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THE EFFECTS OF STOCK LENDING ON SECURITY PRICES: AN EXPERIMENT

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ABSTRACT

Working with a sizeable, anonymous money manager, we randomly make available for lending two-thirds of the high-loan fee stocks in the manager's portfolio and withhold the other third to produce an exogenous shock to loan supply. We implement the lending experiment 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. The supply shocks are sizeable and significantly reduce lending fees, but returns, volatility, skewness, and bid-ask spreads remain unaffected. Results are consistent across both phases of the experiment and indicate no adverse effects from securities lending on stock prices.

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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 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 economic 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 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 ambiguous, 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 institutions' marginal cost of lending, which may be related to other factors, including demand for shorting.

In this paper, we conduct an experiment in which we randomly move the supply of shares available for lending, thereby exogenously shifting the supply of lendable assets. Working with a sizeable (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 and withhold a characteristic-matched random sample of the other third of high loan-fee stocks owned by the Manager.

[1] See Lamont (2004) for some choice quotes about short sellers. One example is in 1989 when Congress held hearings about the evils of short selling. During the hearings, Congressman Dennis Hastert (later speaker of the US House of Representatives), described short selling as "blatant thuggery".

Our experiment compares stocks *randomly* made available for lending to those randomly withheld from lending to identify shocks to supply holding demand and other factors constant.

We restrict the lending experiment to stocks with high loan fees – expected loan fees of at least 25 basis points (0.25%), with an average of 7.3% and 4.1% for the first and second phases, respectively). These are stocks that have high shorting demand relative to their supply. Theoretical work by Duffie (1996) and Duffie, Garleanu, and Pedersen (2002) suggests that the effects of shorting constraints are nonlinear and mostly affect stocks whose supply of lendable shares is restricted relative to demand.[2] According to these theories, our sample of high-loan fee stocks should experience larger than average effects from supply shocks.

We implement the lending program in two phases. The first commences on September 5, 2008, with the loans being recalled on September 18, 2008 due to market conditions,[3] and the last recalled security returned on October 3, 2008. 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 negative supply shock.

The second phase of the experiment commences on June 5, 2009 with a new set of high-loan fee stocks from the Manager's portfolio. On October 1, 2009, the Manager lifted the restriction on lending the withheld stocks, eliminating the control group from that date forward. 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. The second phase provides an independent positive supply shock over a significantly longer time period under less extreme market conditions. It also takes place after the introduction of SEC Rule 204T, which imposes new restrictions on broker dealers designed to limit naked shorting.[4] While each phase of the experiment on its own has moderate statistical power, jointly the two phases of the experiment afford us substantial statistical power.

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[2] Consistent with this implication, Kolasinski, Reed, and Ringgenberg (2010) find that proxies for search costs are highest for stocks in greatest demand, and loan fees only respond to demand shocks among stocks in high demand.

[3] In particular, doubts that arose concerning the financial stability of significant financial intermediaries. A number of institutions in addition to the Manager, suspended their securities lending programs.

[4] If naked shorting is unrestricted, securities lending is not required for short selling to occur.

The increase to the supply of shares available for lending from the experiment is substantial. 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 capitalization of the stocks on average and as much as 5% of total market capitalization. Furthermore, we find that average loan fees decline significantly (on the order of 2 to 3%) 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, we do not find any adverse effects on stock prices from loan supply shocks. During the lending periods, raw and risk-adjusted returns to stocks available for lending are not lower than returns to stocks withheld from lending. Similarly, during the recall period, returns to recalled stocks are not greater than returns to stocks withheld from lending. These results are inconsistent with Miller's (1977) overvaluation hypothesis. Examining changes in volatility, skewness, and bid-ask spreads of stocks available for lending versus those withheld from the lending market, we similarly find no adverse effects on average.

In addition to examining average effects, we also examine the cross-section of stocks to see if certain kinds of stocks are more or less affected by changes in the supply of lendable shares. Across a variety of firm characteristics, including the amount of shares lent, we find no evidence that the returns of stocks are affected by loan supply shocks.

To highlight the importance of our experimental design for drawing inferences about short selling restrictions, we also conduct the same cross-sectional analysis without implementing the exogenous supply treatment. We find that several characteristics are spuriously related to returns and volatility in both the lending and recall periods. These relationships do not exist when we compare the effects to the control group, highlighting the importance of the exogenous experimental design.

Our general lack of results on the moments of returns and bid-ask spreads of the stocks whose shares experience an exogenous supply shock suggests that supply restrictions to shorting may not be an important factor for asset pricing. There are reasons to be both cautious and aggressive in this interpretation. On the side of caution, the sample period for the first phase of the experiment is short and unusual. However, on the aggressive side, the tremendous uncertainty and volatility of markets during the first phase and our focus on high fee stocks should bias the results toward finding a large 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.

Our findings also have implications for a money manager's decision to lend its shares. We estimate that the total revenue from lending, based on our experiment's results, can increase the returns per dollar invested in these stocks by 1.5 to 2% per year, without adversely affecting stock prices. Our findings may also inform policy debates on securities lending and short selling and suggest that regulation designed to restrict loan supply may not be effective or useful.

The paper proceeds as follows. Section II presents a discussion of the existing research on the impact of shorting constraints. Section III details our sample and research 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 theoretical implications of the impact of short sale constraints on asset prices are ambiguous. Miller (1977) posits that the combination of differences of opinion and short sale constraints can lead to overpricing, where stock prices overweight the views of optimists. In contrast, Diamond and Verrecchia (1987) 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.[5]

A key empirical issue is how to measure short sale constraints. One strand of the literature uses direct measures of the cost of shorting such as the rebate rate or the spread between the rebate rate and market interest rates. The rebate rate is the fee that the lender of the stock pays to the borrower on the collateral the borrower leaves with the lender to borrow the shares. The spread between the rebate rate and cash interest rate is a direct cost to the short-seller, or revenue to the lender, known as the "loan fee."

The existing 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, and Jones and Lamont, 2002.) Another strand of the empirical literature focuses on short interest as a proxy for shorting demand. The results are also mixed. (See Desai et al., 2002 for a summary.)

One of the difficulties in interpreting the results from using direct costs of shorting as a measure of shorting constraints or short interest as a proxy for shorting demand is that both the cost of shorting (price) and short interest (quantity) are determined in equilibrium — the intersection of supply and demand. For example, a high level of short interest could either mean high demand for shorting or low cost of shorting. Given the simultaneity of these measures, it is difficult to identify the exact mechanism that causes or fails to cause the observed movement in stock prices. This is likely one of the main reasons the results in this literature are inconclusive.[6]

Some papers try to mitigate the endogeneity issue by examining changes to the market for a stock's shares that indirectly move supply or demand (and are assumed to be otherwise unrelated to direct

[5]The extent to which shorting has an effect on prices might depend on what fraction of overall trading activity is represented by short sales. Recent empirical evidence shows that short selling comprises a fairly large fraction of trading volume. Boehmer, Jones, and Zhang (2008) find that short sales represent 13 percent of NYSE (SuperDOT) share volume from 2000 to 2004. Diether, Lee, and Werner (2009a) find that short sales represent 31 percent of share volume for Nasdaq-listed stocks and 24 percent of share volume for NYSE-listed stocks in 2005.

[6] 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 only about 30% of mutual funds are allowed by their charters to sell short and only 2% actually do. Using this fact, Chen, Hong, and Stein (2002) use breadth of mutual fund ownership as a proxy for shorting supply and Nagel (2005) uses residual institutional ownership as a proxy for shorting demand. They find some mild evidence of overpricing for small, growth firms. However, institutional ownership is also the outcome of supply and demand and hence it is not clear which channel—shorting demand or shorting supply—drives these results.

supply or demand for the stock's shares). For example, 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.[7]

Cohen, Diether, and Malloy (2007) use data on loan fees and loan amounts to identify cases where a shift to shorting demand or a shift to shorting supply clearly occurs. For example, when prices (loan fees) and quantities (loan amounts) both increase, an upward shift in demand must have occurred, and when prices and quantities both decrease, an upward shift in supply must have occurred. However, 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. Hence, the magnitude of the shift in demand or supply is unknown, making interpretation of the size of the impact on prices difficult. The authors find significant price responses associated with demand shifts, but no price responses associated with supply shifts, consistent with our results from exogenous supply shocks.

Several studies use changes in 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. However, they note that it is not possible to know whether the short sale eligibility designations are endogenous. Their results are mixed. While they find large negative returns (on the order of -5%) around the two weeks after stocks become eligible for short sales, they do not find any significant announcement effect when the Exchange announces which stocks will become eligible for shorting.

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. The poor performance of financial firms leading up to the ban continued after the ban was in place. Bris,

[7] Mayhew and Mihov (2005) also find no evidence that investors take disproportionately negative positions in newly listed options, which suggests that the introduction of options has no causal link to the relaxation of short sale constraints.

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 by allowing negative news to be more quickly incorporated into prices.

Diether, Lee, and Werner (2009b) study the effects of short sale price tests (the uptick rule for the NYSE and the bid price test for NASDAQ) that constrain the ability to short. In conjunction with the SEC, they randomly suspend these tests for a third of the stocks (treated stocks) in the Russell 3000 while maintaining the tests for the remaining two-thirds (control stocks). They find that short-selling activity increases for the treated stocks, but daily returns and volatility are unaffected, consistent with the findings from our experiment.

Several key differences between their experiment and ours also highlight the robustness of the results. First, the SEC-imposed price tests only restrict shorting for a very brief period of time (possibly minutes) 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 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 supply constraints should be most binding. 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.[8] Despite these differences in experimental design, samples, and sample periods, the results from both studies are consistent—supply shocks to the shorting market do not have any detectable adverse pricing or spread effects.

One concern with using regulatory changes to identify shorting constraints, however, is that policies may also reflect information about the market, regulator's incentives, and/or may signal the

[8] To give a sense of these differences, the average daily level of the VIX (obtained from Bloomberg) over the sample period studied by Diether, Lee, and Werner (2009b) 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%.

government's willingness to intercede in markets, all of which may affect stock prices. Our randomized experiment generates exogenous changes to loan supply that are immune from these confounding factors.

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 invests 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 would 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, as many competing managers do. 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*

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, based on lending demand for the stock and prevailing fees at the time, projected would have a loan fee of at least 10 basis points. There were 138 stocks in this group. We refer to these stocks as high demand or "revenue stocks". The remaining group of 385 stocks we refer to as low demand or "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 randomize within the Manager's holdings

at the time, we control for any stock selection ability on the part of the Manager 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 or characteristics will difference out from the randomization. Hence, our experiment controls for any effect (observable or unobservable) related to the Manager. There is one exception to the randomization, however. We made sure to lend the three stocks in the revenue stock group with the highest expected revenue in order to reduce the opportunity cost of the experiment. We include these three stocks in our analysis, but our results are qualitatively and quantitatively the same if we exclude them.[9]

Figure 1 presents distributions of the following firm characteristics across the revenue stocks randomly made available for lending and randomly withheld: equity market capitalization, market-to-book ratio, fraction of available (non-insider) shares owned by institutions, fraction of shares outstanding held by the Manager, short interest as a fraction of shares outstanding, previous six month returns, average prior 30 day trading volume, and expected loan fee. Means, medians, and *p*-values for tests of the difference between the two groups are also reported.

For the revenue (high short demand) stocks, Figure 1 shows that 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, previous six month 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), for the two groups. Unobservable differences between the two groups should be negligible because of the random assignment of stocks to the groups. We interpret the experiment as an exogenous supply shock to the lendable shares of the treated group (those randomly made available for lending), holding everything else constant by comparing to the control (withheld) group.

Figure 2 shows the same distributional comparisons across firm characteristics for the available and withheld stocks among the non-revenue (or low shorting demand) stocks. We omit expected loan fee

[9] These results, and those for the other permutations of our sample described below, are reported in the Appendix.

because they are all close to zero for non-revenue stocks by definition. The plots and statistics in Figure 2 show that the same randomization process generates no significant differences between the available and unavailable groups among the non-revenue stocks as well. 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.

Figures 1 and 2 also allow us to compare the revenue and non-revenue stocks along observable characteristics. The revenue stocks are slightly smaller and have lower institutional ownership than the non-revenue stocks. The Manager owns a similar (and substantial) percentage of both sets of stocks, owning an average 4% and as much as 15% of the total shares outstanding of these firms. Not surprisingly, the revenue stocks have higher short interest (another proxy for demand), lower past 6-month returns, and slightly lower prior 30-day trading volume. By construction, revenue stocks have large and positive expected loan fees while the non-revenue stocks do not.

B. 2. Second phase of the experiment

The Manager made shares available for lending in the second phase of the experiment on June 5, 2009. We selected the sample based on the Manager's stockholdings as of April 30, 2009. The Manager intended to lend, and asked us to conduct the randomization on, the Manager's U.S. stocks with expected loan fees greater than 25 basis points (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.

Analogous to Figure 1 for the first phase, Figure 3 compares the available to withheld stocks 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.

C. *The Sample*

The Manager placed three restrictions on the shares to be lent out. First, the experiment included only shares traded on U.S. exchanges. Second, the experiment was restricted to shares that were in high demand at the time the lending program began (i.e., revenue stocks with gross loan spreads of at least 25 basis points at the start of lending). Third, the loan size was restricted to the lesser of (1) three times the average daily trading volume (based upon the past 30 days trading activity) or (2) 5% of the outstanding shares of the issuer of the security.

Applying the first two restrictions to the 93 revenue stocks that were randomly made available for lending in the first phase reduced the sample to 40 stocks. Among the 45 revenue stocks that were randomly withheld from the lending market in the first phase, 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.

Because the experiment randomly selects stocks among the Manager's existing holdings *before* the experiment, the supply shock is not confounded by omitted factors related to the Manager. However, the additional restrictions placed by the Manager on the potential loan size may be related to other factors that could also influence our outcome variables. This depends on why these restrictions are placed and whether or not they are binding. The additional restrictions on loan size are likely driven by concerns of price impact and liquidity based on the Manager's trading experience. Consequently, if these restrictions bind, they might introduce a non-random component to the experimental design. However, we find that these restrictions rarely bind. We regress the potential loan amount for each stock 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). The regression indicates that most of the variation in potential loan amounts is driven by the Manager's ownership share and not the additional restrictions.

For the first phase of the experiment, the daily trading volume restriction enters significantly in the regression, but explains only a small fraction of the variation in potential loan amount relative to ownership share. The 5% shares outstanding restriction does not have any effect on loan size. For the second phase of the experiment, neither the trading volume or shares outstanding restrictions significantly affect potential loan size. Conducting the same regressions using the actual loan amounts made during the two phases of the experiment, we also find that most of the variation comes from the Manager's ownership share and not the additional restrictions. For brevity, we do not tabulate these regressions.

At the start of the lending program for the first phase, eight of the 40 stocks made available for lending on the basis of expected loan fees at the time of the randomization were no longer eligible for lending due to insufficient loan fees. Their actual loan fees in the marketplace had declined to less than 25 basis points. Hence, the primary sample we study for the first phase of the experiment consists of 32 revenue stocks that experienced a supply shock and 20 revenue stocks randomly withheld from the lending market.[10]

None of our results are affected if we consider all 40 stocks—there are no significant differences in the outcome variables we study between the 32 treated stocks and the 8 stocks that experienced a decline in loan fees that made them ineligible for lending. Likewise, of the 20 randomly withheld stocks, three had their loan fees fall below 25 basis points as well. Once again, we find no difference in any of our results between using the full 20 withheld firms or the 17 who did not experience a loan fee decline below 25 basis points.

Similarly, at the start of the lending period for the second phase of the experiment, four of the 23 stocks available for lending no longer had market loan fees of at least 25 basis points and so were excluded from lending. Hence, the primary sample we study for the second phase of the experiment consists of 19 revenue stocks that experienced a supply shock and 9 revenue stocks randomly withheld

[10] In September and October 2008, the SEC restricted short sales of financial institutions. None of the 60 stocks in the sample was subject to these restrictions.

from the lending market. Again, using all 23 available stocks instead of the 19 does not alter any of our findings.

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 mean 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 of the experiment, Table I shows no significant differences between the two groups in terms of firm characteristics, trading activity, the Manager's ownership of shares, or shorting demand and potential loan supply. The only statistically significant differences between the two groups are that the stocks available for lending have slightly higher mean institutional ownership than the stocks withheld (but no median differences in institutional ownership), and the available stocks have marginally higher short interest. For the second phase of the experiment, the only significant difference between the two groups is that stocks made available for lending have significantly higher expected loan fees than stocks withheld. None of these differences are the result of including the additional high revenue stocks in the available group.

Because the only significant differences in stock characteristics from the first phase are not significantly different in the second phase, and vice versa, these differences appear to be consistent with random variation. But, even if these differences are not random, any bias should make it *more* likely we detect an adverse effect from stock lending, because, if anything, the differences, such as higher expected loan fees, indicate more shorting demand relative to supply for the available stocks.

We also consider several other attributes of the stocks that may be related to variation in shorting demand: dividend payments (short positions must cover any dividend payments, thus increasing the cost of shorting), options traded on the stocks (which can be used to create synthetic short positions), and convertible bond and merger activity (which may induce hedging demand for shorting). We find little variation in these attributes among our sample of stocks. Fewer than half of the stocks from both phases of the experiment pay dividends; the average dividend yield is less than 2% over the first phase, and less

than 3% over the second phase (with the slightly higher second phase yield entirely due to lower market prices). Moreover, we find no difference in dividend yields between the available and withheld stocks during either phase of the experiment. Likewise, we find no discernable difference in options trading across the two groups, and while most of the stocks in our sample have traded options, they are highly illiquid. Convertible bond and merger activity is also almost non-existent for our stocks. While it would be interesting to examine the interaction of these attributes with our supply shocks, our sample of firms does not exhibit enough variation along any of these dimensions to conduct any meaningful tests.

The statistics in Table I suggest 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 investment focus on small- and mid-cap stocks. The Manager also owns a meaningful fraction of these firms, averaging 4-5% of total shares outstanding. The stocks in the sample appear to be primarily growth stocks, with a mean market-to-book ratio of about 3.0. The stocks are also majority owned by institutional investors. Both the stocks made available and not available for lending have meaningful 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 figures 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.

In terms of the potential loan size, the potential loan is about double the average daily trading volume of the stocks for both phases of the experiment. The potential loan is 3.7% of institutional ownership for the first phase and 6.9% for the second phase (for available stocks). The potential loan represents 18.3% of pre-experiment short interest for the first phase and 36.8% for the second phase (for available stocks). These statistics suggest that the Manager's holdings and the lending program represent meaningful changes to the supply of lendable shares.

Before reporting the results of the experiment, we consider how independent the samples are in the two phases of the experiment. First, the overlap in stocks between the two phases is small. 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 that are common to both phases of the experiment, we find zero correlation in returns over the two lending periods.

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 out. On September 18, 2008, the Manager asked the lending agent to call the loans back in. The last shares were returned on October 3, 2008. We examine the effects on stocks lent versus those randomly withheld during the "lending period" (September 5 through September 17), where the stocks available for lending experience an exogenous supply increase, and "recall period" (September 18 through October 3), where supply decreases.

It also turns out that during the first phase of our experiment the supply shock we created was likely the *only* large supply shift of lendable shares hitting the market at this time. With the help of data provided by Rizova (2010), who examines securities lending participation and revenue from data manually collected from mutual fund annual reports published on the SEC Edgar website, we find that only one mutual fund company changed its participation and its supply of shares to the loan market during our experiment—our Manager. While we cannot rule out that other institutions or large investors besides mutual funds altered their participation during this period, it seems likely our experiment was the only major supply shock.

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 (based in part on the results of the experiment). We examine the effects on stocks lent out versus those randomly withheld during this "lending period" (June 5, 2009 through September 30, 2009).[11]

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. In addition, 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) are mitigated by the fact that one of the strongest motives for recalling shares is to vote at annual meetings which predominantly take place in April and May and are outside of both phases of our experiment.

E. *Supplemental Loan Fee Data*

From the experiment itself, we obtain expected loan fees provided by the lending agent and the actual loan fees received by the Manager for the stocks randomly made available for lending. We supplement these data with loan fees from Data Explorers for both phases of our experiment. Data Explorers is a leading global provider of securities lending and financing data. They obtain the data from a variety of lending agents that include investment banks and prime brokers. Lending agents, hedge funds, beneficial owners, investment banks, and prime brokers use the data to get a sense of what the market prices for stock loans are. The data cover over 20,000 securities lending programs worldwide and over $2 trillion worth of loans. The fragmented nature of this market makes it difficult to know how much of the lending market this dataset covers.

[11] While we also can't rule out other forms of shorting, such as naked shorting, that do not require borrowing shares, evidence suggests these are unlikely to impact our results. First, unless naked shorting demand moves in the opposite direction of demand for borrowing shares, it is unlikely to affect our results. Second, SEC Rule 204T was put into place on October 14, 2008—in between our two phases. (The "T" for temporary was removed on July 31, 2009, when the rule became permanent.) This rule imposes requirements on broker dealers designed to curtail naked shorting by making the broker responsible for delivering the sold (shorted) securities on the settlement date or else placing a market buy order for the security on the following day. A broker that fails to do so is not allowed to affect any more shorts of the security for its clients until it is corrected. The introduction of this rule makes naked shorting less of a concern for the second phase of our experiment, but given the similarity in results we find across both phases it seems unlikely naked shorting has any impact on our results. Finally, practitioners we have spoken to claim that naked shorting is not an important part of this market.

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). We use the average daily loan fee from Data Explorers for new loans on a given stock on a given day and compute average daily loan fees over the two phases of the experiment. New loans are defined as originations and do not net out shares returned. The fees on new loans are a better match for the actual fees the Manager receives than fees on previously made, existing loans. If no new loans on a stock are made on a given day, we exclude that observation from the averages. These data allow us to compare the actual fees the Manager receives on the randomly available stocks to the prevailing market fees on those same stocks at the same time.

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. Those fees represent the lending fees earned by other shareholders who did lend out their shares of these stocks (and represent the potential opportunity cost from withholding the shares).

F. *Summary Statistics*

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 daily loan fee is 259 (81) basis points in the first phase, and 159 (101) basis points in the second phase. The range of average daily loan fees is 1.4 to 976 basis points. The mean (median) loan size-weighted average daily fee is 768 (523) basis points during the first phase and 147 (101) basis points during the second phase,

indicating that the more lucrative stocks represent a disproportionately larger share of loan volume during the first phase, but not the second.

Mean and median actual lending fees are smaller than the expected fees quoted by the lending agent (comparing Tables I and II). This could occur for several reasons. The lending agent may overstate expected fees to obtain the Manager's business, may overestimate expected fees because of overconfidence, or may fail to account for the price impact of additional loan supply. As best we can tell from conversations with the lending agent (and other lending agents), the expected fees are quoted based on current demand and supply, 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 for such downward pressure. Nevertheless, the price impact we measure for fees does not appear large enough to explain all of the difference, hence the expected fees still appear to have been optimistic.

On average, the shares on loan represent 5.02% of the pre-experiment short interest during the first phase of the experiment and 8.93% during the second phase. On days that new shares are lent, the average total new lending is 42.6% of actual daily trading volume for the first phase of the experiment and 70.3% for the second phase.[12]

Table II also shows that the average daily shares on loan represent about 1% of equity market capitalization, with the maximum loan level accounting for an average of 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 of the companies, so that the shock to the supply of lendable shares in the market for these companies from our experiment is sizeable. Consistent with this, we examine loan fees in the next section, and find that our supply changes have a material effect on market fees.

[12] 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 exceed 100% because exchange-reported trading volume does not include off-exchange trading.

IV. Results

In this section, we examine the effects of increasing the loan supply of shares for the treated firms relative to the control firms (whose supply remains fixed), holding shorting demand and everything else constant. We analyze changes in stock loan fees, mean returns, volatility, skewness, and bid-ask spreads. We also exploit cross-sectional variation in loan supply and firm attributes to examine these effects.

A. *Loan Fees*

Table III examines the impact of the loan supply shocks on prevailing loan fees in the stocks. 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 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 Data Explorers fees for all stocks over both the pre-lending and lending period. 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. Standard errors of the mean return differences (reported in brackets) are robust to heteroskedasticity. Tests of median differences use the Pearson Chi-square test. 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. Six of the eight negative measures are statistically significant. These results strongly suggest that our experiment produces loan supply changes that are sizeable enough to affect fees significantly. The economic magnitude of this impact varies depending on the measures used, but ranges from -9 to -345 basis points during the first phase, and from -227 to -303 basis points during the second phase. The median differences are negative for six of eight measures, and significantly so for four, again consistent with the loan supply changes having an impact on loan fees.

Panel B of Table III reports the results of cross-sectional regressions of loan fee differences between the lending period and pre-period (using the fourth measure of fees above) on the size of the loan made in each stock. We scale the loan size by the size of the potential loan determined by the Manager as well by shares outstanding. Regressions are run 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 (available only and all stocks), there is a consistent negative relationship between loan size and change in loan fee (loan size is zero by definition for withheld stocks). Seven of the eight coefficients are significantly negative. Larger supply shocks to stocks are associated with larger decreases in loan fees. These cross-sectional results provide additional support that our experiment provides sizeable loan supply shocks that matter for loan prices.

The evidence in Table III is consistent with theory (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.

B. *Returns*

Table IV analyzes how the supply shock to lendable shares affects returns by comparing the daily returns to the available stocks relative to the control stocks over three periods: the "pre-lending period", roughly a month before the loan 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 in stocks with higher ex ante shorting demand relative to supply (as implied by high loan fees).

If loan supply shocks from our experiment have pricing effects, then returns should decrease during the lending period when supply increases and reverse (or increase) during the recall period as the supply of shares is suddenly reduced. We report raw returns. Returns adjusted for the Russell 2500 index (the index most closely related to the Manager's holdings) and returns adjusted for returns to the Fama-French 48 industry portfolios yield economically and statistically identical results.[13]

Panel A of Table IV reports the results of the first phase; Panel B, of the second phase. Standard errors are robust to heteroskedasticity. 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 with one exception – during the second phase's pre-lending period, the expected loan fee-weighted portfolio of available stocks outperforms at a marginally statistically significant level.

The next three columns of each panel report average daily returns over the lending period. 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 per day and is statistically insignificant.

[13] If the treated firms are otherwise identical to the non-treated firms there is no need to adjust returns for risk at all, since the two groups should have identical risk characteristics. The fact that the results are unaffected by any risk adjustment is further confirmation that our randomized experimental design achieves its objective.

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 also 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 phase, 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 phase.

In 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.[14]

The final three columns of Panel A of Table IV 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]

[14] We also find no evidence of adverse price pressure on the first two days of each lending period: returns to available and withheld stocks are not significantly different on these days.

[15] We also examine the average daily returns to the non-revenue stocks for the first phase of the experiment based on whether they were supposed to be available or not available to be lent. Unlike the revenue stocks, the vast majority of these stocks were not lent even if available. (Four were lent because they became revenue producing between June and September.) The returns to the two groups of stocks are virtually identical over the lending and recall periods as well.

Overall, we fail to find evidence consistent with Miller's (1997) overpricing hypothesis that supply increases create negative price pressure.

C. *The Power of the Tests*

Before turning to other 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 V reports the 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 of the experiment and the lending period of the second phase of the experiment. We report rejection region cutoffs for one-tailed 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-weighted, value-weighted, and expected loan fee-weighted average differences across all phases of the experiment.

The first column of Table V indicates that for the equal-weighted portfolios during the first phase of the experiment, we can reject that the return difference between available and withheld stocks for the lending period is less than -4 basis points at the 10% significance level . At the 5% and 1% significance levels, the critical values are -21 and -60 basis points, respectively. For the value-weighted portfolios, the cutoff values are more negative. For the expected loan-fee weighted portfolios, however, the cutoff values are uniformly positive. Not only can we reject that the return differences are negative at the 1% level, but we can also reject that they are not below a fairly high 30 basis points.

For the recall period (where the null is a positive return effect), depending on the weighting scheme, we can reject that returns are not larger than 26 to 138 basis points.

For the first phase of the experiment, therefore, the power of our test seems to depend on the weighting scheme we choose.

The second phase of our experiment is much less sensitive to weighting scheme and appears to provide a more powerful test. This makes sense because the second phase covers a longer time period. We can reject at the 10%, 5%, and 1% significance levels that the lending-period return differences are no smaller 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 comes from the fact that we run two independent trials or phases. For example, over the two lending periods from both phases, based on the *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 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 the phase, and is the most efficient (minimum variance) estimator of the combined return difference. Using this combined return difference and its standard error, we calculate rejection regions across the two phases.

The last column of Table V 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% confidence level, we can reject that the return difference between available and

withheld stocks is negative for all three portfolios. In other words, at the 10% confidence level, the data can reject the hypothesis from Miller (1977).

At the 5% confidence level, we can reject that the return difference is less then -3 basis points for the equal- and value-weighted portfolios. We can reject that the return difference is less than 25 basis points for the expected loan fee-weighted portfolios. Even at the 1% confidence level, we can reject that the return difference is less then -12 basis points for the equal- and value-weighted portfolios; and that the return difference is less than 12 basis points for the expected loan fee-weighted portfolios.

Overall, 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.

D. Volatility, Skewness, and Bid-Ask Spreads

The first three columns of Table VI report differences-in-differences of the standard deviations of the stocks made available for lending and the stocks withheld 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 sale constraints hinder price discovery, we expect lower volatility for the stocks available for lending than for those withheld over the lending period; and a reversal over the recall period. On the other hand, if short sales are destabilizing, we expect the opposite pattern.

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 VI report results from the analogous analysis 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 lead to overpricing (e.g., Miller, 1977) or no mispricing (e.g., Diamond and Verrechia, 1987)) according to theory. However, even in the case of no mispricing, 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, across all three averages (equal, value, and expected loan spread-weighted) 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 in magnitude but still statistically insignificant. Two of the three signs are opposite those for the first phase of the experiment. These results suggest that shorting supply shocks have no 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 sale constraints are important for price discovery, we expect lower bid-ask spreads for the stocks available for lending than for those not available for lending 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 stabilization or destabilization. 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. The lack of consistency between the two lending period results and the recall period results cautions against drawing any strong conclusions.

E. *Post-October 1 results for the second phase of the experiment*

As mentioned, on October 1, 2009, the manager lifted the restrictions on lending the withheld stocks in the second phase of the experiment, making these stocks available for lending. Examining the outcomes (returns, volatility, skewness, and bid-ask spreads) for these stocks from October 1 to December 31, 2009, we also fail to detect any evidence of adverse consequences of stock lending. Although 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 similarly finds no significant effect on stock prices.

F. *Cross-sectional Results*

The previous results on returns, volatility, skewness, and bid-ask spreads compare the average stock in the treatment and control groups, which may mask important cross-sectional information. Certain kinds of stocks or loan amounts may have more or less of an effect on the various outcome variables we consider. In fact, Table III shows that variation in loan size is related to loan fees, with larger supply shocks being associated with larger loan fee declines.

To exploit the cross-sectional variation, we estimate cross-sectional regressions of the outcome variables on stock and Manager's holding characteristics interacted with the treatment and control of lendable share availability.

Panel A of Table VII reports results from regressions of returns in the lending periods, and the changes in volatility, skewness, and bid-ask spreads from the pre-lending to lending periods, on a set of cross-sectional characteristics of the holdings. The regressions include both stocks available and not available for lending. We include as independent variables in the multivariate regressions: the stock's short interest (as a percentage of shares outstanding), the maximum potential share loan as a percentage of institutional ownership, and the expected loan spread (estimated at June 30, 2008 and April 30, 2009, respectively for the two phases). In each regression, we also include a dummy variable equal to one if the

stock is available and a variable that interacts each independent variable with the available dummy variable. If short sales constraints are important for stocks with high short interest, high expected loan fees, and high potential loan amounts, the interaction terms in the regressions between the availability dummy and the holdings characteristics will be significant.

In both phases, the return regressions yield no significant coefficients on any of the interaction terms. Hence, we do not find any effect on returns from shorting supply even for stocks with the most extreme short interest, potential loan amount (e.g., largest supply shocks), and expected loan fees (e.g., highest shorting demand).

We find only two significant interaction terms in the regressions that use volatility, skewness, and bid-ask spread changes over the lending period as dependent variables. In the first phase, volatility rises for stocks with the highest expected loan fees. This is consistent with increased shorting activity possibly having a destabilizing influence on stock prices with extreme shorting demand relative to supply. In the second phase, skewness increases when the potential loan is large relative to institutional ownership.

Panel C of Table VII repeats the regressions over the recall period, using returns and changes in volatility, skewness, and bid-ask spreads from the lending to recall period as dependent variables. The only significant interaction term is that high expected loan fees are associated with higher skewness over the recall period. Also, the interaction term on the expected loan spread is negative (albeit not significantly so) reversing the positive relation in the lending period.

Overall, then, the cross-sectional results provide little evidence that stocks in high shorting demand experience different returns or different volatilities as the supply of lendable shares expands.

Panels B and D of Table VII repeat the analyses in Panels A and C, but use only the stocks that are lent. These regressions, therefore, do not control for the exogenous treatment of stocks, and hence, are potentially useful for demonstrating the importance of the experimental design. In panel B, higher short interest is significantly negatively associated with volatility in the first phase. In the second phase, a greater potential loan size is positively related to volatility and skewness; a higher expected loan fee is negatively related to volatility and skewness. Except for the effect of loan size on skewness, these results

are insignificant when we include the withheld stocks and control for the exogenous treatment effect. In Panel D, higher short interest is associated with lower returns and lower bid-ask spreads in the recall period. But, those results, too, disappear when we include the withheld stocks. These results highlight once again the importance of our experimental design for identifying exogenous supply shocks.

G. *Robustness*

Finally, while the results in the paper pertain to comparing the 32 (19) available stocks in the first (second) phase, that had at least 25 basis points of expected loan fees and that are actually lent out, to the random 20 (9) withheld stocks, 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 Appendix, we report all of our main results (differences in returns, and differences-in-differences in volatility, skewness, and bid-ask spreads) 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 (this means all 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, meaning we compare 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 slight changes in sample choice. We consistently find no adverse effects on share prices from exogenous shocks to loan supply.

V. Implications of the Experiment and Economic Magnitudes

In this section, we discuss the implications of the results of our experiment, focusing on three areas.

First, our results have implications for theories of securities lending and shorting constraints. As we stated in motivating the experiment, we provide an exogenous shock to loan supply that allows us to measure the effects of supply changes. 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 disconnect between loan fees and stock prices we find in the data is that other frictions exist in practice that are absent from these models. For example, loan fees may not be market clearing in practice if lending agents ration their lendable securities, reward certain clients, or bundle securities lending with other services. Kolasinski, Reed, and Ringgenberg (2010) present evidence consistent with the latter explanation.

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. Among our sample of stocks we find that options markets are much less liquid than the market for the underlying shares. If options represent another vehicle for hedgers to place their trades, this could be consistent with them being a larger (and more influential) part of the derivative and lending markets and inconsequential in the spot market.

Vaguely consistent with this notion is evidence from Figlewski and Webb (1993) that optionable stocks exhibit higher levels of short interest, but the effect on the underlying stock price is weak. While these conjectures are only speculative, it would be interesting for future theoretical and empirical work to consider the wedge between the lending and underlying security markets.

Second, our results have implications for an investment manager's decision to lend its securities. Indeed, this decision motivated the Manager to allow us to run the experiment. Our findings that average loan fees for high demand stocks are large and that the impact from lending on stock prices is negligible strongly suggest that securities lending generates additional revenue without adversely affecting the fund's holdings.

In Table VIII, we calculate the magnitude of this additional revenue from the results in the experiment. We estimate the return per dollar ownership in each stock from the lending activity over both phases of our experiment. We consider the potential fee revenue per dollar owned of all available *and* withheld stocks, since we are interested in the total opportunity cost of not lending shares. For loan quantities, we use the potential loan size the Manager stated ex ante rather than the total number of shares owned by the manager or the actual loan sizes made during the experiment. We use the potential loan because it is based on the Manager's trading experience and is made before the decision to lend. The ex ante loan size comprises 65-70% of the Manager's ownership share on average over both phases of the experiment. While using the full ownership share would generate more revenue in the absence of price impact, the Manager's scaling back of lendable shares may reflect concerns of price impact based on its trading experience. We also do not use the actual ownership share during the experiment because it is only realized ex post, being sometimes greater and sometimes smaller than the potential loan. We multiply the loan amounts by the loan fees to arrive at total revenue from lending.

We use three different loan fees in Table VIII: (1) the lending agent's ex ante expected loan fees on all stocks (available and withheld); (2) the actual average loan fee during the experiment (using actual loan fees for the available stocks and Data Explorers loan fees for withheld stocks); and (3) the actual loan fee for available stocks and a hypothetical loan fee for withheld stocks, which is an estimate of the

fee the withheld stocks would have received during the lending period if we assume the same discount between the lending agent's expected fee and the actual fee that we measure for the available stocks.

The numbers in Table VIII represent the returns (in percent) per year of total manager ownership in the stock that the manager gets from lending out the full potential loan amount of all high-fee stocks at the various fees. Based on the first phase results, lending revenue would add 2.78% to 4.64% per year, with median fees ranging from 83 to 129 basis points. Using second phase results, lending revenue would add between 49 and 271 basis points per year on average (or 35 to 94 basis points at the median). The fees are quite a bit higher during the first phase because that was a period of high shorting demand.

These estimates represent returns per dollar invested in the high demand stocks for which we ran the experiment. Because the high-fee / high demand (revenue) stocks comprise about 20% of the stocks the Manager held, the additional return contribution as a percentage of total assets under management would be roughly one-fifth of our estimates—ranging from about 10 basis points to as high as 1% of total assets under management per year. These additional returns 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. Indeed, our Manager reached this conclusion based on these results, and consequently began lending all of its high demand stocks on October 1, 2009, thus ending the second (and final) phase of our experiment.

These results suggest that funds should lend out their shares without worrying about significant consequences on the underlying stocks they own, and that the returns from doing so are meaningful. This general conclusion, however, only holds if all else remains equal. For example, would the effects from lending be different for different types of securities than our Manager trades (which are small and mid cap stocks)? Would the effects be the same for funds with different ownership shares or trading activity than our Manager exhibits? While we cannot answer these questions with certainty, the insignificance of our cross-sectional results suggests that the basic results hold across a broad spectrum of stocks.

It is also possible that different economic environments would yield different results. However, the fact that we find similar results over both phases suggests that our results are robust to at least two different economic environments.

Our experiment also only measures the marginal lending from our Manager on top of whatever other lending is already taking place. For stocks with little lending activity, lending fees could be higher (holding demand fixed), or if every fund decides to lend, 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 effect of all funds lending their shares.[16]

Third, our results potentially have implications for regulatory policy towards shorting activity and securities lending. The intense debate over securities lending and short selling 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 that regulatory constraints on the supply of lendable shares are not likely to be effective or useful.

VI. Conclusion

Despite the intense debate over the effects of shorting in both policy circles and among investors and corporate executives, the evidence regarding shorting has remained mixed due in large part to the difficulty in identifying clear supply and demand movements. Our study attempts to fill this void by implementing a randomized experiment that generates a sizeable supply shock to the loan market for securities.

By comparing the differences between the randomly available (treatment) and withheld (control) group of stocks in our experiment, we difference out other confounding influences. We find that the

[16] 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.

exogenous changes in loan supply have significant effects on loan fees, but no adverse effects on security prices or spreads for our sample. We find consistent results across two independent time periods.

We also highlight the importance of exogeneity, finding several significant, but spurious, results when we ignore the randomization from the experiment. This 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 imply that fund managers can earn substantial 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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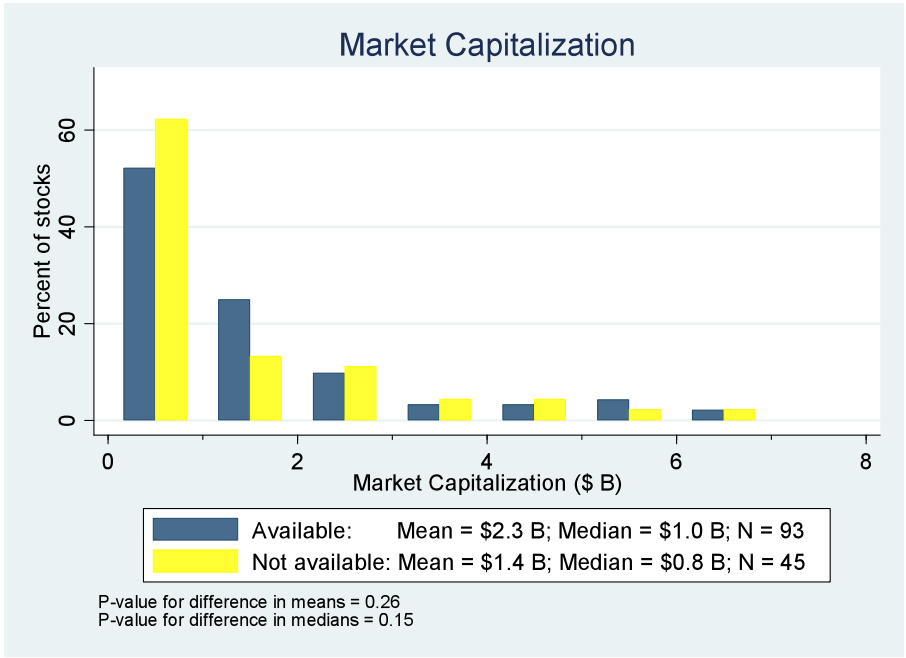
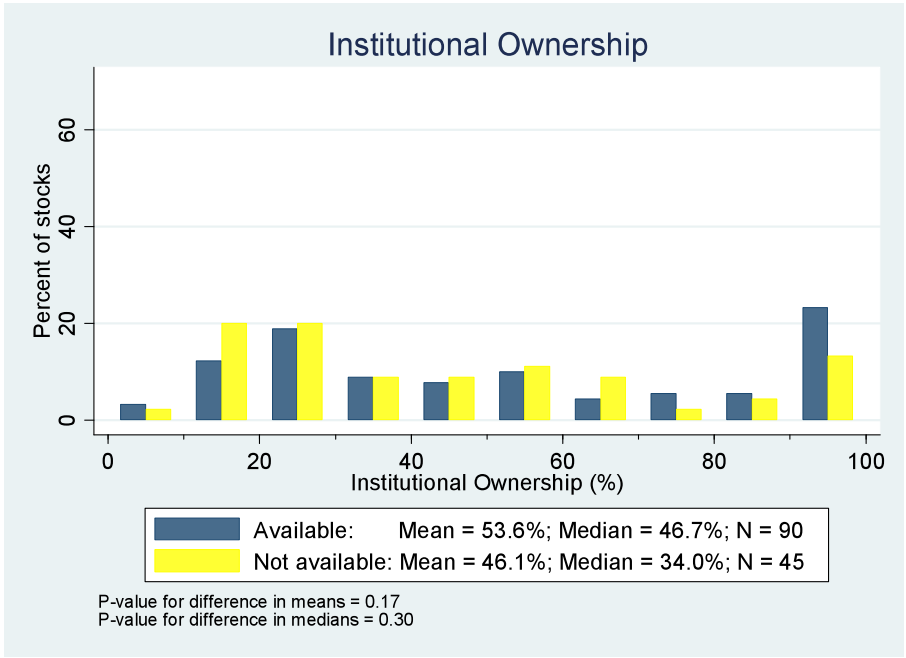
Figure 1: Distribution of revenue stocks characteristics after randomization, first phase of experiment.





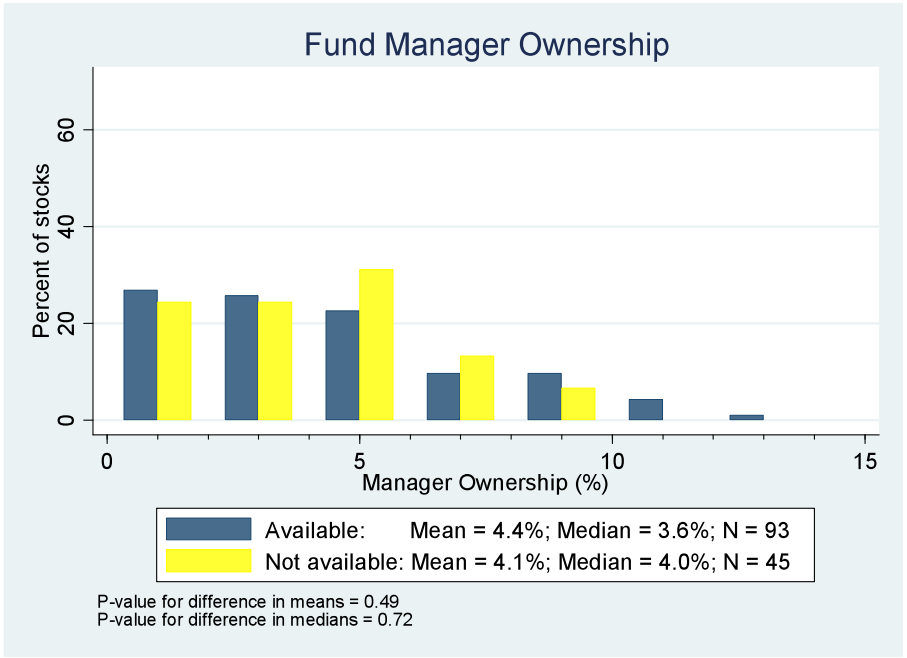
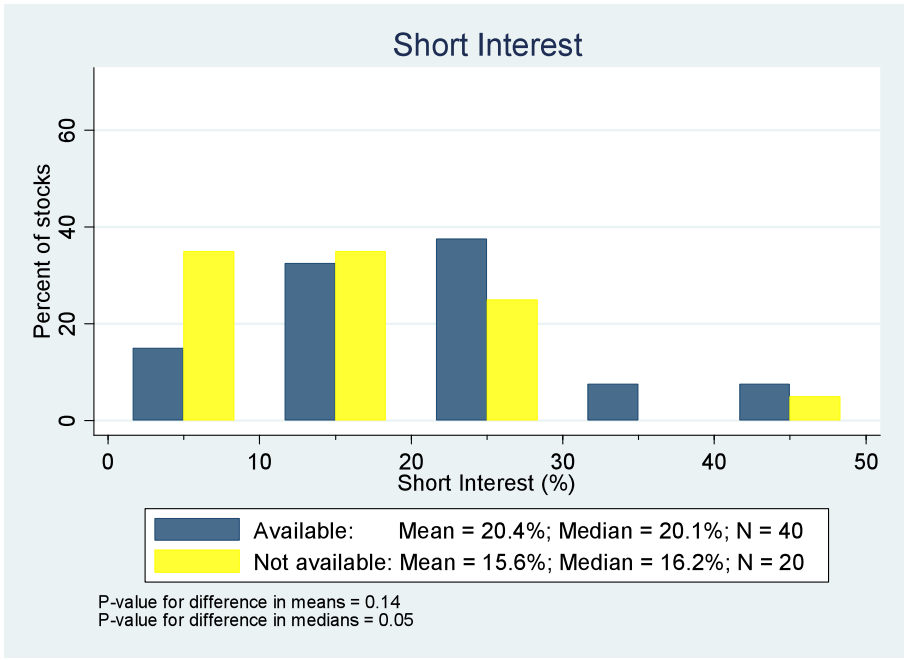
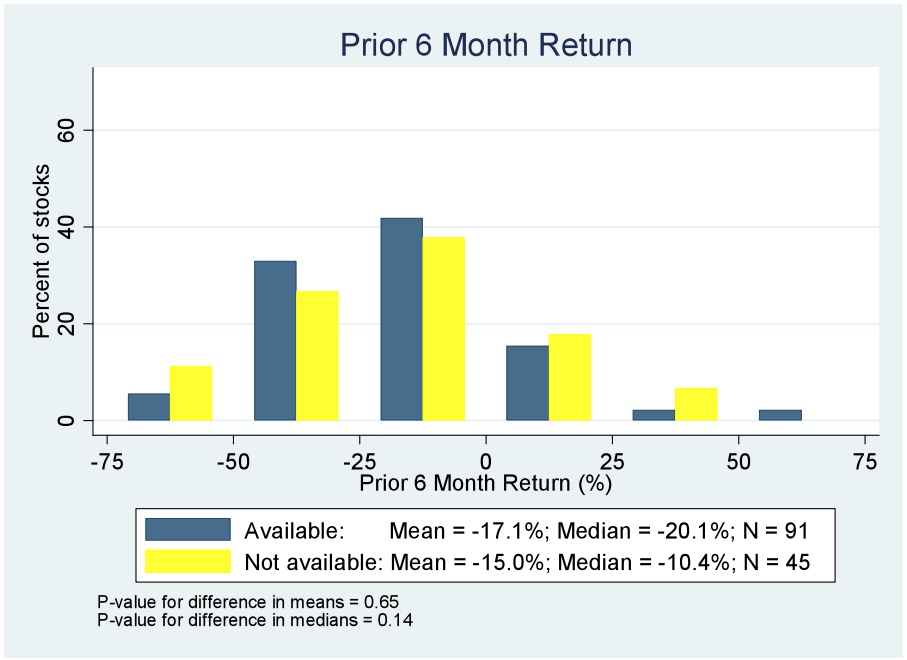
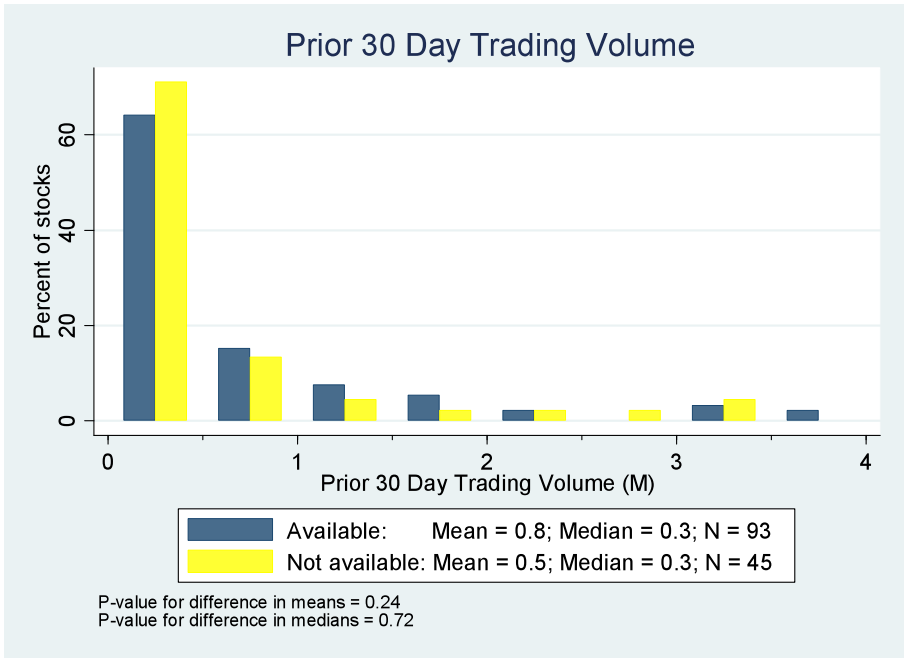


Figure 1 (continued): Distribution of revenue stocks characteristics after randomization, first phase of experiment.







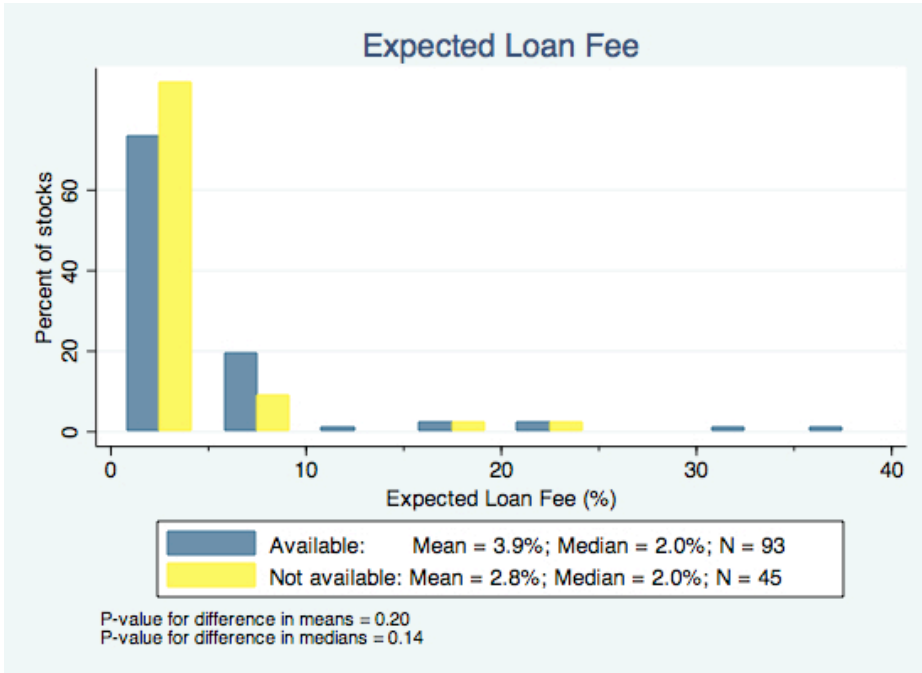


Figure 2: Distribution of non-revenue stocks characteristics after randomization, first phase of experiment.

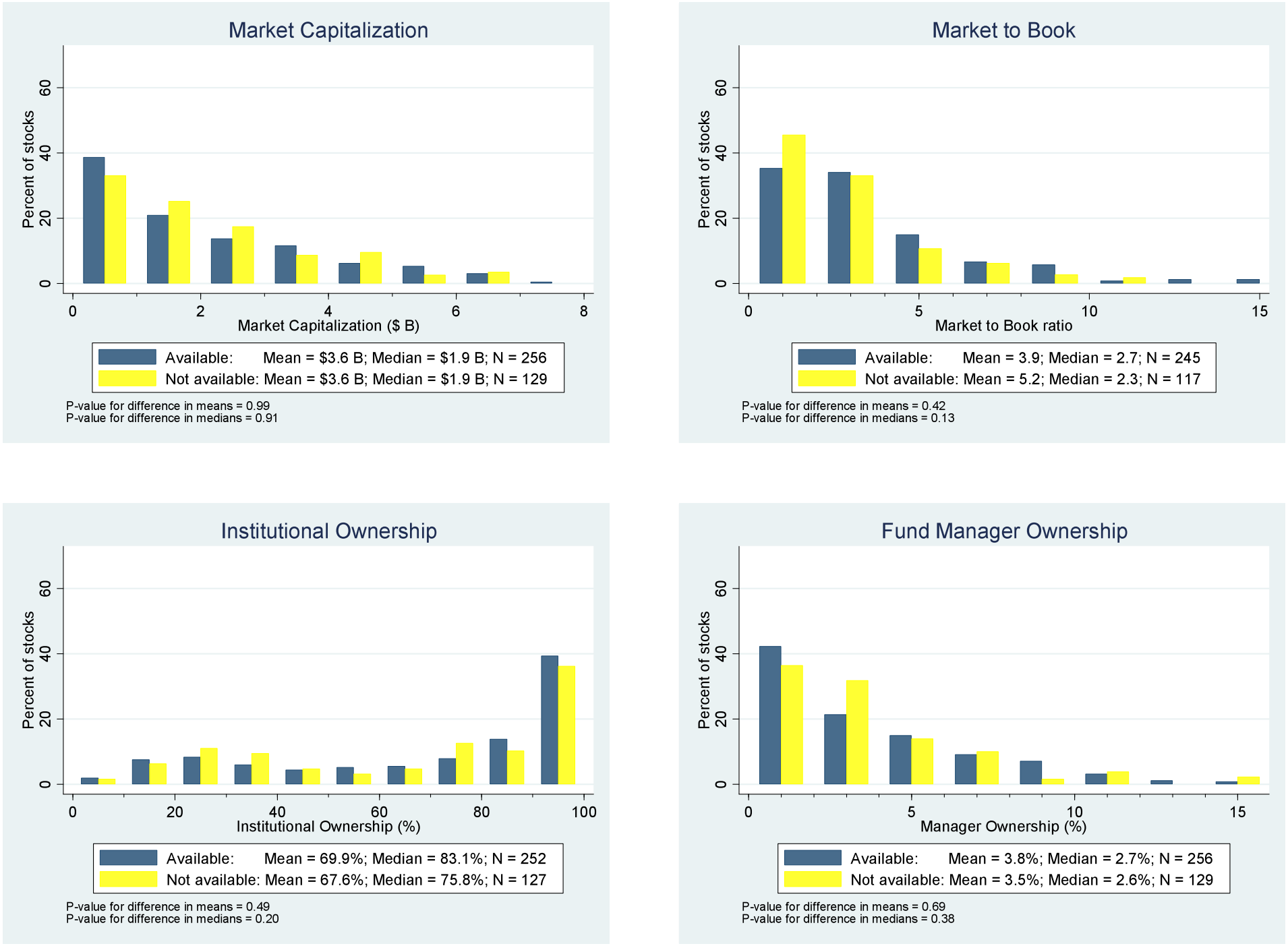
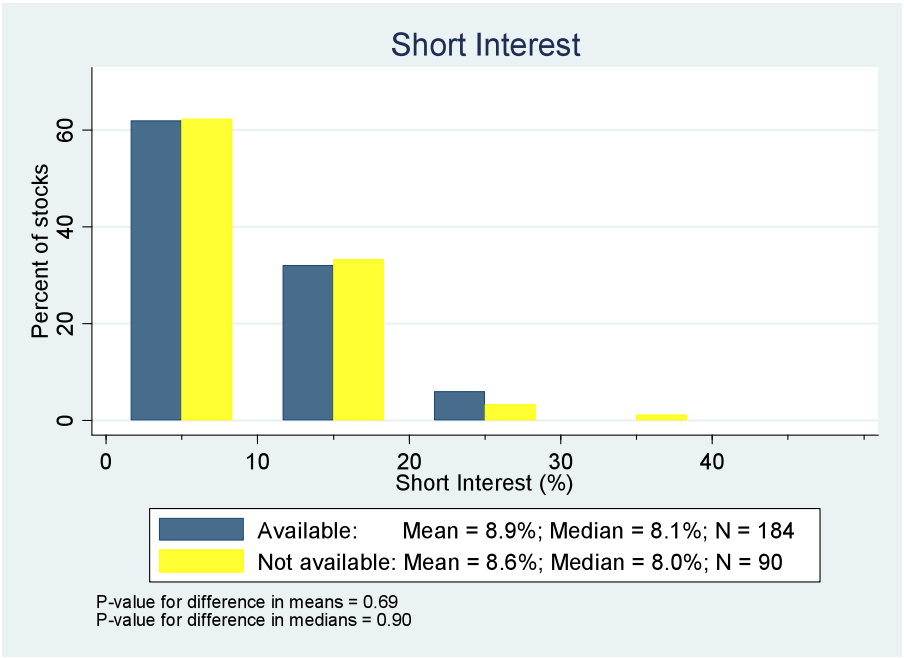
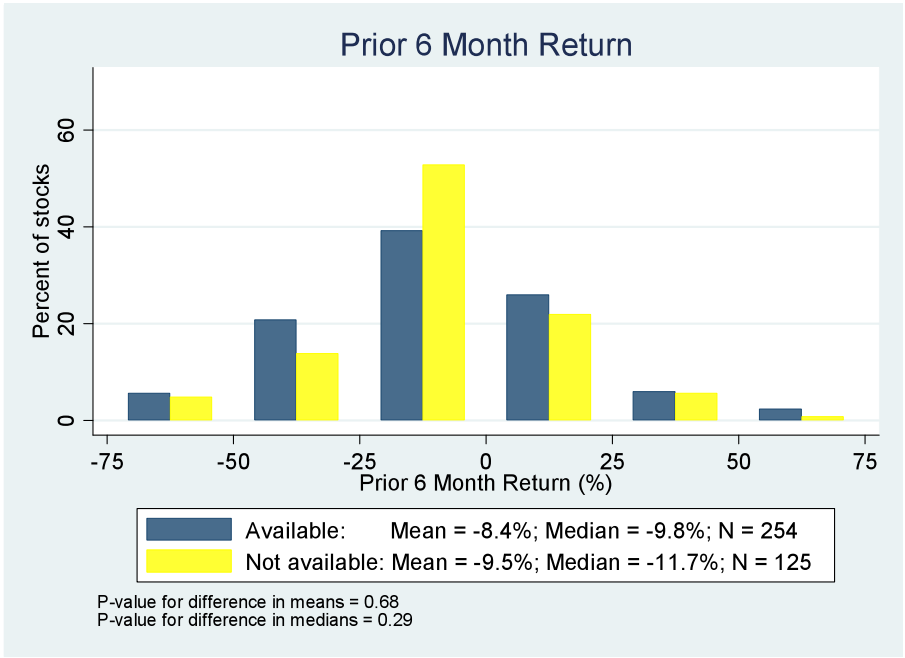


Figure 2 (continued): Distribution of non-revenue stocks characteristics after randomization, first phase of experiment.





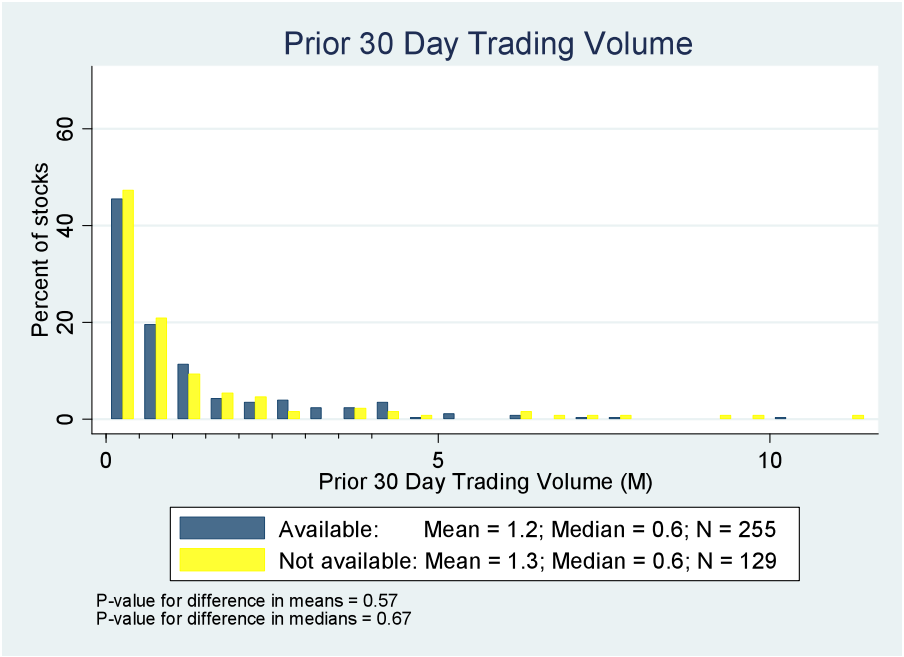
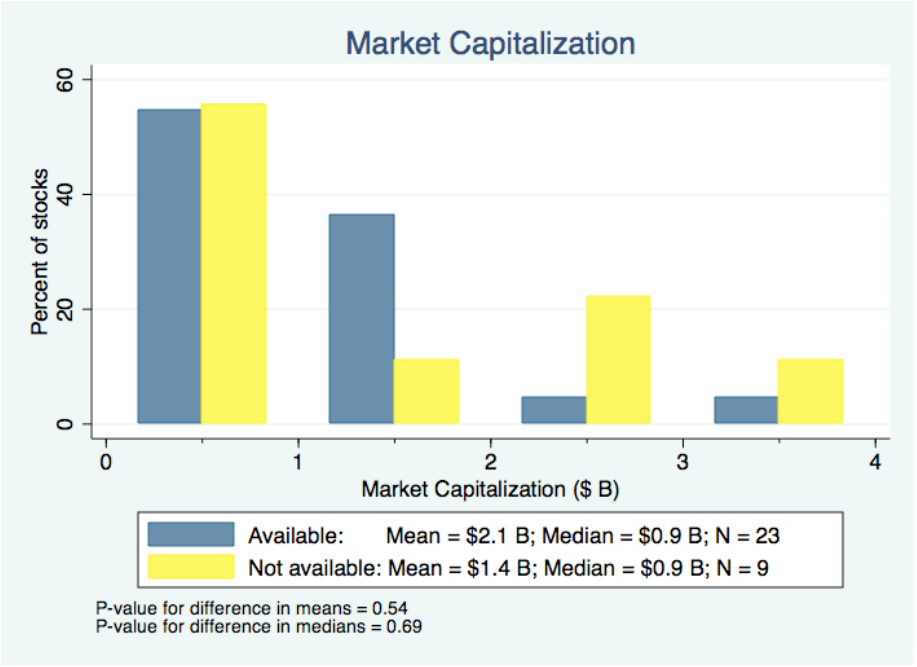
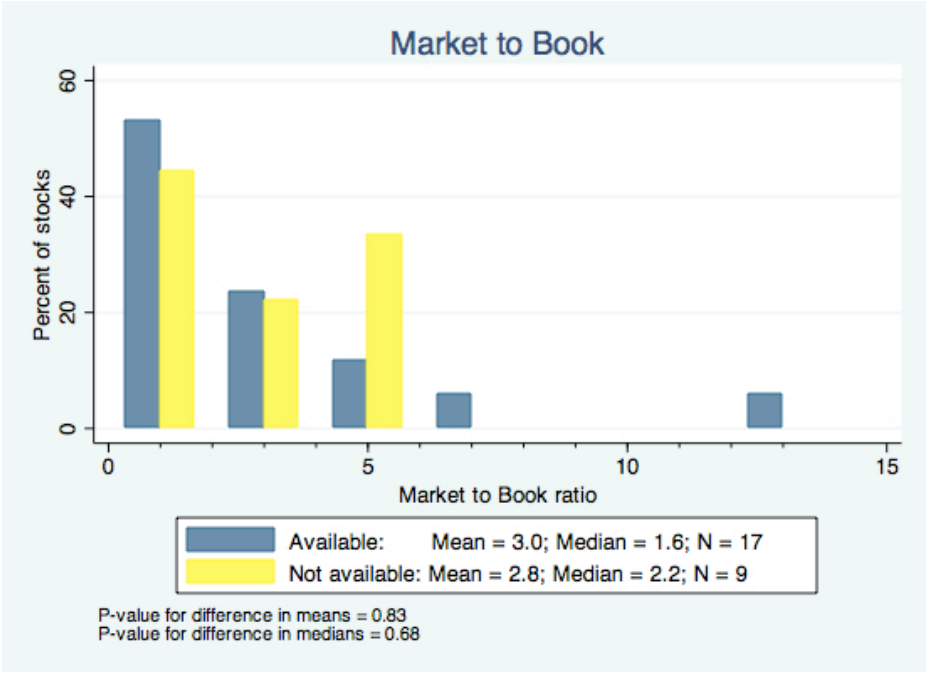
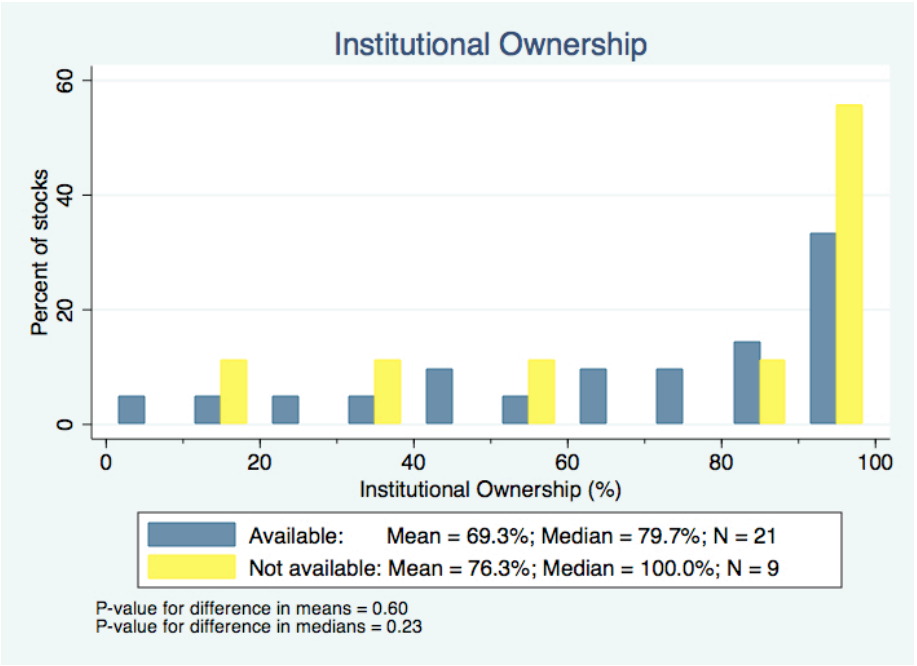


Figure 3: Distribution of revenue stocks characteristics after randomization, second phase of experiment.







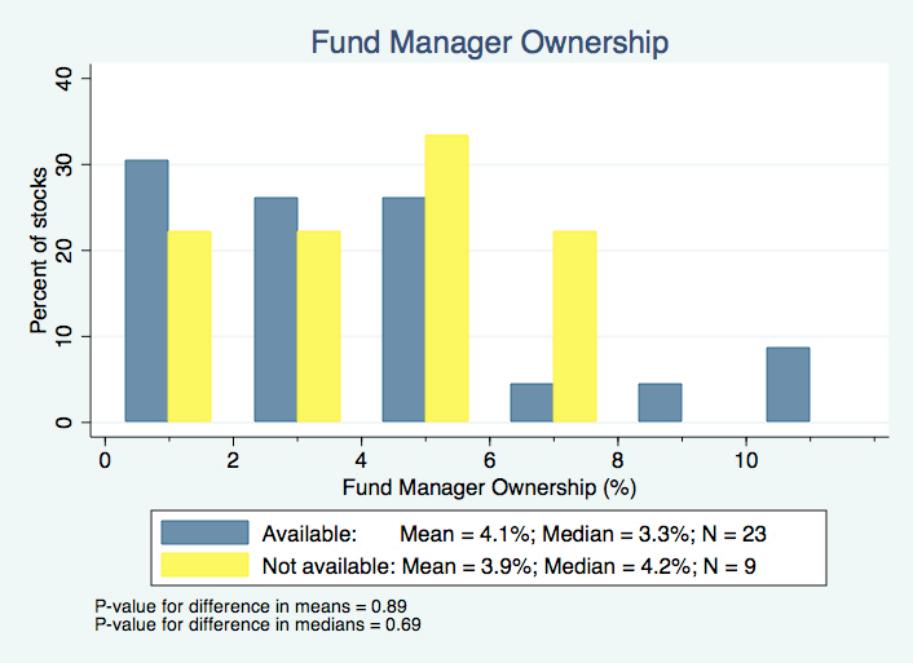
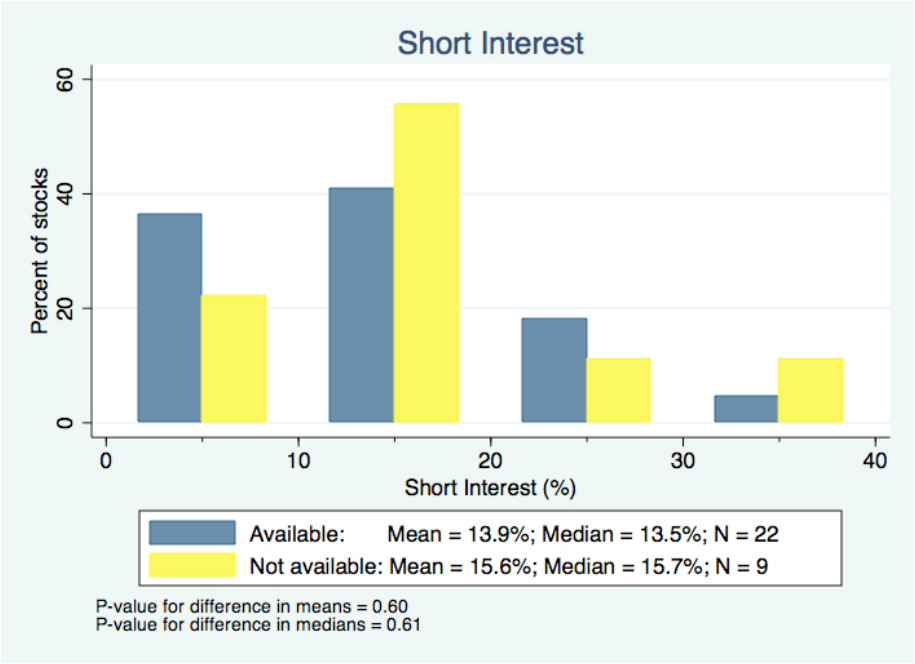
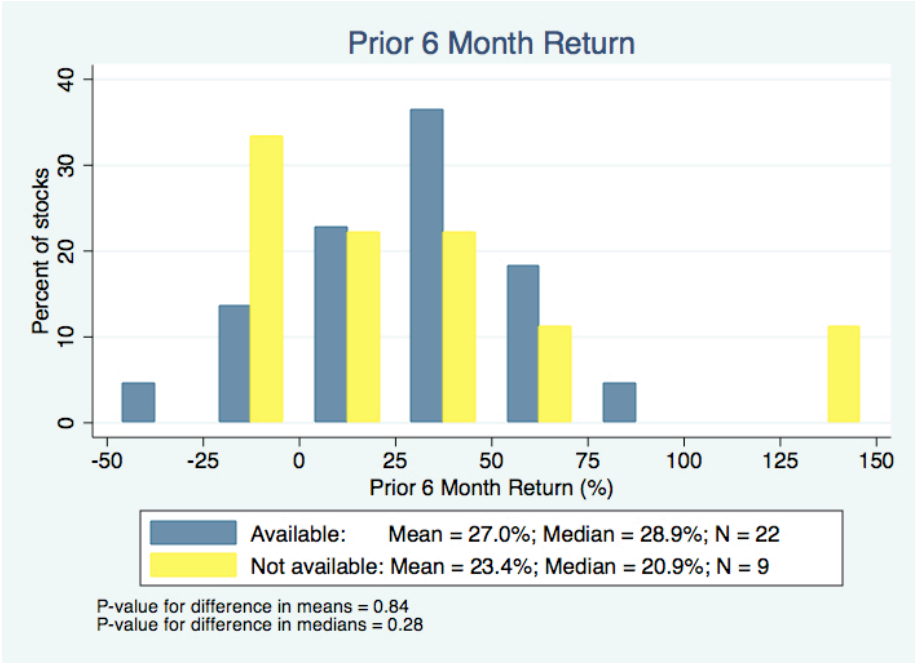
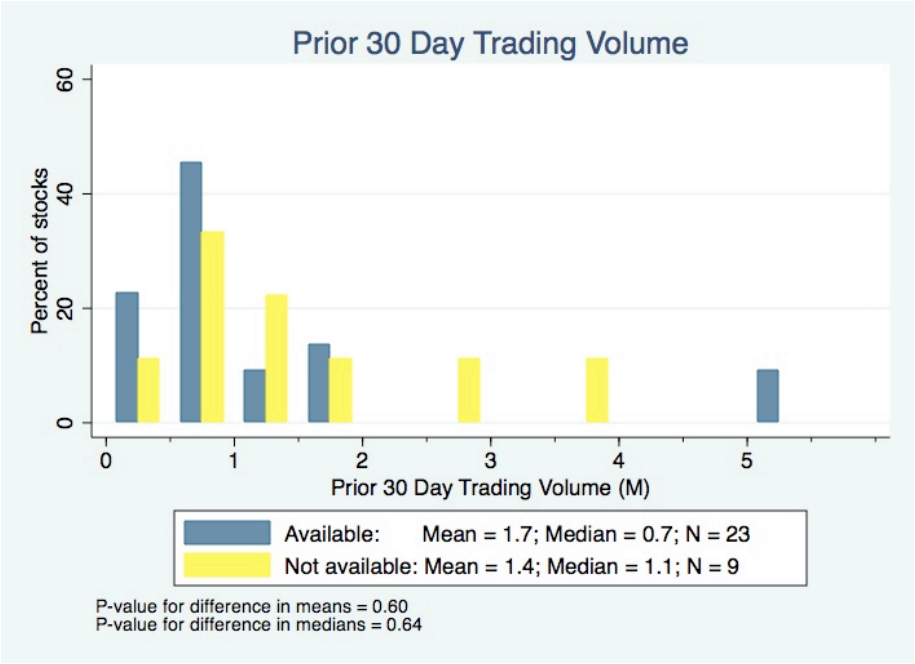


Figure 3 (continued): Distribution of revenue stocks characteristics after randomization, second phase of experiment.







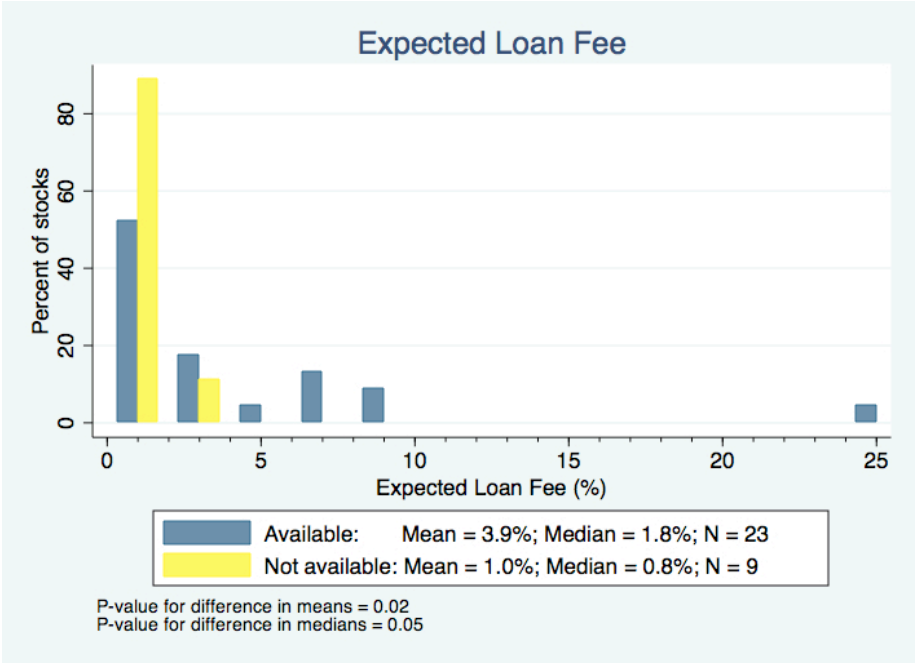


Table I:
Summary Statistics of Randomly Available and Randomly Withheld Stocks

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 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 (%), manager holdings as a percent of prior 30-day average trading volume, institutional ownership, and short interest, and potential loan size as a percent of prior 30-day average trading volume, institutional ownership, and short interest. The *p*-values from tests for differences in means and medians between the two groups for each phase of the experiment are also reported.

	First Phase			Second Phase		
	Available	Withheld	p-value of difference	Available	Withheld	p-value of difference
Market cap ($M)	1,367.1	945.2	(0.21)	923.5	1,378.4	(0.28)
	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 (%)	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)
Number of stocks	32	20		19	9	

Table II:
Summary Statistics of Randomly Lent Stocks 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 days on which the stock was on loan. Loan period is the number of days 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. Average daily loan fee and Loan size-weighted average daily loan fee are the time-series averages of, respectively, the stock's average loan fee for each day, and the stock's loan-size weighted average loan fee for each day. 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 (days)	12.6	13.5	4.8	1	19	49.2	68.0	68.0	2	82
Lending period (days)	6.1	6.0	2.3	1	9					
Recall period (days)	6.5	7.0	2.9	0	12					
Average daily loan fee (bps)	258.5	80.9	312.1	1.4	976.1	158.8	101.4	133.9	27	516
Loan size-weighted average daily loan fee (bps)	767.6	523.3	784.3	63.4	2,311.1	147.1	100.8	110.1	23	412
Average daily lent shares/ trading volume (%)	42.6	25.5	56.2	3.2	326.0	70.3	22.9	134.9	2	604
Average daily loan market value ($M)	11.1	7.3	11.2	0.1	39.1	11.1	6.3	12.0	0	39
Average daily % of market cap on loan	0.9	0.7	0.8	0.0	2.8	1.1	0.8	1.0	0	4
Max. daily dollar value of shares on loan ($M)	19.7	12.0	18.7	0.1	75.8	19.8	11.9	20.2	0	66
Max. daily % of market cap on loan	1.6	1.4	1.2	0.0	5.0	1.7	1.4	1.4	0	5
Average daily % of short interest on loan	5.0	3.8	3.9	0.1	16.4	8.9	5.5	12.5	0	58

Table III:

Loan Fee Differences between Available and Withheld Stocks from the Pre-Lending to Lending Period

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 August 1 to September 4, 2008 and lending period September 5 to September 17, 2008. Second phase: pre-lending period May 1 to June 4, 2009 and lending period June 5 to September 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. Standard errors of the mean return differences are given in brackets and are robust to heteroskedasticity. Tests of median differences are performed using the Pearson Chi-square test. Panel B reports cross-sectional regression results of loan fee differences (using definition 4) above) between the pre-period and lending period for each stock on the size of the loan made in each stock, scaled by both the size of the potential loan determined by the Manager as well as by shares outstanding. Regressions are run for just the available stocks whose shares are actually lent out as well as for all stocks, including those withheld, for which actual loan size is zero. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Loan Fee Differences-in-Differences (%)

	First phase		Second phase	
	Available-Withheld Mean	Available-Withheld Median	Available-Withheld Mean	Available-Withheld Median
Lending period fee (from DE) – pre-lending period fee (from DE)	-0.09 [0.45]	0.05	-2.29* [1.13]	-0.56**
Lending period fee (from DE) – expected fee (from lending agent)	-0.70 [1.28]	0.02	-2.27* [1.14]	-0.48
Actual lending fee – expected fee (from lending agent)	-3.45** [1.46]	-0.50	-3.01** [1.30]	-0.94**
Actual lending fee – pre-lending period fee (from DE)	-2.84*** [0.66]	-1.82***	-3.03** [1.30]	-1.19**

Panel B: Regressions of Changes in Loan Fee on Loan Size

	First phase				Second phase			
	Available stocks		All stocks		Available stocks		All stocks	
Dependent variable =	Actual lending fee – pre-period fee				Actual lending fee – pre-period fee			
Actual loan/potential loan (%)	-0.34*** [0.12]		-0.35*** [0.09]		-0.18** [0.07]		-0.17** [0.06]	
Actual loan/shares outstanding (%)		-3.55 [3.37]		-6.91** [2.94]		-6.09*** [1.53]		-5.83*** [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:

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

Reported are the average daily returns (in percent) for portfolios of stocks randomly made available to lend and randomly withheld from lending. Panel A reports the first phase of the experiment, in which the available portfolio consists of 32 stocks randomly made available to lend and the withheld portfolio consists of 20 stocks randomly withheld. In Panel A, average daily returns for both portfolios 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 two portfolio returns (available minus withheld) are also reported for each period. 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 the second phase of the experiment, in which the available portfolio consists of 19 stocks randomly made available to lend and the withheld portfolio consists of 9 stocks randomly withheld. In Panel B, 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). 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 V:
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 the recall periods, while the second phase just contains differences for the lending period. 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 VI:

Volatility, Skewness, and Bid-Ask Spread Differences-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, 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. 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 arrive at the difference-in-difference estimates reported in the table. We use three different weighting schemes to compute the cross-sectional averages: equal weighting, value-weighting (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 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 VII:
Cross-Sectional Regressions to Explain Returns and Volatility, Skewness, and Bid-Ask Spread Changes

Cross-sectional regressions to explain returns and volatility, skewness, and bid-ask spread changes over the lending and recall periods. Stock-level changes for volatility, skewness, and bid-ask spreads from pre-lending period to lending period, and from lending period to recall period, are calculated as described in Table VI. We regress these variables on various stock characteristics. Panels A and B examine the lending periods and Panels C and D the recall period. "Available" is a dummy variable equal to 1 if the stock is randomly made available for lending, and zero otherwise. All other independent variables are defined in Table II. Two-sided p-values are reported in brackets and coefficients significantly different from zero at the 10%, 5%, and 1% levels are indicated with a *, **, and ***, respectively.

	First Phase				Second Phase			
Dependent variable:	Return	ΔVolatility	ΔSkewness	ΔBid-ask	Return	ΔVolatility	ΔSkewness	ΔBid-ask
				Panel A: Lending period				
Short interest	0.05	-0.08**	-0.03	-0.01	-0.01	-0.03	-0.06	-0.00
	[0.37]	[0.04]	[0.15]	[0.12]	[0.63]	[0.64]	[0.32]	[0.85]
Potential loan to inst. own. (%)	0.15	0.14	-0.24***	-0.01	-0.04	-0.18	-0.17	-0.01*
	[0.52]	[0.27]	[0.00]	[0.43]	[0.33]	[0.40]	[0.17]	[0.08]
Expected loan fee	-0.04	-0.11	0.04	0.01	0.06	-0.88	0.21	-0.01
	[0.81]	[0.17]	[0.45]	[0.61]	[0.74]	[0.18]	[0.79]	[0.76]
Short interest * Available	-0.02	0.03	0.04	0.01	0.01	-0.00	0.06	-0.00
	[0.77]	[0.48]	[0.17]	[0.12]	[0.49]	[0.98]	[0.39]	[0.50]
Potential loan to inst. own. (%)* Available	-0.37	0.05	0.24	0.01	0.04	0.20	0.22*	0.01
	[0.22]	[0.79]	[0.14]	[0.43]	[0.31]	[0.35]	[0.07]	[0.21]
Expected loan fee * Available	0.06	0.16*	-0.06	-0.00	-0.06	0.74	-0.27	0.00
	[0.72]	[0.07]	[0.25]	[0.71]	[0.73]	[0.26]	[0.74]	[0.95]
Available	1.81	-1.81	-1.32	-0.22	-0.18	-1.83	-2.09	-0.02
	[0.35]	[0.19]	[0.17]	[0.14]	[0.66]	[0.33]	[0.24]	[0.78]
	52	52	52	52	28	28	28	28
R-squared	0.14	0.30	0.18	0.17	0.12	0.33	0.20	0.14
				Panel B: Lending period, available stocks only				
Short interest	0.03	-0.05***	0.01	0.00	0.00	-0.03	-0.00	-0.00
	[0.29]	[0.00]	[0.68]	[0.73]	[0.44]	[0.34]	[0.87]	[0.40]
Potential loan to inst. own. (%)	-0.22	0.19	-0.00	0.00	0.00	0.02*	0.06***	-0.00**
	[0.25]	[0.22]	[1.00]	[0.84]	[0.50]	[0.08]	[0.00]	[0.05]
Expected loan fee	0.02	0.05	-0.03	0.00	-0.00	-0.14***	-0.06*	-0.01
	[0.66]	[0.12]	[0.34]	[0.55]	[0.62]	[0.00]	[0.09]	[0.13]
Constant	-0.22	0.31	-0.08	0.00	0.30**	-0.83	0.02	-0.00
	[0.77]	[0.70]	[0.92]	[0.98]	[0.02]	[0.11]	[0.96]	[0.95]
N	32	32	32	32	19	19	19	19
R-squared	0.25	0.18	0.04	0.02	0.03	0.35	0.30	0.11

Dependent variable:	Return	ΔVolatility	ΔSkewness	ΔBid-ask
Panel C: Recall period				
Short interest	-0.04	0.07	-0.00	-0.04**
	[0.12]	[0.21]	[0.81]	[0.02]
Potential loan to inst. own. (%)	-0.13	-0.33	0.01	-0.10
	[0.32]	[0.15]	[0.83]	[0.17]
Expected loan fee	-0.01	0.15	-0.05*	0.04
	[0.83]	[0.25]	[0.05]	[0.18]
Short interest * Available	0.01	-0.01	0.00	0.03
	[0.87]	[0.88]	[1.00]	[0.11]
Potential loan to inst. own. (%) * Available	0.01	0.46	-0.06	0.07
	[0.95]	[0.17]	[0.54]	[0.64]
Expected loan fee * Available	0.01	-0.15	0.06*	-0.05
	[0.90]	[0.31]	[0.08]	[0.15]
Available	-0.47	-1.16	-0.12	-1.18
	[0.63]	[0.62]	[0.84]	[0.18]
N	52	52	52	52
R-squared	0.18	0.14	0.08	0.19
Panel D: Recall period, available stocks only				
Short interest	-0.04*	0.06	-0.00	-0.01**
	[0.10]	[0.20]	[0.88]	[0.02]
Potential loan to inst. own. (%)	-0.12	0.13	-0.05	-0.03
	[0.41]	[0.58]	[0.56]	[0.82]
Expected loan fee	-0.01	-0.00	0.01	-0.01
	[0.80]	[0.99]	[0.57]	[0.57]
Constant	0.57	0.56	0.77	0.87
	[0.38]	[0.70]	[0.13]	[0.20]
N	32	32	32	32
R-squared	0.14	0.07	0.04	0.02

Table VIII:
Marginal Revenue from Lending Out All Securities

Reported are estimates of the net economic returns to lending all of the high-fee securities in the Manager's portfolio. Using the results from our experiment on the impact of lending on loan fees, stock prices, and size of loans, we estimate the return per dollar ownership in each stock from lending activity over both phases of the experiment. Taking all available and withheld stocks, we define the size of the loan as the potential loan stated by the Manager ex ante. On average the potential loan comprises 65-70% of the Manager's ownership. We then multiply this loan amount by the loan fee to arrive at total revenue from lending, where three different loan fees are used: 1) the lending agent's ex ante expected loan fees on all stocks (available and withheld), 2) the actual average loan fee during the experiment (using actual loan fees for the available stocks and Data Explorers loan fees for withheld stocks), and 3) the actual loan fee for available stocks and a hypothetical loan fee for withheld stocks, which is an estimate of the fee the withheld stocks would have received during the lending period if we assume the same discount between the lending agent's expected fee and the actual fee that we measure for the available stocks. The numbers represent the returns (in percent) per year of total manager ownership in the stock that the manager would get from lending out the full potential loan amount of all high-fee stocks.

Potential Fee Revenue per $ Owned of Stocks on Loan (in %)

	First Phase		Second Phase	
	Mean	Median	Mean	Median
Expected loan fee	4.64	1.29	2.71	0.94
Actual loan fee	3.02	1.10	0.60	0.41
Actual (hypothetical) loan fee	2.78	0.83	0.49	0.35

Appendix

In this appendix, we report 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 exogenous shocks to loan supply.

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)				