



Institutional stock trading on loan market information [☆]

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ABSTRACT

One of the most important developments in the corporate loan market over the past decade has been the growing participation of institutional investors. As lenders, institutional investors routinely receive private information about borrowers. However, most of these investors also trade in public securities. This leads to a controversial question: Do institutional investors use private information acquired in the loan market to trade in public securities? This paper examines the stock trading of institutional investors whose portfolios also hold loans. Using the Securities and Exchange Commission filings of loan amendments, we identify institutional investors with access to private information disclosed during loan amendments. We then look at abnormal returns on subsequent stock trades. We find that institutional participants in loan renegotiations subsequently trade in the stock of the same company and outperform trades by other managers and trades in other stocks by approximately 5.4% in annualized terms.

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1. Introduction

Loan syndication and the multiple financial innovations that have accompanied it have transformed the corporate lending market. Today, a large fraction of corporate loans is syndicated; in 2006 new corporate issuance of syndicated

loans was over twice as large as total bond issuance and over five times larger than equity issuance. A syndicated loan is originated and monitored by one bank, yet it is funded by a group (or a syndicate) of lenders. An important fact about loan syndication is that most participants in lending syndicates are not banks but institutional investors, including collateralized loan obligations, hedge funds, mutual funds, pension funds, and insurance companies.¹ It is estimated that in 2006, over 70% of high-yield loans in the United States, including leveraged buyouts and mergers and acquisitions financing, were held by institutional investors, up from under 30% in 1995. Overall, in 2006, there were 254 different non-bank investor groups

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¹ The fraction of mutual funds' portfolios that may be invested in illiquid securities is restricted. At least in the last ten years, however, according to a manager at a prominent mutual fund, most loans would be classified as liquid securities. To the best of our knowledge, there are no other regulatory restrictions on loan investments.

participating in the syndicated loan market, amounting to 720 different investment vehicles.² As a percentage of participants in the syndicated loan market, institutional managers have steadily increased from approximately 25% in the mid-1990s to over 70% by the end of 2006.

The availability of institutional funds allows banks to reduce their risk through diversification, improves loan-market liquidity, and ultimately benefits borrowers through easier and cheaper access to credit. However, most loans are unmistakably private agreements characterized by a material information flow between borrowers and lenders, including financial statements, covenants compliance information, waiver requests, financial projections, and plans for acquisitions (Standard & Poor's, 2007). Thus, institutional investors' simultaneous holding of loans and public securities raises concern about the use of private information disclosed by the borrower.

This issue has been addressed in the popular press but not in the academic literature. In particular, a March 2006 front-page article in the *New York Times* reported on the shift to institutional investors in the loan investor base and on their easy access to privileged information. The news was triggered by allegations raised by Movie Gallery, the second largest U.S. movie and game rental company, following a confidential conference call discussion of its financial results with nearly 200 participants in its lending syndicate, among them many hedge funds. The company was looking to amend its credit agreement in order to remain in compliance with the covenants. The content of the conference call was "inside information, as valuable to investors as a tip about an imminent takeover."³ There were no public news releases; nevertheless, in the two days following the conference call Movie Gallery's stock price dropped by 25%. More recently, insider trading accusations have swirled around Delphi Corporation's bankruptcy exit financing.⁴ Delphi has argued that at least one of its 17 investors, including investment banks, private-equity funds, and hedge funds, shorted the company's bonds after obtaining confidential information.

Are these isolated cases or is there systematic evidence of institutional trading on loan information? This is the question we propose to answer in this paper. It is a challenging question, because establishing a causal relation between private information released in the loan market and trading in public securities requires identifying institutional holdings. We use CDA/Spectrum data on institutional stock holdings to look at stock trading performance of institutional managers that invest in both the syndicated loan market and the stock market. Privileged information about a borrower can bestow an advantage to investors in bonds, credit default swaps, or the stock of the company. It is important to emphasize that we choose to look at the stock market due to data availability, not because trading in stocks is the most frequent or important type of trading using loan information. Limited data prevent us from evaluating the overall effect of information spillover from the loan market to the stock market, or to

public markets in general. Our focus here is to provide convincing evidence of the exploitation of private information disclosed by borrowers to lending syndicates. In that sense, our findings represent the lower bound of this effect (A recent study by Massoud, Nandy, Saunders, Song (2010) extends our findings to short-selling by hedge funds and finds similar results.).

To identify institutional loan holdings, we trace loan renegotiations that resulted in change in loan spread or loan amount. All lenders in a syndicate are governed by the same credit agreement; they all have direct but interdependent claims against the borrower, and they all need to agree on *material* amendments or waivers to the credit agreement.⁵ In practice, loan renegotiation boils down to a private conference call with the lenders, during which the borrower explains its financial condition and reasons for renegotiation. Small loan amendments could be approved by a majority vote; however, a typical loan contract requires the unanimous agreement of all lenders on a change of interest rate or loan amount. Thus, the Securities and Exchange Commission (SEC) filings of loan amendments reveal the names of institutional investors that participate in loan renegotiations. Though the actual loan holdings are not specified, the identities of the institutions with access to information disclosed by the borrower in the process of loan renegotiation are unambiguous. With the report date in the SEC filings and news search releases, we verify that information disclosed to lenders was not made public until after the loan amendment. Identifying the participants in loan renegotiations affecting the interest rate or loan amount has an additional advantage in that such discussions are likely to disclose private material information about the borrower. Having determined the exact time of the renegotiation and the identities of the lenders, we proceed to examine the returns on informed investors' stock trades following loan renegotiations.

We report four main findings. Firstly, we establish that institutional managers with loan holdings realize, on average, a 5.1% annualized abnormal return on stock trades of companies with loans following the loan amendment. Secondly, we look at the stocks of companies that had loan amendments and find that institutions that were part of the loan renegotiation outperform institutions that were *not* part of it by 5.3%. Thirdly, we find that the institutions that were part of the loan amendment trade in stocks of the companies in question and realize a return 5.5% higher than their return from trades on the rest of their stock portfolios. Finally, we confirm that outperformance takes place only following the loan amendment. These results are robust to controls for overall institutional expertise at investing in the loan market.

We acknowledge that the quarterly frequency of institutional stock holdings drawn from SEC 13(f) filings imposes a limitation on the analysis. We can only observe stock holdings at the beginning and end of each quarter. Therefore, we cannot be certain at what point in time within the quarter of the loan amendment the institutional investors made a given trade. However, we find outperformance only on the

² Standard & Poor's Leveraged Lending Review 4Q2008.

³ "As lenders, hedge funds draw insider scrutiny," *New York Times*, October 16, 2006.

⁴ "Delphi's woes: Chapter 11," *Wall Street Journal*, March 7, 2008.

⁵ We use "amendment" to refer to the closing date, the date on which modifications to the contract are approved, and "renegotiation" to refer to the time period preceding the closing date, during which the amendment is negotiated between the borrower and the lending syndicate.

part of investors in the stock and a loan of the same company, only for the stocks affected by the loan amendment, and only during the same quarter as the loan amendment. Taken together, these results strongly support the conclusion that institutions that take part in loan renegotiations trade on information thus acquired.⁶

Our findings contribute to the important and growing literature that investigates cross-market information flow, including Hotchkiss and Ronen (2002), Longstaff, Mithal, and Neis (2005), Blanco, Brennan, and Marsh (2005), Altman, Gande, and Saunders (2010), and Acharya and Johnson (2007). Ours is the first paper to look directly at institutional managers' investments in the loan market and to show evidence of the use of private information acquired there to trade in public securities. We are able to identify the time of private information release and the identity of the investors that have access to it; combined with the institutional stock holdings this allows us to address causality in a direct way. We also add to the findings of Massa and Rehman (2008), who examine stock trading by mutual funds that belong to bank families, by identifying the channel of information transmission between the loan and equity markets (although our prime focus is the broad phenomenon of direct institutional investment in loans and information flow due to direct access to private information about the borrower). More recently, Bushman, Smith, and Wittenberg-Moerman (2008) study the price discovery of the equity prices of firms with syndicated loans. They find that firms with timelier information dissemination from the borrower to the lenders also have faster price discovery in the equity market; however, this is only true when the institutional investors are involved in the firms' syndicated loans. This evidence is consistent with institutional investors in the syndicated loan market exploiting their access to private information to trade in the equity market.

More broadly, our paper relates to the so-called “forensic finance” literature (Ritter, 2008), which provides micro-evidence of unethical or illegal practices in finance. Trading on private information disclosed as part of a borrower-lender relationship violates a confidentiality agreement. It can be harmful to the borrower, but its implications are more far-reaching as it can also create an imbalance between traders in a public securities market. Although there is anecdotal evidence of such violations, it is hard to test how widespread they are because the loan market is private. We overcome this problem by using contractual details – the unanimous vote of all lenders for the material amendment – in order to unambiguously identify those investors with access to information and to the timing of the event. Combining hand-collected information from SEC filings with 13f data on stock holdings, we present large-sample evidence of trading on loan information around loan amendments.

The rest of this paper is organized in four sections. In Section 2 we provide a detailed discussion about treatment of private information on the loan market and insider trading regulation in general. Section 3 outlines the

empirical framework and discusses sample construction. Section 4 presents our central results and robustness tests. Section 5 summarizes our conclusions.

2. Disclosure requirements and regulation of insider trading on loan-market information

The bank's role in information processing is a key difference between bank lending and issuing public securities (Leland and Pyle, 1977; Holmström, 1979; Diamond, 1984). It is not surprising, then, that lenders have information that is not available to other securities' holders. A typical loan agreement requires the borrower to disclose “material” information in a timely fashion to its lenders. However, the borrower is not required to simultaneously disclose this information to the public. Unless the documentation or other information made available by the borrower to its lenders is already publicly available, it is protected by a confidentiality agreement and, in that sense, is not subject to SEC Regulation Fair Disclosure (Regulation FD).

In this paper, we use material amendments as instances that are associated with private conversations between lenders and borrowers. (Note that this is just a subset of situations during which the borrower might have disclosed private material information to its lenders.) Empirically, roughly 40% of amendments are announced through 8-K reports, with the rest reported through 10-Q or 10-K reports.⁷ Reporting requirements for the 8-K exclude agreements reached within “the ordinary course of business.”⁸ Material events that would be required to be disclosed through an 8-K report primarily include events that only happen rarely to a company, “such as delisting from a national securities exchange, bankruptcy and restatements of financial statements.” Given that amendments are an intrinsic feature of a loan contract and given the large volume and high frequency of amendments, it would not be surprising that, under current regulation, a company would have discretion to consider loan amendments as part of the ordinary course of business rather than as events in the same category as delisting and bankruptcy. The SEC also requires disclosure of material contracts; in that sense, the text of the amendment should be required to be disclosed through 10-Q or 10-K filings. Overall, we were able to locate amendment documents in 85% of the cases.

Broadly speaking, trading public securities on the basis of material non-public information is illegal under U.S. federal law. With the exception of information disclosed under the loan agreement, all information disclosures by public companies must be immediately released to the public in order to comply with Regulation FD whenever it is “reasonably foreseeable” that the information will be used to trade in a public market.⁹ For example, any information

⁷ This is consistent with Roberts and Sufi (2009), who also follow first disclosure of material loan amendments and find them through 10-K and 10-Q as well as 8-K reports.

⁸ For a discussion of the reportable items, see SEC Web site <http://www.sec.gov/rules/final/33-8400.htm>.

⁹ There are two federal statutes regulating insider trading: The Securities Exchange Act of 1934 and the Insider Trading Sanctions Act of 1984. These are prosecuted at the federal level. Regulation FD was

⁶ Quarterly data availability is a general limitation in the literature analyzing institutional investors' stock trading (e.g., Dennis and Stickland, 2002).

disclosed to stock analysts must be immediately released in a public statement. Violation of Regulation FD constitutes grounds for filing an insider trading lawsuit. It is fair to say that the conflict in handling private information while investing in public securities, while not regulated directly, is well-understood by loan-market participants. Many practitioner-oriented publications have commented on the matter (Standard & Poor's, 2007; Loan Syndications and Trading Association, 2007; Mugasha, 2008). If not illegal, this practice is considered unethical. Institutional investors typically adopt "ethical walls," primarily consisting of preemptive paper procedures meant to preclude trading on private information acquired in the loan market. These ethical walls are a softer version of what have come to be known as "Chinese walls" between investment and commercial banking, typically associated with physical and functional separation within a bank and with formal wall-crossing procedures.

On the other hand, the lead bank usually prepares a public version of loan-related information to be circulated among self-identified public investors.¹⁰ This might be effective in keeping private information from leaking into the public domain at the marketing stage, but once the loan is placed, restricting the syndicated participants' access to public information is "tricky to pull off in practice" (Standard & Poor's, 2007). If a loan needs to be amended and such an amendment is material, all lenders will be called in to approve or decline contract revisions. Lenders could designate another agent to review private information for them, but this tends to be more of a formality than an effective mechanism for protecting private information.

The reality is that standards for the separation of public and private information among institutional investors are largely discretionary. It was not until October 16, 2006 – coincidentally, the day when Movie Gallery's insider-trading allegations appeared in the news – that the Loan Syndications and Trading Association (LSTA) drafted and circulated among its members a set of principles designed to help loan-market participants handle confidential information. In May 2007, there was still an ongoing debate on what constitutes public and private information.¹¹ The main challenge was that many institutional investors participating in the loan market were small organizations for which implementing ethical walls was a challenge. As one hedge-fund lawyer is said to have remarked, "You can't put an ethical wall down the middle of someone's brain."¹²

(footnote continued)

introduced by the SEC in 2000 as an additional rule governing how companies should disclose information.

¹⁰ Although loan syndication is an over-the-counter market, deals are often publicized to potential investors through an online workspace, such as IntraLinks. It is through this type of service that banks make available public and private sets of information and it is the investors' responsibility to choose the correct set of documents.

¹¹ From "Debate on public/private information deepens on LCDS growth," *Reuters*, May 17, 2007: "Speaking at the LSTA's Loan Only Credit Default Swaps conference in New York Wednesday, Elliot Ganz, general counsel of the Loan Syndications and Trading Association, said there is a call for the loan market to reach consensus on what constitutes public and private information as a broader investor base enters the loan market."

¹² "As lenders, hedge funds draw insider scrutiny," *New York Times*, October 16, 2006.

The focus of this paper is to provide evidence that institutional investors use private information from lending relationships to make profitable equity trades. By documenting that institutional entry into the loan market created a new opportunity to exploit private information, this paper raises the issue that the current regulatory framework for insider trading is inconsistent in its treatment of the loan market. To design new regulatory measures, however, one should understand the full scope of the impact of institutional investment on the loan market.

Why should all syndicate members receive information that is not provided to other securities holders? Could trading on loan information be prevented by simply restricting access to information for some syndicate members? The issue is asymmetric information within the lending syndicate. Loan syndication carries important economic costs due to the information asymmetry between the originating (lead) bank and participant investors (Ivashina, 2009). Thus, simply restricting institutional investors to private information would severely hamper the agency of the lending syndicate and increase the cost of corporate lending. Such a measure might also adversely impact loan-market liquidity (e.g., Gupta, Singh, and Zebede, 2008).¹³ However, we need to be clear that issues raised here have little to do with requiring an immediate disclosure of loan amendments. That said, it is not obvious that such a measure would keep private information disclosed in the loan market from being used to trade in public securities.

It is hard to assess the full economic impact of institutional investors in the corporate loan market. Many institutions invest in loans through the secondary market and precise volume numbers for that market are difficult to find. According to a private source, in 2006, JPMorgan alone managed nearly 500 institutional accounts, representing over \$450 billion in capital, that regularly invested in the secondary loan market. Table 1, Panel A reports institutional participation in the primary loan market. In 2006, institutions provided close to 22% of the funding in that market. However, it is important to note that institutional investment is concentrated in the highly leveraged (or "junk") segment of the loan market, such as loans financing leveraged buyouts.¹⁴ In 2006, conditional on leveraged transactions, institutional investors funded nearly 60% of primary loan origination. The economic magnitude of regulatory actions is therefore likely to be significant, but mainly affecting the less-creditworthy segment of the market.

Many different financial institutions invest in corporate loans. Table 1, Panel B indicates that in 2006, 24% of all primary

¹³ Bainbridge (2000) provides a detailed literature review on the costs and benefits of insider-trading regulation in general. For cross-country differences in the regulation of insider trading, see Bhattacharya and Daouk (2002) and Fidrmuc, Goergen, and Renneboog (2006).

¹⁴ A common explanation for this is that, after the collapse of Drexel Burnham Lambert, high-yield loans (typically loans to companies that do not have an investment-grade rating and/or high leverage) became popular as an alternative to "junk" bonds, attracting essentially the same investor base. Thus, traditionally there are more investors in the leveraged segment of the market. Over time, institutional investors expanded outside the leveraged segment, but most institutional investment is still concentrated there while banks continue to dominate the investment-grade segment of the market (Ivashina and Sun, 2010).

Table 1

Syndicated loan issuance and funding.

The numbers correspond to the U.S. primary syndicated loan market. (Leveraged+Investment-grade+Other=Total). “Leveraged loans” is a term commonly used to describe less-creditworthy loans such as those financing leveraged buyouts. Non-investment-grade-rated loans are a type of leveraged loan, but not all leveraged loans are rated. Criteria for classifying non-rated loans as leveraged vary across different institutions. The numbers reported here are based on the Loan Pricing Corporation/Reuters definition—loans to corporate borrowers with a drawn spread of at least LIBOR+250 and/or non-investment-grade senior unsecured debt rating. Panel A is compiled from Loan Pricing Corporation/Reuters; Panel B is based on Standard & Poor’s (S&P) Leveraged Lending Review 4Q2008. In Panel B, finance companies are non-depository financial institutions that include financing subsidiaries of nonfinancial, general consumer, and business finance companies. Collateralized debt obligations (CDOs) are structured securities backed by corporate loan collateral. A high-yield fund is a mutual fund that invests at least two-thirds of its portfolio in non-investment-grade debt products. Prime rate funds are mutual funds that aim to match the return of the prime rate by investing in corporate debt products. Securities firms are securities brokerages not otherwise classified. S&P only tracks data for finance companies and for hedge, distressed, and high-yield funds starting in 2002.

	Leveraged			Investment-grade	Other	Total	
Year	Bank funds	Institutional funds	Total				
Panel A: Syndicated loan volume (billion USD)							
2000	259.96	50.00	309.96	603.94	282.28	1,196.18	
2001	186.08	31.86	217.94	671.51	217.98	1,107.43	
2002	165.33	99.20	264.53	502.11	202.70	969.34	
2003	210.15	118.42	328.57	419.18	182.14	929.89	
2004	257.02	223.43	480.44	612.03	255.21	1,347.69	
2005	259.85	240.98	500.83	669.35	324.41	1,494.59	
2006	245.93	366.15	612.08	721.30	339.12	1,672.49	
2007	262.69	425.81	688.50	657.75	340.56	1,686.81	
Year	Banks (%)	Finance companies (%)	Insurance companies (%)	CDOs (%)	Hedge, distressed, and high-yield funds (%)	Prime rate funds (%)	Securities firms (%)
Panel B: Composition of investors in leveraged loans at loan origination							
2000	44.8	4.3	1.3	33.8	0.0	14.1	1.6
2001	36.1	9.2	4.6	39.0	0.0	8.8	2.2
2002	30.5	7.6	4.0	42.4	0.7	12.8	2.0
2003	23.6	9.2	4.6	44.0	7.0	10.9	0.6
2004	28.7	6.4	3.5	41.9	6.2	12.0	1.4
2005	25.1	7.0	2.1	44.5	8.3	11.9	1.1
2006	18.2	5.9	2.1	48.1	13.4	10.2	2.0
2007–1H	14.2	4.1	2.0	52.9	17.5	7.1	2.3
2007–2H	16.6	2.9	1.2	23.1	46.7	7.2	2.3

investors in leveraged loans were hedge funds and mutual funds and 48% were collateralized debt obligations (CDOs). While a hedge fund can simultaneously hold stock and loans of the same company, that is not the case for a high-yield fund or a CDO which do not hold stocks in general. Nevertheless, the investment management firms that structure such CDOs and act as CDO managers can. In fact, we find that it is the management firm and not a specific investment vehicle that signs off on loan modifications as a delegated counterparty. In addition, private information about the borrower could also be used to trade in bonds or credit default swaps (CDS), but we only observe stock positions of individual investors. In this paper we design the tests and select the sample to establish causality of information flow. With the data available to us, we cannot identify institutions that benefit the most from trading on information received in the loan market or identify institutions that are most likely to trade on this information.

3. Methodology and data

3.1. Sample construction

We need to know the identities and holdings of the institutions that invest in stock and loans of the same company. To fully understand the scope of insider trading,

we would like to know investors’ holdings of all securities for a given company, including short positions. However, besides information on stock holdings which are available from the CDA/Spectrum 13f database, there is limited investor-specific information for other securities.¹⁵ Reporting of stock holdings is mandated by the Securities Act Amendment of 1975, which requires all institutions to report their investments on a quarterly basis to the SEC.

DealScan’s database records syndicated loans contract details including the list of lenders at loan origination. We need to identify *contemporaneous* holding of stocks and loans and, in that sense, observing loan information only at the origination may be a restriction. Like stocks, loans are traded in the secondary market. Although many institutions invest in loans at their origination, we do not know how long they retain loans in their portfolios. In addition, many investors participate in lending syndicates by purchasing fractions of loans in the secondary market. Thus, without a centralized source of data on loan holdings, identifying loan investors represents our main challenge.

¹⁵ CDA/Spectrum data have been extensively used in the previous literature. For a thorough discussion of the structure and limitations of the data, see Gompers and Metrick (2001).

To get around this problem, we collect data from SEC filings of loan amendments. Loan renegotiations are very frequent and can be triggered by covenant violations, changes in market conditions, or a company's financing needs.¹⁶ According to Roberts and Sufi (2007) who look at a similar sample, 90% of loan contracts are renegotiated before their maturity. In that sense, firms with loan amendments are likely to be representative of the universe of firms borrowing in the syndicated loan market. However, some loan amendments can be approved by majority vote of the lenders; this would typically only require coordination between a few of the largest lenders. When only a partial vote is required, it is hard to say which investors had access to the information related to the amendment or even to identify the investors holding the loan. However, changes in the credit agreement related to the repayment schedule, loan amount, or interest rate – typically referred to as “material” loan amendments – require the consent of *all* lenders. By tracing material loan amendments, we assure that the list of lenders on the document accurately reflects the identities of the institutions in the lending syndicate at the time of the loan amendment. In addition, major changes in loan agreements are typically associated with important corporate events. Because a renegotiation of loan terms entails a *private* conference call in which all lenders participate, it represents a point in time when institutional investors are the first participants in the loan market to receive privileged information.

Amendments to credit agreements are typically disclosed by the borrower as part of its 8-K, 10-Q, and 10-K SEC filings. DealScan also collects the dates of renegotiations and specific changes to loan contracts. We start with the sample of U.S. public firms with loans identified in DealScan between 1996 and 2005 and whose loans have been amended between 1998 and the first half of 2007. We exclude stand-alone revolving lines from the sample as these are typically held by banks and not institutional investors (Kashyap, Rajan, and Stein, 2002; Gatev and Strahan, 2008).¹⁷ We focus on loan amendments that result in interest rate or loan amount changes to assure that the full syndicate is reported.¹⁸ In addition, we need that the loan amendment and the information disclosed during loan renegotiation be private. Therefore, we restrict our attention only to the first price amendment, because subsequent amendments are likely to be related and could be anticipated.

Using these criteria, we find a total of 416 loan amendments.¹⁹ We then search firms' SEC filings for the actual amendment document and record the identities of the investors whose signatures appear at the end of the document. We require that institutional investors simultaneously hold stocks and loans of the same company in their portfolios. Thus, once we have collected managers' names, we match them back to CDA/Spectrum to obtain loan holdings. We say that, in a quarter of the loan amendment, an institutional investor had a joint holding if this investor was part of the lending syndicate according to an SEC filing and was holding the stock of the same company either at the beginning or the end of the quarter. We exclude investors that did not directly sign the amendment but instead had a law firm or in-house lawyer sign for them.²⁰

We find that amendments to loan contracts were, on average, disclosed about 60 days after renegotiation took place. Because we rely on the information released during loan modification to only be available to members of the lending syndicate, we also search public news releases. Overall, however, in less than 7% of the cases, loan amendments were mentioned in the public press on the same day that renegotiation took place. We exclude these cases from the sample. Our final sample contains 131 amendments and 274 amendment-manager observations with stock holdings. The fact that 31% (131 out of 416) of cases are characterized by joint holdings indicates that this is an important phenomenon.

We should point out that we do not observe the date when the renegotiation starts, only the date of the amendment. However, we test if our results are sensitive to extending the event window around the amendment date. Anecdotal, at least in the years of the credit boom (2004 to the first half of 2007), many amendments were passed within a day. This was a result of large institutional demand for loans and of the lead bank being incentivized by the fee structure to pass amendments and having control over loan allocations in the primary and even the secondary markets.²¹ Our results are consistent with this observation.

The other concern is that the information disclosed in the renegotiation could be public. It is true that the renegotiation between a borrower and its lenders is held privately and we verify that the amendment is not disclosed to the public until later. However, information that

¹⁶ Dichev and Skinner (2002) show that covenant violations occur in approximately 30% of loans.

¹⁷ Although loan modifications are at the facility level, they would still require consensus from all of the investors governed by the same loan agreement.

¹⁸ Performance pricing, a common feature of loan contracts, implies automatic adjustment of the interest rate paid by the borrower as a step function of its financial ratios. Basically, performance pricing allows state pricing and it is believed to be a positive contractual feature (see Tchisti, 2006). In our sample, we find cases in which performance pricing is replaced by a flat interest rate, typically the highest rate under a performance-pricing schedule. We categorize such amendments as ‘price amendments’ and include them in our analysis.

¹⁹ Naturally, this is just a subsample of all the loan amendments. Roberts and Sufi (2007) provide a comprehensive study of the loan amendments. Our sample appears to well represent the overall population of first loan amendments. In particular, we also find that, on average, it takes 18 months from the date of loan origination for the loan to be amended.

²⁰ Spectrum 13f information is provided for mutual funds at the fund level. For example, stock holdings are reported as a whole for Vanguard and there is no separate reporting for, say, the Vanguard Winsor Fund. As a result, there is a chance that the entity holding the loan may be different from the entity holding the firm's stock. A similar issue arises with banks. Although the statistical significance of the results is sensitive to the number of events in our sample, we were able to verify in an unreported test that the results remain economically large for the sample excluding mutual fund families and banks.

²¹ This information is based on our interviews with market participants.

was the grounds for the amendment (e.g., an acquisition or reorganization) could be made public independently of the amendment. This should bias against our findings. However, we also verify this point by looking at the returns around the amendment (results are reported in Appendix A). If the information were already public (at least to the loan-market participants), then we would expect to find large abnormal returns around the amendment date; however, that is not the case. We present a detailed discussion of other potential biases at the end of this section.

3.2. Testable hypotheses

Our empirical analysis is structured around an information event: material loan amendment. A material amendment is likely to be associated with valuable information on a corporate event and by requiring a unanimous agreement of its lenders, it provides us with a full list of investors that have access to such information. Our basic hypothesis is that managers who participate in loan amendments subsequently profit from trading in stocks of the same company.

Ideally, we would like to be able to classify amendments into amendments that are linked to good and amendments that linked to bad information about firms' fundamentals. Loan amendments' details are typically available through SEC filings, but they tend to be technical. Often, amendments are triggered by covenants violations, but these violations might be anticipated because renegotiations are a distinctive feature of a loan contract. In general, even when a specific reason is mentioned, it is hard to make inferences about the direction of the trade.²² Thus, we do not know the nature of the information disclosed in loan renegotiations, and we also do not observe full composition of the investors' portfolios (bonds or short positions). In that sense, we cannot clearly anticipate if the investors sell or buy the stocks.

To overcome this issue, we look at abnormal returns (AR) realized on trades of the institutional investors following loan renegotiation. That is, we look at average

($CAR \times Trade\ direction$). CDA/Spectrum stock holdings data are quarterly; thus, in the quarter of the loan amendment, we look at the changes in the stock holdings for managers that were part of the loan renegotiation in the stock that was affected by the loan renegotiation. We then look at the abnormal returns realized on these trades. We expect that institutions with access to private information sell stocks that go down and buy stocks that go up. We assume that stock trades take place immediately after the loan amendment and we look at the cumulative abnormal returns realized between the date of the amendment and the date the information about the amendment becomes public.

It is important to point out that not all material amendments might actually contain information about the firm's fundamental value. Changes in loan amount could be a response to marginal changes in liquidity needs or a response to covenants compliance as opposed to a major corporate event. In other words, it is not clear why good or bad news about the borrower would lead to a marginal increase or decrease in the loan size; most likely, such news would be incorporated through adjustment in spread.²³ In that sense, looking at the subsample of price amendments should produce a sharper test of trading on private information. Overall, not being able to isolate amendments containing fundamental information about the borrower should bias against finding evidence of insider trading. Of 131 amendments, 51 are price amendments (i.e., modification in interest rate). Throughout the paper we report results for the full sample as well as for the sample of price amendments.

The outperformance of the investors that hold stock and loans of the same company following loan modification is consistent with the use of non-public information from the loan market to trade in the stock market. However, access to private information may not be the only possible explanation for the outperformance. Most of the alternative explanations that do not involve use of private information could be classified in three broad groups: (i) there is something unique about the firms with loan amendments, for example, there is a relevant public information release within the same quarter of the loan amendment; (ii) there is something unique about the managers that participate in the loans amendments, for example, they are more skilled (or just smarter); and (iii) endogeneity of the investment choices (i.e., factors that drive the decision to combine stock and loans for a given company), for example, institutions that invest in stock and loans of the same company do so because they know a lot about the firm.

To rule out these alternative explanations, we formulate three additional hypotheses. First, among investors that hold stock of the borrower with loan amendments, only investors that were part of the loan amendments show outperformance. Second, investors that were part of the loan amendments (informed investors) show outperformance only in the stocks of the companies with loan amendments. Third, outperformance of the informed investors in the amended stock only takes place around the loan amendment. In other words, these additional hypotheses are designed to show that

²² For example, Cash America International, September 2003 10-Q, "In connection with the acquisition of Cashland, the Company increased the total commitment amount under its U.S. line of credit from \$90,000,000 to \$135,000,000 and extended the maturity date of this line of credit for an additional year to July 31, 2006. The interest rate on the line of credit varies from 1.50% to 2.25% over London Interbank Offered Rate (LIBOR), depending on the Company's cash flow leverage ratio as defined in the credit agreement. The Company pays a fee of 0.375% per annum on the unused portion of this line of credit. The amended credit agreement also changed certain financial ratios that the Company has to maintain." Buca, Inc., September 2003 10-Q, "On November 4, 2003, we amended our credit agreement. Under this amendment, which waived covenant defaults, we paid a fee of approximately \$100,000. The agreement also increased the interest rate of the credit facility to the lower of our lenders reference rate plus 0.75% to 2.25% or Eurodollar rate plus 2.25% to 3.75% (3.35% to 4.85% as of November 3, 2003). The credit agreement as amended contains covenants that place restrictions on sales of properties, transactions with affiliates, creation of additional debt, limitations on capital expenditures, maintenance of certain financial ratios, eliminates future sale-leaseback transactions and includes other customary covenants. Borrowings under the credit amendment also continue to be collateralized by substantially all of our assets."

²³ Syndicated loans are issued to very large firms that are unlikely to be financially constrained.

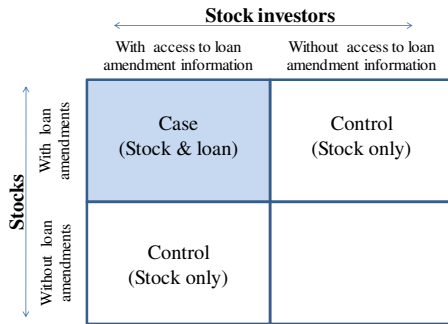


Fig. 1. The empirical methodology is based on performance comparison of investors with access to private information on stocks with loan amendments to: (i) other investors with the same stock and (ii) other stocks held by the same investor.

outperformance is specific only to the investors that were part of the loan amendment, only to the stocks that are affected by the loan amendment, and only at the time of the loan amendment. Empirically we test these hypotheses by comparing the performance of the case group (informed investors' holdings of stocks with loan amendments) to the corresponding control group. The basic intuition for the first two hypotheses is summarized in Fig. 1.

For the comparison across managers, we select a control group of comparable investors using managers in the same beginning-of-the-quarter size quintile and of the same institutional type, as classified by Spectrum 13f.²⁴ However, it could be the case that managers that invest in the loan market in general develop market expertise or, follow secondary market data and/or subscription news, such as Loan Pricing Corporation Gold Sheets. In that case, these managers might not be comparable to those that do not invest in the loan market. On the other hand, firms' decisions to take loans as part of their capital structure might be endogenous, and therefore, these firms are not comparable to publicly traded firms without loans. We address these concerns in the robustness section by re-examining the case and control sample to managers that invest in both, stock and loan markets and to stocks of companies with loans outstanding.

Finally, we believe that differential outperformance of informed managers and careful verification of news releases should rule out the possibility that the results are driven by public news. Yet, the stock data are quarterly, and in over half of the cases the amendment becomes public in the quarter of the loan amendment. That is, within the same quarter investors that were not part of the loan amendment could trade once the amendment becomes public. This, however, should bias against us finding outperformance for the informed investors. Because we know when information was released to the lenders and when it was released to the public, we also provide additional evidence by reporting throughout the results for the full

sample and subsample where the amendments do not become public until the next quarter. This allows us to ensure that the marginal effect is, indeed, attributed to trades on private, rather than public, information. Throughout the paper, we report results for the full sample and the sample where loan information becomes public in a different quarter.

3.3. Methodology for assessing excess returns

To test our hypotheses, we use event-study methodology. Abnormal returns are calculated with daily Center for Research in Securities Prices (CRSP) stock-price data using (i) a one-factor market model (excess returns are calculated using CRSP value-weighted returns) and (ii) a four-factor market model which also includes Fama and French (1993) factors and the Carhart (1997) momentum factor. We report both results as well as unadjusted excess returns. The market model is estimated from trading day ($t_1 - 250$) to day ($t_1 - 50$), where t_1 is the date the loan amendment is approved, and corresponds to

$$\tilde{R}_{it} = \alpha_i + \beta'_i \tilde{R}_{m\tau} + \varepsilon_{it},$$

where \tilde{R}_{it} is the return in excess of the risk-free rate and $\tilde{R}_{m\tau}$ is the vector of market-factor realizations on day τ . The estimated regressions coefficients (factor loadings) are then used to compute predicted returns. For a given day t , abnormal returns for firm i (AR_{it}) are the difference between the predicted and actual returns²⁵:

$$AR_{it} = \tilde{R}_{it} - (\hat{\alpha}_i + \hat{\beta}'_i \tilde{R}_{mt}).$$

Different loan-amendment events are disclosed to the public with different time lags. To make the abnormal return for each event comparable, we use annualized cumulative abnormal returns (CAR) computed from the date the loan amendment takes place (t_1) and the date the amendment becomes public (t_2). The timing of events is summarized in Fig. 2.

Our hypothesis is that managers who invest in the loan market *profit* from trading stocks based on private information. Hence, we look at CAR weighted by trade direction. For a manager j , trade direction (D_{ji}) is computed quarterly using CDS/Spectrum data on stock holdings. We consider two alternative definitions: (i) D_{ji} is equal to -1 , 0 , or 1 if, over the past quarter, the investor reduced, did not change, or increased his position in a given stock; and (ii) D_{ji} is equal to -1 if, over the past quarter, the investor reduced his position in a given stock and equal to one otherwise. The second definition is used to account for the fact that not selling a stock that went up or not buying a stock that went down could be a proactive decision by the investor.

For each amendment, the number of observable managers for M_i can be different. Thus, each amendment can appear in our database several times. While the median number of identified managers per amendment is one,

²⁴ Because the classification of institutional type in Spectrum 13f is inaccurate after 1998, there is an increase in institutions misclassified into type 5 ("other institutional type"). To correct this problem, for managers classified as type 5 after 1998, we replace the value with the one reported at the end of 1998, if such information is available.

²⁵ Strictly speaking, i identifies an amendment, as opposed to a firm. We keep the subscript for simplicity of notation. There are only eight firms with repeated observations and, for these firms, the median number of days between the events is 590.

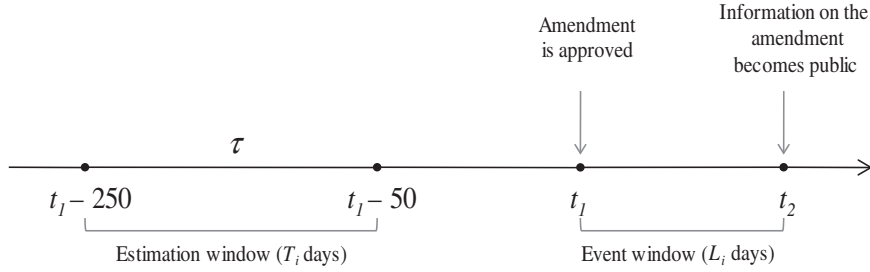


Fig. 2. Time line for the event study. t_1 is the date the loan amendment takes place; t_2 is the date the amendment becomes public.

some amendments include up to eight managers. To assure that amendments with multiple identified managers are not biasing the aggregate result, we take averages for each amendment across managers; in other words, we only consider one observation per amendment.²⁶ Reported returns correspond to

$$\frac{1}{N} \sum_{i=1}^N \frac{\sum_{j=1}^{M_i} D_{ji} \text{CAR}_i}{M_i},$$

where N is the number of amendments. We weight the returns by trade direction and not by trade size. The underlying assumption is that the direction of the trade reflects the nature of the information disclosed during the amendment and is exogenous to the managers; the information released in the loan amendment should have the same implication for the trade direction. The size of the trade, on the other hand, is endogenous to the portfolio and managers' characteristics.

To test whether abnormal stock returns following the loan amendments are statistically different from zero, we calculate Z-statistics following Campbell, Lo, and MacKinlay (1997, Section 4).²⁷ For the one-factor model, standardized cumulative abnormal return for firm i over the holding period t_1 to t_2 is defined as

$$\text{SCAR}_i = \frac{1}{\sqrt{L_i}} \sum_{t_1 \leq t < t_2} \left(\frac{\text{AR}_{it}}{\sigma_i \sqrt{C_t}} \right),$$

where t_1 is the date of the loan amendment, t_2 is one day before the amendment is publicly disclosed, and L_i is the number of days in the predicting period. σ_i^2 is the residual variance

$$C_{it} = 1 + \frac{1}{T_i} + \frac{[\tilde{R}_{m\tau} - E(\tilde{R}_m)]^2}{\sum_{\tau=1}^{T_i} [\tilde{R}_{m\tau} - E(\tilde{R}_m)]^2}$$

is a correction corresponding to increase in variance due to prediction outside the estimation period. T_i is the number

of days in the estimation period. Notice that there is cross-sectional variation in T_i due to differences in data availability. We exclude amendments with fewer than 50 observations in the estimation period.

The Z-statistic is approximately standard normal distributed and is equal to

$$Z = \frac{\sum_{i=1}^N \bar{D}_i \text{SCAR}_i}{\sqrt{\sum_{i=1}^N \bar{D}_i^2 ((T_i - 2)/(T_i - 4))}},$$

where $\bar{D}_i = (\sum_{j=1}^{M_i} D_{ji})/M_i$.

Because standardized abnormal returns on individual securities are assumed to be independent random variables, cross-sectional covariances are assumed to be zero. For this assumption to hold, there needs to be little overlap between event windows in calendar time. Fig. 3 presents the distribution of amendments over time, clustered by calendar month of the amendment. On average, the median number of amendments in a given month is one; thus, the bias is likely to be small.²⁸

3.4. Potential sample biases

By looking at material loan amendments we are able to unambiguously get the identities of the investors that had access to information disclosed in the renegotiation process. However, this raises a few selection bias concerns. First, are the investors participating at the loan amendments representative of the investors in loans in general? The problem is we do not know for sure what "in general" means; we observe the syndicate at the origination, however, there is only limited information about secondary market transactions. Hence, it is hard to assess if the participants at loan origination or the participants that acquire shares through the secondary market are a representative group of investors.

Table 2 compares the structure of the lending syndicate at origination with the lending syndicate at the time of renegotiation (on average, there are 18 months between the loan origination date and the first amendment date). To track the evolution of the syndicate, we compare the list of investors at the origination as reported by DealScan to the investors identified through SEC filings (unconstrained to the CDS/Spectrum data). SEC filings provide us with a

²⁶ Similarly for the control group, each manager in the sample could have several comparable investors. Hence, before taking an average across managers for a given amendment, we construct an average control for each manager. This prevents us from biasing the results toward those managers with the largest number of control matches.

²⁷ There are two main differences with respect to the standard framework. First, abnormal returns are typically aggregated along two dimensions – through time and across securities. Here, we aggregate along three dimensions – through time, across investors, and across securities. Second, the relevant random variable is not the abnormal return but the abnormal return times trade direction.

²⁸ The results are also robust to exclusion of the amendments that took place in March 2001, the month with the highest concentration of amendments.

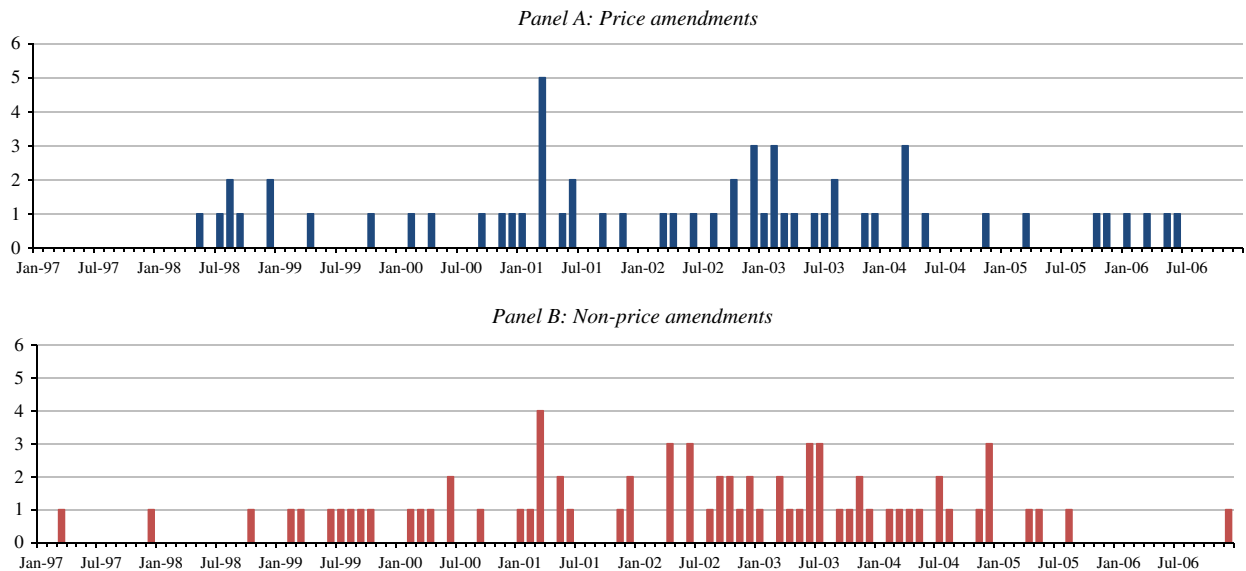


Fig. 3. This figure presents the distribution of loan amendments over time (clustered by calendar month of the amendment). The vertical axis counts the number of amendments in a given month; e.g., there is one non-price amendment in March 1997 and one non-price amendment in December 1997. Panel A: price amendments and Panel B: non-price amendments.

Table 2

Evolution of the lending syndicate.

This table summarizes loan-amendment data collected from SEC filings and public news releases. The sample covers loans originated between 1995 and 2005 and amendments that took place between 1997 and 2007 that changed the loan pricing. We only count the first price amendment; thus, each observation corresponds to a different loan.

	Median	Mean	Std. dev.
Syndicate size (at the loan origination)	11	18.6	21.3
Syndicate size (at the loan amendment)	13	26.2	34.6
% of the syndicate members remaining	85.7	76.2	27.5
% of lead arrangers remaining (Tier 1 lenders)	100	81.4	28.7
% of co-arrangers remaining (Tier 2 lenders)	83.3	65.5	41.4
% of participants remaining (Tier 3 lenders)	66.7	57.5	39.5

comprehensive set of lenders' names but they do not give us shares held by the bank. Thus, numbers in Table 2 are based on the number of participants and not on their actual shares. This is an important observation, as literature on syndicated lending often relies on assumptions about stability of the lending syndicate (e.g., Ivashina, 2009). To the best of our knowledge, this is the first evidence about what happens to the lending syndicate after the loan origination.

Interestingly, a large fraction of the members remains as part of the lending syndicate. In particular, it appears that lead banks (lead arrangers) do not sell their loan holdings, although nearly half of the participants change by the time of renegotiation. Also, the syndicate becomes larger: the number of members in an average lending syndicate increases by over 40%. Institutional investors typically act as participants in the lending syndicate (i.e., they take small stakes). Thus, significant change occurs in the

configuration of institutional investors due to secondary market trading prior to the time of renegotiation; however, it is unlikely that these changes are driven by anticipation of loan amendments.

One alternative hypothesis not considered in the previous sections is the case where institutional investors announce a decision to reduce (increase) their stock holdings in a borrower triggering a decline (increase) in the borrowing firm's stock price. To believe in such a reverse-causation hypothesis, one would need to believe that changes in the borrowers' creditworthiness trigger changes in the demand for a monitoring effort on the part of the syndicate and, accordingly, a change in the stock holdings of the syndicate members in advance of the market's reaction to the news of changes in loan rate and holdings.²⁹ However, borrower's due diligence and monitoring are delegated to the lead bank as it is the most informed agent in the lending syndicate, which according to Table 2 remains as part of the lending syndicate until after the loan amendment.

The fact that the amendments sample ends in June 2007 could introduce a bias toward firms that amend their credit agreements soon after origination date. Both good and bad performance could lead to early loan modifications. Therefore, it is not clear if our sample would be biased toward better or worse companies. We are also likelier to capture the full sample of amendments for loans originated in the earlier years of the sample. To the degree that loans originated in different years have different characteristics, this could bias the results. To address these issues we restrict the sample of loans to those originated before

²⁹ This is consistent with Massa, Yasuda, and Zhang (2008) who find that the withdrawal risk of the firm's bond investor base has a negative and significant effect on the leverage of the firm.

the end of 2005. Given that the average amendment takes place within 18 months of loan origination (median nine months), the remaining bias is likely to be small. Constraining the sample to earlier years yields similar results.

The sample is conditioned on firms that renegotiate successfully and survive until the next quarter, so that we do not see the returns associated with firms that fail after renegotiation. Given that many renegotiations are preceded by financial covenant violations, this could lead to a survivorship bias. Suppose that covenant violations always precede a renegotiation, and suppose that institutional investors on the lending syndicate are always optimistic about the chances of surviving. Firms that successfully renegotiate and survive until the end of the quarter will have big stock price jumps, because Chapter 11 was a real threat that they avoided. This hypothesis is not consistent with the abnormal returns around loan amendments reported in Table 2. In addition, this would lead to outperformance by all the investors that hold the stock of the amended companies, but we only find it for the investors that were part of the loan amendment.

Similarly, we condition on firms that renegotiate successfully and survive until the next quarter, so that we do not see the returns associated with firms that fail after renegotiation. But it may be the case that institutional investors experience large abnormal negative returns after buying equity in firms that go bankrupt. Because we use an event study approach, these observations do not show up in the data. In that sense, we could be only looking at “winners” and this might lead us to find, on average, positive abnormal returns. In particular, we mechanically could see positive abnormal returns for a strategy that always buys. However, we do not find positive returns associated with the loan amendments. We also find that our results hold for a buy-and-sell strategy; and again, we only find it for the investors that were part of the loan amendment.

There is an additional point in that the number of the investors could affect the likelihood of a successful renegotiation (Bolton and Scharfstein, 1996). If that is the case, we could end up observing only the smallest syndicates. With the evidence provided in Table 2, it is hard to believe that there would be a significant difference between probability of renegotiation in a syndicate with 26 participants and a larger syndicate. Regardless, this is an important observation to keep in mind, but it is not essential to our central thesis: existence of stock trading on loan amendment information.

4. Results

4.1. Main results

Results for the “Case” subsample reported in Table 3 correspond to the first hypothesis—managers who participate in loan amendments subsequently profit from trading in stocks of the same company. We find that institutions with access to private information in the loan market realize, on average, a 6.0% excess return (5.1% risk-adjusted return) on trades in the stock of the borrower. This result is statistically significant and robust to different

calculations of abnormal returns, as well as alternative definitions of trade direction. Risk-adjusted returns on buy and sell positions are 5.2% and 3.0%, respectively. (The statistical significance of the returns in the buy and sell subsamples drops with the reduced number of observations.) For the sample of all amendments, we also observe positive abnormal returns. This result is economically and statistically weaker, which is consistent with the prior of lower fundamental information content, as compared to the sample of price amendments. Overall, we find consistently positive and economical returns on the trades of the investors that were part of the loan amendment in the stocks of the borrowers.

To rule out alternative explanations of these findings, we start by addressing the concern that stock trading could be attributed to a release of other public information within the same quarter. For each manager we constructed a control sample, following trading in the same stock in the same quarter by comparable managers who do not have access to the information from the loan market. The intuition is that if there is something special about the amended stock, then all comparable investors should perform equally well; i.e., outperformance should not be characteristic of the investors that participate in loan amendments. As mentioned earlier, for a given quarter, we select a control group of comparable investors using managers in the same beginning-of-the-quarter size quintile and of the same institutional type, as classified by Spectrum 13f.

The excess return on trades by the investors that were not part of the renegotiations, in the stocks affected by the loan renegotiations, is 0.3%, (−0.2% risk-adjusted return) and not statistically different from zero. Institutional investors receiving non-public information from the borrower outperform investors without access to the loan market information by 5.3% on the risk-adjusted basis. For the subsample where loan renegotiations become public in a different quarter, the difference in performance between informed (case) and uninformed (control) investors is even stronger. This is consistent with the fact that outperformance of investors with access to borrowers’ information is likely to be explained by the use of private information. These results are significant and robust to alternative specifications. Hence, we confirm that outperformance is investor-specific.

The outperformance by investors that participate in loan amendments, however, could be explained by the managers’ skill or investment style. There might an unobservable general characteristic of the stocks picked by the managers that invest in the stock and loan markets that is affecting the results. To test it, we look only at the investors that were part of the loan amendment, and compare performance of their trades in the stock of the firms with loan renegotiations, against average performance on other stocks in their portfolio. The intuition is that if there is something special about the manager, then there is no reason why this “special” skill should only apply to the stocks of the firms for which they received information through a loan amendment.

Table 4 summarizes the results for this hypothesis (case results are the same as in Table 3, the control sample is

Table 3

Returns on amended stocks: investors with vs. investors without access to private information.

This table shows returns realized on trades in stocks of companies with loan amendments for investors with and without access to private information. *Case (Stock & Loan)* corresponds to returns for the investors that were part of the loan amendment and *Control (Stock only)* corresponds to returns for those investors that were not part of the loan amendment. Investors in the *Stock only* group are matched by size and type to investors in the *Stock & Loan* group. Returns are computed as:

$$\frac{1}{N} \sum_{i=1}^N \frac{\sum_{j=1}^{M_i} D_{ji} CAR_i}{M_i},$$

where i identifies amendment and j identifies investor. Cumulative abnormal returns (CAR) are annualized and computed over the window from the day of the amendment until the amendment becomes public. The difference between Panels A and B is in the definition of trade direction (D). In Panel A, D is equal to -1 , 0 , or 1 if, over the past quarter, the investor reduced, did not change, or increased his position in a given stock (i.e., the returns are counted only if the stock was traded). In Panel B, D is equal to -1 if, over the past quarter, the investor reduced his position in a given stock and is equal to one otherwise. To account for quarterly reporting of institutional stock holdings, we report the results for the full sample as well as for the subsample restricted to those cases where information about the amendment becomes public in a different quarter. AR is the error term in the regression of daily excess returns. The four-factor model includes Fama and French (1993) factors and the Carhart (1997) momentum factor.

	Full sample								Loan amendment becomes public in a different quarter							
	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat
	Median	Mean	Z-stat	Median	Mean	Z-stat			Median	Mean	Z-stat	Median	Mean	Z-stat		
Panel A: All trades																
<i>Price amendments only</i>																
Unadjusted excess returns	6.13	5.99	(2.74)	0.13	0.27	(0.26)	5.72	(1.75)	7.40	9.33	(2.22)	0.11	0.84	(0.45)	8.49	(1.88)
One-factor model	4.33	5.08	(1.78)	−0.02	−0.24	(0.08)	5.32	(1.70)	6.24	7.24	(1.13)	0.00	0.20	(0.20)	7.04	(1.57)
Four-factor model	5.30	5.38	(1.74)	0.00	−1.81	(1.23)	7.19	(2.21)	6.56	6.84	(0.99)	0.00	−0.09	(0.15)	6.93	(1.55)
Obs. (amendment × manager)		120			120		−			52			52		−	
Number of amendments		58			58		58			29			29		29	
<i>All amendments</i>																
Unadjusted excess returns	1.49	1.94	(1.40)	−0.08	−0.15	(0.12)	2.09	(1.38)	3.69	3.18	(1.15)	0.00	0.16	(0.22)	3.02	(1.12)
One-factor model	1.09	1.95	(0.81)	−0.05	−0.33	(0.24)	2.27	(1.57)	1.76	2.69	(0.63)	0.00	−0.33	(0.06)	3.02	(1.14)
Four-factor model	1.91	2.33	(0.71)	0.00	−0.61	(0.92)	2.94	(1.93)	1.96	2.42	(0.47)	−0.05	−0.31	(0.00)	2.74	(1.04)
Obs. (amendment × manager)		274			274		−			127			127		−	
Number of amendments		131			131		131			66			66		66	
Panel B: All trades (including returns on unchanged stock holdings)																
<i>Price amendments only</i>																
Unadjusted excess returns	4.96	7.22	(2.85)	0.21	0.38	(0.29)	6.84	(2.07)	10.10	11.63	(2.45)	0.21	1.20	(0.45)	10.43	(1.75)
One-factor model	4.33	6.14	(1.98)	0.00	−0.07	(0.04)	6.20	(1.96)	6.24	9.73	(1.55)	0.35	0.93	(0.38)	8.81	(1.48)
Four-factor model	5.48	6.45	(1.91)	−0.01	−1.76	(1.02)	8.21	(2.51)	8.02	9.43	(1.38)	0.01	0.44	(0.25)	8.98	(1.51)
Obs. (amendment × manager)		120			120		−			52			52		−	
Number of amendments		58			58		58			29			29		29	
<i>All amendments</i>																
Unadjusted excess returns	2.16	2.41	(1.45)	0.00	−0.08	(0.22)	2.49	(1.60)	4.10	4.23	(1.28)	0.21	0.63	(0.36)	3.60	(1.33)
One-factor model	1.88	2.12	(0.77)	0.00	−0.34	(0.17)	2.46	(1.67)	2.44	3.80	(0.81)	0.18	0.18	(0.14)	3.61	(1.37)
Four-factor model	1.62	2.14	(0.48)	−0.02	−0.77	(0.87)	2.91	(1.84)	2.68	3.42	(0.44)	0.01	0.02	(0.05)	3.40	(1.29)
Obs. (amendment × manager)		274			274		−			127			127		−	
Number of amendments		131			131		131			66			66		66	

Table 4

Informed investors' returns following loan amendments: stocks with vs. stocks without loan amendments (H2).

This table reports returns following loan amendments only for investors that were part of the loan amendment (i.e., informed investors). The point of the table is to compare returns on stocks of companies with loan amendments to returns on the rest of the stock portfolio. Case (Stock & Loan) corresponds to returns on stocks with loan amendment and Control (Stock only) corresponds to returns on the rest of the portfolio. Returns are computed as:

$$\frac{1}{N} \sum_{i=1}^N \frac{\sum_{j=1}^{M_i} D_{ji} CAR_i}{M_i},$$

where i identifies amendment and j identifies investor. Cumulative abnormal returns (CAR) are annualized and computed over the window from the day of the amendment until the amendment becomes public. The difference between Panels A and B is in the definition of trade direction (D). In Panel A, D is equal to -1 , 0 , or 1 if, over the past quarter, the investor reduced, did not change, or increased his position in a given stock (i.e., the returns are counted only if the stock was traded). In Panel B, D is equal to -1 if, over the past quarter, the investor reduced his position in a given stock and is equal to one otherwise. To account for quarterly reporting of institutional stock holdings, we report the results for the full sample as well as for the subsample restricted to those cases where information about the amendment becomes public in a different quarter. AR is the error term in the regression of daily excess returns. The four-factor model includes Fama and French (1993) factors and the Carhart (1997) momentum factor.

	Full sample								Loan amendment becomes public in a different quarter							
	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat
	Median	Mean	Z-stat	Median	Mean	Z-stat			Median	Mean	Z-stat	Median	Mean	Z-stat		
Panel A: All trades																
<i>Price amendments only</i>																
Unadjusted excess returns	6.13	5.99	(2.74)	−0.08	−0.34	(0.05)	6.34	(2.37)	7.40	9.33	(2.22)	−0.00	0.33	(0.52)	9.00	(1.80)
One-factor model	4.33	5.08	(1.78)	−0.25	−0.43	(0.28)	5.51	(2.14)	6.24	7.24	(1.13)	0.03	−0.08	(0.25)	7.32	(1.22)
Four-factor model	5.30	5.38	(1.74)	−0.19	−0.16	(0.16)	5.54	(1.98)	6.56	6.84	(0.99)	0.17	0.33	(0.77)	6.51	(1.09)
Obs. (amendment × manager)		120			120		–			52			52		–	
Number of amendments		58			58		58			29			29		29	
<i>All amendments</i>																
Unadjusted excess returns	1.49	1.94	(1.40)	−0.01	−0.18	(0.19)	2.12	(1.99)	3.69	3.18	(1.15)	0.06	0.42	(0.94)	2.76	(1.03)
One-factor model	1.09	1.95	(0.81)	−0.09	−0.19	(0.32)	2.14	(2.11)	1.76	2.69	(0.63)	0.01	0.18	(0.32)	2.51	(0.95)
Four-factor model	1.91	2.33	(0.71)	−0.12	−0.08	(0.59)	2.41	(2.40)	1.96	2.42	(0.47)	0.01	0.10	(0.09)	2.33	(0.89)
Obs. (amendment × manager)		274			274		–			127			127		–	
Number of amendments		131			131		131			66			66		66	
Panel B: All trades (including returns on unchanged stock holdings)																
<i>Price amendments only</i>																
Unadjusted excess returns	4.96	7.22	(2.85)	−0.05	−0.03	(0.37)	7.26	(2.23)	10.10	11.63	(2.45)	0.01	0.58	(0.81)	11.05	(2.25)
One-factor model	4.33	6.14	(1.98)	−0.08	−0.14	(0.08)	6.28	(2.01)	6.24	9.73	(1.55)	−0.01	0.05	(0.36)	9.68	(1.64)
Four-factor model	5.48	6.45	(1.91)	0.05	−0.03	(0.12)	6.48	(2.09)	8.02	9.43	(1.38)	0.16	0.29	(0.61)	9.14	(1.55)
Obs. (amendment × manager)		120			120		–			52			52		–	
Number of amendments		58			58		58			29			29		29	
<i>All amendments</i>																
Unadjusted excess returns	2.16	2.41	(1.45)	0.03	−0.12	(0.22)	2.53	(1.65)	4.10	4.23	(1.28)	0.18	0.58	(1.10)	3.65	(1.37)
One-factor model	1.88	2.12	(0.77)	−0.06	−0.18	(0.47)	2.30	(1.58)	2.44	3.80	(0.81)	0.02	0.22	(0.14)	3.58	(1.36)
Four-factor model	1.62	2.14	(0.48)	0.02	−0.14	(0.91)	2.27	(1.54)	2.68	3.42	(0.44)	0.12	0.01	(0.54)	3.41	(1.31)
Obs. (amendment × manager)		274			274		–			127			127		–	
Number of amendments		131			131		131			66			66		66	

Table 5

Returns on amended stocks before loan amendments: Investors with vs. investors without access to private information.

This table presents a placebo test. The results in Table 3 are re-examined for a random quarter in the year preceding the loan amendment (returns are annualized and are measured over the quarter.) The sample is reduced due to data availability. As in Table 3, *Stock & Loan* corresponds to returns for investors that were part of the loan amendment and *Stock only* corresponds to returns for those investors that were not part of the loan amendment. Investors in the *Stock only* group are matched by size and type to investors in the *Stock & Loan* group. The difference between Panels A and B is in the definition of trade direction. In Panel A, trade direction is equal to $-1, 0$, or 1 if, over the past quarter, the investor reduced, did not change, or increased his position in a given stock (i.e., the returns are counted only if the stock was traded). In Panel B, trade direction is equal to -1 if, over the past quarter, the investor reduced his position in a given stock and is equal to one otherwise. *AR* is the error term in the regression of daily excess returns. The four-factor model includes Fama and French (1993) factors and the Carhart (1997) momentum factor. Z-statistics are shown in parentheses.

	Case (Stock & Loan)		Control (Stock only)			
	Mean	Z-stat	Mean	Z-stat	Diff.	t-Stat
Panel A: All trades						
<i>Price amendments only</i>						
Unadjusted excess returns	0.27	(0.12)	−0.05	(0.20)	0.32	(0.47)
One-factor model	−0.31	(1.09)	−0.33	(1.16)	0.02	(0.04)
Four-factor model	−0.62	(1.45)	−0.35	(1.35)	−0.27	(0.55)
Obs. (amendment × manager)	116		116		−	
Number of amendments	54		54		54	
<i>All amendments</i>						
Unadjusted excess returns	0.88	(0.89)	0.22	(0.60)	0.63	(1.01)
One-factor model	0.34	(0.33)	−0.10	(0.93)	0.81	(1.39)
Four-factor model	0.14	(0.66)	−0.29	(1.77)	0.20	(0.36)
Obs. (amendment × manager)	254		254		−	
Number of amendments	119		119		119	
Panel B: All trades (including returns on unchanged stock holdings)						
<i>Price amendments only</i>						
Unadjusted excess returns	0.84	(0.22)	0.25	(0.00)	0.58	(0.65)
One-factor model	−0.37	(1.31)	−0.47	(1.02)	0.10	(0.12)
Four-factor model	−0.32	(1.51)	−0.06	(0.62)	−0.26	(0.30)
Obs. (amendment × manager)	116		116		−	
Number of amendments	54		54		54	
<i>All amendments</i>						
Unadjusted excess returns	1.67	(1.92)	0.91	(1.60)	0.77	(1.12)
One-factor model	0.77	(0.35)	0.16	(0.02)	0.61	(0.90)
Four-factor model	0.74	(0.16)	0.12	(0.19)	0.61	(0.95)
Obs. (amendment × manager)	254		254		−	
Number of amendments	119		119		119	

4.2. Robustness tests

By using differences in performance between case and control groups, we implicitly rely on the assumption that the control groups are comparable to the case. For the comparison between investor groups, we match control samples based on the assets quintile. However, assets-size quintile and investor type could be insufficient characteristics for managers to be comparable. Specifically, we are concerned that investors participating in stock and loan markets are not comparable to those investors that only participate in the stock market.

In Table 6, we verify our result by restricting the control sample of the managers to only those institutions that invest in the loan market. The control sample includes institutional investors holding stock of the company with loan amendments that also hold other loans in their portfolio, but that do not hold loans of the affected company. The sample size for the case group (investors with joint holdings of stock and loans) is reduced because not all firms have a match in the constrained control group. The result remains statistically significant, and the magnitudes are comparable to the findings in Table 4. Therefore, the findings are not specific to the group of institutional managers that invest in the loan

market in general, but only to those institutions that were part of the loan renegotiations.

Similarly, when we compare two groups of stocks held by the same manager, the implied assumption is that all stocks held in the portfolio of the same manager are fundamentally alike. But it could be the case that firms that have loans are not comparable to the firms that do not use the credit market. As in Table 4, in Table 7 we also look at only the investors that were part of the amendment, but instead of comparing trades in the stocks with loan renegotiation to all other stocks in the investors' portfolios, we restrict the control sample to the companies that have loans outstanding. The results reported in Table 7 remain practically unchanged.

An amendment is preceded by a loan renegotiation process. However, the timing of the loan renegotiation is not observable to us. Anecdotal evidence suggests that the renegotiation period can be very short. All information flow between the borrower and the lending syndicate is conducted through the lead bank. If the borrower wants to renegotiate the loan agreement, it contacts the lead. The lead bank is the delegated agent for the syndicate; it not only receives the information but also has the skill to process it and to assess the viability of the amendment (it is

Table 6

Returns on amended stocks: Investors with vs. investors without access to private information (loan-market investors only).

This table replicates results in Table 3, Panel A with control investors (*Stock only*) constrained to managers that invest in stock and loan markets but that were not part of the loan amendment. The *Case* sample here is smaller than the one in Table 3 because we exclude cases without control matches. Investors in the *Stock only* group are matched by size and type to investors in the *Stock & Loan* group. Cumulative abnormal returns are annualized and computed over the window from the day of the amendment until the amendment becomes public. The four-factor model includes Fama and French (1993) factors and the Carhart (1997) momentum factor.

	Full sample								Loan amendment becomes public in a different quarter							
	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat
	Median	Mean	Z-stat	Median	Mean	Z-stat			Median	Mean	Z-stat	Median	Mean	Z-stat		
<i>Price amendments only</i>																
Unadjusted excess returns	5.56	5.78	(2.02)	0.30	0.20	(0.87)	5.58	(2.04)	5.56	10.46	(2.35)	0.39	0.66	(0.91)	9.81	(2.02)
One-factor model	3.01	4.87	(1.22)	−0.21	−0.73	(0.36)	5.61	(2.05)	6.24	8.31	(1.39)	0.00	−0.36	(0.58)	8.68	(1.79)
Four-factor model	4.04	5.19	(1.41)	0.00	−1.08	(0.00)	6.28	(2.30)	6.56	7.89	(1.27)	0.00	−0.25	(0.58)	8.14	(1.69)
Obs. (amendment × manager)		115			115		–			50			50		–	
Number of amendments		53			53		53			27			27		27	
<i>All amendments</i>																
Unadjusted excess returns	1.43	1.72	(0.91)	0.00	−0.30	(0.36)	2.02	(1.71)	1.72	3.55	(1.27)	0.18	0.06	(0.50)	3.49	(1.17)
One-factor model	1.00	1.78	(0.45)	−0.14	−0.95	(0.34)	2.73	(2.30)	1.00	3.11	(0.86)	0.00	−0.74	(0.03)	3.85	(1.31)
Four-factor model	2.26	2.19	(0.50)	−0.22	−1.02	(0.85)	3.21	(2.63)	2.26	2.82	(0.71)	−0.04	−0.75	(0.16)	3.57	(1.23)
Obs. (amendment × manager)		267			267		–			122			122		–	
Number of amendments		125			125		125			62			62		62	

Table 7

	Full sample						Loan amendment becomes public in a different quarter									
	Case (Stock & Loan)			Control (Stock only)			Diff.	t-Stat	Case (Stock & Loan)			Control (Stock only)				
	Median	Mean	Z-stat	Median	Mean	Z-stat			Median	Mean	Z-stat	Median	Mean	Z-stat		
<i>Price adjustments only</i>																
Unadjusted excess returns	6.13	5.99	(2.74)	-0.19	-0.64	(0.09)	6.63	(2.44)	7.40	9.33	(2.22)	-0.05	-0.81	(0.49)	10.14	(2.01)
One-factor model	4.33	5.08	(1.78)	-0.18	-0.79	(0.37)	5.88	(2.25)	6.24	7.24	(1.13)	0.06	-0.79	(0.27)	8.03	(1.60)
Four-factor model	5.30	5.38	(1.74)	-0.11	-0.51	(0.06)	5.88	(2.27)	6.56	6.84	(0.99)	0.07	-0.32	(0.70)	7.16	(1.44)
Obs. (amendment × manager)		120			120		-			52			52		-	
Number of amendments		58			58		58			29			29		29	
<i>All amendments</i>																
Unadjusted excess returns	1.49	1.94	(1.40)	-0.03	-0.37	(0.20)	2.60	(1.67)	3.69	3.18	(1.15)	0.07	0.57	(1.18)	2.62	(1.31)
One-factor model	1.09	1.95	(0.81)	-0.04	-0.32	(0.05)	2.83	(1.91)	1.76	2.69	(0.63)	0.03	0.35	(0.90)	2.34	(1.19)
Four-factor model	1.91	2.33	(0.71)	-0.00	-0.17	(0.14)	3.03	(2.06)	1.96	2.42	(0.47)	0.04	0.23	(0.80)	2.20	(1.12)
Obs. (amendment × manager)		274			274		-			127			127		-	
Number of amendments		131			131		131			66			66		66	

a *bank*, after all). So, when the lead sets up the first conference call, it already has its recommendation for renegotiation ready. Although, in the case of material amendments, individual lenders can oppose the lead's recommendation, they are unlikely to do so, especially in a booming market (a large fraction of our sample). Prolonging renegotiation can be costly. If the supply of credit is plentiful, the loan can be refinanced; prolonging a renegotiation therefore jeopardizes client relationships (between the lead and the borrower and between the lead and the syndicate investors) and the lead's amendment fee.

In Table 8, we report the key results from Tables 3 and 4 for two alternative windows, $[t_1 - 5; t_2]$ and $[t_1 - 30; t_2]$. In general, extending the window around the event dilutes the result, which is consistent with the prior belief that the renegotiation period is short and which confirms that our findings are specific to the timing of the amendment.

Because we observe only a small subset of information-related events (loan amendments), we cannot magnify the total benefits of insider trading for a given investor. However, to better understand the economic value of our findings, in [Table 9](#) we look at the size of the trades for the institutions that participate in loan renegotiations in the stock of the borrower. Using the number of shares held, we find that in the quarter of the loan amendment, investors sell roughly one-third of their positions and more than double their positions on buy trades. In general, portfolio weights in the stock of the borrower tend to be significantly larger than the median portfolio weight of the managers. An average holding for the firm with a loan amendment is six times larger than a median holding.

4.3. Overall implications of trading on private information

We believe that the presented results build a strong case for the existence of stock trading on information privately released in the process of loan amendments. Although this type of cross-market is not directly monitored or regulated, there is enough information and public concern to understand that one could be prosecuted for it. So, why do these managers do it? What does it mean for their overall performance? The results in Table 4 provided interesting insight: the managers that participate in loan amendments show large outperformance in their trades of the amended stocks, yet they simultaneously underperform in the market on the rest of their holdings. Thus, a likely and intuitive explanation is that managers use private information when they get into trouble. Therefore, we expect that there would be no outperformance on the aggregate portfolio of investors in the loan market.

To test this we look at the differences in aggregate performance for institutional managers that invest in the loan market as compared to similar investors that do not invest in the loan market. We no longer condition this sample to the investors identified in the SEC filings of amendments. Instead, we look at all investors in the loan market that we were able to identify by matching either DealScan or SEC filings to the CDA/Spectrum data. The control group for mutual funds is matched by the assets under management quintile and investment objective using CRSP mutual fund data. For all other institutions, we select the control group of comparable investors using managers of the same type in the same size quintile. We

Table 8

Robustness of the results to alternative windows.

This table replicates the key results in Tables 3 and 4, Panel, A (price amendments only) for alternative event windows. In particular, we look at cumulative abnormal returns from five days preceding the day of the amendment until the amendment becomes public, $[t_1 - 5; t_2]$ and from 30 days preceding the day of the amendment until the amendment becomes public, $[t_1 - 30; t_2]$. As before, Case corresponds to returns for the investors that were part of the loan amendment. We also report differences in means between case and control samples. Control (Stock only), Table 3 corresponds to returns for those investors that were not part of the loan amendment; Table 4 corresponds to returns on stocks that were not amended.

	Full sample (price amendments)								Loan amendment becomes public in a different quarter							
	Case (Stock & Loan)			Control (Stock only)				Case (Stock & Loan)			Control (Stock only)					
				Table 3		Table 4					Table 3		Table 4			
	Median	Mean	Z-stat	Diff.	t-Stat	Diff.	t-Stat	Median	Mean	Z-stat	Diff.	t-Stat	Diff.	t-Stat		
	<i>Event window: $[t_1-5; t_2]$</i>															
Unadjusted excess returns	4.63	5.65	(3.12)	4.01	(1.54)	5.65	(2.34)	7.40	6.53	(1.79)	4.58	(0.76)	5.82	(0.97)		
One-factor model	3.40	4.80	(2.11)	3.57	(1.51)	5.17	(2.34)	1.73	4.22	(0.55)	3.10	(0.52)	4.44	(0.74)		
Four-factor model	4.32	4.94	(2.01)	5.03	(2.03)	5.03	(2.35)	3.23	3.81	(0.45)	3.00	(0.50)	3.69	(0.62)		
<i>Event window: $[t_1-30; t_2]$</i>																
Unadjusted excess returns	3.55	3.76	(2.21)	1.37	(0.62)	4.33	(2.02)	4.12	5.80	(1.52)	4.50	(1.06)	5.77	(1.37)		
One-factor model	2.05	2.86	(1.01)	1.07	(0.50)	3.71	(1.80)	2.71	3.09	(0.01)	2.92	(0.72)	3.87	(0.95)		
Four-factor model	1.12	2.96	(0.90)	2.23	(1.14)	3.78	(1.96)	1.53	2.72	(0.13)	3.09	(0.79)	3.25	(0.83)		
Number of amendments	58			58		58		29			29		29			

Table 9

Size of stock trades.

This table examines the relative size of trades by investors that were part of the loan amendment (Stock & Loan) in the stocks of the companies with loan amendments. The numbers correspond to the average of the absolute trades (i.e., buys and sells are unsigned). Z-statistics are shown in parentheses. ***, **, and * indicate *p*-values of 1%, 5%, and 10%, respectively.

	Median	Mean	Z-stat
<i>Price amendments only</i>			
Portfolio weights/median weight	1.12	6.27	(2.41)**
Change in number of shares held following amendment			
Buys	19.68%	150.44%	(2.05)**
Sells	25.92%	33.14%	(4.23)***
<i>All amendments</i>			
Portfolio weights/median weight	0.86	5.36	(4.70)***
Change in number of shares held following amendment			
Buys	42.68%	129.12%	(3.55)***
Sells	24.74%	36.18%	(5.99)***

Table 10

Differences in stock portfolio performance.

This table shows differences in annual abnormal returns on complete stock portfolios between institutional managers who have access to the loan amendment information at some point in time (Stock & Loan) and institutional managers that do not have access to loan amendment information at any point (Stock only). For each institutional manager in the treated group (Stock & Loan), we construct a control group using investors matched for institutional type and in the same beginning-of-quarter size quintile. Portfolio returns are calculated using the Daniel, Grinblatt, Titman, and Wermers (1997) method. The sample covers the period between 1990 and 2007. We first compute the time-series average return for each institutional investor. The numbers reported in the table are the cross-sectional average and median of the time-series means for the managers' returns in each group and the difference in returns between the two groups. Then, for each manager in the case group, we compute the difference between its time-series average return and the time-series mean of the corresponding control group's median returns. We test the significance of the mean of the difference using one-sample paired *t*-test and the significance of the median using Wilcoxon signed rank sum test. *p*-Values are reported in parentheses. ** Indicate *p*-values of 5%.

	Obs.	Case (Stock & Loan)		Control (Stock only)		Difference	
		Mean	Median	Mean	Median	Mean	Median
Stock & Loan investors: All	128	3.86% (0.00)	3.06% (0.00)	3.91% (0.00)	3.44% (0.00)	−0.05% (0.74)	−0.50% (0.04)**
Stock & Loan investors: With stocks of amended companies	42	4.42% (0.00)	3.46% (0.00)	4.22% (0.00)	3.72% (0.00)	0.20% (0.71)	−0.38% (0.20)

identify institutional type using CDA/Spectrum classification. For data after 1999, CDA/Spectrum misclassifies a lot of institutions as “other institutional type.” To correct this problem for managers in this group, for years 1999 and later, we replace the value with the one reported at the end of 1998. The problem seems to be alleviated to some degree, but we still find a sharp increase in the number of managers classified as “other institutional type” around this period.

As can be seen in Table 10, between 1990 and 2007, total stock portfolios of institutional managers that also invest in the loan market are no different from the stock portfolios of investors that do not invest in the loans.³² The difference in performance is not statistically different from zero.

Overall, the evidence suggests that trading on loan market information is not used by investors as an investment strategy, but rather as a way of improving their results. This however, does not change regulatory implications that should be drawn from this evidence.

5. Conclusions and final remarks

Growing institutional investment in the syndicated loan market was one of the defining features of the leveraged loans expansion that lasted until the second half of 2007. Although institutional participation significantly shrunk in 2008 (Ivashina and Scharfstein, 2010), many institutional investors still remain in this market and it is not unlikely that the growth of this investors’ base will become important again.

There are many interesting questions that relate to institutional participation in the credit market. In this paper we focus on one specific point: many institutional investors invest in the loan market and, hence, gain access to private information about the borrower. Simultaneously, these institutions invest in public securities. In the absence of regulation, can private information about the borrower be protected from being used to trade in public markets? The evidence that we find suggests that this is not the case.

Our results indicate a causal relation between superior information and outperformance in stock trades following the release of private information about the borrower. Specifically, we collect investors’ identities from loan amendments data reported in SEC filings and follow each amendment in the news to credibly identify the timing of the public information release. We find that when a loan contract is subject to a loan amendment, institutions holding the loan of the borrower realize large positive abnormal returns in the equity market by trading in the stock of the company with the loan amendment. We further verify that this result is specific (i) to the event (loan amendment), (ii) to the institutions that have access to

information released by the borrower at the loan amendment, and (iii) to the stocks of companies affected by loan amendments. Looking more broadly, we find that managers that hold stock and loans of the same company do not outperform stock investments of comparable managers that only hold stock, or that invest in stock and loans of different companies.

Information processing is central to the role of banking. In that sense, leakage of confidential information received by the lenders could have important costs. It could directly damage the borrower (Campbell and Kracaw, 1980), it could reduce the value of lending relationships overall (Diamond, 1984), and, by creating information asymmetry among investors, it could adversely affect the liquidity of the public markets (Rock, 1986). This leads to some open questions: Wouldn’t borrowers want to avoid similar trading by the institutions? The likely answer is that borrowers may not be able to avoid it. Borrowers do not directly interact with all of their lenders and the lending syndicates tend to be very large. In addition, it is not clear which investors might use such private information or when. The cost for borrowers might be offset by the benefit of available funding. Finally, why would an originating bank want to syndicate a loan to institutional investors if doing so devalues the bank’s information production? There is a risk-sharing benefit and the fee income received by the originating bank might offset the cost (Shleifer and Vishny, 2010).

Appendix A

See Table A1.

Table A1

Loan and stock abnormal returns around loan amendments.

This table presents abnormal returns surrounding loan amendments. We only report results for the price amendments. Assuming the loan-amendment event date is t_1 , $(t_1 - j)$ and $(t_1 + j)$ represent average returns on the j th day before and after the event date, respectively. “Buy” (“Sell”) corresponds to amendments with the positive (negative) average trading direction among the investors with stock and loan holdings. For the loan returns, abnormal returns are calculated as raw return minus average return during the 25th to 5th days before the loan amendment date; number of observations drops due to data availability. For the stock returns, abnormal returns are calculated using the one factor model.

Day	Amendment date ($t=t_1$)				Public news release date ($t=t_2$)			
	Buy		Sell		Buy		Sell	
	Mean	Z-stat	Mean	Z-stat	Mean	Z-stat	Mean	Z-stat
<i>Panel A: Loan returns</i>								
$(t-5)$	-0.01	(0.44)	0.08	(0.94)	-0.02	(0.86)	-0.29	(0.86)
$(t-4)$	0.01	(0.28)	0.01	(0.15)	-0.05	(1.31)	-0.29	(0.91)
$(t-3)$	-0.03	(1.06)	0.05	(0.71)	0.03	(0.86)	-0.31	(0.95)
$(t-2)$	0.00	(0.21)	0.02	(0.38)	-0.01	(0.67)	-0.08	(0.36)
$(t-1)$	0.01	(0.33)	0.10	(0.94)	0.02	(0.86)	-0.28	(0.76)
t	-0.05	(0.99)	0.13	(1.24)	0.10	(1.17)	-0.54	(0.90)
$(t+1)$	0.00	(0.02)	3.25	(1.00)	0.03	(1.52)	-0.22	(0.78)
$(t+2)$	0.06	(1.17)	-3.09	(1.07)	0.02	(1.27)	-0.32	(0.89)
$(t+3)$	0.14	(1.27)	1.73	(1.01)	0.02	(0.86)	-0.34	(0.96)
$(t+4)$	0.00	(0.21)	2.32	(0.97)	0.06	(1.21)	-0.33	(0.94)
$(t+5)$	0.00	(0.14)	0.08	(1.36)	0.01	(0.72)	-0.35	(1.06)
Obs.	12		12		12		11	

³² Portfolio performance is risk-adjusted and it is calculated using the characteristics-based benchmark adjusted return method developed by Daniel, Grinblatt, Titman, and Wermers (1997). The measure calculates the sum of time-series covariance between portfolio weight change and returns of each asset included in the evaluating portfolio. In particular, the measure uses a four-quarter change in portfolio weights and multiplies it by the future return. For example, the performance from quarter t to $t+m$ is calculated as: $P_t = \sum_{j=1}^N \omega_{j,t-1} (R_{j,t} - R_t^{b,t-1})$, where $\omega_{j,t-1}$ is the portfolio weight on stock j at the end of month $t-1$, $R_{j,t}$ is the month t return of stock j , and $R_t^{b,t-1}$ is the month t return of the characteristic-based passive portfolio that is matched to stock j during month $t-1$.

Table A1 (continued)

Day	Amendment date ($t=t_1$)				Public news release date ($t=t_2$)			
	Buy		Sell		Buy		Sell	
	Mean	Z-stat	Mean	Z-stat	Mean	Z-stat	Mean	Z-stat
<i>Panel B: Stock returns</i>								
($t-5$)	0.24	(0.67)	-1.70	(0.96)	-0.71	(1.21)	-0.22	(0.29)
($t-4$)	-0.97	(1.98)	-1.74	(1.39)	-0.85	(1.08)	1.52	(1.45)
($t-3$)	-0.19	(0.27)	-0.10	(0.12)	-0.74	(1.11)	0.21	(0.38)
($t-2$)	0.73	(0.82)	0.79	(1.06)	0.04	(0.05)	-0.11	(0.22)
($t-1$)	0.25	(0.32)	-0.97	(2.23)	1.69	(1.40)	-0.71	(0.93)
t	-0.55	(0.65)	0.63	(0.97)	0.07	(0.08)	-0.26	(0.11)
($t+1$)	0.70	(0.77)	0.24	(0.14)	-0.50	(0.92)	1.56	(1.16)
($t+2$)	0.18	(0.17)	0.96	(0.58)	1.28	(1.75)	-0.46	(0.65)
($t+3$)	-0.49	(0.43)	-1.18	(1.02)	-1.48	(2.61)	0.42	(0.40)
($t+4$)	-0.37	(0.75)	0.01	(0.02)	0.42	(0.85)	-0.57	(0.63)
($t+5$)	-0.29	(0.44)	1.08	(1.25)	-0.52	(1.05)	-0.23	(0.21)
Obs.	26		24		26		24	

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