2018

# ESSAYS ON HEDGE FUND TRADING AND PERFORMANCE

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Digital Object Identifier: https://doi.org/10.13023/etd.2018.237

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# ESSAYS ON HEDGE FUND TRADING AND PERFORMANCE

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the College of Business and Economics at the University of Kentucky

By
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2018

ABSTRACT OF DISSERTATION

# ESSAYS ON HEDGE FUND TRADING AND PERFORMANCE

In the first essay, I create a hedge fund informed trading measure (ITM) that separates information related trades from liquidity driven trades. The results indicate that ITM predicts future stock returns at the trade level, thus is associated with information. By aggregating the most informed trades at the stock level, I find that stocks heavily purchased by informed hedge funds earn a significant alpha. The results indicate that the ITM performs better than some previously documented measures and is robust to two different versions of the measure. The second essay exploits the expiring nature of hedge fund lockups to create a new, within-fund proxy of funding liquidity risk. When funds have lower funding liquidity risk, risk-adjusted performance improves and exposure to tail risk increases. We use fund fixed-effects, a placebo approach, and a regression discontinuity design to establish a link between funding liquidity risk and the ability of funds to capitalize on risky mispricing. The third essay explores hedge fund managers ability to identify and trade on stock mispricing opportunity. We refer to the amount of capital that are is locked up and refrained from redemption as the stable capital, and study how it affects stock mispricing. We find that when funds have more lockup capital, they are more likely to take mispricing risks. Taking all funds together, more stable capital in the industry is driving the reduction or even correction of market-wide stock mispricing. Underpriced stocks benefit more than overpriced stock from hedge funds stable capital.

KEYWORDS: Hedge fund; Fund performance; Informed trading; Funding liquidity risk; Lockup; Mispricing

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Date: Friday 22nd June, 2018

ESSAYS ON HEDGE FUND TRADING AND PERFORMANCE

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# ACKNOWLEDGMENTS

I am most grateful to my Ph.D. advisor, Dr. Chris Clifford, for taking me as his student at my first year and his everlasting support and patience over the past five years. He is the best advisor I can ask for. He is fun and delightful and one of the smartest people I know. I also would like to thank the members of my Ph.D. committee, Dr. Will Gerken, Dr. Paul Childs, and Dr. Jenny Minier.

I would like to thank Dr. Jesse Ellis and Dr. Adam Aiken for their collaboration in the Chapter 2. I also want to take this opportunity to thank the faculty, staff, and students in the Department of Finance at the University of Kentucky. Special thanks also goes to Dr. Brad Jordan and Dr. Russell Jame for their continued support and help.

Lastly, but most importantly, I would like to express my sincere appreciation to my family. I want to thank my wife, Meimei Lin. She is always by my side and helps me go through all difficulties and challenges in my life.

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## 1.1 Introduction

There is ongoing debate as to whether hedge fund managers have superior equity trading skills or the ability to identify mispriced stocks. Ibbotson et al. (2011) find that hedge funds earn positive risk-adjusted returns and outperform mutual funds, based on the performances they self-report to the commercial databases. However, Aiken et al. (2013) attribute the outperformance to database/reporting bias. Moreover, Griffin and Xu (2009) find weak stock trading skills when examining hedge funds' mandated 13F quarterly equity holding reports.

Due to their open-end structure, hedge funds are subject to funding liquidity risk caused by unforeseeable investor flows. As a result, at least some of their trades are the result of liquidity needs rather than information shocks. The trades that funds are forced to make for liquidity reasons tend to be uninformative (Agarwal et al., 2015; Coval and Stafford, 2007; Frazzini and Lamont, 2008). Thus, a test of whether hedge funds have trading skills must try to separate informed trades from liquidity driven trades.

In this paper, I create an informed trading measure ($ITM$) by applying the intuition from the theory work of Bongaerts et al. (2014), who create a stock level private information measure based on aggregated daily trading volume. The intuition is based on the assumption that institutional investors hold an optimal portfolio and only trade when they face a liquidity shock or possess private information. When only experiencing a liquidity shock, managers are better off trading on the most liquid stocks in their investment set and spreading trades among stocks to minimize trading costs. As a result, the liquidity driven trades are approximate to the average trades a fund is making. Conversely, if a manager has private information regarding certain stocks, they will invest in a larger amount that is positively related to the potential gain (or the amount of information) and negatively related to the price impact of their trading volumes. This creates a gap between informed trading and liquidity driven trading. Based on that, I define trade imbalance for each stock as the differences between the dollar amount of trade volumes on the stock and the average trade volumes transacted by the fund in the same day. A trade is more informed if the trade imbalance is larger and the price impact of the trade is higher. Finally, I create the $ITM$ at the daily trade level as the trade imbalance for each trade multiplied by the price impact factor on the stock proxied by the Amihud (2002) illiquidity ratio.

The following examples help to illustrate two distinctive advantages of this $ITM$ measure. First, it takes into account the price impact of the trades. The $ITM$ for \$10 MM of investment into Microsoft (a highly liquid stock) would be much smaller than the same amount invested into an illiquid stock. In addition, it considers the importance of each trade within a fund's own portfolio. The $ITM$ will be higher if a trade deviates more from the fund's average trade. For example, the trade impact of a \$10 MM investment from a several billion dollar fund would be far smaller than the trade impact of the same investment from a \$100 MM dollar fund, and thus much less informed.

In this paper, I use a novel institutional equity trading dataset provided by ANcerno Ltd.[1] to facilitate the creation of *ITM*. ANcerno provides detailed transaction information for each trade executed by institutions, including hedge funds. Transaction data allow for the comparison of different trades executed by a fund during the same day, which is essential for estimating the trade imbalance in this study. High frequency transaction data also provide a more accurate evaluation of funds' trading skills than using 13F quarterly holding data (Puckett and Yan, 2011).

First, I demonstrate that *ITM* at the trade level has predictive power in future stock returns. In a daily regression setting, an increase in each *ITM* decile represents an average of one basis point higher DGTW-adjusted return (Daniel et al. (1997)) over the next day. The most informed purchases (the top *ITM* decile) are associated with eight basis points higher daily return which is significantly different from zero. The most informed sales (the bottom *ITM* decile) are associated with negative future stock returns, however, which is not statistically significant. The findings of insignificant results on the sell side are consistent with Choi et al. (2017) and von Beschwitz et al. (2017), both of which find no evidence of hedge funds having informed trading on selling stocks.

Next, I focus on the most informed trades and examine whether the aggregated hedge fund informed trades can be used to identify mispriced stocks. The role of hedge funds as arbitrageurs to identify and correct mispricing is limited by their funding liquidity risk. The literature find that hedge funds use means such as share restrictions to combat this limit to arbitrage.[2] The funds perform better when they are better protected from investors' withdrawals. Unlike prior literature that treat each fund as a whole, I break down fund's trades based on the motivations, such as accommodating investor flows or capitalizing on private information. Thus, if we focus on transactions that are associated with information, we should better be able to identify a hedge fund's ability to arbitrage away mispricing.

Empirically, I aggregate the trading volume from the most informed buys (high *ITM*) and sells (low *ITM*) to the stock-month level. I sort stocks into quintiles based on the trading volumes in the prior month and focus on the differences between the top and bottom quintile portfolios. The top (bottom) portfolio represents the stocks that are heavily purchased (sold) by hedge funds with informed trading. I employ both a Fama and Macbeth (1973) cross-sectional regression and a time-series portfolio factor approach. Both analyses yield similar results. If we follow a trading strategy that long (short) stocks heavily purchased (sold) by hedge funds with informed trading, we can earn a month alpha of 55-58 basis points, representing an annual alpha of more than 6.6%, which is both economically and statistically significant. The results confirm that hedge funds do possess informed trading when those trades are uncovered by using the *ITM*. I further separated informed trades into purchases and sells and find

[1] The dataset has been used in Jame (2017); Puckett and Yan (2011); Franzoni and Plazzi (2015); and Choi et al. (2017) among others.

[2] See, for instance, Aiken et al. (2018); Aragon (2007); Aragon et al. (2018); Giannetti and Kahraman (2018); and Hombert and Thesmar (2014).

that hedge funds are good at purchasing undervalued stocks while they are not selling overvalued stocks. The outperformance of informed trading mainly comes from long side, but not short side. It is not surprising, given that most of short transactions are to cover the current holding positions instead of short selling[3]. Moreover, by using a unique transaction dataset, von Beschwitz et al. (2017) find that hedge funds leave money on the table by not timing the sales well due to the funding constraints. My results indicate that hedge funds with informed sales are not selling stocks too earlier, rather they are uncovering profitable long positions.

To help put the annual alpha of 6.6% into perspective, I run a similar factor model using the aggregated trading volume from all hedge funds. The alpha for a portfolio that long(short) stocks heavily purchased(sold) by all hedge funds is cut to about half of the size, and is only marginally significant. These results are consistent with Griffin and Xu (2009) in that the average hedge fund has limited stock picking ability. In addition, I test the informed trading measure from Bongaerts et al. (2014) by using the hedge fund sample only instead of all institutions, and also find the alphas in a much smaller magnitude.

There may be concerns that the *ITM* do not capture information but is merely a reflection of institutional herding or price pressure from large hedge fund trades. Brown et al. (2014) find that the institutional herding that are unrelated to information will lead to return reversal in the future quarters. Coval and Stafford (2007) also find a return reversal on stocks which experience extreme mutual fund flows. When the fund's trading is associated with information, the information will be incorporated into the stock price and we would not observe a return reversal in the future. To verify that the informed trading by hedge fund is indeed driven by information, I run the factor models on informed trading portfolios and hold the portfolios for up to two quarters. The results indicate no return reversal and are inconsistent with other non-information related explanations.

One component of the *ITM* is the Amihud liquidity ratio, and as a result the measure is correlated with liquidity and size of the stock. To show that the size or liquidity is not driving the results, I sort stocks by size first and then by informed trading volume in the portfolio models and find similar results. In addition, I run two other robustness tests to ensure that the way I construct the measure does not affect the results. I use daily equity transactions to calculate the *ITM*, as the daily frequency better represents how fund managers make their investment decisions in a timely matter when evaluating information or facing liquidity needs. The measure requires that a fund trades on more than three stocks in a given day to ensure that the benchmark (daily average trades) is reliable. To include all trades in the sample, I change the benchmark to the fund's average trades in the past month and find very similar results. The other robustness test is to aggregate fund's trading at the weekly level, instead of daily level. Funds could split a large order of trade into multiple smaller orders on different days to reduce the price impact. von Beschwitz et al. (2017) define a fund's trades on the same stock within two trading days as the same order. I expand the time period to a week and aggregate the trades on each stock

[3] Choi et al. (2017) indicates that less than 9% of transactions by hedge funds are for short sales.

at the weekly level. The results on the portfolio alpha using weekly *ITM* are in fact stronger than that using daily *ITM*, indicating some evidence of funds splitting large orders.

This paper contributes to the informed trading literature by creating a new measure that focuses on the hedge fund trading. It shows that not only the trade volumes matter, but also the identity of traders and their motives. The paper connects to the limit of arbitrage literature in that the performance of fund's trades is affected by the funding liquidity risks. The results in the paper are also consistent with the hedge fund performance literature that informed trading is associated with positive alphas, while liquidity driven trades are not.

The remainder of the paper is structured as follows. Section 2 reviews the related literature. Section 3 describes the data and the *ITM* measure. Section 4 confirms that the *ITM* measure is information related. Section 5 tests the hedge funds' ability to identify mispriced stocks. Section 6 concludes.

### 1.2 Literature Review

There are disagreements as to whether hedge funds as a group can generate alpha. For example, Ibbotson et al. (2011) finds that hedge funds have positive alpha for every year from 1995 to 2009, even during the financial crisis. However, Aiken et al. (2013) points out that the positive alphas can be attributed to commercial database biases and the entire universe of hedge funds, as a whole, do not perform as well. In addition, by using mandated quarterly 13F filings, Griffin and Xu (2009) confirms that hedge funds are not significantly better than mutual funds regarding equity trading skills. My results also indicate that the average hedge fund has limited trading skills.

Further research has focused on which subsets of hedge funds outperform. The most recent example is Jame (2017), who uses the same dataset as I do and finds that liquidity-supplying hedge funds are skilled equity traders. Sun et al. (2012) confirms that hedge funds with distinctive investment strategies earn superior performance. Gao and Huang (2016) notes that lobbyist connected hedge funds have informed trading when they trade on politically related equities. Another example of hedge funds gaining access to private information is that of Massoud et al. (2011). They find that hedge funds trade on private information gained from the syndicated lending market. I take a distinctive approach by looking into which subsets of trades within a fund outperform.

A recent paper on improving the stock level informed trading measure after Kyle (1985) is Bongaerts et al. (2014), which incorporates order imbalances and price impact of the trades into the measure. Bongaerts et al. (2014) develops a model starting with an optimal portfolio hypothesis and each investor only makes trading when facing liquidity shock or private information shocks. Their model indicates that the investor's trade volumes are positively correlated with private information, and inversely related to price impact parameter. As a result, they create a measure of informed trading by multiplying the stock's price impact parameter by its order imbalance, after aggregating all investors' order flows to the stock level. My paper is distinct from theirs by looking into informed trading from hedge fund's stand of point, instead of aggregating all order flows to the stock level.

The literature use some proxies including activist holdings (Collin-Duferesne and Fos, 2015) and institutional order flow (Boulatov et al., 2013) to infer informed trading. Other papers also attempt to connect hedge fund trading with stock performance. Cao et al. (2016) and Cao et al. (2017) find a significant relationship between stock performance and hedge fund trades. There is one possible drawback of using the above proxies or order imbalance, to infer informed trading. They assume the information from the same amount of trade volumes is indifferent, regardless of which stocks are invested in and where the trades come from. However, a hedge fund investing $10 million in a small cap stock would send a very different signal if the same amount of money was invested in Microsoft. Moreover, which fund invests that $10 million also matters. For a large fund like Fidelity, a $10 million investment would hardly indicate any information, given that its daily average trade may be above that amount. However, a $100 million dollar fund would not invest such an amount into a stock if it does not have any sort of private information. My measure takes both potential issues into consideration, and arguably provides a more precise measure of the informed trading.

This paper takes a similar approach to separate trades as in Cohen et al. (2012) where they separate insider trades into routine and informed insider trading. They define the routine insider as an insider who makes trades in the same month for the past three years, based on the assumption that routine traders are more likely to trade based on liquidity needs. Similarly, I define liquidity driven trades as non-informed trades. Agarwal et al. (2015) finds funds of hedge funds sell more liquid funds first when faced with large outflows from their investors, even though it might not be the optimal decision. Their findings are consistent with my assumption that hedge funds sell the most liquid stocks when facing liquidity shock.

This paper is also related to the limit of arbitrage literature, particularly how funding liquidity risk affects hedge funds' ability to arbitrage. Agarwal et al. (2009) argues those funds with withdrawal restrictions and, as such lower funding liquidity risk, have greater flexibility to pursue risky arbitrage opportunities. Giannetti and Kahraman (2018) finds that hedge funds with greater share restrictions (lower funding liquidity risk) are better positioned to trade against mispricing equities than unrestricted funds. Hombert and Thesmar (2014) believes that hedge funds with greater protection from withdrawals recover more quickly from poor performance. Aiken et al. (2018) finds that hedge fund performance is positively related to the amount of capital that is refrained from withdrawal. In addition, Franzoni and Plazzi (2015) confirms that hedge funds change their trading behavior when funding conditions change. Collectively, these papers support the idea that uncertain investor withdrawal increases funding liquidity risk, thereby affecting a fund's ability to arbitrage away the mispricing. This paper contributes to the literature by demonstrating that hedge funds' trades that are less likely to be subjected to funding liquidity risk are better at capturing mispricing opportunities.

My measure differs from institutional herding in that *ITM* is not associated with future return reversals and, as such, is information related. Brown et al. (2014)

indicates that herding is not related to information and stock returns associated with herding will reverse in future quarters. Koch (2016) finds that mutual funds who lead the herding have an information advantage and outperform, while the followers do not. This is consistent with my finding that parts of trades that are more likely to be associated with information can deliver better performance.

## 1.3 Data

The hedge fund transaction data used in this paper comes from ANcerno Ltd., a consulting firm that provides monitoring trading cost service to institutional investors. The data period is from 1999 to 2010. It ends in 2010 because ANcerno stops providing manager identifier information after 2010. The database provides the following information for each equity transaction: the date of trade, indicator showing if it is a buy or sell, number of shares traded, execution price, and price at the time of trading. For each institution, ANcerno provides both a client identifier and a manager identifier. A client could be a plan sponsor such as CalPERS or a money manager such as Fidelity. A manager is a management company which executes the trades. I follow Jame (2017) and define a fund as a client/manager pair. There are 334 funds in my final sample.

For the stock information, I collect returns, share price, and share outstanding from CRSP, and book value of equity from Compustat. Due to the limitation on the data that ANcerno dataset cannot distinguish the regular sales from the short sells, I supplement the short interest data from Compustat to help identify which part of hedge fund sales is more likely to be short sales.

Table 1.1 provides the summary statistics for hedge funds' trades at the daily level. The number of funds in the sample fluctuates over the years. ANcerno provides the data at quarterly basis and thus it contains dead funds in the historical data. Therefore, the database is free of survivorship bias (Fung and Hsieh, 2000). The fact that hedge funds drop in numbers also provides evidence for that. Hedge funds on average trade 16 stocks a day in my sample. Their trades consist of large amount of purchases and large amount of sells, leading to a much smaller number of net trades.

The database is also free from database/selection bias pointed out by Aiken et al. (2013) where better performing hedge funds choose to report to the database to attract investor flows. Funds reporting to ANcerno are typically due to two reasons: their clients choose to use ANcerno's service, or the fund itself try to reduce its trading costs. If a fund is chosen by its client, the selection bias should be less of an issue as it is not by choice of the fund itself. If a fund chooses to subscribe to ANcerno, there is no reason to believe that the choice is due to its better performance. One could even argue that bad performance might be a better reason to look for a consulting firm to reduce its trading cost.

Ancerno dataset is also free from backfill bias, as it only records fund' transactions after it subscribes. In addition, it does not suffer from other bias, such as hiding transactions from the public. Agarwal et al. (2013) and Aragon et al. (2013) find that hedge funds can seek some confidential treatment or delay their 13F reporting to hide certain transactions from the public. The funds are supposed to submit the entire transactions to ANcerno to enjoy the service they pay for. There is no incentive for them to hide certain transactions. Lastly, another advantage of ANcerno data over 13F holding data as pointed out by Puckett and Yan (2011) is that changes in 13F holding do not capture intraquarter transactions, which they find to be more informative.

### 1.3.1 Inform Trade Measure ($ITM$) Construction

There are two potential issues when using order imbalance or institutional holdings to access the informed trading by institutions. First, it assumes that the same amount of trading would have the same effects on different stocks. However, a $10 million of investment into Microsoft and into a small cap stocks may reveal very different amount of information, as the investment could incur a high price impact on the small cap stock and one would hesitate to do so unless she obtains the private information. Second, it assumes that the source of investment does not matter. A $10 million investment by Fidelity would be treated indifferently from that by a small fund. In reality, a large fund like Fidelity might invest $10 million in multiple stocks regularly and those investments may not contain much information. In contrast, a small fund which rarely invests more than $1 million in a stock would not invest $10 million unless the fund possesses strong private information.

A primary innovation in this paper is that I consider both heterogeneities and create an inform trade measure ($ITM$) at the trade level. I adopt the intuition from the theory work of Bongaerts et al. (2014), who create a stock level private information measure based on aggregated daily trading volume. The basic assumption is that an institutional investor starts with optimal portfolio and only invests based on private information or liquidity shocks. By starting with facing liquidity shocks only, they prove that the investor's optimal trading strategy is to trade the same amount of money on the most liquid stocks. In another word, if there is only liquidity shock, we should observe the investment to happen in the most liquid stocks and the trade imbalance within a fund to be minimal. Next, after adding the private information component, the investor should invest in the informed stocks in an amount that is positively related to the potential gain (or the amount of private information) and negatively related to the price impact of those trades. Therefore, the higher amount of trade imbalance is within a fund, the trade is more informed. The same amount of investment into a highly illiquid stock should contain higher amount of information than that into a liquid stock. I create the trade imbalance measure as the difference between the trade volumes on one stock and the average trade volumes that a fund makes in the same day. Based on all of the above, I create the $ITM$ at trade level as the trade imbalance for each trade multiplied by the price impact factor on the stock, proxy by Amihud (2002) illiquidity ratio.

$$ITM_{i,j,t} = \frac{Vol_{i,j,t} - \frac{1}{N} * \sum_{k=1}^{N}(Vol_{i,k,t})}{|\frac{1}{N} * \sum_{k=1}^{N}(Vol_{i,k,t})|} * Amihud_{j,t} \tag{1.1}$$

where $Vol_{i,j,t}$ is the daily trading volume in dollar amount for $fund_i$ on $stock_j$ at $day_t$. $Amihud_{j,t}$ is the Amihud ratio for stock j calculated based on Amihud (2002) and the monthly average is used. The trade imbalance is scaled by the absolute value of average daily trading volume to account for the size of the funds. I replace *ITM* to zero if the purchase amount is lower than average trading volume when it is positive, or the sell amount is higher than average trading volume when it is negative. One reason for scaling and changing the value to zero is to make sure that the direction of *ITM* is the same as the direction of the trade. It is also based on the assumption that an (absolute) investment amount is less than (absolute) average trade volume, it is less likely to be driven by information.

The measure has taken care of the aforementioned issues. For example, a fund invests \$10 million into Microsoft which hypothetically has an Amihud ratio of 0.2 and invests the same amount into a small cap stock which is more illiquid and has an Amihud ratio of 1. Assume the fund on average invests \$1 million across all stocks in the same day. As a result, the trade imbalances for both stocks are 9. Due to the difference in Amihud ratio, the *ITM* for two stocks would be 1.8 and 9, respectively. Therefore, the \$10 million investing in the small cap stock reveals much higher amount of information. Another issue is regarding the same amount of investment from different funds. I assume that a big fund like Fidelity makes large bet on almost any equity they invest in, such as having \$5 million average trade volume. On the other hand, a small multi-million dollar fund may have less than \$1 million investing in an average stock. The trade imbalance on the stock for Fidelity is 1, but it is 9 for the small fund. With the same Amihud ratio, the *ITM* for small fund's trade is 9 times as large as that for Fidelity's trade. From these examples, we can see that *ITM* is higher for the same amount of trade if it invests in illiquid stocks or it comes from a small fund.

## 1.4 Inform Trade Measure (*ITM*) and Hedge Fund Daily Trades

### 1.4.1 Inform Trade Measure (*ITM*) Summary

I start by investigating the *ITM* at hedge fund trading level. I sort *ITM* into quintiles and Table 1.2 shows the summary statistics for each *ITM* deciles. The bottom *ITM* decile indicates the most informed sells, while the tenth decile is for the most informed purchases. The average *ITM* is much lower in absolute value for decile 2 to 9, and thus the transactions in those deciles are less likely to be informed. Therefore, in the following analyses, I will focus on the transactions in deciles 1 and 10. From the table, we can see that the higher *ITM* (in absolute value) is contributed by both higher trade imbalance (in absolute value) and higher Amihud ratio. When we turn to the difference between deciles 1 and 10, the most informed buy group has lower Amihud ratio but a smaller size than the most informed sell group. There is no significant difference in book-to-market value.

I turn the focus to the future stock performance on these deciles. I follow Daniel et al. (1997) (hereafter DGTW) in computing benchmark-adjusted returns. For each stock, I calculate its excess returns relative to the returns on the DGTW 125 size, industry adjusted book-to-market, and momentum benchmarks. The benchmark returns are scaled by 21/4/2 to match stock returns at daily/weekly/bi-weekly level.

Stocks in the most inform buy group on average earn a 0.11% of DGTW-adjusted daily returns higher than stocks in the most inform sell group, corresponding to a more than 2% monthly return. The outperformance can last up to a month, however, with a much smaller magnitude in monthly return of 0.68%.

It is notable that the stock performance for inform sell decile is not negative or much lower than other deciles as it should be. There are at least two possible explanations: 1) the measure picks up stocks with certain characteristics that predict future performance, such as size or boo-to-market value; 2) some factors may cause the asymmetry between buy and sell transactions. One example for the second explanation is the short sell constraint which limits funds' ability to short certain type of stocks. Moreover, when a fund makes sales decision, it is limited to the stocks in its current portfolio while the fund can purchase any stock in the universe.

To address the first concern, the stock characteristics including size and book-to-market ratio will be included in the regression models. In addition, I will focus on the difference between stocks in decile 1 and 10, where the difference in stock characteristics is much smaller, in the following stock level analyses. More importantly, those stocks represent hedge funds' most informed transactions, which are the focus of this study. Regarding the second concern, I will use the short interest data to attempt the separation of short sells from regular sales to further understand if that contributes to the asymmetries.

### 1.4.2 Daily Regression

Next, I run a pooled, daily return regression on informed trading measure. These results are presented in Table 1.3. The regression model is given in Equation (2) as,

$$Return_{j,t+1} = \beta_0 + \beta_1 \times ITM_{i,j,t} + \sum_{k=2}^{N} \beta_k \times Controls_{j,t} + \theta_i + \gamma_j + \tau_t + \epsilon_{i,j,t} \tag{1.2}$$

where the dependent variable, $Return_{j,t+1}$, is cumulative return of $stock_j$ adjusted by DGTW benchmark return in the subsequent date t+1 (in daily, weekly, biweekly, or monthly) and the variable of interest, $ITM_{i,j,t}$, is the deciles of informed trading measure at day t. In the every second models in Table 1.3, I use the dummies indicating the top and bottom deciles of *ITM* as the main independent variables.

I control for stock characteristics that are known to have effects on future stock returns, including size, Amihud ratio, and Book-to-Market ratio. All continuous variables are normalized to mean of zero and a standard deviation of one. The unit of observation is a fund-stock-day and I include fund fixed effects, stock fixed effects, and time fixed effects in all models. Standard errors are clustered at the fund-level.

The results indicate that future stock return is positively associated with *ITM*. In Model 1, moving up *ITM* in one decile is correspondence to a one basis point increase in stock return, after controlling for size, Amihud ratio, and B/M ratio. Model 5 shows that the trades in the top *ITM* decile (the most informed buy) on average earn about 7.8 basis points higher daily return than the rest trades. The most informed sell decile shows an underperformance of 1.7 basis points in a daily level, it is not significantly different from zero though.

Models 2 to 4 show that the outperformance associated with *ITM* can last up to a month. The magnitude of outperformance is diminishing over the time, with only 4.7 basis points of higher cumulative monthly return for moving up one decile. The trades that belong to the most informed purchase decile outperform for about 48 basis points a month, while the trades under the most informed sell decile underperform for about 12 basis points a month (not significantly different from zero). After controlling for size and Amihud ratio, the performances on most informed sells are negative as predicted; however, it is not statistically significant.

Models 1 to 4 indicate that the *ITM* is associated with future stock performance and could contains information, while Models 5 to 8 points out that *ITM* has stronger predictive power in purchases and show an asymmetry between purchases and sells. To sum up, the table shows that *ITM* can be used to separate trades that are more likely to be informed, especially for informed purchases.

## 1.5 Inform Trade Measure (*ITM*) and Stock Mispricing

The results in Section 4 indicate that hedge funds do have informed trading, and the *ITM* can be used to reveal the most informed trades. In this section, I study whether the most informed trades conducted by hedge funds can be used to identify mispriced stocks. Therefore, I start to analyze hedge fund trades at the stock level by aggregating the hedge funds' informed trading volume to the stock-month level. Due to the asymmetry between purchase and sell, I also look into the stock level informed trading in purchases and sells, separately. I further sort informed trading volume at the stock level into quintiles, and try to understand whether stocks heavily traded by hedge funds are mispriced or not.

### 1.5.1 Fama-Macbeth Cross-Sectional Regression

First, I keep the trades that are identified as the most informed if they fall into the top and/or bottom decile of *ITM*. For simplicity, I refer the informed trades by hedge funds as informed hedge funds. After that, I aggregate those trades to the stock-month level. In each month, the trade volumes are summed up to the stock level and scaled by the share outstanding at the end of the month. When I combine both informed purchases and sells into the stock level, the trade volume represents the net informed trades which take into account the disagreements in informed hedge funds. As a result, the informed trading volume could be a more refined measure. The trade volumes are further sorted into quintiles.

After sorting the informed trading volume into quintiles, I create two dummy variables, inform purchase and inform sell, using the top and bottom quintiles, respectively. Table 1.4 represents the results of monthly Fama-MacBeth cross-sectional regressions where the dependent variable is monthly stock return, adjusted by DGTW benchmark. All independent variables, including inform purchase dummy, inform sell dummy, Amihud ratio, Size, and Book-to-Market, are lagged one month.

Models 1-3 show that stocks purchased by informed hedge funds earn a statistically significant return of 0.4-0.6% per month in the following month, while stocks sold by informed hedge funds earns a statistically insignificant 0.06-0.12% per month. In order to control for size effects, I sort the stocks based on size first. Within each size quintile, I then sort stocks based on informed trading volume. The results are similar in this robustness test, as presented in Models 4 to 6.

The results indicates that informed hedge funds are good at purchasing undervalue stocks, but not as good at selling overvalue stocks. This is consistent with limit of arbitrage theory in two ways. First, funding liquidity risk as one of the limit of arbitrage constrains hedge funds′ ability to trade on mispriced stocks. When I consider trades that are less likely to subject to the funding liquidity risk, it becomes more useful in identifying mispriced stocks. A second possible limit of arbitrage is the short sell constraints, which predict the asymmetry in hedge funds' ability to arbitrage by buying and shorting stocks. Due to the constraints, informed hedge funds are better at trading on undervalued stocks than on overvalued stocks. Overall, those results show that the *ITM* can be used to identify informed trades by hedge funds, and informed trades can be used to identify mispriced stocks.

### 1.5.2 Calendar-time Portfolio Factor Regression

To make results more comparable with the literature in evaluating funds' trading profit, I perform calendar-time factor regressions to evaluate the risk-adjusted performance on the informed trades. The question I ask in this section is what the profits are by following the most informed trades by hedge funds. Although we may not be able to implement this trading strategy due to the availability of hedge funds' trading data in real time, it is important to understand how informed hedge funds perform relative to overall hedge funds or other institutions.

#### 1.5.2.1 Portfolio Results Using Hedge Fund Informed Trading

Using the same approach as in Fama-MacBeth cross sectional regressions, I keep the most informed trades and aggregate the informed trading volume to the stock-month level. In each month, I form equal-weighted monthly portfolios based upon the stocks' lagged informed trading quintiles. Panel A in Table 1.5 shows the results for the alpha and factor betas of the portfolio that long stocks with high informed purchasing volumes and short stocks with high informed selling volumes. In the first model, I regress the average return on the portfolio, on the returns on Fama French 3 factors including market factor, smb, and hml factors (Fama and French (1993)). I add umd factor (Carhart (1997)) in the second model and add an additional Amihud factor (Amihud (2014) in the third model.

The results for portfolio alphas are very similar across Models 1 to 3, showing a positive alpha of around 0.55% per month. That correspondences to an annual performance of 6.6% if we follow both hedge funds' informed purchases and sells. It is also economically significant and the magnitude is large, even comparing to the annual alpha of 3% found on the hedge funds from the commercial databases

(Ibbotson et al., 2011). Models 4 to 6 use portfolios that are sorted by size and then by informed trading volumes. The results are similar.

Due to the asymmetry in performance related to hedge fund purchases and sells, I replicate the portfolios in Panel A using only informed purchases or sells. Models 1 to 3 in Panel B shows alpha and factor betas for portfolios that long stocks heavily purchased by informed hedge funds and short stocks lightly purchased. Models 4 to 6 use hedge funds' informed sells volume only. The results indicate that stocks heavily purchased by informed hedge funds significantly outperform, and the magnitude is larger when comparing to using both informed purchases and sells. The alphas for portfolios that follow informed sells by hedge funds are negative, but not statistically significant.

The portfolio results are consistent with the findings in Fama-MacBeth cross-sectional regressions. It confirms that trades conducted by informed hedge funds are highly profitable, and the positive alpha is mainly driving by informed purchases.

#### 1.5.2.2 Portfolio Results Using Other Measures

One benefit of *ITM* is in its ability to isolate the informed trading by hedge funds and use it in identifying mispriced stocks. However, it would be pointless to do so if we can achieve the goal by using all hedge fund trades. From Griffin and Xu (2009), we know that aggregated hedge fund trading, implied by 13F quarterly holding, are not informed and yield insignificant return. In this section, I test the performance of portfolios following all hedge funds' trades. In each month, I aggregate trading volumes from all hedge funds into the stock level, and scale it by the stock's share outstanding at the end of the month. The trading volumes are further sorted into quintiles. The equal-weighted monthly portfolios are formed based upon the stocks' lagged trading volume quintiles. The top (bottom) quintile portfolio represents stocks that heavily purchased (sold) by hedge funds in the database.

Models 1 to 3 in Table 1.6 show the results for the alpha and factor betas of the portfolio that long stocks with high hedge funds' purchase volumes and short stocks with high hedge funds' sell volumes. The magnitude of alpha earned from following hedge funds' trades is lower than that from following hedge funds' informed trades. In fact, the alphas from Panel A are either marginally higher than zero or indifferent from zero. The results are consistent with the findings by Griffin and Xu (2009) that hedge funds on average do not have equity trading skills.

In Models 4 to 6, I test the performance of stock portfolio sorted using informed trading in the previous literature. In particularly, I adopt Bongaerts et al. (2014) informed trading measure by using ANcerno hedge fund data. The measure is calculated as order imbalance multiple by price impact factor. I replace the order imbalance as difference in aggregate buy volume and sell volume by all hedge funds, weighted by share outstanding. Price impact factor is using Amihud ratio. The measure is more relevant in this paper than its original measure as it is more comparable to informed trading measure (*ITM*) in this paper, where only hedge fund trades are considered.

The results are similar to Panel A and show a lower economic magnitude. The alpha is less than half of the alpha earned from informed trading, and is marginally different from zero. The both tests indicate that the *ITM* in the paper can produce a significant and higher alpha than other measures in the literature. Although we may not find skills in hedge funds' stock trading, we can still identify a group of trades associated with information.

#### 1.5.2.3 Return Reversal

There could be concerns that the positive stock returns are trigged by the autocorrelation of funds' own trades or the price pressure by the trades (Coval and Stafford, 2007), and it is not related to information. Another possible explanation could be institutional herding which is associated with contemporary stock returns and likely induces high order imbalance (Brown et al., 2014). Such events that are not related to information have one common feature  the performance will reverse in the long run.

Table 1.7 explores the informed trading portfolio performance in one or two quarters after portfolio being formed. If there is reversal in two quarters, then the positive performance might not be driven by information. However, Models 1 to 6 show very consistent results that the alpha on those portfolios are positive and insignificantly different from zero. The results indicate that the outperformance of informed trading portfolio are likely driven by information.

#### 1.5.2.4 Robustness Tests

In previous sections, I have shown that the *ITM* is unlike herding measure and associated with information. In addition, it performs better than other measures used in the literature. In this section, I test two variations of the measure to make sure that the results hold under such conditions.

There might be concerns on how hedge funds trade in nature and how it would affect their daily trading. Hedge fund managers may split one large order into several small orders and spread it into multiple days to reduce the price impact. If all trades are conducted in this way, it will impose some concerns on the *ITM* which is based on daily trade. Therefore, I re-create the *ITM* under the weekly frequency. At the weekly level, I aggregate the trades on one stock into weekly level and evaluate it against the average trade in the same week. The results are presented in Models 1 to 3 in Table 1.8. It takes the same approach as in Table 1.5, where I keep the most informed trades based on weekly *ITM*, and aggregate the informed trading volumes into the stock-month level. Lastly, I create portfolios based on the quintile of aggregated informed trading volumes, and calculate the differences in the future returns on the quintile 1 and quintile 5. Table 1.8 presents alpha and betas for factor exposure for such a portfolio. The results from Models 1 to 3 are similar to what we observe in Table 1.5. The alphas are significant across three models and have an economic magnitude similar to previous finding.

Another concern one may have is the fact that a fund might only trade at one or two stocks a day. If that is the case, it is harder to signal information from a single trade. To resolve the issue, I create a *ITM* using a different benchmark  past month's average trade that a fund has, instead of using the same day trade. In this case, no observation is dropped from the sample and single trade can be evaluated. As we can see from Models 4 to 6, the results are consistent with the main results. Table 1.8 confirms that the *ITM* can be used to identify trades that are more likely to be associated with information, and alpha can be earned by following most informed trades conducted by hedge funds.

## 1.6 Conclusion

Under the assumption that funds trade on equity market based on two reasons: liquidity needs and information, I create an Informed Trading Measure (*ITM*) to separate informed trades from liquidity driven trades. It has been shown that trades associated with liquidity reasons are not optimal. This paper looks into the other part of trades and finds that informed trades lead to the good performance.

In a trade level daily regression setting, I confirm that the *ITM* can be used to identify trades that are more likely to be informed. I aggregate the informed trading by hedge funds to the stock level, and find strong evidence that informed hedge funds are good at trading on undervalue stocks. The findings are unlikely to be driven by institutional herding or price pressure impact, as the stock performances associated with *ITM* do not reverse in two quarters indicating that it is associated with information.

Table 1.1: **Summary Statistics**

This table presents summary statistics for ANcerno institutional trading data. The first column reports the number of manager-client pairs (funds) in the ANcerno sample each year or during the sample period between 1999 and 2010. The rest columns show the mean value for each hedge fund trade at daily level. The trade/purchase/sell volumes are in million shares. The transaction value is in $million. The negative numbers indicate sells from hedge funds.

| Year | Number of funds | Number of equity holdings | Trade volume | Purchase Volume | Sell Volume | Transaction Value |
|---|---|---|---|---|---|---|
| 1999 | 121 | 8.91 | -0.05 | 5.88 | -5.84 | -10.37 |
| 2000 | 118 | 8.89 | 0.95 | 7.69 | -6.66 | 4.81 |
| 2001 | 122 | 10.71 | 0.54 | 10.22 | -11.27 | 14.37 |
| 2002 | 123 | 12.45 | -0.15 | 11.42 | -12.01 | -2.73 |
| 2003 | 106 | 11.32 | 0.14 | 16.64 | -15.16 | 7.02 |
| 2004 | 108 | 18.57 | -0.17 | 16.08 | -16.70 | -4.17 |
| 2005 | 116 | 17.08 | -0.32 | 30.68 | -39.77 | -64.22 |
| 2006 | 90 | 20.17 | 1.93 | 40.37 | -35.18 | 101.28 |
| 2007 | 94 | 21.54 | -0.92 | 37.11 | -39.44 | -21.22 |
| 2008 | 69 | 19.54 | -3.70 | 58.47 | -59.93 | -92.94 |
| 2009 | 59 | 25.33 | 4.43 | 62.57 | -60.36 | 60.99 |
| 2010 | 43 | 27.17 | -1.66 | 60.58 | -62.92 | -65.50 |
| Total | 334 | 16.57 | 0.16 | 29.47 | -30.56 | -3.94 |

Table 1.2: **Summary Statistics for Informed Trading Measure**

This table presents the stock characteristics associated with each informed trading measure (*ITM*) deciles. I sort *ITM* into deciles at daily hedge fund trade level. The first column indicates the average *ITM* value for each decile. Trade imbalance and Amihud ratio are the two components consisting the *ITM*. Amihud ratio is calculated based on Amihud (2002) and average across days the same month. Size is the market capitalization measured as stock price multiply by share outstanding, both of which come from CRSP. B/M is book value of equity (obtained from Compustat) devided by market capitalization. Stocks returns are adjusted by the returns on the DGTW 125 size, industry-adjusted book-to-market, and momentum benchmarks (Daniel, Grinblatt, Titman, and Wermers, 1997). The last four columns present the DGTW-adjusted stocks returns, cumulative 1 day, 5 days, 10 days, and 21 days, respectively. The bottom two rows show the difference in means between most informed buy portfolio and most informed sell portfolio and its t-statistics. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| *ITM* decile | *ITM* | Trade imbalance | Amihud | B/M | Size ($billion) | Future DGTW adjusted cumulative stock return | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | Day | Week | Bi-week | Month |
| 1 (Most inform sell) | -0.073 | -23.05 | 0.01 | 0.63 | 3.5 | 0.03 | 0.14 | 0.39 | 0.87 |
| 2 | -0.003 | -11.2 | 0.001 | 0.52 | 10.9 | 0 | 0.1 | 0.21 | 0.55 |
| 3 | -0.001 | -6.28 | 0 | 0.49 | 20.4 | 0.02 | 0.04 | 0.17 | 0.43 |
| 4 | 0 | -3.57 | 0 | 0.46 | 37.3 | 0 | 0.03 | 0.05 | 0.16 |
| 5 | 0 | -1.71 | 0 | 0.42 | 73.1 | 0 | 0.03 | 0.02 | 0.13 |
| 6 | 0 | 1.67 | 0 | 0.44 | 70.6 | 0.05 | 0.07 | 0.19 | 0.29 |
| 7 | 0 | 3.62 | 0 | 0.48 | 33.8 | 0.07 | 0.15 | 0.29 | 0.38 |
| 8 | 0.001 | 6.39 | 0.001 | 0.5 | 18.1 | 0.07 | 0.15 | 0.38 | 0.57 |
| 9 | 0.003 | 11.15 | 0.001 | 0.53 | 9.3 | 0.12 | 0.35 | 0.59 | 0.92 |
| 10 (Most inform buy) | 0.064 | 22.47 | 0.009 | 0.62 | 3.1 | 0.14 | 0.41 | 0.89 | 1.55 |
| Inform buy - inform sell | 0.138*** | 45.52*** | -0.001*** | -0.01 | -0.47*** | 0.11*** | 0.27*** | 0.5*** | 0.68*** |
| t-stat | [45.45] | [210.00] | [-2.890] | [-0.80] | [-5.6] | [4.47] | [5.22] | [7.10] | [6.52] |

Table 1.3: **Stock Return and Informed Trading Measure**

This table reports the pooled daily OLS regression of future DGTW-adjusted stock returns on *ITM* index. The dependent variables are defined the same as variables in the last four columns of Table 1.2. I sort trade level *ITM* measure into deciles and define it as Inform Decile. Inform Buy (Sell) is the dummy variable indicating the decile 10 (1) for *ITM*. Control variables include Amihud ratio, Size, and B/M which are the same as defined in Table 1.2. I include fund fixed effects, stock fixed effects, and time fixed effects throughout. Standard errors are clustered at the fund level. I report t-statistics in square brackets. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
|---|---|---|---|---|---|---|---|---|
| DGTW adj. return | Daily | Week | Bi-week | Month | Daily | Week | Bi-week | Month |
| Inform Decile | 0.0100*** | 0.0220*** | 0.0327*** | 0.0468*** | | | | |
| | [4.326] | [3.371] | [3.521] | [3.091] | | | | |
| Inform Buy (10) | | | | | 0.0784*** | 0.2068*** | 0.3380*** | 0.4849*** |
| | | | | | [3.450] | [3.812] | [3.906] | [3.434] |
| Inform Sell (1) | | | | | -0.0168 | -0.0335 | -0.0500 | -0.1233 |
| | | | | | [-0.991] | [-0.778] | [-0.821] | [-1.129] |
| Amihud ratio | 0.9830** | 6.1346*** | 8.8200*** | 14.7444*** | 0.9740** | 6.1239*** | 8.8137*** | 14.7351*** |
| | [2.355] | [4.932] | [3.753] | [3.703] | [2.354] | [4.957] | [3.784] | [3.720] |
| Size | 0.1442*** | 0.5976*** | 0.7782*** | -0.3377** | 0.1454*** | 0.6024*** | 0.7868*** | -0.3270** |
| | [10.035] | [9.927] | [7.896] | [-2.226] | [10.064] | [10.105] | [8.049] | [-2.169] |
| B/M | -0.0513*** | -0.2348*** | -0.3386*** | -0.0948 | -0.0513*** | -0.2349*** | -0.3387*** | -0.0950 |
| | [-4.167] | [-4.464] | [-4.213] | [-0.933] | [-4.172] | [-4.468] | [-4.217] | [-0.934] |
| Fund FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stock FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Date FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster SE in fund | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 438,006 | 430,799 | 422,637 | 403,319 | 438,006 | 430,799 | 422,637 | 403,319 |
| R-squared | 0.312 | 0.313 | 0.297 | 0.293 | 0.312 | 0.313 | 0.297 | 0.293 |

Table 1.4: **Fama-Macbeth Regression on Informed Trading Volume**

This table reports the estimates from monthly Fama-Macbeth regression from January 2000 to December 2010. The dependent variable is the monthly DGTW-adjusted return in the next month. I keep the most informed transactions and aggregate them into stock monthly level. For each stock-month, I calculate trade volumes that represent more informed trades and scaled by share outstandings (*1000). In Columns 1 to 3, I further sort the volumes into quintiles. In Columns 4 to 6, I first sort stocks into quintiles based on their market capitalization, and further sort stocks in each size quintiles based on informed trade volume. The top (bottom) quintile represents the stocks that are heavily purchased (sold) by informed hedge funds, and I name it Inform Purchase (Inform Sell). All other control variables are defined the same as in Table 1.3. T-statistics, based on standard errors computed from the time-series standard deviation, are reported in parentheses. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | Sort on informed volume | | | Sort on size and informed volume | | |
|---|---|---|---|---|---|---|
| Future monthly stock return | 1 | 2 | 3 | 4 | 5 | 6 |
| Inform Purchase | 0.0062*** | 0.0053*** | 0.0042** | 0.0053*** | 0.0053*** | 0.0055*** |
| | [3.25] | [2.83] | [2.32] | [2.88] | [2.90] | [3.06] |
| Inform Sell | 0.0012 | 0.0006 | 0.0006 | 0.0009 | 0.0008 | 0.0016 |
| | [0.66] | [0.31] | [0.32] | [0.45] | [0.44] | [0.88] |
| Amihud | | 0.0117*** | 0.0036 | | 0.0119*** | 0.0039 |
| | | [3.90] | [1.52] | | [3.96] | [1.61] |
| Size | | | -0.0020** | | | -0.0019** |
| | | | [-2.05] | | | [-2.02] |
| B/M | | | 0.0169*** | | | 0.0171*** |
| | | | [4.56] | | | [4.64] |

Table 1.5: **Portfolio Results Using Informed Trading Volume**

Panel A in this table reports factor exposures for the portfolios that long (short) stocks that are heavily purchased (sold) by informed hedge funds. Each month, stocks are sorted into quintiles based on their lagged informed trade volumes in Models 1 to 3. In Models 4 to 6, I sort stocks by size first and then sort by lagged informed trade volumes in each size quintiles. I calculate the difference between portfolio returns on quintile 5 and quintile 1, and regress on Fama-Frech 3-factor model, adding a Momentum factor, and adding an Amihud factor (Amihud, 2014), respectively. Panel B takes the same approach as in Models 1 to 3 in Panel A, but with a focus on informed purchase (Models 1 to 3) or informed sells (Models 4 to 6). t-statistics are reported in square brackets. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

Panel A. Portfolios sorted on informed trade volumes

| | Inform trading portfolio | | | Inform trading portfolio by size | | |
|---|---|---|---|---|---|---|
| Quintile 5-1 | 1 | 2 | 3 | 4 | 5 | 6 |
| mktrf | 0.0645 | 0.0132 | 0.0108 | 0.0577 | 0.0220 | 0.0158 |
| | [1.314] | [0.250] | [0.202] | [1.203] | [0.422] | [0.298] |
| smb | -0.1561*** | -0.1374** | -0.1149 | -0.1717*** | -0.1587*** | -0.0985 |
| | [-2.757] | [-2.454] | [-1.162] | [-3.102] | [-2.862] | [-1.009] |
| hml | -0.0400 | -0.0792 | -0.0657 | -0.0822 | -0.1095* | -0.0735 |
| | [-0.610] | [-1.195] | [-0.797] | [-1.283] | [-1.669] | [-0.903] |
| umd | | -0.0816** | -0.0827** | | -0.0567* | -0.0598* |
| | | [-2.382] | [-2.388] | | [-1.674] | [-1.748] |
| amihud_factor | | | -0.0299 | | | -0.0798 |
| | | | [-0.277] | | | [-0.749] |
| Alpha | 0.0055** | 0.0057*** | 0.0058*** | 0.0051** | 0.0052** | 0.0056** |
| | [2.489] | [2.645] | [2.627] | [2.374] | [2.466] | [2.567] |

Panel B. Portfolios sorted on informed purchase or sell volumes

| | Informed Purchase portfolio | | | Informed Sell portfolio | | |
|---|---|---|---|---|---|---|
| Quintile 5-1 | 1 | 2 | 3 | 4 | 5 | 6 |
| mktrf | -0.1364** | -0.0840 | -0.0710 | -0.0534 | -0.0410 | -0.0155 |
| | [-2.145] | [-1.219] | [-1.019] | [-0.703] | [-0.491] | [-0.185] |
| smb | 0.2859*** | 0.2668*** | 0.1413 | 0.4421*** | 0.4376*** | 0.1915 |
| | [3.895] | [3.639] | [1.098] | [5.039] | [4.919] | [1.240] |
| hml | -0.0235 | 0.0165 | -0.0586 | 0.1029 | 0.1124 | -0.0348 |
| | [-0.277] | [0.191] | [-0.546] | [1.013] | [1.067] | [-0.270] |
| umd | | 0.0834* | 0.0898** | | 0.0197 | 0.0323 |
| | | [1.861] | [1.993] | | [0.363] | [0.597] |
| amihud_factor | | | 0.1666 | | | 0.3266* |
| | | | [1.187] | | | [1.938] |
| Alpha | 0.0069** | 0.0066** | 0.0059** | -0.0001 | -0.0001 | -0.0017 |
| | [2.419] | [2.361] | [2.032] | [-0.027] | [-0.043] | [-0.485] |

Table 1.6: **Portfolio Results with Other Measures**

Models 1 to 3 in this table reports factor exposures for the portfolios that long (short) stocks that are heavily purchased (sold) by all hedge funds. Each month, stocks are sorted into quintiles based on their lagged trade volumes. I calculate the difference between portfolio returns on quintile 5 and quintile 1, and regress on Fama-Frech 3-factor model, adding a Momentum factor, and adding an Amihud factor (Amihud, 2014) in models 1 to 3, respectively. Models 4 to 6 take the same approach, but using the modified Bongaerts el. al. measure as explained in section 5.2.2. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | Hedge fund trading portfolio | | | Bongaerts et. al. informed trading portfolio | | |
|---|---|---|---|---|---|---|
| Quintile 5-1 | 1 | 2 | 3 | 4 | 5 | 6 |
| mktrf | 0.1051** | 0.0249 | 0.0401 | 0.0778** | 0.0259 | 0.0341 |
| | [2.300] | [0.533] | [0.861] | [2.228] | [0.709] | [0.929] |
| smb | -0.0801 | -0.0508 | -0.1973** | -0.0949** | -0.0760* | -0.1555** |
| | [-1.517] | [-1.024] | [-2.296] | [-2.355] | [-1.959] | [-2.293] |
| hml | 0.1228** | 0.0615 | -0.0262 | 0.0302 | -0.0095 | -0.0571 |
| | [2.010] | [1.046] | [-0.365] | [0.647] | [-0.207] | [-1.010] |
| umd | | -0.1277*** | -0.1202*** | | -0.0826*** | -0.0785*** |
| | | [-4.207] | [-3.992] | | [-3.484] | [-3.305] |
| amihud_factor | | | 0.1944** | | | 0.1056 |
| | | | [2.073] | | | [1.426] |
| Alpha | 0.0032 | 0.0035* | 0.0026 | 0.0023 | 0.0025* | 0.0020 |
| | [1.559] | [1.859] | [1.363] | [1.460] | [1.685] | [1.323] |

Table 1.7: **Portfolio Results with Return Reversal**

This table reports factor exposures for the portfolios that long (short) stocks that are heavily purchased (sold) by informed hedge funds. Each month, stocks are sorted into quintiles based on their lagged informed trade volumes. In Models 1 to 3, I hold the portfolio for 3 months (6 months for Models 4 to 6), and calculate the portfolio returns. The rest procedures follow the same as in Table 1.6. t-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

| Inform volume portfolio | Returns in 3 months | | | Returns in 6 months | | |
|---|---|---|---|---|---|---|
| Quintile 5-1 | 1 | 2 | 3 | 4 | 5 | 6 |
| mktrf | 0.1348 | 0.0752 | 0.1073 | 0.1474 | 0.0932 | 0.1305 |
| | [1.348] | [0.689] | [0.982] | [1.016] | [0.586] | [0.814] |
| smb | -0.2937** | -0.2720** | -0.5814*** | -0.2372 | -0.2175 | -0.5770* |
| | [-2.544] | [-2.341] | [-2.880] | [-1.417] | [-1.284] | [-1.949] |
| hml | -0.2235* | -0.2691* | -0.4542*** | -0.1702 | -0.2116 | -0.4266* |
| | [-1.672] | [-1.956] | [-2.697] | [-0.878] | [-1.056] | [-1.728] |
| umd | | -0.0948 | -0.079 | | -0.0861 | -0.0677 |
| | | [-1.334] | [-1.116] | | [-0.832] | [-0.653] |
| amihud_factor | | | 0.4107* | | | 0.477 |
| | | | [1.864] | | | [1.477] |
| Alpha | 0.005 | 0.0053 | 0.0034 | 0.0057 | 0.0059 | 0.0037 |
| | [1.122] | [1.184] | [0.740] | [0.880] | [0.915] | [0.558] |

Table 1.8: **Portfolio Results with Robustness Tests**

In this table, I replicate Table 1.5 with two alternative measure of the *ITM*. Models 1 to 3 use *ITM* that is based on weekly trades, while Models 4 to 6 create the measure that uses past month's average trade as benchmark. After two alternative measures are created, this table follows exact same steps as illustated in Table 1.5. t-statistics are reported in square brackets. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | Portfolio using weekly measure | | | Portfolio using ITM with different benchmark | | |
|---|---|---|---|---|---|---|
| Quintile 5-1 | 1 | 2 | 3 | 4 | 5 | 6 |
| mktrf | 0.0674 | 0.0094 | 0.0086 | 0.0933* | 0.0359 | 0.0349 |
| | [1.136] | [0.148] | [0.132] | [1.844] | [0.665] | [0.636] |
| smb | -0.1454** | -0.1242* | -0.1165 | -0.1243** | -0.1034* | -0.0939 |
| | [-2.122] | [-1.826] | [-0.980] | [-2.129] | [-1.799] | [-0.925] |
| hml | -0.0318 | -0.0760 | -0.0715 | -0.0220 | -0.0658 | -0.0601 |
| | [-0.401] | [-0.946] | [-0.720] | [-0.325] | [-0.968] | [-0.711] |
| umd | | -0.0923** | -0.0927** | | -0.0912** | -0.0917** |
| | | [-2.219] | [-2.202] | | [-2.596] | [-2.579] |
| amihud_factor | | | -0.0102 | | | -0.0126 |
| | | | [-0.079] | | | [-0.114] |
| Alpha | 0.0064** | 0.0067** | 0.0067** | 0.0055** | 0.0058** | 0.0058** |
| | [2.432] | [2.568] | [2.499] | [2.436] | [2.611] | [2.557] |

# Chapter 2 Funding Liquidity Risk and the Dynamics of Hedge Fund Lockups

## 2.1 Introduction

Theories of efficient capital markets hinge on the concept that mispricing will be arbitraged away by competitive traders. In practice, however, market frictions may affect traders' behavior. In this paper, we focus on one such friction, funding liquidity risk, i.e., traders' ability to attract and retain the capital necessary to trade against risky mispricing (Shleifer and Vishny, 1997). Funding liquidity risk is a critical friction that reduces a fund manager's ability to take risks, and has wide-reaching implications for not only fund performance, but also asset market liquidity, stability and efficiency (Ben-David et al., 2012; Jylha, 2015; and Koch, 2016). As such, there is growing interest in understanding how funds manage funding liquidity risk and overcome limits to arbitrage by placing restrictions on investor withdrawals.[1]

In addition, because of the importance of hedge funds as arbitrageurs, much attention has been paid to how withdrawal restrictions enable hedge funds to take greater risks and correct mispricings.[2] For example, many hedge funds choose to include an expiring lockup provision in their limited partnership agreements. Lockups are redemption restrictions that prevent new capital from being withdrawn for an initial period (typically 12 months), after which time the lockup expires and the shares become redeemable. These provisions are typically incorporated into fund offering agreements at the fund's inception and maintained throughout the life of the fund. The extant literature has focused its attention on the difference in outcomes between funds with and without a lockup, and evidence from these studies suggest that lockups reduce funding liquidity risk, increase managerial flexibility, and ultimately lead to an improvement in performance of between 4-7% a year compared to hedge funds that do not impose a lockup (Aragon, 2007).

However, the presence of a lockup is an endogenous and fixed fund characteristic. It is difficult to disentangle the effects of the presence of the lockup from other time-invariant, omitted factors that may affect fund performance and risk characteristics. For example, Linnainmaa and Moreira (2017) argue that higher skilled managers can use the lockup provision as a means of signaling their type to investors. Thus, the superior performance of lockup funds documented in the literature could reflect the superior skill of their managers (or other omitted factors), rather than the effects of reduced funding liquidity risk. Moreover, any effects the lockup does have on funding liquidity risk are unlikely to be static, as lockups expire over time. This unique feature

[1] For example, there is evidence in the literature that closed-end mutual funds, which do not offer redeemable shares, are better able to invest in illiquid assets and employ risky arbitrage strategies than are open-end funds, which offer daily liquidity to their investors (Cherkes et al., 2009; and Giannetti and Kahraman, 2018.

[2] See, for instance, Agarwal et al. (2009), Akbas et al. (2015), Aragon (2007), Aragon et al. (2018), Giannetti and Kahraman (2018), Hombert and Thesmar (2014), Linnainmaa and Moreira (2017).

of lockups implies that the amount of capital a hedge fund has locked up, and thus, its funding liquidity risk, is actually dynamic and varies across funds and through time.

In this paper, we create a fund-level, time-varying measure of capital restrictions for hedge funds that allows us to examine the dynamic nature of funding liquidity risk. By comparing the time series of capital inflows relative to a fund's lockup period, we are able to estimate the proportion of fund capital that is restricted from withdrawals at any given time. Doing so allows us to disentangle the effects of binding share restrictions from other omitted factors and helps us to better understand the connection between funding liquidity risk, fund performance, and risk taking.

Our sample includes over 3,700 lockup funds from the union of five different hedge fund databases. Our funding liquidity risk variable, *Dynamic Lockup*, is a proxy measure for the fraction of hedge fund capital locked up for each fund in each month in our sample. Figure 2.1 shows the evolution of dynamic lockup over the course of a fund's life, and reveals that the proportion of locked-up capital varies considerably through time. Although new lockup funds begin operations with 100% of their capital locked up, this percentage steadily declines over time. By the fifth anniversary of their inception, the average lockup fund has only 20% of its capital locked up. This raises the question: is the lockup premium attributable to decreased funding liquidity risk created by binding withdrawal restrictions, or the endogenous decision to have a lockup in the first place?

To answer this question, we begin by examining the relation between a lockup fund's performance and dynamic lockup in a regression framework. Our results indicate that a one standard deviation increase in lagged dynamic lockup is associated with a 10 basis points (bps) increase in monthly fund returns. The difference in annual performance between a fund without locked-up capital and a fund that is fully locked up is about 3%.[3] This result holds up to a series of robustness checks, including controls for backfill bias and varying sampling restrictions. Collectively, our findings are consistent with the idea that funds with more protected capital have more flexibility to pursue higher expected return strategies.

Further, because the dynamic lockup measure is time-varying, it enables us to employ a fund fixed effects estimator and control for time-invariant factors that could also be driving the outperformance of lockup funds. For example, fund fixed effects would control for managerial skill, which could also be related to the presence of a lockup, thereby controlling for the potential endogeneity between skill and lockups. After including fund fixed effects, we find that a one standard deviation increase in lagged dynamic lockup is associated with a 7 basis points (bps) increase in monthly fund returns. This implies that, within a fund, decreases in funding risk (i.e., increases in locked-up capital) lead to an increase in future performance. This is an important contribution to the literature, as most of what we currently know about the relation between funding risk and fund performance is derived from comparative studies of time-invariant contractual designs (e.g., open versus closed-end mutual funds, lockup

[3] The standard deviation of dynamic lockup is 34%. When the dynamic lockup changes from zero to 100%, the fund's performance increases by 19 - 29 bps per month.

versus non-lockup hedge funds) or time-series studies of aggregate funding conditions (such as studies of financial crises).[4]

A concern is that because our measure of dynamic lockup is calculated from the time series of capital flows, it could potentially be proxying for other fund characteristics, such as a fund's past performance, age, or size, which have been shown in the literature to be related to future hedge fund returns. While we control for these fund characteristics in our main regressions, we further address this concern in two additional ways.

First, we conduct a placebo test, where we randomly assign a lockup period to non-lockup funds and calculate a placebo value of dynamic lockup using the same methodology as with the lockup funds. If the effect of dynamic lockup is driven by the inputs to the measure (i.e., flows or past performance), rather than the capital restrictions of the lockup, then we should also find a similar relation between dynamic lockup and the returns of non-lockup funds. Instead, we find that non-lockup funds exhibit no relation between returns and their placebo measure of dynamic lockup.

Second, we employ a regression discontinuity (RD) design using the expiration of a fund's initial lockup period as a source of exogenous variation in dynamic lockup. The initial lockup expiration leads to a sharp reduction in the fund's proportion of restricted capital, as the original fund investors simultaneously become eligible to withdraw their investments for the first time. Our identifying assumption is that fund characteristics (other than those driven by restricted capital) should be quite similar in the narrow window surrounding the initial expiration date, allowing us to identify the causal effect of dynamic lockup on fund performance. As with our placebo approach, we compare the returns patterns of lockup funds to non-lockup funds in order to ensure that any changes in returns around the lockup expiration are actually being driven by changes in restricted capital.

Figure 2.2 reveals a sharp reduction in performance around the initial expiration for lockup funds, whereas non-lockup funds exhibit no such reduction around their placebo lockup expiration.[5] We test this further using a fuzzy RD design. We use the initial lockup expiration date to create an instrument for dynamic lockup and test for a difference in returns in the pre- and post-expiration periods using 2SLS regressions. Controlling for fund fixed effects and a linear time trend (i.e., the running variable), we find a significant and positive relation between instrumented dynamic lockup and returns for the sample of lockup funds, while finding no relation for the sample of non-lockup funds. Because of the exogenous nature of the initial lockup expiration, our regression discontinuity results provide strong evidence that changes in capital restrictiveness, rather than other omitted factors, explain the relation between dynamic lockup and fund performance.

While our findings suggest that reductions in funding liquidity risk help to explain a portion of the lockup premium that has been documented in the literature, they do

[4]One important exception is Agarwal et al. (2017), who examine changes in funding risk for hedge funds with a prime brokerage connection to Lehman Brothers following their bankruptcy.

[5]We confirm that this jump in returns for lockup funds is statistically significant using local linear regression methods with a bandwidth of one month around the expiration date. The change is insignificant for non-lockup funds.

not rule out that the endogenous choice of a fund manager to have a lockup may still play a role. To address this issue, we repeat our performance regressions, but pool both lockup and non-lockup funds into the same model. For the non-lockup funds, we assign them a dynamic lockup equal to their true value of zero or a dynamic lockup equal to a placebo value.

We find that even after controlling for dynamic lockup, lockup funds still outperform non-lockup funds by 82-86 bps/year. Collectively, our findings suggest that the lockup premium documented in prior literature is comprised of two effects: a direct, time-varying effect related to binding capital restrictions and an indirect, time-invariant fixed effect related to other differences between lockup and non-lockup funds.

Why would lockup funds outperform non-lockup funds, even when their capital is available for withdrawal? Consistent with previous literature, we hypothesize that the lockup provision serves as a screen for patient investor clienteles and/or creates various incentives for investors to remain patient with their capital, even after their lockup expires. Nanda et al. (2000), for example, argue that mutual funds use share classes to screen for investor clienteles with differing liquidity needs. Further, funds may use the lockup provision to signal their high managerial skill, which should make investors in lockup funds more patient in the face of poor short-run performance (Linnainmaa and Moreira, 2017). In addition, holders of unlocked shares can withdraw capital, but rationalize that any future investments they make in the fund will revert to locked-up status. This effectively raises the shadow cost of redeeming unlocked shares.

To test this conjecture, we examine the flow behavior of lockup funds versus non-lockup funds. We find that even after controlling for dynamic lockup, lockup funds have lower outflows and lower flow-performance sensitivity than non-lockup funds, consistent with a clientele effect. This suggests the lockup provision's contribution to capital stability goes beyond merely the strict prohibition of withdrawals.

Finally, we seek to better understand the mechanism that is driving both the time-varying, direct effect and the fixed, indirect effect of the lockup return premium. Put differently, how do hedge funds with a lockup use their patient capital to generate higher returns? Are they better able to exploit arbitrage opportunities, do they increase their risk to capture factor premiums, or both?

To explore these questions, we run a series of portfolio tests using the Fung and Hsieh seven-factor model Fung and Hsieh (2004) to control for common risks associated with hedge fund investment strategies. We add two factors to this model that are likely to capture trading strategies that benefit from the presence of stable capital: an asset-illiquidity factor and a tail-risk factor. Each month, we sort the sample of lockup funds into portfolios based on their lagged level of dynamic lockup and compare the alphas and factor loadings of the high and low dynamic portfolios.

The portfolio tests reveal that even on a risk-adjusted basis, funds with more stable capital generate higher performance. We find that funds in the highest dynamic lockup portfolio generate monthly alphas that are 16 bps higher than those in the lowest dynamic lockup portfolio, implying that the time-varying component of the lockup premium is not merely driven by differences in factor risk across funds.

Turning to risk taking behavior, we again find that the two sources of the lockup premium affect fund behavior differently. Funds with more binding capital restrictions in place pursue tail risk strategies that suffer when funding liquidity dries up, such as during market downturns. Surprisingly, however, we find no evidence that funds with more binding capital restrictions increase exposure to more illiquid assets. Rather, we find that the indirect effect of the patient clientele allows lockup funds to increase their exposure to illiquid assets, regardless of their dynamic lockup. In other words, lockup funds in general have more patient investors, affording them the ability to hold more illiquid assets regardless of how much of their capital is contractually restricted by the lockup terms. But even patient investors can become impatient during market downturns. Therefore, lockup funds increase tail risk only when they know that their capital is restricted from withdrawals.

### 2.2 Contribution Relative to Prior Literature

Our work contributes to the growing literature that examines how funding risk affects asset manager performance and risk taking. Agarwal et al. (2009) argue that hedge funds with redemption restrictions have more flexibility to pursue risky arbitrage opportunities, and find that hedge fund performance is positively related to redemption restrictions. Giannetti and Kahraman (2018) find that closed-end mutual funds and hedge funds with greater share restrictions are better able to trade against mispricing than unrestricted funds. Franzoni and Plazzi (2015) find that a hedge fund's ability to provide liquidity is particularly sensitive to funding conditions, but that redemption restrictions mitigate the impact of market-wide funding shocks risk on hedge fund liquidity provision. Collectively, these papers support the idea that redemption restrictions reduce funding risk, which in turn increases a fund's ability to capture higher returns from risky strategies. However, because these studies focus on static withdrawal restrictions, they do not disentangle the differential effects of time-varying capital restrictiveness from other omitted differences between restricted and unrestricted funds. Our results support this prior work by showing that even within funds, increases in capital restrictiveness lead to increased fund performance.

Our paper is related to, yet distinct from Hombert and Thesmar (2014) (HT), who argue that funds will set up their capital structure (i.e., choose withdrawal restrictions) to encourage a more stable capital base in order to engage in riskier strategies. Although one of the withdrawal restrictions they examine is a time-varying measure called duration, they do not focus on its time-varying nature. Rather, their paper is primarily concerned with testing the cross-sectional relationship between fund performance and withdrawal restrictions. When HT regresses returns on duration, they combine both lockup and non-lockup funds together in the same sample and do not control for lockup status or fund fixed effects in the regressions. Because a fund's duration is driven by its lockup status, it is unclear whether their duration results reflect the time invariant (i.e., cross-sectional) differences between lockup and non-lockup funds, or the time varying differences in capital restrictions within lockup funds. Because we have a different research question than HT, our methodology focuses on distinguishing between the time-varying and time invariant effects of the lockup, precisely because the presence of the lockup is a pre-existing and endogenous feature of the hedge fund contracting landscape. Our approach differs from that of HT in that we control for lockup status and include fund fixed effects, and also employ both a placebo approach and a regression discontinuity design. As such, we are able to show that fund returns are significantly related to time series changes in capital restrictiveness, as well as to cross-sectional differences between lockup and non-lockup funds.

In addition, our work contributes to the literature concerning the premium of lockup funds. Aragon (2007) finds that funds that institute a lockup earn a substantial premium of between 4-7% over other hedge funds, and he connects this premium to the lockup fund's ability to more efficiently manage illiquid investments that carry higher returns. Subsequent work has shown that lockup funds are more likely to trade against mispriced securities and provide liquidity than non-lockup funds (Giannetti and Kahraman, 2018; Aragon et al., 2018), which points to other sources of the lockup premium. By constructing a dynamic measure of locked-up capital, we are better able to identify the role that binding capital restrictions play in determining the outperformance of lockup funds, while holding constant omitted factors that may be correlated with the presence of the lockup. Though we find that binding capital restrictions do lead to higher performance, they are not the only factor that differentiates funds with a lockup from those without a lockup. Our results suggest that funding risk may also be partially mitigated by simply having a lockup provision in the fund's contract, which can attract more patient investors and lead to the formation of a more stable capital base.

Our work is also relevant to the debate about the optimal structure of redemption rights in the asset management industry. Fama and Jensen (1983) argue that demand deposits reduce agency problems and improve fund governance, since investors can vote with their feet. However, the dark side of unrestricted redemptions is that it hinders managerial flexibility to pursue higher expected return investments (Shleifer and Vishny, 1997). As a result, Stein (2005) argues that competitive pressures to remain open-ended lead to an inefficiently low supply of closed-end managers that are free to engage in risky arbitrage, stabilize prices, and contribute to market efficiency. Though the debate concerning redemption rights often centers on the extremes of open-end versus closed-end funds, the heterogeneous structure that has emerged in the hedge fund industry may be a more suitable solution to the problem of excessive open-endedness. In addition to directly restricting investor redemptions through lockups, our finding of the lockup fixed effect, i.e., that investors behave more patiently with unlocked shares than they do with shares in unrestricted funds, suggests that funds can also combat limits to arbitrage by creating contract mechanisms that screen for and incentivize more patient capital.

### 2.3 Data and Methodology

The hedge fund data in our paper comes from the union of five hedge fund databases: Lipper TASS, BarclayHedge, HFR, Eureka, and Morningstar. Our sample period covers the years 1994-2013 and is formed from snapshots of the commercial databases collected in 2013. We follow Joenväärä et al. (2016) and merge the databases together to remove duplicate funds and share classes through a name matching and returns correlation algorithm. Because each hedge fund database categorizes investment strategies differently, we use the style-correspondence created by Joenväärä et al. (2016) to condense the investment strategy space to 13 different strategies.[6]

We remove funds of funds and non-US dollar denominated share classes. Our final sample contains 13,124 hedge funds with a total of 793,431 monthly return observations. Of these, 3,714 funds (about 29.8% of fund-months) have a lockup in their contract with an average length of 12 months.

In Table 2.1, we present summary statistics for both our full sample (Panel A) and for just those funds with a lockup provision in their contract (Panel B). We note that funds with a lockup have higher average monthly returns than the full sample, which is consistent with prior literature Aragon (2007). Aiken et al. (2015) argue that different share restrictions can serve a complementary role in hedge fund contracting. Consistent with this argument, we find that lockup funds also have longer redemption notice periods and redemption frequencies than non-lockup funds. In an effort to isolate the specific effects of the lockup, we control for these restrictions in our tests.

### 2.3.1 Dynamic Lockup Measure

Our key variable of interest is the time-varying measure of restricted capital we refer to as *dynamic lockup*. We estimate a fund's dynamic lockup using the return and flow history of the fund combined with the lockup period characteristics of the fund. Specifically, for any given fund$_i$, we estimate its dynamic lockup$_{i,t}$ using the following formula:

$$Dynamic\ Lockup_{i,t} = \frac{\sum_{j=1}^{L}(flow_{i,t-L+j} * \prod_{k=j+1}^{L}(1 + r_{i,t-L+k}))}{AUM_{i,t}} \tag{2.1}$$

where $flow_{i,t}$ is the *positive* net flow received by the fund at the end of each $quarter_t$, $r_{i,t}$ is the return in $quarter_t$, $L$ is the length of lockup period measured in quarters, and $AUM_{i,t}$ is the assets under management for the fund. Essentially, each set of inflows is grossed up by the returns it receives each quarter and is assumed to be locked up for the length of the fund's contractual lockup provision. We add these locked up inflows together and scale by total assets to arrive at our dynamic lockup measure.

For example, suppose that a fund begins with $100 million in assets at inception and has a one-year lockup period. We begin by assuming that a lockup fund's capital is fully locked up at the fund's inception (i.e., dynamic lockup = 100%). This new fund is fully locked up until the lockup period ends. If the fund received no additional investments during its first year (and returns were zero), it would have a dynamic lockup = 100% for months 1 through 12. In month 13, the lockup period would have expired, and the fund would become fully unlocked (i.e., dynamic lockup = 0%).

[6]The 13 strategies are: CTAs, Emerging Markets, Event Driven, Fund of Funds, Global Macro, Long Only, Long/Short, Market Neutral, Multi-Strategy, Relative Value, Sector, Short Bias, and Others.

We treat any additional capital inflows the fund receives as new investments subject to the same 12 month lockup period. Using the previous example, let us instead assume the fund received \$20 million of additional capital at the beginning of month 10. This capital would be locked up until month 22. In this case, assuming zero returns, we would define the fund's dynamic lockup as 100% from months 10-12, and 16.67% (\$20MM/\$120MM) from months 13-22, all else equal.

We take care to avoid reporting bias in constructing our measure. Specifically, if a fund's first reporting date is subsequent to its inception month, we can not identify its initial lockup fraction. Thus, for these funds, we let an entire lockup cycle elapse before we can begin calculating the fund's dynamic lockup. For example, we wait 12 months from the time the fund first appeared in the database before calculating the fund's dynamic lockup for a fund with a 12 month lockup period. The ensures that we do not mistakenly assume that the fund has 100% of their capital locked up during their first 12 months in the database.

We find that, on average, only 27.9% of lockup funds' assets are restricted over our sample period. There is a great deal of variation across funds, however. The 25th percentile of dynamic lockup is only 0.9%, implying that in over a quarter of our sample, lockup funds have almost no capital locked up. On the other hand, a fund in the 90th percentile is fully locked-up. The static lockup indicator variable that is typically used in the literature is unable to capture this fact, and would treat both the fully locked and unlocked funds equivalently. Our goal is to exploit this variation to better understand how a fund's capital restrictiveness is related to its performance and risk.

### 2.3.2 Measurement Issues

We acknowledge that our measure is a proxy for the fund's locked-up capital and is likely measured with error. Concerns inherent in empirical hedge fund research, such as database reporting accuracy and unobserved heterogeneity in hedge fund contracts, make it impossible to create a precise measure of each fund's capital restrictiveness. As such, we take a number of steps to ensure that dynamic lockup correlates to the underlying construct it is meant to proxy for and that measurement error does not bias our findings.

One data limitation we face is that gross inflows and outflows are not available in the databases, and thus we are forced to proxy for the gross inflows with positive net inflows. To the extent that some monthly inflows are masked by countervailing outflows in the same month, our dynamic lockup measure would understate the true proportion of locked-up capital.[7] One way we address this issue is by executing a placebo test in Section 4.3, whereby we randomly assign a lockup period to non-lockup funds and calculate a placebo value of dynamic lockup using the same methodology as with the lockup funds. If measurement error is driving our main result, then we should

[7] Because mutual funds disclose both gross and net flows, we can use this setting to illustrate the correlation between dynamic lockup measures calculated with gross vs. net flows. We randomly assign lockups to all mutual funds and reconstruct our measure separately using both gross and net flows and find the correlation between the two measures is 0.85.

see a similar relationship between dynamic lockup and returns in both the lockup and placebo (non-lockup) samples. In addition, our regression discontinuity design in Section 4.4 exploits the exogenous shock to capital restrictiveness that follows the fund's initial lockup expiration. Assuming that the timing of the initial lockup expiration is unrelated to the measurement error embedded in dynamic lockup, our fuzzy RD design (i.e., instrumental variables approach) should provide a consistent estimate of the effect even in the presence of measurement error (Angrist and Krueger, 2001).

Another potential issue is that some hedge fund contracts may have additional redemption features, such as side letters and other sources of contract heterogeneity that are not captured by the hedge fund databases and, thus, are not captured by our dynamic lockup measure. To address this, in robustness tests (Table 2.4) we show results for the sub-sample of hedge funds that operate in equity-only strategies. Equity-only funds are less likely to hold extremely illiquid assets that could necessitate complex redemption terms that are more difficult to summarize accurately in the databases. Thus, the dynamic lockup measure for equity-only funds is less likely to be contaminated by any unobserved database reporting issues.

We also consider the issue that some fund managers have the option to use their discretion to limit withdrawals during extreme markets with discretionary liquidity restrictions (DLRs), such as side pockets, gates, or withdrawal suspensions (Aiken et al., 2015). We are able to calculate dynamic lockup for a small subsample of funds from the Aiken et al. (2015) study and find that our primary results still hold after controlling for DLR use. In addition, we find that funds with higher dynamic lockup are less likely to enact DLRs. This result further bolsters the claim that the dynamic lockup measure captures capital restrictiveness, as funds that have more capital restricted via the lockup mechanism (which is known and agreed to by investors *ex ante*) do not need to rely on the more costly *ex post* mechanism of the DLR.[8]

Before we examine the relation between dynamic lockup and returns, we first confirm that it is related to capital stability. To test this, we regress capital outflows on dynamic lockup (for lockup funds only). These results are reported in Table 2.2. The dependent variable in all models is fund outflows, where outflows are defined as -min(0, netflow). The variable of interest is dynamic lockup and we include a standard set of flow determinants, such as fund size, age, performance, fees, and other contractual restrictions on redemptions. Importantly, the dynamic nature of

[8] One concern is that, if funds can simply restrict capital at their discretion, all funds could have an effective dynamic lockup of 100%. However, this view presumes that enacting DLRs is costless and would imply their use to be common. On the contrary, Aiken et al. (2015) found few funds had ever enacted DLRs outside of the financial crisis in 2007-2009. Moreover, funds that enacted them during the financial crisis suffered *ex post* penalties from investors in the form of decreased flows to both the fund and its fund-family affiliates. In other words, managers cannot withhold investor money with impunity, as investors will be far less likely to entrust future dollars to a manager that refused to honor redemption requests in the past. In fact, the coexistence of *ex ante* contract features such as lockups and notice periods with *ex post* mechanisms such as DLRs speaks to the fact that these features serve different purposes.

our measure allows us to include fund fixed effects (Models 3 and 4) and study how within fund variation in restricted capital affects outflows.

We document a strong, negative relationship between dynamic lockup and fund outflows, even when employing fund fixed effects. For example, in Model 4 we find that a one standard deviation increase in dynamic lockup is associated with a 37 bps decrease in monthly outflows. Based on the average outflow for our sample, this represents a 19% decrease in fund outflows, holding other fund characteristics constant and including both time and fund fixed effects. Since fund fixed effects absorb typical static capital restriction proxies such as redemption frequency, redemption notice periods, and the lockup period, this helps to verify that changes in dynamic lockup correlate to changes in a fund's capital restrictiveness.

### 2.4 Dynamic Lockups and Fund Returns

In this section, we investigate the relationship between the returns of lockup funds and the proportion of locked-up capital (dynamic lockup). As discussed, previous work has focused on the average differences between funds with a lockup and those without. However, our dynamic lockup measure allows us to study within-fund variation in funds with a lockup feature to more clearly identify the link between changes in funding risk and asset manager performance.

#### 2.4.1 Multivariate Regression

We begin by estimating a pooled, monthly return regression, where we restrict our sample to just those hedge funds that have a lockup. These results are presented in Table 2.3. Our regression model is given in equation (2) as

$$Return_{i,t} = \alpha + \beta \times Dynamic\ Lockup_{i,t-1} + \gamma \times Controls_{i,t-1} + \theta_i + \tau_t + \epsilon_{i,t} \quad (2.2)$$

where the dependent variable, $Return_{i,t}$, is the fund's return in month t and the variable of interest, $Dynamic\ Lockup_{i,t-1}$, is the percentage of the fund's capital under contractual lockup in month t-1.

$Controls_{i,t-1}$ is a vector of time-varying controls, including the fund's past performance, flow, age, and size, as well as time-invariant controls, including the fund's minimum investment, fees and other capital restriction features, such as redemption frequency and notice period. All continuous variables are normalized to a mean of zero and a standard deviation of one. The unit of observation is a fund-month and we include time fixed effects in all models. Standard errors are clustered at the fund-level. $\theta_i$ includes style or fund fixed effects, as noted.

We find that dynamic lockup is positively related to future fund returns in all model specifications. In Model 1, we find that a one standard deviation increase in dynamic lockup is associated with an 18 bps/month ($t$-statistic of 11.69) increase in the fund's future performance. In Model 2, where we control for fund characteristics shown to be related to fund performance, we again find a positive and significant relation between dynamic lockup and future fund performance. Specifically, a one standard deviation increase in dynamic lockup is associated with a 10 bps increase in monthly returns ($t$-statistic of 6.26).

One of the advantages of our dynamic lockup measure is that we can capture within-fund variation. As such, in Models 3 and 4 we perform similar tests, but include fund-level fixed effects to control for unobservable fund characteristics that may be related to the performance of lockup funds. For example, if a high skill manager is also better able to negotiate a lockup in the fund contract, including fund fixed effects will control for the potential endogenous relation between managerial skill and the presence of the lockup. Thus, we will be picking up the within-fund changes in dynamic lockup that are unrelated to (time invariant) managerial skill.[9] Using this within-fund approach, we continue to find a significant and positive relation between dynamic lockup and returns. Model 4 reveals that a one standard deviation increase in dynamic lockup leads to a 7 bps/month increase in average returns within a given fund. Overall, this result is consistent with a greater degree of capital stability (i.e. a reduction in funding risk) allowing managers to pursue strategies with greater expected returns. We argue this is an important finding, as there is little within-fund evidence in the prior literature establishing a link between funding liquidity risk and performance.

### 2.4.2 Robustness

We examine the robustness of our findings in the following section and report our results in Table 2.4. Model 1 shows our baseline model results for comparability and is a replica of Model 4 of Table 2.3 (which includes fund fixed effects). The same control variables and fixed effects from Model 4 of Table 2.3 are included in all models, but omitted for brevity.

One particular concern is that our results could be driven by backfill bias. Funds have the option to start reporting to commercial databases after a successful incubation period, and can backfill their performance history with the good performance from the incubation period. This causes the well-known backfill bias, which means that the returns of young funds are biased upwards on average. Because dynamic lockup tends to be highest when funds are young (e.g., see Figure 2.1), our measure may simply be a proxy for the high returns of very young funds.

In Model 2, we address this issue by adding fund age (in years) fixed effects to our main regression. This approach nets out the average performance of funds for each age cohort, and captures the relation between dynamic lockup and performance within each year of age group. We continue to find that dynamic lockup is positively related to future returns.

We model the lockup length as fixed throughout our main analysis. Hong (2014), however, finds that approximately 5% of hedge funds change their lockup length over their life. To mitigate concerns that the manager's choice to change lockup length may affect our results, in Model 3 we utilize monthly snapshots of the BarclayHedge

[9] To remove any concern of a dynamic panel bias, we run a specification of Model 4 of Table 2.3 where we excluded the lagged dependent variable (untabulated). Our inferences are unchanged.

database and exclude any fund that changes its lockup length. Our results are similar. Further, it could be that amongst lockup funds, funds with longer or shorter redemption frequencies could bias our results. Amongst our sample of lockup funds, 60% of funds have quarterly withdrawal frequencies. In Model 4, we re-run our test only on these funds and find similar results.

Another concern is that hedge fund contracts may include additional redemption features, such as side letters, that are not captured by the hedge fund databases and, thus, are not captured by our dynamic lockup measure. Under the assumption that equity-only funds are less likely to hold extremely illiquid assets that could necessitate complex redemption terms, we follow Agarwal et al. (2017) in defining equity-only styles. In Model 5, we repeat our test on the equity only sample and find similar results.

Previously, we calculated dynamic lockup using quarterly flows data to reduce the noise in monthly flow calculations.[10] As a further robustness check, we estimate the dynamic lockup variable using monthly flows in Model 6, and find similar results. In Model 7, we include a delisting return of -50% when a fund leaves a database to account for survivorship bias; our results are similar.

### 2.4.3 Placebo Approach

Dynamic lockup is created using the past flow history of the fund and will mechanically be related to the age, size, performance, and net inflows of the fund. Because these factors have been shown to predict hedge fund performance (e.g., see Boyson (2008) and Aggarwal and Jorion (2010)), one concern is that dynamic lockup is simply a proxy for these factors, rather than a measure of capital restrictiveness. Though we control for these factors in our regression, there remains a potential for nonlinear confounding effects. We address this concern by executing the following placebo test. We randomly assign a placebo lockup period to non-lockup funds, ensuring the distribution of placebo lockup periods matches the actual lockup period distribution of the lockup fund sample.[11] We then use these placebo lockup periods to create a placebo version of dynamic lockup for non-lockup funds using the definition of dynamic lockup given in equation (1). If dynamic lockup is simply a proxy for flows, age, and performance, then it should have similar effects for non-lockup funds as it does for lockup funds. On the other hand, if dynamic lockup reflects changing capital restrictiveness, then it should have no ability to predict future performance for the non-lockup sample, as their capital is not actually locked up.

[10] Some funds report AUM at irregular frequencies even if returns are reported monthly.

[11] By year of fund founding, we obtain the frequency distribution of lockup periods for lockup funds and apply that distribution to non-lockup funds founded in the same year. In 2000, for example, 76% of newly founded lockup funds in our sample have a one-year lockup period. Accordingly, we randomly assign a one-year placebo lockup period to 76% of the non-lockup funds founded in 2000. We repeat the process for each lockup length/frequency combination for each year in the sample. Under this approach, the distribution of placebo lockup periods matches the actual lockup distribution of the lockup fund sample. We find similar results if we assign placebo lockup periods using a propensity score matching approach.

In Table 2.5, we repeat the performance regressions from Model 4 of Table 2.3 (which includes fund fixed effects) for both the lockup and non-lockup fund samples. Model 1 reports the results for the lockup sample (the same results as Model 4 of Table 2.3) for reference. Recall that a one standard deviation increase in dynamic lockup is associated with about a 7 bps/month increase in next month performance for lockup funds. Importantly, we find no relation between dynamic lockup and returns when we consider the sample of non-lockup funds in Model 2. The coefficient for the placebo version of dynamic lockup is 0.00 (t-stat=0.04). The fact that the relation between returns and dynamic lockup is positive and significant for lockup funds, but is insignificant for non-lockup funds, suggests that our results reflect the effects of changing capital restrictiveness, rather than merely the inputs to the dynamic lockup calculation.

### 2.4.4 Regression Discontinuity Design using Exogenous Variation in Dynamic Lockup

To address any lingering concerns that our results are somehow driven by factors other than capital restrictiveness, in this section we employ a regression discontinuity (RD) design, exploiting the fact that the fund's initial lockup expiration represents an exogenous shock to the fund's locked-up capital.

The initial lockup expiration leads to a sharp reduction in the fund's proportion of restricted capital as the original fund investors simultaneously become eligible to withdraw their investments for the first time.

Recall that Figure 2.1 depicts the evolution of dynamic lockup for the sample of funds with 12 month lockup periods, which make up nearly three quarters of the sample of lockup funds. From this figure, we see a steep and discontinuous drop in dynamic lockup around the fund's one year anniversary. This drop is due to the fact that month 13 serves as the initial investors' first opportunity to make withdrawal requests from the fund. This pattern holds similarly for funds with other lockup period lengths. In other words, by construction, dynamic lockup is 100% during the fund's initial lockup period, and drops by an average of 49% in the first month following the initial lockup expiration. This steep drop in dynamic lockup is mechanically due to the initial expiration, and is unlikely to be associated with other variables that may contribute to measurement error in dynamic lockup. Moreover, the effect of any omitted variables, such as managerial skill, are unlikely to also experience a discontinuous change around the initial lockup expiration.[12] By focusing on the months immediately before and after the initial lockup expiration date, we can employ both sharp and fuzzy RD methods to identify the causal effect of changes in locked up capital on fund performance. To further ensure that any differences around the lockup expiration date are due to changes in capital restrictiveness, we also run our RD tests on non-lockup funds that have been assigned a placebo lockup period as

[12] Another advantage of using initial lockup expiration as an exogenous change in locked up capital is that the time periods covered are different across funds, depending on when the funds were founded. Therefore, it is unlikely that common macroeconomic shocks are creating a relationship between fund returns and the amount of locked capital.

in Section 4.3 (i.e., the placebo group). If there are unobserved characteristics that are related to returns and exhibit a similar discontinuity to dynamic lockup, yet are unrelated to locked up capital, then we should find a similar performance pattern for the placebo group. If we do not see a similar performance pattern for non-lockup funds, it lends greater credence to the conclusion that the change in locked up capital is what is driving the performance changes we document in lockup funds.

Our sharp RD results are presented in Figure 2.2, which shows the average monthly returns of funds in event time relative to their initial lockup expiration date for both lockup (Panel A) and non-lockup funds (Panel B). To ensure we measure when investors are actually able to withdraw their capital, we adjust the lockup expiration for the fund's redemption period.[13] Both sets of funds show a general decreasing relation between returns and age, which has been found in the prior literature. However, lockup funds experience a sharp drop in returns around the lockup expiration date. Using a local linear regression approach, we test for the difference in returns between the month before and the month after expiration and find a local Wald estimate of -0.0024 ($z$-score = -1.92), indicating a statistically significant 24 basis point drop in returns around the initial lockup expiration date. In contrast, we find no such drop for the non-lockup funds around their placebo lockup expiration date (local Wald estimate of 0.0003 with a $z$-score = 0.35). The lack of effect for non-lockup funds suggests that it is the drop in locked up capital, and not other fund characteristics, that causes the large performance drop around the lockup expiration.

In order to directly connect the dynamic lockup measure used throughout the paper with the initial lockup expiration, we use a fuzzy RD design in Table 2.6. Because dynamic lockup is highly and discontinuously related to the the initial lockup expiration, we can use the initial lockup date to define an instrument for dynamic lockup, using the two stage least squares approach for fuzzy RD as suggested by Hahn et al. (2001). We use the same sample described above, only including fund returns within the 24 month window around the initial lockup expiration. We create an indicator, After Lockup Expiration, for whether or not a return is in the pre- or post-expiration sample. We then use this indicator as our instrument for dynamic lockup. Importantly, we also control for a linear time trend (Months since Lockup Expiration), which in this case is often referred to as the "running variable" in fuzzy RD designs. By controlling for a time trend, we ensure that the After Lockup Expiration indicator does not simply proxy for the overall association between fund age and performance. That way the indicator will satisfy the exclusion restriction, since, as we argue above, it is not correlated with any factor that may affect fund performance except for dynamic lockup when used locally around the initial expiration date. We also include the same controls, including fund fixed effects as those used in Table 2.3, Model 4. Models 1 and 2 of Table 2.6 show the results for lockup funds.

Model 1 shows the results of the first stage model predicting dynamic lockup. Not surprisingly, dynamic lockup is highly negatively related to the After Lockup

[13] Specifically, we add the redemption frequency period ÷ 2 to the initial lockup expiration date, thereby assuming the distance to the next distribution date is uniformly distributed. We do not add the notice period, since investors can give notice prior to the lockup expiration and still exit the fund on the first available distribution date.

Expiration instrument. The second stage estimates use the instrumented dynamic lockup to explain monthly fund returns around the initial expiration date and are given in Model 2. Here we find that instrumented dynamic lockup is positive and significantly related to fund returns ($t$-stat = 3.76). Thus it appears that exogenous decreases in dynamic lockup due to the initial lockup expiration lead to decreased returns for lockup funds.

We repeat the same two-stage analysis in Models 3 and 4 for non-lockup funds (using placebo dynamic lockup). Although the After Lockup Expiration instrument is highly related to placebo dynamic lockup for non-lockup funds (Model 3), we find no relation between instrumented dynamic lockup and returns in the second stage regression (Model 4). In other words, the relation between fund performance and dynamic lockup around the initial lockup expiration is only present for lockup funds. These results corroborate our sharp RD results, and lend further support to the notion that reductions in locked up capital cause reduced fund performance.

### 2.5 The Lockup Fund Premium and Patient Capital

In Section 4, we found that dynamic lockup was a significant predictor of fund performance. We now examine the degree to which dynamic lockup can explain the lockup fund premium, i.e., the performance difference between lockup and non-lockup funds found in prior literature. If the lockup premium is solely due to the fact that lockup funds have more restricted capital than non-lockup funds, then the lockup premium should disappear after controlling for dynamic lockup. On the other hand, if the presence of a lockup reflects endogenous characteristic differences between lockup and non-lockup funds, then the lockup premium could remain even after controlling for the amount of contractually restricted capital. To test this, in Table 2.7 we pool both lockup and non-lockup funds together in the same returns regression and control for both dynamic lockup and an indicator for whether the fund is a lockup fund or not. The variable of interest in these regressions is the lockup fund indicator, whose coefficient reflects the residual lockup fund premium left after controlling for dynamic lockup. The regressions include the same controls as in Model 2 of Table 2.3.[14] We assign two different measures of dynamic lockup to non-lockup funds. In Model 1, we assign their actual dynamic lockup, which would be zero for all non-lockup funds. In Model 2, we use the placebo version as defined in Section 4.3. This allows us to similarly control for any measurement issues that may impact the dynamic lockup calculation.

We note that in both models, dynamic lockup is positive and significant, meaning that after controlling for whether the fund is a lockup fund or not, funds with more restricted capital earn higher returns. These results are in line with our results in Section 4. Strikingly, however, we find that even after controlling for dynamic lockup, the lockup fund indicator remains positive and significant. For example, the results of Model 1 indicate that, even after controlling for their restricted capital, lockup funds

[14]We do not include fund fixed effects because they would not allow us to identify the lockup fund indicator, as it is a time invariant fund characteristic.

earn an extra 7 bps per month as compared to non-lockup funds. We interpret these results as evidence of a fixed lockup premium (lockup fixed effect) – lockup funds earn a premium relative to non-lockup funds that is unrelated to the proportion of capital that is locked up.

Why would lockup funds outperform non-lockup funds, even when their investors can withdraw their capital? We posit that this fixed lockup premium could, in part, be due to lockup funds having a more patient capital base than non-lockup funds. There are several reasons why investors in lockup funds may be more patient than other investors. Investors that are willing to accept a lockup *ex ante* might be long horizon investors who understand that earning a risk premium over the long run (such as an illiquidity premium) entails suffering lower returns in some states of the world. Linnainmaa and Moreira (2017) argue that managers and investors agree to lockups in order to allow both parties to capture gains from long-term arbitrage opportunities and avoid incentives to focus on short-run performance. Thus, a lockup could serve as a screening device to attract investors that are less likely to react quickly to poor performance. In other words, lockup fund managers may have more skill, encouraging investors to not only accept a lockup in the first place, but also to be more patient in the face of subsequent poor short term performance. In addition, the presence of a lockup raises the cost to re-enter the fund. Investors may be less willing to withdraw capital if they know that any capital they choose to reinvest with the fund will again be subject to a lockup. Finally, investors may be more patient with their own unlocked capital if they know that other investors' restricted assets are enabling all investors in the fund to earn a risk or liquidity-based premium.

We test this conjecture in Table 2.8 by exploring how the presence of a lockup relates to fund outflows and the relationship between outflows and fund performance. As in Table 2.7, we pool all lockup and non-lockup funds together and regress a fund's forward monthly outflow on lockup characteristics and other fund controls known to affect fund flows. Because we are interested in the flow-performance relation across lockup and non-lockup funds, in each model we include a measure of lagged annual fund performance interacted with an indicator for whether the observation comes from a lockup fund or a non-lockup fund. In each model, we include the same controls as in Model 2 of Table 2.2, including dynamic lockup.

We begin by examining the coefficient on *Lockup Fund*. Because we control for dynamic lockup in each model, the *Lockup Fund* indicator reflects the average differences in outflows between lockup and non-lockup funds after controlling for the contractually restricted capital of lockup funds. Across both models, *Lockup Fund* is negative and statistically significant at less than the 1% level. For example, the coefficient from Model 1 suggests that the outflows of lockup funds are 38 bps per month lower than non-lockup funds, all else equal. This implies that even lockup funds with very little contractually restricted capital are less likely to receive redemption requests than non-lockup funds, supporting the notion that lockup funds have more stable capital.

Next, we turn to the relation between flows and performance. If lockup fund investors are more patient, we expect their redemption requests to be less sensitive to performance. We find robust evidence that this is indeed the case. In Model 1, where we measure performance using trailing twelve month returns, the relation between flows and performance is predictably negative for both lockup and non-lockup funds. That is, on average, as performance decreases, outflows increase for both sets of funds.

To capture the differential effect of flow-performance sensitivity for lockup funds, we interact past performance with the *Lockup Fund* indicator variable. Our results indicate that lockup fund outflows are significantly less sensitive to performance than the outflows of non-lockup funds. While a one standard deviation decrease in past performance increases outflows by 170 bps/month for non-lockup funds, a similar reduction in performance increases outflows by only 92 bps/month for lockup funds. That is, after controlling for other fund characteristics such as age and size, and controlling for the dynamic lockup of the fund, lockup fund investor flows are 46% less sensitive to performance. Our inferences are similar when we use a low return indicator (defined as below median annual returns) as our measure of performance in Model 2. Again, both lockup and non-lockup funds have higher outflows if they have low returns, but non-lockup funds lose significantly more capital than the lockup funds do (135 bps for non-lockup funds vs. 95 bps for lockup funds). The differences in the flow-performance relationship between lockup and non-lockup funds is significant in both models at the less than 1% level.[15]

Collectively, the results of Table 2.8 suggest that lockup funds are less likely to receive redemption requests, and their investors are less sensitive to performance when requesting redemptions, even after controlling for performance and the amount of capital that lockup funds have contractually restricted. Our findings indicate that in addition to maintaining stable capital through direct capital restrictions, lockup contracts also allow funds to maintain a more stable base of unrestricted capital. Viewed from the limits to arbitrage perspective, the patient unlocked capital of lockup funds helps explain how lockup funds are able to realize superior performance even after their lockups expire.

Of course, if lockup funds outperform non-lockup funds, a natural question may be, why don't all funds have a lockup? It is important to remember that lockups do not come without a cost. Namely, increased realized performance comes at the cost of more illiquid shares (Aragon, 2007). Moreover, as discussed above, the presence of a lockup is the endogenous outcome of unobserved bargaining between managers and investors. Investors may only be willing to accept a lockup from superior managers, or from managers that plan to take greater risks in the face of their more patient/restricted capital base.[16]

### 2.6 Risk Models

The results thus far demonstrate that the lockup premium is a function of two separate mechanisms. One is dynamic and related to how much capital the manager has under

[15] We also find similar differences in flow-performance sensitivity when we examine implied net flows instead of outflows (unreported).

[16] Joenväärä et al. (2018) note that investors are less able to take advantage of hedge fund performance persistence when lockups and other liquidity constraints are considered. Investors may be willing to trade-off less flexibility for the lockup premium.

contractual lockup (i.e., dynamic lockup). The other is time-invariant and associated with the presence of a lockup feature in the fund's contract (i.e., the lockup fixed effect). In this section, we ask if this return premium is related to manager skill, or if more restricted capital allows funds to take more risk. For example, perhaps managers who are able to negotiate a lockup *ex ante* are also more skilled. If this is the case, then we should observe positive alpha for managers with a lockup, independent of the percentage of capital under contractual restriction. However, perhaps limits to arbitrage are relaxed when dynamic lockup is higher, making managers better able to engage in more complex arbitrage activities without the fear of investor outflows. In this case, estimates of alpha should increase as the percentage of capital under lockup increases. Finally, managers with less fragile capital might also earn higher returns from increased factor exposures. In this situation, lockup funds would have larger betas and these betas may increase as the amount of capital under lockup is increased.

We focus on two risk characteristics that we hypothesize to be especially related to lockup fund characteristics: asset illiquidity and tail risk. We focus on asset illiquidity because the past literature has suggested that one of the reasons lockup funds earn a premium is that they are better able to invest in illiquid assets given their relatively stable capital base (Aragon, 2007). We proxy for asset illiquidity using portfolio betas on the lagged market return. Lagged market return betas can reflect autocorrelation in hedge fund returns (Asness et al., 2001; Getmansky et al., 2004), which is often interpreted as a sign that a fund owns illiquid and/or difficult-to-value securities.

We focus on tail risk because it is well known that hedge funds have significant exposures to nonlinear systematic risk factors.[17] Indeed, many hedge fund strategies are characterized as "picking up nickels in front of bulldozers"[18], which is to say they pursue strategies that have high Sharpe ratios or positive alphas, but are also more likely to incur a substantial loss, especially in periods of market stress. In particular, Agarwal et al. (2017) (henceforth ARW), document a strong link between a hedge fund's returns and its exposure to market crashes, which they refer to as "tail risk". Funds with higher tail risk have higher average returns, but are more likely to suffer losses during market downturns.

High tail risk could be particularly problematic for funds that are exposed to funding liquidity shocks. Large losses during periods of market stress are more likely to trigger redemption requests that could lead to loss spirals, as in Brunnermeier and Pedersen (2009) (i.e., redemption requests cause fire sales, which hurt performance, which cause more redemption requests, etc.). Understanding these consequences *ex ante*, we would expect funds to manage their tail risk exposure to be aligned with their funding liquidity risk. Thus, funds with greater (less) funding liquidity risk should be less (more) willing to pursue high tail risk strategies.

We follow ARW and create a tail risk factor defined as the difference in returns of funds in the top quintile of tail risk and the returns of funds with zero tail risk

[17] Some examples include Fung and Hsieh (1997, 2004); Mitchell and Pulvino (2001); Agarwal and Naik (2004); Brown et al. (2012); Jiang and Kelly (2012) and Bali et al. (2007).

[18] From Roger Lowenstein's account of the collapse of Long Term Capital Management, "When Genius Failed".

(henceforth, *Tail*).[19] We then measure tail risk exposure as the beta of the dynamic lockup tercile portfolios with respect to *Tail*.[20]

We report the results of calendar-time factor model regressions in Table 2.9. Among those funds with a lockup, we form equal-weighted monthly portfolios based upon the fund's lagged dynamic lockup tercile. Furthermore, we adjust each portfolio's return by netting out the average return of a non-lockup portfolio whose placebo dynamic lockup is in the same tercile. We do this as a way to control for the inputs to dynamic lockup that are also likely to be important determinants to fund performance and risk taking. For example, if lockup and placebo funds in the high dynamic lockup tercile share certain characteristics that are associated with higher returns (e.g. both are smaller and younger funds), then subtracting placebo returns will adjust for that source of premium. All alpha and factor betas reported in Table 2.9 are, therefore, in excess of the abnormal return earned or the risks taken by the placebo group in that tercile.

To illustrate how the placebo adjustment can reveal both the time varying and time invariant components of the lockup premium, we present excess returns (no risk adjustment) of these placebo-adjusted returns across the terciles of dynamic lockup in Panel A. We find that Low, Mid, and High dynamic lockup portfolios earn returns of 16, 20, and 34 bps/month, respectively. The difference between the High and Low portfolios is a statistically significant 18 bps/month. Given that portfolio excess returns are net of the returns of the placebo funds, these results reveal two effects. First, consistent with our results in Section 4, excess returns increase monotonically with the amount of capital under lockup, revealing the time varying, dynamic lockup-based component of the lockup premium. Second, consistent with our results in Section 5, the excess returns are significant in each tercile, meaning that lockup funds earn a premium over non-lockup funds regardless of the amount of locked-up capital the fund has, indicating the presence of a time invariant, lockup fixed effect.

In Panel B, we use the same tercile portfolios but adjust for several risk factors to illustrate the relation between lockup characteristics and fund risk taking. In each model, we control for asset illiquidity and tail risk, along with the the seven risk factors from the Fung and Hsieh (2004) model for hedge fund returns.[21]

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[19] See Agarwal et al. (2017) for more details on how to construct the tail risk factor.

[20] Note that although Agarwal et al. (2017) create the tail risk factor from fund-level measures of tail risk, we do not use fund-level tail risk measures in our analysis. The reason is that ARW's fund-level measures reflect a fund's trailing tail risk over the past 24 months, whereas dynamic lockup changes rapidly through time as lockups expire. Thus, the relation between a fund's dynamic lockup and its fund-level tail risk measure in fact reflects the relation between a fund's historical average tail risk and its current proportion of locked-up capital, which is not the relation we are interested in studying. Because we change the composition of the dynamic lockup portfolios every month based on their current dynamic lockup, the correlation between the dynamic lockup tercile portfolio returns and the tail risk factor is more likely to reveal contemporaneous relation between dynamic lockup and tail risk exposures.

[21] The Fung and Hsieh (2004) model includes the following returns: the S&P 500 total return, a size spread return (Wilshire Small Cap 1750 - Wilshire Large Cap 750), a bond market factor (quarterly change in the 10-year constant maturity treasury yield), a credit spread factor (quarterly change in the Moody's Baa yield less the 10-year treasury constant maturity yield), and three

The risk factor models reveal several interesting patterns. We first note that the alphas are increasing across each tercile of dynamic lockup, and that the high tercile portfolio earns an alpha that is a statistically significant 16 bps per month larger than the alpha of the low tercile portfolio. Thus, even controlling for common risk factors, funds with more restricted capital earn higher returns. Interestingly, the alpha in the low tercile is insignificant. That means that non-lockup funds and lockup funds with low levels of dynamic lockup earn similar risk-adjusted returns, even though lockup funds in the low tercile outperform non-lockup funds without a risk adjustment (Panel A). This result suggests that, at least in the case of low tercile funds, the outperformance of lockup funds is in part due to their greater risk taking than non-lockup funds.

We see evidence of increased risk taking when we examine the asset illiquidity betas, which are positive and significant in all models. This means that lockup funds own more illiquid assets than non-lockup funds, even within the same dynamic lockup tercile. However, the betas are not significantly different from one another across the terciles, meaning that lockup funds' illiquid asset exposure is independent of the amount of contractually restricted capital. In other words, even lockup funds with very little restricted capital have high exposure to illiquid assets, which helps account for their raw return advantage over non-lockup funds. This result is striking because a common conjecture in the literature is that lockup funds own more illiquid assets because they have more restricted capital (Aragon, 2007).

Panel B of Table 2.9 also reveals that tail risk exposure is positively related to dynamic lockup in the cross section of funds. The tail risk beta for the bottom tercile funds is insignificant. On the other hand, funds in the top tercile of dynamic lockup have a positive and significant tail risk beta, and the difference between the high and low tercile is significant at the 1% level. This implies that funds with more locked-up capital increase their exposure to tail risk, which is consistent with the idea that greater funding stability provides a fund manager additional flexibility to pursue risky arbitrage.

Why would dynamic lockup be related to tail risk, but not the decision to hold illiquid assets? We believe this asymmetric result could be due to the different nature of the two premiums. Illiquid assets have high transaction costs, and therefore must offer higher gross returns to encourage investors to hold them. Thus, investors with long horizons can earn a net illiquidity premium by buying and holding illiquid assets, effectively amortizing compensation for expected transaction costs. Because lockups expire, dynamic lockup represents a relatively short-term measure of binding withdrawal restrictions. For example, suppose a fund has 100% of its capital locked up for the next two months, but afterwards will revert to an unlocked fund. Buying more illiquid assets today will not necessarily be an attractive strategy for this fund, as the manager should still expect to liquidate some proportion of the illiquid assets and bear the high transactions costs in two months when the fund becomes un-

trend-following factors for the bond market, the currency market, and the commodities market. See David Hsieh's web page at http://faculty.fuqua.duke.edu/%7Edah7/HFRFData.htm for a complete description.

locked. However, lockup funds in general have more patient investors, meaning that they can afford to hold more illiquid assets regardless of how much of their capital is contractually restricted by the lockup period.

On the other hand, the premium earned from holding greater tail risk stems from the possibility that a fund will hold the asset in a state of the world where the market crashes and withdrawal requests appear at the same time. In this case, a relatively short-term withdrawal restriction could be quite effective. The same fund whose lockup is expiring in two months could hold high tail risk assets this month with the knowledge that if the market experiences a shock and their assets decline in value, the fund will not be forced to sell at the bottom and get caught in a short-term loss-spiral (Brunnermeier and Pedersen, 2009). Once the fund reverts to unlocked status, it can reduce its tail risk to an appropriate level given the change in its funding liquidity risk. In other words, perhaps lockups encourage a more patient investor clientele on average, but even patient investors can become impatient during a crisis. Therefore, lockup funds can afford to take more tail risk when they know that their capital is restricted from withdrawals.

## 2.7 Conclusion

Funding liquidity risk, i.e., the risk that traders will not be able to obtain outside funding to take advantage of attractive investment opportunities, is a central friction in models of financial market disequilibrium and limits to arbitrage. It is crucial that we understand how funding risk influences the performance and risk taking of hedge funds because of their key role as arbitrageurs that provide liquidity, stabilize markets, and help push prices to their fundamental value. In this paper, we create a novel proxy of funding liquidity risk (dynamic lockup) that is both a fund-level and time-varying measure, which allows us to better identify the connections between funding liquidity risk, performance, and risk-taking in the cross section of hedge funds.

We document a strong, positive association between dynamic lockup and hedge fund performance and risk taking. This effect is robust to including several fund-level control variables, different sample formations, and changes in how we measure dynamic lockup. Moreover, our results hold when we include fund-fixed effects in the regressions, meaning that within-fund changes in capital restrictions are associated with improvements in fund performance. Finally, we use a placebo control group and an exogenous change in dynamic lockup created by the expiration of the initial lockup period in order to isolate the effect of locked capital on fund returns.

We also find that regardless of how much capital lockup funds have restricted, they still outperform non-lockup funds by approximately 1% per year. This lockup fixed effect appears to be driven by increased risk taking by lockup funds as compared to non-lockup funds, including an increased exposure to illiquid investments. We conjecture that lockup funds take greater risk perhaps because the lockup provision screens for patient investors and incentivizes incumbent investors to be patient after the lockup expires. This allows lockup funds to retain more stable *unrestricted* capital, even after the lockup expires. Consistent with this conjecture, we find that lockup funds have lower outflows and their flows are less sensitive to performance, even after controlling for dynamic lockup. Collectively, our results suggest that funds can combat limits to arbitrage by not only directly restricting their investors' withdrawals, but also by creating mechanisms that incentivize a more patient investor base.

Figure 2.1: Percentage of Capital Under Lockup by Fund Age

This figure examines funds with an annual lockup and presents the percentage of capital under lockup.

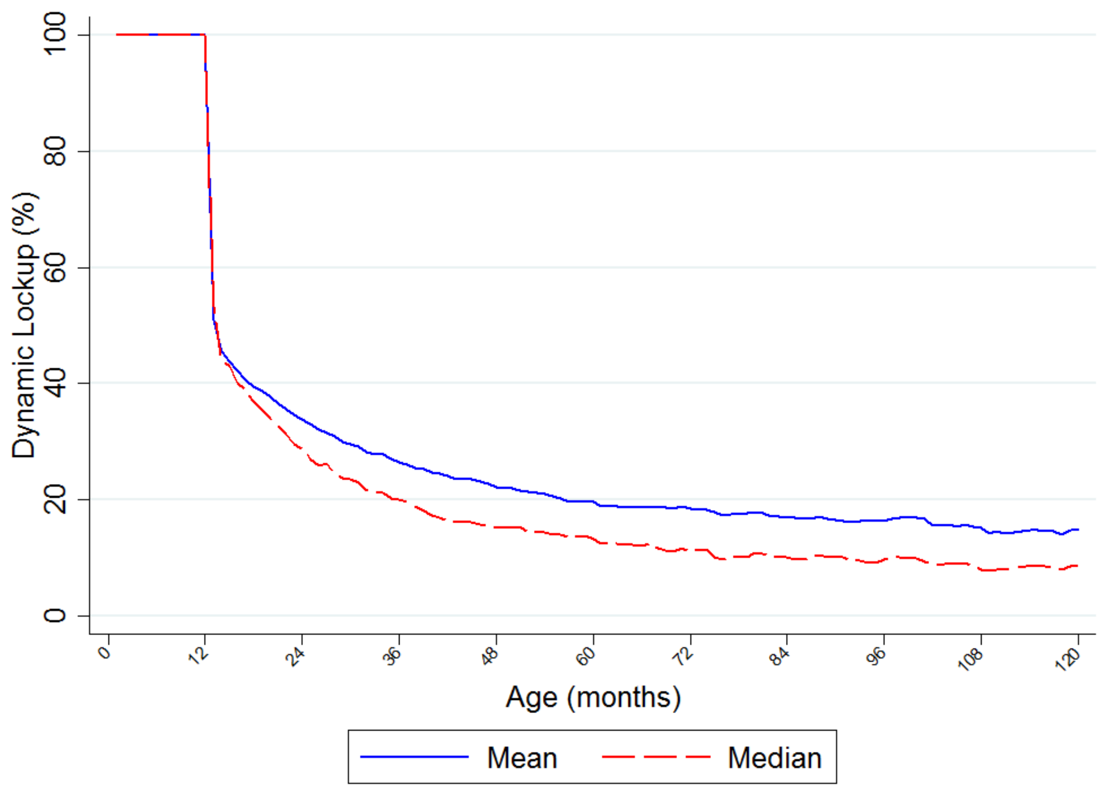

Figure 2.2: Regression Discontinuity Around Lockup Anniversary

This figure presents the average monthly returns of funds in event time relative to their initial lockup expiration date for both lockup and non-lockup funds.

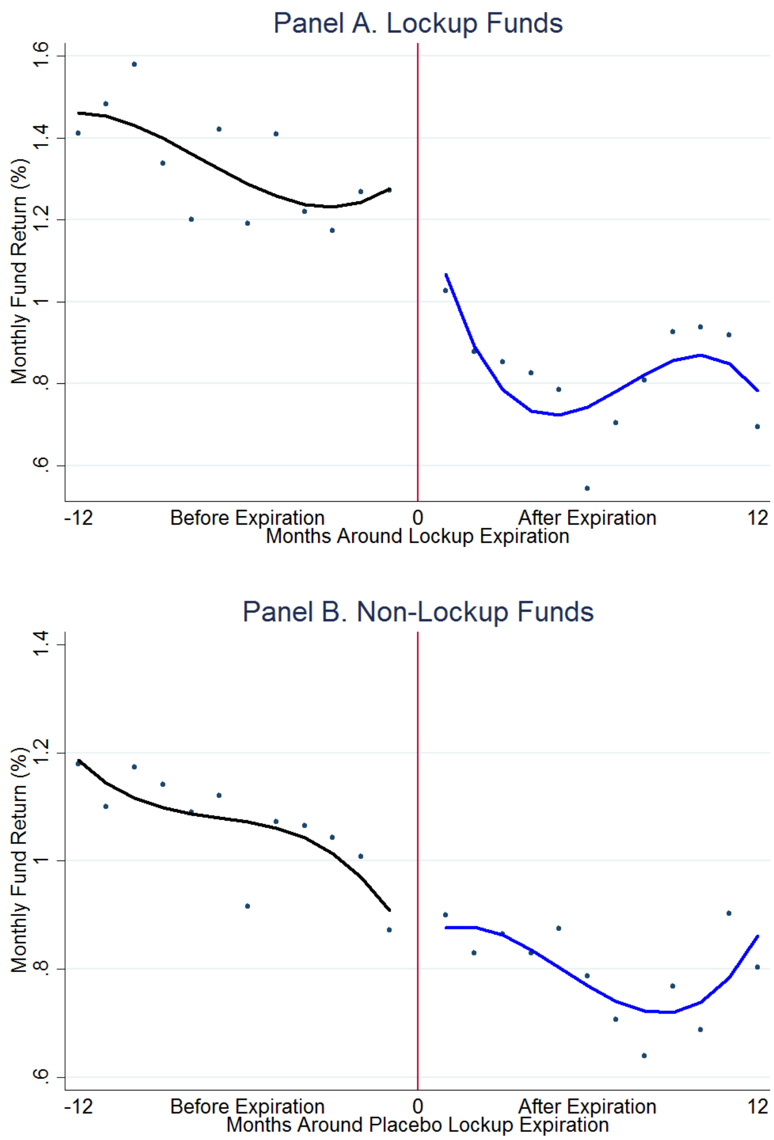

Table 2.1: **Summary Statistics**

This table presents the summary statistics for the hedge funds in our sample. The unit of observation is a hedge fund-month. Our sample period is 1994-2013. In Panel A, we examine the full sample of funds. In Panel B, we only examine the sample of funds with a lockup. *Lockup Fund* is an indicator variable equal to one if the fund has a lockup, and zero otherwise. *Dynamic Lockup* equals the percent of capital the fund has locked up (see equation 1). *AUM* is a fund's reported assets under management at the end of each month ($ millions). *Age* measures years since fund's first reported AUM. *Return* is the monthly return net of fees (%). *Flow* is a fund's implied, monthly net flows scaled by AUM (%). *Management fee* is the annual fee charged to investors as a percent of AUM (%). *Incentive fee* is the annual performance-based fee charged to investors (%). *Redemption notice* is the number of days of advance notice an investor must provide the fund to withdraw capital. *Redemption frequency* is the number of days between withdrawal periods. *Minimum Investment* is the minimum investment required to invest in the fund ($ millions). The full sample includes 13,124 funds and 793,431 fund-months.

*Panel A: Full Sample*

| | Mean | 10th | 25th | 50th | 75th | 90th | sd |
|---|---|---|---|---|---|---|---|
| Lockup Fund % | 29.81 | 0.00 | 0.00 | 0.00 | 100.00 | 100.00 | 45.74 |
| AUM ($MM) | 166.03 | 2.80 | 9.51 | 33.93 | 114.00 | 345.00 | 628.83 |
| Age (years) | 4.82 | 0.83 | 1.75 | 3.58 | 6.67 | 10.58 | 4.28 |
| Return % | 0.69 | -3.85 | -1.00 | 0.63 | 2.35 | 5.22 | 5.36 |
| Flow % | 1.33 | -5.01 | -0.38 | 0.00 | 1.36 | 8.00 | 11.52 |
| Management fee % | 1.47 | 1.00 | 1.00 | 1.50 | 2.00 | 2.00 | 0.62 |
| Incentive fee % | 18.17 | 10.00 | 20.00 | 20.00 | 20.00 | 20.00 | 5.88 |
| Redemption notice (days) | 36.52 | 2.00 | 15.00 | 30.00 | 45.00 | 90.00 | 34.35 |
| Redemption frequency (days) | 68.15 | 7.00 | 30.00 | 30.00 | 90.00 | 90.00 | 79.57 |
| Minimum Investment ($MM) | 1.18 | 0.10 | 0.20 | 0.50 | 1.00 | 2.00 | 3.88 |

*Panel B: Lockup Sample Only*

| | Mean | 10th | 25th | 50th | 75th | 90th | sd |
|---|---|---|---|---|---|---|---|
| Dynamic Lockup % | 27.92 | 0.00 | 0.92 | 11.98 | 43.79 | 100.00 | 34.07 |
| AUM ($MM) | 157.96 | 2.96 | 9.43 | 33.19 | 109.23 | 330.00 | 555.53 |
| Age (years) | 4.83 | 0.83 | 1.75 | 3.67 | 6.75 | 10.58 | 4.06 |
| Return % | 0.78 | -3.85 | -0.89 | 0.74 | 2.48 | 5.36 | 5.56 |
| Flow % | 1.46 | -3.56 | -0.18 | 0.00 | 1.33 | 7.33 | 10.57 |
| Management fee % | 1.40 | 1.00 | 1.00 | 1.50 | 2.00 | 2.00 | 0.48 |
| Incentive fee % | 19.20 | 18.00 | 20.00 | 20.00 | 20.00 | 20.00 | 3.99 |
| Redemption notice (days) | 51.68 | 30.00 | 30.00 | 45.00 | 60.00 | 90.00 | 41.98 |
| Redemption frequency (days) | 106.14 | 30.00 | 30.00 | 90.00 | 90.00 | 180.00 | 98.19 |
| Minimum Investment ($MM) | 1.11 | 0.10 | 0.25 | 1.00 | 1.00 | 2.00 | 1.70 |

Table 2.2: **Hedge Fund Outflows and Dynamic Lockup**

This table reports the results of regressions of monthly outflows on lagged dynamic lockup. The dependent variable, *Outflow*, is defined to be the $-min(0,$ monthly net flow). The unit of observation is a hedge fund-month. All control variables are defined in Table 2.1. All continuous, independent variables are normalized to mean of zero and a standard deviation of one. We include time fixed effects throughout and style/fund fixed effects where indicated. We cluster standard errors at the fund-level. We report $t$-statistics in square brackets. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | Outflow | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| $DynamicLockup_{t-1}$ | -0.208*** | -0.151*** | -0.459*** | -0.372*** |
| | [-8.65] | [-5.73] | [-17.16] | [-12.54] |
| $LogAge_{t-1}$ | | 0.032 | | 0.444*** |
| | | [1.15] | | [5.26] |
| $LogAUM_{t-1}$ | | 0.145*** | | 0.436*** |
| | | [6.31] | | [8.13] |
| $AunnualReturn_{t-1}$ | | -0.259*** | | -0.279*** |
| | | [-6.46] | | [-6.24] |
| $Flow_{t-1}$ | | 0.379*** | | 0.068** |
| | | [13.66] | | [2.53] |
| Minimum Investment | | -0.142*** | | |
| | | [-3.10] | | |
| Management Fee | | 0.194*** | | |
| | | [3.32] | | |
| Incentive Fee | | 0.102*** | | |
| | | [3.58] | | |
| Redemption Frequency | | -0.088*** | | |
| | | [-4.08] | | |
| Redemption Notice | | -0.020 | | |
| | | [-1.25] | | |
| Time FE | Yes | Yes | Yes | Yes |
| Style FE | Yes | Yes | - | - |
| Fund FE | - | - | Yes | Yes |
| Observations | 208,374 | 208,374 | 208,374 | 208,374 |
| $R^2$ | 0.026 | 0.035 | 0.085 | 0.089 |

Table 2.3: **Hedge Fund Performance and Dynamic Lockup**

This table reports the results of regressions of monthly returns on lagged dynamic lockup. The unit of observation is a hedge fund-month. All continuous, independent variables are normalized to mean of zero and a standard deviation of one. We include time fixed effects throughout and style/fund fixed effects where indicated. We cluster standard errors at the fund-level. We report $t$-statistics in square brackets. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | Return | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| $DynamicLockup_{t-1}$ | 0.179*** | 0.100*** | 0.156*** | 0.066*** |
| | [11.69] | [6.26] | [8.24] | [3.17] |
| $Flow_{t-1}$ | | 0.092*** | | 0.058*** |
| | | [5.52] | | [3.60] |
| $LogAge_{t-1}$ | | -0.023 | | -0.119** |
| | | [-1.23] | | [-2.35] |
| $LogAUM_{t-1}$ | | -0.130*** | | -0.765*** |
| | | [-7.47] | | [-17.65] |
| $LagReturn_{t-1}$ | | 0.652*** | | 0.503*** |
| | | [14.15] | | [10.46] |
| Minimum Investment | | 0.133*** | | |
| | | [5.44] | | |
| Management Fee | | 0.065 | | |
| | | [1.49] | | |
| Incentive Fee | | 0.057*** | | |
| | | [3.80] | | |
| Redemption Frequency | | 0.007 | | |
| | | [0.72] | | |
| Redemption Notice | | 0.001 | | |
| | | [0.03] | | |
| Time FE | Yes | Yes | Yes | Yes |
| Style FE | Yes | Yes | - | - |
| Fund FE | - | - | Yes | Yes |
| Observations | 236,554 | 236,554 | 236,554 | 236,554 |
| $R^2$ | 0.165 | 0.177 | 0.191 | 0.201 |

Table 2.4: **Hedge Fund Performance and Dynamic Lockup – Robustness**

This table reports the results of regressions of monthly returns on lagged dynamic lockup. The unit of observation is a hedge fund-month. Each model reports the coefficient on dynamic lockup only, and includes the same controls, including fund fixed effects, as in Model 4 of Table 2.3. Model 1 is intended for reference and is identical to that of Model 4 from Table 2.3. In the remaining models, we perform a series of robustness tests to rule out alternative hypotheses. In Model 2, we mitigate backfill bias by including age fixed effects to study the within age cohort effects of dynamic lockup on future returns. In Model 3, we exclude funds that endogenously change their lockup length by using monthly snapshots of the BarclayHedge database to exclude all funds that change their lockup duration. In Model 4, we mitigate the role of redemption frequency by including only those funds with quarterly redemption frequency (the modal frequency in our sample). In Model 5, we repeat our analysis on equity-only funds, as defined by Agarwal, Ruenzi, and Weigert (2016). In Model 6, we estimate dynamic lockup using monthly flows. In Model 7, we add a delisting return of -50% in the months funds exit the database to mitigate survivorship bias. We report $t$-statistics in square brackets. We include time and fund fixed effects in each model and cluster standard errors at the fund-level. ***, **, * represents statistical significance at the 1%, 5%, and 10% level respectively.

| | Return |
|---|---|
| 1. Baseline | 0.066*** |
| | [3.17] |
| 2. Age Fixed Effects | 0.056*** |
| | [2.59] |
| 3. Constant Lockup Funds only | 0.069*** |
| | [3.27] |
| 4. Quarterly Redemption Funds only | 0.093*** |
| | [3.39] |
| 5. Equity Funds only | 0.068*** |
| | [2.65] |
| 6. Monthly Dynamic Lockup | 0.058*** |
| | [2.89] |
| 7. Add Delisting Return | 0.103*** |
| | [3.34] |

Table 2.5: **Hedge Fund Performance and Dynamic Lockup – Placebo Approach**

This table reports the results of regressions of monthly returns on lagged dynamic lockup. The unit of observation is a hedge fund-month. All continuous, independent variables are normalized to mean of zero and a standard deviation of one. Model 1 is intended for reference and is identical to that of Model 4 from Table 2.3, where we only include funds with a lockup in the sample (Lockup Funds). In Model 2, we estimate the same model, but only include funds without a lockup (Non-lockup Funds). For these funds, we randomly assign a pseudo-lockup period and calculate a placebo dynamic lockup value following the same methodology used with the lockup funds. We include time and fund fixed effects in both models. Standard errors are clustered at the fund-level. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

| | Return | |
|---|---|---|
| | 1 | 2 |
| | Lockup Funds | Non-lockup Funds |
| $DynamicLockup_{t-1}$ | 0.066*** | |
| | [3.17] | |
| $DynamicLockup(placebo)_{t-1}$ | | 0.000 |
| | | [0.04] |
| $Flow_{t-1}$ | 0.058*** | 0.029*** |
| | [3.60] | [3.46] |
| $LogAge_{t-1}$ | -0.119** | -0.215*** |
| | [-2.35] | [-7.00] |
| $LogAUM_{t-1}$ | -0.765*** | -0.639*** |
| | [-17.65] | [-25.32] |
| $Return_{t-1}$ | 0.503*** | 0.464*** |
| | [10.46] | [19.32] |
| Time FE | Yes | Yes |
| Fund FE | Yes | Yes |
| Observations | 236,554 | 556,877 |
| $R^2$ | 0.201 | 0.148 |

Table 2.6: **Hedge Fund Performance and Dynamic Lockup – IV Approach**

This table reports the results of a 2SLS instrumental variable regression of monthly returns on the expected value of lagged dynamic lockup. The unit of observation is a hedge fund-month. We only include up to twenty-four months of fund returns centered around the initial lockup expiration based on the fund's inclusion in a commercial database. All continuous, independent variables are normalized to mean of zero and a standard deviation of one. We instrument for *Dynamic Lockup* (*Dynamic Lockup (placebo)*) using an indicator variable that equals one in the twelve months prior to the anniversary and zero in the twelve month following the anniversary. In Models 1 and 2, we only include funds with a lockup (Lockup Funds), while in Models 3 and 4 we only include funds without a lockup (Non-lockup Funds). For the non-lockup funds, we calculate *Dynamic Lockup (placebo)* as in Table 2.5. In Models 1 (3), we show the first stage result of the indicator *After Lockup Expiration* in predicting *Dynamic Lockup* (*Dynamic Lockup (placebo)*). In Models 2 (4), we show the effect of the expected value of lagged *Dynamic Lockup* (*Dynamic Lockup (placebo)*) on monthly returns. We include identical control variables (omitted) to those in Model 4 of Table 2.3. We include fund fixed effects throughout. Standard errors are clustered at the fund-level. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

| | Lockup Funds | | Non Lockup Funds | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| | 1st Stage IV<br>Dynamic Lockup | 2nd Stage IV<br>Return | 1st Stage IV<br>Dynamic Lockup (placebo) | 2nd Stage IV<br>Return |
| After Lockup Expiration | -0.880*** | | -0.345*** | |
| | [-50.95] | | [-25.39] | |
| $DynamicLockup_{t-1}$ | | 0.467*** | | |
| | | [3.76] | | |
| $DynamicLockup(placebo)_{t-1}$ | | | | 0.209 |
| | | | | [1.09] |
| Months Since Lockup Expiration | -1.464*** | 1.625*** | -1.858*** | 0.797* |
| | [-43.33] | [4.23] | [-74.26] | [1.70] |
| Controls | Yes | Yes | Yes | Yes |
| Fund FE | Yes | Yes | Yes | Yes |
| Observations | 59,357 | 59,357 | 126,839 | 126,839 |
| $R^2$ | 0.786 | 0.096 | 0.714 | 0.102 |

Table 2.7: **Lockup Premium Fixed Effect**

This table reports the results of regressions of monthly returns on lagged dynamic lockup. The unit of observation is a hedge fund-month. All continuous, independent variables are normalized to mean of zero and a standard deviation of one. We include both lockup and non-lockup funds in the models. *Lockup Fund* is an indicator variable equal to one if the fund has a lockup, and zero otherwise. In Model 1, *Dynamic Lockup* takes on its true value for lockup funds and a value of zero for non-lockup funds. In Model 2, *Dynamic Lockup*$^\star$ takes on its true value for lockup funds and a placebo value for non-lockup funds. We include identical control variables (omitted) to those in Model 2 of Table 2.3. We include time and style fixed effects. Standard errors are clustered at the fund-level. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

| | Return | |
|---|---|---|
| | 1 | 2 |
| | All Funds | All Funds |
| Lockup Fund | 0.072*** | 0.068*** |
| | [3.93] | [4.14] |
| Dynamic Lockup$_{t-1}$ | 0.089*** | |
| | [6.51] | |
| Dynamic Lockup$^\star{}_{t-1}$ | | 0.042*** |
| | | [5.23] |
| Controls | Yes | Yes |
| Time FE | Yes | Yes |
| Style FE | Yes | Yes |
| Observations | 793,431 | 793,431 |
| $R^2$ | 0.136 | 0.136 |

Table 2.8: **Hedge Fund Outflows and Dynamic Lockup**

We test for differences in the flow-performance sensitivity of *Lockup* and *Non-Lockup* funds. The unit of observation is a hedge fund-month. All continuous, independent variables are normalized to mean of zero and a standard deviation of one. The dependent variable, *Outflow*, is defined to be the $-min(0$, monthly net flow). We estimate *Dynamic Lockup* for all funds following the placebo approach in Table 2.5 and control for *Dynamic Lockup* throughout (as well as all controls from Table 2.2). *Lockup Fund* is an indicator variable equal to one if the fund has a lockup, and zero otherwise. *Return* is the fund's holding period return over the prior twelve months. *Below Median Return* is an indicator equal to one if the fund's holding period return over the prior twelve months was below the sample median, and zero otherwise. We include time and style fixed effects throughout. Standard errors are clustered at the fund-level. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

| | Outflows | |
|---|---|---|
| | 1 | 2 |
| | All Funds | All Funds |
| Lockup Fund (Indicator Variable) | -0.382*** | -0.094*** |
| | [-11.68] | [-3.81] |
| $Return_{(t-1,t-13)}$ | -1.701*** | |
| | [-13.22] | |
| $Return_{(t-1,t-13)}$ × Lockup Fund | 0.779*** | |
| | [4.55] | |
| Below Median $Return_{(t-1,t-13)}$ | | 1.347*** |
| | | [58.01] |
| Below Median $Return_{(t-1,t-13)}$ × Lockup Fund | | -0.397*** |
| | | [-10.71] |
| Controls (including Dynamic Lockup) | Yes | Yes |
| Time FE | Yes | Yes |
| Style FE | Yes | Yes |
| Observations | 700,064 | 700,064 |
| $R^2$ | 0.040 | 0.046 |

Table 2.9: **Risk Taking and Dynamic Lockup**

This table reports excess returns, factor exposures, and alphas for a series of equal-weighted, placebo-adjusted portfolios. Each month, funds are sorted into terciles based on their lagged *Dynamic Lockup*. We placebo-adjust each tercile by subtracting the monthly return for the placebo portfolio (*Lockup Fund = 0*) from the monthly return of the lockup portfolio (*Lockup Fund = 1*). In Panel A, we report placebo-adjusted excess returns by tercile. In Panel B, we test for fund's exposure to a tail risk factor from Agarwal, Ruenzi, and Weigert (2017) and an Asset Illiquidity factor from Getmansky, Lo, and Makarov (2004). We also include the risk factors from the Fung and Hsieh (2004) 7-factor model (coefficients omitted). High-Low represents a long-short portfolio that invests long in the high dynamic lockup portfolio and short in the low dynamic lockup portfolio. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

*Panel A: Excess Return*

| | 1<br>Low | 2<br>Mid | 3<br>High | 4<br>High-low |
|---|---|---|---|---|
| Return | 0.157** | 0.201*** | 0.337*** | 0.180*** |
| | [2.23] | [3.14] | [4.52] | [3.05] |

*Panel B: Seven Factor Model*

| | 1<br>Low | 2<br>Mid | 3<br>High | 4<br>High-low |
|---|---|---|---|---|
| Alpha | 0.051 | 0.108** | 0.210*** | 0.159*** |
| | [0.96] | [2.22] | [4.19] | [2.75] |
| Tail Risk | -3.157 | 4.156* | 5.271** | 8.427*** |
| | [-1.37] | [1.96] | [2.43] | [3.36] |
| Asset Illiquidity | 4.117*** | 2.521** | 3.259*** | -0.858 |
| | [3.29] | [2.20] | [2.77] | [-0.63] |

## Chapter 3 Hedge Fund Lockup Capital and Stock Mispricing

### 3.1 Introduction

From one of the earliest anomalies, momentum effect (Jegadeesh and Titman, 1993, and Carhart, 1997), to the latest volatility anomaly (Jordan and Riley, 2015), stock mispricing has never been fully arbitraged away. Shleifer and Vishny (1997) offer an explanation that professional arbitrageurs, who are typically money managers that trade using other people's capital, are subject to funding risks that limit their ability to trade against mispricing. One of the most significant funding risks lies in investors' ability to withdraw money at any time. We are interested in testing how changes in such a funding risk for hedge funds, the well-known arbitrageurs in the market, affects their behavior in taking risks and impacts stock mispricing.

Hedge funds use varies share restrictions to limit redemptions from investors, including a lockup period, a redemption frequency period, redemption notice, and gates. There are several studies that examine how funds with greater redemption restrictions are in a better position to trade against mispricing. Giannetti and Kahraman (2018) believe that close-end mutual funds and hedge funds with greater share restrictions are more likely to engage in risky arbitrage strategies. Hombert and Thesmar (2014) argue that funds choose their withdrawal restriction terms to attract more stable capital to facilitate their ability to take riskier strategies. Aragon (2007) finds that funds with lockup terms are better performers than non-lockup funds due to their better ability to trade on illiquid assets.

We also focus on using lockup periods, the most important share restrictions, to determine how they can affect funds' funding risks. The previously listed literature examine how different type of funds exhibit different risk behavior due to the setup of redemption restrictions [1]. However, they did not explore how the correlation between funds' funding risk and their taking on arbitrage risk changes over time. Thus it is hard to identify what role the hedge fund industry plays in correcting mispricing.

We utilize a lockup measure developed by Aiken et al. (2018) that captures the changing amount of capital under lockup for hedge funds over time. The capital lockup for an average hedge fund expires in 12 months. After the initial 12 months, investors are free from lockup and can redeem their capital from hedge funds. The new flows will be subject to the lockup until it expires. As a result, the amount of capital under lockup is determined by the capital flowing into the fund in the previous lockup period. Based on this feature, a dynamic lockup measure is created by using past flows and the performance associated with the flows. In their paper, they find that the dynamic lockup within a fund varies over time and can predict fund performance. In our paper, we utilize the same measure to proxy for stable capital and attempt to understand how stable capital enables hedge funds to take advantage

[1] The average lockup period for lockup funds is 12 months, which is much higher than the average 51 days of redemption notice and 106 days of redemption frequency for the same funds. Hedge funds only use gate or discretionary liquidity restrictions in the extreme situations (Aiken et al., 2015).

of arbitrage opportunities. More importantly, it helps us to understand how hedge funds as a group can contribute to market efficiency.

To measure the arbitrage opportunity in the stock market, we follow Stambaug et al. (2012, 2015) in defining a mispricing score for each stock. Each month, we categorize the stocks in the top (bottom) decile of the mispricing score as most overvalue (undervalue) stocks and form a portfolio of overpricing (underpricing). We calculate the equally-weighted returns on the two portfolios and call them the overpricing return and underpricing return, respectively [2]. The difference between underpricing and overpricing returns is called the mispricing return. To correct mispricing, we should trade as long underpriced stocks and short overpriced stocks. When the mispricing is corrected, the returns on underpriced stocks and overpriced stocks are positive and negative, respectively. The mispricing return should be even higher than the underpricing return. Thus, if hedge funds are engaging in trading against mispricing, we should find the returns on the mispricing portfolios to be consistent with those patterns.

The first question we ask is whether or not hedge funds change their trading on mispriced stocks based on the amount of stable capital. We adopt a time-series portfolio factor regression approach and examine how a high dynamic lockup fund portfolio loads differently on mispricing risk than a low dynamic lockup fund portfolio. We include Fung and Hsieh (2004) seven factors and one of the three mispricing factors. The results indicate that hedge funds take much greater mispricing and underpricing risks when their lockup capital is higher. Hedge funds are more likely to trade against (or in other words purchase) underpriced stocks when additional capital is under lockup. To control for potential measurement errors, we use a placebo lockup portfolio as a control group and find that the significant results we obtain from lockup funds disappear on the placebo funds. Thus, the evidence supports that hedge funds engage in arbitrage opportunities when their capital is more secure.

The other and more important question is how hedge fund trading affects stock mispricing in the market. If hedge funds help to correct mispricing, we should observe a positive return on underpriced stocks or a negative return on overpriced stocks. Afterward, if mispricing is successfully corrected, there will be no significant return on either set of mispriced stocks. Since we are interested in hedge funds' role as a whole entity, we aggregate lockup capital to the industry level and create a time varying amount of capital under lockup in the hedge fund industry. Another benefit of this method is that we can also create placebo lockup capital for all non-lockup funds and use the placebo variable as a control in all regressions. At the monthly level, we regress the mispricing, underpricing, and overpricing returns on aggregated lockup capital and placebo lockup capital. We follow Akbas et al. (2015) and include the returns on the Fama and French (1993) three factors, market liquidity, and average stock turnover as controls. Another important control variable in the model is hedge fund flow. The addition of hedge fund flow is to help distinguish whether it is the stable capital driving the correction of mispricing or if it is investor flows choosing funds with the ability to trade against mispricing.

[2] They are sometime referred to as overpricing factor and underpricing factor

The results indicate that lockup capital is associated with the correction of mispricing, in particular of underpricing. When the lockup capital increases by one standard deviation, amounting to $32 billion, hedge funds can earn 44 basis points of monthly returns on trading against mispricing. The average market capitalization for all mispriced stocks is over $3.1 trillion. If hedge funds can secure $32 billion more stable capital, all investors with a stake in mispriced stocks can benefit more than $13 billion each month. The results indicate that hedge funds are important players in promoting an efficient market and they can become more capable arbitrageurs if they have a more stable capital structure.

In the above regression, we include placebo lockup capital as a control and find no significant results in all six models. The placebo lockup in the models directly controls for any potential measurement errors that could compromise the dynamic lockup measure. For example, one of the components constituting dynamic lockup, cash flows, could also correlate with stock mispricing. In addition to using placebo lockup, we also add the cash flows in the three models to control for flow effects. When we employ lockup capital and flows in the same regression, we still observe significant coefficients on lockup capital, but no significance on flows. To further confirm that the stable capital is facilitating the correction of mispricing, we run a forward one month average return of mispricing stocks on lockup capital and find that the coefficient becomes insignificant. The results support that mispricing in the stock market has been reduced or corrected after a period of high stable capital.

Our work contributes to the limits to arbitrage literature. Shleifer and Vishny (1997) finds that one of the limits to arbitrage is funding liquidity risk, which constrains institutional investors from trading against mispricing. The fund's ability to retain capital during underperformance affects the fund's willingness to trade on arbitrage opportunities. Hombert and Thesmar (2014) finds that hedge funds with greater share restrictions are in a better position to overcome the limits of arbitrage and obtain better performance after low performance. Alternatively, Stein (2005) believes that open-end structure, which is the opposite of having strong protection to retain capital, discourages trading against mispricing. Our results are consistent with their findings and indicate that hedge funds' trading on mispriced stocks is associated with the stability of their capital. In addition, we find that hedge funds are indeed effective arbitrageurs and help correct mispricing, especially when they can secure more stable capital.

Our paper is closely related to Akbas et al. (2015), which finds that hedge funds help correct stock mispricing when flows into the funds are higher, while the opposite is true for mutual funds whose flows exacerbate mispricing. They define hedge fund flows as smart money and believe that either investors are smart in choosing skilled funds or fund managers are smart in trading against mispricing. Another paper by Akbas et al. (2016) defines arbitrage capital as flows to mutual funds whose trading resembles a quant strategy. The profitability of trading on market mispricing is associated with the availability of arbitrage capital. We use a different type of arbitrage capital, lockup capital by hedge funds, and find similar results.

## 3.2 Data

We are interested in understanding the relationship between hedge funds' stabilized capital and performance on trading of mispriced stocks. In this section, we explain how we construct those two major measures.

### 3.2.1 Hedge Fund Data

The hedge fund data comes from the union of five hedge fund commercial databases: Lipper TASS, BarclayHedge, HFR, Eureka, and Morningstar. The sample period is from 1994 to 2013 and fund characteristics are based on the snapshots of the databases collected in 2013. We follow Joenväärä et al. (2016) in defining the fund style and drop funds of funds from the sample. We also drop non-US dollar denominated share classes.

Our main independent variable is the amount of capital under lockup for hedge funds at any given time, created following Aiken et al. (2018). We assume that any cash inflows a fund receives during the prior lockup period, as well as any returns associated with the inflows, are under lockup and not available for redemption. We calculate cash flows as current assets under management(AUM) minus previous AUM multiplied by a fund's current return. Since we do not observe the gross flows from hedge funds, we use positive net flows as inflows into hedge funds. [3] At each month, we calculate the lockup capital from funds with a lockup term by using following equation:

$$Dynamic\ Lockup_{i,t} = \frac{\sum_{j=1}^{L}(flow_{i,t-L+j} * \prod_{k=j+1}^{L}(1 + r_{i,t-L+k}))}{AUM_{i,t}} \tag{3.1}$$

where $flow_{i,t}$ is the *positive* net flow received by the fund at the end of each $month_t$, $r_{i,t}$ is the return in $month_t$, $L$ is the length of the lockup period measured in quarters, and $AUM_{i,t}$ is the assets under management for the fund. [4]. Lockup capital is an estimate of capitals that cannot be withdrawn by investors in this month. We use the lockup capital in hedge fund industry as a proxy for stable capital that funds can deploy in trading against mispricing.

Since we use similar measures as in Aiken et al. (2018), the measure is subject to the same possible measurement errors outlined in that paper. To mitigate the concerns, we also employ the placebo approach and calculate the placebo lockup capital using hedge funds without any lockup terms. First, we obtain the frequency of the lockup period for lockup funds each year based on a fund's inception year. In addition, based on the year the fund was founded, we apply the frequency of the lockup period randomly into non-lockup funds and assign a placebo lockup period accordingly. Finally, we calculate the placebo lockup capital for non-lockup funds

[3]The uses of positive net flows may bias against finding any correlations, yet we take steps later to ensure that our results is not driven by the bias

[4]we also calculate the lockup capital in quarterly frequency, due to the concerns regarding possible reporting issues on fund's AUM. The main results remain qualitatively the same

using the same methodology as in lockup funds. [5] We use the placebo lockup capital measure as a control in all regressions.

Table 3.1 presents the summary statistics for the hedge fund industry. Panel A reports the aggregated AUM and the average cash flow as a percentage of a fund's AUM for funds with a lockup term, while Panel B lists the same for non-lockup funds. Aggregated AUM for non-lockup funds is much larger than that of lockup funds, as only about 30% of hedge funds have lockup terms. Cash flow is implied by the difference in AUMs and the return gained during the period. The monthly cash flow is about 1.4% of funds' AUM for lockup funds, while average lockup capital accounts for almost 30% of AUM.

Even though we are only interested in stable capital coming from the lockup funds, the flows into non-lockup funds are very important for us to determine whether it is hedge funds' flows or their stable capital that enables them to take advantage of mispricing opportunities. Akbas et al. (2015) argue that flows into hedge funds are smart money and investors choose hedge funds that are good at trading against mispricing. We believe that it is the capital that cannot be withdrawn from hedge funds driving the funds trading behavior, not the flows per se. Since lockup capital is calculated based on past flows and performance, it is important to distinguish the effect of lockup capital from that of flows. To do so, we create a placebo lockup capital for all non-lockup funds. We aggregate lockup capital for lockup and non-lockup funds separately. Placebo lockup capital is larger than real lockup capital as there are greater numbers of non-lockup funds. When comparing the dynamic lockup (percentage of capital under lockup - lockup capital scaled by AUM) between them, we find that real dynamic lockup is higher by 2.5%.

Placebo lockup capital is useful in testing whether or not any effects are associated with stable capital. If the performance on trading of mispriced stocks is associated with stable capital, but not with flows into hedge funds, we should not observe any effect on placebo lockup capitals, and vice versa.

### 3.2.2 Stock Mispricing

The stock sample is derived from the CRSP database. We include all common stocks listed on the NYSE, AMEX, and Nasdaq from 1994 to 2013, the same period as in the hedge fund databases. We only include stocks with share prices greater than \$5.00. We follow Akbas et al. (2015) and Stambaug et al. (2012, 2015) in developing the aggregate mispricing measure.

The mispricing measure is a combination of 11 return anomalies documented in the literature.[6] Those 11 anomalies are chosen because they are a good represen-

[5] Please refer to Aiken et al. (2018) for detail process in creating placebo lockup measure

[6] The 11 anomalies include financial distress (Campbell et al. (2008)) , O-Score bankruptcy probability (Ohlson (1980)), net stock issues (Ritter (1991), Loughran and Ritter (1995), Fama and French (2008)), composite equity issues (Daniel and Titman (2006)), total accruals (Sloan (1996)), net operating assets (Hirshleifer et al. (2004)), momentum (Jegadeesh and Titman (1993)), gross profitability (Novy-Marx (2013) ), asset growth (Cooper et al. (2008)), return on assets (Fama and French (2006), Chen et al. (2010)), and investment-to-assets (Titman et al. (2004), Xing (2008)). We thank Yu Yuan for making the measure available online

tation of anomalies that cannot be explained by the Fama and French (1993) three factors, and they have low correlations among themselves. For each of 11 anomalies, stocks are ranked each month by the individual anomaly in a way that higher (lower) rank implies greater overpricing (underpricing). The 11 anomalies ranks are further equally-weighted for each stock to construct the aggregated mispricing measure. Stocks within the top (bottom) decile of the mispricing measure are categorized as the most overpriced (underpriced) stocks. The stocks in the top (bottom) decile are relatively overpriced (underpriced) when compared to the rest of stocks during the same period of time.

The summary for underpriced and overpriced stocks can be found in Table 3.2. In each month, the market values for each category of stocks are aggregated and the returns are averaged. The results are listed in Panel A. The market capitalization of stocks falling into the most underpriced category reaches \$2.6 trillion on an average month from 1994-2013. The overpriced stocks are much smaller and only amount to about 1/5 of the market share for underpriced stocks. The average monthly return on underpriced stocks is 1.5%, while it is -0.1% for overpriced stocks. The spread between the returns on the underpriced and overpriced stocks is 1.6%.

The mispricing measure consists of 11 different anomalies. Each one is shown in the literature to deliver abnormal returns after adjusting for risk factors. To verify that the combined measure can still produce abnormal returns, we run the time series returns on the underpriced stock portfolio, overpriced stock portfolio, and the spread on Fama-French four factor models [7]. Panel B indicates that the alphas for the three portfolios are 0.82%, -0.79%, and 1.61%, respectively, and all of which are statistically significant at 1%. On average, both the alphas from buying underpriced stocks and shorting overpriced stocks are close to 10% per year.

It appears that trading on mispriced stocks has been highly profitable in the past 20 years. Why is the mispricing not arbitraged away after those anomalies are known to investors over the years? As Shleifer and Vishny (1997) point out, trading on arbitrage does not come without costs. There are risks associated with trading on mispriced stocks. For example, the stock price could move further away from its fundamental value in the short-term causing the investors to suffer from temporary losses before gaining a profit. One issue with institutional investors who manage other peoples money is that their impatient capital will deteriorate if poor performance, even though it might be temporary, is spotted. The situation is particularly severe for open-end mutual funds or hedge funds with few share restrictions (Giannetti and Kahraman, 2018). However, even for hedge funds with stricter share restrictions, such as having a lockup term, their capital is not fully restricted from redemption, but only 30% of their capital is under lockup on average (Aiken et al., 2018). As a result, funds are hesitant to trade against mispricing due to concerns regarding funding risk and further leave mispricing uncorrected. Our paper examines the question by determining the relationship between the stable capital in hedge funds and mispricing in the stock market. When hedge funds have more stable capital, they are expected to trade on mispriced stocks and help to correct mispricing in the stock market.

[7]Fama-French three factor models yield similar results

## 3.3 Stable Capital and Mispricing

Before delving into the relationship between lockup capital and stock mispricing, we first plot the sizes of lockup capital and mispriced stocks in the time series and see how they vary over time. Figure 3.1 plots the aggregated overpriced stock size and the underpriced stock size on the left y-axle and the size of hedge fund lockup capital on the right y-axle. Underpriced stocks are much larger than overpriced stocks. Both of them vary a lot over the past 20 years. Alternatively, hedge funds seem to significantly build up stable capital from 1994-2008 when the financial crisis hit. After the financial crisis, lockup capital decreased by more than half and still didnt recover.

From Figure 3.1, we note that the size of the lockup capital is only a small fraction of the underpriced stocks. When hedge funds have more stable capital and can afford to take arbitrage risks, they can have a large enough market to invest in and take advantage of market mispricing. Next, we explore whether or not hedge funds take advantage of arbitrage opportunities. Additionally, we query whether hedge funds help to correct mispricing by their trades.

### 3.3.1 Hedge fund portfolio approach

In this section, we examine whether hedge funds with greater lockup capital are more likely to take advantage of stable capital and trade on mispriced stocks. To address this issue, we adopt a portfolio factor approach and run a risk model by including the Fung and Hsieh (2004) seven factors and a mispricing factor.[8] We follow Stambaug et al. (2012, 2015) in creating mispricing factors.[9] If returns on a portfolio load significantly on a mispricing factor, it could be an indicator that funds in the portfolio are likely to take a significant arbitrage risk. Each month, we calculate the average return on the most underpriced (overpriced) stocks and use the time-series returns as an underpricing (overpricing) risk factor. We use the return difference between the underpriced stocks and the overpriced stocks as mispricing spread factor.

We run the calendar-time factor regression models and report the results in Table 3.3. For lockup funds, we sort them into equally-weighted monthly portfolios based on their lockup capital quantiles and calculate the return on each portfolio. At each month, we subtract the return on the lowest quantile portfolio from the return on the highest quantile portfolio, and use the differences in returns to run the regression on the risk factors. We repeat the same process for non-lockup funds using placebo lockup capital. We run time-series of portfolio returns on the risk factors for lockup and non-lockup portfolios separately. We do this as a way to control for input to

[8] The Fung and Hsieh (2004) model includes the following returns: the S&P 500 total return, a size spread return (Wilshire Small Cap 1750 - Wilshire Large Cap 750), a bond market factor (quarterly change in the 10-year constant maturity treasury yield), a credit spread factor (quarterly change in the Moody's Baa yield less the 10-year treasury constant maturity yield), and three trend-following factors for the bond market, the currency market, and the commodities market. See David Hsieh's web page at http://faculty.fuqua.duke.edu/%7Edah7/HFRFData.htm for a complete description.

[9] Stambaug et al. (2012, 2015) aggregate the 11 anomalies into two risk factors, while our paper aggregates them into one mispricing factor.

lockup capital, such as past flows, that is also likely to be important determinants of risk taking behavior. For example, if hedge funds take greater mispricing risks as a result of cash flows, then we should expect both lockup funds with high lockup capital and non-lockup funds with high placebo lockup capital to load significantly on the mispricing risk factor.

Models 1-3 in Table 3.3 list the betas and alpha for the lockup fund portfolios. We are interested in the beta loadings on the three mispricing factors. The significant coefficient on the underpricing factor indicates that lockup funds with higher lockup capital are more likely to take greater risks on trading against underpricing than lockup funds with low lockup capital. However, there is no significant difference in their risk taking when it comes to the overpriced stocks. It is consistent with literature that overpricing is harder to correct due to short sale constraints. Even though the coefficient on the overpricing factor is not significant, the direction is consistent with what we expect. Combined with positive and significant loading on the underpricing factor and the negative coefficient on the overpricing factor, we find that the correlation between hedge fund returns with high lockup capital and the mispricing factor is significantly positive.

To illustrate that the results are not driven by the construction of the lockup capital, we run the same portfolio factor regression on the non-lockup funds using placebo lockup capital and present the results in Models 4-6. Different from the results using real lockup capital, the coefficient of the mispricing factors on all three models are insignificant. The insignificant results on the placebo sample help to mitigate the concerns regarding the effects being driven by factors other than stable capital, such as past flows or past performance.

Thus, we have established a cross-sectional connection between individual hedge fund lockup capital and the mispricing risks taken by a fund. When a hedge fund has more stable capital, it is more likely to take greater mispricing risks, in particular underpricing risks. The next question we want to examine concerns the role of hedge funds as arbitragers as a whole. How does the hedge fund industry interact with mispriced stocks? Do hedge funds help correct mispricing when they have more stable capital?

### 3.3.2 Market-wide Stable Capital and Stock Mispricing

In this section, we investigate the effect of hedge fund lockup capital on stock mispricing. Hedge funds as an important arbitrager group help correct mispricing in the stock market. When hedge funds have more flows, they are able to apply more capital to trade against mispriced stocks (Akbas et al., 2015). However, their ability to trade against mispricing is limited by their funding liquidity risk. If hedge funds impose stronger share restrictions, they are more likely to engage in arbitrage activities (Giannetti and Kahraman, 2018). The creation of a lockup capital measure enables us to differentiate our study from Giannetti and Kahraman (2018) in two ways.

First, lockup capital varies over the time. As such, we can test the relationship between lockup capital and stock mispricing in a time-series setting. It helps us to not only understand the effect within industry variation, but also the effect of the entire hedge fund industry over time. In addition, we query how hedge funds affect the stock market when they have more stable capital at their disposal. In the previous test, we find that, within hedge fund industry, funds with greater stable capital are more likely to trade against mispricing, especially underpricing. Thus, we hypothesize that the aggregated lockup capital in the industry should be associated with trading on mispriced stocks and correcting mispricing.

To test this hypothesis, we follow the regression framework of Akbas et al. (2015) in their time series regression and replace their aggregated mutual fund flow and hedge fund flow variables with our aggregated lockup capital. When hedge funds have more capital under lockup, they are in a better position to engage in arbitrage opportunities. If hedge funds help to correct the mispricing, we should expect to see the return on mispriced stocks to be higher in the same period. As a result, we should find a positive correlation between aggregated lockup capital and contemporaneous performance on mispricing.

The regression results in Table 3.4 confirm the above hypothesis. In Model1, we use the time series return on mispriced stocks (the difference between the return on underpriced stocks and that on overpriced stocks) as the dependent variable. The independent variable include lockup capital aggregated across all lockup funds and placebo lockup capital aggregated across all non-lockup funds. One advantage of this regression setting is that we can use dynamic lockup capital as a control variable to mitigate any measurement error concerns directly. Other control variables include the Fama and French (1993) three-factor returns to control for risks associated with market, size and value effects. We also follow Akbas et al. (2015) and include two additional variables - aggregate liquidity and aggregate turnover. Aggregate liquidity is a monthly measure of average Amihud (2002) ratio in the stock market. Aggregate turnover is a monthly measure of average trading turnover in the stock market. Both variables are proxies for trading costs. It is associated with stock mispricing as higher trading costs could deter arbitragers' trading.

Model 1 indicates that lockup capital is positively correlated with the returns on mispriced stocks. A standard deviation increase in lockup capital is associated with a 44 basis point higher monthly return. The coefficient on the placebo lockup capital is insignificant indicating that the effect of lockup capital is not driven by the construction of the dynamic lockup measure. To better understand the source of performance, we separate mispricing returns into underpricing and overpricing. The results are found in Models 2 and 3. The results indicate that the effect of lockup capital is primarily driving the outperformance of underpriced stocks. The effect is insignificant on overpricing performance. However, the coefficient is in the right direction. Thus, when aggregate lockup capital is higher, more capital can be deployed into underpriced stocks and used to correct the underpricing.

Models 4-6 repeat Models 1-3 and add aggregate hedge fund flows as a control variable. Akbas et al. (2015) find that hedge fund flows have a significant effect on mispricing returns. However, our results indicate no correlation between the two. There might be two possible explanations. One is the difference in database used. They use hedge fund information from the Lipper TASS database, while we use five databases, including Lipper TASS database, which contain more hedge funds. The second possible reason is that we do not control aggregate mutual fund flows in the model. Nevertheless, our results are not altered after adding hedge fund flows in the model.

When lockup capital is associated with contemporaneous trading on mispriced stocks, we should expect the mispricing to be corrected or partially corrected in the next period. Table 3.5 presents such results where the dependent variables are the forward one-month returns on mispriced (or underpriced/ overpriced) stocks. All of the independent variables are the same as those in Table 3.4. The coefficients on lockup capital across all six models are insignificant indicating that the profits on once mispriced stocks are no long significant after a period of high lockup capital. The results in Table 3.5 support the previous results and demonstrate that the mispricing has been corrected by hedge funds with high lockup capital.

## 3.4 Conclusion

The limits of arbitrage theory examined by Shleifer and Vishny (1997) lead to a series in the literature that explores methods for investors to combat the limits. We explore one of those limits, funding risk, which is proxied by the dynamic lockup of hedge funds. We use mispricing in the stock market as one of the arbitrage risks, and examine how funding risks by hedge funds affect stock mispricing. Our results indicate that when individual hedge funds have lower funding risks or more stable capital, they are more likely to take greater mispricing risks. When the hedge fund industry has more stable capital, the return on mispriced stocks increase indicating that the mispricing has been corrected during the period. After a period of high stable capital, we do not find abnormal performance on the mispriced stocks supporting our argument that hedge funds help to correct mispricing. We also confirm that hedge funds engage in trading against underpricing more than overpricing.

Figure 3.1: Stock capitalization of overpriced and underpriced stocks and hedge funds capital under lockup

This figure outlines the aggregated market capitalization of stocks under the top 10% and bottom 10% by mispricing measures under blue and red lines, respectively. The green line indicates the amount of capital under lockup in the hedge fund industry from 1994-2013.

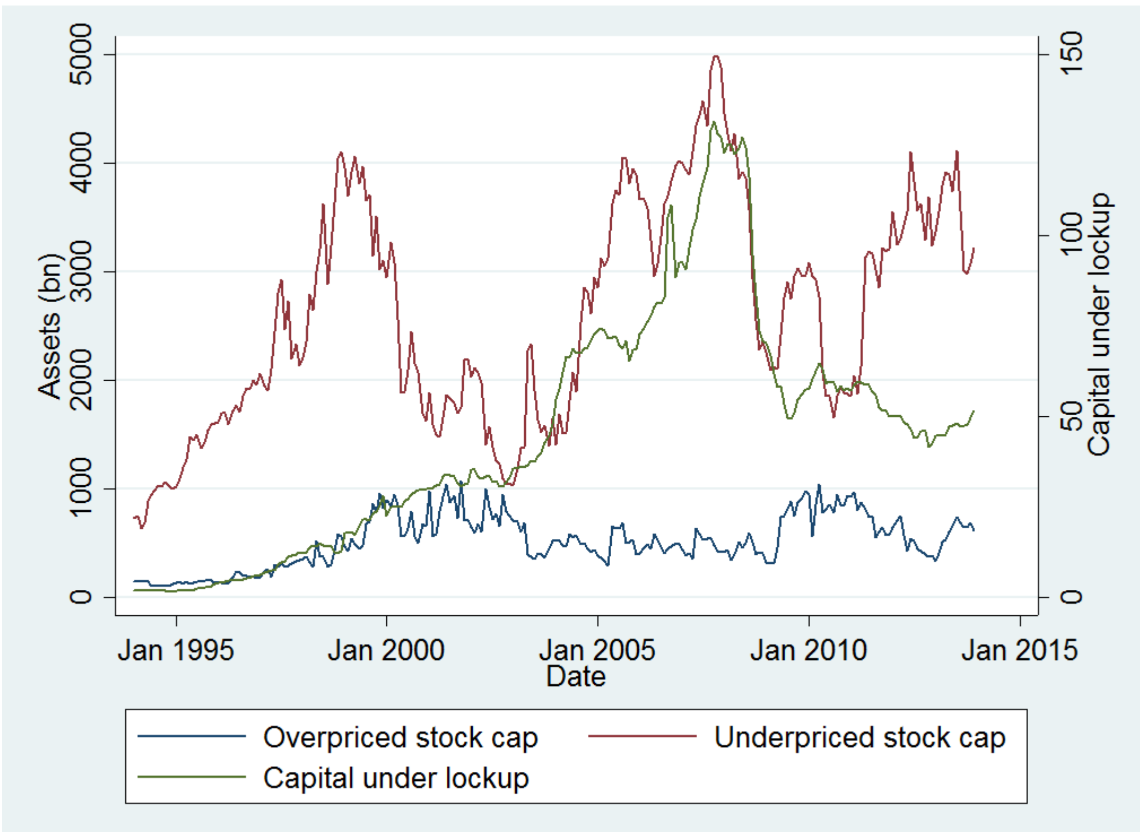

Table 3.1: **Summary statistics for hedge fund sample**

This table presents summary statistics for hedge funds with a lockup term (Panel A) and without a lockup term (Panel B). It reports the aggregated Assets Under Management and aggregated lockup capital from 1994-2013. The cash flow and dynamic lockup are averaged across all funds during the same month.

Panel A. Lockup hedge funds sample

| | N | mean | p50 | sd | min | p10 | p25 | p75 | p90 | max |
|---|---|---|---|---|---|---|---|---|---|---|
| Aggregated AUM ($bn) | 240 | 160.84 | 157.71 | 120.20 | 7.56 | 13.09 | 40.62 | 243.56 | 320.52 | 422.28 |
| Cash flow (%) | 240 | 1.4% | 1.3% | 1.8% | -3.9% | -0.7% | 0.5% | 2.3% | 3.5% | 10.6% |
| Lockup capital ($bn) | 240 | 44.64 | 43.32 | 32.93 | 1.45 | 3.73 | 16.00 | 63.74 | 89.58 | 131.50 |
| Dynamic lockup (%) | 240 | 29.4% | 29.4% | 6.6% | 17.9% | 20.3% | 23.5% | 34.2% | 38.2% | 43.6% |

Panel B. Non-lockup hedge funds sample

| | N | mean | p50 | sd | min | p10 | p25 | p75 | p90 | max |
|---|---|---|---|---|---|---|---|---|---|---|
| Aggregated AUM ($bn) | 240 | 436.08 | 368.58 | 312.11 | 43.77 | 63.35 | 150.01 | 741.24 | 848.06 | 1,006.79 |
| Cash flow (%) | 240 | 1.3% | 1.4% | 1.3% | -5.8% | 0.1% | 0.7% | 2.1% | 3.0% | 5.0% |
| Placebo lockup capital ($bn) | 240 | 112.53 | 123.77 | 78.64 | 12.07 | 16.53 | 42.42 | 164.32 | 209.46 | 292.69 |
| Placebo dynamic lockup (%) | 240 | 26.9% | 27.1% | 4.2% | 19.2% | 21.2% | 23.6% | 29.3% | 32.4% | 37.7% |

Table 3.2: **Summary statistics for mispriced stocks**

This table presents the summary statistics for stocks classified as underpriced stocks and overpriced stocks from 1994-2013. Panel A summarizes the aggregate market capitalization of all underpriced and overpriced stocks. The returns are equally-weighted returns on the portfolios of underpriced and overpriced stocks. Panel B lists the beta coefficient and alpha (constant) on the Fama and French (1993) and Carhart (1997) four factor models. We report *t*-statistics in square brackets. ***, **, * represent statistical significance at the 1%, 5%, and 10% level respectively.

Panel A. Summary on mispriced stocks

| | Mean | Median | sd | min | p25 | p75 | max |
|---|---|---|---|---|---|---|---|
| Underpriced stock value ($bn) | 2,614.4 | 2,588.3 | 1,031.4 | 630.3 | 1,763.1 | 3,557.2 | 4,987.1 |
| Overpriced stock value ($bn) | 512.5 | 500.2 | 241.8 | 99.6 | 348.5 | 685.2 | 1,069.3 |
| Return on Underpriced stocks (1) | 0.0153 | 0.0213 | 0.0435 | -0.1621 | -0.0091 | 0.0450 | 0.1263 |
| Return on Overpriced stocks (2) | -0.0011 | 0.0052 | 0.0718 | -0.2674 | -0.0406 | 0.0419 | 0.2257 |
| Difference in (1) and (2) | 0.0163 | 0.0143 | 0.0412 | -0.1729 | -0.0040 | 0.0363 | 0.1680 |

Panel B. Alpha of mispriced stocks portfolios

| | 1<br>Underprice | 2<br>Overprice | 3<br>Spread |
|---|---|---|---|
| mktrf | 0.8232*** | 1.1078*** | -0.2846*** |
| | [40.118] | [38.463] | [-7.833] |
| hml | 0.2618*** | 0.0040 | 0.2578*** |
| | [9.229] | [0.101] | [5.131] |
| smb | 0.4854*** | 0.8112*** | -0.3258*** |
| | [18.273] | [21.753] | [-6.924] |
| umd | 0.0730*** | -0.3775*** | 0.4505*** |
| | [4.299] | [-15.831] | [14.974] |
| Alpha | 0.0082*** | -0.0079*** | 0.0161*** |
| | [9.529] | [-6.547] | [10.569] |

Table 3.3: **Hedge fund dynamic lockup portfolio regression**

This table reports factor exposures, and alphas for a series of equally-weighted portfolios. Each month, funds are sorted into quantiles based on their *Dynamic Lockup*. The dependent variables are the returns from the highest dynamic lockup portfolio minus that of the lowest dynamic lockup portfolio. In Models 1-3, we report portfolio consisting of lockup funds. In Models 4-6, we report portfolio consisting of non-lockup funds. The mispricing risk factors are created based on Stambaugh et al. (2012, 2015). We also include the risk factors from the Fung and Hsieh (2004) 7-factor model. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Lockup | | | Non-lockup | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| Underpricing factor | 11.364* | | | -1.929 | | |
| | [1.95] | | | [-1.02] | | |
| Overpricing factor | | -3.594 | | | 1.658 | |
| | | [-1.15] | | | [1.07] | |
| Mispricing factor | | | 4.593* | | | 1.903 |
| | | | [1.80] | | | [0.54] |
| mktrf | 2.399 | -8.403* | 4.695 | 1.058 | -1.158 | 2.659 |
| | [1.02] | [-1.71] | [1.08] | [0.74] | [-0.39] | [1.01] |
| sizespread | -1.673 | -7.891** | -0.188 | -1.249 | -2.464 | -0.293 |
| | [-0.65] | [-2.13] | [-0.06] | [-0.80] | [-1.09] | [-0.15] |
| bondmarket | 33.827 | 27.272 | 33.463 | -20.212 | -22.193 | -19.604 |
| | [0.96] | [0.78] | [0.94] | [-0.94] | [-1.03] | [-0.91] |
| creditspread | 29.791 | 45.236 | 32.626 | 11.861 | 17.245 | 10.840 |
| | [0.60] | [0.92] | [0.65] | [0.39] | [0.58] | [0.36] |
| ptfsbd | 0.794 | 0.662 | 0.736 | 0.323 | 0.265 | 0.325 |
| | [1.38] | [1.17] | [1.27] | [0.93] | [0.77] | [0.93] |
| ptfsfx | -0.564 | -0.638 | -0.544 | -0.339 | -0.353 | -0.327 |
| | [-1.20] | [-1.36] | [-1.16] | [-1.19] | [-1.23] | [-1.15] |
| ptfscom | -0.628 | -0.478 | -0.658 | 0.382 | 0.413 | 0.360 |
| | [-1.00] | [-0.76] | [-1.04] | [1.00] | [1.08] | [0.94] |
| alpha | 0.339*** | 0.309*** | 0.394*** | 0.174*** | 0.186*** | 0.189*** |
| | [3.60] | [3.08] | [4.59] | [3.04] | [3.04] | [3.63] |
| Observations | 239 | 239 | 239 | 239 | 239 | 239 |
| R-squared | 0.037 | 0.040 | 0.029 | 0.037 | 0.034 | 0.037 |

Table 3.4: **Mispricing return and lockup capital**

This table reports the results of the time series regressions of monthly mispricing returns on aggregated lockup capital. The dependent variables in Models 1 and 4 are the difference between the average return of underpriced stocks and overpriced stocks. The dependent variables in Models 2 and 5 are average return of underpriced stocks. The dependent variables in Models 3 and 6 are the average return of the overpriced stocks. We include aggregate placebo lockup capital and aggregate hedge fund flows as control variables. We also include the /citeFama1993 three factors and two additional variables used in Akbas et al. (2015) - the average liquidity in the market and the average stock turnover, the coefficients of which are omitted for brevity.. $t$-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

| | 1<br>Mispricing return | 2<br>Underpricing return | 3<br>Overpricing return | 4<br>Mispricing return | 5<br>Underpricing return | 6<br>Overpricing return |
|---|---|---|---|---|---|---|
| Lockup capital | 0.0044* | 0.0023** | -0.0021 | 0.0043* | 0.0023** | -0.0020 |
| | [1.935] | [2.472] | [-1.114] | [1.886] | [2.530] | [-1.031] |
| Placebo lockup capital | 0.0024 | -0.0004 | -0.0028 | 0.0024 | -0.0003 | -0.0027 |
| | [0.682] | [-0.312] | [-0.969] | [0.683] | [-0.193] | [-0.911] |
| HF flows | | | | 0.0094 | -0.0799 | -0.0893 |
| | | | | [0.055] | [-1.158] | [-0.628] |
| Observations | 240 | 240 | 240 | 239 | 239 | 239 |
| R-squared | 0.448 | 0.918 | 0.873 | 0.449 | 0.919 | 0.873 |

Table 3.5: **Forward mispricing return and lockup capital**

This table reports the results of the time- series regressions of monthly mispricing returns on aggregated lockup capital. Dependent The dependent variables in Models 1 and 4 are the difference between the average return of the underpriced stocks and the overpriced stocks. Dependent The dependent variables in Models 2 and 5 are the average return of the underpriced stocks. Dependent The dependent variables in Models 3 and 6 are the average return of the overpriced stocks. We include aggregate placebo lockup capital and aggregate hedge fund flows as control variables. We also include the Fama and French (1993) three factors and two additional variables used in Akbas et al. (2015), - average liquidity in the market and average stock turnover, the coefficients of which are omitted for brevity. t-statistics are reported in square brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | 1<br>Mispricing return | 2<br>Underpricing return | 3<br>Overpricing return | 4<br>Mispricing return | 5<br>Underpricing return | 6<br>Overpricing return |
|---|---|---|---|---|---|---|
| Lockup capital | 0.0030 | -0.0004 | -0.0035 | 0.0026 | -0.0004 | -0.0030 |
| | [1.044] | [-0.142] | [-0.677] | [0.902] | [-0.113] | [-0.577] |
| Placebo lockup capital | 0.0058 | -0.0057 | -0.0116 | 0.0049 | -0.0054 | -0.0103 |
| | [1.304] | [-1.184] | [-1.460] | [1.094] | [-1.110] | [-1.297] |
| HF flows | | | | 0.4635** | -0.1419 | -0.6054 |
| | | | | [2.131] | [-0.599] | [-1.567] |
| Observations | 239 | 239 | 239 | 238 | 238 | 238 |
| R-squared | 0.090 | 0.049 | 0.061 | 0.108 | 0.052 | 0.072 |

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