio), focal	0.005***	−0.027***	0.008***	−0.058***
	(4.012)	(−7.477)	(4.020)	(−7.051)
Information asymmetry proxy, focal	−0.014	−0.135	−0.000	−0.146
	(−0.298)	(−1.354)	(−0.006)	(−0.680)
QEA dummy, focal	0.021***	0.087***	0.024***	0.166***
	(17.089)	(32.989)	(14.648)	(30.565)
Current month return, focal	−0.097***	0.223***	−0.136***	0.522***
	(−23.130)	(30.891)	(−21.979)	(33.006)
Past 12m-return, excl. current month, focal	−0.006***	0.051***	−0.010***	0.127***
	(−5.334)	(18.215)	(−6.263)	(18.745)
Observations	362,823	362,823	362,823	362,823
Adjusted R^2	0.074	0.248	0.076	0.262
Month FE	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: Indicator for D&O non-rout. open-mkt. focal purchases (Col. (1)) (sales (2)),
log number of such purchases (3) (sales (4))

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

The results reported in Table 6 support this conjecture. Contrary to direct competitors, trades in industry peers only have a very limited impact on focal insider trading behavior. While such peer trades predict the trading direction (not reported, but NPR has a significant, positive effect similar to column (1) in Table 3), the impact on the probability and the amount of focal insider trading is small. Especially for the estimations using the number of transactions (columns (3) and (4)), the effect is with one exception not significant and tends to be smaller in size.

4.3 Using Routine Trades to Identify Peer Effects

Cleanly identifying the effect of a group's behavior on single entities within that group is a difficult empirical task. One part of this process is to handle the reflection problem, a wording coined by Manski (1993). This means to ensure that one measures the effect of the group on the individual and not the other way around. By inserting a one-month gap between the peer trading and the focal firm trading, I believe that I can tackle this issue. The second part of the identification is concerned with the separation of the endogenous effects from the exogenous and correlated effects. In the case of this paper, exogenous effects might be similar taste for risk among insiders; for correlated effects, common shocks to all firms in an industry come to mind. Both effects could in principle cause me to observe an effect of peer trading on trading in the focal firm and thus pose a threat to identification.

One approach to identify endogenous effects more stringently is to look at the reaction to peer trades that are caused by a shock which is orthogonal to focal firm insider trading, but has an impact on peer insider trading. One category of such events is a liquidity shock to peer firm insiders, which causes them to trade stock purely for liquidity reasons. However, insiders only list the reason for trading on rare occasion using the comment line on Form 4, which makes it difficult to perfectly identify liquidity shocks. An approximation is the use of routine trades as classified following Cohen et al. (2012). The authors show that such trades do not predict future returns and should not contain any valuable information about the state of the company. If focal firm insiders react to these trades in the same way as to informative trades, it is likely that they do so due to the peer effect, as opposed to common characteristics or exposure to common, exogenous shocks. At the same time, I use these routine trades to further carve out the learning mechanism. When looking at profitability of non-routine trades in the focal firm following routine peer trades, there should be no significant effect on abnormal returns from the interaction term, since there is no valuable information to be gained from these routine trades.

The results of this analysis reveal some—albeit limited—evidence for the occurrence of a peer effect. Table 7 reports the results. For the NPR, the effect is statistically significant. When looking at purchases and sales separately, the only significant reaction to routine trades is that the occurrence and the number

Table 7: Trading Following Routine Peer Trades

This table reports regressions of insider trading measures in the focal firm on previous-month peer insider trading. The dependent variable in each column is the same trading measure as for the main explanatory variables, but for the focal firm and only considering non-routine, open market trades by D&Os. *NPR (routine trades), peer* denotes NPR of all routine peer insider trades (routine classfication following Cohen et al. (2012)). *Routine purchase (sale) dummy, peer* is an indicator for having a routine purchase (sale) transaction in the group of peer firms. *Log(N) routine purchases (sales), peer* measures the natural logarithm of the number of routine peer purchase (sale) transactions. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *QEA dummy, focal* is an indicator for a QEA taking place in the month before the focal firm trading. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm.

	(1) Net Purchase Ratio	(2) Purch. Dummy	(3) Sale Dummy	(4) Log(N) Purch.	(5) Log(N) Sale
NPR (routine trades), peer	0.024**				
	(2.257)				
Routine purchase dummy, peer		0.001	−0.002		
		(0.630)	(−0.607)		
Routine sale dummy, peer		−0.003**	0.004		
		(−2.152)	(1.463)		
Log(N) routine purchases, peer				0.001	−0.000
				(0.697)	(−0.053)
Log(N) routine sales, peer				−0.002***	−0.001
				(−2.921)	(−0.229)
Log(Market Cap (millions of USD)), focal	−0.101***	−0.013***	0.085***	−0.023***	0.133***
	(−22.916)	(−7.860)	(21.857)	(−9.320)	(15.942)
Log(B/M-Ratio), focal	0.032***	0.004***	−0.027***	0.006***	−0.063***
	(7.780)	(2.706)	(−7.461)	(2.733)	(−7.753)
Information asymmetry proxy, focal	0.097	0.009	−0.081	0.005	−0.079
	(0.834)	(0.191)	(−0.834)	(0.070)	(−0.372)
QEA dummy, focal	−0.072***	0.022***	0.088***	0.024***	0.168***
	(−26.901)	(17.314)	(33.531)	(14.766)	(31.592)
Current month return, focal	−0.337***	−0.099***	0.232***	−0.140***	0.535***
	(−38.207)	(−23.952)	(31.601)	(−23.211)	(33.587)
Past 12m-return, excl. current month, focal	−0.061***	−0.006***	0.052***	−0.011***	0.128***
	(−18.826)	(−5.927)	(18.282)	(−6.955)	(18.625)
Observations	363, 187	363, 187	363, 187	363, 187	363, 187
Adjusted R^2	0.229	0.074	0.245	0.077	0.261
Month FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: NPR of D&O non-rout. open-mkt. focal trades' volume (Col. (1)),
indicators for such purchases (2) (sales (3)), log number of such purchases (4) (sales (5))

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

Table 8: Trade Profitability Following Routine Peer Trades

This table reports regressions of returns on focal and peer firm insider trading, and control variables. The dependent variable is the focal firm's six-month CAR, in decimals. *D&O non-rout. open market purch. (sale), focal* is an indicator equal to one for non-routine, open market purchases (sales) by D&Os in the focal firm, and zero otherwise (routine trade classification following Cohen et al. (2012)). *Routine purchase (sale), peer* is an indicator for routine purchases (sales) taking place in the peer firm during the ten days preceding the focal firm trade. *D&O n.-r. o. m. purch (sale), focal* $\times$ *rout. purch. (sale), peer* denote the four interaction terms of the aforementioned trading indicators in focal and peer firms. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm.

	(1) Focal Purchases	(2) Focal Sales	(3) Both
D&O non-rout. open market purch., focal	0.130***		0.033***
	(18.796)		(3.101)
D&O non-rout. open market sale, focal		−0.138***	−0.106***
		(−18.953)	(−8.773)
Routine purchase, peer	0.008**	0.032***	0.004
	(2.235)	(2.677)	(0.140)
Routine sale, peer	−0.000	0.002	0.016
	(−0.015)	(0.190)	(0.882)
D&O n.-r. o. m. purch, focal × rout., peer	0.025**		0.029
	(2.086)		(1.203)
D&O n.-r. o. m. purch, focal × rout. sale, peer	−0.001		−0.015
	(−0.099)		(−0.882)
D&O n.-r. o. m. sale, focal × rout. purch., peer		−0.024*	0.004
		(−1.925)	(0.170)
D&O n.-r. o. m. sale, focal × rout., peer		−0.002	−0.017
		(−0.226)	(−0.902)
Log(Market Cap (millions of USD)), focal	−0.153***	−0.153***	−0.153***
	(−20.567)	(−20.540)	(−20.532)
Log(B/M-Ratio), focal	0.075***	0.075***	0.075***
	(10.483)	(10.466)	(10.464)
Information asymmetry proxy, focal	−0.746***	−0.746***	−0.746***
	(−3.667)	(−3.665)	(−3.667)
Current month return, focal	−0.930***	−0.928***	−0.928***
	(−62.274)	(−62.245)	(−62.208)
Previous year return, focal	−0.171***	−0.171***	−0.171***
	(−19.253)	(−19.249)	(−19.248)
Constant	1.275***	1.409***	1.375***
	(22.916)	(25.640)	(24.681)
Observations	279,546	279,546	279,546
Adjusted R^2	0.358	0.359	0.359
Month FE	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: 6m-CAR

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

of peer sales predicts a lower chance and number of focal firm purchases. With a p-value of 0.143, the effect of the sale dummy on focal sales is relatively close to statistical significance, and the coefficient is positive as can be expected.

In Table 8, I show the results of the abnormal returns analysis. It reveals that none of the interaction coefficients are significant which is in line with the lack of information contained in routine trades. This evidence is supportive of the learning channel.

5 Robustness Tests

5.1 Trading Activity

The first robustness check that I perform is concerned with the clustering of standard errors. Petersen (2009) argues that in a panel with companies as the unit of observation, multi-dimensional clustered standard errors lead to unbiased estimations even in the presence of time and firm effects. I report the results for my baseline regressions on trading activity using double clustering in the focal firm and month in Panel A of Table 9. The results remain largely unchanged in significance. The only notable effect on the main variables of interest is that the effect of the number of peer sales on subsequent purchases in the focal firm is no longer above the 90% significance level.

A second concern is that officers and directors might react differently to trades from different agents. In the baseline results, I include trades from all filing insiders, including large shareholders (i.e., holders of 10% or more of shares outstanding), and other corporate investors. For a second robustness test, I only include trades by the D&O group in peer firms. The results in Panel B (Tab. 9) indicate that the reaction is similar and in one case even stronger as compared to the baseline results. In particular, the effect of the peer purchase indicator on focal sales is negative and statistically significant in this specification. Apart from this, the results remain largely unchanged.

Table 9: Trade Direction - Robustness Tests

This table reports robustness tests to the analysis in Table 3, with firm *and* month clustering in Panel A and only D&O peer trades in Panel B. In Panel B, standard errors are clustered by focal firm. The dependent variable in each column is the same trading measure as for the main explanatory variables, but for the focal firm and only considering non-routine, open market trades by D&Os. *NPR, peer* denotes NPR of all peer insider trades. *Purchase (Sale) dummy, peer* is an indicator for having a purchase (sale) transaction in the group of peer firms. *Log(N) purchases (sales), peer* measures the natural logarithm of the number of peer purchase (sale) transactions. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *QEA dummy, focal* is an indicator for a QEA taking place in the month before the focal firm trading. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. In both panels, the same fixed effects are used.

Panel A: Month and Firm Clustering

	(1) Net Purchase Ratio	(2) Purch. Dummy	(3) Sale Dummy	(4) Log(N) Purch.	(5) Log(N) Sale
NPR, peer	0.013***				
	(2.764)				
Purchase dummy, peer		0.003*	−0.003		
		(1.918)	(−1.398)		
Sale dummy, peer		−0.002	−0.001		
		(−0.982)	(−0.236)		
Log(N) purchases, peer				0.002*	−0.007***
				(1.954)	(−2.881)
Log(N) sales, peer				−0.001	0.006**
				(−1.585)	(2.444)
Log(Market Cap (millions of USD)), focal	−0.101***	−0.013***	0.085***	−0.023***	0.133***
	(−20.827)	(−7.176)	(20.234)	(−8.034)	(14.456)
Log(B/M-Ratio), focal	0.032***	0.004***	−0.026***	0.006***	−0.062***
	(7.384)	(2.640)	(−7.075)	(2.612)	(−7.095)
Information asymmetry proxy, focal	0.096	0.010	−0.081	0.006	−0.071
	(0.813)	(0.188)	(−0.814)	(0.074)	(−0.327)
QEA dummy, focal	−0.072***	0.022***	0.088***	0.024***	0.169***
	(−19.560)	(13.783)	(23.945)	(11.455)	(20.622)
Current month return, focal	−0.337***	−0.099***	0.232***	−0.139***	0.534***
	(−19.996)	(−13.258)	(19.638)	(−11.560)	(18.001)
Past 12m-return, excl. current month, focal	−0.061***	−0.006***	0.052***	−0.011***	0.127***
	(−12.717)	(−5.145)	(12.543)	(−6.012)	(12.235)
Observations	363,187	363,187	363,187	363,187	363,187
Adjusted R^2	0.229	0.074	0.245	0.077	0.261

Panel B: D&O Peer Trades

	(1)	(2)	(3)	(4)	(5)
NPR of D&Os, peer	0.013***				
	(2.728)				
Purchase dummy (D&Os), peer		0.003**	−0.004*		
		(2.277)	(−1.696)		
Sale dummy (D&Os), peer		−0.002	0.000		
		(−1.232)	(0.120)		
Log(N) purchases (D&Os), peer				0.002**	−0.007**
				(2.028)	(−2.559)
Log(N) sales (D&Os), peer				−0.001	0.007***
				(−1.588)	(2.793)
Log(Market Cap (millions of USD)), focal	−0.101***	−0.013***	0.085***	−0.023***	0.133***
	(−22.883)	(−7.894)	(21.882)	(−9.294)	(15.792)
Log(B/M-Ratio), focal	0.032***	0.004***	−0.026***	0.006***	−0.062***
	(7.771)	(2.697)	(−7.459)	(2.713)	(−7.728)
Information asymmetry proxy, focal	0.097	0.010	−0.081	0.005	−0.070
	(0.838)	(0.196)	(−0.832)	(0.068)	(−0.328)
QEA dummy, focal	−0.072***	0.022***	0.088***	0.024***	0.169***
	(−26.914)	(17.328)	(33.521)	(14.750)	(31.620)
Current month return, focal	−0.337***	−0.099***	0.232***	−0.140***	0.534***
	(−38.185)	(−23.937)	(31.596)	(−23.189)	(33.552)
Past 12m-return, excl. current month, focal	−0.061***	−0.006***	0.052***	−0.011***	0.127***
	(−18.807)	(−5.924)	(18.287)	(−6.943)	(18.591)
Observations	363,187	363,187	363,187	363,187	363,187
Adjusted R^2	0.229	0.074	0.245	0.077	0.261
Month FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

t statistics in parentheses
Dependent variable: NPR of D&O non-rout. open-mkt. focal trades' volume (Col. (1)),
indicators for such purchases (2) (sales (3)), log number of such purchases (4) (sales (5))
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Trade Profitability

The first test reported in columns (1) to (3) of Table 10 re-computes the baseline regressions on trade profitability using firm and month clustering of standard errors. Similar to the first test of Sec. 5.1, the results are robust to this change in estimation method.

A second potential concern is the occurrence of common shocks to the industry which directly impact the insider trading behavior as well as the returns. In this case, I would not observe the effect of trades on returns, but of unobserved shocks. One way to alleviate such a concern is to think about the focal firm fixed effects used in the baseline specification as a controlling for unobserved characteristics of the industry. In the spirit of Hoberg and Phillips (2016), each company forms their own industry together with its peer firms in the product market space. Therefore, a firm identifier can in this case also be thought of as an industry identifier. As an alternative way to control for such industry shocks, I estimate Regression 2 including fixed effects for the focal firm's four-digit SIC industry instead of focal firm fixed effects.

In columns (4) to (6) (Tab. 10), I report the results of this procedure. Including SIC-industry fixed effects leads to a slightly more pronounced impact of focal firm trading. Specifically, focal purchases have a statistically significant and positive effect on future six-month CARs. At 11.6%, the estimated effect on returns of the combination of peer and focal firm trades is moderately higher than for the main results. In addition, the interaction of focal sales and peer purchases is significant even in the full specification of column (6). However, as discussed in Sec. 3.2, in economic terms, the positive effect of peer purchases (5%) and the negative one of the interaction term (−4.5%) balance each other.

In similar vein to the tests on trading direction and frequency, I also check for robustness of my results to a change in the sample of insider trades. Excluding trades from large shareholders which are not officers or have a seat on the board of directors, and from other corporate investors, does not lead to material changes in the results. At 9.9%, the combined effect of purchases in focal and peer firms reported in column (3) of Table 11 is virtually identical to the baseline analysis. The other three interaction terms are again insignificant.

Table 10: Trade Profitability - Robustness to Clustering and Fixed Effects

This table reports tests of robustness of the analysis in Table 4 to standard error clustering and fixed effects. The dependent variable is the focal firm's six-month CAR, in decimals. *D&O non-rout. open market purch. (sale), focal* is an indicator equal to one for non-routine, open market purchases (sales) by D&Os in the focal firm, and zero otherwise (routine trade classification following Cohen et al. (2012)). *D&O purch. (sale), peer* is an indicator for purchases (sales) by D&Os taking place in the peer firm during the ten days preceding the focal firm trade. *D&O n.-r. o. m. purch (sale), focal × purch. (sale), peer* denote the four interaction terms of the aforementioned trading indicators in focal and peer firms. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm and month in columns (1)-(3) and focal firm in cols (4)-(6).

	(1) Focal Purchases	(2) Focal Sales	(3) Both	(4) Focal Purchases	(5) Focal Sales	(6) Both
D&O non-rout. open market purch., focal	0.115^{***}		0.025	0.148^{***}		0.035^{*}
	(8.084)		(1.280)	(14.020)		(1.817)
D&O non-rout. open market sale, focal		-0.122^{***}	-0.098^{***}		-0.155^{***}	-0.121^{***}
		(−8.124)	(−4.054)		(−14.098)	(−5.725)
Purchase, peer	0.002	0.074^{***}	0.025	0.005	0.082^{***}	0.049^{**}
	(0.671)	(4.981)	(1.141)	(1.572)	(8.469)	(2.465)
Sale, peer	0.004	-0.023	0.005	0.001	-0.042^{***}	-0.022
	(0.912)	(−1.540)	(0.162)	(0.268)	(−3.425)	(−0.836)
D&O n.-r. o. m. purch., focal × purch., peer	0.071^{***}		0.049^{***}	0.075^{***}		0.033^{*}
	(4.936)		(2.724)	(7.764)		(1.896)
D&O n.-r. o. m. purch., focal × sale, peer	-0.029^{**}		-0.028	-0.044^{***}		-0.020
	(−1.978)		(−1.228)	(−3.648)		(−0.879)
D&O n.-r. o. m. sale, focal × purch., peer		-0.071^{***}	-0.023		-0.077^{***}	-0.044^{**}
		(−4.738)	(−1.060)		(−7.523)	(−2.205)
D&O n.-r. o. m. sale, focal × sale, peer		0.027^{*}	-0.001		0.043^{***}	0.023
		(1.714)	(−0.031)		(3.360)	(0.866)
Log(Market Cap (millions of USD)), focal	-0.153^{***}	-0.153^{***}	-0.152^{***}	-0.002	-0.002	-0.002
	(−13.856)	(−13.829)	(−13.826)	(−1.587)	(−1.367)	(−1.343)
Log(B/M-Ratio), focal	0.075^{***}	0.074^{***}	0.074^{***}	0.043^{***}	0.043^{***}	0.043^{***}
	(9.138)	(9.132)	(9.132)	(10.624)	(10.603)	(10.589)
Information asymmetry proxy, focal	-0.749^{***}	-0.749^{***}	-0.750^{***}	-0.321^{***}	-0.323^{***}	-0.323^{***}
	(−3.363)	(−3.362)	(−3.365)	(−2.645)	(−2.660)	(−2.664)
Current month return, focal	-0.929^{***}	-0.927^{***}	-0.926^{***}	-0.990^{***}	-0.988^{***}	-0.988^{***}
	(−32.428)	(−32.462)	(−32.443)	(−64.953)	(−64.886)	(−64.850)
Past 12m-return, excl. current month, focal	-0.171^{***}	-0.171^{***}	-0.171^{***}	-0.180^{***}	-0.180^{***}	-0.180^{***}
	(−15.509)	(−15.510)	(−15.509)	(−19.799)	(−19.793)	(−19.792)
Constant	1.271^{***}	1.389^{***}	1.363^{***}	0.082^{***}	0.235^{***}	0.200^{***}
	(15.303)	(16.062)	(15.441)	(6.386)	(15.076)	(8.304)
Observations	279,546	279,546	279,546	279,620	279,620	279,620
Adjusted R^2	0.359	0.359	0.359	0.257	0.258	0.258
Month FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*No*	*No*	*No*
Industry FE	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: 6m-CAR

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

Table 11: Trade Profitability - Robustness to Trade Measure

This table reports tests of robustness of the analysis in Table 4 to the peer trades used. The dependent variable is the focal firm's six-month CAR, in decimals. *D&O non-rout. open market purch. (sale), focal* is an indicator equal to one for non-routine, open market purchases (sales) by D&Os in the focal firm, and zero otherwise (routine trade classification following Cohen et al. (2012)). *D&O purch. (sale), peer* is an indicator for purchases (sales) by D&Os taking place in the peer firm during the ten days preceding the focal firm trade. *D&O n.-r. o. m. purch (sale), focal × (purch.) (sale), peer* denote the four interaction terms of the aforementioned trading indicators in focal and peer firms. *Log(Market Cap), focal* is the natural logarithm of the focal firm market capitalization in millions of USD in the month preceding the focal firm trading. *Log(B/M-Ratio), focal* denotes the natural logarithm of the B/M Ratio. *Information asymmetry proxy, focal* is the magnitude of CARs around QEAs over the past five years. *Current month (previous year) return, focal* is the return in the month (the eleven months before this month) preceding the focal trading, in decimals. The sample period is Sep 2002 to Dec 2021. Standard errors are clustered by focal firm.

	(1) Focal Purchases	(2) Focal Sales	(3) Both
D&O non-rout. open market purch., focal	0.116***		0.030*
	(11.536)		(1.661)
D&O non-rout. open market sale, focal		−0.123***	−0.094***
		(−11.729)	(−4.802)
D&O purch., peer	0.002	0.071***	0.029
	(0.659)	(7.817)	(1.506)
D&O sale, peer	0.004	−0.020*	0.008
	(1.185)	(−1.832)	(0.314)
D&O n.-r. o. m. purch, focal × peer	0.068***		0.042**
	(7.594)		(2.425)
D&O n.-r. o. m. purch, focal × sale, peer	−0.027**		−0.028
	(−2.500)		(−1.238)
D&O n.-r. o. m. sale, focal × purch., peer		−0.068***	−0.027
		(−7.285)	(−1.409)
D&O n.-r. o. m. sale, focal × peer		0.024**	−0.003
		(2.143)	(−0.140)
Log(Market Cap (millions of USD)), focal	−0.153***	−0.152***	−0.152***
	(−20.582)	(−20.548)	(−20.543)
Log(B/M-Ratio), focal	0.075***	0.074***	0.074***
	(10.470)	(10.451)	(10.451)
Information asymmetry proxy, focal	−0.749***	−0.749***	−0.749***
	(−3.685)	(−3.682)	(−3.685)
Current month return, focal	−0.929***	−0.927***	−0.926***
	(−62.222)	(−62.187)	(−62.149)
Past 12m-return, excl. current month, focal	−0.171***	−0.171***	−0.171***
	(−19.254)	(−19.252)	(−19.249)
Constant	1.270***	1.389***	1.359***
	(22.832)	(25.295)	(23.504)
Observations	279,546	279,546	279,546
Adjusted R^2	0.359	0.359	0.359
Month FE	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*

t statistics in parentheses

Dependent variable: 6m-CAR

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

6 Conclusion

In this paper, I analyze non-routine, open market trades by directors and officers following insider trades in product-market peer companies. I find that there is a reaction in the form of trading in the same direction as these peer insider trades at both the intensive and the extensive margin. A second set of results suggests higher abnormal returns to purchases closely following peer purchases. This evidence is consistent with a peer effect, but also with intra-industry information spillovers where the component which is applicable industry-wide outweighs the peer-firm specific component.

In further analyses, I establish that directors and officers in smaller firms react to trades taken in larger peer firms, but not vice versa. Such cross-sectional variation can be expected for peer effects as well as learning (compare to e.g., Leary and Roberts (2014)). To move towards a causal identification of endogenous effects (Manski (1993)), I use a subset of trades which are likely to be caused by reasons independent to the focal firm (e.g., liquidity of peer insiders). A reaction to such uninformative trades would help distinguish the peer effect from common shocks or characteristics. However, I only find limited evidence for such an effect. The lack of effect on returns for such trades on the other hand is in line with the learning channel.

Lastly, my analysis of reactions to different sets of peer groups specifies the extent to which learning from peer trades about industry-specific factors is possible. While I find a reaction to trades in close product-market peers as identified by Hoberg and Phillips (2016), there is no significant effect to SIC-peers, which are more likely to operate in more distant markets to the focal firms. This adds to papers discussing managerial expertise such as Ben-David et al. (2019) or Hutton et al. (2012). My findings suggest that insider trading in peer firms can be value-relevant for the focal firm. The results could also help to refine trading strategies based on the publication of insider trades.

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How Do Founders and Venture Capitalists Sell Their Shares after the IPO? *

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Abstract

This paper analyzes insider sales patterns in newly public firms. Based on a hand-collected sample, we document that founders usually sell their restricted shares in the open market as governed by Rule 144. We argue that the rule has several weaknesses. Namely, Form 144 is often filed concurrently with Form 4, making the former obsolete. The sales limits are set generously, and financial venture capitalists frequently circumvent the rule by distributing shares in-kind. Both transaction types generate abnormal returns. This is the case despite expanded disclosure mandates over the past two decades and indicates the necessity for further efforts towards shareholder protection in newly public firms.

Keywords: IPO, Founder, Venture Capitalist, Insider Trading, Information Asymmetry
JEL Classification Numbers: G14, G24, G32, G38

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1 Introduction

Corporate insiders have privileged access to private information about their firm and are hence subject to increased public scrutiny, regulation, and restrictions regarding their trading activities. Many large and established public firms have introduced blackout periods and strict internal rules about when and how much insiders are allowed to sell so that they are not exposed to risks related to insider trading (Bettis et al. (2000)). As a consequence, Cohen et al. (2012) find that most insider trades are routine and do not contain much new information. Similarly, Jeng et al. (2003) show that insider sales in large public corporations are driven by many motives including liquidity or consumption needs of executives paid in stock and are not predictive of future returns.

In newly public firms, the information asymmetry between insiders and other shareholders tends to be higher than in more mature companies. There is less information about the company, business models are still changing, and profits are often not meaningful. Much of the valuation comes from future growth options. At the same time, ownership levels by different groups of insiders are not yet in steady state. Founders and early employees typically retain much of their stake in the Initial Public Offering (IPO) so that most shares are sold by the company and the money raised in the IPO can be used for investment purposes. Founders and other insiders then reduce their stakes after the IPO lockup period expires (e.g., Helwege et al. (2007)). Similarly, venture capitalists (VCs) often retain their shares in the IPO and distribute them or sell them afterwards when their partnerships reach the end of their lives. Early-stage investors and regulators have recognized these two issues and closely monitor and even restrict sales of securities early in the life of a public firm.

The SEC, in particular, has put a legal framework in place to protect shareholders of young public companies. Stock that is held by insiders in the transition from private to public ownership is considered restricted stock. These are shares sold by the issuer, among other transactions, in private placements, employee stock benefit plans, or in exchange for the provision of seed money. All the shares distributed prior to the IPO are therefore restricted and can only be sold under the circumstances laid out in SEC's Rule 144.[1]

In this paper, we first ask how binding Rule 144 is for a variety of groups of insiders, and how successful the regulation in place is to prevent insiders from trading on their private information. We use a sample of 228 U.S. firms which had their IPO between 2016 and 2020. For these companies, we manually match ownership information from SEC's 14A and 424B4 filings with trades filed on Form 4

[1] Rule 144 posits five conditions which need to be met before restricted shares can be sold: a holding period of six months, adequate public information on the issuing company, sufficient trading volume, trades have to be ordinary brokerage transactions, and the intention to sell has to be filed with the SEC.

and new data from Form 144 filings announcing sales of restricted stock from the Washington Service. Most important for our work are the volume limitations and the requirement of filing the intention to sell restricted stock via Form 144 if sales exceed 5'000 shares or USD 50'000 (see Table 1). The volume cap is set to 1% of shares outstanding and the four-week average of weekly trading volume. We find that the restrictions are rarely binding, and that corporate insiders sell much less than what they could under Rule 144. In addition, an important insider, venture capitalists, can circumvent Rule 144 restrictions by distributing the shares in-kind to their limited partners instead of selling in the open market. After a holding period of six months to which the time when the shares were held by the venture capitalist are tacked on, the recipients can sell the shares. In reality, this means that usually they can sell immediately upon receipt.

In the second part of the paper, we examine the timing of insider sales for different groups of insiders at the IPO. One may be concerned about more trading on private information in young firms for a variety of reasons. First, the corporate governance framework of young public firms is less developed (see e.g., Krishnan et al. (2011) or Baker and Gompers (2003)), be it the organizational structure (e.g., no separation of the CEO and the chairman function), or the lack of blackout windows for insider trading (Bettis et al. (2000)). Also, the decision to go public initiates a transition of the company, which could temporarily increase the information advantage of insiders before eventually decreasing it (Iliev and Lowry (2020)). The lower amount of analyst attention and of public information that has been produced on the company further increase the asymmetry. The information edge provides insiders with an opportunity to strategically sell the shares they obtained before going public and earn abnormal returns, at the expense of the other market participants. Such insider trading threatens the fairness of markets (see, e.g., Bhattacharya (2014)) for a review) and can lead to a welfare loss (Ausubel (1990)).

We document abnormal returns to open market sales and in-kind distributions by founders and venture capitalists, indicating private information. For open market sales by founders, we find a CAR of 9.6% over 100 trading days after the trade. For VCs' in-kind distributions, the magnitude over the same period is even stronger at almost 20%. Compared to Avci et al. (2024) and Gompers and Lerner (1998), whose samples contain more mature firms, we find stronger effects. We suggest that they are driven by the high information asymmetry in newly public firms. We also shed light on interaction effects between venture capitalists and founders. Our findings point towards an information flow between the two groups.

Our paper is related to several strands of the literature. A large literature examines the cross-sectional variation in future stock returns as a function of past insider trading activity. Many of these works, including those by Jaffe (1974), Seyhun (1986) and (2000), Rozeff and Zaman (1988), Bettis et al.

(1997), and Lakonishok and Lee (2001), focus on the abnormal returns associated with firms based on various measures of insider trading activity over specific time periods. Seyhun (2000) reviews this body of evidence, concluding that several insider trading strategies generate profits. Jeng et al. (2003) adopt a performance evaluation approach, showing that insider purchases yield abnormal returns exceeding 6% annually, while insider sales do not result in significant abnormal returns. In a more general setting, Cohen et al. (2012) show that when insiders trade differently from their usual routine, these trades tend to be profitable. Avci et al. (2024) report that insiders can hide valuable transactions in plain sight by filing them as *other transactions*. We expand this literature by documenting significant abnormal returns to sales and in-kind distributions in newly public firms.

Another strand of literature analyzes the lockup period and the consequences of its expiration. In Brav and Gompers (2003)' sample, the duration of the lockup period depends on the threat of moral hazard. Field and Hanka (2001) find negative abnormal returns and a spike in trading volume at the expiration date. The volume permanently increases thereafter. They also show that venture capitalists sell more aggressively than other investors.

Several papers are concerned with ownership at IPO and how it evolves over time once a firm is public. Most notably, Helwege et al. (2007) show that in the majority of firms, insider ownership drops below 20% after ten years. The effect is stronger in highly valued firms with liquid stock and strong stock market performance. Paeglis and Veeren (2013) examine how founders and VCs influence each other and find that VC-exits increase founder influence in newly public firms. Broughman and Fried (2020) analyze founder roles at the IPO. They show that at the IPO, only 7% of founders hold strong control over their companies at this time. Our paper provides a more detailed overview on ownership evolution and highlights the difference between financial and strategic VCs. Furthermore, we link ownership to trading and document the different sales strategies among the insider groups.

Most closely related to our work is Gompers and Lerner (1998). They examine the in-kind distributions of VC shares to their limited partners and find an inverted V-shape to in-kind distributions by venture capitalists that are exiting their portfolio companies. Relative to their study, we examine all corporate insiders and separate founders, strategic investors, and venture capitalists. We provide an update to their work, which is due because regulation has changed in the meantime. Our results indicate that increased disclosure did not succeed at reducing returns to informed trading.

The remainder of the paper is structured as follows. Section 2 develops testable hypotheses. In Section 3, we describe the data sources, sample selection, and offer summary statistics. In Section 4, we document the evolution of insider ownership over time. In Section 5, we discuss the restrictions on restricted stock according to Rule 144, examine maximum possible sales under Rule 144, and compare them with actual sales of insiders. In Section 6, we analyze the timing of insider sales by group. We conclude in Section 7.

2 Shareholder Protection in Newly Public Companies

In newly public firms, the information asymmetry between insiders and other shareholders tends to be higher than in more mature companies. This is due to a variety of factors. First, newly public firms are usually younger and their corporate governance is less strict (see, e.g., Krishnan et al. (2011) or Baker and Gompers (2003)). This could concern the organisation structure (e.g., no separation of the CEO and the chairman function), or the lack of blackout windows for insider trading (Bettis et al. (2000)). Also, the decision to go public initiates a transition of the company, which could temporarily increase the information advantage of insiders before eventually decreasing it (Iliev and Lowry (2020)). The lower amount of analyst attention and of public information that has been produced on the company further increase the asymmetry. The information edge provides insiders with an opportunity to strategically sell the shares they obtained before going public and earn abnormal returns, at the expense of the other market participants. Such insider trading threatens the fairness of markets (see e.g., Bhattacharya (2014) for a review) and can lead to a welfare loss (Ausubel (1990)).

The regulators have acknowledged this issue and a legal framework is in place to protect shareholders. In principle, restrictions and filing requirements increase with the amount of private information that a person or entity is expected to have access to. The first important instrument is restricted stock. These are shares sold by the issuer, among other transactions in private placements, employee stock benefit plans, or in exchange for the provision of seed money. All the shares distributed prior to the IPO are therefore restricted and can only be sold under the circumstances laid out in SEC's Rule 144. Only after three months of not being an affiliate[2] and an overall holding period of six months can the shares be sold freely. We summarize the rules for affiliates in Table 1 (simplified version based on Thomson Reuters). Most important for our work are the volume limitations and the requirement of filing the intention to sell restricted stock via Form 144 if sales exceed 5'000 shares or USD 50'000. The volume cap is set to 1% of shares outstanding and the four-week average of weekly trading volume.

[2]An affiliate is defined by the Securities Act of 1933 as "a person that directly, or indirectly through one or more intermediaries, controls or is controlled by, or is under common control with, the person specified". Generally, the D&O group and shareholders owning 10% or more of a firm's stock are considered affiliates (SEC Correspondence).

Table 1: Form 144 Requirements for (Non-)Affiliates

This table lists the filing requirements stated by Rule 144 for affiliates and non-affiliates. Affiliates are defined as persons who have the power to control or direct management and policies of the issuer. Officers and directors are generally considered affiliates, the same is usually (i.e. with some exemptions) the case for stockholders owning 10% or more of the issuer's stock. The table is a simplified version of the information provided by Thomson Reuters.

Requirement	Seller is an affiliate	Seller is not affiliated since 3m+
Holding period	6 months	6 months
Volume limitations	Must not exceed the greater of: 1% of shares outstanding *and* 4-week avg. of weekly trading vol.	None
Form 144 notice	Required if sale > 5k shares or $50k	Not required

Figure 1 provides an overview on the filing requirements based on whether an insider is active among a company's group of Directors and Officers (D&O), and depending on the amount of company stock held. As long as an insider is considered affiliate, they have to file Form 144 before selling restricted shares, Form 4 for all actual sales, and the company has to publish their ownership stake in its 14A filing. After an insider has left the company, the requirement to report trades only applies for ownership above 10% of shares outstanding. Ownership still has to be reported as long as it exceeds 5%. One notable characteristic of this set of rules is that for non-affiliates with ownership below 10%, trades do not have to be published anymore. This allows former decision makers to sell their shares quickly and inconspicuously. An example is John L. MacFarlane, the co-founder and former CEO of Sonos Inc. Within around a month after the end of the lockup period, he reduced his ownership from 7.2% on 15 February 2019 to below 5% on 1 March 2019.[3] Even though it seems reasonable to assume that as co-founder and third-largest shareholder (according to the proxy statement on 18 January 2019), he was well-informed about the company, he was able to do so without displaying any of his trades.

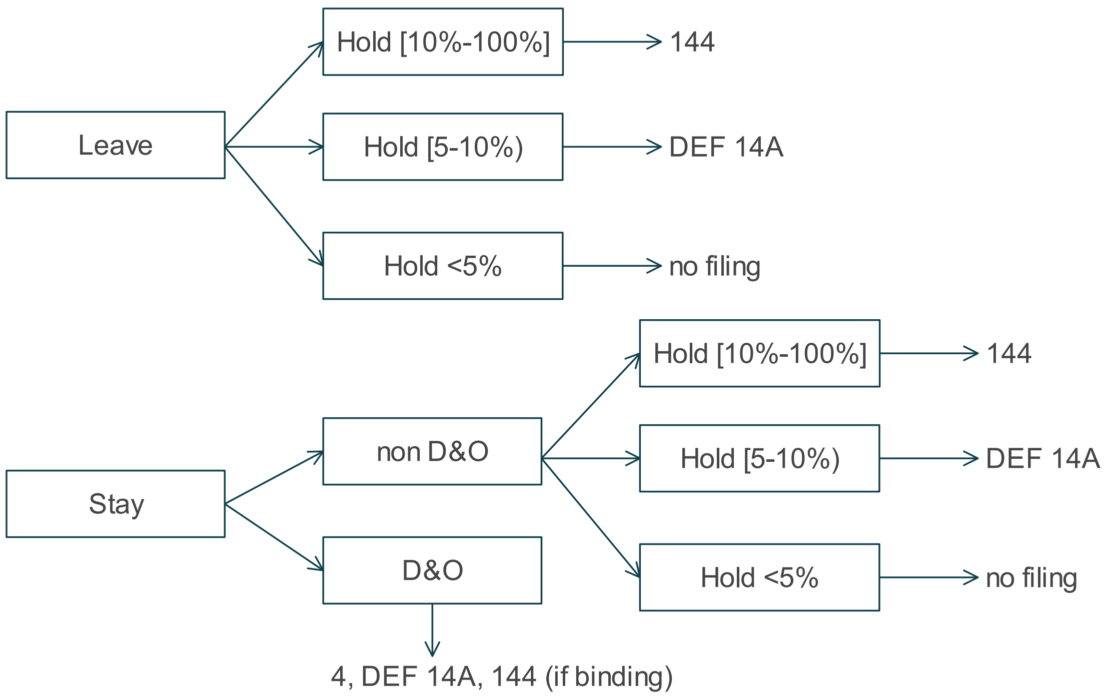
Besides restricted shares and disclosure obligations, the lockup period is another important instrument for shareholder protection. It is not mandated by law, but instead agreed upon between the IPO firm and the underwriters. In fact, *all* IPOs in our sample have a lockup period of 180 days, starting at the IPO. Brav and Gompers (2003) show that the lockup serves as a tool for insiders to signal their commitment. Field and Hanka (2001) report negative abnormal returns around the end of the lockup period and a permanent increase in trading volume as more stock previously held by insiders enters the

[3]Further reductions cannot be observed since there are no more filing requirements.

free float.

Figure 1: Filing Requirements by Affiliate Status

This figure depicts the filing requirements with the SEC, depending on the affiliate status. *Stay* denotes that an insider is either a director or an officer. *Leave* means no such position is held.



The third instrument is not geared specifically towards newly public firms, but is relevant for our analysis nevertheless. It is the Sarbanes-Oxley Act of 2002, which mandates the timely publication of insiders' trading activity. Namely, Form 4 has to be filed within two business days, which is much shorter than before when insiders had time to file until ten days after the end of the calendar month in which the trade took place. Besides open market trades, this has an impact on in-kind distributions. Gompers and Lerner (1998) obtain their data directly from the institutional investors. The current view of the SEC however is that the distributions are not exempt from the publication mandate.[4] This allows us to collect the data on distributions from EDGAR, just as we do for the open market trades.

Despite these described regulations, there is evidence consistent with insiders trading strategically with shares of their company to earn abnormal results. In a more general setting, Cohen et al. (2012) show that when insiders trade differently from their usual routine, these trades tend to be profitable. More closely related to our work, Gompers and Lerner (1998) find an inverted V-shape to in-kind distributions by venture capitalists (VCs) that are exiting their portfolio companies. Based on this, we

[4]See comment on Section 217, Rule 16a-9.

posit the following, testable hypothesis:

Hypothesis 1: *In newly public firms, where information asymmetry tends to be high, a lack of regulatory protection allows insiders to generate abnormal returns at the expense of non-insider investors when selling or distributing restricted stock.*

We conduct our analysis using a hand-collected sample of newly public, VC-backed firms in which at least one founder and one VC are still active at the IPO. This sample provides a formidable setting to study strategic selling of insiders in firms with high information asymmetry. Including ownership, intention to trade (filed via Form 144), and trades for founders, strategic and financial VCs, and the D&O group allows for a holistic analysis. We analyze the four groups separately because we expect their incentives to differ. For example, founders often have invested most of their personal wealth in the company. Given that they are still active in the company, their goals may not be purely financial, instead they might also want to enjoy private benefits of control (see e.g., Jensen and Meckling (1976)). Financial VCs on the other hand tend to hold a larger position at IPO (see Figure 2), but their investments are more diversified. In contrast to founders, their investment horizon is limited and they can come under pressure to repay their limited partners. We show that these differences impact the roles that the groups have within a company as well as the ownership levels. This leads to our second hypothesis:

Hypothesis 2: *Due to differential access to inside information and motivations to reduce the holdings, there are differences between the founders and venture capitalists in the selling behavior, namely the transaction types used to offload shares, and the speed of ownership decrease.*

These differences in turn have implications for shareholder protection, since not all of the regulators' tools are useful to curb strategic selling, depending on how the sales are conducted.

3 Data and Sample Statistics

3.1 Sample Construction

To obtain our sample, we start with all IPOs that took place in the U.S. between 2016 and 2020.[5] We match all the newly public firms with the CRSP/Compustat merged database (CCM) for firm fundamentals, and with Form 4, Form 144, 424B4, and 14A filings. We obtain Form 4, 424B4 and 14A data from SEC's EDGAR database and Form 144 filings from the The Washington Service. We use VentureXpert to identify venture capitalists (VCs).

[5] We are grateful to Jay Ritter for providing us with this data. His selection includes IPOs with an offer price of at least USD 5.00, excluding ADRs, unit offers, closed-end funds, REITs, natural resource limited partnerships, small best efforts offers, banks and S&Ls, and stocks not listed on CRSP (CRSP includes AMEX, NYSE, and NASDAQ stocks).

To ensure that we only capture firms with meaningful founder and VC involvement at IPO, we introduce restrictions based on the ownership table of 424B4 filings. Namely, we exclude firms in which none of the founders are listed in the table and in which there are no VCs listed as *5% or more stockholders*. The number of firms excluded in each step is listed in Table 2.

Table 2: Evolution of Sample Size

This table shows the evolution of the number of sample firms. The upper bound of companies excluded by each restriction is given by the lines *Firms dropped due to...*. The lower bound is given by the lines *Firms dropped exclusively due to...*.

Panel A: Stock Sample	
	N
Firms from Jay Ritter	**591**
Firms dropped due to founder-related issues	142
Firms dropped exclusively due to founder-related issues	*57*
Firms dropped due to VC-related issues	205
Firms dropped exclusively due to VC-related issues	*88*
Firms dropped due to having multiple share classes	126
Firms dropped exclusively due to having multiple share classes	*76*
Firms dropped due to having major events	24
Firms dropped exclusively due to having major events	*11*
Firms in the sample	**228**

Panel B: Flow Sample	
Firms w/o F144 filings by any of our entities	*59*
Firms w/o F4 filings by any of our entities	*39*
Firms with both types of filings	**157**

To correctly identify founders, we first rely on mentions in 424B4 filings. If founders are not listed there, we additionally rely on the company website, or external sources such as the Wall Street Journal, Forbes, Crunchbase, or LinkedIn. This yields one or multiple founders for most of the firms in the original sample from Jay Ritter. The IPOs where we cannot identify any founder are either older firms which were privately held before, spin-offs, or incorporated outside the United States.[6] In addition to the founders themselves, we also assign their legal entities (e.g., trusts, investment companies) to them. We do so by using the ownership tables' footnotes. We manually assign Central Index Keys (CIKs) to founders and their entities from EDGAR, either through their list of all CIKs or through the firm's EDGAR page.

For venture capitalists, we manually check the VentureXpert entries against the 424B4-shareholder list and select those which are in both tables. We find that this procedure usually captures the vast majority or even all of the institutional blockholders at IPO and allows us to exclude large investors which enter at the IPO, but are not providers of venture capital, such as Blackrock. For the selected VCs, we manually look up the CIKs from all entities mentioned in the 424B4-footnotes. For the distinction between financial and strategic VCs, we also rely on VentureXpert. Entities classified as *Investment Management Firm*, *Private Equity Advisor or Fund of Funds*, *Private Equity Firm* (most populated group), and *Bank Affiliated* are aggregated as financial VCs, while those classified as *Corporate PE/Venture*, *Government Affiliated Program*, *University Program*, and *Endowment, Foundation or Pension Fund* are subsumed under strategic VCs, based on the assumption that their interests are not limited to financial performance. In some cases, decision makers in VCs are listed as founders in the portfolio company (a prominent example is Noubar Afeyan of Flagship Pioneering). This complicates the distinction between a legal entity belonging to a founder, and a VC. In such cases, we treat the entities as VCs if they are listed in VentureXpert and as founder-related if they are not (see, e.g., the McClain Charitable Remainder Unitrust in the company Sailpoint Technologies Holdings, Inc., which is an entity of the founder Mark McClain). In an alternative specification, we assign VCs that we deem to be controlled by the founder (based on footnotes in 424B4 or 14A) to founder- instead of VC-ownership (see Figures 2a and 2b for the difference in ownership). In a last step, we restrict our sample to IPOs with one share class[7] and we exclude companies for which we know that a major event which has fundamentally altered the company's structure has taken place. These events include transactions such as mergers resulting in a new company, but not SEOs or takeovers where our firm was the acquirer.

For the 228 companies which are left, we then protocol the development of ownership stakes for founders, their related entities, VCs, and the group of D&Os using the proxy statements (DEF 14A) up to and including fiscal year 2021. We also distinguish founders from founder-CEOs. When analyzing trades, we use the same sample. However, we drop trading days of entities after the latest of the following

[6] Examples are Levi Strauss & Co. (founded in 1853), Cambium Networks (spin-off from Motorola), or Afya Ltd. (incorporated in Brazil).

[7] Based on our own classification checked against Jay Ritter's. We exclude companies with multiple share classes because they complicate the linking between ownership and trading beyond the additional benefit of including them.

three dates: 90 days after their last mentioning in a 14A-filing, because we cannot track their holdings anymore; 90 days after the last filing of Form 144 (F144); the day after the last filing of Form 4 (F4).

We assign F4- and F144 filings to each entity using their CIK(s).[8] We do so by matching on the single entities first, then aggregate to the level of entities as listed in 14A (e.g., all entities of Flagship Pioneering, which might file Form 4 for their trades are aggregated under one entity. Same for indirect trades filed by fund managers). In a next step, we aggregate economically linked entities (e.g., a founder and their investment company which is listed separately in 14A or 424B4 filings). We make sure not to double-count ownership or trading activities.
We keep all F144-filings. For Form 4, we consider open market dispositions and filings with code *J*. This code denotes *other transactions* and includes in-kind distributions.

3.2 Sample Description

3.2.1 Firm Characteristics

The resulting sample contains companies which have been public for no more than five years. Naturally, these young companies differ from the CRSP-Compustat universe, which includes more mature firms. Table 3 reports the differences along a variety of firm characteristics. First, our sample firms tend to be smaller. Their capital structure differs, in that they are significantly less indebted. This reflects the fact that private companies are more restricted in their access to debt markets, which is mainly due to the high information asymmetry (see, e.g., Ewens and Farre-Mensa (2022)) and forces them to rely more heavily on equity. Another reason for the low leverage ratio is that our sample firms have fewer assets to pledge, which manifests in a significantly lower tangibility. Also, constituents of our IPO sample hold significantly more cash relative to their total assets. One reason for this phenomenon could be the cash influx from the IPO, another possibility is that the holdings are again a consequence of restricted access to capital, which mandates a more conservative financial policy.

[8] Some Form 144 filings do not have a CIK, because the filing entity does not have one. In these cases we match by name, which we manually look up in the Washington Service database.

Table 3: Sample Comparison: Our IPO Sample vs. CRSP-Compustat

This table reports a comparison of summary statistics between CRSP-Compustat merged database and the IPO sample used in our paper. The sample covers the fiscal years 2016-2020. The variables are constructed as described in the Appendix in Table A1. The column labelled "IPO Sample" contains firms that constitute our sample of newly public firms. The column labelled "CRSP-Compustat" contains all firms in the CRSP-Compustat merged database. All continuous variables are winsorized at the 1% and 99% levels, respectively. ***, **, and * indicate statistical significance of the underlying coefficient at the 1%, 5%, and 10% levels, respectively (based on a t-test allowing for unequal variances, and a non-parametric Mann-Whitney-Wilcoxon rank-sum test of equality of distributions, respectively).

	Means		Medians	
Variables	IPO Sample	CRSP-Compustat	IPO Sample	CRSP-Compustat
Ln(Assets)	5.48	6.60***	5.44	6.64***
Debt/Assets	0.13	0.26***	0.05	0.23***
Capex/Assets	0.02	0.04***	0.01	0.02***
Cash/Assets	0.72	0.26***	0.83	0.14***
EBIT/Assets	-0.33	-0.10***	-0.23	0.04***
PP&E/Assets	0.08	0.25***	0.04	0.14***
Return on assets	-0.33	-0.11***	-0.24	0.03***

Our sample is dominated by IPOs in the Health Care sector. More than half are active in Biotech (54.11%), and another 9% are Pharmaceutical firms. The third big group are Software developers with roughly 10%. This is consistent with the higher IPO propensity for healthcare companies following the Food and Drug Administration Amendments Act of 2007 (FDAAA) (see Aghamolla and Thakor (2022)). It also helps to explain the lower profitability of our sample firms, since especially Biotech firms often do not have any significant earnings when they go public (Lo and Thakor (2022)).
In the appendix (Table A2), we split our sample by occurrence of Form 144 filings. The two subgroups are similar in all characteristics except for size, where firms with no Form 144 filers tend to be smaller.

3.2.2 Sales

In Table 4 we present summary statistics on the three types of filings, split by insider group. Additionally, Panel D lists meaningful interactions between the filing types. Since the linking between the different databases is intensive in manual labor, we do not consider the trades by directors and officers, except if they are founders or if the VCs are the beneficial owners in a trade.

We only consider trades made after the expiration of the lockup period. The annualized values are calculated as the value per sample day, multiplied by 252, the number of trading days in a typical year.

For example, *F4-filings per year* for founders is the number of trades filed by founders, divided by the number of sample days that we have collected for founders, times 252. The variable *Founder F4-trades / year bef. in-kind* in Panel D is calculated in the same spirit, but only Form 4 filings by founders within ten trading days prior to an in-kind distribution are considered. The variable *Frac. of F144-quota sold immediately* denotes the fraction of Form 144 filings, for which the filer sells the entire capacity on the same trading day. A F144 filing opens a 90-day window in which the filed amount can be sold. Further filings within such a window lead to an extension. For the calculation of *F4-filings per F144*, we look at windows of consecutive days in which sales of restricted securities are possible and calculate the ratio of F4 to F144 filings. If for example a founder files Form 144 twice within ten days, the numerator is the number of actual sale transactions within this 100-day window.

The table reveals marked differences between founders, financial, and strategic VCs. The three groups differ in the frequency at which they sell, in the typical trade size, and in the instrument that they prefer to use:

Founders are the most frequent filers of Form 144, in absolute and in relative terms. Their transaction size however is considerably smaller than for both VC groups. The median filing amounts to less than USD 1m. These filings usually cover the subsequent open market sales, where the median trade size at USD 383'100 is below half of the amount intended to sell. With an average of 2.27 sale transactions per F144 filing, across our entire sample the filed capacity is quite precisely utilized. This indicates that founders rarely buy additional stock that they could sell without restrictions, but instead obtain their holdings either before the IPO or later on through Restricted Stock Units (RSUs) or Performance Stock Units (PSUs). Panel D—most pronounced for founders—unveils a weakness of Rule 144: in half of the cases in which the intention to sell is filed, the entire amount filed for is immediately sold on the same day. This makes the filing of the intention obsolete for most shareholders as it leaves them with no time to process the information. Form 144 filings can take place at any time during the day and we find examples where the intention to sell was accepted by the SEC in the closing minutes of the same trading day during which the actual sale was executed. The electronic filing requirement enforced since April 2023 allows investors to access the information more quickly, but does not resolve this issue.

For the sake of completeness, we report the transactions with code J for the founders, even though they do not make in-kind distributions. We find that these transactions are mostly connected to RSUs or PSUs, e.g., when they vest, or when they are cancelled because performance goals are not reached. One of the largest transactions is due to a change in the founder group, where shares are transferred from a jointly held investment company to separate entities of each owner.[9]

[9]See filings on EDGAR for Unity Software on 15 October 2021.

Table 4: Summary Statistics on Trading Behavior

This table presents summary statistics on trading behavior by insider group. Panel A shows information on open market trades filed via SEC Form 4 (transaction code S), Panel B is on in-kind distributions filed via Form 4 (J), Panel C on filings of Form 144. Panel D displays interactions between filing types and insider groups. Annual values are calculated based on the number of sample days per group, scaled by 252. *Frac. of F144-quota sold immediately* is the ratio of F144 filings for which there is an open market sale for the full filing amount on the same day by the same entity. *F4-filings per F144* is calculated per consecutive F144 window as the ratio of open market sale transactions and F144-filings (as there can be multiple consecutive F144-filings within a 90d-window, which will extend said window).

	Founders			Financial VCs			Strategic VCs		
	N	Mean	Median	N	Mean	Median	N	Mean	Median
Panel A: Open Market Sales									
Open market shares sold	3,464	62,326	10,000	1,652	274,137	25,000	102	512,979	7,100
Open market USD amount	3,461	2,470,276	383,100	1,652	10,590,691	896,044	102	32,906,543	184,413
F4-filings per year		2.95			.743			.234	
Panel B: In-kind Distributions									
In-kind trx. shares sold	36	1,820,749	319,531	282	1,951,519	802,662	7	1,071,293	1,160,430
In-kind trx. USD amount	36	143,535,999	5,975,207	280	76,708,679	28,426,124	7	92,351,518	84,443,632
In-kind trx. per year		.0307			.126			.0161	
Panel C: Form 144 Filings									
F144 shares filed	1,632	136,204	20,000	597	490,217	130,972	31	1,024,228	489,819
F144 USD amount	1,632	5,264,224	820,768	597	17,721,953	4,135,998	31	110,099,876	9,619,860
F144-filings per year		1.39			.268			.0712	
Panel D: Interactions									
Founder F4-trades / year bef. in-kind		8.88							
Frac. of F144-quota sold immediately		.513			.201			.0313	
F4-filings per F144		2.27	1.33		2.24	1		1.25	1

In contrast to founders, financial VCs are less likely to sell on the open market, but more likely to make in-kind distributions to their limited partners. Founders report roughly four times as many open market sales as financial VCs. Accordingly, venture capitalists also file F144 less frequently. It seems that when they file however, they file for much larger amounts, not only relative to founders, but also relative to the actual sales (the median F144 filing amounts to USD 4.14m, open market sales to USD 896'044). We interpret this as either financial VCs announcing the intention to sell more generously than founders who are more likely to use up the full capacity. Or that VCs are more restricted by stock liquidity, which hampers their sale capacity. In accordance with the liquidity hypothesis, VCs are also less likely to exhaust the capacity immediately, we only observe this for a fifth of all filings. With in-kind distributions, a VC shifts the actual sale decision to its limited partners. Unsurprisingly, these transactions are considerably larger than open market sales, the median value lies at more than USD 28m.

Compared to the other two groups who actively use their sale type of choice, strategic VCs typically keep their pre-IPO investment over a longer period of time (compare to e.g., Fig. 3). We conjecture that this is in connection with their different goals compared to e.g., financial VCs. For example, big pharmaceutical companies often have venture capital subsidiaries which allow them to have a foot in the door early on in possible new competitors. This facilitates acquisitions later on, either to include the product into their own portfolio, or simply to "kill" competition (Cunningham et al. (2021)). In fact, when there is strategic VC involvement at IPO, the probability that the firm exits our sample by Mergers & Acquisitions (M&A) transaction increases from 12.9% to 18.2%. The different goals manifest in lower selling activity, especially for in-kind distributions compared to financial VCs. This is intuitive, since for a strategic buyer, there is no incentive to distribute the shares to the mother company. All seven transaction that we find in our sample are from F-Prime Capital Partners, a subsidiary of Fidelity and therefore closer to a financial VC, despite the differing classification. When strategic VCs sell, they do so on the open market. The high average sale amount compared to the low median points to the existence of a few big transactions. This observation is consistent with possible liquidity restrictions, since strategic VCs tend to sell at later points in time (relative to the IPO), when the volume is larger.

Overall, these differences are supportive of our second hypothesis and motivate researching the three groups separately. Especially between the VC groups we find remarkable differences which are often overlooked in the literature when all VCs are subsumed in one group. The different instruments which are primarily used have differing implications for shareholder protection, depending on which groups are present in a company. Besides looking at the groups separately, we argue that interactions between the insiders have to be taken into account, since they can pose a threat to the fairness in stock markets.

We present some descriptive evidence for this in Panel D, where we show that founders are more active in the days ahead of in-kind distributions. Namely, they file almost thrice the number of Form 4 as compared to the rest of the sample period. This is consistent with private information exchange between the groups, which allows founders to front-run the distributions with their own sales. In Chapter 6, we investigate the consequences of this behavior in more detail.

4 Evolution of Insider Holdings

4.1 Holdings at IPO

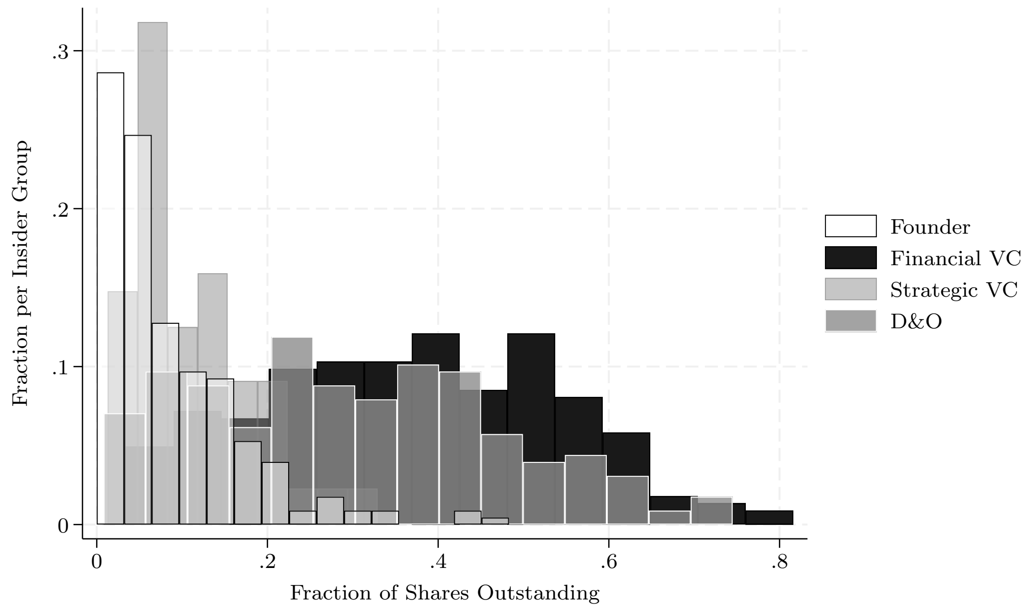
For a better understanding of the different sales strategies between the groups, it helps to first look at the ownership distribution at the IPO. Figure 2 provides this overview and shows differences between the groups. The relative ownership stakes are taken from 424B4 filings and include the shares distributed at the IPO. We find the most marked difference between the two prominent groups, founders and financial VCs. While founders often have most of their private capital invested in their company, at the company level these holdings are small. The majority of founders holds 6% or less of shares outstanding. The maximum value depends on how we classify founder-related entities (see Section 3.1). Throughout the paper, we assign founder-related VCs to the VC group (Subfig. 2a). Subfigure 2b shows that because financial VCs on average hold larger blocks than founders, reclassifying some of these observations mostly impacts the upper end of the founder holdings distribution. However, the affected observations are among the highest ones even for venture capitalists, since the combination of supplying capital and founding the company leads to an especially powerful position for these "investor-founders". Depending on the classification, the maximum founder ownership lies below 50% or below 60%.

Strategic VCs' holdings are distributed similarly to founders'. Compared to independent, financial VCs, these mostly corporate-owned investment vehicles pursue different goals. The relatively low holdings are in line with the conjecture of incumbent firms using these investments as an option for later strategic interaction. This can be beneficial, independent of whether the products are complements or substitutes. However, as Hellmann (2002) finds, strategic VCs' search for synergies can lead to conflicts with the founders. Specifically in biomedical companies though, strategic VCs can provide the necessary capital to fund lengthy and costly drug developments (Lo and Thakor (2022)). In the average firm, financial VCs and D&Os hold the largest ownership stakes at the IPO. The overlap is no coincidence, since in young firms, VCs usually have board representation (Amornsiripanitch et al. (2019)). In 424B4 filings, the VC holdings are in these cases also attributed to the D&O group. Founders active on the board also contribute to heightened D&O holdings.

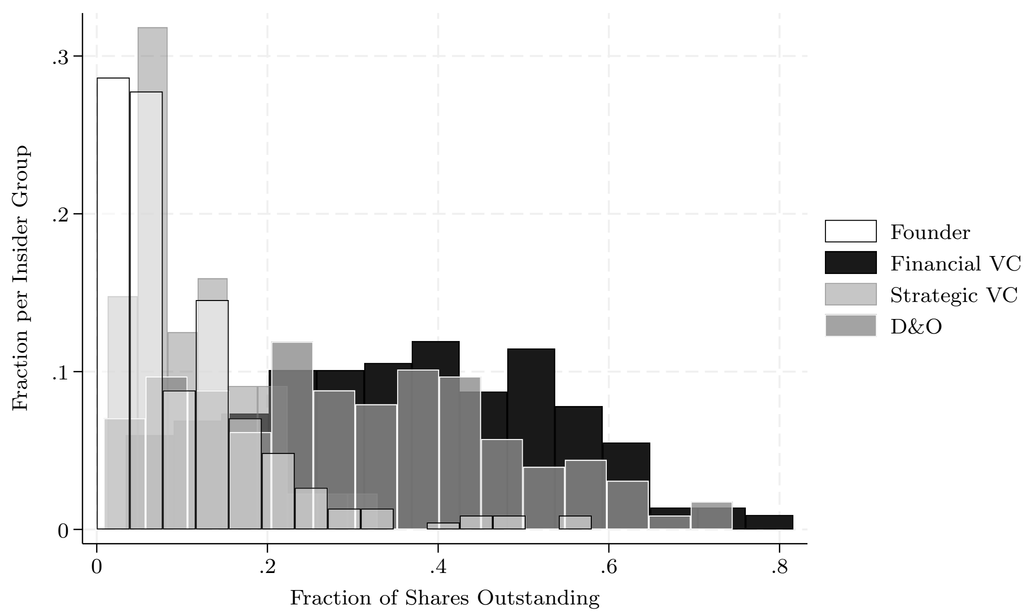
Figure 2: Distribution of Ownership by Group, Including Shares Issued at IPO

This figure plots the ownership distribution as listed in our sample firms' 424B4 filings. We use the values that include the shares distributed at the IPO. The x-axis depicts the relative ownership stake, the y-axis depicts the fraction of each insider group with holdings in the respective bin.

(a) Distribution with Founder-VCs as VCs



(b) Distribution with Founder-VCs as Founder Entities



We summarize the incentives of the four groups as follows: Founders face a trade-off between diversifying their holdings (i.e. selling stock) and remaining active in the company, enabling them to enjoy private benefits of control. Financial VCs tend to hold the largest ownership stakes. Because of their funds' limited lifetime and the need to repay their investors, VCs are under the highest pressure to reduce their holdings. Strategic VCs on the other hand strive for synergies benefiting their parent company. Their primary goal is therefore not to sell the holdings as fast as possible. Lastly, the fate of Directors and Officers in newly public companies is intertwined with the financial VCs. The holdings of these groups usually co-move in the early stages.

4.2 Evolution over Time

In Figure 3, we display the evolution of ownership split by group. We use the values from 424B4 (for pre- and post-IPO values) and 14A filings (for the following years), respectively. The x-axis is therefore roughly to scale for the post-IPO years, but not for the two values around the IPO. Within each company, we aggregate the ownership per group. A caveat, caused by the filing requirements, is that we cannot observe entities once their ownership drops below 5%, given that they are not directors or officers. First, this leads to some uncertainty about the true position of these entities within the firm, since they could still be holding up to 4.99% of shares. Second, it might lead to an underestimation of the actual ownership decrease, since the entities remaining in the firm for a longer period might tend to holder higher stakes. This could help to explain the increase of mean founder ownership between years three and four (Subfig. 3a). To better approximate the true decrease, we additionally show the number of reporting entities. Since we have an observation for the D&O group for each firm-year, the dots in Subfig. 3d describe the number of sample firms.

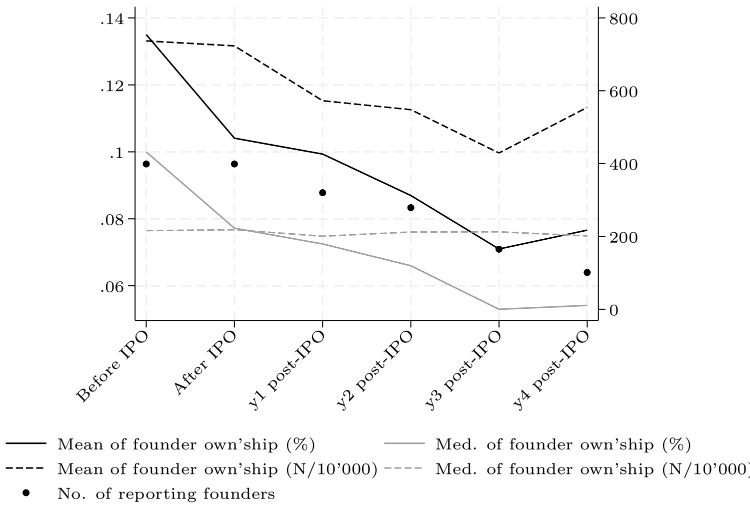
Contrary to popular belief, the IPO is not used by any of the groups to exit the company quickly. The relative ownership decreases steeply, but this effect is almost entirely due to the increase in shares outstanding. The absolute number of shares held remains constant across all groups. In the first year after the IPO, we find that both financial and strategic VCs only reduce their holdings slightly, or even increase them. While the graphs might underestimate the true decrease, the observation is consistent with the finding of Iliev and Lowry (2020) that VCs in some cases provide additional capital to their portfolio companies even after the IPO. Another reason for the increase in ownership is that the tables filed with the SEC contain stock options exercisable within 60 days. If more options are in this category than in the filing before, this leads to a reported increase in ownership.

Over the entire period, financial VCs display the strongest decrease. Before the IPO, they hold on average roughly 55% of outstanding shares. After four years, the number decreases to 20% in the firms

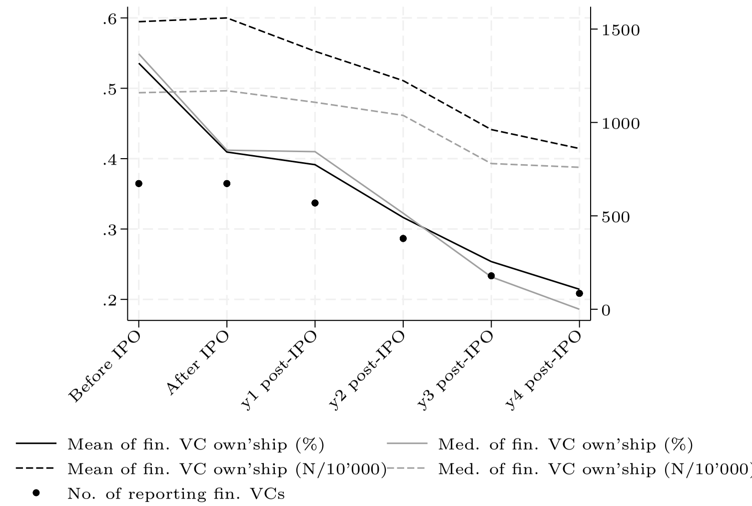
Figure 3: Ownership Evolution up to Four Years after IPO, Split by Group

This figure plots the development of ownership aggregated within each firm, by insider group. The left y-axis denotes the ownership relative to shares outstanding, the right one denotes the number of shares scaled by 10’000, and the number of reporting entities. The plots are based on 424B4 filings for the IPO-values, and 14A filings for subsequent years.

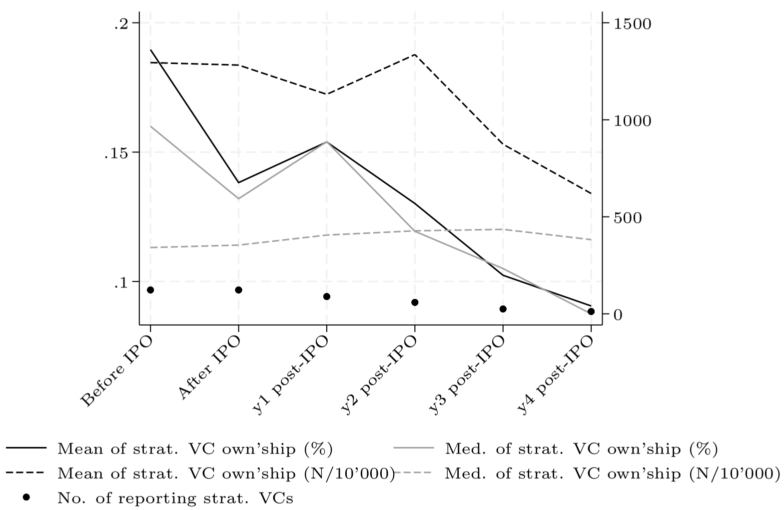
(a) Founders



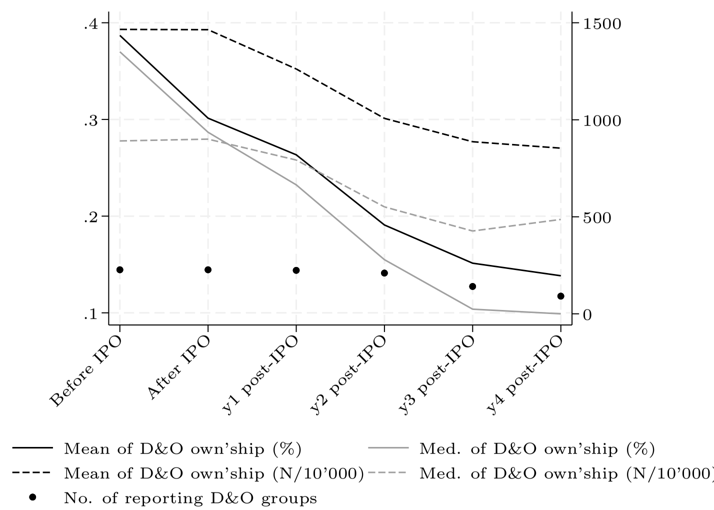
(b) Financial VCs



(c) Strategic VCs



(d) Directors & Officers



that do not exit the sample earlier. Similarly, the number of entities also drops significantly. Founders and strategic VCs have lower ownership at the IPO and their reduction in holdings is less steep. Another difference to financial VCs is that founders and strategic VCs either leave the sample (be it by dropping below the 5% threshold and/or not being directors or officers anymore), or keep their absolute ownership stake constant (median absolute ownership line is horizontal for the two groups). Financial VCs on the other hand sell considerable amounts of shares even conditional on remaining in the company. Directors and officers' holdings are reduced with a pattern similar to financial VCs. This confirms the differences in selling speed as suggested in our Hypothesis 2 and agrees with the observation that firms become increasingly widely held over time, reducing insiders' impact (Helwege et al. (2007)).

The evolution of holdings is linked to trading. Due to the size of their ownership stake, financial VCs are likely restricted in their selling behavior: either by liquidity, which makes it impossible to sell without a big price impact, or by Rule 144, which sets sales limits based on trading volume and shares outstanding. Strategic VCs on average keep their investments for a longer time period, allowing them to sell after the trading volume has increased.

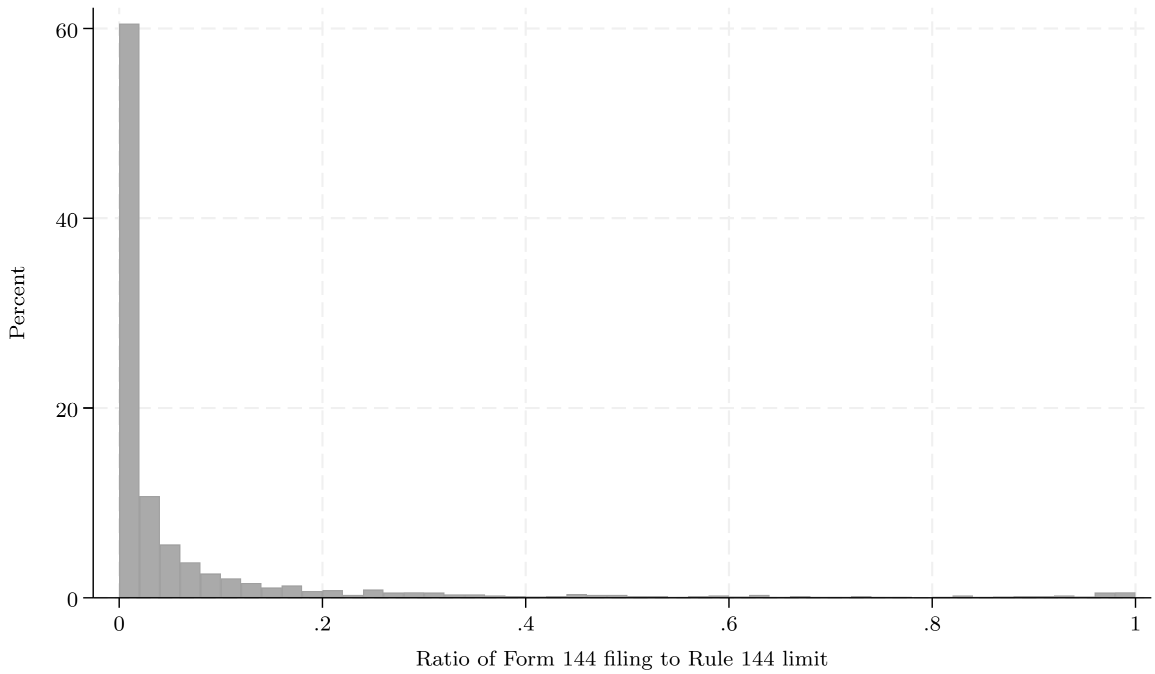
5 Rule 144

Before looking at insiders' strategic sales, we ask how tightly the sales restrictions in Rule 144 are set. The rule, which is designed to protect shareholders from extensive insider sales, posits that affiliates need to announce the intention to sell before executing the actual transaction, if they sell more than 5'000 restricted shares or if these shares' worth exceeds USD 50'000. In this case, filing Form 144 opens a three-month window for the insiders to sell the filed amount, which must not exceed the greater of 1% of shares outstanding and the four-week average of weekly trading volume. This allows for larger sales contingent on higher stock liquidity, in which case the price impact of sales is alleviated.

We find that the rule is rarely binding. Figure 4 depicts the distribution of the amount of shares filed for using Form 144, relative to the maximum that the rule would allow. The 95$^{\text{th}}$ percentile is at 45.15% of the allowed amount. Only in 37 cases throughout our sample do insiders exhaust 90% or more of what they could. While we cannot rule out that insiders endogenously decide to not utilize the full amount, the fact that the vast majority of filings lies below the 20% mark indicates a non-restrictive rule. We suggest three possible explanations which we examine in more detail. First, the limits could be set generously. Second, insiders could be reluctant to leave quickly (e.g., because of the possible price impact such large trades might have). Third, the use of open market trades and thus Rule 144 could be limited to certain insiders.

Figure 4: Ratio of Filed Shares to Rule 144 Minimum

This histogram displays the distribution of the ratio of amounts filed for via Form 144 to the maximum allowed by Rule 144. The x-axis denotes the ratio. The y-axis denotes the percent of observations per bin.



5.1 Sales Limits Set by Rule 144

In Table 5, we show that indeed, the rule allows for strong reductions in holdings. Within the first year after lockup expiration, in 85.02% of sample firms, founders could reduce their stake to 5% of what they owned at the IPO. For VCs and the D&O group, this is the case in half of the firms. Such large drops would be allowed on the first day after the lockup in 29.52% of firms for founders, and 6.61% for VCs. The table provides a conservative estimate of the possible selling speed, since we aggregate by company. For single entities, the numbers tend to be even higher.[10]

[10] This is the case especially for venture capitalists, because there are usually multiple of them in the firm at IPO.

Table 5: Possible Ownership Drop in Relation to Holdings after the IPO

This table reports the fraction of firms, in which the ownership of the respective group could possibly drop (relative to post-IPO holdings) within the specified time frame after the lockup period expiration. We use trading days (one week refers to five trading days, one month to 21 trading days, one quarter to 62 trading days, and one year to 252 trading days). Panel A shows the possible ownership drops for founder ownership. Panel B shows the possible ownership drops for VC ownership. Panel C shows the possible ownership drops for managerial ownership.

Panel A: Founder Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	29.52	32.60	40.09	53.30	85.02
Drop to 10%	30.40	34.36	40.53	53.74	86.34
Drop to 25%	34.80	37.00	47.14	60.79	87.22
Drop to 50%	43.17	47.14	58.15	73.57	89.87
Drop to 75%	63.88	66.52	75.77	84.14	91.63

Panel B: VC Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	6.61	8.37	11.45	17.62	49.34
Drop to 10%	7.05	8.81	12.33	18.06	54.63
Drop to 25%	9.25	10.57	16.30	19.82	59.47
Drop to 50%	14.10	15.86	19.82	25.99	69.60
Drop to 75%	23.79	27.75	34.80	47.14	83.70

Panel C: D&O Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	7.05	8.81	12.33	18.50	55.95
Drop to 10%	7.93	9.69	13.22	18.50	56.83
Drop to 25%	9.69	11.45	16.74	20.70	60.35
Drop to 50%	14.54	16.74	20.70	27.31	70.48
Drop to 75%	25.99	29.96	37.00	51.98	84.14

5.2 Persistence of Insiders

A second explanation is that insiders have no desire to exit quickly. In Figure 5, we provide evidence consistent with such persistence. The figure compares the theoretical minimum ownership allowed by Rule 144 (grey line, assuming an entity always sells the maximum quantity allowed) with the actual ownership. Unlike Fig. 3, the graphs are based on the mean across all entities per group. Since 14A filings do not occur in exact annual intervals, we place them at the median trading day relative to IPO. The more parallel the two lines are, the faster a group reduces their ownership. After the lockup period expires, the lines drift apart, indicating that insiders sell much less than what Rule 144 would allow.

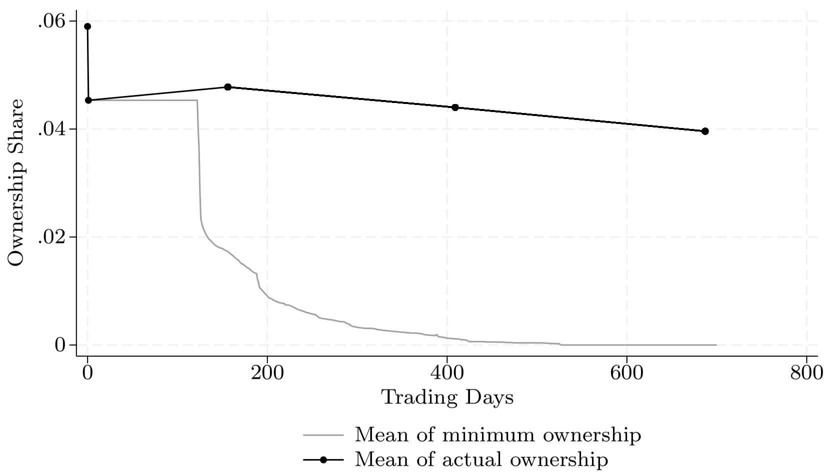
In Tables 6 and 7, we break down the ownership drops during the first year after the lockup expiration in more detail. First across the entire sample, then split by industry for the VC holdings. Similar to Figure 5, the tables show that most insiders take their time to exit. For founders and strategic VCs, there are few cases in which ownership is reduced to less than 5% within the first day (0.88% and 0.44%, respectively), but even after a year post-lockup, over three quarters of financial VCs and 90% of founders still hold more than 50% of their initial stake. As in the previous analyses, strategic VCs are the most passive among the insider groups, and quick sales are an exception. Contrary to Table 5, the numbers are not increasing monotonically from left to right, because there can be increases in ownership.

Table 7 provides an interesting new insight. In the health care industry, due to the legal framework, companies tend to go public earlier and often without marketable products, which hampers their profitability (Aghamolla and Thakor (2022) and Lo and Thakor (2022)). We show that this has implications for the venture capital providers. Namely, the ownership reductions by VCs in this industry (Panel A) are significantly lower than in other industries, where it is more common that positions are reduced at faster pace (Panel B). The fact that corporate VC subsidiaries which follow long-term strategies are an important source of capital for health care companies (Lerner and Merges (1998)) is another factor that helps explain this finding.

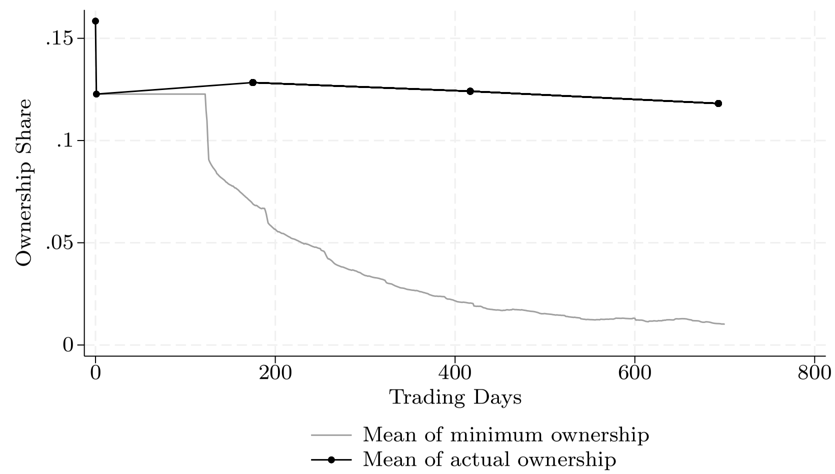
Figure 5: Actual Ownership Compared to Rule 144 Minimum

This figure compares the actual ownership to the minimum allowed by Rule 144, by insider group. All values are based on the average across all entities per group. The drop on day 0 displays the change from pre- to post-IPO holdings. All subsequent observations denote the ownership as of the indicated trading day.

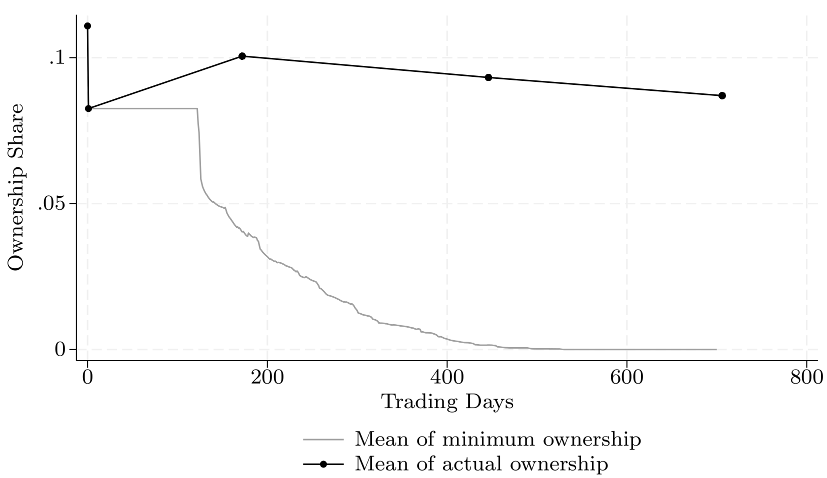
(a) Founders



(b) Financial VCs



(c) Strategic VCs



(d) Directors & Officers

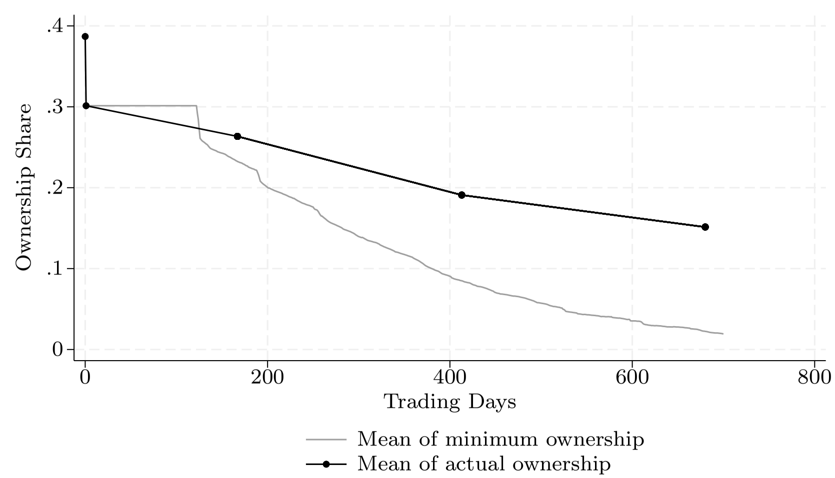


Table 6: Actual Ownership Drop in Relation to Holdings after the IPO

This table reports the fraction of firms, in which the ownership of the respective group drops (relative to post-IPO holdings) within the specified time frame after the lockup period expiration. We use trading days (one week refers to five trading days, one month to 21 trading days, one quarter to 62 trading days, and one year to 252 trading days). Panel A shows the ownership drops for founder ownership. Panel B (C) shows the ownership drops for financial (strategic) VC ownership.

Panel A: Founder Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	0.88	0.88	1.76	1.32	3.08
Drop to 10%	0.88	0.88	1.76	1.32	3.08
Drop to 25%	1.32	1.32	2.64	2.20	3.08
Drop to 50%	2.64	2.64	3.96	4.41	4.41
Drop to 75%	3.96	3.96	5.73	6.61	10.13
Number of Founders	337	337	337	335	327

Panel B: Financial VC Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	0.44	0.44	0.44	0.00	0.88
Drop to 10%	0.44	0.44	0.44	0.00	0.88
Drop to 25%	0.88	0.88	0.88	1.32	4.41
Drop to 50%	1.76	1.76	3.08	4.85	10.13
Drop to 75%	7.05	7.93	11.89	13.22	23.79
Number of financial VCs	661	661	661	658	631

Panel C: Strategic VC Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	0.00	0.00	0.00	0.00	0.00
Drop to 10%	0.00	0.00	0.00	0.00	0.00
Drop to 25%	0.00	0.00	0.00	0.00	0.00
Drop to 50%	0.00	0.00	0.00	0.44	0.44
Drop to 75%	0.00	0.00	0.44	0.88	2.64
Number of strategic VCs	119	119	119	116	111

Table 7: Actual Ownership Drop: VCs in Health Care vs. other Industries

This table reports the fraction of firms, in which the ownership of VCs drops (relative to post-IPO holdings) within the specified time frame after the lockup period expiration. We use trading days (one week refers to five trading days, one month to 21 trading days, one quarter to 62 trading days, and one year to 252 trading days). Panel A shows the ownership drops for VCs in the Health Care sector (*GICS Sector 35*). Panel B shows the ownership drops for VCs in all other sectors.

Panel A: VCs in Health Care

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	0.00	0.00	0.00	0.00	0.00
Drop to 10%	0.00	0.00	0.00	0.00	0.00
Drop to 25%	0.00	0.00	0.00	0.58	1.75
Drop to 50%	1.17	1.17	1.75	2.92	4.68
Drop to 75%	3.51	4.09	6.43	8.19	12.87
Number of VCs	627	627	627	621	605

Panel B: VCs in Other Industries

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Drop to 5%	1.82	1.82	1.82	0.00	3.64
Drop to 10%	1.82	1.82	1.82	0.00	3.64
Drop to 25%	3.64	3.64	3.64	1.82	12.73
Drop to 50%	3.64	3.64	5.45	7.27	27.27
Drop to 75%	14.55	16.36	21.82	27.27	54.55
Number of VCs	153	153	153	153	140

5.3 Rule 144 Circumvention

To what extent does Form 144 explain the decrease in ownership? In Table 8, we show that the different insider groups are not equally likely to announce sales of restricted stock via Form 144. For founders, F144-filings explain a higher fraction of the ownership decrease during the first year after the lockup. This fraction is lower for VCs, which shows that they are using other instruments to reduce ownership. We identify in-kind distributions as an important channel through which VCs can circumvent Rule 144.

At the same time, distributions allow them to offload a large amount of shares without suffering from the price impact.

Table 8: Form 144 Filings in Relation to Holdings after the IPO

This table reports the fraction of firms, in which Form 144 filings of the respective group allow for drops (relative to post-IPO holdings) within the specified time frame after the lockup period expiration. We use trading days (one week refers to five trading days, one month to 21 trading days, one quarter to 62 trading days, and one year to 252 trading days). Panel A shows the ownership drops for founder ownership. Panel B (C) shows the filed-for ownership drops for financial (strategic) VC ownership.

Panel A: Founder Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Filings for drop to 5%	0.00	0.00	0.00	0.44	0.88
Filings for drop to 10%	0.00	0.00	0.00	0.44	0.88
Filings for drop to 25%	0.00	0.00	0.00	0.44	0.88
Filings for drop to 50%	0.00	0.44	0.44	0.88	1.32
Filings for drop to 75%	0.00	0.88	0.88	1.76	7.49

Panel B: Financial VC Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Filings for drop to 5%	0.00	0.00	0.00	0.00	0.00
Filings for drop to 10%	0.00	0.00	0.44	0.44	0.44
Filings for drop to 25%	0.00	0.00	0.44	0.44	0.44
Filings for drop to 50%	0.00	0.00	0.44	0.88	1.76
Filings for drop to 75%	0.88	0.88	0.88	2.20	4.41

Panel C: Strategic VC Ownership

	1 Day	1 Week	1 Month	1 Quarter	1 Year
	(1)	(2)	(3)	(4)	(5)
Filings for drop to 5%	0.00	0.00	0.00	0.00	0.44
Filings for drop to 10%	0.00	0.00	0.00	0.00	0.44
Filings for drop to 25%	0.00	0.00	0.00	0.44	0.88
Filings for drop to 50%	0.00	0.00	0.44	0.88	1.32
Filings for drop to 75%	0.00	0.00	0.44	1.32	1.76

5.4 Potential for Improvement

Based on the evidence that we present, we conclude that Rule 144 has two important drawbacks which hinder effective shareholder protection: First, we show that the intention to sell restricted shares is often filed concurrently with the actual sales transaction, making this announcement obsolete. This is most frequently the case for sales by founders. Second, our analysis reveals that the limits set by Rule 144 are only binding on rare occasions. On one hand, this has to do with the relatively generous limits the rule sets. The possible drop after the lockup expiration displayed in Fig. 5 is significant. This might be explained by the fact that the rule's limits were set at times when market liquidity was not as high. On the other hand, we show that often times, especially founders and strategic VCs do not have an incentive to quickly exit the company after it has gone public. The evidence presented in this chapter hints at the sub-optimal shareholder protection that we conjecture in our first hypothesis.

In our view, both flaws could be corrected: The first one by introducing a gap of one or multiple trading days between the publication of Form 144 and the actual sale transaction. The second one by regularly (e.g., annually) adjusting the sales limits to current market volume. Apart from Rule 144, transparency on insider investors could be increased by lowering the publication threshold for holdings from 5% to e.g., 3%, as is the case in Switzerland. Furthermore, disclosure for restricted shares could be ameliorated by mandating the publication of the amount of such shares held and the holding duration.

6 Strategic Insider Selling

In this section, we use event studies to analyze the extent to which insiders sell their shares strategically in order to maximize their trading profits.[11] As discussed in Sec. 3.2.2, we concentrate on founders and VCs when it comes to trading. For these two groups, we assign the trades of all related entities to the respective beneficial owner.

To facilitate comparability, we use similar model parameters as Gompers and Lerner (1998). Namely, we use an event window spanning from trading days -60 to $+100$ around the trades and distributions in our sample. We use a window of 100 days to estimate the normal returns, where we posit a minimum of 50 valid return observations. The abnormal returns are calculated using a market model, with the CRSP value-weighted market return as a proxy for the market. For VCs, we exclude open market trades below USD 50'000 and in-kind distributions below USD 100'000 to make sure that we capture relevant transactions. Since founders generally trade in lower quantities, we do not exclude trades there.

[11] We use the WRDS Event Study tool for the calculations.

6.1 Founders

The literature shows that open market insider sales tend to be uninformative (e.g., Jeng et al. (2003)) and insiders have found other ways to monetize their informational advantage (Avci et al. (2024)). However, founders, as long as they remain active in the company, should be among the best-informed parties in newly public firms. We conjecture that paired with the lower amount of public scrutiny as compared to larger, established firms, less developed corporate governance, and the weaknesses of Rule 144, this information edge can still lead to abnormal returns to sales. Indeed, in Figure 6a and Table 9 we provide evidence consistent with strategic selling by founders. The price surge ahead of the trades is mild, at 2.53% outperformance of the market over the previous 60 days. After the trades, the market reaction is stronger and we find a steady decrease of the CARs up to −9.61% at the end of the event window. Overall, this leads to a distinct inverted V-shape. This decrease after the trades could be caused by the sales themselves which are interpreted as a negative signal. However, in this case we would expect that they are priced in quicker. The persistence of the downward trend in the CARs is consistent with founders enjoying private information about subsequent news. This finding also points towards a lack of regulatory protection for non-insider shareholders in newly public firms, which is in line with Hypothesis 1.

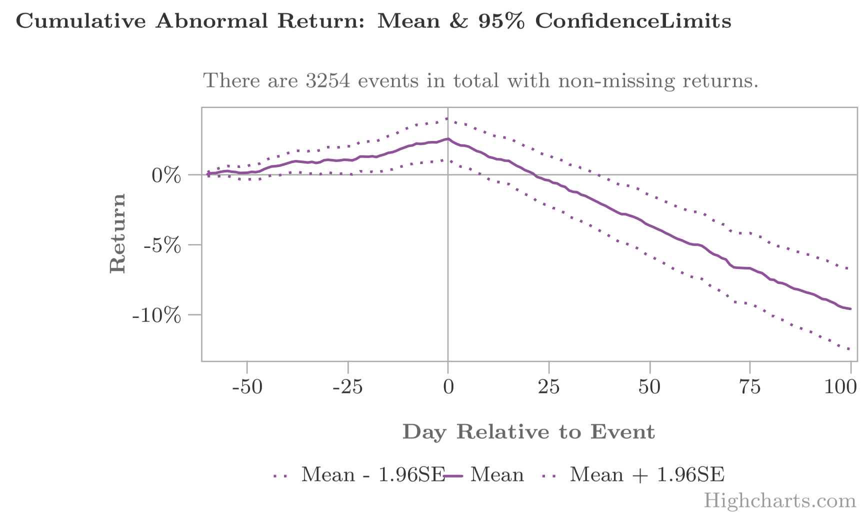
We analyze the CARs in more detail and split trades by time passed since lockup expiration. Figures 6b and 6c show the abnormal returns to trades within one year after the lockup, and later on, respectively. It is striking that the price surge observed across the entire sample is borne by later sales for which we find the inverted V-shape. For early sales we find a similar market reaction, but a slight, although not statistically significant underperformance vis-à-vis the market in the days preceding the trades. In our view, this could be caused by post-IPO underperformance (see e.g., Ritter (1991) or Loughran and Ritter (1995)) or by the fact that founders selling early are more strongly driven by liquidity reasons and therefore care less about the trade profitability. We believe that the pattern is not driven by Rule 144, because stock sold later on still consists largely of restricted shares.

In Figures 7b and 7c, we split the sample by stock liquidity and compare trades in the most liquid tercile to those in the bottom tercile. The number of sales in the subsample of illiquid shares is considerably lower than when shares are liquid. This is consistent with insiders fearing the price impact of their sales when trading volume is not sufficient. When there are sales however, they tend to happen after strong share price run-ups. We find a CAR of 14.49% over the sixty days leading to the trades. This could be explained by the strong performance leading to sufficient liquidity allowing founders to go through with their sales. For the frequently traded shares we find a pattern very similar to the full sample.

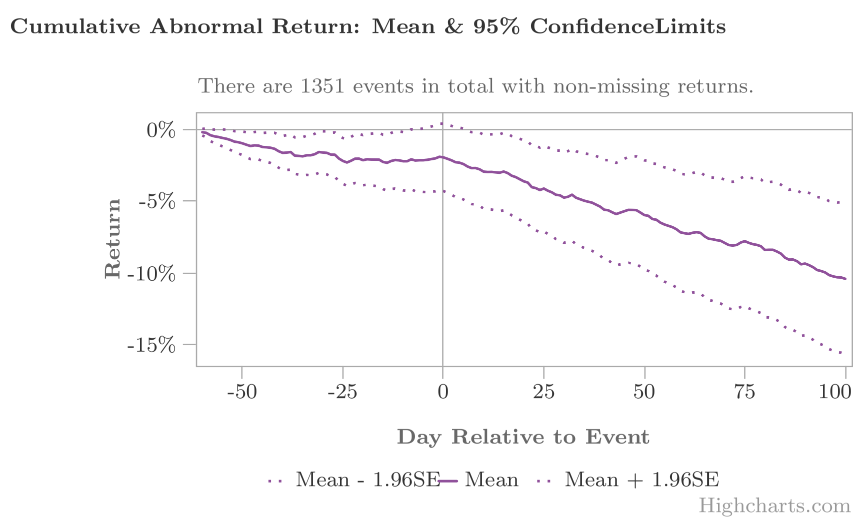
Figure 6: Event Studies around Founder Trading, Split by Time since Lockup

This figure shows the CARs to open market trades by founders. Subfigure 6a is based on the full sample, 6b on trades within the first year after lockup expiration, and 6c on founder open market trades taking place later than one year post-lockup. The estimation window is 100 trading days (minimum of 50 valid obs.). The event window is 160 days, starting 60 days before the trade and ending 100 days after. The standard errors are proxied for by the error term of the market model regression. The event studies are calculated using the Wharton Research Data Services (WRDS) Event Study Tool.

(a) Founders Open Market, all



(b) Founders Open Market, First Year Post-Lockup



(c) Founders Open Market, later than One Year Post-Lockup

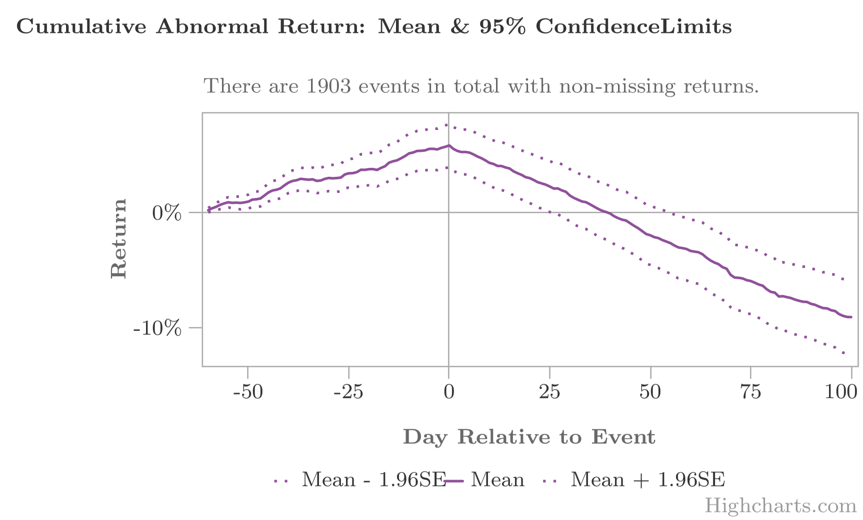
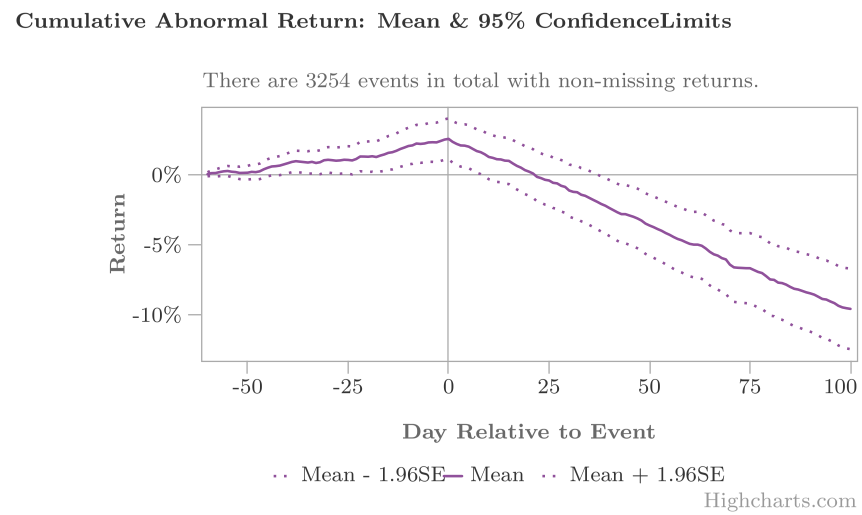


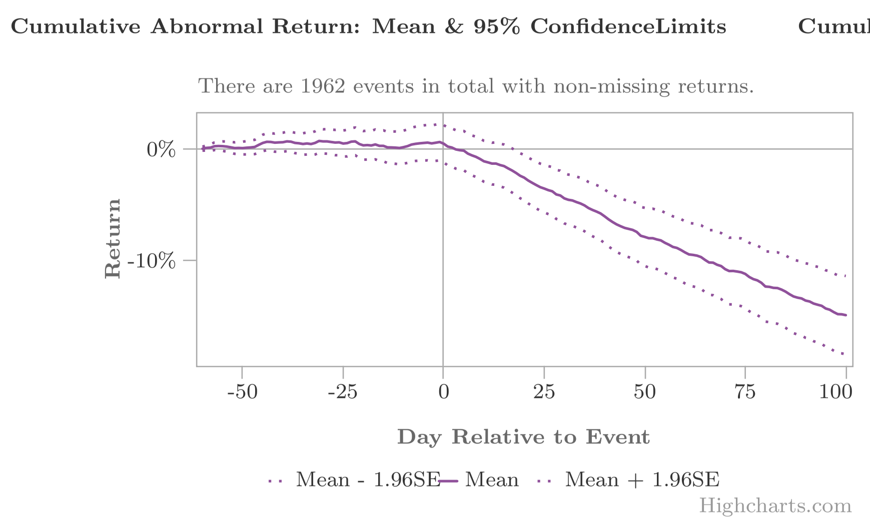
Figure 7: Event Studies around Founder Trading, Split by Stock Liquidity

This figure shows the CARs to open market trades by founders. Subfigure 7a is based on the full sample, 7b on trades in sample firms in the top tercile of stock liquidity (calculated following Amihud (2002)), and 7c on trades in sample firms from the bottom liquidity tercile. The estimation window is 100 trading days (minimum of 50 valid obs.). The event window is 160 days, starting 60 days before the trade and ending 100 days after. The standard errors are proxied for by the error term of the market model regression. The event studies are calculated using the WRDS Event Study Tool.

(a) Founders Open Market, all



(b) Founders Open Market, Liquid



(c) Founders Open Market, Illiquid

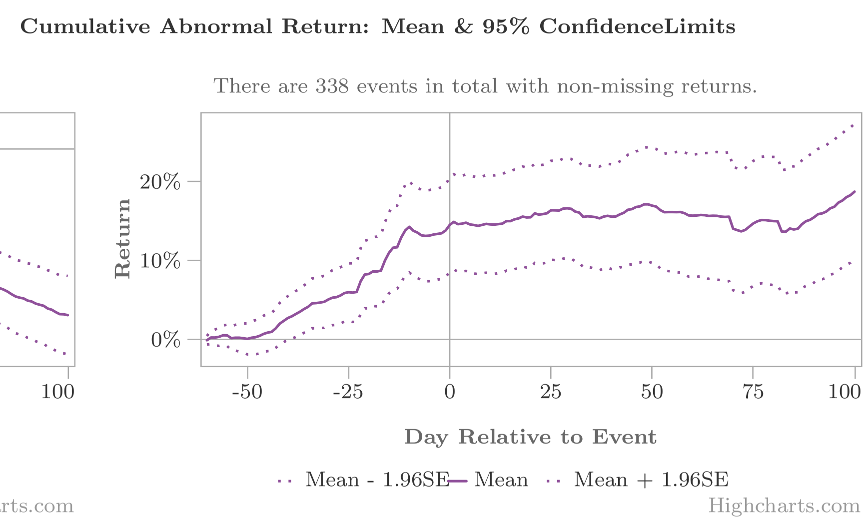


Table 9: Event Study Results - Founders

This table lists the CARs to founder transactions. Panel A is based on all open market sales. Panel B (C) is based on all sales within one year after lockup expiration (sales later than one year after the lockup period has expired). Panels D and E are based on sales in stocks in the top and bottom liquidity tercile, respectively. *Trading Day* denotes the trading day relative to the event date (day 0). N reports the number of observations. CAR is the CAR starting at day -60. *p-value* reports the probability that CAR is different from zero, based on a cross-sectional t-test. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Trading Day	N	CAR	$p-$value
Panel A: All Open Market Sales			
-25	3254	1.02%**	0.041
-10	3254	2.02%***	0.003
-5	3254	2.27%***	0.001
-1	3254	2.47%***	0.001
0	3254	2.53%***	0.001
1	3254	2.31%***	0.002
5	3253	1.97%**	0.012
10	3252	1.24%	0.128
25	3247	−.44%	0.626
100	3230	−9.61%***	0.000

Trading Day	N	CAR	$p-$value
Panel B: First Year Post-Lockup			
-25	1351	−2.2%***	0.007
-10	1351	−2.21%**	0.033
-5	1351	−2.14%*	0.057
-1	1351	−1.91%	0.106
0	1351	−1.94%	0.104
1	1351	−2.04%*	0.092
5	1351	−2.41%*	0.055
10	1351	−2.94%**	0.025
25	1351	−4.12%***	0.007
100	1347	−10.4%***	0.000
Panel C: Later than One Year Post-Lockup			
-25	1903	3.31%***	0.000
-10	1903	5.02%***	0.000
-5	1903	5.4%***	0.000
-1	1903	5.58%***	0.000
0	1903	5.7%***	0.000
1	1903	5.4%***	0.000
5	1902	5.08%***	0.000
10	1901	4.2%***	0.000
25	1896	2.17%*	0.052
100	1883	−9.04%***	0.000

Trading Day	N	CAR	$p-$value
Panel D: Sales of Liquid Shares			
-25	1962	.52%	0.384
-10	1962	.19%	0.801
-5	1962	.57%	0.478
-1	1962	.65%	0.437
0	1962	.5%	0.555
1	1962	.24%	0.777
5	1961	−.12%	0.895
10	1960	−1.08%	0.251
25	1955	−3.52%***	0.001
100	1942	−14.88%***	0.000
Panel E: Sales of Illiquid Shares			
-25	338	6.01%***	0.001
-10	338	14.25%***	0.000
-5	338	13.14%***	0.000
-1	338	13.8%***	0.000
0	338	14.49%***	0.000
1	338	14.88%***	0.000
5	338	14.55%***	0.000
10	338	14.55%***	0.000
25	338	16.33%***	0.000
100	336	18.68%***	0.000

6.2 Venture Capitalists

For VCs, we have meaningful observations for both J-type- (in-kind distributions) and open market trades. Therefore, we differentiate between the two categories (Figures 8a and 8b). The differentiation is justified e.g., by the findings of Avci et al. (2024) who show that J-type transactions can be misused to conceal profitable insider sales. As mentioned in Sec. 3.2, virtually all in-kind distributions are done by financial VCs. For this reason, we do not analyze the two VC-categories separately here.

Both transaction types in principle indicate private information by venture capitalists, providing further support for our first hypothesis. Nevertheless, there are some differences. For in-kind distributions, we find no preceding price increase, but a consistently lower performance than the market in the following 100 days which results in a CAR of -19.97% at the end of the event window. In our sample, open market sales tend to happen after significant overperformance, but over the entire event window, there are no significant differences to the market. We compare our results to Gompers and Lerner (1998) who made a similar analysis using in-kind distributions at a time when these transactions were not public. They find an inverted V-shape and show that distributions on average are preceded by a 7.4% 60-day CAR. Interestingly, despite the improved disclosure mandate for in-kind distributions, we find higher abnormal returns, even though the pattern has shifted as described above. While the lack of positive CARs ahead of distributions nowadays could hint at more cautious timing due to the increased scrutiny, comparing the pattern with open market trades leads us to abandon this theory. Furthermore, we show that despite the disclosure improvement, the market still does not anticipate the price decrease at the time when these transactions are disclosed.

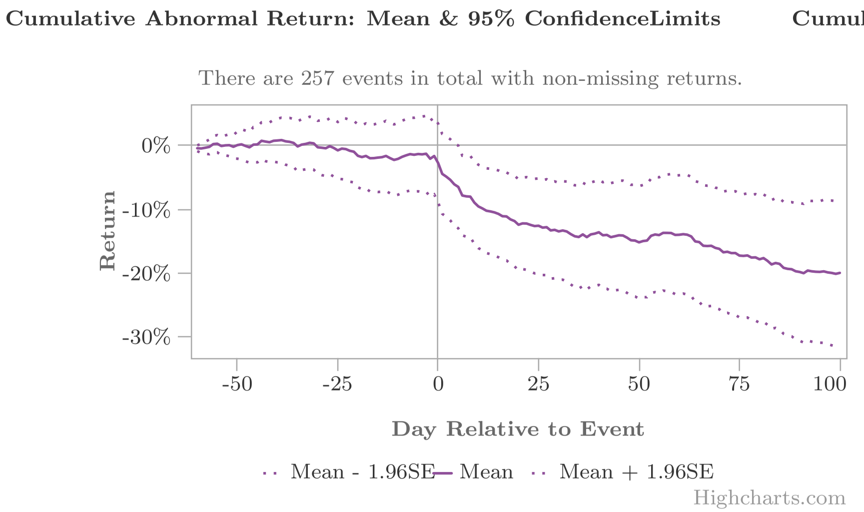
Our results agree with the argument of Avci et al. (2024) that insiders often use J-type transactions to hide profitable insider trading. They measure abnormal returns to J-trades with footnotes containing "VC" or "Venture Capital" and find CARs of 4% over the following year. We attribute the strength in magnitude of our results to the fact that we look at newly public firms with higher information asymmetry.

In Fig. 8c, we provide novel evidence on the interaction between insider groups. As we show in Table 4, founders are more likely to trade in the ten trading days before in-kind distributions. Such interactions tend to happen after periods of positive abnormal returns. Founders show remarkable timing in their sales. They are able to sell in the short window between the surge and the in-kind distribution which is followed by underperformance vis-à-vis the market. We interpret this finding with founders enjoying access to VCs' private information about upcoming distributions. Knowing about the negative price impact, they then move quickly to sell.

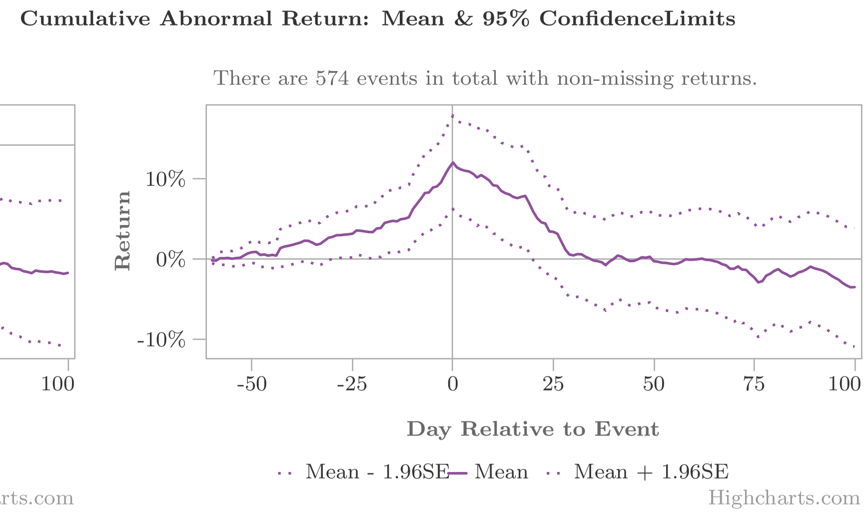
Figure 8: Event Studies around Venture Capitalist Trading

This figure shows the CARs to VC transactions. Subfigure 8a uses in-kind distributions, 8b is based on open market trades, and 8c on in-kind distributions with founder open market sales in the preceding ten trading days. The estimation window is 100 trading days (minimum of 50 valid obs.). The event window is 160 days, starting 60 days before the trade and ending 100 days after. The standard errors are proxied for by the error term of the market model regression. The event studies are calculated using the WRDS Event Study Tool.

(a) VC In-kind Distribution, all



(b) VC Open Market, all



(c) VC In-kind Distribution, Preceding Founder Sales

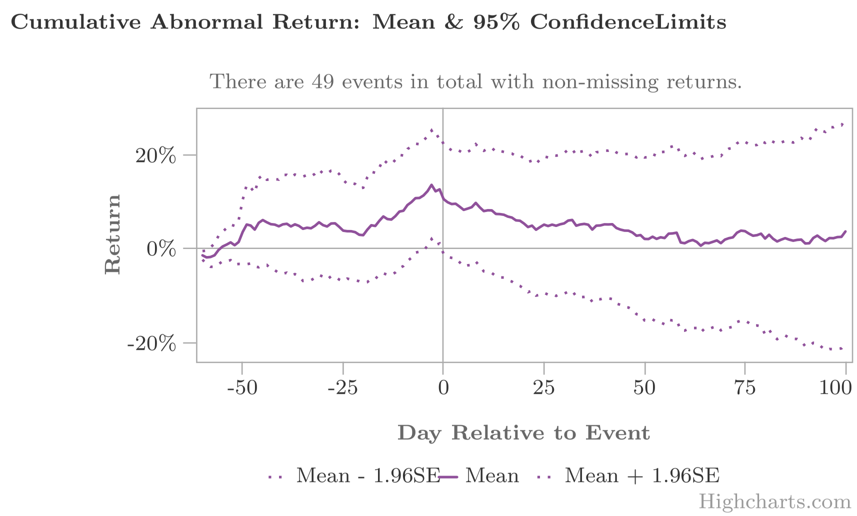


Table 10: Event Study Results - Venture Capitalists

This table lists the CARs to VC transactions. Panel A is based on in-kind distributions, Panel B on open market sales, and Panel C on in-kind distributions with founder open market sales in the preceding ten trading days. *Trading Day* denotes the trading day relative to the event date (day 0). N reports the number of observations. CAR is the CAR starting at day -60. *p-value* reports the probability that CAR is different from zero, based on a cross-sectional t-test. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Trading Day	N	CAR	$p-$value
Panel A: In-kind Distributions			
-25	257	$-.81\%$	0.723
-10	257	-2.14%	0.456
-5	257	-1.38%	0.643
-1	257	-1.69%	0.582
0	256	-2.81%	0.372
1	255	-4.49%	0.160
5	255	$-6.53\%^{**}$	0.049
10	254	$-9.58\%^{***}$	0.004
25	253	$-12.6\%^{***}$	0.001
100	252	$-19.97\%^{***}$	0.001
Panel B: Open Market Trades			
-25	574	$3.12\%^{**}$	0.038
-10	574	$6.17\%^{***}$	0.005
-5	574	$8.87\%^{***}$	0.001
-1	574	$11.3\%^{***}$	0.000
0	574	$11.99\%^{***}$	0.000
1	574	$11.37\%^{***}$	0.000
5	574	$10.58\%^{***}$	0.001
10	572	$9.14\%^{***}$	0.003
25	566	3.35%	0.246
100	551	-3.52%	0.351
Panel C: In-kind Distributions Preceded by Founder Sales			
-25	49	3.89%	0.485
-10	49	8.12%	0.183
-5	49	$11.39\%^{*}$	0.064
-1	49	$12.69\%^{**}$	0.036
0	49	$10.6\%^{*}$	0.082
1	49	9.94%	0.101
5	49	8.31%	0.182
10	49	8.05%	0.225
25	48	5.17%	0.489
100	48	3.71%	0.762

Table 11: Volume Event Study Test Statistics

This table compares trading volume before in-kind distributions (only those after lockup expiration are considered) to the time after. The two windows are described in the first column. In rows 1 and 2, the "event window" includes five trading days before the transactions. Column (3) reports the results of a one-sided t-test.

	(1) Vol. before In-kind Dist.				(2) Vol. after In-kind Dist.				(3) Difference	
Trading Days rel. to Event	N	Mean	Median	SD	N	Mean	Median	SD	Diff.	p(pre < post)
-110 to -10 vs. -5 to 10	370	1,019,648	563,719	2,253,643	375	1,313,584	691,120	2,870,989	-293,936*	0.060
-25 to -10 vs. -5 to 10	370	987,749	519,821	2,178,146	375	1,313,584	691,120	2,870,989	-325,835**	0.041
-110 to -10 vs. 0 to 10	370	1,019,648	563,719	2,253,643	373	1,296,829	680,274	3,090,338	-277,181*	0.081
-25 to -10 vs. 0 to 10	370	987,749	519,821	2,178,146	373	1,296,829	680,274	3,090,338	-309,080*	0.058

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

We also look at the impact of in-kind distributions on trading volume. A report by the law firm Morgan, Lewis & Bockius (2015) mentions as a rule of thumb that one third of recipients of in-kind distributions sell immediately, sells within three months, and remains longer, respectively. We expect that such an increase of the free float positively affects trading volume. The results of the event study reported in Table 11 confirm this conjecture. Independent of the estimation window, trading volume after the distributions is significantly higher than before.

7 Conclusion

We analyze how founders and venture capitalists (VCs) decrease their ownership in the time period up to four years after the IPO. The documented differences are in line with Hypothesis 2. We ask whether current regulation sufficiently protects shareholders from strategic selling in younger companies where information asymmetry tends to be higher and corporate governance less developed. Our analysis, which is based on a small sample of venture-backed IPOs where the founders are still active, reveals that insiders consistently earn abnormal profits when selling their restricted shares obtained before the firm went public. This finding is in line with our first hypothesis, and differs from e.g., Jeng et al. (2003) or Lakonishok and Lee (2001) in whose samples of more mature companies insider sales do not predict future returns. It also indicates room for improvement in shareholder protection, especially in newly public firms.

We show that Rule 144, which governs the sale of restricted shares, is only binding on rare occasions and provide multiple explanations for this phenomenon. Namely, insiders often show little desire to exit shortly after the IPO, the rule's sales limits are set generously, and venture capitalists can circumvent the rule by distributing shares in-kind. The last point highlights an important concern for shareholders:

in-kind distributions allow for the offload of large amounts of shares to VCs' limited partners without any price impact. In most cases (namely after a total holding period of six months), the recipients can freely sell the shares. We provide a long due update to the findings of Gompers and Lerner (1998) and show that tighter regulations on disclosure of such distributions had no effect on the abnormal returns that VCs enjoy. Last but not least, we show that in-kind distributions are almost exclusively used by financial venture capitalists, while founders and strategic VCs rely more on open market sales. The fact that such trades are also linked to negative future abnormal returns and that founders often file Form 144 concurrently with the actual sale indicates that regulators need to concentrate on multiple dimensions when trying to keep insiders from misusing private information for their own benefit.

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Appendix

A1 Sample Construction

This section contains additional details regarding the sample construction:

- Values from 14A filings, Form 4, Form 144, and CRSP are adjusted for (reverse) stock splits using *cfacshr* or *cfacpr*. This is to ensure a "smooth" number of shares outstanding, among others. It is crucial for F144-related calculations, since the SEC states that shares sold under Rule 144 have to be calculated as if the stock split happened on day one of the past-90d-period (see here).
- Illiquidity measures are calculated monthly and annually (Fong, Holden, and Trzcinka (FHT) and Amihud). Since the monthly FHT measure is predominantly zero, only Amihud is used in the monthly version. Illiquidity terciles are assigned based on either months relative to IPO or relative to first 14A filings.[12] A firm could in principle switch between terciles on a monthly basis. However, the classification is relatively sticky and jumps from top to bottom or vice versa are rare.

[12] The two versions are highly correlated (ca. 0.95). Monthly and annual Amihud Illiquidity Measure (AM): 0.8 ; annual AM and FHT: 0.6

A2 Variable Definitions and Sample Comparison

Table A1: Definitions of variables

Variable name	Variable definition
Ln(Assets)	Natural logarithm of total assets. The variable is constructed using Compustat data item *at*. Winsorized at the 1% and 99% levels.
Debt/Assets	Ratio of total debt in current liabilities plus total long-term debt to total assets. The variable is constructed using Compustat data items $(dlc + dltt)/at$. Winsorized at the 1% and 99% levels.
Capex/Assets	Ratio of capital expenditures to total assets. The variable is constructed using Compustat data items $capx/at$. Winsorized at the 1% and 99% levels.
Cash/Assets	Ratio of cash plus short-term investments divided by total assets. The variable is constructed using Compustat data items che/at. Winsorized at the 1% and 99% levels.
EBIT/Assets	Ratio of earnings before interest and taxes to total assets. The variable is constructed using Compustat data items $ebit/at$. Winsorized at the 1% and 99% levels.
PP&E/Assets	Ratio of property, plant and equipment to total assets. The variable is constructed using Compustat data items $ppent/at$. Winsorized at the 1% and 99% levels.
Tobin's q	The ratio of market assets to book assets – $(csho \times prcc_f + atceq - txdb)/at$.
Total q	The ratio of firm value to the sum of physical and intangible capital – $(csho \times prcc_f + dltt + dlc\text{–}act)/(K^{int} + ppegt)$. A detailed description of the intangible capital measure, K^{int}, can be found in Peters and Taylor (2017).

Table A2: Sample Comparison: Our Form 144 Filing vs. Non-Filing IPO Firms

This table reports a comparison of summary statistics of Form 144 filing and non-filing firms. The firm characteristics are measured as of the first fiscal year following the IPO date. The variables are constructed as described in the Appendix in Table A1. The column labelled "Filers" contains firms that file F144. The column labelled "Non-Filers" contains firms that do not file F144. All continuous variables are winsorized at the 1% and 99% levels, respectively. ***, **, and * indicate statistical significance of the underlying coefficient at the 1%, 5%, and 10% levels, respectively (based on a t-test allowing for unequal variances, and a non-parametric Mann-Whitney-Wilcoxon rank-sum test of equality of distributions, respectively).

	Means		Medians	
Variables	Filers	Non-Filers	Filers	Non-Filers
Ln(Assets)	3.60	3.03*	3.71	3.12**
Debt/Assets	0.12	0.14	0.00	0.00
Capex/Assets	0.04	0.05	0.02	0.02
Cash/Assets	0.65	0.73*	0.75	0.85
EBIT/Assets	-2.41	-0.92	-0.43	-0.55
PP&E/Assets	0.09	0.09	0.04	0.06