# The Causal Effect of Limits to Arbitrage on Asset Pricing Anomalies

Yongqiang Chu     David Hirshleifer     Liang Ma*

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**Abstract**

We examine the causal effect of limits to arbitrage on ten well-known asset pricing anomalies using Regulation SHO, which relaxes short-sale constraints for a random set of pilot stocks, as a natural experiment. We find that the anomalies become substantially weaker on portfolios constructed with pilot stocks during the pilot period. Regulation SHO reduces the combined anomaly long-short portfolio returns by 77 basis points per month, a difference which survives risk adjustment with standard factor models. The effect comes only from the short legs of the anomaly portfolios.

Keywords: Limits to Arbitrage, Anomalies, Short Sale Constraints, Regulation SHO

JEL classification: G12, G18

*Yongqiang Chu: Department of Finance, Moore School of Business, University of South Carolina. Email: yongqiang.chu@moore.sc.edu. David Hirshleifer: Merage Chair in Business Growth, Merage School of Business, University of California at Irvine. Email: david.h@uci.edu. Liang Ma: Department of Finance, Moore School of Business, University of South Carolina. Email: liang.ma@moore.sc.edu. We are grateful for comments from Karl Diether, Lukasz Pomorski, Lin Sun, and participants at the 2016 Rodney L. White Center for Financial Research Conference on Financial Decisions and Asset Markets at Wharton.

# 1 Introduction

Over the last several decades, finance researchers have discovered many cross-sectional asset pricing anomalies, wherein predetermined security characteristics predict future stock returns.[1] Such patterns can derive from either rational risk premia or market mispricing. The mispricing explanation goes hand-in-hand with the idea that there are limits to arbitrage which delay the flow of wealth from irrational to sophisticated investors (Shleifer and Vishny 1997). In contrast, if return predictability is the result of rational risk premia for bearing factor risk, limits to arbitrage should not affect expected returns.

It is therefore interesting to ascertain whether return anomalies are driven by limits to arbitrage. However, it is hard empirically to measure pure variations in limits to arbitrage which exclude variations in other economic forces that might affect either risk premia or mispricing. In this paper, we study the *causal* effect of limits to arbitrage on ten well-known asset pricing anomalies—namely, the momentum, gross profitability, asset growth, investment to assets, return on assets, net operating assets, accruals, net stock issuance, composite equity issuance, and financial distress anomalies. These ten were the focus of Stambaugh, Yu, and Yuan (2012) in their study of sentiment and anomalies, and were chosen because of their survival after adjusting for the Fama-French three factors. Examining the causal effect of limits to arbitrage on these anomalies provides insight into whether, and to what extent, well-known return anomalies derive from risk versus mispricing.

It is challenging to identify the causal effect of limits to arbitrage, as we seldom directly observe them, or pure variations in them. The existing literature therefore often relies on firm characteristics, such as idiosyncratic volatility, size, and stock liquidity, as proxies for limits of arbitrage. However, these proxies are likely to be correlated with risk. For example, size has been offered as the basis for a risk factor in the three-factor model of Fama and French (1993), and volatility can be a risk measure in models with limited diversification such

[1] Harvey, Liu, and Zhu (2015) provide a comprehensive list of variables that can predict cross-sectional stock returns.

as settings with costs of trading or with asymmetric information. This raises the possibility that effects attributed to limits to arbitrage may actually be due to rational risk premia.

We offer here a pure test of the causal effect of limits to arbitrage on asset pricing anomalies. Short sale constraints are one of the most important limits of arbitrage (e.g., Jones and Lamont 2002, Lamont and Thaler 2003, Nagel 2005, Gromb and Vayanos 2010). Research on the effect of short-sale constraints on asset prices relies mainly on indirect proxies such as breadth of ownership (Chen, Hong, and Stein 2002), institutional ownership (Asquith, Pathak, and Ritter 2005, Nagel 2005, Hirshleifer, Teoh, and Yu 2011), firm size (Ali and Trombley 2006, Israel and Moskowitz 2013), short interest (Asquith, Pathak, and Ritter 2005), and shorting cost estimated from stock borrowing and lending behavior (Jones and Lamont 2002, Geczy, Musto, and Reed 2002, Drechsler and Drechsler 2014). Several of these proxies may be correlated across stocks or over time with variations in factor risk.

We exploit a natural experiment, Regulation SHO, to identify the *causal* effect of limits to arbitrage, and in particular short-sale constraints, on asset pricing anomalies. Regulation SHO was adopted by the Securities and Exchange Commission (SEC) in July 2004. The pilot program of Regulation SHO removes short sale restrictions on a randomly selected group of stocks. Within stocks in the Russell 3000 index as of June 2004, Regulation SHO removed the "uptick rule" for a subset of NYSE/AMEX stocks.[2] Regulation SHO designated every third stock ranked by trading volume on each of NYSE and AMEX (as well as Nasdaq) as pilot stocks. The pilot stocks were exempted from the uptick rule or the bid price test from May 2, 2005 to August 6, 2007. The pilot program made it easier to short sell pilot stocks relative to non-pilot stocks. Because the assignment of pilot and non-pilot firms is

[2] In particular, the uptick rule prevents short sales from being placed when stock prices are decreasing. Regulation SHO also removed the bid price test for a subset of NASDAQ stocks. (The bid price test prevents short sales from being placed at or below the (inside) bid when the current inside bid is at or below the previous insider bid.) However, as pointed out by Diether, Lee, and Werner (2009), the bid price test for Nasdaq stocks is not very restrictive, and a significant fraction of trading volume in Nasdaq-listed stocks is executed on ArcaEx and INET, which do not enforce bid price tests. As a result, the effect of Regulation SHO on Nasdaq-listed stocks should be minimal. We therefore focus on NYSE/AMEX stocks.

essentially random, the program provides an ideal setting to examine the causal effect of short-sale constraints on asset pricing anomalies.

We investigate two hypotheses about the effect of short-sale constraints on ten well-known asset pricing anomalies, which follow from the broader hypothesis that these anomalies represent mispricing. The first is that the anomalies will become weaker for pilot firms relative to non-pilot firms during the pilot period. During the pilot period, arbitrageurs could more easily short pilot firms to construct arbitrage portfolios, reducing mispricing. It follows that the return spread of arbitrage portfolios declines for pilot firms relative to non-pilot firms.

To test the first hypothesis, for each asset pricing anomaly, we construct long-short portfolios with pilot and non-pilot stocks separately. Specifically, we first sort all pilot firms into deciles according to the return-predicting characteristic, and then calculate the anomaly returns as the return differences between the highest performing decile based on existing anomaly evidence (the long leg) and the lowest performing decile (the short leg). We then do the same with all non-pilot stocks. In a difference-in-differences framework, we find that the anomalies are much weaker in long-short portfolios constructed using pilot stocks during the pilot period. The effect is statistically significant in four of the ten anomalies. When the ten anomalies are combined in a joint test, the effect is both statistically and economically significant. Regulation SHO reduces the anomaly returns by 77 basis points per month, or 9.24% per year.

The second hypothesis is that the effect of short selling on asset pricing anomalies will come mostly from the short leg portfolios. In general, anomaly returns can come from either overpriced short legs or underpriced long legs. A loosening of short-sale constraints should reduce profitability of short leg arbitrage portfolios. In the same difference-in-differences framework, we find that the returns of short leg portfolios constructed with pilot stocks are significantly and substantially higher during the pilot period, i.e., short strategies become less profitable. In contrast, there is no significant effect of the pilot program on long leg portfolios.

These results show that limits to arbitrage, and in particular, short-sale constraints play an important role in generating the ten well-known anomalies. These findings therefore suggest that these anomalies are, at a minimum, driven in substantial part by mispricing. We also show that our basic results remain intact after adjustment for the CAPM and the Fama-French three-factor model (Fama and French 1993). Moreover, we show that as the difference in short-sale restrictions between pilot and non-pilot stocks disappears after Regulation SHO ends in August 2007, the difference in anomaly returns between pilot and non-pilot also vanishes, which offers further validation of the conclusions of the main tests.

As a placebo test, we maintain the assignment of pilot and non-pilot firms but change the timing of the pilot period fictitiously to 2001-2003 and test whether this fictitious pilot program also affects the asset pricing anomalies during the 1980-2003 period.[3] We find that the fictitious pilot program has no effect on asset pricing anomalies, which suggests that the basic results are indeed driven by the pilot program of Regulation SHO. As another falsification test, we show that the effect of Regulation SHO on asset pricing anomalies is small and insignificant for Nasdaq stocks, which again confirms that our main results come from the relaxation of short-sale constraints.

We further explore the cross-sectional heterogeneity in the effect of Regulation SHO on asset pricing anomalies. Diether, Lee, and Werner (2009) argue that small and less liquid stocks can be more affected by the suspension of the uptick rule.[4] Consistent with this, we

[3] We end the placebo test sample in year 2003 so that the actual pilot program does not affect the placebo test results.

[4] The effect of the uptick rule on impeding short selling is expected to be greater for smaller and less liquid stocks. The reason is that these stocks have wider spreads and therefore short sellers have to become liquidity providers to ensure compliance with the uptick rule, which makes short-sale orders more passive in the presence of the uptick rule. In addition, for small stocks, a penny tick may be a more significant impediment to shorting them. Consistent with this argument, Diether, Lee, and Werner (2009) find that the suspension of the uptick rule has a greater effect on spreads and some intraday volatility measures for small and less liquid stocks.

find that the effect of easier short-selling on anomalies is more pronounced among small and less liquid stocks.

The behavioral finance literature has long argued that limits to arbitrage help explain the persistence of asset pricing anomalies despite the incentives of sophisticated investors to trade profitably against such anomalies (Shleifer and Vishny 1997, Hirshleifer 2001, Barberis and Thaler 2003, Gromb and Vayanos 2010). Empirical tests have examined the association between various proxies for limits to arbitrage and asset returns. These proxies for limits to arbitrage include stock price (Pontiff 1996, Mashruwala, Rajgopal, and Shevlin 2006), size (Pontiff 1996, Ali, Hwang, and Trombley 2003, Israel and Moskowitz 2013), idiosyncratic volatility (Ali, Hwang, and Trombley 2003, Mashruwala, Rajgopal, and Shevlin 2006), transaction costs (Ali, Hwang, and Trombley 2003), investor sophistication (Ali, Hwang, and Trombley 2003), dollar trading volume (Mashruwala, Rajgopal, and Shevlin 2006), and capital constraints of merger arbitrageurs (Baker and Savaşoglu 2002) in the context of merger arbitrage.

Many of the proxies for limits to arbitrage used in existing literature may actually capture risk, which makes it hard to distinguish between risk-based and mispricing-based explanations of anomalies. As documented by Lam and Wei (2011), proxies for limits to arbitrage are often highly correlated with proxies for investment frictions (risk).[5]

Our paper is more closely related to the empirical literature on how short sale constraints or costs affect asset prices and asset pricing anomalies.[6] One strand of this literature employs indirect proxies for short-sale constraints (Chen, Hong, and Stein 2002, Nagel 2005, Ali and Trombley 2006, Asquith, Pathak, and Ritter 2005, Hirshleifer, Teoh, and Yu 2011, Israel

[5]Lam and Wei (2011) attempt to distinguish between mispricing-based and risk-based ($q$-theory with investment frictions) explanations of the asset growth anomaly. They examine a comprehensive list of proxies for limits to arbitrage: idiosyncratic volatility, the number of institutional shareholders, three measures of information uncertainty including analyst coverage, dispersion in analysts' earnings, and five measures of transaction costs including stock price, effective bid-ask spread, institutional ownership, Amihud illiquidity, and dollar trading volume.

[6]See Reed (2013) and the references therein for more discussion on the role of short selling in financial markets.

and Moskowitz 2013). The indirect proxies of short-sale constraints used in this strand of literature may, however, also reflect variations in risk.

Another strand of this literature uses more direct proxies for short sale costs or constraints, measured using data sets of stock borrowing and lending (D'Avolio 2002, Geczy, Musto, and Reed 2002, Jones and Lamont 2002, Cohen, Diether, and Malloy 2007, Cao, Dhaliwal, Kolasinski, and Reed 2007, Saffi and Sigurdsson 2010, Engelberg, Reed, and Ringgenberg 2014, Drechsler and Drechsler 2014, Beneish, Lee, and Nichols 2015). These proxies are more direct in the sense that they are associated with aspects of the equity lending process, e.g. with stock loan fees and stock lending supply. Nevertheless, these proxies can still be correlated across stocks or over time with shifts in factor risk, so that the return effects can still be risk premia. In contrast, the natural experiment in our paper focuses on a regulatory shift that only alters permitted short-selling behavior, and therefore is unlikely to be correlated with shifts in factor risk.

Our paper contributes to the existing literature by providing a clean and powerful test of the causal effect of limits to arbitrage in general and short-sale constraints in particular on asset pricing anomalies. In contrast with existing literature that mainly relies on proxies for limits to arbitrage and short-sale constraints (which may capture risk or be correlated with risk as discussed above), we use exogenous shocks to short-sale constraints generated by a natural experiment, Regulation SHO. The randomness of the assignment of pilot and non-pilot stocks makes a stock's assignment unlikely to be correlated with the loadings of stocks on risk factors. We are therefore able to conclude from our analysis whether limits of arbitrage and thereby mispricing actually affect asset pricing anomalies.

# 2 Data and Anomalies

## 2.1 Sample

Starting with the June 2004 Russell 3000 index, we follow the procedure described in SEC's first pilot order of Regulation SHO (Securities Exchange Act Release No. 50104) to build our sample of pilot and non-pilot stocks. We exclude stocks that were not listed on the NYSE, AMEX, or Nasdaq NM, and stocks that went public or had spin-offs after April 30, 2004. The initial sample consists of 986 pilot stocks (based on the list published in the SEC's pilot order[7]) and 1,966 non-pilot stocks. We then merge this initial sample with the Center for Research in Security Prices (CRSP) and Compustat (both annual and quarterly data) data sets to form portfolios and analyze portfolio returns of the ten anomalies. As mentioned in the introduction, Diether, Lee, and Werner (2009) point out that the bid price test for Nasdaq-listed stocks is likely to have minimal effect. Our final sample therefore consists of pilot and non-pilot stocks in the Regulation SHO program that are listed on NYSE or AMEX at portfolio formation. Within the initial sample of pilot and non-pilot stocks of Regulation SHO, 1,025 non-pilot stocks and 515 pilot stocks are included in our final sample, among which 1,477 stocks are traded on NYSE and 63 stocks are traded on AMEX. The ratio of non-pilot stocks to pilot stocks is roughly 2:1.[8] The sample period for our main empirical analysis is from January 1980 to July 2007, after which the pilot program of Regulation SHO ends.

## 2.2 Anomalies

We focus on the ten anomalies studied also by Stambaugh, Yu, and Yuan (2012) which they select based on survival after adjustment for the Fama-French three factors. Stambaugh,

[7] https://www.sec.gov/rules/other/34-50104.htm

[8] In untabulated analysis, we examine the robustness of our results when we set the number of pilot and non-pilot firms to be equal, by randomly removing half of the non-pilot firms with simulation. We show that our results are robust in this aspect.

Yu, and Yuan (2012) in fact select 11 anomalies, but two of them represent the financial distress anomaly, the O-score (Ohlson 1980) and the firm failure probability by Campbell, Hilscher, and Szilagyi (2008). Since these two variables are conceptually similar (and both measure the probability of default) and the failure probability is better estimated with a dynamic logit model incorporating both accounting and market information as opposed to a static model, we only include failure probability as the return-predictor for the financial distress anomaly.[9]

Below we briefly describe each anomaly, leaving details of variable construction for the Appendix. For each anomaly, there is a corresponding long-short trading strategy that goes long in the stocks that tend to earn high returns (the long leg) and those that earn low returns (the short leg). The relationship between the subsequent stock performance and the ranking variable is positive for some anomalies and negative for others. For example, stocks with high past returns outperform those with low past returns for the momentum anomaly, while stocks with low asset growth rate outperform those with high asset growth rate for the anomaly of asset growth. Table 1 summarizes the characteristics of stocks in the long and short legs for each anomaly.

*Anomaly 1: Momentum.* The momentum effect in stock returns was first documented by Jegadeesh and Titman (1993), and is one of the most prominent anomalies in asset pricing. It refers to the phenomenon that stocks with higher past recent returns continue to outperform stocks with lower past recent returns. We employ the conventional 11-1-1 momentum strategy to construct our momentum portfolios. The ranking period is 11-month from $t-12$ to $t-2$. The holding period is month $t$. Month $t-1$ is skipped to eliminate the short-run reversal effect.

[9]There are also two anomaly variables related to equity issuance, i.e. net stock issues and composite equity issues, but these two differ in capturing share issuance activity over different horizons. Net stock issues is an annual (1-year) issuance variable and composite equity issues is a 5-year issuance variable. We therefore include both of them in our analysis. However, our main results and conclusion remain if we include only one of them.

*Anomaly 2: Gross profitability.* As documented by Novy-Marx (2013), stocks with high gross profitability on average earn higher returns than stocks with low gross profitability. He further shows that the profitability premium becomes more pronounced after controlling for the value premium. Following Novy-Marx (2013), we measure gross profitability as total revenue minus cost of goods sold, scaled by total assets.

*Anomaly 3: Asset growth.* Cooper, Gulen, and Schill (2008) find that stocks with a high growth rate in their total assets earn low subsequent returns. A possible explanation for this phenomenon is that investors tend to overreact to growth rates in total assets. We measure asset growth as the change in total assets, scaled by lagged total assets.

*Anomaly 4: Investment to assets.* Titman, Wei, and Xie (2004) find that firms increasing capital investments earn negative benchmark-adjusted returns subsequently. They propose that this phenomenon is consistent with the hypothesis that investors underreact to the empire building implications of increased investment expenditures. We measure investment to assets as the annual change in gross property, plant, and equipment plus the annual change in inventories, scaled by lagged total assets.

*Anomaly 5: Return on assets.* Fama and French (2006) document that in Fama-MacBeth cross-sectional regressions, earnings can predict stock returns. Chen, Novy-Marx, and Zhang (2011) and Stambaugh, Yu, and Yuan (2012) find that return on assets, measured on a quarterly basis, can predict subsequent stock returns. A higher past return on assets leads to higher subsequent stock returns. We measure return on assets as quarterly earnings scaled by quarterly total assets.

*Anomaly 6: Net operating assets.* Hirshleifer, Hou, Teoh, and Zhang (2004) find that firms with higher net operating assets earn lower subsequent returns. They attribute this phenomenon to investor limited attention. Net operating assets capture cumulative differences between operating income and free cash flow. Investors with limited attention may not process all information thoroughly and therefore may focus on accounting profitability without sufficiently taking into account cash profitability information, leading to overvaluation of firms with higher net operating assets. We measure net operating assets as the difference between all operating assets and all operating liabilities on the balance sheet, scaled by lagged total assets.

*Anomaly 7: Accruals.* As documented by Sloan (1996), firms with higher accruals on average earn lower subsequent returns. This suggests that stock prices fail to fully reflect information contained in the accruals and cash flow components of current earnings, which is consistent with investors having limited attention. We measure operating accruals as changes in non-cash working capital minus depreciation expense, scaled by lagged total assets.

*Anomaly 8: Net stock issues.* As documented by Loughran and Ritter (1995) and Pontiff and Woodgate (2008), net share issues negatively predict stock returns in the cross section. One explanation for this phenomenon in the literature is that firms issue stocks when they are overvalued and retire stocks when they are undervalued. We measure net stock issues on the annual basis as the change in the natural logarithm of a firm's adjusted shares over the last year.

*Anomaly 9: Composite equity issues.* Daniel and Titman (2006) find that an alternative measure of equity issuance, the composite equity issuance, is also a negative predictor of stock returns in the cross section. They propose that this measure is related to the "intangible" component of past returns. Measured as the part of growth rate in market equity not attributable to stock returns, composite equity issuance captures the amount of equity a firm issues (or retires) in exchange for cash or services. As a result, this measure increases with seasoned equity issuance, employee stock option plans, and share-based acquisitions, and decreases with share repurchases, dividends, and other actions that take cash out of the firm.

*Anomaly 10: Financial distress.* Dichev (1998) shows that more distressed firms earn lower subsequent returns on average than less distressed firms. We use the failure probability proposed by Campbell, Hilscher, and Szilagyi (2008) to measure financial distress, which is estimated from a dynamic logit model to match empirically observed default events, with both market and accounting information taken into account. Campbell, Hilscher, and Szilagyi (2008) show that with this measure, more distressed firms earn lower subsequent returns on average than less distressed firms, especially after 1981.

## 2.3 Summary of Anomaly Returns in Our Sample

Before proceeding to the main empirical analysis, we first verify the existence of the ten anomalies in our sample of pilot and non-pilot firms. For each anomaly, we sort stocks in our sample into deciles based on the corresponding ranking variables and calculate the anomaly returns as the return differences between the highest performing decile (the long leg) and the lowest performing decile (the short leg).

We use data from CRSP to construct portfolios of Anomalies 1 and 9, use Compustat annual data to construct portfolios for Anomalies 2, 3, 4, 6, 7, and 8, use Compustat quarterly data to construct portfolios for Anomaly 5, and use CRSP and Compustat quarterly data to construct portfolios for Anomaly 10. For anomalies that use annual Compustat data, we follow Fama and French (1992) to match the accounting data for all fiscal years ending in calendar year $t-1$ with the stock returns from July of year $t$ to June of $t+1$. For anomalies that use quarterly Compustat data, we use accounting information lagged by one quarter to match with stock returns.

We examine the average of raw anomaly returns and benchmark-adjusted anomaly returns controlling for the Capital Asset Pricing Model (CAPM) and the Fama-French three factor model over the sample period January 1980 to December 2004. We end the sample period in December 2004 to avoid overlap with the pilot program. The average of benchmark-adjusted returns is the alpha from regressing the time series of raw returns onto the time series of appropriate factors (the market excess return for the CAPM, and two additional factors, the SMB and HML factors, for the Fama-French three-factor model). Table 2 reports these average returns.

Table 2 reveals that the long-short portfolio returns for all ten anomalies survive risk-adjustment with the Fama-French three-factor model. The average Fama-French-three-factor-adjusted anomaly returns are listed in the last column of Table 2 and they are positive and statistically significant for all ten anomalies. These results are consistent with the evidence in Stambaugh, Yu, and Yuan (2012). We therefore confirm that these anomalies exist on our more restricted sample of stocks.

# 3 Empirical Analysis

Our two main hypotheses are:

**Hypothesis 1.** *The relaxation of short sale constraints caused by Regulation SHO reduces anomaly returns for pilot stocks relative to non-pilot stocks.*

**Hypothesis 2.** *This decrease in anomaly returns comes primarily from the short leg anomaly portfolios.*

## 3.1 Verifying the Randomness of Pilot Stocks

Before testing Hypotheses 1 and 2, we verify the random assignment of firms to the pilot program. As discussed above, the pilot firms are assigned in what appears to be a random fashion (every third firm in a sorting of firms by trading volume on NYSE and, separately, on AMEX).

Before conducting the difference-in-differences tests, it is important to empirically verify that the pilot firms are in fact randomly assigned with respect to firm characteristics. To do so, we compare firm characteristics associated with the ten anomalies between pilot and non-pilot firms at the end of year 2003, before the announcement of the pilot program (July 2004). We calculate the mean of these anomaly variables for the pilot and non-pilot firms at the end of year 2003, and calculate their differences and the robust $t$-statistics of the differences. All variables are winsorized at the 1st and 99th percentiles of all firm-month observations to limit the effect of outliers. The results are reported in Table 3. Except for the measure of gross profitability, for which the difference is only significant at the 10% level, other anomaly predictors show no statistically significant differences between the pilot and non-pilot firms. Furthermore, the difference in gross profitability between pilot and non-pilot firms is small in magnitude compared with the two sample means.[10] These results collectively suggest that there is no significant difference between the pilot and control firms before the announcement of the pilot program.

## 3.2 Basic Difference-in-Differences Results

To test the two hypotheses, we explore whether the pilot program of Regulation SHO leads to differences in anomaly returns for the pilot stock sample compared to the non-pilot stock sample. We first construct anomaly portfolios based on pilot and non-pilot firms separately. Specifically, we sort all pilot stocks into deciles according to the predictors of the anomalies, and then calculate the returns of the highest performing decile (the long leg returns), the returns of the lowest performing decile (the short leg returns), and the difference between the two (the long-short returns). We then do the same on all non-pilot firms. We then examine whether the returns of pilot portfolios are different from returns on non-pilot portfolios during the pilot period using a difference-in-differences approach.

[10] In untabulated results, we confirm that the difference in size and book-to-market ratio between pilot and non-pilot firms is also small and statistically insignificant.

The main difference-in-differences test employs the following specification:

$$r_{it} = \alpha_t + \beta Pilot_i \times During_t + \beta_1 Pilot_i + \epsilon_{it}, \tag{1}$$

where $r_{it}$ is the monthly return of portfolio $i$, which can be the long leg, the short leg, or the long-short portfolio of an anomaly, in month $t$; $\alpha_t$ is the time fixed effects; $Pilot_i$ is a dummy variable which is equal to one if portfolio $i$ is formed on pilot firms, and zero otherwise; $During_t$ is a dummy variable, which equals one if month $t$ is between June 2005 and July 2007, i.e. when the pilot program of Regulation SHO takes action. Since $During_t$ is subsumed by the time fixed effects, it is dropped from the regression. The time fixed effects $\alpha_t$ capture the common factors and/or common macroeconomic variables that drive the portfolio returns for both pilot and non-pilot portfolios. In these regressions, the unit of analysis is a portfolio month observation. The regression in equation (1) is carried out for the long leg, the short leg, and the long-short portfolio separately.

The difference-in-differences coefficient $\beta$ in equation (1) is the main coefficient of interest. It captures the effect of the pilot program on portfolio returns for pilot stocks relative to non-pilot stocks. We run the regression in equation (1) for each individual anomaly and also for all ten anomalies combined. In the aggregate analysis, we replace the time fixed effects by the anomaly-time fixed effects, i.e. the fixed effects associated with each pair of anomaly and time. The aggregate analysis enhances the power of our test and produces the average effect of the pilot program across all ten anomalies. The results are reported in Table 4.

Hypothesis 1 predicts that $\beta$ is negative for anomaly long-short returns. Hypothesis 2 predicts that it is positive for short-leg returns, and approximately zero for long-leg returns. The results support the two hypotheses. For the long-short returns, $\beta$'s are consistently negative for all ten anomalies and are statistically significant for four of them. When the ten anomalies are combined, i.e. in the aggregate analysis where equation (1) is estimated for all ten anomalies together with the time fixed effects replaced by the anomaly-time fixed effects, $\beta$ is $-0.77\%$ with a $t$-statistic of $-4.92$. In other words, the pilot program reduces the monthly anomaly returns by 77 basis points per month, or 9.24 percentage points per year, on average. These results support Hypothesis 1.

The results also indicate that the decreases in anomaly returns come almost entirely from the short legs. For short leg portfolios, $\beta$'s are consistently positive for all ten anomalies and are statistically significant for six of them. When the ten anomalies are combined, $\beta$ is 0.69% with a $t$-statistic of 5.81. In contrast, for long leg portfolios, $\beta$'s are close to zero and statistically insignificant for most anomalies. When the ten anomalies are combined, the $\beta$ is still close to zero and statistically insignificant. These results support Hypothesis 2.

We also explore an alternative empirical design to capture the effect of the pilot program on anomalies. Specifically, for each portfolio (the long/short leg or the long-short portfolio) of an anomaly, we take the difference between the portfolio returns from pilot portfolios and those from non-pilot portfolios and denote the time series of this difference as $r^d_{it}$. By doing so, we isolate the cross-sectional difference between pilot and non-pilot stocks. We expect that this difference will only predict returns when $During_t = 1$. Therefore, we run the regression

$$r^d_{it} = \alpha + \beta During_t + \epsilon_{it}, \tag{2}$$

where the coefficient $\beta$ captures the effect of the pilot program on portfolio returns between pilot and non-pilot stocks. We also run the regression in equation (2) for each individual anomaly, and for all ten anomalies combined. It can be shown that the two specifications in equations (1) and (2) are mathematically equivalent, and we confirm in untabulated results that they produce identical estimates of $\beta$'s. In the rest of the paper, we focus our discussion on results from the specification in equation (1).

## 3.3 Benchmark-Adjusted Returns

In the basic empirical analysis, we used raw portfolio returns as the dependent variable in the regression equation (1). In this subsection, we test the validity of the hypotheses when we use benchmark-adjusted returns as the dependent variable in regression equation (1). If the mean return premia of the Fama-French three factors represent rational risk premia (an issue on which we do not take a stand here), then this analysis would verify whether relaxation of short sale constraints reduces mispricing measured against this benchmark.

To obtain benchmark-adjusted returns, we first regress the time series of raw returns onto the time series of appropriate factors (the market excess return for the CAPM, and two additional factors, the SMB and HML factors, for the Fama-French three-factor model). We then obtain the time series of benchmark-adjusted returns as the constant plus the residuals from the regression.

We would not expect this factor adjustment to change the results much, since the selection of pilot firms is basically random, implying similar loadings on the benchmark factors of pilot versus non-pilot firms. The results for benchmark-adjusted returns are presented in Table 5. Consistent with this intuition, all the $\beta$ estimates are similar to those in Table 4 of basic results, and Hypotheses 1 and 2 are still strongly confirmed.

## 3.4 Power

Our tests use a relatively short pilot period (a little over two years) to estimate the effect of relaxing short-sale constraints on anomaly returns. This immediately raises the question of whether the sample size generates enough power to distinguish hypotheses. In the results presented in Tables 4 and 5, we do indeed obtain statistically significant effects on some individual anomalies, especially on the short legs, as well as strong significance for the results that aggregate across the ten anomalies. This is reassuring, but we now address the issue of power explicitly.

Intuitively, our tests gain power by two means. First is the aggregation across ten anomalies. Second, even for a single anomaly in the pilot period, what is relevant for our test is not the raw strength of that anomaly, it is the *difference* in strength of an anomaly between pilot versus non-pilot firms over the same time period.[11] This differencing effectively hedges away much of the factor volatility of returns, greatly increasing the precision of the test. To see this in a very simple way, suppose that the momentum return of Portfolio A were equal to the return of Portfolio B plus a constant. Then even if both portfolios were highly volatile, the difference in returns would be a constant, implying that the difference would be significant with an infinite $t$ statistic. Of course a constant difference is unrealistic, but this example illustrates that testing for a difference filters out a large amount of variability from the test.

Consistent with this point, in untabulated results, we show that taking differences between portfolios constructed with pilot and non-pilot stocks substantially reduces return volatility. Monthly standard deviations of return differences between long-leg/short-leg portfolios constructed with pilot and non-pilot stocks are much smaller than those of returns on long-leg/short-leg portfolios themselves. For example, averaged across the ten anomalies, the monthly standard deviation of return differences between pilot and non-pilot stocks for the short leg is 1.65%, while the monthly standard deviation of short leg returns is 4.17% for non-pilot stocks and 3.98% for pilot stocks.

This contrasts with conventional tests for estimating average anomaly returns (rather than differences in returns), in which sampling noise derived from factor realizations reduces power. In such tests, much longer time periods are often needed to confirm an anomaly reliably. It is of course sometimes possible to identify anomaly returns using a sample period measured in years rather than decades. For example, in an out-of-sample test of their

[11]Econometrically, this is achieved by including anomaly-time fixed effects in our regression specification. Also, our tests actually examine the difference in this difference between the pilot and non-pilot periods, but this is not crucial for our argument.

1993 paper (Jegadeesh and Titman 1993), Jegadeesh and Titman (2001) find significant momentum in the sample period of 1990 to 1998 (9 years), with a $t$-statistic of 4.71.

A further consideration which enhances the power of our tests is that the pilot period is one in which the ten anomalies are relatively strong. If we estimate the mean anomaly returns (long-minus-short returns) on *non-pilot stocks* during the pilot period, the anomalies tend to be stronger, both economically and statistically, than might ordinarily be expected for a three-year period.

Specifically, we calculate the mean monthly anomaly returns and CAPM/Fama-French-three-factor alphas for the ten anomalies individually and in aggregate, over the pilot period from June 2005 to July 2007. Table 6 presents the results for non-pilot stocks. It shows that the anomaly returns and alphas of the ten anomalies for non-pilot stocks are mostly positive (28 out of 30), and many of them are statistically significant (13 out of 30). When we combine the ten anomalies together, both the mean return and alphas are positive and statistically significant. The magnitudes are also large. The mean monthly return and alphas are about 57-79 bps, when the ten anomalies are combined.

# 4 Robustness and Further Analysis

## 4.1 The Effect of the Ending of Regulation SHO

The pilot program of Regulation SHO ends in August 2007, and the difference in short-sale restrictions between pilot and non-pilot stocks disappeared afterwards (see e.g. Fang, Huang, and Karpoff 2016). If our basic results are indeed driven by the pilot program, we should expect that the difference in anomaly returns between pilot and non-pilot firms also vanishes after the pilot program ends. To test this, we convert our basic difference-in-differences specification in equation (1) into:

$$r_{it} = \alpha_t + \beta Pilot_i \times During_t + \beta_1 Pilot_i + \beta_2 Pilot_i \times Post_t + \epsilon_{it}, \tag{3}$$

where $Post_t$ is a dummy variable that represents the post-pilot period and equals 1 if month $t$ is after July 2007 and zero otherwise, and other notation is defined exactly the same as in equation (1). The difference-in-differences coefficient $\beta_2$ in equation (3) is the coefficient of interest in this subsection and we expect it to be close to zero.

We run the regression in equation (3) for the whole sample period of January 1980 to December 2013. The coefficients $\beta$ (representing the effect of the pilot program of Regulation SHO) are identical to those reported in Table 4 (which can be shown mathematically) and therefore are not shown. Table 7 reports the coefficients $\beta_2$ for the ten anomalies individually and in aggregate. All coefficients $\beta_2$ are statistically insignificant and they are close to zero when ten anomalies are combined. This confirms that as the difference in short-sale restrictions between pilot and non-pilot firms disappears, the difference in anomaly returns between them also vanishes, which is consistent with our main conclusions.

## 4.2 A Placebo Test

In general, a potential problem with the difference-in-differences method is that the results can be driven by unobservable shocks that affect pilot firms and non-pilot firms differently, which may then undermine the causal inference of the basic difference-in-differences results. Because of the volume ranking method used to choose pilot firms, this choice seems close to a random draw. This makes it unlikely that unobserved shocks are highly correlated with the assignment of pilot and non-pilot firms. Nonetheless, as a precaution, we conduct a falsification test.

Specifically, we create a pseudo-event in year 2000 and perform a test as if the pseudo-event relaxes short-sale constraints for the pilot firms.[12] To mimic the actual pilot program closely, we assume that this pseudo pilot program is effective from May 2001 to August 2003. We then run the difference-in-differences regression (similar to equation (1)) as follows:

$$r_{it} = \alpha_t + \beta Pilot_i \times PseudoDuring_t + \beta_1 Pilot_i + \epsilon_{it}, \quad (4)$$

where $PsedudoDuring_t$ is a dummy variable, which equals one if month $t$ is between between June 2001 and July 2003, i.e., when the pseudo-event is effective and other notation is defined exactly the same as in equation (1). The sample period is from January 1980 to December 2003 to avoid the contamination effect from the real pilot program.

The results of the placebo test are presented in Table 8. The coefficients on $Pilot_i \times PseudoDuring_t$ are mostly statistically insignificant and have mixed signs in Table 8. When all ten anomalies are combined, the coefficients are much smaller in magnitude than those in Table 4 for the short leg and the long-short portfolio and are statistically insignificant. The placebo test results therefore suggest that our basic results are unlikely to be driven by unobserved shocks that affect pilot and non-pilot firms differently.

## 4.3 Different Sample Periods

We conduct robustness checks with respect to the sample period. By doing so, we address any possible concerns regarding the fact that the pilot period is shorter (in a relative sense) than the non-pilot period and examine whether our choice of the beginning year of the sample period is critical. Specifically, we explore two shorter sample periods: January 1990 to July 2007 and January 2000 to July 2007. The results are presented in Panels A and B of Table 9, respectively. The $\beta$ estimates are qualitatively similar across different sample

[12] We choose year 2000 for a pseudo-event as it is prior to the real event and the pseudo pilot program created accordingly does not overlap with the real pilot program. In untabulated results, we find similar results when we create a pseudo-event in e.g. year 1998 or year 1999.

periods. In aggregate, the $\beta$ estimates are both statistically and economically significant for the short leg and the long-short portfolio with the two different sample periods. With all ten anomalies combined, the coefficient $\beta$ for the long-short portfolio is $-0.59\%$ with a $t$-statistic of $-3.57$ from 1990 to 2007 and the $\beta$ is $-0.83\%$ with a $t$-statistic $-4.12$ from 2000 to 2007. The coefficient $\beta$ for the short leg is $0.58\%$ with a $t$-statistic 4.64 from 1990 to 2007 and $0.85\%$ with a $t$-statistic 5.72 from 2000 to 2007. These results again lend support to the two hypotheses.

Furthermore, we show in untabulated results that our basic results can also be identified using the pilot period *per se*, by simply taking the difference between portfolio returns of pilot and non-pilot stocks during the pilot period.[13] This suggests again that anomaly returns in non-pilot and pilot stocks are very similar before the pilot period, and our basic results indeed come from the pilot stocks during the pilot period. Finally, we also run the regression in equation (3) for the sample period of January 2003 to December 2009, and thereby set the lengths of the pre-pilot period, the pilot period, and the post-pilot period to be roughly equal. In untabulated results, we show that the results from this regression are similar to those reported in Tables 4 and 7.

## 4.4 Results from Nasdaq Stocks

Our main empirical analysis is carried out on the sample of pilot and non-pilot stocks traded on NYSE/AMEX. In this subsection, we conduct a falsification test using the sample of pilot and non-pilot stocks traded on Nasdaq. As stated in the introduction, Regulation SHO also removed the bid price test for pilot stocks traded on Nasdaq. Nevertheless, the bid price test for Nasdaq stocks is not very restrictive and a significant fraction of trading volume in Nasdaq-listed stocks is executed on ArcaEx and INET that do not enforce it (see

[13]Specifically, when we take differences between portfolio returns of pilot and non-pilot stocks during the pilot period for the ten anomalies and estimate their means, we obtain qualitatively similar estimates to those reported in Table 4.

e.g. Diether, Lee, and Werner 2009). We therefore expect that at most a minimal effect of Regulation SHO on anomaly returns of Nasdaq-listed stocks.

This falsification test helps rule out a potential alternative explanation for our main results. Specifically, one might argue that Regulation SHO changed the information environment of pilot stocks and made them more salient, and this increase in salience for pilot stocks drives our main results. Since both Nasdaq pilot stocks and NYSE/AMEX pilot stocks are included in the pilot program of Regulation SHO, the same information environment change (if any) would also occur for Nasdaq pilot stocks during the same pilot period. So if this salience mechanism were driving our main results, we should observe an apparent effect of Regulation SHO on anomaly returns of Nasdaq stocks similar to that of NYSE/AMEX stocks. On the other hand, if the relaxation of short-sale constraints drives our main results, we would expect to see a minimal effect on anomaly returns of Nasdaq stocks.

We repeat our basic DiD analysis (equation (1)) on the sample of pilot and non-pilot stocks traded on Nasdaq National Market; the results are reported in Table 10. The DiD coefficient $\beta$ is mostly statistically insignificant and has mixed signs across the ten anomalies, for the long-leg, short-leg, and long-short returns. In aggregate, the coefficient is also small and insignificant for the long-leg, short-leg, and long-short returns. Overall, these results indicate that Regulation SHO does not affect anomaly returns for Nasdaq stocks much and confirm that our main results derive from the relaxation of short-sale constraints generated by Regulation SHO.

# 5 The Effect of Short-Sale Constraints on Mispricing of Different Kinds of Stocks

We next explore how Regulation SHO affects asset pricing anomalies among different classes of stocks. As mentioned in the introduction, Diether, Lee, and Werner (2009) argue that small and less liquid stocks can be more affected by the suspension of the uptick rule. In our context, we test whether the effect of Regulation SHO on asset pricing anomalies is more pronounced for small and less liquid stocks.

We first explore the difference in the effects of Regulation SHO on small versus large stocks. To this end, at the end of each month $t - 1$, we first split our sample into two subsamples of small and large stocks based on their market capitalization. Large stocks are those with market capitalization above the median and small stocks are those with market capitalization below the median. We then form anomaly portfolios using pilot/non-pilot stocks in these subsamples, collect portfolio returns in month $t$, and repeat the main difference-in-difference analysis for each subsample.[14] The results are presented in Table 11.

In Panel A, the difference-in-differences estimates on long-short portfolio returns are negative for all ten anomalies and statistically significant for two of them, consistent with the argument that decrease in short sale constraints reduces the anomalies. When we aggregate over all ten anomalies, the pilot program reduces the long-short portfolio return by 69 basis points. The results also show that the effect concentrates only on short-leg portfolios. The difference-in-differences estimates on short-leg portfolio returns are positive for all ten anomalies and statistically significant for six out of the ten anomalies. The statistical significance of these six estimates is on average slightly enhanced compared with their counterparts in Table 4.

[14] In the empirical analysis of this section, we sort pilot/non-pilot stocks within each subsample into quintiles based on the corresponding ranking variables and calculate the anomaly returns as the return differences between the highest performing quintile (the long leg) and the lowest performing quintile (the short leg).

In contrast, the effect is much weaker for the subsample of large stocks. In Panel B, the difference-in-differences estimates on long-short portfolios switch signs across anomalies. The overall difference-in-differences estimate when the ten portfolios are combined is small and statistically insignificant. The results in Table 11 therefore suggest that the effect of the pilot program on asset pricing anomalies concentrates mostly on small stocks.

Next, we explore the difference in the effects of removing the uptick rule on less liquid versus more liquid stocks. To this end, at the end of each month $t-1$, we first split our sample into two subsamples of less liquid and more liquid stocks based on the Amihud illiquidity measure (Amihud 2002) calculated using daily returns in month $t-1$. The Amihud illiquidity measure for a stock $k$ in month $\tau$ is calculated as follows:

$$ILLIQ_{k\tau} = \frac{1}{D_{k\tau}} \sum_{d=1}^{D_{k\tau}} \frac{|r_{k\tau d}|}{VOL_{k\tau d}}, \tag{5}$$

where $D_{k\tau}$ is the number of trading days for which data are available for stock $k$ in month $\tau$, $r_{k\tau d}$ is the return of stock $k$ in day $d$ of month $\tau$, and $VOL_{k\tau d}$ is the dollar trading volume of stock $k$ in day $d$ of month $\tau$. Less liquid stocks are those with the Amihud measure above the median and more liquid stocks are those with the Amihud measure below the median.

We then form anomaly portfolios using pilot/non-pilot stocks in these subsamples, collect portfolio returns in month $t$, and repeat the main difference-in-difference analysis for each subsample. The results are presented in Table 12. Although the overall difference-in-differences estimates (when ten anomalies are combined) on both less liquid and more liquid stocks are negative and statistically significant, the magnitude of the estimate is much larger on less liquid stocks. The results again suggest that the effect of the pilot program on asset pricing anomalies is more pronounced among less liquid stocks.

We also note that the anomalies themselves are stronger among small and less liquid stocks. The subsample results above therefore imply that the uptick rule which is expected to impede short selling more for small and less liquid stocks contributes to the stronger anomalies in these stocks.

# 6 Conclusion

Using the pilot program of Regulation SHO, which relaxed short-sale constraints for a random set of stocks, we examine the causal effect of limits to arbitrage, and in particular short sale constraints, on ten well-known asset pricing anomalies. We find that the long-short strategies for the ten anomalies produce much smaller abnormal returns on portfolios constructed with pilot stocks during the pilot period. This suggests that these anomalies reflect mispricing, and that making arbitrage easier reduces such mispricing. The effect of the pilot program is only significant for the short legs of the anomaly long-short portfolios, which is consistent with the prediction that easy short arbitrage weakens the short side of anomalies. Finally, we show that the effect of Regulation SHO on asset pricing anomalies is more pronounced among small and less liquid stocks. Together, these findings provide strong and clearcut confirmation that limits to arbitrage have a *causal* effect on the strength of well-known asset pricing anomalies.

# Appendix: Definition of Anomaly Variables

The data to construct anomaly variables are from CRSP and annual and quarterly Compustat.

**Anomaly 1: Momentum (RET)**. The past return $RET_t$ for a stock is calculated as the compounded return over the 11-month ranking period $t-12$ to $t-2$.

**Anomaly 2: Gross profitability (GP/A)**. The gross profitability measure $GP/A_t$ for a firm is calculated as the difference between total revenue ($REVT_t$) and cost of goods sold ($COGS_t$), scaled by total asset: $AT_t$.

**Anomaly 3: Asset growth (AG)**. The asset growth measure $AG_t$ for a firm is calculated as the the change in total asset $AT_t - AT_{t-1}$, scaled by total asset $AT_{t-1}$.

**Anomaly 4: Investment to assets (IVA)**. Investment to assets $IVA_t$ is defined as the annual change in gross property, plant, and equipment plus the annual change in inventories, $PPEGT_t - PPEGT_{t-1} + INVT_t - INVT_{t-1}$, scaled by lagged total asset $AT_{t-1}$.

**Anomaly 5: Return on assets (ROA)**. Return on assets $ROA_t$ is measured as the quarterly earnings, or income before extraordinary item, $IBQ_t$, scaled by quarterly total asset $ATQ_t$.

**Anomaly 6: Net operating assets (NOA)**. Net operating assets are calculated as the difference between operating assets and operating liabilities, scaled by lagged total assets: $NOA_t = (Operating\ Assets_t - Operating\ Liabilities_t)/AT_{t-1}$, where $Operating\ Assets = Total\ Assets\ (AT) - Cash\ and\ Short\text{-}term\ Investment\ (CHE)$, and $Operating\ Liabilities = Total\ Assets\ (AT) - Short\text{-}term\ Debt\ (DLC) - Long\text{-}term\ Debt\ (DLTT)$
$- Minority\ Interest\ (MIB) - Preferred\ Stock\ (PSTK) - Common\ Equity\ (CEQ)$.

**Anomaly 7: Accruals (AC)**. Operating accruals are measured as the change in non-cash current assets, less the change in current liabilities (exclusive of short-term debt and taxes payable), less depreciation expense, all divided by lagged total assets: $Accruals_t =$
$[(\Delta Current\ Assets - \Delta Cash) - (\Delta Current\ Liabilities - \Delta Short\text{-}term\ Debt - \Delta Taxes\ Payable) -$

*Depreciation and Amortization Expense*)]/$AT_{t-1}$. In terms of Compustat item notations, *Current Assets* is ACT, *Cash* is CHE, *Current Liabilities* is LCT, *Short-term Debt* is DLC, *Taxes Payable* is TXP, and *Depreciation and Amortization Expense* is DP.

**Anomaly 8: Net stock issues (NSI)**. Net stock issues on the annual basis are measured as the change in the natural logarithm of a firm's split-adjusted shares over the last year, $NSI_t = Ln(Adjusted\ Shares_t) - Ln(Adjusted\ Shares_{t-1})$, where $Adjusted\ Shares_t$ is the product of the common share outstanding ($CSHO_t$) and the adjustment factor ($AJEX_t$).

**Anomaly 9: Composite equity issues (CEI)**. Composite equity issues are measured over the past five-year window and it is defined as the part of growth rate in market equity not attributable to stock returns, $CEI_t = Ln(ME_t/ME_{t-5}) - r(t-5,t)$. In June of year $t$, for example, $ME_t$ is the market equity at the end of June of year $t$ and $ME_{t-5}$ is the market equity at the end of June in year $t-5$, while $r(t-5,t)$ is the cumulative log return of the stock from the end of June of year $t-5$ to the end of June of year $t$.

**Anomaly 10: Financial distress (Distress)**. Following Campbell, Hilscher, and Szilagyi (2008), we use coefficients in Column 4 of their Table IV to construct the measure of $Distress_t$, which is related to the failure probability through a monotonic transformation. Specifically, $Distress_t$ is measured as:

$$\begin{aligned} Distress_t = & -9.16 - 20.26NIMTAAVG_t + 1.42TLMTA_t - 7.13EXRETAVG_t \\ & + 1.41SIGMA_t - 0.045RSIZE_t - 2.13CASHMTA_t + 0.075MB_t - 0.058PRICE_t, \end{aligned}$$

where the details of variables can be found in Campbell, Hilscher, and Szilagyi (2008).

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Table 1: **Characteristics of stocks for the ten anomalies**

This table summarizes the characteristics of stocks in the long leg (the highest performing group) and those in the short leg (the lowest performing group).

| | Stocks in the long leg | Stocks in the short leg |
|---|---|---|
| Momentum | High past return | Low past return |
| Gross profitability | High gross profitability | Low gross profitability |
| Asset growth | Low asset growth | High asset growth |
| Investment to assets | Low investment to assets | High investment to assets |
| Return on assets | High return on assets | Low return on assets |
| Net operating assets | Low net operating assets | High net operating assets |
| Accruals | Low accruals | High accruals |
| Net stock issues | Low equity issuance | High equity issuance |
| Composite equity issues | Low equity issuance | High equity issuance |
| Financial distress | Low financial distress | High financial distress |

Table 2: **Summary of anomaly returns in our sample**

This table reports the mean monthly raw return, the CAPM $\alpha$, and the Fama-French-three-factor $\alpha$ for the ten anomalies individually and in aggregate, constructed using stocks in our sample. The sample period is January 1980 to December 2004. For each anomaly, stocks are sorted into deciles based on the corresponding ranking variable and the raw anomaly return is obtained as the portfolio return of buying the highest performing decile and shorting the lowest performing decile. All returns are monthly and in percentage. The robust $t$-statistics are presented in the parentheses below the estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Raw | CAPM $\alpha$ | Fama-French $\alpha$ |
|---|---|---|---|
| Momentum | 0.80** | 0.81** | 0.99*** |
| | (2.42) | (2.50) | (2.68) |
| Gross profitability | 0.13 | 0.06 | 0.38** |
| | (0.74) | (0.35) | (2.19) |
| Asset growth | 0.60*** | 0.65*** | 0.43*** |
| | (3.50) | (3.82) | (2.97) |
| Investment to assets | 0.51*** | 0.57*** | 0.46*** |
| | (3.24) | (3.69) | (2.91) |
| Return on assets | 0.16 | 0.21 | 0.53*** |
| | (0.69) | (0.98) | (2.67) |
| Net operating assets | 0.54*** | 0.51*** | 0.52*** |
| | (3.82) | (3.69) | (3.61) |
| Accruals | 0.31* | 0.36** | 0.33** |
| | (1.94) | (2.31) | (2.12) |
| Net stock issues | 0.41*** | 0.44*** | 0.42*** |
| | (3.13) | (3.47) | (3.20) |
| Composite equity issues | 0.26 | 0.42*** | 0.39** |
| | (1.55) | (2.68) | (2.49) |
| Financial distress | -0.05 | 0.19 | 0.44* |
| | (-0.23) | (0.94) | (1.91) |

Table 3: **Comparing pilot and non-pilot firms: anomaly variables**

This table compares pilot and non-pilot firms in terms of the ten ranking variables corresponding to the ten asset pricing anomalies, at the end of year 2003. The notations for these ranking variables are reported in Column (1) and the details of variable definition are in the Appendix. Columns (2) and (3) report the mean of these variables while Column (4) reports their difference. All variables are winsorized at the 1st and 99th percentiles of all firm-month observations to remove the effect of outliers. The robust $t$-statistics for the differences are reported in Column (5). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | (1) Variable | (2) Pilot mean | (3) Non-pilot mean | (4) Difference | (5) $t$-stat |
|---|---|---|---|---|---|
| Momentum | $RET$ | 0.442 | 0.452 | -0.010 | -0.452 |
| Gross profitability | $GP/A$ | 0.293 | 0.269 | 0.024* | 1.867 |
| Asset growth | $AG$ | 0.134 | 0.166 | -0.032 | -0.772 |
| Investment to assets | $IVA$ | 0.052 | 0.066 | -0.015 | -1.077 |
| Return on assets | $ROA$ | 0.010 | 0.010 | 0.001 | 0.775 |
| Net operating assets | $NOA$ | 0.605 | 0.593 | 0.012 | 0.474 |
| Accruals | $AC$ | -0.048 | -0.049 | 0.001 | 0.182 |
| Net stock issues | $NSI$ | 0.045 | 0.052 | -0.007 | -0.603 |
| Composite equity issues | $CEI$ | 0.028 | 0.048 | -0.020 | -0.847 |
| Financial distress | $Distress$ | -8.172 | -8.143 | -0.029 | -0.830 |

Table 4: **Basic results**

This table reports the coefficient $\beta$ from the regression in equation (1) for the ten anomalies individually and all of them in aggregate. The sample period is January 1980 to July 2007. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Long leg | Short leg | Long-short |
|---|---|---|---|
| Momentum | -0.08 | 0.75 | -0.83 |
| | (-0.22) | (1.48) | (-1.29) |
| Gross profitability | 0.04 | 0.72** | -0.67 |
| | (0.14) | (2.02) | (-1.57) |
| Asset growth | -0.08 | 0.50* | -0.58 |
| | (-0.21) | (1.96) | (-1.40) |
| Investment to assets | 0.08 | 0.35 | -0.27 |
| | (0.21) | (0.99) | (-0.49) |
| Return on assets | 0.14 | 0.85 | -0.71 |
| | (0.66) | (1.52) | (-1.15) |
| Net operating assets | -0.11 | 0.95*** | -1.06** |
| | (-0.32) | (3.83) | (-2.28) |
| Accruals | -0.44 | 0.80** | -1.25*** |
| | (-1.27) | (2.37) | (-2.60) |
| Net stock issues | 0.04 | 0.78*** | -0.74* |
| | (0.13) | (3.23) | (-1.95) |
| Composite equity issues | -0.52* | 0.51* | -1.03** |
| | (-1.75) | (1.68) | (-2.59) |
| Financial distress | 0.12 | 0.68 | -0.56 |
| | (0.44) | (1.56) | (-1.15) |
| Combination | -0.08 | 0.69*** | -0.77*** |
| | (-0.81) | (5.81) | (-4.92) |

Table 5: **Benchmark-adjusted return results**

This table reports the coefficient $\beta$ from equation (1) for the ten anomalies individually and all of them in aggregate, with benchmark-adjusted returns used as the dependent variable. Panel A displays results for CAPM-adjusted returns while Panel B displays results for Fama-French-three-factor-adjusted (FF-adjusted) returns. The sample period is January 1980 to July 2007. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Panel A: CAPM-adjusted returns | | | Panel B: FF-adjusted returns | | |
|---|---|---|---|---|---|---|
| | Long leg | Short leg | Long-short | Long leg | Short leg | Long-short |
| Momentum | -0.08 | 0.75 | -0.83 | -0.10 | 0.75 | -0.85 |
| | (-0.22) | (1.48) | (-1.29) | (-0.24) | (1.49) | (-1.27) |
| Gross profitability | 0.04 | 0.71** | -0.67 | 0.03 | 0.71** | -0.68 |
| | (0.14) | (2.02) | (-1.54) | (0.12) | (2.03) | (-1.59) |
| Asset growth | -0.08 | 0.50* | -0.58 | -0.10 | 0.50** | -0.60 |
| | (-0.21) | (1.96) | (-1.39) | (-0.26) | (2.00) | (-1.38) |
| Investment to assets | 0.07 | 0.35 | -0.27 | 0.05 | 0.34 | -0.29 |
| | (0.20) | (0.99) | (-0.49) | (0.13) | (1.00) | (-0.50) |
| Return on assets | 0.14 | 0.85 | -0.71 | 0.13 | 0.85 | -0.71 |
| | (0.68) | (1.52) | (-1.15) | (0.61) | (1.50) | (-1.15) |
| Net operating assets | -0.11 | 0.95*** | -1.06** | -0.12 | 0.95*** | -1.07** |
| | (-0.33) | (3.83) | (-2.28) | (-0.34) | (3.80) | (-2.26) |
| Accruals | -0.44 | 0.80** | -1.25** | -0.46 | 0.80** | -1.26*** |
| | (-1.27) | (2.34) | (-2.56) | (-1.37) | (2.29) | (-2.67) |
| Net stock issues | 0.04 | 0.77*** | -0.74* | 0.03 | 0.78*** | -0.75* |
| | (0.13) | (3.21) | (-1.95) | (0.12) | (3.14) | (-1.95) |
| Composite equity issues | -0.52* | 0.51* | -1.03*** | -0.52* | 0.50* | -1.02*** |
| | (-1.77) | (1.68) | (-2.60) | (-1.77) | (1.68) | (-2.65) |
| Financial distress | 0.12 | 0.68 | -0.55 | 0.11 | 0.68 | -0.56 |
| | (0.44) | (1.57) | (-1.17) | (0.41) | (1.57) | (-1.16) |
| Combination | -0.08 | 0.69*** | -0.77*** | -0.09 | 0.69*** | -0.78*** |
| | (-0.82) | (5.81) | (-4.90) | (-0.90) | (5.81) | (-4.90) |

Table 6: **Asset pricing anomalies during the pilot period**

This table presents the mean monthly raw return, the CAPM $\alpha$, and the Fama-French three factor $\alpha$ of the ten asset pricing anomalies individually and in aggregate, over the pilot period of June 2005 to July 2007. The anomaly portfolios are constructed with non-pilot stocks. The robust $t$-statistics are presented in the parentheses below the estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Raw | CAPM $\alpha$ | FF $\alpha$ |
|---|---|---|---|
| Momentum | 1.23* | 1.48* | 1.57* |
| | (1.76) | (1.77) | (1.80) |
| Gross profitability | 0.52 | 0.46 | 0.69* |
| | (1.39) | (1.09) | (1.85) |
| Asset growth | 0.32 | 0.37 | 0.34 |
| | (0.84) | (0.97) | (0.70) |
| Investment to assets | 0.15 | 0.32 | 0.37 |
| | (0.32) | (0.64) | (0.59) |
| Return on assets | 0.94* | 1.08* | 1.23* |
| | (1.73) | (1.72) | (1.93) |
| Net operating assets | 0.72* | 0.92** | 1.09*** |
| | (1.99) | (2.50) | (3.07) |
| Accruals | -0.10 | 0.08 | -0.24 |
| | (-0.23) | (0.15) | (-0.56) |
| Net stock issues | 0.38 | 0.46 | 0.51 |
| | (1.19) | (1.50) | (1.61) |
| Composite equity issues | 0.15 | 0.32 | 0.24 |
| | (0.50) | (1.03) | (0.81) |
| Financial distress | 1.39** | 1.76** | 2.06*** |
| | (2.09) | (2.18) | (2.85) |
| Combination | 0.57** | 0.72*** | 0.79*** |
| | (2.76) | (3.44) | (4.30) |

Table 7: **The effect of ending of Regulation SHO**

This table reports the coefficient $\beta_2$ from the regression in equation (3) for the ten anomalies individually and all of them in aggregate. The sample period is January 1980 to December 2013. The unit of $\beta_2$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Long leg | Short leg | Long-short |
|---|---|---|---|
| Momentum | -0.09 | 0.77 | -0.87 |
| | (-0.31) | (1.05) | (-1.08) |
| Gross profitability | 0.06 | -0.18 | 0.24 |
| | (0.21) | (-0.37) | (0.44) |
| Asset growth | 0.18 | -0.02 | 0.20 |
| | (0.46) | (-0.07) | (0.44) |
| Investment to assets | -0.05 | 0.27 | -0.31 |
| | (-0.10) | (0.71) | (-0.64) |
| Return on assets | 0.34 | 0.70 | -0.36 |
| | (1.44) | (1.42) | (-0.64) |
| Net operating assets | 0.07 | -0.26 | 0.33 |
| | (0.17) | (-0.71) | (0.67) |
| Accruals | 0.39 | -0.27 | 0.66 |
| | (0.73) | (-0.80) | (1.08) |
| Net stock issues | -0.17 | -0.06 | -0.11 |
| | (-0.56) | (-0.16) | (-0.22) |
| Composite equity issues | 0.06 | -0.06 | 0.12 |
| | (0.22) | (-0.15) | (0.23) |
| Financial distress | 0.08 | -0.08 | 0.16 |
| | (0.32) | (-0.20) | (0.33) |
| Combination | 0.09 | 0.08 | 0.01 |
| | (0.78) | (0.58) | (0.03) |

Table 8: **The placebo test results**

This table reports the results from the placebo test. We create a pseudo-event in year 2000 and assume the pseudo-event also relaxes short-sale constraints for the pilot firms. To mimic the actual pilot program closely, we assume that this pseudo-event is effective from May 2001 to August 2003. We then run the difference-in-differences regression in equation (4) for the ten anomalies individually and all of them in aggregate. The sample period is from January 1980 to December 2003. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Long leg | Short leg | Long-short |
|---|---|---|---|
| Momentum | 0.92* | -0.26 | 1.18 |
| | (1.81) | (-0.39) | (1.21) |
| Gross profitability | 0.04 | 0.60 | -0.55 |
| | (0.11) | (1.57) | (-0.96) |
| Asset growth | 0.04 | -0.54 | 0.58 |
| | (0.07) | (-1.26) | (0.80) |
| Investment to assets | -0.28 | -0.39 | 0.12 |
| | (-0.64) | (-0.74) | (0.17) |
| Return on assets | 0.02 | 0.19 | -0.17 |
| | (0.05) | (0.36) | (-0.28) |
| Net operating assets | 0.11 | -0.43 | 0.54 |
| | (0.26) | (-0.87) | (0.87) |
| Accruals | 0.38 | -0.30 | 0.68 |
| | (0.54) | (-0.49) | (0.66) |
| Net stock issues | -0.05 | -0.21 | 0.16 |
| | (-0.13) | (-0.47) | (0.26) |
| Composite equity issues | -0.06 | 0.19 | -0.26 |
| | (-0.15) | (0.41) | (-0.40) |
| Financial distress | 0.19 | -0.77 | 0.96** |
| | (0.54) | (-1.43) | (1.98) |
| Combination | 0.13 | -0.19 | 0.32 |
| | (0.89) | (-1.18) | (1.41) |

Table 9: **Robustness test: different sample periods**

This table reports the coefficient $\beta$ from equation (1) for the ten anomalies individually and all of them in aggregate. The sample period is January 1990 to July 2007 for Panel A and January 2000 to July 2007 for Panel B. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Panel A: 1990 to 2007 | | | Panel B: 2000 to 2007 | | |
|---|---|---|---|---|---|---|
| | Long leg | Short leg | Long-short | Long leg | Short leg | Long-short |
| Momentum | -0.07 | 0.54 | -0.61 | -0.10 | 0.72 | -0.81 |
| | (-0.17) | (1.02) | (-0.89) | (-0.19) | (1.21) | (-1.01) |
| Gross profitability | 0.14 | 0.52 | -0.38 | 0.33 | 0.48 | -0.14 |
| | (0.46) | (1.40) | (-0.84) | (0.92) | (1.19) | (-0.28) |
| Asset growth | -0.01 | 0.54** | -0.55 | 0.18 | 0.90*** | -0.72 |
| | (-0.02) | (1.99) | (-1.25) | (0.37) | (2.66) | (-1.19) |
| Investment to assets | 0.13 | 0.17 | -0.04 | -0.12 | 0.34 | -0.45 |
| | (0.32) | (0.45) | (-0.07) | (-0.26) | (0.72) | (-0.65) |
| Return on assets | 0.30 | 0.85 | -0.56 | 0.49 | 1.25* | -0.76 |
| | (1.25) | (1.48) | (-0.87) | (1.41) | (1.94) | (-1.05) |
| Net operating assets | 0.13 | 0.85*** | -0.72 | -0.00 | 1.18*** | -1.18* |
| | (0.34) | (3.22) | (-1.48) | (-0.01) | (3.37) | (-1.98) |
| Accruals | -0.58 | 0.75** | -1.33** | -0.28 | 1.05** | -1.33* |
| | (-1.48) | (2.04) | (-2.51) | (-0.57) | (2.25) | (-1.91) |
| Net stock issues | 0.16 | 0.64** | -0.49 | 0.14 | 0.90** | -0.76 |
| | (0.55) | (2.36) | (-1.18) | (0.40) | (2.30) | (-1.39) |
| Composite equity issues | -0.53* | 0.33 | -0.86** | -0.60 | 0.76* | -1.36** |
| | (-1.67) | (1.03) | (-2.01) | (-1.58) | (1.89) | (-2.46) |
| Financial distress | 0.22 | 0.58 | -0.36 | 0.13 | 0.90 | -0.77 |
| | (0.75) | (1.29) | (-0.72) | (0.37) | (1.64) | (-1.29) |
| Combination | -0.01 | 0.58*** | -0.59*** | 0.02 | 0.85*** | -0.83*** |
| | (-0.11) | (4.64) | (-3.57) | (0.13) | (5.72) | (-4.12) |

Table 10: **Results from Nasdaq Stocks**

This table reports the coefficient $\beta$ from the regression in equation (1) for the ten anomalies individually and all of them in aggregate. The sample is pilot and non-pilot stocks traded on Nasdaq National Market and the sample period is January 1980 to July 2007. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Long leg | Short leg | Long-short |
|---|---|---|---|
| Momentum | 0.52 | -0.67 | 1.19 |
| | (0.83) | (-0.83) | (1.10) |
| Gross profitability | -0.16 | -1.03 | 0.87 |
| | (-0.33) | (-1.19) | (0.83) |
| Asset growth | -0.89 | -0.07 | -0.83 |
| | (-1.30) | (-0.12) | (-1.05) |
| Investment to assets | -0.33 | -0.65 | 0.32 |
| | (-0.49) | (-1.08) | (0.38) |
| Return on assets | -0.41 | -0.88 | 0.48 |
| | (-0.69) | (-1.08) | (0.48) |
| Net operating assets | -0.86 | 0.20 | -1.06 |
| | (-1.28) | (0.38) | (-1.32) |
| Accruals | 0.65 | 0.56 | 0.08 |
| | (1.04) | (1.17) | (0.11) |
| Net stock issues | -0.24 | -1.30** | 1.06 |
| | (-0.47) | (-2.45) | (1.39) |
| Composite equity issues | -0.06 | 0.62 | -0.67 |
| | (-0.14) | (0.85) | (-0.88) |
| Financial distress | -1.10** | -0.20 | -0.89 |
| | (-2.42) | (-0.27) | (-0.92) |
| Combination | -0.29 | -0.34 | 0.06 |
| | (-1.54) | (-1.58) | (0.19) |

Table 11: **Different effects for small and large stocks**

This table reports the coefficient $\beta$ from equation (1) for the ten anomalies individually and all of them in aggregate. Panel A displays results for the subsample of small stocks and Panel B contains results for the subsample of large stocks. The sample period is January 1980 to July 2007. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Panel A: Small stocks | | | Panel B: Large stocks | | |
|---|---|---|---|---|---|---|
| | Long leg | Short leg | Long-short | Long leg | Short leg | Long-short |
| Momentum | -0.25 | 0.93 | -1.18 | 0.10 | 0.10 | -0.00 |
| | (-0.58) | (1.63) | (-1.57) | (0.29) | (0.39) | (-0.00) |
| Gross profitability | 0.38 | 0.82** | -0.44 | -0.35 | 0.58** | -0.93** |
| | (1.24) | (2.35) | (-0.98) | (-1.58) | (2.07) | (-2.49) |
| Asset growth | 0.17 | 0.82*** | -0.65 | -0.10 | 0.05 | -0.16 |
| | (0.40) | (2.64) | (-1.31) | (-0.48) | (0.20) | (-0.48) |
| Investment to assets | 0.48 | 0.58 | -0.11 | -0.04 | 0.09 | -0.13 |
| | (1.32) | (1.30) | (-0.18) | (-0.15) | (0.36) | (-0.36) |
| Return on assets | 0.33 | 0.56 | -0.23 | 0.38 | 0.04 | 0.34 |
| | (1.11) | (1.13) | (-0.36) | (1.61) | (0.16) | (1.05) |
| Net operating assets | 0.65 | 0.79*** | -0.14 | 0.05 | -0.05 | 0.10 |
| | (1.59) | (2.94) | (-0.28) | (0.19) | (-0.20) | (0.29) |
| Accruals | 0.03 | 0.87* | -0.83 | -0.44 | -0.25 | -0.19 |
| | (0.07) | (1.89) | (-1.27) | (-1.38) | (-0.80) | (-0.49) |
| Net stock issues | -0.10 | 1.39*** | -1.49*** | -0.28 | 0.21 | -0.49* |
| | (-0.28) | (4.29) | (-3.44) | (-1.24) | (0.92) | (-1.82) |
| Composite equity issues | -0.12 | 1.04*** | -1.16*** | -0.25 | -0.33 | 0.08 |
| | (-0.43) | (2.84) | (-2.96) | (-1.09) | (-1.42) | (0.27) |
| Financial distress | -0.09 | 0.58 | -0.67 | 0.34 | 0.39 | -0.04 |
| | (-0.25) | (1.31) | (-1.15) | (1.28) | (1.56) | (-0.12) |
| Combination | 0.15 | 0.84*** | -0.69*** | -0.06 | 0.08 | -0.14 |
| | (1.22) | (6.38) | (-3.86) | (-0.71) | (1.02) | (-1.31) |

Table 12: **Different effects for less liquid and and more liquid stocks**

This table reports the coefficient $\beta$ from equation (1) for the ten anomalies individually and all of them in aggregate. Panel A displays results for the subsample of less liquid stocks and Panel B contains results for the subsample of more liquid stocks. The sample period is January 1980 to July 2007. The unit of $\beta$ is percentage. The robust $t$-statistics are presented in the parentheses below the coefficient estimates. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | Panel A: Less liquid stocks | | | Panel B: More liquid stocks | | |
|---|---|---|---|---|---|---|
| | Long leg | Short leg | Long-short | Long leg | Short leg | Long-short |
| Momentum | -0.20 | 0.74 | -0.94 | -0.23 | 0.32 | -0.55 |
| | (-0.43) | (1.57) | (-1.35) | (-0.77) | (1.12) | (-1.46) |
| Gross profitability | 0.42 | 0.71** | -0.30 | -0.33 | 0.55* | -0.88** |
| | (1.20) | (2.04) | (-0.68) | (-1.38) | (1.95) | (-2.49) |
| Asset growth | 0.15 | 0.70** | -0.55 | -0.17 | 0.12 | -0.29 |
| | (0.36) | (2.54) | (-1.14) | (-0.88) | (0.47) | (-0.92) |
| Investment to assets | 0.48 | 0.53 | -0.05 | -0.11 | 0.20 | -0.31 |
| | (1.23) | (1.07) | (-0.08) | (-0.45) | (0.70) | (-0.90) |
| Return on assets | 0.32 | 0.50 | -0.18 | 0.35 | 0.09 | 0.26 |
| | (1.00) | (0.99) | (-0.27) | (1.35) | (0.34) | (0.80) |
| Net operating assets | 0.65 | 0.53** | 0.11 | 0.02 | 0.38 | -0.37 |
| | (1.45) | (2.19) | (0.24) | (0.06) | (1.61) | (-1.03) |
| Accruals | -0.09 | 0.94** | -1.03* | -0.37 | -0.34 | -0.03 |
| | (-0.17) | (2.30) | (-1.71) | (-1.24) | (-0.96) | (-0.07) |
| Net stock issues | -0.27 | 1.31*** | -1.58*** | -0.20 | 0.17 | -0.37 |
| | (-0.72) | (4.69) | (-4.20) | (-0.74) | (0.71) | (-1.32) |
| Composite equity issues | -0.26 | 0.76** | -1.02*** | -0.09 | -0.17 | 0.08 |
| | (-0.91) | (2.08) | (-2.80) | (-0.35) | (-0.65) | (0.25) |
| Financial distress | -0.01 | 0.24 | -0.24 | 0.40 | 0.50* | -0.11 |
| | (-0.02) | (0.54) | (-0.44) | (1.42) | (1.88) | (-0.35) |
| Combination | 0.12 | 0.70*** | -0.58*** | -0.07 | 0.18** | -0.26** |
| | (0.94) | (5.56) | (-3.31) | (-0.86) | (2.08) | (-2.39) |