The Effects of Stock Lending on Security Prices: An Experiment

by

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Abstract

We examine the impact of short selling by conducting a randomized stock lending experiment. Working with a large, anonymous money manager, we create an exogenous and sizeable shock to the supply of lendable shares by taking high-loan fee stocks in the manager's portfolio and randomly making available and withholding stocks from the lending market. The experiment ran in two independent phases: the first, from September 5 to 18, 2008, with over $580 million of securities lent; and the second, from June 5 to September 30, 2009, with over $250 million of securities lent. While the supply shocks significantly reduce market lending fees and raise quantities, we find no evidence that returns, volatility, skewness, or bid-ask spreads are affected. The results, therefore, provide novel evidence on the impact of shorting supply and do not indicate any adverse effects on stock prices from securities lending.

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Abstract

We examine the impact of short selling by conducting a randomized stock lending experiment. Working with a large, anonymous money manager, we create an exogenous and sizeable shock to the supply of lendable shares by taking high-loan fee stocks in the manager's portfolio and randomly making available and withholding stocks from the lending market. The experiment ran in two independent phases: the first, from September 5 to 18, 2008, with over $580 million of securities lent; and the second, from June 5 to September 30, 2009, with over $250 million of securities lent. While the supply shocks significantly reduce market lending fees and raise quantities, we find no evidence that returns, volatility, skewness, or bid-ask spreads are affected. The results, therefore, provide novel evidence on the impact of shorting supply and do not indicate any adverse effects on stock prices from securities lending.

I. Introduction

The impact of short selling is the subject of an ongoing debate among academics, investment committees, corporate boards, and regulators. One view is that short selling helps make markets more efficient by improving price discovery. An alternative view is that short selling distorts markets and adversely affects prices by moving them further away from fundamentals. Indeed, short-sellers have often been characterized as immoral, unethical, and unpatriotic.[1] Interest in the effects of short selling has intensified with the sharp drop in asset prices, particularly those of financial institutions during the recent financial crisis, sparking new discussions of the consequences of short selling among policy makers worldwide.

The theoretical impact of short sales on asset prices is ambiguous. Miller (1977) argues that differences of opinion and short sales constraints can lead to overpricing. Others (e.g., Hong and Stein, 2003 and Abreu and Brunnermeier, 2001) argue that short sales constraints can lead to excess volatility or destabilized prices (Allen and Gale, 1991). Conversely, in a rational expectations model, Diamond and Verrecchia (1987) argue that traders adjust for short sales restrictions in equilibrium so that there is no overpricing on average, but the skewness of returns may be affected.

Empirically, the effect of short sale restrictions on asset prices is also mixed, largely due to the difficulty in separately identifying demand and supply effects. For example, supply shifts in stock lending are typically driven by changes in investors' marginal cost of lending, which may be related to other factors, including demand for shorting the security. Disentangling these two effects is difficult and hence gives rise to multiple interpretations of the evidence.

In this paper, we provide a new empirical perspective on shorting by conducting an experiment in which we randomly move the supply of shares available for lending, holding demand fixed, to produce an exogenous shock to the supply of lendable shares. Working with a large (greater than $15 billion in assets), anonymous money manager ("the Manager"), we randomly make available for lending two-thirds of the high-loan fee stocks in the Manager's portfolio (treatment) and withhold from lending the other

[1] See Lamont (2004) for some choice quotes about short sellers. During the 1989 congressional hearings on short selling, Congressman Dennis Hastert (later speaker of the U.S. House of Representatives) described the activity as "blatant thuggery."

third of high loan-fee stocks owned by the Manager (control). The randomized design of the treatment and control groups is crucial since other factors such as demand differences, lending by other institutions, market activity, regulation, liquidity, risk, and mispricing, should all be unrelated to the randomized treatment and hence difference out when we compare the available (treated) stocks to the withheld (control) stocks.[2] Our experiment identifies shocks to supply holding demand and other factors constant.

We restrict the lending experiment, both treatment and control groups, to stocks with high loan fees – expected loan fees of at least 25 basis points per year (0.25%), with an average loan fee of more than 4% per year. High loan fee stocks are those that have high shorting demand relative to their supply. Theoretical work by Duffie (1996) and Duffie, Garleanu, and Pedersen (2002) argues that the effects of shorting constraints are nonlinear and mostly affect stocks whose supply of lendable shares is restricted relative to demand. According to these theories, our sample of high-loan fee stocks should experience larger than average effects from supply shocks.[3]

The original intent of the experiment was to implement it in a single phase lasting approximately six months. Lending began on September 5, 2008. Because of the subsequent market turmoil, including the collapse of Lehman Brothers on September 15, 2008 and concerns over the stability of financial intermediaries at that time, the Manager decided to recall the loans on September 18, 2008.[4] The last recalled security was returned on October 3, 2008. These events define what turned out to be the first phase of the experiment. We refer to the period September 5 to September 17, 2008 as the lending period—a positive supply shock—and the period September 18 to October 3, 2008 as the recall period—a

[2] One caveat to the randomization is that the Manager required that we lend three specific securities on the basis of their high loan fees *before* the experiment. While this request is clearly not random, our results are robust to excluding these three stocks from the analysis. In the results that follow, we include these stocks in our analysis because they experienced a supply shock. Given their high fees, including these stocks should, if anything, make it more likely to find effects from lending on stock prices.

[3] The Manager could have run the same experiment among a set of low demand/low loan fee stocks to see if they behave differently, but chose not to do so. However, because low shorting demand stocks are not constrained by supply, it is unlikely that our supply shock experiment would have had an impact on shorting constraints. In fact, the Manager did not want to lend out its low loan fee stocks because it expected to earn little fee revenue from these stocks (e.g., they had no excess shorting demand). The Manager decided it was not worth incurring the cost of setting up a lending program for these stocks.

[4] A number of other institutions also suspended their securities lending programs at this time.

negative supply shock. While the first phase of the experiment ended prematurely, it was due to exogenous and unforeseen events. And, the decision to terminate the first phase was made *before* any results were examined (by us or the Manager). Accordingly, the termination should not have introduced any bias into our results.

Because the first phase was unintentionally shortened, the Manager decided to conduct a second experiment after general market conditions stabilized. This second phase of the experiment provides another independent shock to shorting supply in the market. This allows us to assess the robustness of the findings from the first phase under more normal market conditions and over a longer time period. The second phase began on June 5, 2009 with a new set of high-loan fee stocks from the Manager's portfolio using another randomization. On October 1, 2009, the Manager lifted the restriction on lending the withheld stocks, eliminating the control group from that date forward and effectively ending the experiment.[5] We refer to the period June 5, 2009 to September 30, 2009 as the lending period. There is no recall period for this second phase. Each phase of the experiment on its own has moderate statistical power, but jointly the two independent phases provide substantial statistical power.

The experiment substantially increases the supply of shares available for lending. In the first phase, the potential loan supply by the Manager comprises 229% of daily trading volume, 18.3% of short interest, and 3.7% of total institutional ownership, for the average stock in the experiment. In the second phase, these figures are 214%, 36.8%, and 6.9%, respectively. In terms of actual lending, at the peak of the first (second) phase of the experiment, over $580 million ($250 million) of securities are lent. The daily maximum shares on loan in the first (second) phase comprises 1.6% (1.7%) of the total market

[5] The decision to lend the control stocks and end the second phase of the experiment was partially due to the fact that we detected no adverse effects from lending. In principle, this endogeneity could affect the interpretation of the results. For example, if the Manager had detected adverse effects early in the second phase and chose to continue the experiment until these effects disappeared, a finding of no adverse effects in the overall period could be misleading. However, we also analyze event studies with arbitrarily chosen windows and find identical results, as well as results consistent with the first phase of the experiment that does not face this issue. At no event window under either phase of the experiment do we detect significant adverse effects of lending on stock prices. Conversely, while we cannot rule out that we would have detected adverse pricing effects had the second phase continued beyond four months, the Manager has detected no such effects since (albeit without the benefit of a control group) and is still lending its shares as of October 2011, a full two years after the experiment.

capitalization of the stocks on average and as much as 5% at the maximum. Furthermore, actual loan fees decline significantly (on the order of 200 to 300 basis points per year) and loan quantities rise significantly for the stocks that experience supply increases relative to those that do not.

Despite the focus on high demand stocks and the sizeable changes to supply that cause loans fees to decline and shares borrowed to increase, we do not find any adverse effects on average stock prices from these loan supply shocks. Positive and negative exogenous shocks to shorting supply have no price consequences on the underlying shares of stock. Similarly, we fail to find any consistent adverse changes in volatility, skewness, and bid-ask spreads of stocks available for lending versus those withheld.

These results are inconsistent with overpricing and disagreement theories of shorting activity. A possible explanation for the lack of price effects is that shorting in our sample of high loan fee stocks may be driven primarily by hedging demand. We examine two sources of hedging demand—merger activity and convertible bond trading—and find that very few of our firms are exposed to either activity and that cross-sectionally these two proxies for hedging demand are unrelated to differences in returns, volatility, skewness, or bid-ask spreads between available and withheld stocks.

When we analyze the cross-section of returns, we find weak evidence that returns are lower for available stocks in which supply shocks are largest and where analyst disagreement (a proxy for investor disagreement) is highest. This is consistent with overvaluation theories, where the most overvalued firms (those with the most disagreement) who experience the largest shorting supply shocks have lower returns. However, these findings are only borne out in the second phase and are not robust across the two phases of the experiment. We also find that high market-to-book stocks made available for lending have higher returns during the lending period of both phases, which is the opposite of what an overvaluation story predicts. We also find no corroborating evidence of any adverse effects of securities lending on volatility, skewness, or bid-ask spreads in the cross-section of stocks.

To highlight the importance of our experimental design for drawing inferences about short selling constraints, we also conduct the same cross-sectional analysis without imposing the exogenous supply treatment. We find that several characteristics are spuriously related to returns, volatility, and bid-ask

spreads in both the lending and recall periods. These relationships do not hold when we compare the same effects to the control group, highlighting the importance of the experimental design.

Overall, our lack of results on returns and bid-ask spreads for the securities that experience an exogenous supply shock suggests that supply restrictions to shorting may not be an important factor for asset pricing. In particular, the results appear unsupportive of Miller's (1977) overvaluation hypothesis.

There are reasons to be both cautious and aggressive in this interpretation. On the side of caution, while the supply shocks seem large by a variety of measures and have an effect on market loan fees and quantities, it is possible that larger supply shocks than we could provide would affect stock prices. Put differently, if shorting supply restrictions have measurable asset pricing consequences, then it must be at supply shock levels greater than those we provide – two to three times average daily trading volume, 18 to 37% of total short interest over the two phases of the experiment. In addition, the sample period for the first phase of the experiment is short and unusual, and hence may not be generalizable.

On the aggressive side, the uncertainty and volatility of markets during the first phase and our focus on high loan fee stocks should bias the results toward finding a large price impact from shorting. In addition, the second phase of the experiment provides an independent test during a longer and less extreme period, and yields similar results.

At the investment manager level, our findings have implications for the decision to lend shares. Over different time periods, our money manager has earned lending revenue net of fees ranging from 1.5 to 13 basis points per year on its largest mutual fund without adversely affecting stock prices. This strongly suggests that individual money managers and their investors benefit from securities lending.

At a macro or market level, our findings do not provide any support to the view that regulation designed to restrict loan supply can support stock prices. We acknowledge, however, that our experiment cannot address higher levels of lending or equilibrium effects of securities lending across all stockholders.

The paper proceeds as follows. Section II discusses existing research on the impact of shorting constraints and how our experiment provides novel evidence to this literature. Section III details our

sample and experimental design. Section IV presents the results from the experiment. Section V discusses the implications of our findings and their economic impact. Section VI concludes.

II. Existing Research on Shorting Constraints

The impact of short sale constraints on asset prices is theoretically ambiguous. Miller (1977) finds that the combination of differences of opinion and short sale constraints can lead to overpricing, where stock prices overweight the views of optimists. Diamond and Verrecchia (1987), in contrast, argue that rational uninformed agents take short sale constraints into account when setting prices, resulting in no overpricing. The effect of short sale constraints on stock prices is therefore an empirical question.

A key empirical issue is how to measure short sale constraints. One strand of the literature measures shorting constraints using direct measures of shorting costs such as the rebate rate or the spread between the rebate rate and the interest rate. The rebate rate is the fee the lender pays to the borrower on the collateral left with the lender. The spread between the rebate rate and interest rate is a direct cost to the short-seller, or revenue to the lender, known as the "loan fee." The evidence on the impact of rebate rates and loan fees on asset prices is mixed. See D'Avolio (2002), Geczy, Musto, and Reed (2002), Ofek, Richardson, and Whitelaw (2004), Jones and Lamont (2002), and Banerjee and Graveline (2011).

Another strand of the empirical literature uses short interest as a proxy for shorting demand. The results, summarized by Desai et al. (2002), are also mixed. Other studies use the unwillingness or inability to short among certain investors to proxy for shorting costs or demand. For example, Almazan, Brown, Carlson, and Chapman (2004) find that very few mutual funds sell short. Using this fact, Chen, Hong, and Stein (2002) use breadth of mutual fund ownership as a proxy for shorting supply while Nagel (2005) uses residual institutional ownership as a proxy for shorting demand. Both studies find mild evidence of overpricing for small, growth firms.

One of the difficulties in interpreting the results from using shorting costs to measure shorting constraints or using short interest to measure shorting demand is that both the price and quantity of shorting are determined in equilibrium. For example, a high level of short interest could imply a low cost

of shorting or a high shorting demand. Given that these measures are determined simultaneously and endogeneously, it is not surprising that the main results in this literature are so mixed.

Some studies try to mitigate the endogeneity issue by examining changes to the market for a stock's shares that indirectly move supply or demand. Sorescu (2000) uses options' introductions and Ofek and Richardson (2003) use lockup expirations among internet IPOs as proxies for reducing short sale constraints for the underlying stocks. Both papers find significant negative abnormal returns following these events. However, the introduction of options and use of lockup provisions may also be related to the demand for the stock (e.g., the endogeneity issue may not be resolved). Mayhew and Mihov (2005) find no evidence that investors take disproportionately negative positions in newly listed options, which suggests that option introduction may not causally relax short sale constraints.

Cohen, Diether, and Malloy (2007) use loan fees and amounts to identify shifts in shorting demand or supply. They exploit the fact that when prices (loan fees) and quantities (loan amounts) both increase, an upward shift in demand must have occurred; when prices and quantities both decrease, an upward shift in supply must have occurred. While a shift in demand (or supply) may be identified by this empirical strategy, it does not rule out that a shift in supply (or demand) did not also occur simultaneously, but to a lesser extent. In addition, not all demand and supply shifts can be captured by this method. Hence, the magnitude and importance of demand and supply changes is difficult to interpret. Our experiment, on the other hand, provides clear exogenous supply shocks, where magnitudes are known, but for a smaller set of securities. Despite taking a very different approach, Cohen et al. (2007) obtain similar results to ours. They find no price responses associated with supply shifts, consistent with our exogenous supply results. At the same time, they find significant price responses associated with demand shifts (that we do not study).

Several studies use financial securities regulation to help identify shorting constraints. Chang, Cheng, and Yu (2007) compare stocks that the Hong Kong Stock Exchange designates as eligible for shorting with stocks that are ineligible. They note that it is not possible to know whether the short sale eligibility designations are endogenously determined. Their results are mixed. While they find large

negative returns (roughly -5%) in the two weeks after stocks become eligible for short selling, they do not find any significant returns when the Exchange announces which stocks will become eligible for shorting. Greenwood (2009) exploits rules governing stock splits in Japan, and finds that sales restrictions have a significant impact on stock prices. Bris (2008) examines the ban on short selling of 19 financial firms following the SEC's July 15, 2008 Emergency Order, and finds no evidence that shorting affects the share prices of these firms. Bris, Goetzmann, and Zhu (2007) examine short sales restrictions from market regulators and practitioners across 46 countries and find some evidence that shorting improves information efficiency. Battalio and Schultz (2011) find that the September 2008 short sale restrictions in the U.S. had adverse effects on equity options markets. Beber and Pagano (2011) examine the effects of short selling bans around the world in the 2007-2009 crisis, and find that the bans reduced liquidity and price discovery, but failed to prop up stock prices.

All of the studies that use financial regulation to identify shocks to the shorting market are subject to the concern that the regulatory intervention and application to certain securities is unlikely to be exogenous, but rather related to other factors influencing pricing. For example, the shorting ban on financial stocks during the crisis was likely driven by other factors, such as market conditions and sentiment, that were related to the prices of those firms. In fact, regulatory changes more likely identify endogenous rather than exogenous variation in shorting, making the results difficult to interpret.

Closer in spirit to our study, Diether, Lee, and Werner (2009b) in conjunction with the SEC, randomly suspend short sale price tests for a third of the stocks in the Russell 3000 while maintaining the tests for the remaining two-thirds. They find that short-selling activity increases for the stocks whose tests are suspended, but daily returns and volatility remain unaffected. The Diether, Lee, and Werner (2009b) study is different from ours in several respects. First, the SEC-imposed price tests only restrict shorting for a very brief period of time (often seconds) and are completely binding by eliminating all shorting for that period. Our experiment provides exogenous variation in the supply of shares over the entire lending period (two weeks for the first phase, four months for the second phase) and provides differences in the magnitude of supply changes across stocks. Second, our experiment focuses on stocks

with ex ante high shorting demand relative to supply, where theory indicates supply constraints should be most binding. Third, we examine the impact of our experimental supply shocks on the lending market (loan fees and quantities) as well as the underlying stock market. Finally, Diether, Lee, and Werner (2009b) conduct their experiment from February to July of 2005—a relatively low volatility environment, whereas the first phase of our experiment takes place during the extremely volatile period of September and October 2008, and the somewhat less volatile June to September 2009 period for the second phase.[6] The two studies, therefore, cover a wide range of market conditions. The results from both studies are consistent, however, in that they do not find any detectable adverse pricing effects.

III. Experimental Design and Data

We briefly describe the (anonymous) Manager with whom we conduct the experiment and detail our data and experimental design.

A. *The Manager and Motivation for the Experiment*

The Manager is an active investment manager investing in mid-cap and small-cap equities, both inside and outside the U.S. Historically, the Manager had not lent out the stocks it owned out of concern that doing so might lower the prices of the stocks and increase their volatility. The motivation for this experiment arose when the Manager considered the fees it could receive from lending its shares. The experiment would allow the Manager to measure and weigh the benefits of lending its shares against the costs of any adverse price or volatility effects.

B. *Experimental Design*

The original intent was to run the experiment in a single phase of lending lasting approximately six months. Lending commenced on September 5, 2008. Because of the subsequent market turmoil,

[6] The average daily level of the VIX (obtained from Bloomberg) over Diether, Lee, and Werner 's (2009b) sample period from February to July 2005 is 11.2%. Over the first phase of our experiment from September 5 to October 3, 2008, the average daily VIX is 32.2%; over our second phase from June 5 to September 30, 2009, it is 26.2%.

including the collapse of Lehman Brothers on September 15 and concerns over the financial stability of many financial intermediaries, the Manager recalled the loans on September 18, 2008. The last recalled security was returned on October 3, 2008. These events define the first phase of the experiment. We emphasize that the decision to terminate the first phase of the experiment was made *before* any results were examined (by us or the Manager).

Because the length of the first phase was unintentionally shortened, the Manager decided to conduct a second phase after general market conditions stabilized. The second phase allows us to assess the robustness of the findings from the first phase and conduct the lending experiment under more normal market conditions over a much longer time period. The second phase also provides another independent shock to shorting supply in the market for a different set of stocks.

The second phase of lending began on June 5, 2009 and ended on October 1, 2009 when the Manager decided to lend out the previously withheld shares. The decision to terminate the second phase was driven in part by the results from the experiment. We discuss below how the decision to end the experiment might affect our interpretation of the results.

B. 1. First phase of the experiment

The Manager made shares available for lending in the first phase of the experiment on September 5, 2008. We selected the sample based on the Manager's stockholdings as of June 30, 2008. At that time, the Manager owned 523 individual stocks that were (in total) worth in excess of $15 billion. We divided the stocks into two groups. The first group included stocks that the lending agent projected would have a loan fee of at least 10 basis points per year, and so generate significant revenue from lending. The agent based these projections on existing lending demand and loan fees. We refer to the 138 stocks in this group as "revenue stocks." The positive fee implies these stocks have excess shorting demand relative to supply. We refer to the remaining group of 385 stocks are "non-revenue" stocks.

Within each of the two groups, we randomly selected two-thirds of the stocks to be available for lending and one third of the stocks to be withheld. Because we randomized within the Manager's

holdings at the time, we control for any stock selection ability that might affect security lending and its impact on share prices. For example, if the Manager's selection criteria are related to shorting demand, anything associated with the Manager's ability will difference out from the randomization. Hence, our experiment controls for any effect (observable or unobservable) related to the Manager.

The randomization also controls for other factors possibly affecting stock prices, such as general demand for shorting, lending by other institutions, market activity, security regulation, liquidity, risk, and mispricing. The control sample of stocks should difference out all observable and unobservable effects since assignment to the treatment and control groups is random. There is one exception to the randomization. The Manager asked us to lend the three stocks in the revenue group with the highest expected fees in order to reduce the experiment's opportunity cost. We include these three stocks in our analysis because they experience a supply shock, but our results are similar if we exclude them.[7]

Figure A-1 in the internet appendix shows that among revenue stocks, stocks randomly made available for lending do not differ from those randomly withheld in their equity market capitalization, market-to-book ratio, institutional ownership, shares owned by the Manager, prior six months return, short interest, prior 30 day average trading volume, or expected loan fee. The randomization process succeeds in selecting samples with similar characteristics, including proxies for loan demand (short interest and expected loan fee).[8] We interpret the experiment as an exogenous supply shock to the lendable shares of the available (treated) group of stocks, holding everything else constant by comparing to the withheld (control) group.

When the first phase lending period began, eight of the 40 stocks made available for lending based of expected loan fees at the time of the randomization were no longer eligible for lending because their actual loan fees in the marketplace had fallen to less than 25 basis points per year. Hence, the

[7] These results, and those for the other permutations of our sample described below, are reported in the internet Appendix. The inclusion of the additional high loan fee stocks should, if anything, make us more likely to detect adverse stock price effects from lending. Hence, any bias is likely to go against our main finding.

[8] Figure A-2 in the Appendix similarly shows that the randomization process generates no significant differences in the same characteristics between the available and withheld groups among the non-revenue stocks as well. While the experiment was not run on the non-revenue stocks, this confirms the success of our experimental design.

primary sample we study for the first phase of the experiment consists of 32 available revenue stocks that experienced a supply shock and 20 revenue stocks withheld from the lending market.[9] In the Internet Appendix, we show that our findings are not sensitive to different permutations of the sample and discuss briefly the robustness of our findings in Section IV.E.

B. 2. Second phase of the experiment

The Manager made shares available for lending in the experiment's second phase on June 5, 2009. We selected the sample based on the Manager's stockholdings as of April 30, 2009. The Manager asked us to conduct the randomization on U.S. stock holdings with expected loan fees greater than 25 basis points per year (revenue stocks), of which there were 32. We randomly selected 22 of these stocks to be made available for lending, and withheld 10 from the lending program. After reviewing the randomization, the Manager requested that we lend out one of the withheld stocks that had a particularly high expected fee (to reduce the opportunity cost of the experiment), resulting in 23 available stocks and 9 withheld. We report results including this selected stock as part of the group available for lending, but our results are the same if we exclude it. (See internet Appendix Table A-II.)[10]

When the second phase lending period began, four of the 23 stocks available for lending no longer had market loan fees of at least 25 basis points per year and so were excluded from lending. Hence, the primary sample we study for the second phase consists of 19 revenue stocks that experience a supply shock and 9 revenue stocks withheld from the lending market (hence, no supply shock). The second phase results, too, are not sensitive to different sample permutations.

C. *The Sample*

[9] Three of the available stocks and four of the withheld stocks were subject to the short-selling ban imposed by the SEC on September 18, 2008. However, as discussed above the Manager recalled all of its loans on that date.
[10] Figure A-3 in the internet Appendix compares the distribution of characteristics for the available and withheld stocks for the second phase of the experiment. The only significant difference between the two groups is that the stocks made available for lending have a slightly higher expected loan fee, which is primarily due to the one stock that the Manager specifically requested we make available.

C.1. Restrictions on loan size

The experiment is restricted to U.S. stocks whose gross loan fees were at least 25 bps per year at the start of the experiment. In the first phase, these criteria reduce the sample from 93 revenue stocks available to lend to 40 available stocks. Among the 45 withheld revenue stocks, 20 passed the two criteria. These two restrictions were already in place at the time of randomization for the second phase, so no additional stocks were filtered out (23 available, 9 withheld).

The Manager placed an additional restriction on the maximum loan size to be the lesser of (1) three times the average daily trading volume (based on the past 30 days trading activity) or (2) 5% of the outstanding shares of the company. We define the "potential loan amount" as the Manager's ownership in a stock subject to the above constraints. Hence, the potential loan is the minimum of the number of shares owned, three times 30-day average daily trading volume, or 5% of shares outstanding.

Because the experiment randomly selects stocks among the Manager's existing holdings *before* the experiment took place, the supply shock is not confounded by omitted factors related to the Manager. The restrictions placed by the Manager on the potential loan size, however, could be related to other factors that influence our outcome variables. For example, the additional restrictions on loan size may be driven by concerns of price impact and liquidity based on the Manager's trading experience. If these restrictions bind, they could introduce a non-random component to the experimental design.

We find, however, that these restrictions rarely bind in either phase of the experiment. When we regress the potential loan size on the Manager's ownership share in that stock plus the variables potentially restricting loan size (three times average daily trading volume and 5% of shares outstanding), we find that most of the variation is driven by the Manager's ownership share. For brevity, we do not report these regressions.

C.2. Summary statistics on stock characteristics

Table I presents summary statistics for the randomly available and randomly withheld stocks in our sample that pass the above criteria from the Manager. For these two groups of stocks, we report the means and medians for a variety of characteristics measured at the time of the randomization as well as the *p*-values from tests of differences in means and medians.

For the first phase, Table I shows no significant difference between the two groups in firm characteristics, trading activity, the Manager's ownership of shares, expected loan fee, or potential loan supply. The only statistically significant differences between the two groups are that stocks available for lending have slightly higher mean institutional ownership (but not higher median ownership), and have marginally higher short interest. For the second phase of the experiment, the only significant difference is that stocks made available for lending have significantly higher expected loan fees. None of these differences are the result of including the additional high revenue stocks in the available group.

Because the significant differences in stock characteristics from the first phase are not significantly different in the second phase, and vice versa, these differences are arguably consistent with chance. But, even if these differences are not random, any bias is likely to make it *more* likely to detect an adverse effect from stock lending, because higher expected loan fees and short interest indicate, if anything, more shorting demand.

Table I also provides summary statistics on proxies for hedging-related shorting demand. Two likely sources of shorting for hedging purposes are merger and acquisition (M&A) activity and convertible bond trades. We find that only 3% (5%) of available (withheld) stocks are involved in merger activity during the first phase (as bidders, none of our sample stocks were acquired during the experiment) and only 5% (11%) of available (withheld) stocks are involved in mergers during the second phase. Less than a quarter of our sample of stocks had convertible bonds outstanding during the experiment periods. These numbers suggest that hedging demand related to "merger arbitrage" or capital structure is unlikely to drive the bulk of shorting demand for our sample of stocks.[11] In addition,

[11] We also consider two other attributes of the stocks that may be related to variation in shorting demand: dividend payments and options traded on the stocks (which can be used to create synthetic short positions). We find little

differences in hedging demand between the available and withheld groups are negligible, as the random assignment of stocks is by design unrelated to hedging activity. Nevertheless, in Section IV we examine whether the measures of hedging demand are related to our outcome variables in the cross-section.

Finally, we report summary statistics for measures of valuation and disagreement that researchers have used to test overvaluation theories. Diether, Malloy, and Scherbina (2002) use lack of analyst coverage and dispersion in analyst forecasts as indicators of value uncertainty and disagreement to test Miller's (1977) overvaluation hypothesis. We examine these characteristics for our sample of stocks to see whether our sample is useful for testing overpricing. For example, if our stocks are unlikely to be overpriced because there is little uncertainty or disagreement about their values, then this may not be the best sample to investigate the role of shorting constraints on overpricing. We find, however, that based on those variables, our sample of stocks are likely good candidates to test overpricing theories. Across both phases, 41% of our stocks have no analyst coverage (data from IBES)— 44% (25%) of available (withheld) stocks in the first phase, and 58% (33%) of available (withheld) stocks in the second phase. Among stocks that have more than one analyst, earnings forecast dispersion (the standard deviation of earnings estimates across analysts divided by the mean per Diether, Malloy, and Scherbina, 2002) is quite high as well. In the first phase, the mean forecast dispersion is 0.27 (including both available and withheld stocks), which is above the 85th percentile of the IBES universe at the same time. In the second phase, the mean forecast dispersion is 0.57, which is also above the 85th percentile of the IBES universe at that time. In addition, our sample of stocks have market-to-book ratios, another variable that is used as an indicator of overpricing (Lakonishok, Shleifer, and Vishny, 1994), exceeding 2.5 on average, placing them above the 80th percentile of stocks at the same time (distributions from Ken French's website).

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variation in these attributes among our sample of stocks. Fewer than half of the stocks pay dividends (in either phase); the average dividend yield among those that do is less than 2% (3%) over the first (second) phase. We find no difference in dividend yields between the available and withheld stocks during either phase of the experiment. While most of the stocks in our sample have traded options, they are highly illiquid and infrequently traded.

Our sample of stocks do not appear to have much hedging-related shorting demand and have significant disagreement and high prices relative to fundamentals. Since these stocks also have high loan fees, our sample closely resembles the types of stocks envisioned by Miller's (1977) model.

C.3. Size of the supply shock

Table I suggests that the experiment provides a potentially sizeable shock to loan supply. The stocks in the sample have a median market capitalization of less than $1 billion, consistent with the Manager's focus on small- and mid-cap stocks. The Manager owns a meaningful fraction of these firms, averaging 4 to 5% of total shares outstanding. The stocks also are majority owned by institutional investors. Both the stocks made available and withheld from lending have significant short interest. For the first phase of the experiment, the mean short interest as a percentage of shares outstanding is 22.1% for available stocks and 15.6% for withheld stocks; these numbers are 14.3% and 15.6%, respectively, for the second phase of the experiment. In addition, the Manager's holdings average 3 to 7 times average daily trading volume.

The potential loan size is more than double the average daily trading volume of the stocks for both phases of the experiment. The potential loan is 3.7% of total institutional ownership for the first phase and 6.9% for the second phase. The potential loan represents 18.3% of pre-experiment short interest for the first phase and 36.8% for the second phase. These statistics suggest that the lending program represents sizeable changes to the supply of lendable shares.

D. *The Lending Experiment*

The first phase of the lending experiment began on September 5, 2008. At its peak, on September 17, over $580 million of securities were lent. On September 18, 2008, the Manager called the loans back. The last shares were returned on October 3, 2008. We examine the effects on stocks lent versus those withheld during the lending period (September 5 to 17), where the stocks available for lending experience an exogenous supply increase, and the recall period (September 18 to October 3), where supply decreases.

The experiment's second phase began on June 5, 2009. On October 1, 2009, the Manager decided to remove the restrictions on lending the withheld stocks to maximize revenue from the lending program. We examine the effects on stocks lent out versus those withheld during this lending period (June 5, 2009 through September 30, 2009). There is no recall period for the second phase.

The two phases cover quite different market environments—the first phase being unusually volatile with market prices falling and the second phase following the beginning of a significant market upturn—providing additional robustness for our findings.

What if other institutions, retail investors, or insiders changed their lending at the same time? Even if other new lending entered or exited the market simultaneously, our results and their interpretation would not be affected because of our randomized design. The treatment and control groups of stocks should be affected similarly by any market-wide shocks, including changes in securities lending by other investors, since by definition those changes are unrelated to the random assignment of stocks to treatment and control groups. Any changes in other lending will therefore be differenced out in our tests.[12]

What about shorting without needing to borrow shares? Shorting without borrowing shares, i.e., naked shorting, is also unlikely to affect our results. Given the randomized design, naked shorting demand will difference out when comparing available and withheld stocks. Furthermore, SEC Rule 204T was put into place on October 14, 2008, in between our two phases (the rule became permanent on July 31, 2009.) This rule imposes requirements on broker dealers designed to curtail naked shorting, making it less of a factor for the second phase of our experiment. Because we find similar results in both phases, it seems unlikely naked shorting affects our results. Also, practitioners with whom we have spoken believe that naked shorting is not an important part of this market.

Concerns over the likelihood of share recalls potentially affecting the lending period (e.g., if potential borrowers worried about getting squeezed scale back their demand as a result) also are unlikely

[12] Data provided by Rizova (2010), who examines securities lending participation from data manually collected from mutual fund annual reports, shows that only one mutual fund company changed its supply of shares to the loan market during the first phase—our Manager. Of course, other institutions or investors besides mutual funds may have altered their participation during this period or during the second phase. While we cannot rule this out, our experimental design differences out any other lending effects.

to be important in our samples. The strongest motive for recalling shares is to vote them at annual meetings. Because these predominantly take place in April and May, they are outside of both phases of our experiment. Consistent with this, no stocks had record dates for proxy votes during the first phase. In the second phase, only 3 stocks did (2 available, 1 withheld).

Finally, we consider how independent the samples are in the two phases of the experiment. The overlap in stocks between the two phases is modest. Of the 52 revenue stocks from the first phase, only 16 are in the second phase, and only 9 of these appear in the same group in the second phase as they did in the first phase (7 switch groups). Second, even for the same stocks, the supply shocks themselves and the outcome variables are independent over the two phases. Daily returns and changes in volatility, skewness, and bid-ask spreads of individual stocks are independent through time, particularly over the nine months separating the two phases. Looking only at available stocks common to both phases of the experiment, we find zero correlation in returns or other outcome variables over the two lending periods. Hence, we treat the two phases as two independent trials of the lending experiment.

E. *Supplemental Loan Fee and Quantity Data*

From the experiment itself, we obtain expected loan fees (before the lending periods) provided by the lending agent and the actual loan fees received by the Manager for the stocks randomly lent out. We supplement these data with daily loan fees and new loan amounts from Data Explorers for both phases of our experiment. Data Explorers obtains their data from a variety of lending agents that include investment banks and prime brokers. The data cover over 20,000 securities lending programs worldwide and over $2 trillion worth of loans. The fragmented and voluntary nature of this market makes it difficult to know how much of the lending market this dataset covers and how reliable the data are.

We employ the Data Explorers data as another measure of market-based loan fees. Because the securities lending market is fragmented, there are often multiple fees on the same stock at the same time from different lending agents (see Kolasinski, Reed, and Ringgenberg, 2010). On each day, Data Explorers reports the average loan fee for new loans (originations) of a given stock. We use these data to

compare the fees received by the Manager for loans of a given stock on a given day to fees received by other lenders for loans of the same stocks on the same day.

The correlation between the Data Explorers fees and the actual fees the Manager received on the available stocks is 0.65 during the first phase's lending period and 0.69 during the second phase's lending period. This indicates the fees are strongly correlated, albeit not perfectly so—another indication of the degree of segmentation in this market. We also use the Data Explorers data to obtain estimates of loan fees on the stocks we randomly withhold from the market.

F. *Summary Statistics on Lent Stocks*

Table II presents summary statistics on the loans actually made over the two phases. In the first phase, the average (median) stock is on loan for 12.6 (13.5) days total—approximately 6 days during the lending period and 7 days during the recall period. The average (median) stock is on loan for 49.2 (68.0) days during the second phase. For stocks on loan, the mean (median) equal-weighted average annualized loan fee is 259 (81) basis points in the first phase, and 159 (101) basis points in the second phase. The range of average loan fees is 1.4 to 976 basis points. The mean (median) loan size-weighted average fee is 768 (523) basis points during the first phase and 147 (101) basis points during the second phase.

Mean and median actual fees are smaller than the expected fees quoted by the lending agent (comparing Tables I and II). Conversations with the lending agent (and other lending agents) indicate that the expected fees are quoted based on current, not expected, demand and supply. Hence, the potential impact of additional supply from the Manager could partly explain this discrepancy, and would be consistent with the supply shock from our experiment being large enough to put downward pressure on fees. In the next section, we find evidence corroborating the downward price pressure on loans.

On average, the shares on loan represent 5.02% (8.93%) of the pre-experiment short interest during the first (second) phase of the experiment. When new shares are lent, the average new lending is

42.6% (70.3%) of exchange-reported daily trading volume for the first (second) phase.[13] The average daily shares on loan represent about 1% of equity market capitalization, with the maximum loan level averaging 1.6% of equity market capitalization for the first phase and 1.7% for the second phase.

Overall, Table II suggests that the actual loans made by the Manager represent a meaningful number of shares in many companies, and therefore that the supply shock of lendable shares from our experiment is sizeable. Next, we investigate the effect of the supply changes on loan fees and quantities.

IV. Results

In this section, we examine the effects of increasing the loan supply of shares for the treated (available) firms relative to the control (withheld) firms. We first analyze changes in stock loan fees and lending quantities. We then turn our attention to returns, volatility, skewness, and bid-ask spreads of the underlying shares. We also exploit cross-sectional variation in loan supply and stock characteristics to examine these effects.

A. *Loan Fees and Quantities*

A.1. Loan Fees

Table III examines the impact of the loan supply shocks on loan fees. We compare the average and median daily loan fees of stocks randomly made available for lending to those randomly withheld and look at changes in these lending fees before and after the loan supply shock. For both phases of the experiment, we use a difference-in-differences approach. We compute the average change in daily loan fees from the pre-lending to the lending period for the available and withheld stocks and then calculate the difference between the two groups of those changes.

Panel A of Table III reports the results. We use four different methods and data sources to measure loan fee changes. The first row of Panel A uses (average) Data Explorers fees for all stocks for

[13] We use trading volume three business days before the loan date because short sellers need only borrow the shares for settlement three business days after placing the trade. The maximum numbers can exceed 100% because exchange-reported trading volume does not include off-exchange trading.

both the pre-lending and lending periods. The second row uses Data Explorers fees for the lending period and the expected fees reported by the lending agent for the pre-lending period. The third row uses the actual lending fees for available stocks and the Data Explorers fees for the withheld stocks during the lending period, and the expected fees from the lending agent for all stocks for the pre-lending period. Finally, the fourth row uses the actual lending fees for available stocks and Data Explorers fees for withheld stocks during the lending period, and the Data Explorers fees for all stocks for the pre-lending period. Because fees are averaged across days for each stock, the serial correlation in fees is accounted for (i.e., we have one difference measure per stock).

Panel A reports a negative average impact on fees from the supply shock for all four measures of loan fees and for both phases of the experiment. We report both simple differences-in-differences estimates and estimates employing nearest neighbor matching on pre-lending short interest. The latter, which we refer to as Average Treatment Effect of the Treated (ATT) estimates, accounts for the possibility that changes in loan fees may be related to the amount of shorting. Both sets of estimates produce negative numbers, indicating a decline in loan fees for the available (treated) stocks relative to the withheld (control) stocks. For for both sets of estimates, six of the eight negative measures are statistically significant. These results indicate that our experiment produces loan supply changes large enough to affect fees. The economic magnitude varies across the measures from -9 to -345 basis points per year during the first phase, and from -210 to -303 basis points per year during the second phase.

Panel B of Table III reports cross-sectional regressions of loan fee differences between the lending and pre-period (using the fourth measure of fee differences above) on the size of the loan made in each stock. We scale the loan size by the potential loan size determined by the Manager as well by shares outstanding (to measure the loan size relative to the market). We run regressions for the available stocks whose shares are actually lent (first column) and for all stocks including those withheld (second column).

In both phases of the experiment and for both samples of stocks, there is a consistent negative relation between the size of the loan shock and the change in loan fee (loan size is zero for withheld stocks). Seven of the eight coefficients are significantly negative. Larger lent amounts are associated

with larger decreases in loan fees, providing additional support that our experiment provides sizeable loan supply shocks that matter for loan prices.

The evidence in Table III is consistent with theory that larger supply shifts have bigger fee consequences (e.g., Duffie, Garleanu, and Pedersen, 2002) and suggests that our experiment generates meaningful loan supply changes. Moreover, the significant decrease in loan fees also suggests that our tests have enough power to reject the null hypothesis at conventional levels of significance.

A.2. Loan Quantities

Table IV investigates the effect of our experiment on loan quantities and market-wide short interest (from Compustat). Given the strong decline in loan fees we find in Table III, it is not clear whether we should expect much effect on short interest. If the quantity of demand for shorting is relatively price inelastic, then a supply shock should result in a strong price (loan fee) response, which we find, but not much of an effect on equilibrium quantities. If, on the other hand, demand is relatively price elastic, then we should find an impact on quantities, too.

Panel A of Table IV reports estimates of differences between available and withheld stocks in changes in market-wide short interest from the pre-lending to lending period for the two phases. We report differences-in-differences estimates and nearest neighbor matching based on pre-lending loan fees (that take into account the possibility that changes in shorting quantities may be related to loan fees).

For the first phase, we find that both estimates of the supply shock impacts on short interest from our experiment are positive, but not statistically significant. Short interest , however, is highly volatile and infrequently reported (biweekly). During the lending period for the first phase, short interest is only reported once (on September 15, 2008), which limits the power of our tests. Focusing on economic rather than statistical, significance, the numbers in Table IV imply an increase in short interest of available relative to withheld stocks of 67 to 85 basis points of shares outstanding.

For the second phase, the difference-in-difference estimate is negative, but insignficant. The matched estimate, in which the short interest of each available stock is compared to that of the withheld

stock with the closest pre-lending loan fee, is positive and significant, implying an increase in short interest of available stocks compared to withheld stocks of 2.76% of total shares outstanding.

Another problem with using infrequently reported short interest to measure shorting activity is that large changes in shorting activity may be missed if the typical short holding period is shorter than the reporting interval. To address this possibility, we use Data Explorers data on daily new loans (originations). These daily data allow us to construct a more powerful test than is possible with biweekly short interest data, and to capture more dynamics of shorting activity. While the Data Explorers data may not capture all lending activity in the market, as long as it is representative we should see the Manager's lending reflected in lending activity reported by Data Explorers if that activity is significant.

Panel B of Table IV reports results from regressions of the change in loan quantity in the market for each stock on the size of the loan made by the Manager (both represented as a percentage of shares outstanding) and on a dummy variable indicating whether the stock is available (=1) or withheld (=0). The coefficient on the loan size made by the Manager (equal to zero for all withheld stocks) represents the impact of our supply shock on market loan quantities, where the available dummy variable controls for differences between the treatment and control groups. For both phases, the loans made by the Manager on a given day are significantly positively related to the total new loans in the market on that day reported by Data Explorers. And, the zero coefficient on the available dummy indicates that our randomization effectively controls or differences out any market-wide lending activity changes that might have occurred. Hence, the regression has a clear interpretation as a pure exogenous supply shock.

Overall, then, we interpret the evidence in Tables III and IV to indicate that the experiment had a measureable and sizeable impact on market loan prices and quantities.

B. *Returns*

We now investigate how stock returns are affected by the loan supply shocks. We conduct three different tests. First, we examine average returns during the experiment (lending and recall periods).

Second, we look at event studies for returns at various windows. Finally, we examine cross-sectional tests relating returns to stock characteristics and loan supply.

B.1. Average Returns

Table V analyzes how the supply shock to lendable shares affects returns by comparing the average daily returns of the available stocks to those of the withheld stocks over three periods: the "pre-lending period" (roughly a month before the lending period), the lending period, and the recall period.

We form two portfolios, one containing the available stocks and one containing the withheld stocks, and compare the average daily portfolio returns over each period. We compute average returns using several weighting methods: equal-weighted, value-weighted, and expected loan fee (at the time of the randomization)-weighted. The difference between the equal-weighted and value-weighted results provide insight into whether expanding loan supply is more important for smaller or larger stocks, while the expected loan fee-weighted results indicate whether the effect is stronger for stocks with higher ex ante shorting demand relative to supply (as implied by high loan fees).

If loan supply shocks from the experiment have pricing effects, then returns should decrease during the lending period when supply increases and increase during the recall period as the supply of shares is suddenly reduced. We report raw returns, though returns adjusted for the Russell 2500 index (the index most closely related to the Manager's holdings) yield similar results.[14]

Panel A of Table V reports the results for the first phase; Panel B, for the second phase. Standard errors are robust to heteroskedasticity (White, 1980). Because we average returns across stocks for a given day, standard errors also account for cross-correlation in returns at a point in time. The first three columns of each panel indicate that returns to available stocks do not differ significantly from returns to withheld stocks during the pre-lending periods of the two phases. The next three columns of each panel report average daily returns over the lending period.

[14] There should be no need to adjust returns for risk or a benchmark, because the treatment and control groups are randomly assigned and should have identical risk characteristics. We confirm that risk/benchmark adjustment yields nearly identical results.

For the equal-weighted portfolios, during the first phase, the returns to available stocks are negative, but less so than the returns for stocks not available for lending (-26 versus -72 basis points). The difference between the two portfolio returns is 47 basis points (annualized) per day and is statistically insignificant. This result highlights the importance of the experiment. Without the random control group of stocks, we might erroneously conclude that the -26 basis points for the available stocks indicates price pressure from increased shorting supply. However, because the control group of stocks experience an even greater decline in returns without any change in loan supply, the negative returns must have been driven by other factors unrelated to loan supply. In the second phase, available stocks outperform withheld stocks on an equal-weighted basis, but by an insignificant 13 basis points.

For the value-weighted portfolios, available stocks underperform by 28 basis points in the first phase, but outperform by 21 basis points in the second, with the latter marginally statistically significant.

For the expected loan fee-weighted portfolios, available stocks outperform by a statistically significant 111 basis points in the first phase, and outperform by an insignificant 13 basis points in the second.

Across both phases of the experiment, therefore, we fail to detect any significant *negative* pricing effect from shocks to loan supply. Indeed, the point estimates are largely in the wrong direction for a price pressure story.

The final three columns of Panel A of Table V report the difference in returns between the available and withheld stocks over the recall period for the first phase. The equal-weighted returns to available stocks average -69 basis points while the returns to stocks not made available average -27 basis points. This difference of -42 basis points is insignificant. The value- and expected loan fee-weighted results also deliver negative return differences between the available and withheld stocks that are not

significant. The more negative returns to available stocks over the recall period, when loan supply decreases, is also inconsistent with a price pressure story.[15]

Overall, we fail to find evidence consistent with Miller's (1977) overpricing hypothesis that supply increases in the shorting market create negative price pressure for the underlying stock.

B.2. The Power of the Average Return Tests

It is useful to consider the power of our tests for detecting any adverse price consequences, bearing in mind that our analysis of loan fees in Table III suggests that the experiment is sizeable enough to have power to detect loan fee effects. In this section, we gauge the power of our tests by considering the magnitude of return differences that we can reject at various levels of significance.

Table VI reports rejection cutoff values for tests of differences in returns between the available and withheld stocks over the lending and recall periods of the first phase and the lending period of the second phase. We report rejection region cutoffs for one-tailed *t*-tests on return differences at the 10%, 5%, and 1% significance levels. For lending period differences, the cutoff value is a lower bound (because the test is for negative return differences), while for the recall period differences the critical value is an upper bound (because the test is for positive return differences). The numbers reported represent the values of the difference between available and withheld stocks that we can reject at the specified level of significance over the relevant period in a one-tailed test. We report critical values for equal-, value-, and expected loan fee-weighted average differences across all phases of the experiment.

Table VI indicates that for the first phase, the power of our test depends on the weighting scheme and varies considerably. But, the second phase is less sensitive to the weighting scheme and provides a more powerful test. This makes sense because the second phase covers a longer and less volatile time period. We can reject at the 10%, 5%, and 1% significance levels that the lending-period return

[15] We also examine the non-revenue stocks in the first phase. The vast majority of these stocks were not lent even if available. Four available non-revenue stocks were lent because their loan fees increased above 25 basis points. The returns to these stocks over the lending and recall periods are indistinguishable from those of the withheld stocks.

differences are less than -5 to -19 basis points when equal-weighting, between +5 and -8 basis points when value-weighting, and between -18 and -43 basis points when weighting by expected loan fees.

The greater power of our experiment, however, comes from running two independent trials or phases. Over the lending periods from the two phases, based on *p*-values for the equal-weighted results, the joint probability that we would observe one observation of at least 47 basis points (*p*-value = 0.12), and then another of at least 13 basis points (*p*-value = 0.175), relative to a null that the return difference in each phase is less than or equal to zero is 2.1%. The analogous calculation for the expected loan fee-weighted portfolios yields a joint probability of less than 1%.

To calculate rejection regions that account for both phases requires combining the results from those phases. One simple and robust way to obtain rejection regions for the two phases jointly is to take the precision-weighted average of the estimated return differences for each phase. The precision (inverse of variance) of the estimates for each phase takes into account the number of stocks, the volatility of stock-level returns, and duration of each phase, and is the most efficient (minimum variance) estimator of the combined return difference.

The last column of Table VI reports rejection cutoff values for the combined phases of the experiment. We obtain much tighter rejection regions that vary less across the different weighting schemes. At the 10% significance level, we can reject that the return difference between available and withheld stocks is negative for all three portfolios. At the 95% confidence level, we can reject that the return difference is less then -3 basis points for the equal- and value-weighted portfolios, and less than +25 basis points for the expected loan fee-weighted portfolios. At the 99% confidence level, we can reject that the return difference is less then -12 basis points for the equal- and value-weighted portfolios, and is less than +12 basis points for the expected loan fee-weighted portfolios. Thus, the loan fee-weighted results reject the Miller (1977) hypothesis even at the 1% significance level.

These are fairly tight rejection regions, suggesting that our tests have sufficient power to detect reasonably small return effects, and that the results are largely inconsistent with negative price pressure.

Another issue regarding the power to detect return effects is to consider our previous results on loan fees. Table III indicates our experiment had a significant impact on actual loan fees of 284 to 345 basis points per year. Are these effects large enough to detect return effects over our sample period given the power of our tests? We think so, based on reasonable assumptions. For instance, if the supply shock is permanent (which, after the second phase it has been), and if the Miller (1977) model is correct and there is a one-to-one mapping from loan fees to returns, then the experiment should produce a permanent 284 to 345 basis point shock to the annual expected return of the treated stocks. This would translate into a huge permanent price effect that should be easily detectable. On the other hand, if the supply shock is temporary, then the return effect could be much smaller. To take an extreme case, suppose the supply shock only lasts one day (an assumption at odds with the actual loans made), then the expected return effect would be about 1.2 basis points, which is not easily detectable. The actual duration of the supply shock was about 12.6 days on average in the first phase, which suggests a large enough price impact to be detectable based on our critical values in Table VI. For the second phase, the average actual duration is 49 days, but the supply shock has been maintained since June 5, 2009, which as of this writing is 805 days (and counting), and therefore implies an even larger price effect. These implied price effects should be easily detectable according to our critical values in Table VI.

B.3. Event Studies for Returns

Another question is how quickly the market recognizes the supply shocks and incorporates them into prices. The loan fees seem to change on the day of the supply shock, but it's possible that effects show up in returns at different times. While we do not find any return effects over the entire lending or recall periods, perhaps the effects are present at higher frequencies that are masked by the entire period. To investigate this possibility, we conduct event studies of returns at various windows. Another benefit

of the event study approach is that it circumvents the potential concern that the average return results could be biased by the Manager's potentially endogenous decision to end each phase.[16]

Table VII reports results of event studies for returns to available versus withheld stocks at various windows after the beginning of lending for each phase of the experiment (and after the beginning of recall for the first phase). These event studies allow us to assess what the results would have been had the experiment ended at various hypothetical and exogenous points in time. In addition, since the first phase contains the Lehman Brothers bankruptcy on September 15, 2008, it is interesting to see how sensitive the results are to this episode of enormous market turmoil.

The results in Table VII show that for no event window, in either phase of the experiment, do we observe significantly lower returns to available stocks compared to those withheld. These results confirm our previous conclusions from the entire lending and recall periods. Had any of these event windows been chosen as an exogenous termination date ex ante, the experiment would have yielded similar results. In addition, there is no consistent pattern in returns over the first day or two of the experiment across the two phases. Nor is there any evidence of negative price pressure before the Lehman bankruptcy (the first week of the first phase of lending).

One caveat to these conclusions is that it is possible that we would have detected adverse effects from lending had the second phase continued for more than four months. This caveat holds no matter when the second phase would have ended, even if exogenously specified in advance. However, we note that since the experiment ended, the Manager has chosen to lend all of its shares and we have not detected any adverse price effects (albeit without the benefit of a control group) as of this writing (two years later).

B.4. Cross-sectional Return Tests

[16] While we already discussed that the decision to recall the loans in the first phase was not endogenous to the results, as neither we nor the Manager had yet analyzed the data, the decision to lend the control stocks in the second phase after four months, thereby ending the experiment, occurred partially because we detected no adverse pricing consequences from lending. If the Manager had detected adverse effects of lending early on in the second phase and chosen to continue the experiment until these effects disappeared, then our finding of no adverse return effects in the lending period could be misleading. The event studies can help shed light on whether this occurred and offer arbitrary cutoff points that avoid this potential issue.

The previous results compare the average stock in the treatment and control groups which may mask important cross-sectional information. Returns may be related to certain stock characteristics or the magnitude of the loan supply. In fact, Table III shows that larger supply shocks are associated with larger loan fee declines. To investigate this possibility, we estimate cross-sectional regressions of stock-level average daily returns during the lending and recall periods on stock characteristics and potential loan supply interacted with the treatment and control of lending availability. We also report results for the available stocks only (i.e., with no control group) to highlight the potential for misleading inference without the benefit of an exogenous control group. Given the limited degrees of freedom, we focus in all cases on specifications with one explanatory variable at a time (and its interaction with treatment).

Panel A of Table VIII reports results examining cross-sectional variation in loan supply from the Manager and shorting demand. Relaxing shorting constraints is likely to be most important when the relaxation is largest, for stocks with the highest shorting demand relative to supply, or for stocks whose shorting demand does not stem from hedging motives.

The first set of tests in Panel A of Table VIII shows that the Manager's potential loan supply (as a percentage of total institutional ownership in the stock) is modestly negatively related to the returns of available stocks relative to the returns of withheld stocks during the first phase lending period. This is suggestive of negative price pressure at high levels of loan supply, but the result is only statistically significant at the 10% level. During the lending period for the second phase, however, we do not find this result. Potential loan supply is also unrelated to returns in the recall period of the first phase.

The next three sets of tests in Panel A of Table VIII show that returns to available stocks relative to withheld stocks are unrelated to pre-lending short interest or expected loan fees—both measures of shorting demand—and are unrelated to an indicator for whether the stock has meaningful hedging-driven shorting demand. The hedging demand indicator is equal to one if the firm has merger activity or convertible bonds outstanding over the relevant period, and zero otherwise.

Comparing the results for all stocks with the available versus control interaction to the results for available stocks only (hence, no control group) highlights the importance of the exogenous experimental

design. In tests using available stocks with no control group, we find several significant results: short interest is positively related to first phase lending period returns and negatively related to first phase recall period returns; expected loan fees are positively related to first phase lending period returns; hedging demand is negatively related to lending period returns. However, these relations are spurious because they disappear when we include the control group of withheld stocks. Relative to the returns of withheld stocks, there is no evidence that returns of available stocks are related to any of these characteristics.

Panel B of Table VIII focuses on measures of stock valuation and disagreement. According to Miller's (1977) model, relaxing shorting constraints should be most important for overvalued stocks when shorting is constrained. Examining the interaction with market-to-book ratio, a proxy for overvaluation, in the first set of tests, we find that returns to available stocks versus withheld stocks increase with market-to-book, for both phases of the experiment. This result is opposite from the prediction of the overvaluation hypothesis that returns are lowest among the most highly valued stocks.

Examining the interactions with analyst coverage, during the lending period of the first phase returns to available stocks with no analyst coverage are lower than those of withheld stocks with no analyst coverage, consistent with the overvaluation hypothesis. However, we do not find similar results for the second phase nor a reversal in the recall period of the first phase.

Looking at analyst forecast dispersion (among stocks with more than one analyst), during the lending period of the second phase returns to available stocks with high analyst forecast dispersion are lower than those of withheld stocks, while the opposite pattern holds during the recall period of the first phase. There is no relation during the lending period of the first phase. This is mildly suggestive of price pressure effects among stocks with the highest degree of disagreement.

Overall, then, we fail to find consistent evidence of a cross-sectional relation of greater price pressure on the available stocks to measures of overvaluation.

C. Volatility, Skewness, and Bid-Ask Spreads

We next turn to an analysis of changes in stock-level volatility, skewness, and bid-ask spreads.

C.1. Differences-in-Differences

The first three columns of Table IX report difference-in-differences of the standard deviations of the stocks made available for lending relative to the stocks withheld from lending from the pre-period to the lending period, and from the lending period to the recall period. Volatilities for each stock are computed from daily returns over each of the periods. We report equal-, value-, and expected loan spread-weighted difference-in-differences with *p*-values reported in parentheses.

If short selling improves price discovery, then reducing constraints to shorting by lending shares should improve price discovery and lead to lower volatility. Hence, we expect the volatility of available stocks should decrease relative to that of withheld stocks in the lending period, and should increase in the recall period. On the other hand, if shorting destabilizes markets, then we should find the opposite effects.

For both phases, the difference-in-differences between the available and withheld stocks from the pre-lending through the lending period are negative, but insignificant. These results suggest that the additional supply of lendable shares reduces volatility, consistent with the notion that shorting improves price discovery and inconsistent with a destabilizing effect of shorting. The difference-in-differences estimate of the changes in volatility from the lending to recall period between available and withheld stocks are statistically and economically insignificant, oscillating around zero.

The next three columns of Table IX report results from analogous tests using estimates of individual stock return skewness (estimated using daily returns). If shorting constraints are binding, negative news may not be adequately incorporated into prices, which may or may not lead to overpricing (e.g., Miller, 1977; Diamond and Verrechia, 1987). However, even in the case of no mispricing, Diamond and Verrechia (1987) find that the distribution of returns may be affected, resulting in positive skewness for stock returns. Therefore, if shorting constraints are important, we expect to see negative changes to skewness during the lending period and a reversal (positive changes) in the recall period.

In the first phase, there are no detectable differences in skewness changes for the available stocks during either the lending or recall periods relative to the withheld stocks. The difference-in-differences estimates are negligible. In the second phase, the difference-in-differences estimates are somewhat larger,

but still statistically insignificant. Two of the three signs are opposite those for the first phase. These results suggest that shorting supply shocks have little effect on the skewness of returns.

The last three columns of Table VI repeat the analysis using individual stock bid-ask spreads. To the extent that short selling, and therefore relaxing short sale constraints, aids in price discovery, we expect bid-ask spreads to decline for available stocks compared to withheld stocks over the lending period and a reversal over the recall period. If short sales are destabilizing, we expect to see the opposite pattern.

The difference-in-differences estimates from the pre-lending to the lending period are negligible at only a few basis points, for both phases, suggesting no reliable effect. The difference-in-differences estimates from the lending to the recall period are negative and generally significant, indicating spreads narrow after supply is reduced. This is consistent with a stabilizing effect from reduced loan supply and broadly consistent with some of the results for volatility. The lack of consistency between the two lending period results and the recall period results cautions against drawing any strong conclusions.

C.2. Cross-sectional Volatility, Skewness, and Bid-Ask Spread Tests

Table X reports cross-sectional regressions for stock-level changes in volatility, skewness, and bid-ask spreads from the pre-lending to lending periods, and from the lending period to the recall period for the first phase, on various stock and loan supply characteristics. The regressions are analogous to those for returns presented in Table VIII. For brevity, Table X focuses on measures of the Manager's loan supply and shorting demand.

Panel A of Table X reports results for changes in volatility. There is evidence that volatility increases in the second phase lending period for available stocks (relative to withheld stocks) with high Manager loan supply or high pre-lending expected loan fees. However, we do not find the same results in the first phase. As with the cross-sectional tests for return differences, comparing the tests with available stocks only (which ignore the exogenous control) to those including all stocks (that control for the withheld group) reveals several spurious relations that disappear when the control group is included.

Panel B of Table X reports results for changes in skewness. There is weak evidence of an increase in skewness for available stocks with high Manager loan supply in the first phase lending period, but not in the second phase. The other results in Panel B are generally insignificant, with the exception of an increase in skewness for high expected loan fee stocks in the recall period of the first phase.

Finally, Panel C of Table X reports results for changes in bid-ask spreads. There is some evidence that bid-ask spreads increase for available stocks with high pre-lending short interest in the first phase lending period. There is also evidence that bid-ask spreads increase (decrease) for available stocks with high Manager loan supply (hedging demand) during the lending period of the second phase. None of these patterns are robust across phases of the experiment, however. Once again, comparing tests using only available stocks to those controlling for withheld stocks reveals several opportunities for spurious or misleading inference without the benefit of the exogenous experimental design.

D. *Post-October 1, 2009 results for the second phase of the experiment*

On October 1, 2009, the Manager lifted the restrictions on lending the withheld stocks in the second phase, making these stocks available for lending. Examining returns, volatility, skewness, and bid-ask spreads for these stocks from October 1 to December 31, 2009, we again detect no evidence of adverse consequences of stock lending. While only five of the withheld stocks experienced a significant supply shock after October 1, making the sample too small, this is a *third* independent test of the impact of a positive supply shock that finds no significant effect on stock prices.

E. *Robustness*

Finally, other perturbations of the available and withheld groups yield similar findings. The sample we use throughout the paper is arguably the most likely to detect a lending effect because it compares only those stocks actually experiencing supply shocks to those whose lendable shares remain constant. Other samples, however, can be justified as well.

In the Internet Appendix, we report all of our main results across various permutations of stock samples for the available and withheld groups for both phases of the experiment: (1) all available vs. all withheld revenue stocks (40 available vs. 20 withheld for the first phase and 23 available vs. 9 withheld for the second phase); (2) removing the non-random additionally made available stocks with extremely high revenue (there were three such stocks for the first phase and one for the second, making 37 available vs. 20 withheld for the first phase and 22 available vs. 9 withheld for the second phase); (3) removing all stocks whose loan fees declined to less than 25 basis points before the experiment (32 available vs. 17 withheld for the first phase and 19 available vs. 8 withheld for the second phase); and (4) removing both the additional non-random available stocks and the stocks whose lending fees declined below 25 basis points (29 available versus 17 withheld for the first phase and 18 available vs. 8 withheld for the second phase). The bottom line is that the results are qualitatively unaffected by these changes in sample choice.

V. Implications of the Experiment and Economic Magnitudes

We discuss the implications of the results of our experiment, focusing on three main areas.

A. *Theoretical implications*

First, our results have implications for theories of securities lending and shorting constraints. In motivating the experiment, we provide an exogenous shock to loan supply that allows us to measure the effects of supply changes in a way no other study to date has been able to do. We find no adverse effects on stock prices from loan supply changes, even though loan fees change significantly.

This raises an important question: Does theory provide a mechanism for why supply shocks would effect loan fees but *not* stock prices? Our finding that exogenous supply changes move loan fees is consistent with Duffie, Garleanu, and Pedersen (2002). Their model, however, also predicts an impact on stock prices because there is a direct link between loan fees and prices in the model, though the link is not one-to-one. In Diamond and Verrechia (1987), the market maker absorbs loan supply effects so that prices are not impacted, consistent with our results, but their model has no role for loan fees.

One possible explanation for the different effects on loan fees and stock prices is that our sample of firms may not have been overpriced during the experiment. Because we run two independent phases of the experiment during two very different market environments (the first having huge shorting demand), this seems unlikely. Furthermore, we find mixed evidence, at best, of overpricing on the stocks that the literature considers the most likely to be overpriced – those with the highest valuation ratios and with the highest analyst disagreement.

Another possibility is that the market for stock borrowing is segmented from the underlying stock market. For example, if demand for stock borrowing is primarily driven by hedgers who are too small to affect the underlying stock, and trading in the underlying stock is driven by other investors, then we could see effects on loan prices without affecting the stock price. Using merger activity and convertible bond trading to proxy for hedging demand, we find little evidence to support this explanation. However, we do find that among our sample of stocks that options markets are much less liquid than the market for their underlying shares. If options represent another vehicle for hedgers to place their trades, this could be consistent with hedgers being a larger and more influential part of the less liquid derivative and lending markets but an inconsequential part of the spot market.

Other frictions may exist in practice that are absent from the models we test. Loan fees may not be market clearing in practice if lending agents ration lendable securities, reward certain clients, or bundle securities lending with other services. Kolasinski, Reed, and Ringgenberg (2010) present evidence consistent with the latter explanation. Hence, loan fees may be affected by supply shocks that do not impact securities prices because lending agents compete on non-price dimensions. Future theoretical and empirical work may consider the wedge between the lending and underlying security markets.

Finally, it is possible that our supply shocks, despite all the evidence we argue to the contrary, are simply too small to detect any pricing effects. If so, then a minimalist view of our experiment is that it provides a set of characteristics that any supply movement must exceed in order to have any measurable pricing effects. These characteristics include a lending supply effect of 42 to 70 percent of daily trading volume, 5 to 9 percent of short interest, and a 200 to 300 basis point reduction in actual loan fees. If these

are not big enough, then any supply effects on prices consistent with theory must be larger still. While no systematic evidence exists, anecdotally supply shocks of at least this magnitude seem rare. Hence, while the theories could be correct at more extreme supply magnitudes, it is likely that these shocks occur infrequently.

B. *Investment management*

Second, our results have implications for an investment manager's decision to lend its securities. Indeed, this issue motivated the Manager to run the experiment. Our findings that average loan fees for high demand stocks are meaningful and that the impact from lending on stock prices is negligible strongly suggests that individual money managers and their investors benefit from securities lending.

In the case of our Manager, its largest mutual fund earned roughly one basis point net of lending fees in the first phase of the experiment. The first phase ran for less than a month. If we conservatively annualize that by multiplying by 12, we obtain an annual benefit of 12 basis points. This is meaningful relative to the 75 to 125 basis points active mutual fund managers often charge, and substantial relative to the lower fees charged by index fund managers. For a $15 billion mutual fund, the 12 basis points would amount to $18 million per year. And these figures understate the true potential of lending during this period because they exclude the 1/3 of revenue stocks that were withheld.

Loan fees have declined since the financial crisis began. As a result, the Manager earned less (in basis points) in 2009 and 2010. In both 2009 and 2010, the Manager earned roughly 2 basis points (annualized) on its largest mutual fund. While this is substantially less than the 12 basis points of the earlier period, it still amounts to over $3 million per year on a $15 billion asset base.

These additional returns, even in the later period, appear to be large relative to the (low) administrative and other costs of setting up a lending program, and, therefore, would improve an investment manager's overall performance. Our Manager reached this conclusion based on these results. Whether the same conclusion would apply to all funds is an open question. While we cannot answer this

questions precisely, the insignificance of our cross-sectional results suggests that the basic findings hold across a broad spectrum of stocks.

Our experiment also only measures the marginal lending from our Manager over whatever other lending is already taking place. For stocks with little lending activity, lending fees could be higher (holding demand fixed), or if other funds participate, fees could drop even further than what we find. The effects on underlying share prices could also vary depending on the number of funds participating. Our results, therefore, do not address the equilibrium effects of all funds lending their shares simultaneously.[17]

C. *Financial Regulation*

Finally, our results potentially have implications for regulatory policy on shorting activity and securities lending. The intense debate over such regulation could benefit from additional experimental evidence like ours that identifies one channel of constraints while holding everything else fixed. While we are cautious about extrapolating our results to general equilibrium effects, our findings suggest that shorting and securities lending activity do not adversely affect stock prices and, therefore, do not provide any support to the view that regulation designed to restrict loan supply can support stock prices.

VI. Conclusion

Despite the intense debate over the effects of shorting in policy circles as well as among investors and corporate executives, the evidence regarding the effects of short sales on asset prices is mixed, due in large part to the difficulty in identifying clear supply and demand movements. Our study provides a novel randomized experiment that generates a sizeable supply shock to the loan market for securities.

Comparing the differences between the randomly available (treatment) and withheld (control) group of stocks in our experiment to difference out other confounding influences, we find that exogenous changes in loan supply have significant effects on loan fees and quantities, but no adverse effects on

[17] Rizova (2010) finds that loan participation among U.S. mutual funds increases from 39% to 69% over the last decade. It would be interesting to know the impact of this increased trend in loan participation on both loan fees and stock prices and how the lending and underlying stock markets have changed as a result.

security prices. We find consistent results across two independent phases of the experiment, covering two very different market environments.

We also highlight the importance of exogeneity, finding several significant results in the cross-section when we ignore the randomization from the experiment. These results are spurious in that they disappear when we add the withheld stocks as a control group. Hence, inability to adequately control for omitted variables may explain some of the mixed evidence from the existing literature.

In addition to the short sale constraints literature, our results have implications for money managers and policy debates regarding securities lending. Our findings suggest that fund managers can earn meaningful lending fees to enhance their returns without generating adverse effects on the value of their holdings. Other forms of experimentation can be useful in fine-tuning these numbers and addressing the optimal lending quantities across stocks to release to the market. Likewise, policy debates on regulation of securities lending and shorting can be better informed by experiments, where identifiable shocks to demand and supply can be isolated and examined.

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Table I:
Summary Statistics of Stocks Randomly Made Available and Withheld from Lending

Reported are the means (first row) and medians (second row) across a variety of firm characteristics for the sets of high-loan fee stocks randomly made available for lending and randomly withheld from the loan market. Characteristics are measured as of the date of the portfolio holdings used for the randomization (June 30, 2008 for the first phase of the experiment and April 30, 2009 for the second phase). Reported characteristics are size (market cap), percent of shares owned by the manager, market-to-book ratio, institutional ownership (%), short interest (%), expected loan fee (% per year), manager holdings as a percent of prior 30-calendar day average daily trading volume, institutional ownership, and short interest, and potential loan size as a percent of prior 30-day trading volume, institutional ownership, and short interest. We also report the fraction of stocks that have analyst coverage and the analyst earnings forecast dispersion (standard deviation of forecasts divided by the mean forecast) for those stocks (both from IBES). These statistics are computed immediately prior to the lending periods. Finally, we report the fraction of stocks with merger activity (as bidder, none of the stocks are acquired) or with convertible bonds outstanding (from FISD) during the lending periods. The *p*-values from tests for differences in means and medians (assuming independence) between the two groups are also reported.

		First Phase			Second Phase		
		Available	Withheld	p-value of difference	Available	Withheld	p-value of difference
Market cap ($M)	mean	1,367.1	945.2	(0.21)	923.5	1,378.4	(0.28)
	median	985.9	568.2	(0.25)	706.6	863.7	(0.69)
% of shares outstanding owned by manager		5.5	4.8	(0.45)	4.4	3.9	(0.64)
		4.8	4.4	(0.57)	4.0	4.2	(0.69)
Market-to-book ratio		2.8	2.5	(0.75)	3.1	2.8	(0.77)
		2.0	1.5	(0.26)	1.9	2.2	(0.55)
Institutional ownership (%)		82.6	63.7	(0.03)	66.0	76.3	(0.45)
		97.9	61.7	(0.09)	69.4	100.0	(0.23)
Short interest (%)		22.1	15.6	(0.06)	14.3	15.6	(0.68)
		20.1	16.2	(0.02)	13.6	15.7	(0.69)
Expected loan fee (% per year)		7.3	4.0	(0.12)	4.1	1.0	(0.03)
		5.0	1.1	(0.14)	1.8	0.8	(0.04)
Manager holdings as % of 30-day volume		486.4	655.2	(0.38)	389.1	284.2	(0.39)
		370.2	472.5	(0.57)	236.5	197.8	(0.69)
Manager holdings as % of institutional ownership		8.4	11.0	(0.44)	9.9	5.7	(0.11)
		5.9	6.8	(0.57)	5.3	5.5	(0.69)
Manager holdings as % of short interest		42.4	493.0	(0.24)	99.4	29.9	(0.32)
		25.0	28.0	(0.57)	26.2	36.6	(0.23)
Potential loan as % of 30-day volume		229.3	216.7	(0.65)	214.3	182.4	(0.46)
		300.0	300.0	N/A	236.5	188.1	0.7
Potential loan as % of institutional ownership		3.7	4.7	(0.23)	6.9	4.6	(0.34)
		3.9	3.9	(1.00)	4.4	3.8	(0.69)
Potential loan as % of short interest		18.3	84.9	(0.19)	36.8	23.6	(0.40)
		15.3	18.1	(0.57)	22.2	20.5	(0.69)
Analyst coverage		0.56	0.75	(0.18)	0.42	0.67	(0.24)
Analyst forecast dispersion		0.19	0.37	(0.32)	0.90	0.13	(0.44)
		0.04	0.14	(0.14)	0.10	0.05	(0.59)
Merger activity during lending period		0.03	0.05	(0.74)	0.05	0.11	(0.59)
Convertible bond outstanding during lending period		0.19	0.20	(0.91)	0.26	0.22	(0.82)
Number of stocks		32	20		19	9	

Table II:
Summary Statistics of Stocks Lent During the Lending Experiment

Reported are cross-sectional summary statistics on actual lending activity and loan fees for stocks lent in the experiment (32 stocks in the first phase, 19 stocks in the second phase). All stock-level statistics are computed over trading days on which the stock is on loan. Lending period is the number of days on loan during the lending period, and Recall period is the number of days on loan during the recall period, both for the first phase only. Also reported are the stock's average loan fee for each day, and the stock's loan-size weighted average loan fee for each day. Loan fees are expressed in basis points per year. Average daily lent shares / trading volume is the time-series average of the new shares lent each day expressed as a percentage of total trading volume 3 days earlier (reflecting the settlement date lag). Average daily loan market value is the time series average of the dollar amount on loan each day, and Average daily % of market cap on loan is the former quantity expressed as a percentage of the stock's market cap. Max. daily dollar value of shares on loan is the stock-level maximum dollar value of shares on loan during the experiment period, and Max. daily % of market cap on loan is the former quantity expressed as a percentage of market cap. Average daily % of short interest on loan is the time-series average of the number of shares on loan expressed as a percentage of pre-experiment short interest.

	First Phase					Second Phase				
	Mean	Median	Stdev.	Min.	Max.	Mean	Median	Stdev.	Min.	Max.
Loan period (trading days)	12.6	13.5	4.8	1	19	49.2	68.0	68.0	2	82
Lending period (trading days)	6.1	6.0	2.3	1	9					
Recall period (trading days)	6.5	7.0	2.9	0	12					
Average daily loan fee (bps per year)	258.53	80.93	312.10	1.41	976.06	158.79	101.42	133.95	27.25	516.28
Loan size-weighted average daily loan fee (bps per year)	767.62	523.30	784.33	63.35	2,311.12	147.05	100.80	110.06	22.70	412.49
Average daily lent shares/ trading volume (%)	42.57	25.52	56.24	3.22	326.05	70.28	22.92	134.86	1.75	603.86
Average daily loan market value ($M)	11.11	7.33	11.16	0.07	39.09	11.13	6.34	11.98	0.12	38.54
Average daily % of market cap on loan	0.90	0.74	0.76	0.02	2.81	1.06	0.80	1.02	0.03	3.93
Max. daily dollar value of shares on loan ($M)	19.71	11.99	18.66	0.10	75.78	19.79	11.92	20.22	0.12	66.29
Max. daily % of market cap on loan	1.58	1.44	1.21	0.03	5.01	1.74	1.37	1.39	0.04	4.52
Average daily % of short interest on loan	5.02	3.76	3.92	0.12	16.36	8.93	5.50	12.50	0.16	57.72

Table III:
Changes in Loan Fees from the Experiment

Panel A reports loan fee differences-in-differences between available and withheld stocks from the pre-lending to the lending period for both phases of the lending experiment. First phase: pre-lending period Aug. 1 to Sept. 4, 2008 and lending period Sept. 5 to Sept. 17, 2008. Second phase: pre-lending period May 1 to June 4, 2009 and lending period June 5 to Sept. 30, 2009. Four methods are used to measure loan fees: 1) Data Explorers (DE) fee data for all stocks and both periods, 2) Data Explorers fee data for lending period and expected fee reported by lending agent for pre-lending period, 3) actual lending fee during lending period (Data Explorers fee data for withheld stocks) and expected fee from lending agent for pre-lending period, 4) actual lending fee during lending period (Data Explorers data for withheld stocks) and Data Explorers fee data for pre-lending period. Reported difference-in-difference estimates are the mean change in average daily loan fee for the available stocks less the mean change in average daily loan for the withheld stocks. Standard errors of the differences-in-differences are given in brackets. Average Treatment Effect of the Treated (ATT) estimates use nearest neighbor matching based on pre-lending short interest, where each available stock is matched with a withheld stock that has the closest level of short interest before the experiment. Panel B reports cross-sectional regressions of loan fee differences (using definition 4) above) from the pre-period to lending period for each stock on the size of the loan made in each stock, scaled by both the Manager's potential loan size as well as by shares outstanding. In Panel B, all variables are stock-level averages over the respective periods. Regressions are run for available stocks only as well as for all stocks, including those withheld for which actual loan size is zero. A constant is estimated in each specification but not reported. All loan fees are expressed in percent per year. All standard errors are White (1980) heteroskedasticity-consistent and *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Change in Loan Fee (% per year) Differences-in-Differences

	First phase		Second phase	
	Available - Withheld		Available - Withheld	
	Difference-in-Differences estimate	ATT matched (on short interest) estimate	Difference-in-Differences estimate	ATT matched (on short interest) estimate
Lending period fee (from DE) – pre-lending period fee (from DE)	-0.09	-0.13	-2.29*	-2.34**
	[0.45]	[0.48]	[1.13]	[0.76]
Lending period fee (from DE) – expected fee (from lending agent)	-0.70	-0.24	-2.27*	-2.10***
	[1.28]	[1.24]	[1.14]	[0.79]
Actual lending fee – expected fee (from lending agent)	-3.45**	-2.99**	-3.01**	-2.84***
	[1.46]	[1.44]	[1.30]	[0.87]
Actual lending fee – pre-lending period fee (from DE)	-2.84***	-2.88***	-3.03**	-3.08***
	[0.66]	[0.66]	[1.30]	[0.85]

Panel B: Relation between Changes in Loan Fee and Changes in Loan Size

Dependent variable =	Actual lending fee during experiment – pre-period lending fee (% per year)							
	First phase				Second phase			
	Available stocks		All stocks		Available stocks		All stocks	
Actual loan/potential loan (%)	-0.34***		-0.35***		-0.18**		-0.17**	
	[0.12]		[0.09]		[0.07]		[0.06]	
Actual loan/shares outstanding (%)		-3.55		-6.91**		-6.09***		-5.83***
		[3.37]		[2.94]		[1.53]		[1.41]
N	32	32	52	52	19	19	28	28
R-square	0.28	0.03	0.42	0.15	0.50	0.78	0.54	0.79

Table IV:
Changes in Loan Quantities from the Experiment

Panel A reports changes in short interest differences-in-differences between available and withheld stocks from the pre-lending to the lending period for both phases of the lending experiment. Short interest is reported biweekly, and only once during the lending period for the first phase. For the second phase, we take the stock-level average short interest over the lending period. Standard errors of the mean short interest differences-in-differences are given in brackets. Average Treatment Effect of the Treated (ATT) estimates use a nearest neighbor matching sample based on pre-lending expected loan fee (from the lending agent). Panel B reports regressions of the daily number of shares lent market-wide (from Data Explorers) on the Manager's daily lending activity and an indicator variable for whether the stock is available for lending. The Manager's lending activity is zero for withheld stocks. A constant is estimated in each specification but not reported. All loan quantities are expressed as a percentage of shares outstanding. Standard errors are reported in brackets and are clustered by stock. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Changes in Short Interest (% of shares outstanding) Differences-in-Differences

	First phase		**Second phase**	
	Available - Withheld		Available - Withheld	
	Difference-in-Differences estimate	ATT matched (on loan fee) estimate	Difference-in-Differences estimate	ATT matched (on loan fee) estimate
Change in short interest	0.67	0.85	-1.02	2.76**
	[1.52]	[1.47]	[0.91]	[1.38]

Panel B: Relation between Daily Market Lending Quantities and Loans Made by the Manager

	First phase	**Second phase**
Dependent variable =	Market loan quantity (as % of shares outstanding)	
Loan size made by Manager	0.355***	0.661***
(as % of shares outstanding)	[0.111]	[0.010]
Available indicator	0.001	-0.004
	[0.001]	[0.123]
N	468	2286
R-square	0.08	0.05

Table V:
Return Differences between Available and Withheld Stocks over Pre-Lending, Lending, and Recall Periods

Reported are average daily returns (in percent) for portfolios of stocks randomly made available to lend and randomly withheld from lending. Panel A reports results from the first phase of the experiment, where average daily returns are reported over the "pre-lending period" (August 1 to September 4, 2008), the "lending period" (September 5 to September 17, 2008), and the "recall period" (September 18 to October 3, 2008). The differences between the returns of a portfolio of available stocks minus a portfolio of withheld stocks are also reported. Three sets of portfolio weights are used: equal-weighting, value-weighting (by market capitalization at the time of randomization into available and withheld groups), and expected loan fee weighting (using the expected loan fee on each stock at the time of randomization into available and withheld groups). Panel B reports results from the second phase of the experiment, where average daily returns for both portfolios are reported over the "pre-lending period" (May 1 to June 4, 2009), and the "lending period" (June 5 to September 30, 2009). White (1980) heteroskedasticity-consistent standard errors are in brackets, with two-tailed p-values for differences in returns reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Pre-lending period			Lending period			Recall period		
	Available	Withheld	Difference	Available	Withheld	Difference	Available	Withheld	Difference
Panel A: First Phase									
Equal-weight	0.16	0.15	0.01	-0.26	-0.72	0.47	-0.69	-0.27	-0.42
	[0.47]	[0.47]	[0.26]	[0.86]	[1.05]	[0.37]	[1.50]	[1.49]	[0.50]
			(0.98)			(0.24)			(0.42)
Value-weight	0.14	0.01	0.13	-0.19	0.09	-0.28	-0.70	0.01	-0.71
	[0.48]	[0.40]	[0.29]	[0.86]	[0.94]	[0.47]	[1.47]	[1.46]	[0.77]
			(0.67)			(0.57)			(0.38)
Expected loan fee-weight	0.23	-0.09	0.32	0.31	-0.80	1.11***	-0.85	-0.55	-0.29
	[0.52]	[0.60]	[0.41]	[0.84]	[0.93]	[0.28]	[1.59]	[1.58]	[0.49]
			(0.44)			(0.00)			(0.56)
Panel B: Second Phase									
Equal-weight	0.67	0.44	0.23	0.36	0.23	0.13			
	[0.66]	[0.53]	[0.31]	[0.23]	[0.20]	[0.13]			
			(0.46)			(0.35)			
Value-weight	0.53	0.67	-0.14	0.34	0.13	0.21*			
	[0.56]	[0.51]	[0.35]	[0.20]	[0.19]	[0.13]			
			(0.69)			(0.09)			
Expected loan fee-weight	1.59	0.27	1.32*	0.35	0.22	0.13			
	[1.02]	[0.61]	[0.75]	[0.31]	[0.21]	[0.24]			
			(0.09)			(0.59)			

Table VI:
Power of the Tests—Rejection Regions for One-Tailed Tests of Return Differences

Reported are the rejection cutoff values for tests of differences in returns between the available and withheld stocks over both phases of the experiment separately and combined. The first phase contains return differences for the lending and recall periods, while the second phase contains differences for the lending period only. The combined phases only look at return differences for the lending periods. Rejection region cutoffs for one-tailed tests on return differences are reported at the 10%, 5%, and 1% significance levels. For lending period differences, the cutoff value is a lower bound (since the test is for negative return differences), while for the recall period differences the critical value is an upper bound (since the test is for positive return differences). For the combined phases, estimates of the average return difference between available and withheld stocks for each phase are weighted by their precision (inverse of their variance) to produce the most efficient (lowest variance) combined estimate. This combined estimate and its associated standard error are used to compute the reported rejection region cutoffs, which represent the value that we can reject, at the specified level of significance, that the difference between available and withheld stocks over the lending (recall) period is less (greater) than in a one-tailed test. We report critical values for equal-weighted, value-weighted, and expected loan fee-weighted average differences across all phases of the experiment.

		Rejection Cutoff Values for Return Differences (in %)			
		First phase		**Second phase**	**Combined**
	Significance level	Lending (lower bound)	Recall (upper bound)	Lending (lower bound)	Lending (lower bound)
Equal-weight	10%	-0.04	0.26	-0.05	**0.01**
	5%	-0.21	0.48	-0.10	**-0.03**
	1%	-0.60	0.94	-0.19	**-0.12**
Value-weight	10%	-0.94	0.34	0.05	**0.01**
	5%	-1.16	0.67	0.00	**-0.03**
	1%	-1.65	1.38	-0.08	**-0.12**
Expected loan fee-weight	10%	0.72	0.38	-0.18	**0.31**
	5%	0.59	0.59	-0.27	**0.25**
	1%	0.30	1.04	-0.43	**0.12**

Table VII:
Event Study Returns for Available Versus Withheld Stocks

Reported are panel regressions of daily stock returns for various windows during each phase of the experiment. Panel A focuses on the lending period of the first phase, Panel B on the recall period of the first phase, and Panel C on the lending period of the second phase. The windows are 1 trading day (i.e, the first day of lending or recall), 2 trading days, 3 trading days, 5 trading days, and for the second phase 1 month, 2 months, and 3 months. Monthly windows are in calendar time and include all trading days in the calendar window. In each specification, the dependent variable is the daily stock return (in %). The independent variable is an indicator for whether the stock is available for lending. A constant is estimated in each specification but not reported. The reported regression coefficients therefore equal the average daily stock return difference (in percent) between available and withheld stocks over the event window. Analogous to Table V, we report equal-weighted, value-weighted, and expected loan fee-weighted averages. Standard errors (in brackets) are heteroskedasticity-consistent and clustered by trading day (except in the 1 day tests). *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Average daily stock return difference (%) between available and withheld stocks

	Panel A: First Phase Lending Period				**Panel B: First Phase Recall Period**			
Event window:	1 day	2 days	3 days	1 week	1 day	2 days	3 days	1 week
Equal-weight	-0.58	0.36	0.23	0.13	0.08	-0.04	-1.36	-0.32
	[0.75]	[0.95]	[0.56]	[0.33]	[2.33]	[0.12]	[1.33]	[1.02]
Value-weight	-0.38	0.93	0.65	0.23	0.33	-0.61	-2.33	-0.67
	[0.66]	[1.31]	[0.81]	[0.59]	[2.40]	[0.94]	[1.82]	[1.43]
Expected loan fee-weight	-0.05	-0.20	0.23	0.80	0.65	0.75*	-0.40	0.07
	[0.67]	[0.15]	[0.44]	[0.45]	[4.98]	[0.11]	[1.16]	[0.79]

	Panel C: Second Phase Lending Period						
Event window:	1 day	2 days	3 days	1 week	1 month	2 months	3 months
Equal-weight	0.94	-0.40	0.32	-0.03	-0.29	0.08	0.09
	[1.28]	[1.36]	[1.07]	[0.63]	[0.32]	[0.23]	[0.16]
Value-weight	1.74	-0.24	0.37	0.14	0.02	0.19	0.22
	[1.27]	[2.00]	[1.30]	[0.78]	[0.29]	[0.19]	[0.15]
Expected loan fee-weight	0.86	-0.74	0.34	-0.01	-0.55	-0.02	0.08
	[1.28]	[1.61]	[1.43]	[0.91]	[0.54]	[0.37]	[0.29]

Table VIII:
Cross-Sectional Return Regressions

Reported are cross-sectional regression results of returns during the lending and recall periods on a variety of stock characteristics. The dependent variable is the stock-level average daily return (in percent) during the specified period. Each specification focuses on one explanatory variable. Panel A focuses on lending supply and demand measures while Panel B focuses on valuation and disagreement measures. Regressions are run for available stocks only and for all stocks. The latter include an interaction for whether the stock in question is available for lending with the explanatory variable. "Hedging demand indicator" is equal to 1 if the stock had either a convertible outstanding or was involved in a merger during the lending period, and 0 otherwise. All other variables are defined in previous tables. A constant is estimated in each specification but not reported. Standard errors (in brackets) are White (1980) heteroskedasticity-consistent. Coefficients significantly different from zero at the 10%, 5%, and 1% levels are indicated with a *, **, and ***, respectively.

Period:	Lending phase 1		Lending phase 2		Recall phase 1	
Sample:	Available	All	Available	All	Available	All
Panel A: Lending Supply and Demand Measures						
Potential loan/institutional ownership (%)	-0.32	0.11	0.00	-0.02	-0.05	-0.12
	[0.19]	[0.16]	[0.00]	[0.01]	[0.12]	[0.12]
Potential loan/institutional ownership (%) × Available		-0.42*		0.03		0.07
		[0.25]		[0.01]		[0.17]
Available		2.16		0.01		-0.80
		[1.31]		[0.15]		[0.77]
R-square	0.16	0.08	0.00	0.10	0.01	0.06
Short interest (%)	0.05*	0.04	0.00	0.00	-0.03*	-0.04
	[0.03]	[0.05]	[0.00]	[0.01]	[0.02]	[0.02]
Short interest (%) × Available		0.00		0.00		0.00
		[0.05]		[0.01]		[0.03]
Available		0.11		0.10		-0.27
		[1.39]		[0.22]		[0.67]
R-square	0.14	0.09	0.02	0.08	0.11	0.12
Expected loan fee (%)	0.05*	-0.01	-0.00	-0.04	-0.01	-0.04
	[0.03]	[0.12]	[0.00]	[0.10]	[0.02]	[0.07]
Expected loan fee (%) × Available		0.06		0.04		0.02
		[0.12]		[0.10]		[0.08]
Available		0.06		0.09		-0.47
		[0.50]		[0.15]		[0.44]
R-square	0.13	0.06	0.00	0.07	0.02	0.04
Hedging demand indicator	-1.24**	-2.56	0.01	0.11	-0.20	-1.63**
	[0.55]	[1.79]	[0.09]	[0.21]	[0.44]	[0.73]
Hedging demand × Available		1.31		-0.10		1.43
		[1.87]		[0.23]		[0.86]
Available		0.10		0.16		-0.78*
		[0.39]		[0.11]		[0.41]
R-square	0.16	0.21	0.00	0.08	0.01	0.15

Period:	Lending phase 1		Lending phase 2		Recall phase 1	
Sample:	Available	All	Available	All	Available	All
Panel B: Valuation and Disagreement Measures						
Market/Book ratio	0.05	-0.19***	0.00	-0.08**	0.03	-0.16
	[0.10]	[0.05]	[0.01]	[0.03]	[0.08]	[0.20]
Market/Book ratio × Available		0.24**		0.08**		0.19
		[0.11]		[0.04]		[0.22]
Available		-0.68		-0.15		-1.17*
		[0.60]		[0.17]		[0.61]
R-square	0.01	0.07	0.00	0.20	0.00	0.10
No analyst coverage indicator	-0.53	1.96**	-0.10	0.20	-0.28	0.47
	[0.46]	[0.78]	[0.11]	[0.20]	[0.38]	[0.63]
No analyst coverage × Available		-2.49***		-0.30		-0.74
		[0.90]		[0.23]		[0.74]
Available		1.19		0.25**		-0.18
		[0.76]		[0.12]		[0.49]
R-square	0.04	0.11	0.06	0.15	0.02	0.04
Analyst forecast dispersion	-0.17	0.68	-0.03	0.68***	1.07***	-1.30*
	[0.28]	[1.35]	[0.02]	[0.13]	[0.24]	[0.64]
Analyst forecast dispersion × Available		-0.85		-0.70***		2.37***
		[1.38]		[0.13]		[0.69]
Available		1.73		0.36**		-0.61
		[1.25]		[0.13]		[0.69]
R-square	0.01	0.13	0.06	0.41	0.35	0.19

Table IX:
Volatility, Skewness, and Bid-Ask Spread Difference-in-Differences between Available and Withheld Stocks

Reported are cross-sectional average differences-in-differences of individual stock volatility, skewness, and bid-ask spreads for stocks randomly made available to lend and randomly withheld from lending from both phases of the experiment. For each stock, volatility and skewness are calculated using daily returns of the stock over the pre-lending period, the lending period, and the recall period separately for both phases of the experiment. Likewise, average bid-ask spreads for each stock are calculated separately over each period and each phase as the time-series average daily closing bid-ask spread as a percentage of the closing price for the stock. All variables are expressed in percent per day. For each stock, we calculate the difference in each of these variables from the pre-period to the lending period (i.e., lending minus pre) and (for the first phase) from the lending period to the recall period (i.e., recall minus lending). We then subtract the cross-sectional average difference for withheld stocks from the cross-sectional average difference for available stocks to produce the difference-in-differences estimates reported in the table. We use three different weighting schemes to compute cross-sectional averages: equal weighting stocks, value-weighting stocks (by market capitalization at the time of randomization), and expected loan fee weighting stocks (using the expected loan fee on each stock at the time of randomization). Two-tailed *p*-values for the difference-in-differences are calculated using White (1980) heteroskedasticity-consistent standard errors and are reported in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels, respectively.

	Volatility differences			**Skewness differences**			**Bid-ask spread differences**		
	First phase		Second phase	First phase		Second phase	First phase		Second phase
	Lending - pre	Recall - lending	Lending - pre	Lending - pre	Recall - lending	Lending - pre	Lending - pre	Recall - lending	Lending - pre
Equal-weight	-0.82	0.50	-0.59	0.09	-0.06	-0.46	-0.04	-0.68**	-0.04
	(0.14)	(0.59)	(0.38)	(0.80)	(0.78)	(0.43)	(0.36)	(0.04)	(0.31)
Value-weight	-0.54	-0.31	-0.48	0.08	0.07	-0.56	0.00	-0.33**	-0.04
	(0.50)	(0.79)	(0.39)	(0.86)	(0.82)	(0.28)	(0.88)	(0.03)	(0.18)
Expected loan fee-weight	-0.07	0.01	-1.15	-0.13	0.48	-0.68	-0.05	-0.96	-0.08
	(0.94)	(1.00)	(0.49)	(0.85)	(0.29)	(0.19)	(0.63)	(0.13)	(0.30)

Table X:
Cross-Sectional Regressions of Changes in Volatility, Skewness, and Bid-Ask Spreads

Reported are results from cross-sectional regressions of changes in stock-level volatility (Panel A), skewness (Panel B), and bid-ask spreads (Panel C) from the pre-period to the lending period of each phase of the experiment, and from the lending period to the recall period for the first phase, on various stock characteristics. The dependent variable is the stock-level change in volatility, skewness, or bid-ask spread, defined in Table IX and expressed in percent per day. Regressions are run for available stocks only and for all stocks, where the latter specification includes an interaction for whether the stock is available for lending with the explanatory variable. All variables are defined in Table VIII. A constant is estimated in each specification but not reported. Standard errors (in brackets) are White (1980) heteroskedasticity-consistent. Coefficient estimates significantly different from zero at the 10%, 5%, and 1% levels are indicated with a *, **, and ***, respectively.

	Panel A: Changes in Volatility					
Period:	Lending phase 1		Lending phase 2		Recall phase 1	
Sample:	Available	All	Available	All	Available	All
Potential loan/institutional ownership (%)	0.17	0.12	-0.01	-0.23**	0.03	-0.29
	[0.14]	[0.14]	[0.05]	[0.08]	[0.23]	[0.19]
Potential loan/institutional ownership (%) × Available		0.05		0.23**		0.31
		[0.20]		[0.10]		[0.29]
Available		-0.90		-1.62*		-0.96
		[0.93]		[0.90]		[1.85]
R-square	0.04	0.08	0.00	0.10	0.00	0.04
Short interest (%)	-0.04***	-0.09**	-0.03	0.02	0.06	0.09
	[0.01]	[0.04]	[0.04]	[0.04]	[0.04]	[0.06]
Short interest (%) × Available		0.05		-0.05		-0.03
		[0.04]		[0.06]		[0.07]
Available		-1.25		0.14		0.60
		[0.83]		[1.24]		[1.58]
R-square	0.10	0.22	0.03	0.06	0.06	0.08
Expected loan fee (%)	0.01	-0.09	-0.13***	-1.30**	0.02	0.10
	[0.03]	[0.09]	[0.03]	[0.48]	[0.07]	[0.11]
Expected loan fee (%) × Available		0.10		1.17**		-0.07
		[0.09]		[0.48]		[0.13]
Available		-1.22*		-1.32		0.72
		[0.62]		[0.81]		[1.01]
R-square	0.00	0.08	0.30	0.29	0.01	0.02
Hedging demand indicator	-0.44	1.39	-1.35**	-0.94	-0.74	0.66
	[0.54]	[1.03]	[0.62]	[1.27]	[0.76]	[2.67]
Hedging demand × Available		-1.83		-0.41		-1.40
		[1.17]		[1.42]		[2.78]
Available		-0.37		-0.48		0.82
		[0.62]		[0.79]		[0.88]
R-square	0.02	0.10	0.21	0.18	0.02	0.02

	Panel B: Changes in Skewness					
Period:	Lending phase 1		Lending phase 2		Recall phase 1	
Sample:	Available	All	Available	All	Available	All
Potential loan/institutional ownership (%)	0.05	-0.20***	0.05	-0.06	-0.08	-0.02
	[0.13]	[0.05]	[0.03]	[0.09]	[0.08]	[0.03]
Potential loan/institutional ownership (%) × Available		0.25*		0.10		-0.06
		[0.14]		[0.10]		[0.09]
Available		-1.06		-1.04		0.15
		[0.67]		[0.96]		[0.39]
R-square	0.01	0.12	0.17	0.12	0.03	0.02
Short interest (%)	-0.00	-0.02	-0.01	-0.03	0.00	-0.00
	[0.01]	[0.02]	[0.02]	[0.04]	[0.02]	[0.01]
Short interest (%) × Available		0.02		0.02		0.01
		[0.02]		[0.05]		[0.02]
Available		-0.19		-0.74		-0.21
		[0.61]		[0.84]		[0.44]
R-square	0.00	0.02	0.01	0.04	0.00	0.01
Expected loan fee (%)	-0.02	-0.01	-0.03	-0.16	0.02	-0.05**
	[0.02]	[0.07]	[0.03]	[0.69]	[0.02]	[0.02]
Expected loan fee (%) × Available		-0.02		0.12		0.06**
		[0.07]		[0.69]		[0.03]
Available		0.24		-0.47		-0.36
		[0.44]		[1.22]		[0.24]
R-square	0.04	0.02	0.04	0.05	0.03	0.07
Hedging demand indicator	0.38	0.22	-0.32	-1.16	-0.16	-0.07
	[0.65]	[0.75]	[0.40]	[0.87]	[0.30]	[0.30]
Hedging demand × Available		0.16		0.84		-0.09
		[0.99]		[0.97]		[0.43]
Available		0.06		-0.75		-0.04
		[0.37]		[0.79]		[0.24]
R-square	0.02	0.01	0.02	0.11	0.01	0.01

	Panel C: Changes in Bid-Ask Spreads					
Period:	Lending phase 1		Lending phase 2		Recall phase 1	
Sample:	Available	All	Available	All	Available	All
Potential loan/institutional ownership (%)	-0.002	-0.006	-0.004*	-0.011***	0.006	-0.054
	[0.010]	[0.010]	[0.002]	[0.002]	[0.103]	[0.069]
Potential loan/institutional ownership (%) × Available		0.004		0.007**		0.060
		[0.014]		[0.003]		[0.124]
Available		-0.063		-0.067		-0.957
		[0.101]		[0.046]		[0.725]
R-square	0.00	0.03	0.05	0.09	0.00	0.10
Short interest (%)	0.001	-0.007*	-0.002	0.002	-0.013**	-0.036**
	[0.002]	[0.004]	[0.003]	[0.002]	[0.006]	[0.015]
Short interest (%) × Available		0.008*		-0.005		0.023
		[0.004]		[0.004]		[0.016]
Available		-0.171		0.028		-0.960**
		[0.104]		[0.074]		[0.412]
R-square	0.01	0.13	0.02	0.04	0.02	0.16
Expected loan fee (%)	0.002	0.003	-0.007**	-0.035	-0.010	0.023
	[0.002]	[0.008]	[0.003]	[0.024]	[0.007]	[0.039]
Expected loan fee (%) × Available		-0.002		0.028		-0.033
		[0.009]		[0.024]		[0.039]
Available		-0.042		-0.047		-0.515
		[0.034]		[0.051]		[0.367]
R-square	0.02	0.03	0.06	0.09	0.01	0.10
Hedging demand indicator	-0.000	0.184	-0.112*	0.043	-0.276	0.680
	[0.063]	[0.151]	[0.060]	[0.029]	[0.251]	[0.684]
Hedging demand × Available		-0.184		-0.155**		-0.955
		[0.164]		[0.068]		[0.729]
Available		0.003		0.007		-0.449
		[0.027]		[0.056]		[0.359]
R-square	0.00	0.14	0.11	0.13	0.02	0.13

Appendix

In this appendix, we report robustness tests and supplementary figures. We begin with the robustness tests described in Section IV. G. of the paper. We report results for differences in returns, and differences-in-differences for volatility, skewness, and bid-ask spreads, across various permutations of our sample for the available and withheld groups for both phases of the experiment.

Table A-1 reports results for all available vs. all withheld revenue stocks. This means all 40 available vs. 20 withheld for the first phase and 23 available vs. 9 withheld for the second phase. Table A-II reports results removing the high-revenue stocks that the manager requested we make available. There were three such stocks for the first phase and one for the second, meaning we compare 37 available vs. 20 withheld for the first phase and 22 available vs. 9 withheld for the second phase. Table A-III reports results removing all stocks whose loan fees declined to less than 25 bps before the experiment. Relative to the sample in the paper, this results in removing 3 withheld stocks in the first phase and 1 in the second phase, so we compare 32 available vs. 17 withheld for the first phase and 19 available vs. 8 withheld for the second phase. Table A-IV reports results removing both the high-revenue stocks that the manager requested we make available and the withheld stocks whose lending fees declined below 25 bps, resulting in 29 available vs. 17 withheld stocks for the first phase and 18 available vs. 8 withheld for the second phase. The bottom line is that the results are qualitatively unaffected by these slight changes in sample choice. We consistently find no adverse effects on share prices, volatility, skewness, or bid-ask spreads from shocks to loan supply.

Figure A-1 presents distributions of various firm characteristics for the revenue stocks made available for lending and withheld from lending in the first phase of the experiment. The figure shows that the randomization process succeeds in selecting available and withheld samples with similar characteristics. Figure A-2 presents the same distributions for the non-revenue stocks in the first phase of the experiment. Once again, characteristics are similar across the available and withheld groups. Although the Manager did not lend out the low demand stocks, the lack of meaningful differences between the two groups provides additional support for the robustness of our experimental design. Figure A-3 presents the analogue of Figure A-1 for the second phase of the experiment. The only significant difference between the available and withheld groups is that the stocks made available for lending

have a higher expected loan fee, which is primarily due to the one stock that the Manager specifically requested we make available. As discussed above, results are similar if we exclude this stock.

Figure A-4 presents a scatter plot of the stock-level changes in daily return volatility from the pre-lending period to the lending period for each phase of the experiment (i.e., lending period volatility minus pre-lending period volatility).

Table A-I:
Return Differences, and Volatility, Skewness, and Bid-Ask Spread Differences-in-Differences between Available and Withheld Stocks over Lending and Recall Periods

First phase sample: 40 available, 20 withheld.
Second phase sample: 23 available, 9 withheld.

This table reports results for all available vs. all withheld revenue stocks. Panel A reports return differences, and volatility, skewness, and bid-ask spread differences-in-differences (all in percent), between available and withheld stocks for the first phase of the experiment. Panel B reports analogous results for the second phase. Heteroskedasticity-consistent standard errors are reported in brackets, with two-tailed p-values reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Lending	**Lending-Pre**			**Recall**	**Recall-Lending**		
	Returns	Volatility	Skewness	Bid-ask	Returns	Volatility	Skewness	Bid-ask
				Panel A: First Phase				
Equal-weight	0.45	-0.68	0.12	-0.05	-0.46	0.42	-0.03	-0.60*
	[0.30]	[0.54]	[0.34]	[0.05]	[0.47]	[0.89]	[0.21]	[0.31]
	(0.17)	(0.21)	(0.73)	(0.25)	(0.34)	(0.64)	(0.88)	(0.06)
Value-weight	0.36	0.57	0.16	-0.02	-2.59**	0.24	-1.07	-0.39***
	[0.79]	[1.50]	[0.38]	[0.03]	[1.06]	[2.05]	[0.73]	[0.15]
	(0.66)	(0.71)	(0.67)	(0.40)	(0.03)	(0.91)	(0.15)	(0.01)
Expected loan fee-weight	1.10***	-0.08	-0.12	-0.05	-0.29	-0.01	0.49	-0.95
	[0.27]	[1.03]	[0.68]	[0.10]	[0.50]	[1.99]	[0.44]	[0.62]
	(0.00)	(0.94)	(0.86)	(0.62)	(0.57)	(1.00)	(0.27)	(0.13)
				Panel B: Second Phase				
Equal-weight	0.10	-0.60	-0.55	-0.05				
	[0.13]	[0.64]	[0.56]	[0.04]				
	(0.44)	(0.36)	(0.33)	(0.17)				
Value-weight	-0.10	0.10	-0.69*	-0.02				
	[0.18]	[0.48]	[0.39]	[0.03]				
	(0.58)	(0.83)	(0.09)	(0.47)				
Expected loan fee-weight	0.11	-1.02	-0.72	-0.08				
	[0.22]	[1.49]	[0.47]	[0.07]				
	(0.61)	(0.50)	(0.13)	(0.29)				

Table A-II:
Return Differences, and Volatility, Skewness, and Bid-Ask Spread Differences-in-Differences between Available and Withheld Stocks over Lending and Recall Periods

First phase sample: 37 available, 20 withheld.
Second phase sample: 22 available, 9 withheld.

This table reports results for available vs. withheld stocks, removing the three (first phase) and one (second phase) high loan fee stocks that the Manager requested we make available. Panel A reports return differences, and volatility, skewness, and bid-ask spread differences-in-differences (all in percent), between available and withheld stocks for the first phase of the experiment. Panel B reports analogous results for the second phase. Heteroskedasticity-consistent standard errors are reported in brackets, with two-tailed p-values reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Lending	**Lending-Pre**			**Recall**	**Recall-Lending**		
	Returns	Volatility	Skewness	Bid-ask	Returns	Volatility	Skewness	Bid-ask
				Panel A: First Phase				
Equal-weight	0.40	-0.75	0.13	-0.05	-0.45	0.44	-0.06	-0.56*
	[0.34]	[0.55]	[0.35]	[0.05]	[0.47]	[0.89]	[0.21]	[0.32]
	(0.28)	(0.18)	(0.71)	(0.27)	(0.36)	(0.62)	(0.77)	(0.08)
Value-weight	0.36	0.56	0.15	-0.02	-2.71**	0.32	-1.14	-0.39***
	[0.83]	[1.58]	[0.39]	[0.03]	[1.15]	[2.16]	[0.78]	[0.15]
	(0.68)	(0.72)	(0.70)	(0.40)	(0.04)	(0.88)	(0.15)	(0.01)
Expected loan fee-weight	0.84**	-0.56	-0.20	-0.03	-0.37	0.35	0.35	-0.81
	[0.31]	[0.99]	[0.70]	[0.11]	[0.57]	[1.93]	[0.34]	[0.65]
	(0.03)	(0.57)	(0.78)	(0.79)	(0.52)	(0.85)	(0.31)	(0.21)
				Panel B: Second Phase				
Equal-weight	0.10	-0.47	-0.51	-0.05				
	[0.12]	[0.64]	[0.56]	[0.04]				
	(0.40)	(0.47)	(0.37)	(0.23)				
Value-weight	-0.10	0.12	-0.69*	-0.02				
	[0.18]	[0.48]	[0.39]	[0.03]				
	(0.57)	(0.80)	(0.09)	(0.49)				
Expected loan fee-weight	0.14	-0.27	-0.42	-0.05				
	[0.14]	[0.96]	[0.41]	[0.06]				
	(0.31)	(0.78)	(0.31)	(0.40)				

Table A-III:
Return Differences, and Volatility, Skewness, and Bid-Ask Spread Differences-in-Differences between Available and Withheld Stocks over Lending and Recall Periods

First phase sample: 32 available, 17 withheld.
Second phase sample: 19 available, 8 withheld.

This table reports results for available vs. withheld stocks, removing all stocks whose loan fees declined to less than 25 basis points before the beginning of the respective phase of the experiment. Panel A reports return differences, and volatility, skewness, and bid-ask spread differences-in-differences (all in percent), between available and withheld stocks for the first phase of the experiment. Panel B reports analogous results for the second phase. Heteroskedasticity-consistent standard errors are reported in brackets, with two-tailed p-values reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Lending	**Lending-Pre**			**Recall**	**Recall-Lending**		
	Returns	Volatility	Skewness	Bid-ask	Returns	Volatility	Skewness	Bid-ask
				Panel A: First Phase				
Equal-weight	0.41	-0.66	-0.00	-0.05	-0.41	0.28	-0.04	-0.50
	[0.36]	[0.58]	[0.39]	[0.05]	[0.54]	[1.04]	[0.21]	[0.31]
	(0.28)	(0.26)	(1.00)	(0.32)	(0.47)	(0.79)	(0.84)	(0.11)
Value-weight	-0.35	-0.52	0.08	0.00	-0.76	-0.39	0.06	-0.24*
	[0.52]	[0.84]	[0.49]	[0.03]	[0.79]	[1.23]	[0.32]	[0.13]
	(0.52)	(0.54)	(0.87)	(0.95)	(0.36)	(0.75)	(0.84)	(0.07)
Expected loan fee-weight	1.10***	-0.04	-0.14	-0.05	-0.29	-0.04	0.48	-0.93
	[0.28]	[1.07]	[0.70]	[0.10]	[0.50]	[2.03]	[0.45]	[0.63]
	(0.00)	(0.97)	(0.85)	(0.62)	(0.57)	(0.99)	(0.28)	(0.14)
				Panel B: Second Phase				
Equal-weight	0.16	-0.37	-0.39	-0.04				
	[0.13]	[0.69]	[0.63]	[0.04]				
	(0.23)	(0.60)	(0.54)	(0.32)				
Value-weight	0.22*	-0.46	-0.55	-0.04				
	[0.13]	[0.54]	[0.51]	[0.03]				
	(0.09)	(0.40)	(0.29)	(0.18)				
Expected loan fee-weight	0.15	-0.94	-0.62	-0.08				
	[0.23]	[1.65]	[0.52]	[0.08]				
	(0.51)	(0.57)	(0.24)	(0.31)				

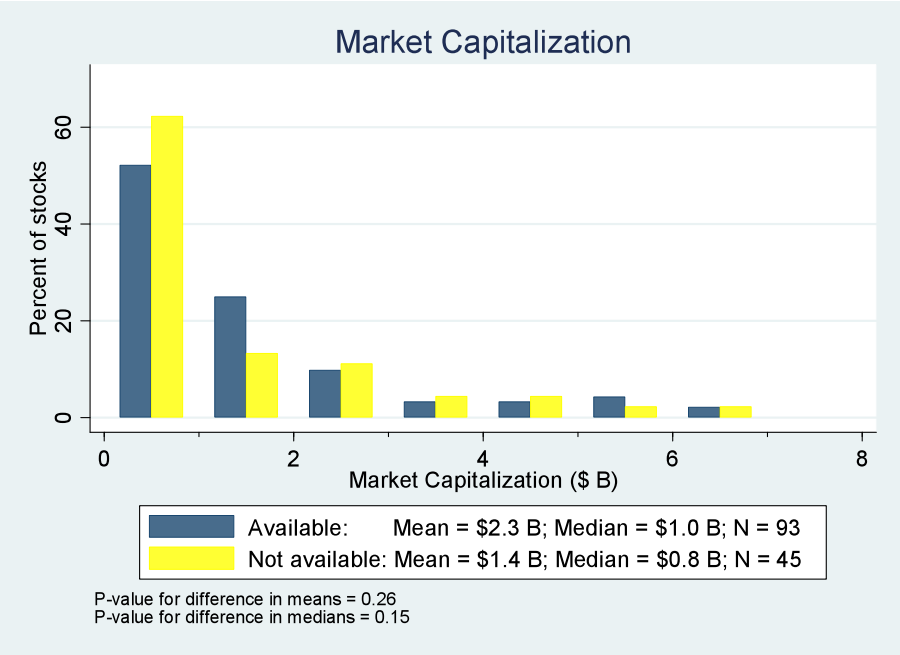
Table A-IV:
Return Differences, and Volatility, Skewness, and Bid-Ask Spread Differences-in-Differences between Available and Withheld Stocks over Lending and Recall Periods

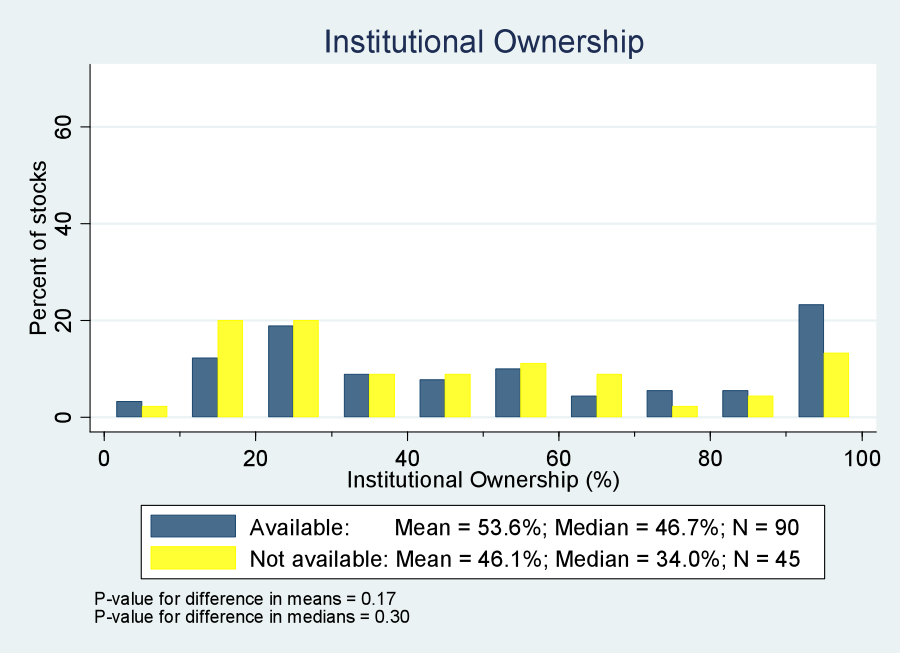
First phase sample: 29 available, 17 withheld.
Second phase sample: 18 available, 8 withheld.

This table reports results for available vs. withheld stocks, excluding both high loan fee stocks that the Manager requested we make available and all stocks whose loan fees declined to less than 25 basis points before the beginning of the respective phase of the experiment. Panel A reports return differences, and volatility, skewness, and bid-ask spread differences-in-differences (all in percent), between available and withheld stocks for the first phase of the experiment. Panel B reports analogous results for the second phase. Heteroskedasticity-consistent standard errors are reported in brackets, with two-tailed p-values reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Lending	**Lending-Pre**			**Recall**	**Recall-Lending**		
	Returns	Volatility	Skewness	Bid-ask	Returns	Volatility	Skewness	Bid-ask
				Panel A: First Phase				
Equal-weight	0.34	-0.76	0.01	-0.05	-0.40	0.31	-0.08	-0.47
	[0.44]	[0.59]	[0.40]	[0.05]	[0.55]	[1.04]	[0.21]	[0.32]
	(0.45)	(0.20)	(0.98)	(0.34)	(0.48)	(0.77)	(0.69)	(0.15)
Value-weight	-0.50	-0.79	0.02	0.01	-0.69	-0.26	0.09	-0.21
	[0.61]	[0.83]	[0.53]	[0.04]	[0.76]	[1.25]	[0.35]	[0.13]
	(0.44)	(0.35)	(0.96)	(0.84)	(0.38)	(0.84)	(0.79)	(0.13)
Expected loan fee-weight	0.84**	-0.54	-0.22	-0.03	-0.38	0.35	0.34	-0.80
	[0.32]	[1.02]	[0.73]	[0.11]	[0.56]	[1.96]	[0.35]	[0.66]
	(0.03)	(0.60)	(0.76)	(0.80)	(0.51)	(0.86)	(0.34)	(0.23)
				Panel B: Second Phase				
Equal-weight	0.16	-0.21	-0.33	-0.04				
	[0.12]	[0.68]	[0.63]	[0.04]				
	(0.19)	(0.76)	(0.60)	(0.42)				
Value-weight	0.22*	-0.42	-0.54	-0.04				
	[0.13]	[0.54]	[0.51]	[0.03]				
	(0.09)	(0.45)	(0.30)	(0.20)				
Expected loan fee-weight	0.20	-0.10	-0.24	-0.05				
	[0.15]	[1.05]	[0.45]	[0.07]				
	(0.19)	(0.93)	(0.60)	(0.45)				

Figure A-1: Distribution of revenue stocks characteristics after randomization, first phase of experiment.





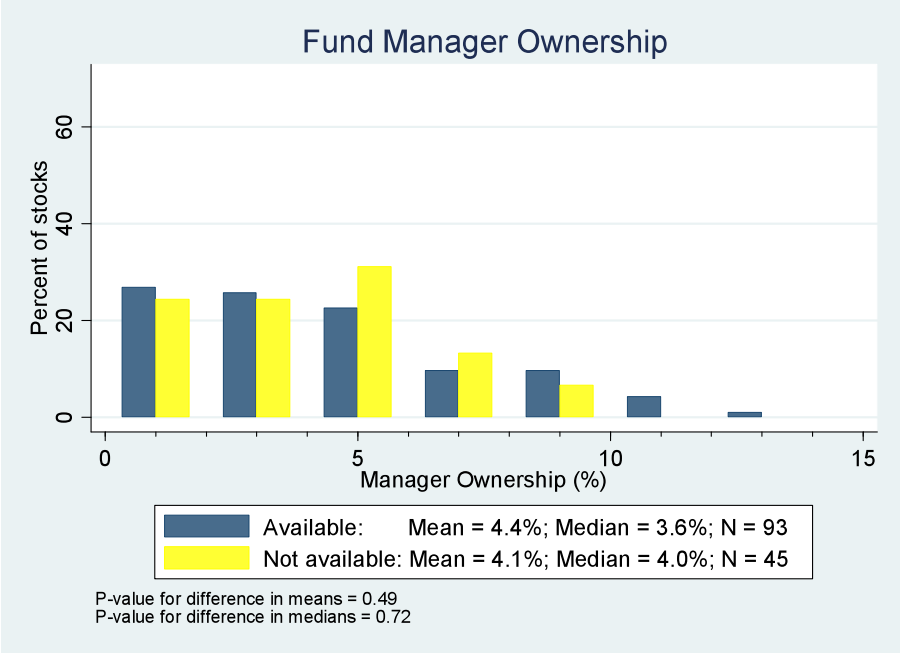
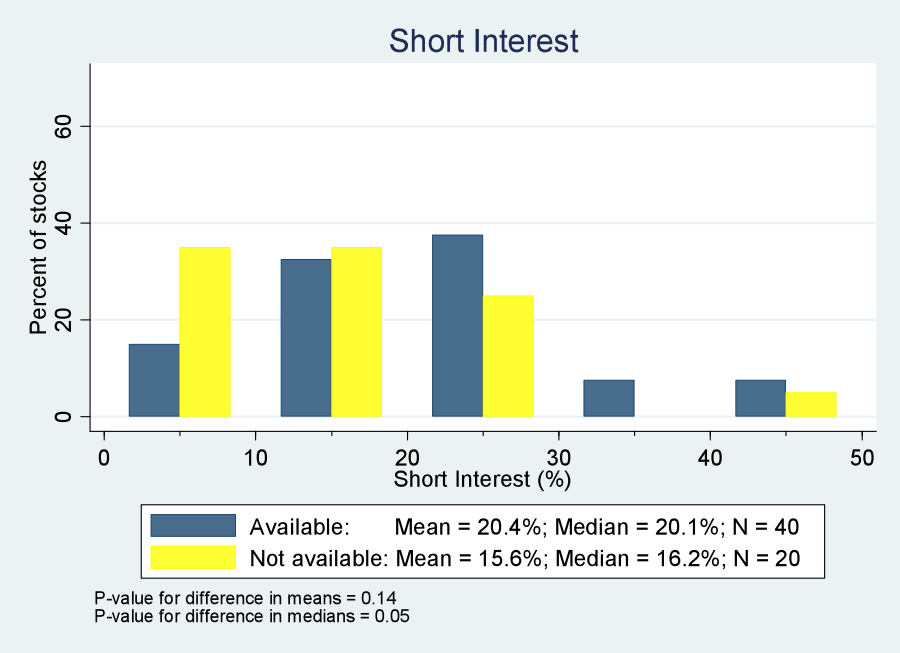
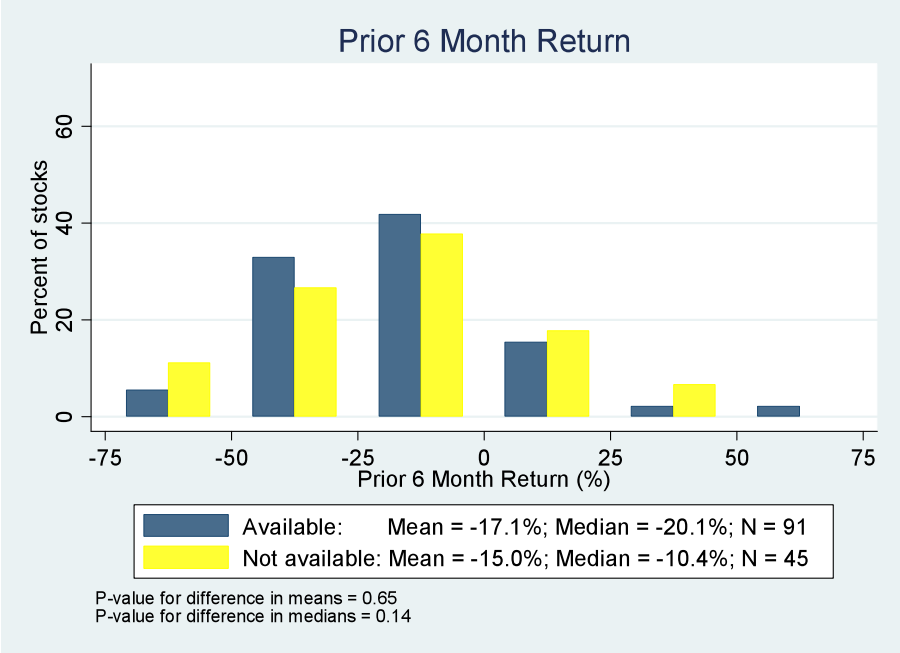
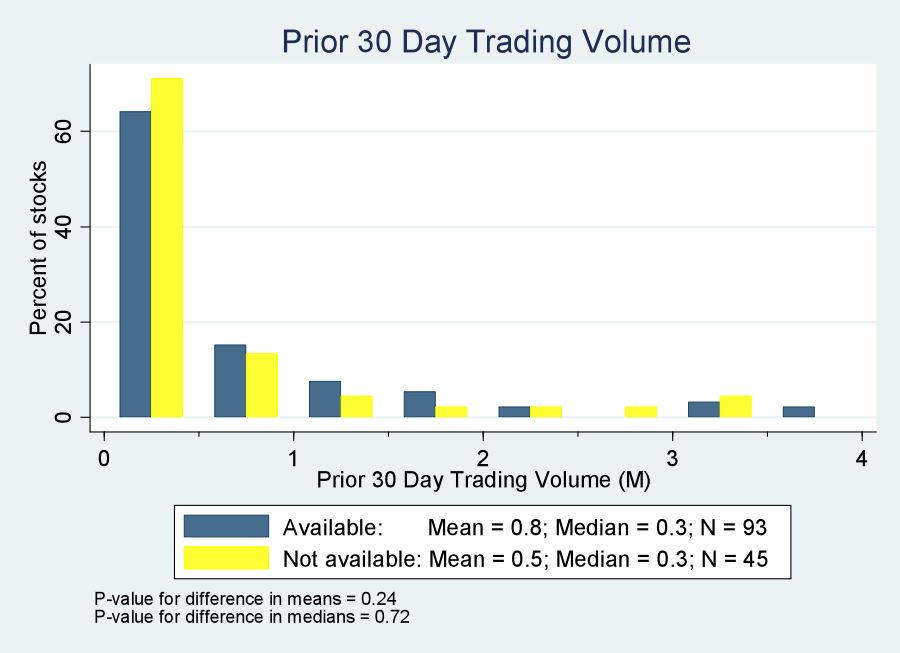


Figure A-1 (continued): Distribution of revenue stocks characteristics after randomization, first phase of experiment.







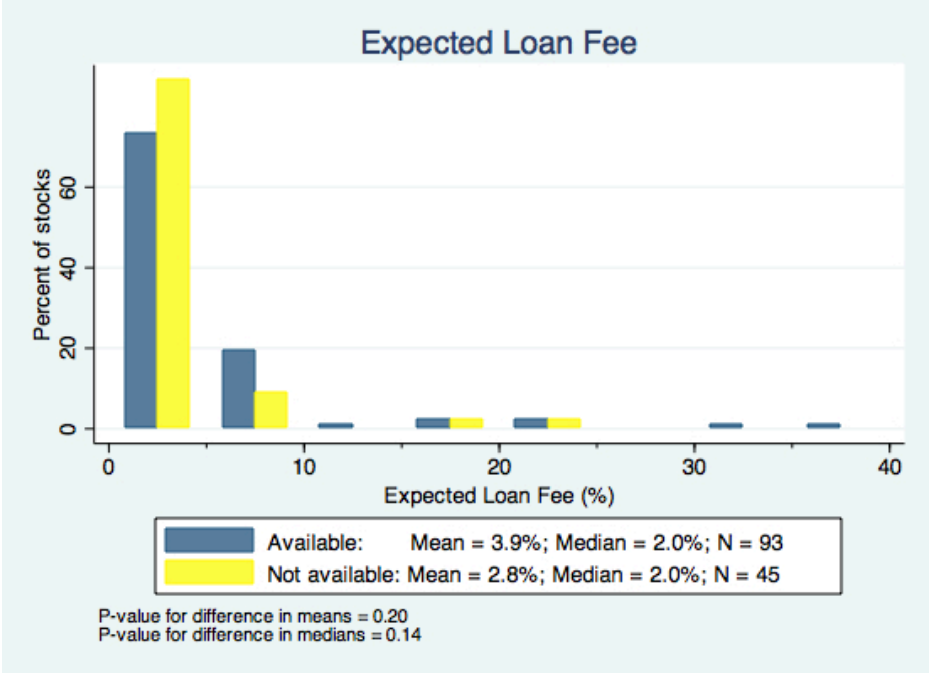
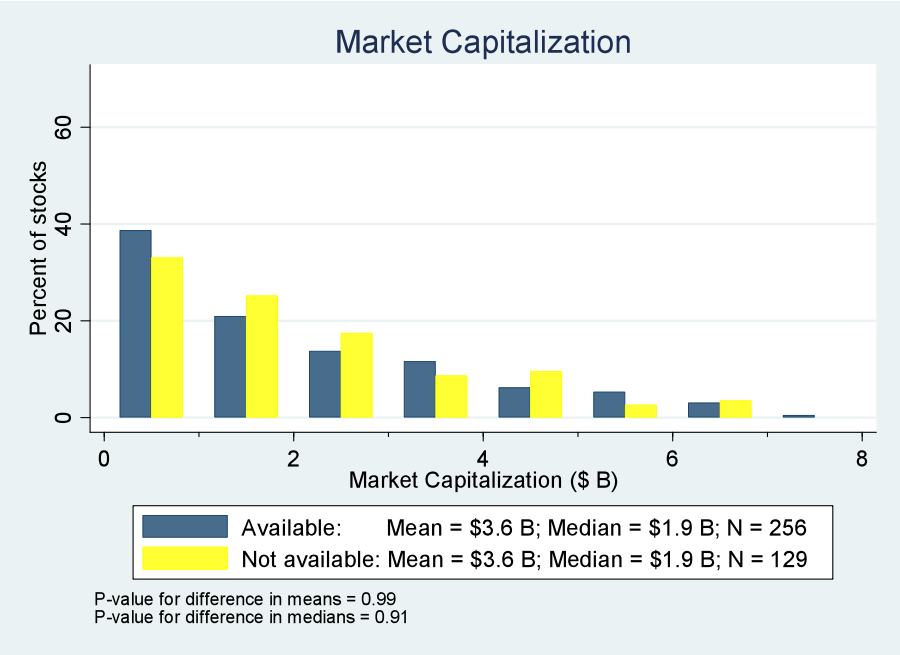
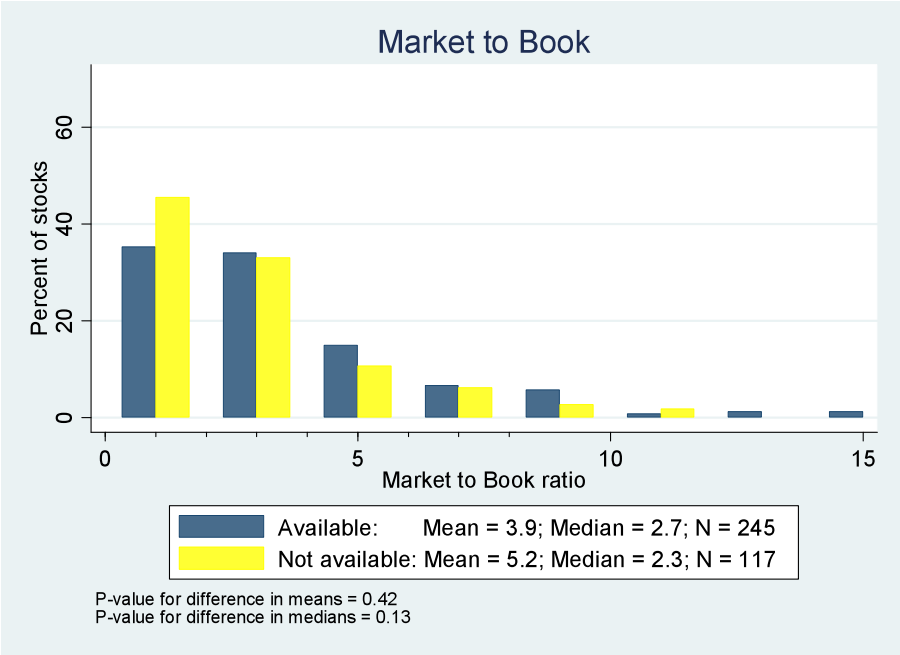
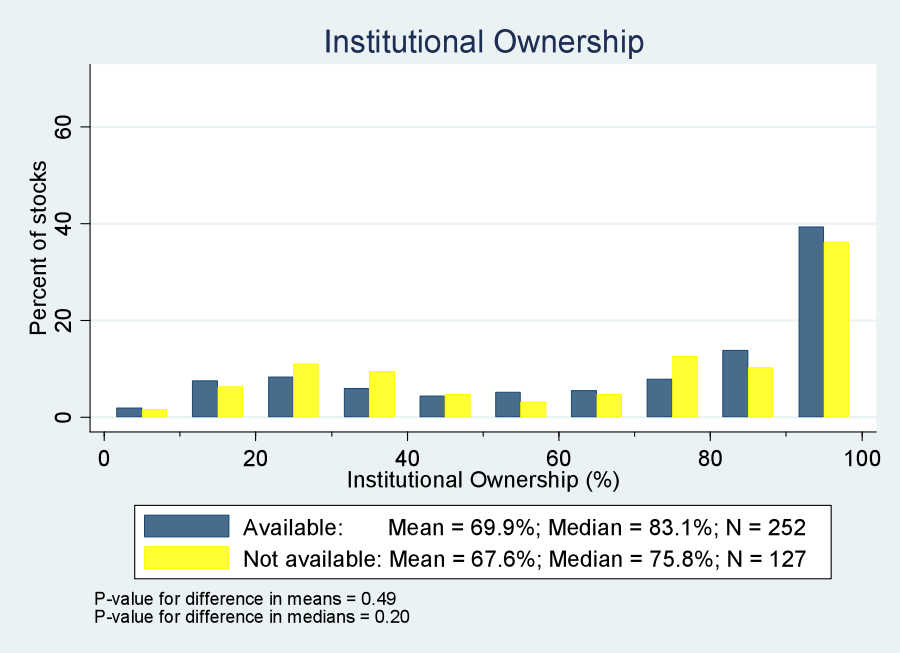


Figure A-2: Distribution of non-revenue stocks characteristics after randomization, first phase of experiment.







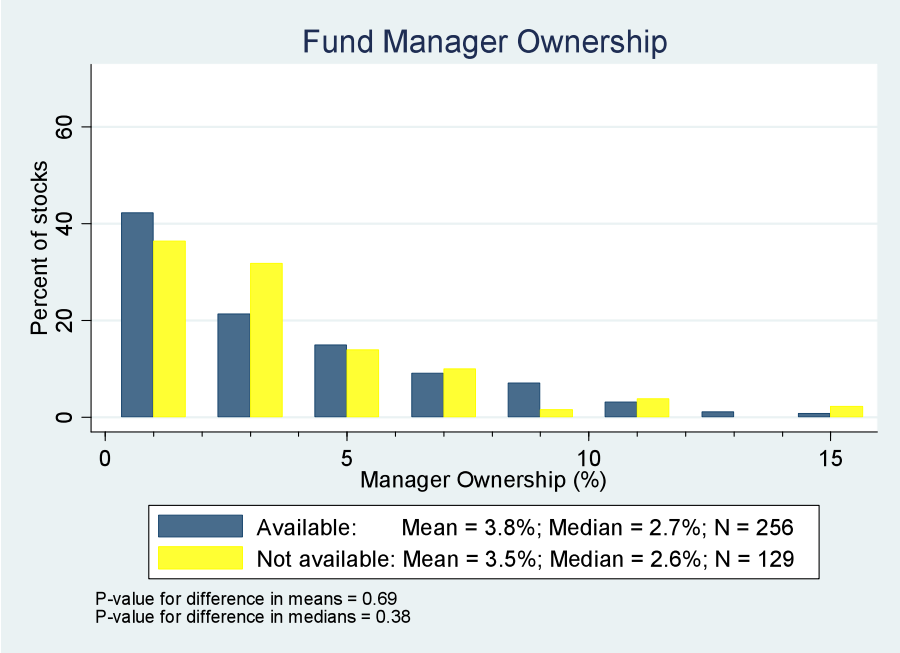
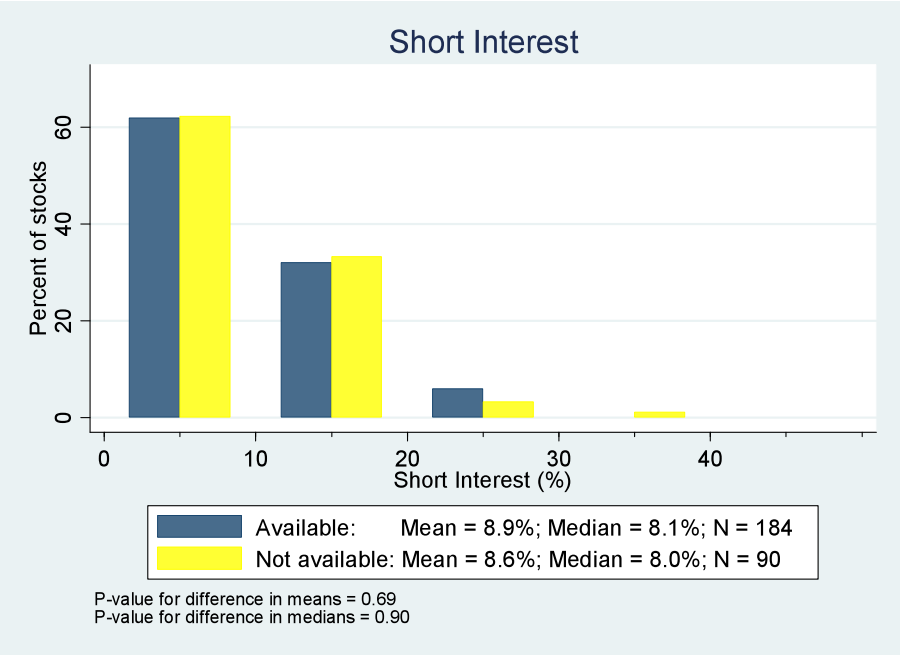
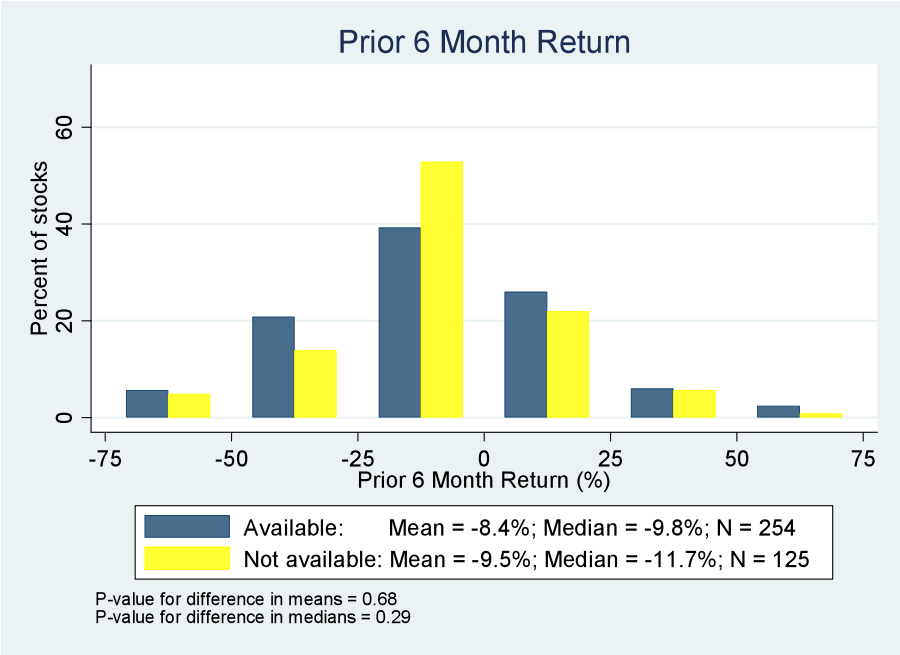


Figure A-2 (continued): Distribution of non-revenue stocks characteristics after randomization, first phase of experiment.





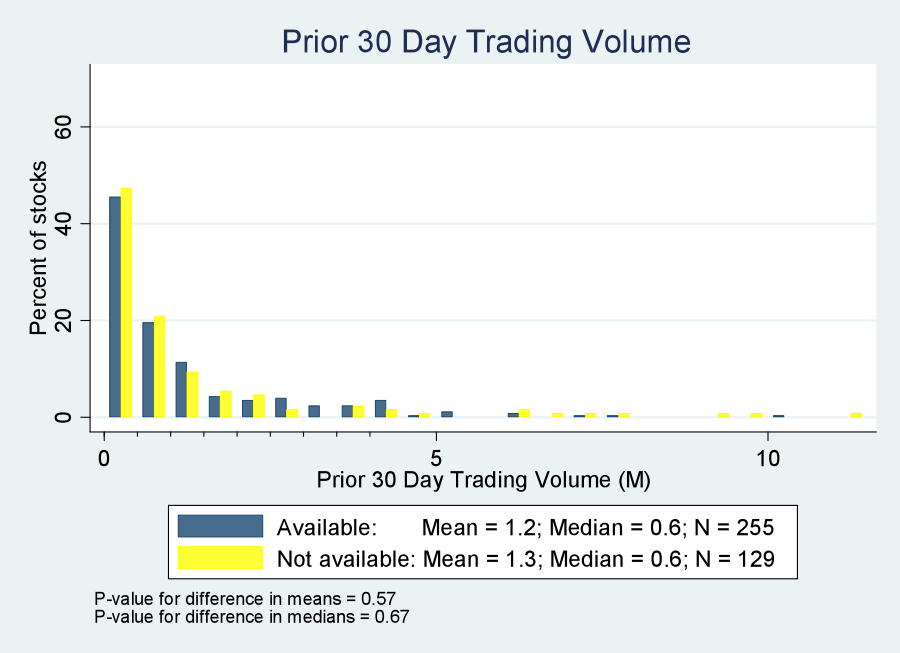
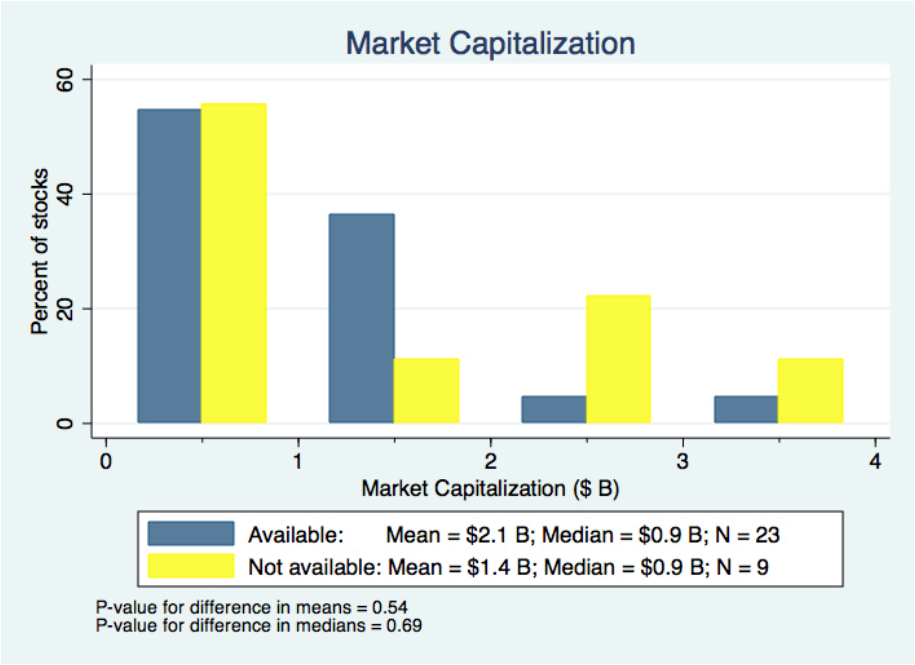
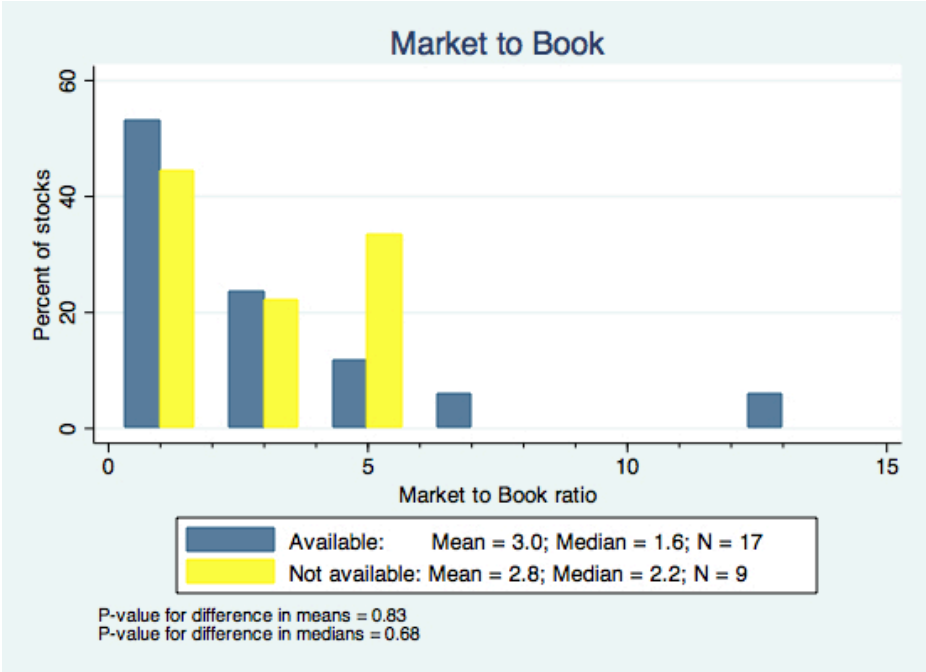
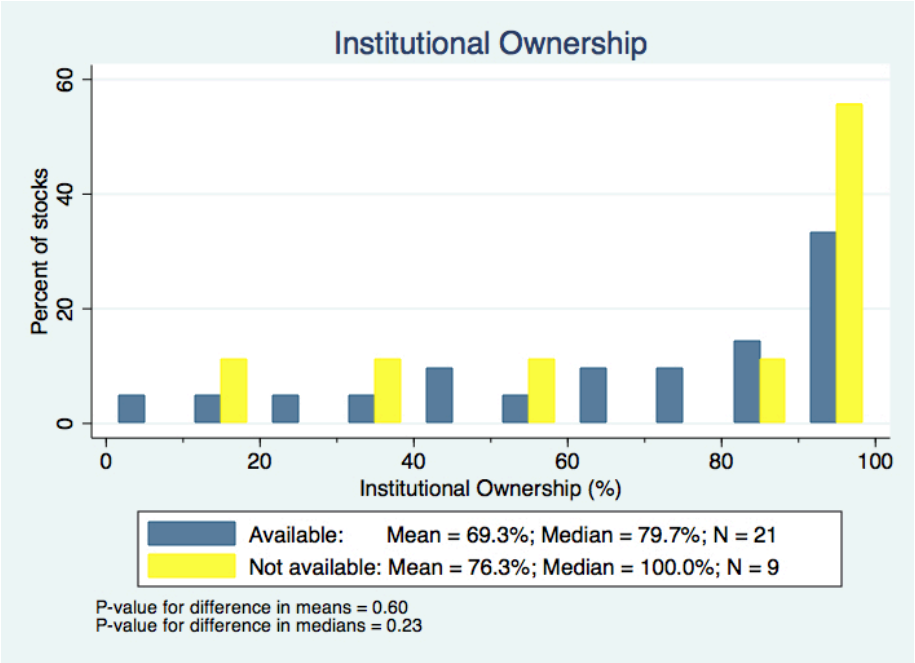


Figure A-3: Distribution of revenue stocks characteristics after randomization, second phase of experiment.







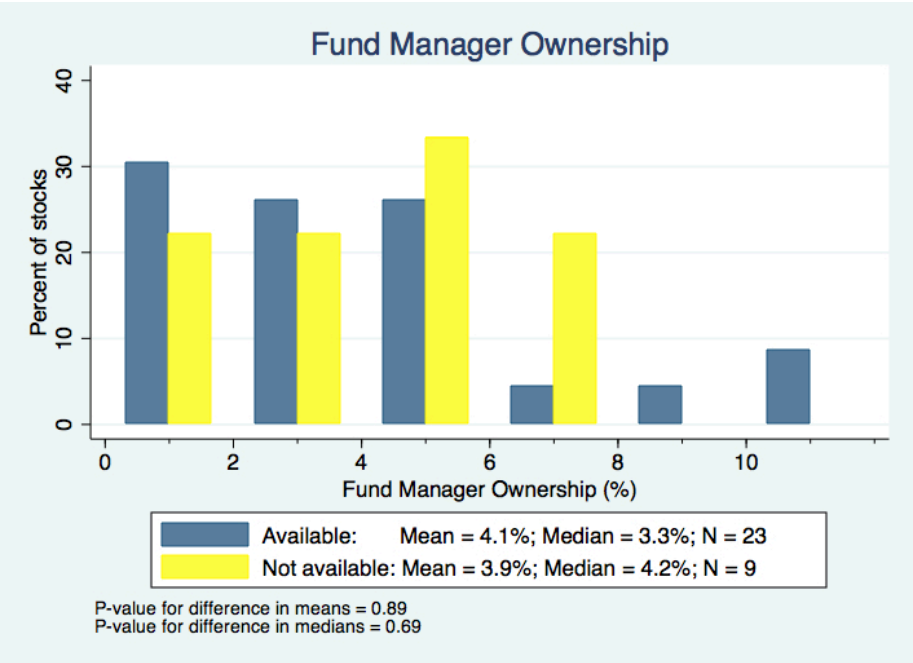
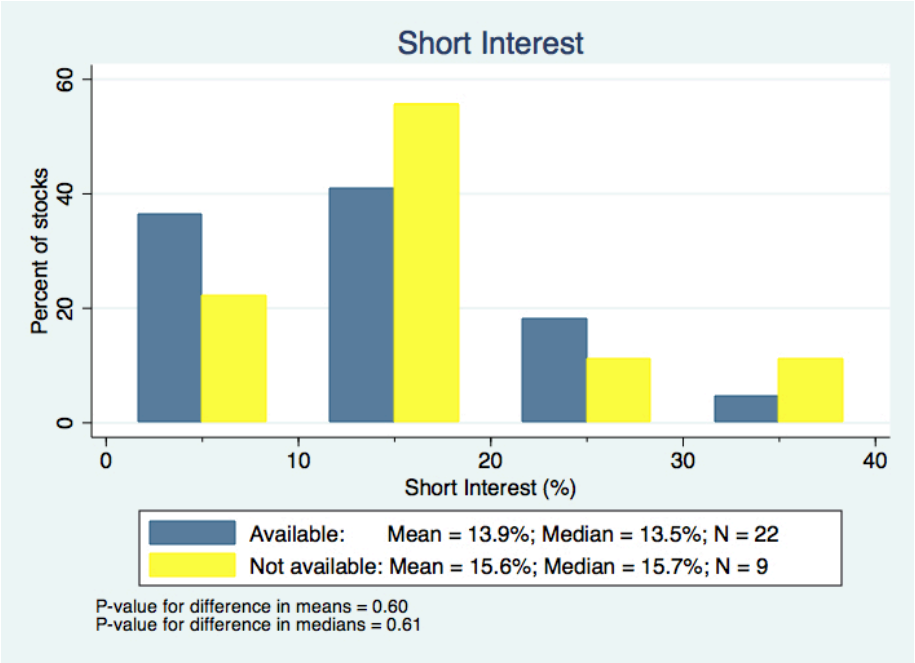
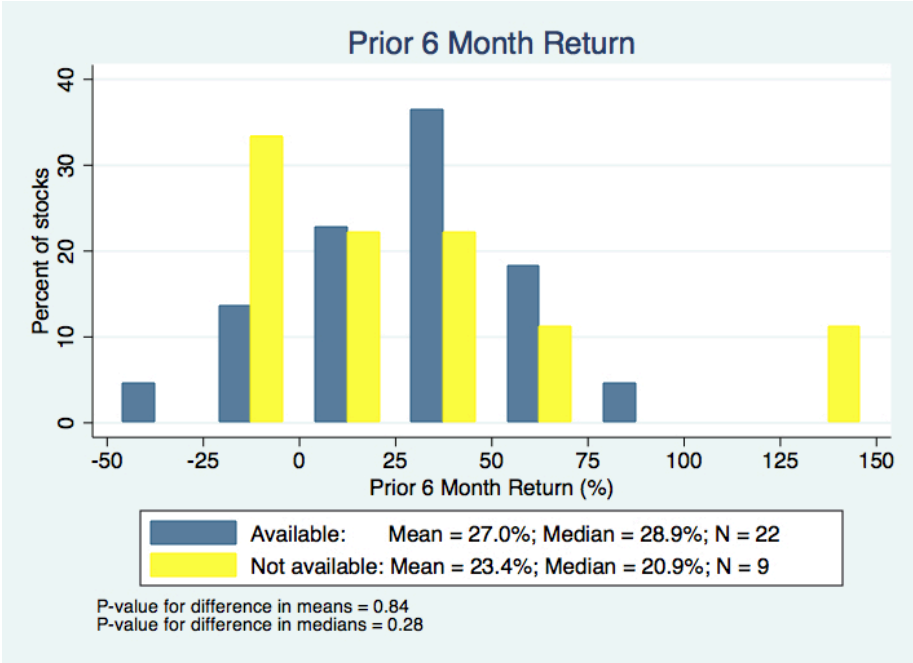
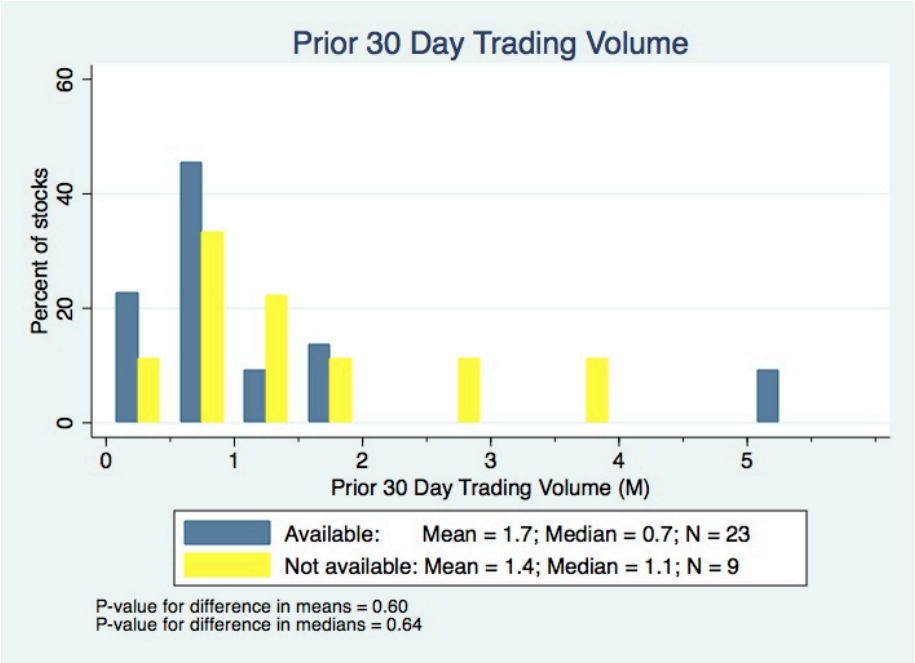


Figure A-3 (continued): Distribution of revenue stocks characteristics after randomization, second phase of experiment.







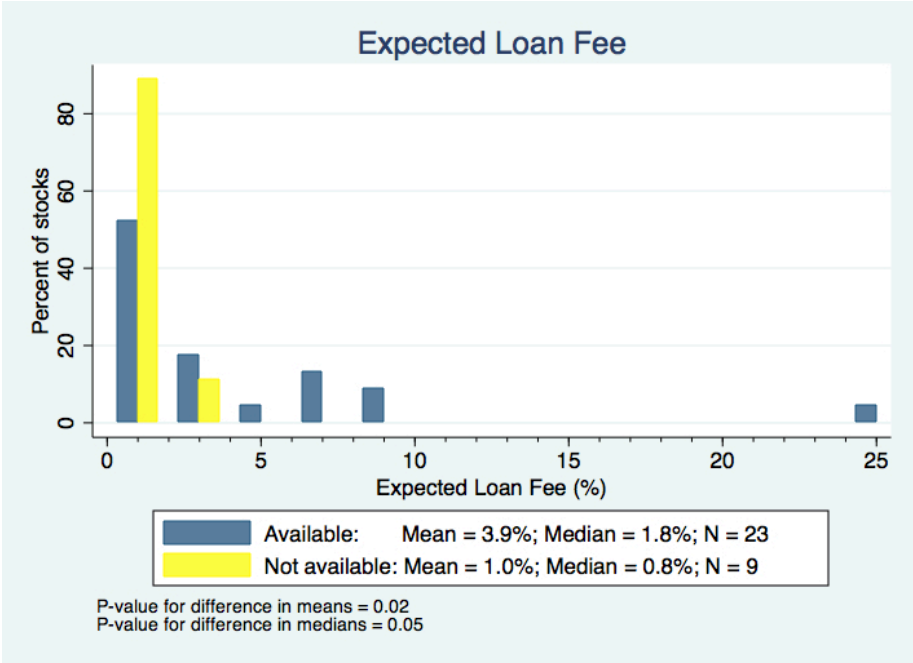


Figure A-4: Scatterplot of changes in daily volatility from pre-lending period to lending period.

Each scatter point is one stock's change in volatility. The color-coding shows first phase, available stocks in blue, first phase, withheld stocks in red, second phase, available stocks in green, and second phase, withheld stocks in orange.