Essays in Empirical Asset Pricing and Investments

Christopher Reilly

A dissertation submitted to the Faculty of the department of Finance in partial fulfillment of the requirements for the degree of Doctor of Philosophy

Boston College
Wallace E. Carroll School of Management
Graduate School

March, 2022

Essays in Empirical Asset Pricing and Investments

Christopher Reilly

Advisors:
Jeffrey Pontiff, Ph.D.
Edith Hotchkiss, Ph.D.
Vyacheslav (Slava) Fos, Ph.D.

Abstract

My thesis contains four essays on the pricing of financial assets and the role of non-professional investors. The first two essays describe the legal framework governing Exchange-Traded Funds (ETFs) and the liquidity transformation functions of ETFs. The third essay examines how trading by nine different types of market participants are related to characteristics that have previously documented to predict the cross-section of equity returns. The fourth and final essay examines whether and how orders originating from retail brokerages respond to analyst recommendations.

In my first essay, I describe the legal framework that governs ETFs and theoretical benefits of the ETF security design relative to two other popular investment management security structures: open-end and close-end mutual funds. To do so, I briefly describe the history of the modern investment management industry. I describe the role of Authorized Participants (APs), the main security design innovation of ETFs, and highlight the key theoretical differences between the three classes of funds. Lastly, I describe SEC rulemaking that governs the behavior of ETF Managers and their APs.

In the second essay, I document a hidden but substantial cost associated with the liquidity transformation that corporate bond exchange-traded funds (ETFs) provide. When creating new shares, authorized participants (APs) deliver a subset of the portfolio of bonds that underlie a corporate bond ETF. This subset contains bonds that realize low future returns,

reducing ETF performance by 48 basis points per annum. This loss in performance cannot be attributed to forgone compensation for risk or illiquidity, but instead results from APs utilizing information regarding future changes in net asset values to strategically deliver bonds when those bonds are expected to realize poor performance in the near future.

My third essay is joint work with Jeff Pontiff and David McLean. We provide the most comprehensive study of market participation to date. We assess the informativeness of 9 different participants' trades, and how each participant's trades relate to 130 different variables that together reflect the cross-section of expected stock returns. Firms and short sellers tend to be the smart money—both sell stocks with low expected returns, and their trades predict returns in the intended direction. Firms, however, also seem to possess private information, while short sellers do not. Retail investors buy (sell) stocks with low (high) expected returns and their trades predict returns opposite to the intended direction. All 6 types of institutional investors are weighted towards stocks with low expected returns, but none of their trades robustly predict returns.

My fourth essay is joint work with Jeff Pontiff and David McLean. We ask whether retail investors are responsive to analysts' revisions. We consider revisions in recommendations, price targets, and EPS forecasts, all of which predict returns. Revisions in recommendations and price targets portend greater retail trading in the direction of the revision. The effects are stronger for All-Star Analysts' revisions, and retail investors also respond to All-Star's revisions in EPS forecasts. Retail investors trade in anticipation of revisions in price targets and recommendations, consistent with analysts or brokers "tipping" some retail investors. Retail trades earn higher returns when aligned with analysts' revision. The results show that retail investors are one channel through which analysts' information gets into prices. Our findings also support the idea that spikes in retail trading reflect informed trading, some of which is informed by analysts.

Contents

Acknowledgements

I would like to deeply thank my advisors, Jeff Pontiff, Edie Hotchkiss and Slava Fos for their guidance, wisdom, and support throughout my doctoral studies. The work contained was dramatically improved by their thoughtful and generous suggestions. Working with Jeff has been the highlight of my studies and I am greatly improved scholar for doing so. I would also like to thank the Finance Faculty of the Carroll School of Management at Boston College. My studies were greatly enriched by my interactions with all of them. In particular I would like to thank Simcha Barkai for his wit and generosity.

I am also particularly grateful for the guidance of mentorship of Nadya Malenko. Her warmth and support throughout my studies was noteworthy. Caitlin Dannhauser was also incredibly helpful in providing valuable feedback regarding my solo authored paper and helping me navigate the academic finance profession as a graduate student. I am lucky to have interacted with her.

I would additionally like to thank the graduate students of the Boston College Finance, Accounting and Economics doctoral programs. In particular, I would like to thank my entering cohort: Jiajie Xu, Song Zhang, and Francisca Rebelo.

I thank Caitlin Dannhauser, Jeff Pontiff, Edie Hotchkiss, Slava Fos, Max Clarke, John Griffin, Daniel Taylor, Ananth Madhavan, Jayoung Nam and seminar participants at the UT Austin PhD Student Symposium, Charles Schwab, Boston College, Southern Methodist University, Vanderbilt University, University of British Columbia, University of Toronto, Texas Christian University, Northeastern University, Rice, University of Texas at Dallas, Texas AM, University of Kentucky, Tulane, the Federal Reserve Board of Governors, Indiana University, HEC Paris, MIT, Arizona State University, the University of Chicago Booth School of Business, and the Midwest Finance Association for helpful comments and discussion of essay two. I would additionally like to thank Caitlin Dannhauser for providing code that greatly assisted me in writing the essay.

My third essay is joint work with Jeff Pontiff and David McLean. We thank and seminar participants at Hong Kong Polytechnic University, Chinese University of Hong Kong, Nanyang Technological University, the Norwegian School of Economics, William and Mary, George Mason, and the U.S. Securities and Exchange Commission for helpful comments. We would also like to thank Vikas Agarwal, Andrew Ellul, Slava Fos, Wei Jiang, Fabio Moneta, for sharing data regarding the identification of hedge funds, and Rick Sias for especially helpful comments.

My fourth essay is joint work with Jeff Pontiff and David McLean. For helpful comments, we thank Mark Bradshaw, Lu Zhang, Goufu Zhou, Tu Jun, seminar participants at the 2020 Shanghai Financial Forefront Symposium, and the University of Colorado Leed's School of Business and brownbag participants at Boston College and Georgetown.

Last but not least, I would like to thank my family whose love and support made it possible for me to pursue this work. My wife, Anila, made me a better person and a better scholar. My daughter, Fia, provided me with the strength and energy to complete this work. The rest of of my family has provided unbelievable support to make this outcome possible.

1 The Structure of the Asset Management Industry and the Regulatory Framework that Governs It

1.1 Introduction

The asset management industry managed over 100 trillion dollars at the end of 2021.[1] Additionally, in 2020 over 45% of US households reported own shares in mutual funds, the largest form of asset management.[2] Given the massive scale of the asset management industry, it has been of particular interest to the academic community. Three forms of asset management are of particular popularity among U.S. households: closed-end mutual funds, open-end mutual funds and ETFs. ETFs, the latest of the three security designs to be introduced, have grown from 1.3 to 7.7 trillion dollars between 2010 and 2020.[3] In this essay I outline key aspects of the regulatory framework that governs ETFs and compare ETF security design features to that off open-end and closed-end mutual funds.

1.2 History and Overview of Mutual Fund and ETF Security Designs

Although investment trusts existed in other countries, the Boston Personal Property Trust was introduced in 1893 and is credited by Brown Brothers Harriman as the first U.S. based closed-end fund. In 1934, the Massachusetts Investment Trust was introduced and allowed investors on a semiregular basis to redeem shares for a cash creating one of the earliest securities that functions in a manner similar to open-end mutual funds. While mutual funds were a large industry since the 1920's, it was almost completely dominated by active managed products in which fund mangers selected assets that they believed would provide superior returns. The first index funds were introduced in the 1970's in which managers did not seek

[1]See Boston Consulting Group's Global Asset Management 2021 Publication.
[2]See the Investment Company Institutes' 2021 Investment Company Factbook.
[3]See ETFGI's Global ESG ETF and ETP Industry Insights

to select individual assets, but instead simply track a stated portfolio of assets such as the SP 500. Since their introduction, passive mutual funds and ETFs have grown tremendously and as of 2019 US passive funds manage more assets than US actively managed mutual funds. The first ETF, the SP 500 Trust ETF (commonly referred to as "spider" in reference to its ticker SPDR) was introduced by State Street Global Investors in 1993. Since then, ETFs have seen rapid growth and as of 2021 surpassed passive mutual funds in assets under management (the industry for active ETFs is much smaller and thus open-end mutual funds still manage more total assets than ETFs). Passively managed vehicles, particularly ETFs, have also shown rapid growth over the past two decades.

Closed-end mutual funds, such as the Boston Personal Property Trust, are designed to raise a inflexible amount of capital and then invest that capital in a diversified portfolio of assets. The shares of that fund (or trust) are listed on secondary exchanges that allow investors to trade their equity claims on the fund. Thus, if an investor wishes to invest (divest) in a given closed end fund, they must purchase (sell) from an existing (entering) end investor. The trade on these secondary exchanges determines a market price for shares of these closed-end funds. Market prices (or other valuation techniques) can also be utilized to determine the net asset value (NAV) of a closed-end funds' portfolio. Because investors are not able to directly exchange a share for the net asset value (or the assets), there does not exist a direct mechanism to ensure that closed-end fund prices and NAVs stay well connected. Thus, many closed-end funds trade at a premium or discount relative to the funds' NAVs. Closed-end funds do have some mechanisms to both facilitate the flow of funds and thus reducing premiums and discounts: they can engage in secondary offerings, at the market offerings, and buyback shares. Lastly, funds can issue rights offerings that often economically incentivize existing investors to invest additional capital within the fund.

Open-end mutual funds, in contrast to closed-end funds, allow investors to directly purchase or redeem mutual fund shares for a cash equivalent to the NAV at the end of set period (daily frequencies are quite common in modern markets). Therefore, there is no economic incentive for investors to trade shares (investors instead simply transact with the fund itself) and open-end mutual funds shares are almost never listed on exchanges. Since shares can be redeemed frequently and without a discount, open-end mutual funds are often more liquid than closed-end funds. Despite this fact, many open-end funds hold quite illiquid assets (that are costly to trade), and thus the funds provide a liquidity transformation service. This liquidity transformation is the main advantage of open-end mutual funds relative to closed-end funds and is often cited when explaining the relative sizes of the open-end vs closed-end mutual fund markets. Additionally, since open-end investors can withdraw funds (thus shrinking assets under management and thus fees) from managers who perform poorly, open-end mutual funds are also often less exposed to agency costs relative to closed-end funds.

The ETF security design seeks to reduce the sizes of premiums and discounts relative to closed-end funds and thus provide a form of liquidity transformation similar to mutual funds while still trading on secondary exchanges. To do so, the ETFs are designed to change size quicker than closed-end funds in response to fund flows and to allow market participants to profitably remove discounts and premiums to the benefit of ETF investors via the share creation and redemption process. ETFs enter agreements with Authorized Participants (APs), who are allowed to create and redeem ETF shares by exchanging the assets that underlie an ETF share for a share of the ETF or vice versa. This process is referred to as "in-kind exchange." Consider a highly stylized and simplified example: suppose investors wish to hold an ETF that tracks the S&P 500 and purchase a sufficiently large quantity of the SPY ETF to positively impact SPY's secondary market price. This creates a premium in which the ETF trades at a higher price than the underlying assets. APs can seek to arbitrage this premium away by buying the underlying assets and selling short the share of the ETF. An AP notifies an ETF manager that it wishes to create a share as outlined in their agreement. At the end of the trading day, the AP delivers all of the underlying assets, receives a newly created ETF share and utilizes this share to cover the short-sale position, thereby locking in the arbitrage profit. In doing so, APs will reduce the size of premiums and discounts.

Since ETFs can be traded throughout the day at values that are often very close to NAV (although not exactly equal to NAV), ETFs are close to as liquid (or even more liquid) than open-end mutual funds and thus provide a similar liquidity transformation service. Additionally, ETFs also have the ability to defer capital gains to the benefit of end investors. Under U.S. tax rules, in-kind-exchange are not treated as the realization of capital gains on behalf of ETF investors. In contrast, when open-end or closed-end mutual funds trade, for example when open-end funds liquidate positions to meet investor redemptions, those transactions create capital gains obligations for all of the investors of the fund. For a detailed description of the tax benefits of ETFs and ETF managers strategies to maximize those benefits, see (Moussawi et al. (2019)). Lastly, historically ETFs have also charged lower management fees than mutual funds (Kostovetsky (2003)).

Many ETFs, for example ETFs that hold equities of US corporations with large market capitilizations, require their APs to deliver or receive all underlying assets when they create or redeem shares. Conversely, when ETFs hold fewer liquid assets or a very large number of assets, it is often difficult or overly cumbersome to exchange all underlying assets in kind when seeking to create shares. With corporate-bond ETFs in particular, a very small notional amount of a given bond may underlie each share, making it logistically unwieldy to locate and deliver such a small amount of a given bond. To overcome these barriers, facilitate creation activity, and thus remove premiums, ETF managers instead often allow creation to occur with a select subset of assets that they believe is representative of the full portfolio of underlying assets. For example, if a corporate bond ETF holds 1,000 separate bonds, they might at the start of the trading day identify 100 of those bonds as the "sampling basket,", also referred to as the "creation basket," which is utilized for share creation. In such a case, APs can deliver only these 100 bonds at the end of the day while delivering "cash-in-lieu" for the remaining underlying assets in exchange for a share of the bond ETF. Depending on the AP agreement established by an ETF manager, an AP can actually deliver cash in conjunction with the sampling basket. Much more commonly, APs can instead deliver the sampling basket alone and receive ETF shares on a pro-rata basis based on the NAV of the sampling basket versus the NAV of the ETF share.[4]

At the start of a trading day, ETF managers will disseminate to their APs which assets belong to that day's baskets via a clearing house such as the Depository Trust & Clearing Corporation. Additionally, APs form relationships with ETF managers and may call them to request "custom creation baskets." They may request the omission of certain assets from baskets that they cannot readily locate or suggest alternative assets. ETF managers have the right to accept these custom creation baskets at their discretion and often do so in practice. ETF managers often charge a "fee on cash" for an actual cash settlement by their APs. This is particularly necessary because bond NAVs are often priced using market bid prices rather than ask prices. Relatedly, ETF managers also charge flat creation or redemption fees to APs. Lastly, ETF managers have the right contractually to refuse creation or redemption if they deem it unfavorable to their funds, although this right is rarely exercised.

1.3 Rules and Regulations Governing Mutual Funds and ETFs

The issuance and advertising of mutual funds and ETFs are primarily governed by the Securities Act of 1933 and Investment Company Act of 1940. Both acts are broad in their scope and stipulate required filings, legal forms of disclosure and accounting and the duties of investment managers. At the federal level, the Securities and Exchange Commission (SEC) acts as the primary regulatory body that monitors mutual funds and ETFs and enforces the relevant securities laws. In addition, state or local agencies may further regulate the investment management industry or bring enforcement actions, usually under statutes governing wire fraud.

Mutual fund and ETF managers posses a fiduciary duty to their shareholders. While

[4]For example, if the sampling basket of 100 shares represented 10% of the NAV of the ETF share, they could deliver 10 shares worth of the sampling basket in exchange for one ETF share.

the legal nature of a fiduciary duty is quite complex with deep legal precedent, some legal scholars utilize a "best interest" definition in which fund managers must act in the best financial interest of their shareholders. Thus, even if an action by a fund manager is not explicitly restricted, it may be restricted by the existence of the fiduciary duties of those managers.

The SEC has the right under the Investment Company Act of 1940 to issue new rules that establish or clarify previously existing requirements of fund managers. Of particular importance to the mutual fund and ETF industry are rules that govern the determination of fair value of assets utilized to calculate the NAVs of funds. In 2020, the SEC announced the adoption of rule 2a-5 of the Investment Company Act of 1940 which governs the "Good Faith Determinations of Fair Value" of assets. When market quotations for assets are "readily available", fund managers are required to utilize those market quotations. When they are not available, funds are required to be valued in good faith by the funds' boards of directors or their designees. The rule also governs the evaluation of valuation practices, oversight of the fair valuation process, and record-keeping of the fair valuation process. In practice, most funds utilize third-party services when pricing assets for which market quotations are not readily available. While no explicit safe harbor provisions exist, it is likely that many funds feel the use of independent third-party pricing services dramatically reduces the risk of the SEC bringing an enforcement action against a fund.

The valuation of portfolio assets is of particular importance for open-end mutual funds and subsets of ETFs that utilize partial creation baskets. Closed-end funds and ETFs that receive all underlying assets during share creations and redemptions are also required to value their assets and thus the NAV of their portfolio, but since investors never directly transact or settle at those prices, the NAV is primarily relevant in the education of and monitoring by end investors. In contrast, open-end mutual funds and some ETFs settle at NAVs. It thus becomes critically important whether entering (exiting) investors or APs pay (receive) the proper amount of cash in exchange for their shares. The use of prices that do not reflect all information used to estimate fair value, often referred to as “stale prices”, can allow entering and exiting investors to expropriate wealth at the expense of other investors (Chalmers et al. (2001)). Such problems were of particular academic and regulatory focus in the late 1990’s and early 2000’s (Zitzewitz (2006). Recent academic work has documented that stale prices still persist in many fixed income open-end mutual funds Choi et al. (2019)). Chapter 2 theoretically describes and empirically documents the impact of stale NAVs on the performance of corporate bond ETFs.

Another particularly relevant SEC rule adoption was rule 6c-11 of the Investment Company Act of 1940. The adoption of the final rule was announced in 2019. Prior to rule 6c-11, the security design of the majority of ETFs violated existing statute of the Investment Company Act of 1940. Therefore, the near absolute majority of ETFs applied for and received exemptive orders from the SEC exempting the funds from portions of the Investment Company Act of 1940 and allowing them to operate. As part of the exemptive orders, ETFs usually obtain the ability to utilize partial and custom creations baskets previously described. ETFs that hold less liquid assets often routinely use partial creation baskets in order to facilitate AP activity and reduce premiums and discounts. Chapter 2 empirically studies the impact of such policies on the performance of corporate bond ETFs. Even if an ETF typically demands all underlying assets during share creation and redemption activity, they may still occasionally utilize custom creation baskets to the benefit of end investors. For example, ETFs will often conduct a “heartbeat trade” at the time of index composition changes to avoid incurring capital gains obligations to end investors (Moussawi et al. (2019)).

With the adoption of rule 6c-11, the SEC sought to reduce regulatory burden and foster ETF market competition by “ establishing a clear and consistent framework for the vast majority of ETFs operating today.” (SEC). In doing so, the allowed all ETFs to engage in share creation and redemption activities with APs of the fund managers’ choosing. Additionally, the rule allowed the use of partial and custom creation baskets for all ETFs without seeking an exemptive order and established new disclosures to be conducted by ETF managers.

The originally proposed rule included mandatory disclosures regarding the composition of the published partial creation basket and the utilization of custom creation baskets. After public commentary of the proposed rule recommending changes to these rules including commentary by the Investment Company Institute and Vanguard that was cited in the SEC exemptive order, the final and adopted rule removed those disclosure requirements and free data on the composition of baskets is not available to end ETF investors.

1.4 Conclusion

The asset management industry has undergone both incredible growth and substantial changes over the past century. Of particular relevance, ETFs are a rapidly growing security structure of already large but still growing importance. In conjunction with this rise, the US SEC has adopted numerous new rules governing ETF managers. Recently, two new rules have been adopted: one governing the valuation of portfolio assets and one governing the share creation and redemption process that ETFs utilize to handle fund flows. Chapter two empirically investigates consequences on ETF performance and the cost of liquidity transformation provided by ETFs security design features that were codified in those rules. Given the growing importance of ETFs and changing regulatory landscape, ETFs remain and active and exciting area of future academic research.

References

(????), "Sec adopts new rule to modernize regulation of exchange-traded funds." URL https://www.sec.gov/news/press-release/2019-190.

Chalmers, John MR, Roger M Edelen, and Gregory B Kadlec (2001), "On the perils of financial intermediaries setting security prices: the mutual fund wild card option." *The Journal of Finance*, 56, 2209–2236.

Choi, Jaewon, Mathias Kronlund, and Ji Yeol Jimmy Oh (2019), "Sitting bucks: Zero returns in fixed income funds." *Available at SSRN 3244862*.

Kostovetsky, Leonard (2003), "Index mutual funds and exchange-traded funds." *The Journal of Portfolio Management*, 29, 80–92.

Moussawi, Rabih, Ke Shen, and Raisa Velthuis (2019), "Etf heartbeats, tax efficiencies, and clienteles." *Working Paper*.

Zitzewitz, Eric (2006), "How widespread was late trading in mutual funds?" *American Economic Review*, 96, 284–289.

2 The Hidden Cost of ETFs

2.1 Introduction

The ETF market is large and growing rapidly. Between 2010 and 2020, ETF assets under management increased from 1.3 to 7.7 trillion dollars. By 2021, the ETFs surpassed passive index-tracking mutual funds in size. This growth was supported by the creation of numerous new funds that hold illiquid assets such as corporate bonds. Despite underlying asset illiquidity, ETFs remain relatively liquid, providing a valuable liquidity transformation service.

Under standard share-creation rules, ETFs utilize in-kind exchange of all underlying assets to allow Authorized Participants (APs) to conduct trades that arbitrage differences in ETF net asset values (NAVs) and secondary-market prices. Underlying asset illiquidity presents a challenge to ETF security design: ETF managers believe that standard creation rules coupled with portfolios that consist of numerous illiquid assets will prevent APs from conducting arbitrage trades that are necessary to improve ETF price efficiency. Corporate bond ETF managers therefore allow APs to deliver only a subset of the assets in portfolios that ETFs wish to track. These rule modifications embed a hidden cost that ETF investors incur in a manner not previously recognized by the literature. Share creations shift ETF holdings in a way that overweights delivered assets and underweights undelivered assets. I investigate whether bonds delivered to ETFs by APs subsequently underperform and to what extent this contributes to fund underperformance.

APs deliver corporate bonds that underperform bonds that are held by the same ETF Which are not delivered during the creation process by 2.4 basis points. This causes ETFs on average to underperform their stated benchmarks following a creation event by 1.4 basis points. Given the frequent occurrence of creations within my sample, this results in a 48 basis-point-per-annum cost to the average corporate bond fund, which is higher than the 35 basis point average net expense ratio reported by the same funds. The valuable liquidity transformation provided by corporate bond ETFs is therefore not a "free lunch." I document a novel trade-off that ETF managers face. By allowing their APs greater flexibility, ETF managers encourage APs to more aggressively arbitrage tracking errors to the benefit of ETF investors while simultaneously allowing APs to interact strategically with ETF portfolios at the expense of ETF investors. My sample consists of funds that manage over 200 billion dollars in assets in 2019 and the mechanism described applies as well to other classes of ETFs that also utilize modified creation rules.

My empirical strategy leverages the fact that bonds delivered by APs to ETFs are accompanied by a natural control population: bonds held by the same ETFs but are not delivered during that specific creation event. Using this natural control group, bond fixed effects, fund-by-date fixed effects, and additional controls, I estimate the performance of delivered bonds relative to that of undelivered bonds in periods around deliveries versus the relative performance of those same bonds during periods that lack deliveries. My empirical method is similar to a differences-in-differences framework. This identification approach illustrates that delivered bonds do not underperform simply because ETFs hold low-risk or high-liquidity subsets of underlying benchmarks and receive lower compensation for their lower risk. Instead, ETF holdings shift in the time series and ETFs increase holdings in bonds when the short-term future return on those bonds is expected to be lower.

To estimate the size of such the hidden cost that is borne at the fund level, I estimate the performance of ETFs relative to that of their stated benchmarks conditional on experiencing a creation event relative to the performance of the same ETF when it does not experience a creation event. Specifically, I calculate spreads between ETF performance and underlying benchmark changes and regress them on a binary variable that indicates whether a creation event occurred. By investigating relative performance spreads with high-dimensional fixed effects, I am able to rule out many alternative explanations. Most importantly, I can reject the null hypothesis that such performance can be explained solely by the arbitrage motive for APs' trades and deliveries. The arbitrage trading that share creations are designed to facilitate implies that differences in long-run spreads should be mean zero. While arbitrage opportunities are still of the first order in APs' trade decisions and in explaining the time-series of spreads, I document a two-month underperformance that cannot be explained by arbitrage opportunities alone and instead represents the manifestation of a hidden cost.

I also investigate the underlying mechanism by which delivered bonds underperform. I consider two main sources of bond performance that embed the cost ETFs incur. First, asset NAVs used during settlement may not accurately reflect underlying market-microstructure dynamics and may allow APs to settle with temporarily over-valued assets. Second, APs may utilize information, such as bond momentum or private information inferred from their own balance sheets in related market-making businesses, to predict future changes in asset NAVs and offload securities that will be more costly to hold on their balance sheets. While both mechanisms make similar predictions of post-delivery underperformance by delivered bonds, they make contrasting predictions regarding pre-delivery performance. Bonds underperform prior to delivery, consistent with the utilization by APs of public and private information that is not reflected in asset NAVs.

I perform a number of robustness exercises that confirm the finding of a hidden cost. First, instead of relying on the idea that APs are incentivized to arbitrage cumulative spreads to mean zero over long horizons, I control for the main determinants of APs' arbitrage opportunities: premiums and discounts. In so doing, I isolate the spread dynamics that are attributable to the hidden cost and find them to be significant. Second, I consider spreads between the performance of ETFs' portfolios and that of underlying indexes. Since the hidden cost results from the fact that corporate bond ETFs hold portfolios that differ from target benchmarks, underperformance should be observable in NAVs, not only in the secondary-market prices of ETFs. Third, I show that ETF characteristics such as underlying liquidity and the size of a creation basket predict the size of the hidden cost, a finding that is consistent with the mechanism I posit. Last, I utilize ETFs that hold US equity in companies with large market capitalization (Large-Cap ETFs) that require all assets to be delivered during creation events as a sample for falsification tests. I find no empirical evidence of fund underperformance in this class of ETFs during creation events, a finding that is consistent with the idea that the hypothesized hidden cost drives corporate bond ETF underperformance.

My paper contributes to three major strands of literature. First, I contribute to the literature that investigates the costs of liquidity transformation in the investment management industry. In open-end mutual funds, entering-and-exiting investors demand for liquidity results in transaction costs that are borne by all investors (Edelen (1999)). These costs rise in the presence of stale NAVs. Investors can time their entry and exit decisions strategically at the expense of other investors (Chalmers et al. (2001)). The accuracy of NAVs attracted particular attention in the late 1990s and early 2000s, but has received renewed scrutiny with regard to fixed-income mutual funds (Zitzewitz (2006) and Choi et al. (2019)). It is commonly believed that the ETF mechanism design protects ETF buy-and-hold investors from the liquidity demands of other investors.[5] I document that many ETFs consistently pay a large cost for liquidity transformation in a manner that has not been recognized previously in the literature. This costs will not be evident in tracking errors or premiums because such metrics describe the relative performance of ETF NAVs and secondary-market prices, while the cost instead arises as a result of wedges between ETF and benchmark portfolios. Additionally, because ETF creations are settled using NAVs, this cost is theoretically larger in the presence of inaccurate NAV pricing.

Prior literature has also considered implicit costs associated with the liquidity transformation that ETFs provide. In doing so, academics and policymakers have overwhelmingly focused on the potential for financial fragility in ETFs. Inspired by canonical models such as that proposed in Diamond and Dybvig (1983) and theoretical understanding of fragility in open-end mutual funds (e.g. Chen et al. (2010a)), many have considered whether ETF

[5]For example, etf.com states that the "system is inherently more fair than the way mutual funds operate. In mutual funds, existing shareholders pay the price when new investors put money to work in a fund, because the fund bears the trading expense. In ETFs, those costs are borne by the AP (and later by the individual investor looking to enter or exit the fund)." (ETF.com (2021)).

portfolios may be exposed to runs (see Pan and Zeng (2019); Dannhauser and Hoseinzade (2021); Haddad et al. (2020); and Ma et al. (2020)). Runs may result in persistent periods of mispricing, impacts on underlying asset markets, or spillovers to related asset markets. In its focus on financial fragility, the literature has overlooked another large and important cost: rule modifications necessary in face of underlying asset illiquidity result in performance drags on ETFs borne in good times. While they are theoretically plausible, the realization of permanent costs that are borne by buy-and-hold ETF investors due to financial fragility have been difficult to document empirically. The cost introduced in this paper has already been incurred and is shown empirically to be high.

Second, since Jensen (1968), a strand of literature has assessed the relationship between flows in the investment-management industry and the subsequent performance of investment funds. Although such tests were originally interpreted as pertaining to the assessment of both the skills of investment managers and the rationality of investors in delegating capital to those managers, drawing inferences from such tests is difficult, for two reasons. First, if active management experiences decreasing returns to scale, in equilibrium flows will not predict future performance despite being motivated by managers possessing skills (Berk and Green (2004)). Second, open-end mutual funds must engage in transactions when they experience flows that incur transaction costs (Edelen (1999)). Despite such challenges, a number of papers have investigated the relationship between flows and future performance, often after accounting for transaction costs (for example, see Zheng (1999); Daniel et al. (1997); Wermers (2000); Edelen and Warner (2001); Frazzini and Lamont (2008); and Friesen and Sapp (2007)). I introduce a mechanism that relates ETF flows to short-run future performance in a manner that is consistent with Edelen (1999).[6] My paper introduces the need for work that assesses the market-timing ability of ETF investors to account for indirect costs of flows (akin to the mutual fund literature's accounting for transaction costs).

[6]Edelen (1999) finds a relationship between flows and contemporaneous returns at a frequency of six months. I show that daily flows are associated with the following month's daily returns. The difference between focusing on contemporaneous and focusing on future returns reflects the different frequency that I utilize for estimation.

In related work, researchers have advocated utilizing investment-management flows as proxies for non-rational shifts in investor demand curves. Numerous studies have shown that investment-management flows are predictive of underlying asset returns (See for example Sirri and Tufano (1998); Coval and Stafford (2007); Frazzini and Lamont (2008); Ben-Rephael et al. (2012); Ben-David et al. (2018); Dannhauser (2017); and Doan (2020)). Specifically, ETF flows have been shown to negatively predict future fund and underlying asset returns (Brown et al. (2019)). Prior literature has failed to identify rational-agent-based explanations and instead has posited that flows act as a proxy for investor sentiment when explaining such return predictability.[7] I introduce for the first time a fully rational mechanism that relates ETF flows to short-term future returns. I also present new evidence pertaining to the relationship between ETF flows and future performance, as I measure performance as spreads between fund and benchmark returns rather than raw returns. Demand shocks suggested in the relevant prior literature would impact both ETF and underlying asset markets and thus would not be observable in spreads. The mechanisms I introduce also suggest an alternative explanation for a portion of the short-term return predictability that has been documented previously and helps to reconcile the return predictability documented in the sample utilized in Brown et al. (2019) and the lack of return predictability in the sample utilized by Dannhauser and Pontiff (2019): The former sample includes leveraged ETFs and exchange-traded notes that are more likely to use partial creation baskets or cash settlement. In summary, I present a novel mechanism that helps to rationalize previous puzzles regarding the information content of ETF flows.

Third, my paper contributes to a very new and growing strand of literature that considers the incentives imposed by rules governing ETF creations. APs may behave strategically to utilize the implicit liquidity provided by the share-creation process to offload inventory

[7] Studies also investigate the relationship between past returns and future flows, (see e.g. Ippolito (1992) and Berk and Green (2004)). Clifford et al. (2014) and Dannhauser and Pontiff (2019) present evidence that flow-to-performance is stronger in ETFs than in active investment products, which is hard for many rational models to reconcile. My paper does not shed light on this empirical relationship to past returns and instead investigates the relationship between flows and contemporaneous or future returns.

in periods of market turmoil after experiencing balance-sheet shocks. Such behavior could expose ETF portfolios to financial fragility (Pan and Zeng (2019)). Conversely, when markets are stressed, ETF managers may allow NAVs to become stale for strategic purposes, as stale NAVs make redemption trades by APs more costly, allowing ETF managers to retain assets under management and protect ETFs from financial fragility (Shim and Todorov (2021)). While AP and ETF-manager incentives carry implications for financial fragility, I demonstrate that they also embed a consistent cost that is borne outside periods of market stress. This cost is motivated by the same strategic considerations that occupy APs as those discussed in Pan and Zeng (2019) but result in new and novel effects. The complimentary findings mirror those reported in the mutual fund literature. Pan and Zeng (2019) illustrate that ETFs face the same financial fragility theorized in Chen et al. (2010b) while I illustrate that ETF investors can pay consistent rents in a manner similar to those described by Chalmers et al. (2001). While stale NAVs may benefit ETF managers when markets are stressed (Shim and Todorov (2021)), they impose a large and offsetting cost during normal times. My paper suggests that, even if financial fragility is not empirically likely, APs' incentives to interact strategically with ETF portfolios at the expense ETF investors must be considered when evaluating the liquidity transformation that ETFs provide.

In addition to these contributions to the academic literature, my paper also bears direct implications for policymakers and ETF managers. First, as discussed previously, my paper is the first to establish that ETF performance is affected by the accuracy of asset NAVs. Recently, mutual funds have received renewed scrutiny over the practices they use to set NAVs. Many have argued that ETF-creation mechanisms render them immune to similar issues. My paper illustrates the flaw in such reasoning when applied to large subsets of ETFs and suggests that such ETFs should not be exempt from NAV-pricing regulations. Second, in 2019 the U.S. Securities and Exchange Commission (SEC) approved Rule 6c-11, which allows all ETFs to utilize custom creation baskets. When enacting such a rule, the SEC acknowledged the theoretical risk that custom creation baskets pose to ETF investors, but concluded that they are unlikely to occur in practice.[8] I present evidence that they do occur and the cost borne by investors is high.

Lastly, my paper helps resolve open questions regarding the organization of the investment management industry. ETFs provide many benefits, including empirically lower fees (Kostovetsky (2003)), lower tax obligations (Moussawi et al. (2019)), and the liquidity transformation on which this paper focuses. Researchers have identified very few costs associated with ETFs relative to those associated with mutual funds that would offset such benefits. While ETFs have grown massively, open-end mutual funds still represent the majority of funds in the investment-management industry. By documenting a novel and large cost, I help explain why ETFs do not represent the dominant security design, particularly when investors wish to hold illiquid assets. My findings help extend the framework of Chordia (1996) to the ETF industry by showing that underlying asset liquidity is a first-order determinant of optimal security design.

2.2 The Share-Creation Process

To provide a number of advantages, such as intraday liquidity, ETFs are designed to be traded on secondary markets. Insofar as ETFs will have secondary-market prices that do not necessarily equal the value of their underlying assets and because the vast majority of investors will purchase ETFs via this secondary market, ETFs require a mechanism that enables them to remove large discounts or premiums that differentiate net asset values from ETF share prices.[9] To facilitate the elimination of discounts and premiums, ETFs enter agreements with APs, who are allowed to create and redeem ETF shares by exchanging the assets that underlie an ETF share for a share of the ETF or vice versa. This process

[8] In rule 6c-11, the SEC defines "custom creation baskets" as distinct from "sampling baskets," the use of which was already permitted. The cost documented in this paper can result from both types of creation baskets: APs can interact strategically with any creation basket that does not exactly match the benchmark portfolio. Obviously, when APs are allowed to request custom creation baskets, the level of adverse selection and thus the cost is likely higher.

[9] This mechanism is the main security design innovation that distinguishes ETFs from close-end mutual funds.

is referred to as "in-kind exchange." Consider a highly stylized and simplified example: suppose investors wish to hold an ETF that tracks the S&P 500 and purchase a sufficiently large quantity of the SPY ETF to positively impact SPY's secondary market price. This creates a premium in which the ETF trades at a higher price than the underlying assets. APs can seek to arbitrage this premium away by buying the underlying assets and selling short the share of the ETF. An AP notifies an ETF manager that it wishes to create a share as outlined in their agreement. At the end of the trading day, the AP delivers all of the underlying assets, receives a newly created ETF share and utilizes this share to cover the short-sale position, thereby locking in the arbitrage profit. Because all relevant shares of the S&P 500 companies still underlie the SPY ETF, investors who hold the ETF throughout this entire process face no costs. Specifically, the ETF experiences a temporary tracking error induced by the premium but when the premium is arbitraged away the tracking error reverses and the buy-and-hold ETF investor receives a return that exactly tracks the performance of the S&P 500 gross of the ETF net expense ratio. Thus, when all underlying assets are exchanged in kind, ETF investors are not impacted by other investors' entry.

When ETFs hold fewer liquid assets or a very large number of assets, however, it is often difficult or overly cumbersome to exchange all underlying assets in kind when seeking to create shares. With corporate-bond ETFs in particular, a very small notional amount of a given bond may underlie each share, making it logistically unwieldy to locate and deliver such a small amount of a given bond. To overcome these barriers, facilitate creation activity, and thus remove premiums, ETF managers instead often allow creation to occur with a select subset of assets that they believe is representative of the full portfolio of underlying assets. For example, if a corporate bond ETF holds 1,000 separate bonds, they might at the start of the trading day identify 100 of those bonds as the "sampling basket,", also referred to as the "creation basket," which is utilized for share creation. In such a case, APs can deliver only these 100 bonds at the end of the day while delivering "cash-in-lieu" for the remaining underlying assets in exchange for a share of the bond ETF. Depending on the AP agreement established by an ETF manager, an AP can actually deliver cash in conjunction with the sampling basket. Much more commonly, APs can instead deliver the sampling basket alone and receive ETF shares on a pro-rata basis based on the NAV of the sampling basket versus the NAV of the ETF share.[10]

At the start of a trading day, ETF managers will disseminate to their APs which assets belong to that day's baskets via a clearing house such as the Depository Trust & Clearing Corporation. ETF managers have incentives to try to match the baskets to the overall portfolios that they wish to hold along key dimensions such as duration and credit risk. Specifically, to the extent that ETF managers believe there is a factor structure that characterizes bond returns, ETF managers have incentives for creation baskets to contain the same total factor exposure as their target portfolios. They also may consider asset liquidity and notional sizes to encourage more aggressive arbitrage activity, thus reducing premiums and discounts. Additionally, APs form relationships with ETF managers and may call them to request "custom creation baskets." They may request the omission of certain assets from baskets that they cannot readily locate or suggest alternative assets. ETF managers have the right to accept these custom creation baskets at their discretion and often do so in practice. Relatedly, ETF managers sometimes utilize custom creation baskets to facilitate "heartbeat trades," as documented in Moussawi et al. (2019). While ETF managers have incentives to ensure that creation baskets are fairly priced and match their target portfolios along key dimensions, many of them have longstanding relationships with APs and rely on APs to arbitrage away premiums and discounts that may deter future fund flows. Thus, it may often not be in ETF managers' interests to reject creation activity even if doing so embeds a cost that their customers bear.

When pro-rata settlement occurs, which is the norm for corporate bond ETFs, creations shift ETF portfolio weights at the point of creation. The portfolio weights of delivered assets naturally increase as they now hold more of these bonds. Portfolio weights as a

[10]For example, if the sampling basket of 100 shares represented 10% of the NAV of the ETF share, they could deliver 10 shares worth of the sampling basket in exchange for one ETF share.

percentage of an ETF's portfolio decrease for assets that are not delivered because the holdings of those assets are unchanged but the total assets under management of the fund increase. These shifts also imply that ETFs often hold portfolios that differ from the indices that they track. Naturally, one would expect ETF managers and APs to select larger and more liquid bonds for inclusion in the delivery basket as they are likely cheaper to secure. Thus, it is possible that many funds are systematically overweighted in liquid assets and thus ETFs fail to capture a liquidity premium but in turn face lower risk. While this may also be occurring, I produce the results of this paper while controlling for such persistent compositions of ETF portfolios and instead investigate whether the timing of portfolio shifts imposes a cost on ETF investors. Bond-level estimates of performance control for time-varying bond liquidity and bond fixed effects and thus reveal whether bonds delivered to ETFs perform worse around delivery than when they are not delivered. Relatedly, fund-level results include fund fixed effects that would capture time-invariant portfolio differences between ETFs and their benchmark indices. Thus, the hidden cost described does not simply reflect differences in the risk/reward profiles of ETFs' actual portfolios as opposed to those of their benchmark portfolios. Instead, it reflects the nature of creations in which ETFs dynamically receive assets with lower expected future returns in such a way that creation events negatively impact the performance of ETFs relative to that of their benchmarks. Insofar as pro-rata settlements are based on the NAVs of underlying assets, it is important that ETFs set timely and correct NAVs for underlying assets, as would be the case with an open-end mutual fund. If these NAVs are not timely and accurate, APs can strategically time creations and redemptions in the same way as open-end mutual fund investors can strategically time their investments to profit from stale NAVs. Indeed, even if NAVs that are used to price ETF assets are timely and accurate, APs may utilize information (public or private) that is not reflected in current prices when deciding which assets to deliver or when to deliver pre-specified assets. They may wish to offload inventory that will be costly to hold (based on lower expected future returns) or liquidate bonds in exchange for ETF shares that they can sell more easily. In so doing, ETFs will be left with adversely selected sets of assets, specifically assets that conditionally possess lower expected returns than the unconditional average expected returns on those assets. This will result in ETF underperformance relative to that of the underlying indexes that they state as their benchmarks.

When shares are created pro-rata, any market force that causes delivered assets to underperform non-delivered assets held by the same ETF will result in a performance drag on an ETF. Both incorrect NAVs and the utilization of private information by APs will result in equivalent performance drags because both make the same predictions regarding post-delivery asset performance. Nonetheless, the two mechanisms may imply distinct remedies and hypothesize divergent asset performance prior to delivery. NAVs are often incorrect when assets that APs purchase to deliver temporarily impact prices in a way that is reflected in those NAVs. Such temporary price impacts would on average revert, resulting in bond underperformance. Thus, NAVs that fail to reflect market micro-structure forces predict that delivered bonds will outperform other bonds prior to delivery (with that outperformance attributable to the temporary price impact). If instead APs utilize information that is not reflected in NAVs, there is no clear hypothesis to explain the performance of delivered assets. If APs utilize information indicating price reversions (not caused by their own trading), the predicted dynamics are similar to those that occur when NAVs incorrectly reflect temporary price impacts. Instead, if APs utilize information embedded in past prices and NAVs, such as bond momentum as in Jostova et al. (2013), bonds will underperform both prior to and following delivery. Lastly, if the information utilized by APs is not reflected in prices, i.e. it is private information acquired from their own balance sheets in related market-making businesses, delivered bonds may perform no differently prior to creation but underperform following creation. The results of my analysis reveal underperformance prior to delivery that is consistent with the utilization of information by APs, and is thus consistent with APs' taking advantage of bond-momentum information as well as private information.

ETF managers often recognize that the NAVs utilized in these exchange processes fail to fully reflect the transaction costs that must be paid to re-balance an ETF portfolio, and thus charge a "fee on cash" for an actual cash settlement. This is particularly necessary because bond NAVs are often priced using market bid prices rather than ask prices. Relatedly, perhaps because ETF managers know that creation events may impose costs on ETF investors, ETF managers also charge flat creation or redemption fees to APs. Lastly, ETF managers have the right contractually to refuse creation or redemption if they deem it unfavorable to their funds, although this right is rarely exercised. It remains an empirical question whether fees are sufficiently large to offset or erase the hidden cost of share creation. My results suggest instead that fees on cash are not sufficiently high to fully offset this cost and thus ETF investors are affected by fund flows. ETF managers also face a difficult tradeoff between protecting investors from this hidden cost and encouraging APs to arbitrage discounts and premiums away. While it is possible that this hidden cost is understood by ETF investors who view it is as fair compensation for reduced tracking error, for many corporate bond ETF investors this cost may not be particularly salient as it does not show up in many key fund statistics such as tracking errors and expense ratios. Thus ETF managers who hope to attract fund flows may find it useful to pay a high but non-salient cost in exchange for the small but salient benefit of reduced tracking errors. The rationales that ETF investors follow are not observable, so it is impossible to distinguish between these two theories, but comparing this hidden cost with the explicit costs of administering a fund in the context of an established strand of literature on the behavioral biases of mutual fund investors suggests that some ETF investors may not be making informed decisions regarding these costs.

Unlike share creations, redemptions are much less likely to involve pro-rata settlements and thus do not embed the same cost as creations. It is much easier logistically for ETF managers to provide APs with portfolios that consist of all the assets that underlie an ETF rather just a subset as they do not have to enter the market and execute transactions to secure those assets. Additionally, APs may not need to sell those assets immediately, instead adding them to their balance sheets that they hold in their related market-making businesses. Thus, in practice, redemptions exert a far smaller distortionary effect on the portfolios of ETFs than creations and instead tend to deviate when doing so is optimal for ETF investors (such as when they wish to cycle out a bond that is coming to maturity to avoid transactions). As a result of this asymmetry between creations and redemptions, the tests conducted for this paper focus on creations and not simply flows of either kind.

2.3 Sample and Data

I construct a two samples with which to test for the existence of the hidden cost I have posited. I use the first sample to estimate the performance of bonds around delivery during creation events. I begin with the universe of holdings data of funds with "Fixed Income" as their asset class and "Corporate Bonds" as their category from ETF Global. ETF Global captures data reported by APs and ETF managers on creation activities and fund holdings. I exclude leveraged or inverse ETFs, as their buy-and-hold return characteristics are affected by other factors such as costs associated with leverage and realized benchmark volatility. Because I lack data that enable me to observe the stated creation baskets of ETFs or custom creation baskets, I infer what was delivered by measuring changes in ETF holdings. Thus, I assume that ETFs do not trade themselves and instead rely on creation and redemption events to manage their portfolios.[11] Such an assumption is in line with the motives ETFS possess to defer taxes (in-kind exchanges that occur during creations are not taxable events, whereas trades made by ETFs do create tax obligations for investors). Thus, in my sample of bond, I rely solely on bonds with non-missing notional values to observe changes in holdings that are untainted by any measurement error in bond returns.

I augment these data with bond returns calculated using trades reported in TRACE and bond characteristics reported by FISD. I calculate bond returns as changes in dirty bond prices paid by customers who purchase bonds plus changes in accrued interest implied by bond coupon characteristics. Unlike in many other studies, in this study I calculate

[11] Shim and Todorov (2021) rely on the same methodology to identify the composition of creation baskets.

bond returns using prices that are implied by the most recent transactions inferred to hit bids rather than measuring the implied mid-point between bids and asks. Bond NAVs are priced at dirty bid prices (with accrued interest accounted for during settlement); this return measure will therefore more accurately reflect the NAVs used during settlements of ETF share creations. If a bond increases its notional value while a fund increases in size, I classify the bond as "Delivered." I also calculate time-varying measures of bond liquidity utilizing TRACE data. I draw my liquidity measures from those utilized by Dannhauser (2017) in her construction of liquidity principle components. I calculate Ahimud price impacts in a manner that is consistent with Amihud (2002), implied round-trip costs as described by Feldhütter (2012), and bid-ask spreads consistently with Hong and Warga (2000) and Chakravarty and Sarkar (2003). All liquidity values use the median measure from the calendar month prior to a creation event.

As reported in Table 1, this creates a sample of 9,546,138 bond-by-fund-by-day observations across 9,819 bonds contained in 45,088 fund-by-day cross-sections over 1,258 trading days. Importantly, the same bond-day return may enter my sample multiple times if the bond is held by multiple funds but the fund characteristics assigned to that bond (such as delivered flags or fund fixed effects) will differ across these multiple observations.

Second, I create a sample of ETF performance measures, creation events, and ETF characteristics to investigate the hidden cost at the fund level. I begin with the universe of ETF Global, which reports daily creation and redemption activity via reporting the number of ETF shares outstanding. $Creation_{f,t}$ reports a simply binary indicating whether a fund experienced a net creation event on a given trading date (its shares outstanding increased). While it is possible that one AP creates shares while another redeems, this is unobservable and, theoretically, should be rare. While it is likely that on any given day factors other than the premium/discount (such as AP information sets or current balance sheets) would motivate one AP to create while another did not, it less likely that these factors would be so large as to make creation profitable for one AP while making redemption profitable for another. Unlike other data sources that are utilized in the literature to infer ETF creation and redemption activity, such as CRSP market data or CRSP mutual fund data, ETF Global captures data reported by APs and ETF managers on days of or following creation activity (depending on whether the particular fund reports on T or T+1) rather than a later share-settlement date. I again exclude leveraged or inverse ETFs. I utilize ETF Global to identify each fund's stated benchmark and restrict my sample to those with benchmarks, thus excluding the rare cases of actively managed ETFs. I obtain benchmark levels from Bloomberg and ETF return and dividend information from CRSP. As I report in Table 1, this yields a sample of 137 corporate bond ETFs over 1,887 trading days.

Table 1: Summary of Samples

Panel A of this table provides descriptive statistics for the sample of bonds analyzed in this study. $BondRet_{b,t}$ is the daily bond return implied by customer-to-dealer purchases reported in TRACE data. Bond returns cumulated over one month and two months are reported in $\prod_{n=-22}^{-1} BondRet_{b,f,t+n}$ and $\prod_{n=-22}^{20} BondRet_{b,f,t+n}$ respectively. $Delivered_{b,f,t}$ is a binary variable that takes the value of one if the notional value of a bond increases on a share creation day and zero otherwise. $Amihud_{b,t}$ is calculated in a manner that is consistent with Amihud (2002), $ImpliedRoundtripCost_{b,t}$ is calculated in a manner that is consistent with Feldhütter (2012), and $BidAsk_{b,t}$ is calculated in a manner that is consistent with Hong and Warga (2000) and Chakravarty and Sarkar (2003). All three liquidity values reflect the median value in the prior calendar month. Results for the same bond may be reported multiple times if the bond is held by multiple ETFs and fund-level variables such as $Delivered_{b,f,t}$ may vary across these observations. Panel B of this table provides descriptive statistics for the sample of analyzed ETFs. $Ret_{f,t}$ is the daily secondary-market return on an ETF. $NAVRet_{f,t}$ is the implied return on the underlying assets calculated as $NAVRet_{f,t} = \frac{NAV_{f,t}+Dividend_{f,t}}{NAV_{f,t-1}}$. $\Delta Index_{f,t}$ is the percentage change in the benchmark index. $RetSpread_{f,t,k}$ is the cumulative difference between an ETF return and the benchmark index cumulated through k days following the observation date: $RetSpread_{f,t,k} = \prod_{n=-22}^{k} ETFRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $RetSpread_{f,t,-1}$ is the cumulative difference between the ETF and the index up to 1 day prior to observation (or share creation in the case of a creation event). $NAVSpread_{f,t,k}$ is the cumulative difference between the $NAVRet_{f,t}$ and the benchmark index cumulated through k days following the observation date. $Creation_{f,t}$ is a binary variable that takes the value of one if a fund's shares outstanding increased and zero otherwise. $Premium_{f,t}$ is the premium or discount as represented in percentage terms (%100 representing no discount or premium). All returns or spreads in both panels are reported in basis points.

Panel A: Underlying Bond Sample

			Percentile				
Variable	Mean	Std. Dev.	1st	25th	50th	75th	99th
$BondRet_{b,t}$	0.56	45.13	-179.80	-4.93	1.10	6.87	162.88
$\prod_{n=-22}^{-1} BondRet_{b,f,t+n}$	7.50	171.16	-580.24	-54.55	12.64	78.73	542.74
$\prod_{n=-22}^{20} BondRet_{b,f,t+n}$	18.56	246.31	-796.21	-88.26	20.68	129.41	769.37
$Delivered_{b,f,t}$	3.91%						
$Amihud_{b,t}$	0.04bp	0.05bp	0.00bp	0.00bp	0.02bp	0.04bp	0.27bp
$ImpliedRoundtripCost_{b,t}$	1.39bp	1.71bp	0.00bp	0.48bp	0.86bp	1.54bp	9.34bp
$BidAsk_{b,t}$	\$0.24	\$0.23	\$0.00	\$0.10	\$0.18	\$0.29	\$1.18
0.00	0.10	0.18	0.29	1.18			
Obs: 9,546,138; Trading Days: 1,258; ETF/Date Cross-sections: 45,088							
Bonds: 9,819; ETF Bond Pairs: 84,781							

Panel B: Fund Sample

			Percentile				
Variable	Mean	Std. Dev.	1st	25th	50th	75th	99th
$Ret_{f,t}$	1.5	29.2	-96.3	-10.7	1.0	14.7	94.7
$NAVRet_{f,t}$	1.5	23.0	-79.7	-6.4	1.1	10.4	77.1
$\Delta Index_{f,t}$	1.7	22.1	-76.7	-6.0	1.4	10.3	74.8
$Ret_{f,t} - \Delta Index_{f,t}$	-0.2	21.8	-72.7	-9.5	-0.2	9.1	73.6
$NAVRet_{f,t} - \Delta Index_{f,t}$	-0.1	16.1	-68.9	-1.9	-0.1	1.6	68.8
$RetSpread_{f,t,-1}$	-3.4	35.4	-126.2	-18.5	-2.8	11.6	117.3
$NAVSpread_{f,t,-1}$	-3.5	25.1	-112.7	-7.3	-1.9	2.0	89.9
$RetSpread_{f,t,20}$	-6.7	42.0	-157.4	-23.9	-5.5	10.8	136.2
$NAVSpread_{f,t,20}$	-6.8	31.6	-148.3	-12.4	-3.7	1.2	105.3
$Creation_{f,t}$	13.53%						
$Premium_{f,t}$	100.19%	0.42%	98.71%	99.99%	100.18%	100.37%	101.97%
$InvestmentGrade_f$	66.24%						
$AverageBasketSize_f$	64.9	91.7	1.0	16.9	40.3	75.6	600.9
$AverageAmihud_f$	0.04bp	0.02bp	0.00bp	0.03bp	0.04bp	0.04bp	0.09bp
$AverageIRC_f$	1.50bp	0.65bp	0.27bp	1.03bp	1.43bp	1.81bp	3.46bp
$AverageBidAsk_f$	\$0.30	\$0.15	\$0.05	\$0.21	\$0.26	\$0.36	\$0.80
Obs: 134,523; Trading Days: 1,887; Number of ETFs: 137							

2.3.1 ETF Relative Performance

To estimate the underperformance of ETFs that is attributable to the hidden cost I describe, I calculate the following ETF spread metric:

$$RetSpread_{f,t,k} = \prod_{n=-22}^{k} ETFRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$$

This measure represents the net-of-fees buy-and-hold performance of an ETF share relative to its stated benchmark over a horizon of k days. This also represents the performance an ETF investor would actually achieve relative to the stated benchmark if that investor were to purchase and sell ETFs on the secondary market. This measurement is similar to tracking error measures used in the mutual fund or ETF literature but departs from them in a very significant way: it is signed. If an ETF experiences many mean zero daily tracking errors, this relative performance measure would be zero as the tracking errors would offset each other. Negative values of this measure represent negative drifts in tracking errors that harm investors by reducing returns rather than by increasing volatility. Because this is net-of-fee performance, one obvious driver of such underperformance relative to these untradeable benchmarks is the net expense ratio, including management fees, incurred by ETFs.

I calculate another spread in performance, the spread between NAVs and the underlying index, as follows:

$$\begin{aligned} NAVRet_{f,t} &= \frac{NAV_{f,t} + Dividend_{f,t}}{NAV_{f,t-1}} \\ &\approx \Delta NAV_{f,t} + DividendYeild_{f,t} \\ NAVSpread_{f,t,k} &= \prod_{n=-22}^{k} NAVRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n} \end{aligned}$$

This represents the net-of-fees buy-and-hold performance of the portfolio of assets that underlies an ETF share relative to that of its stated benchmark. Unlike the previous spread measure, this measure cannot be realized by a common investor (who is not an AP) who must purchase the ETF on the secondary market. Importantly, because creation and redemption activity seemingly seeks to arbitrage differences between ETF prices and NAVs, creation and redemption activity should have a very different effect on an ETF's relative performance and relative NAV performance. For example, creation activity should reduce ETF prices via the sale of ETF shares and thus lower ETF relative performance, but it should not affect differences between fund NAVs and the underlying index. If creations do contain information indicating differences between NAVs and underlying indexes, it would likely indicate that NAVs are too low and thus would outperform, not underperform, the underlying index. I report summary statistics for both performance measures in Table 1.

2.4 Main Findings

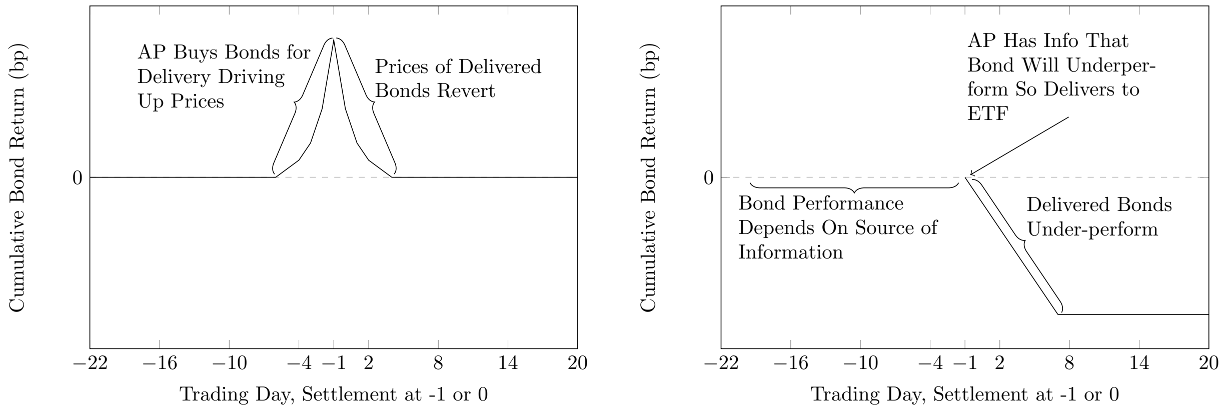
2.5 Corporate Bond Underperformance around Delivery

In this section I ask how corporate bonds perform before and after they are delivered by APs to an ETF during a creation event. As discussed in the above discussion of creation, any performance following t=-1 (because of T+1 reporting, some but not all creations are reported with a one-day lag), will embed a hidden cost that ETF investors incur. To determine whether this occurs, I estimate regressions of bond returns cumulated from 22 days prior to a creation event to various horizons on a binary variable that indicates whether a bond was delivered, time-varying bond liquidity controls, bond fixed effects, and fund-by-date fixed effects.

$$\prod_{n=-22}^{k} BondRet_{b,t+n} = \beta_k^D Delivered_{b,f,t} + \beta^L Liquidity_{b,t} + \gamma_{f,t} + \alpha_b + \varepsilon_{b,f,t}$$

By including fund-by-date fixed effects, I take advantage of the fact that delivered bonds have a natural control: bonds that are held by the same ETF but not delivered during that specific creation event. ETF managers have incentives to include more liquid and larger bonds in the creation basket. While this would lower ETF returns, ETFs would also bear lower risk in such a case. Bond fixed effects (which account for time-invariant bond sizes) and time-varying liquidity controls help to rule out this mechanism. Instead, by estimating this performance I can measure differences in performance between delivered bonds during a given creation event and undelivered bonds during a creation event and compare these performances with the relative performances of those same bonds at other times that do not surround creation days, much like what occurs with a differences in differences framework. Additionally, bond fixed effects will rule out alternative explanations in which delivered bonds are less risky in a time-invariant way. As discussed above in relation to creations, there are two main reasons why bonds may underperform following delivery: 1) bond prices (and/or NAVs) are temporarily high as a result of the temporary impact on prices of APs' securing of bonds to deliver or 2) The utilization of information about future changes in NAVs by APs when deciding which bonds to deliver to the creation basket and when to deliver them. If prices are temporarily too high in a manner that is consistent with APs' temporarily impacting prices before settlement, one would expect to see a dynamic of the sort illustrated in Figure 1a. If the main mechanism is APs' utilization of information, bond behavior prior to delivery depends on the sources of that information. If APs utilize bond-price reversals, the dynamics should be similar to what can be seen in Figure 1a. If APs take advantage of bond momentum the bonds should underperform before delivery. If APs utilize private information it likely would not be observable in prices before delivery. A combination and thus the weighted average of such effects should be observable, as depicted in Figure 1b.

I estimate the effects of asset delivery on bond returns over horizons ranging from 1 to 42 trading days. Figure 2 displays the dynamics of the estimates of β_k^D across various k. The figure also shows 95 % confidence intervals using standard errors that account for two-way clusters by bond and by date. Full parameter estimates for select horizons can be found in Appendix Table A1. As can be seen, bonds that will be delivered begin underperforming in the month prior to delivery and the cumulative return is statistically significant by t=-



(a) Bond Dynamics if APs Temporarily Impact Bonds' Prices around Delivery

(b) Bond Dynamics if APs Utilize Information About Future Returns

Figure 1: Conjectured Bond-Return Dynamics
Hypothesized dynamics of β_k^D estimated using

$$\prod_{n=-22}^{k} BondRet_{b,t+n} = \beta_k^D Delivered_{b,f,t} + \beta^L Liquidity_{b,t} + \gamma_{f,t} + \alpha_b + \varepsilon_{b,f,t}$$

for various k are shown. $Delivered_{b,f,t}$ is a binary variable that takes the value of one if a bond increases in notional amount while a fund experiences a creation. $Liquidity_{b,t}$ includes the prior month median Ahimud measure, implied round-trip costs, and bid-ask spreads. $\gamma_{f,t} + \alpha_b$ are fund-by-date and bond fixed effects. Subfigure (a) displays hypothesized bond-return dynamics if bond NAVs are temporarily impacted by AP trading activity. Subfigure (b) displays hypothesized bond return dynamics if APs utilize information when deciding when to create shares and which bonds to deliver.

12. By t=-1, the earliest date at which creation could occur,[12] bonds that will be delivered have underperformed by 2.6 basis points. Thus, the pre-delivery performance is consistent with APs' utilizing information when deciding which bonds to deliver and when. On net, it appears that APs rely on some combination of bond momentum and private information. Following t-1, bonds underperform by an additional 2.4 basis points in the following month. Thus, following receipt by ETFs, precisely when the ETF portfolio weights of these bonds increase, the bonds underperform going forward. This underperformance is evidence that ETFs receive an adversely selected set of bonds from APs that will hurt ETF performance relative to their underlying benchmarks. Importantly, this is identified with time-series

[12]Creation occurs at either t=-1 or t=0, depending on whether the fund utilizes T or T+1 reporting; it is unobservable for a given fund which reporting standard managers utilized.

shifts in portfolio weights, not persistent differences in ETFs' portfolios and benchmark indices' portfolios, and thus represents a pure performance drag.[13] Thus, put simply, when ETFs experience creation events they receive inferior assets (assets with low expected future returns) from APs.

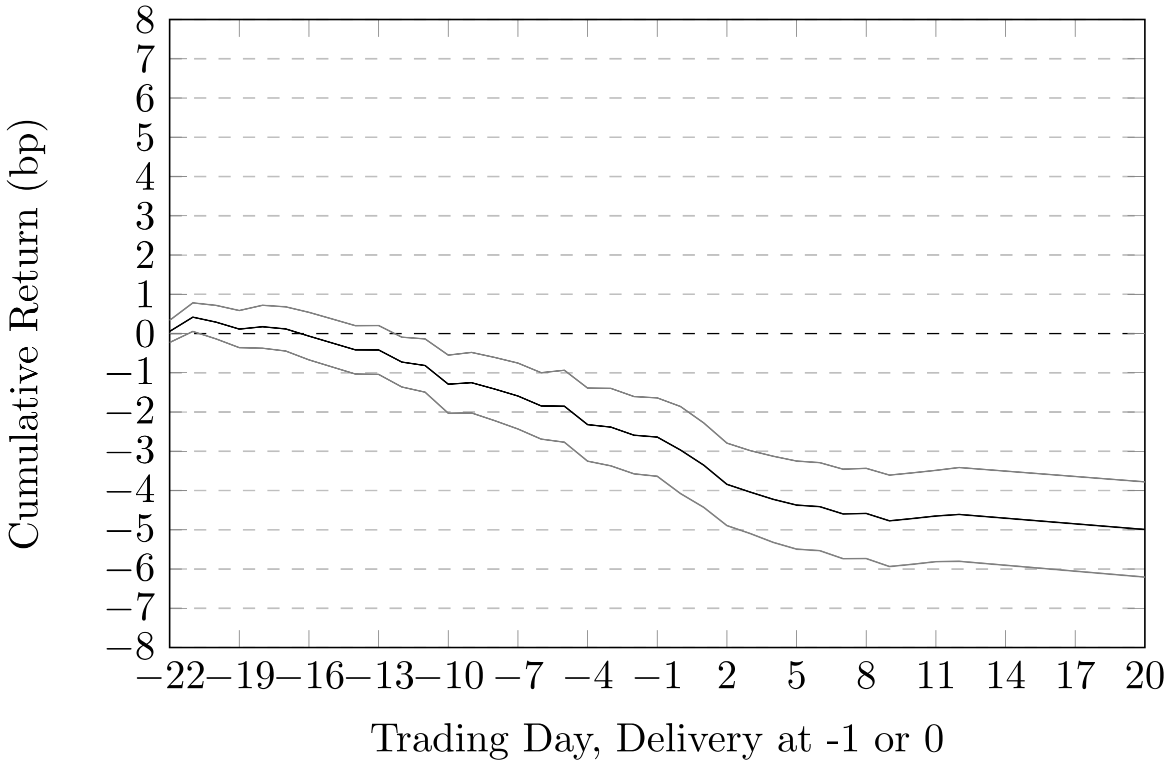


Figure 2: Bond Dynamics around Delivery to ETFs
This figure plots $\widehat{\beta_k^D}$ estimated using $\prod_{n=-22}^{k} BondRet_{b,t+n} = \beta_k^D Delivered_{b,f,t} + \beta^L Liquidity_{b,t} + \gamma_{f,t} + \alpha_b + \varepsilon_{b,f,t}$ for various k. The figure also displays 95% CIs using standard errors clustered by bond and trading date. $Delivered_{b,f,t}$ is a binary variable that takes the value of one if a bond increases in notional amount while a fund experiences a creation event. $Liquidity_{b,t}$ includes the prior month median Ahimud measure, implied round-trip costs, and bid-ask spreads. $\gamma_{f,t} + \alpha_b$ are fund-by-date and bond fixed effects.

2.5.1 ETF Underperformance around Creation Events

As described above in the discussion of share creations, the bond underperformance documented in Figure 2 should translate to a hidden cost of lower relative fund performance when

[13]Nevertheless, additional creations may also imply smaller premiums that may be desirable to ETF investors.

creations occur. To test this hypothesis at the fund level and obtain empirical estimates of the size of the hidden cost, I regress spreads over varying horizons between cumulative fund returns and changes in underlying indices on a binary variable that indicates whether a fund experienced a creation event on a given day, fund fixed effects, and time fixed effects:

$$RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$$

Importantly, I estimate this hidden cost using spreads between ETF returns and changes in the underlying indices. Creation events still likely mean good news for ETF investors. Conditional on a creation event, ETF returns and underlying indices are both higher than they would be had no creation events occurred: this result likely reflects increased demand for a given asset. While flows raise price levels for both ETFs and assets in the underlying index, they also embed a hidden cost incurred for receiving assets that earn lower future returns. Thus, the hidden cost causes ETF investors to "miss a bit of the upside." By estimating the effects on spreads I am able to isolate these effects and increase the power of my tests significantly. Additionally, by including fund fixed effects I implicitly control for time-invariant characteristics, such as management fees and net expense ratios, that may have attracted investor flows during the sample period while explaining ETF performance. If more expensive ETFs were introduced during the sample period that were able to attract greater flows (for example by targeting a novel set of corporate bonds), flows might be correlated with, without being the source of, the poor performance. Fund fixed effects should control for this problem and instead allow the estimates to document the direct impact of share creations on performance.

The stated and desirable purpose of creation events is to arbitrage away premiums. Thus, conditional on observing a creation event at t=-1, one can infer that a premium was likely created in the days prior to the creation. The creation of this premium will likely result in positive cumulative spreads, in the periods prior to share creation. As the APs arbitrage away premiums, spreads should revert back to zero over some time horizon. Over long time horizons, such as the +/- one-month window that I have investigated, the premium arbitrage mechanism implies that cumulative spreads should be zero.[14] Thus, if no hidden cost incurred from creation events arise, the dynamics of return spreads should appear as in Figure 3a. If the receipt of assets with lower future returns indeed impacts fund performance, this effect would appear as an additional negative drift in spreads beginning at time zero, as illustrated in Figure 3b. I hypothesize that both of these mechanisms should occur and thus their joint effects should be observable, as in Figure 3c. Importantly, because the mechanism that drives the arbitrage motivation for creations implies a long-run zero impact on spreads (the null hypothesis that only the arbitrage mechanism is at play implies a long-run coefficient of zero), negative performance at the end of the +/- one-month window can instead be attributed to the hidden cost and acts as a point estimate of the size of this cost. Figure 4 displays the dynamics of the β_k coefficients, also showing 95 % confidence intervals using standard errors that account for two-way clusters by ETF and by date. Consistent with both the hypothesized hidden cost and the arbitrage mechanism that is associated with creations, ETFs outperform the underlying index prior to creations and revert following creations, but over the long horizon underperform the underlying index by 1.4 basis points. Full parameter estimates for select horizons can be found in Appendix Table A2. Thus, ETF investors suffer a 1.4 basis-point performance drag per creation event.

2.5.2 ETF NAV Dynamics around Creation Events

I next estimate the performance of spreads between ETF NAVs and the underlying index. If ETF managers do well in maintaining portfolios that are representative of the underlying indices that they seek to track and they utilize the same pricing services as those used to set underlying indices, the NAV of an ETF and the underlying index should move closely

[14] If the spreads were not zero but the ETF successfully mirrored the underlying index, premiums and discounts would have been permanently drifting from the value of one.

together and deviations should be mean zero. Thus, the arbitrage mechanism predicts that creations will have no impact on spreads between NAVs and underlying indices. If the conjectured hidden cost exist, it occurs because an ETF portfolio fails to represent the underlying indices sufficiently over time. Specifically, this occurs when an ETF portfolio shifts to assets with lower expected future returns exactly at the point of creation. Thus, if the hidden cost exists, it should be observable in a dynamic that is very similar to that shown in Figure 3b. To test whether the hidden cost is observable in ETF NAVs, I estimate the following regression over various horizons. This estimation is the same as the estimation reported in Figure 4 but with an alternative spread as the dependent variable:

$$NAVSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t,k}$$

Figure 5 reports the dynamics of β_k in the +/- one-month window around share creation. I am unable to reject the null hypothesis that the long-run spread between NAVs and underlying indices is zero. Thus, I am unable to reject the null hypothesis that the dynamics are fully explained by the arbitrage motives associated with a creation event. It is possible that I lack sufficient statistical power to identify such an effect given the long time horizon over which I cumulate returns. Additionally, the use of stale NAVs by ETFs could lengthen the time that passes until the hidden cost is observable in NAVs in a manner that would reduce the power of my test. Full parameter estimates for select horizons can be found in Appendix Table A3.

2.5.3 ETF Underperformance around Creation Events after Accounting for the Arbitrage Mechanism

In the prior section I explain why long-horizon spreads should be zero if the only force at play is the arbitrage mechanism that creations facilitate. To test the robustness of this result, I instead seek to alternatively estimate the impact of creations by excluding the arbitrage mechanism while isolating the dynamics of the hidden cost. To do so, I control for the premium at the time of share creation and describe the post-creation dynamics of spreads between cumulative ETF returns and underlying index changes. Insofar as creations occur during trades that seek to capture premiums, the sizes of such premiums are key determinants of the arbitrage mechanism, as described in the previous section, while controlling for this mechanism should isolate the hidden cost of creations. I therefore estimate the following regression over various time horizons:

$$RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \beta^P Premium_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$$

This specification involves two key differences from those that underlie the results reported in Figure 4. First, the premium as of the end of day t=0 is included as a control. Second, cumulative returns are benchmarked to the end of day zero and only post-creation performance is accumulated. I present present the dynamics of β_k in Figure 6. Following a creation event and after controlling for the premium at the time of the creation, ETFs underperform their stated benchmarks by an additional 2.3 basis points, a finding that is consistent with the presence of a hidden cost of creations. Full parameter estimates for select horizons can be found in Appendix Table A4.

After controlling for premiums and discounts, the hidden cost my analysis reveals should also be observable in the implied performance of fund NAVs relative to changes in the underlying index. Thus, I estimate similar regressions but with spreads between NAVs and underlying indices:

$$NAVSpread_{f,t,k} = \beta_k Creation_{f,t} + \beta^P Premium_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$$

Figure 7 presents the dynamics of β_k. Full parameter estimates for select horizons can be found in Appendix Table A5. Following a creation event after controlling for premiums and discounts, ETF NAVs underperform on average by .6 basis points. Over the full 20-day horizon this .6 basis-point figure is significant at the 10% level but not at the 5% level. For 15 of the 19 horizons considered, the estimates are significant at the 5% level. The size estimates peak 18 days following creation at .8 basis points. These dynamics are hard to rationalize based on APs' arbitrage motives. If creations do not shift ETF portfolios away from a representative sample of the underlying index and the same prices are used to calculate both, NAVs and the underlying index should remain tightly linked even during arbitrage and no underperformance should be observable. Additionally, a reader may be concerned that controlling for premiums fails to rule out arbitrage and that pricing differences between the two may exist. Nonetheless, these results would still be hard to explain by reference to the arbitrage mechanism. APs engage in share creation to capture premiums that occur when ETF prices are too high relative to NAVs. Thus, one would expect creations to imply that ETF prices are too high, returns should be lower, and NAVs are too low and thus should outperform the index. Instead, Figure 7 documents NAV underperformance, contradicting even the hypothesis arising from arbitrage mechanisms in which NAVs are too low. NAV underperformance is not consistent with the objectives of the creation/redemption process and instead is best explained by the hidden cost embedded when share creation with a subset of assets with lower expected future returns is allowed.

2.6 Additional Findings

2.6.1 The Relationship between Fund Characteristics and the Size of the Hidden Cost

The return dynamics of delivered bonds both prior and subsequent to delivery are consistent with APs' utilizing information regarding future changes in fund NAVs to decide strategically which assets to deliver into ETF portfolios. Such strategic interactions provide marginal incentives on top of APs' well-understood incentives to profitably arbitrage differences in ETF prices and underlying assets' value. Theory clearly predicts when strategic behavior should be more profitable,and thus more likely to occur, conditional on observing a creation event, and finally therefore where the hidden cost should be the highest.

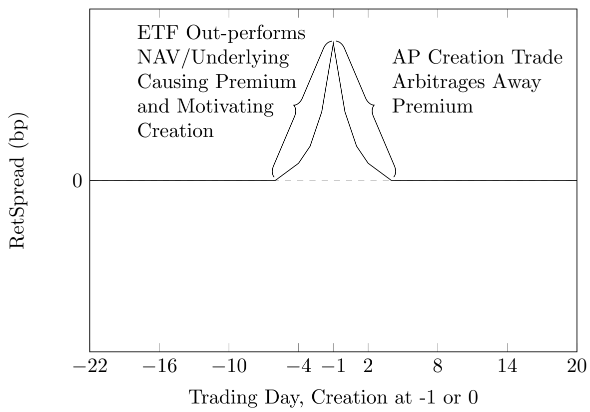
First, APs' strategic incentives will be stronger when their information regarding future NAVs is more valuable. It is well understood that agents are likely to possess more valuable information in less liquid or less efficient markets (in fact, agents who possess valuable information can endogenously make those markets less liquid in many classical market-microstructure models). Additionally, ETF managers adopt less stringent share-creation rules in the presence of asset liquidity. Therefore, the hidden cost should be higher when underlying assets are less liquid.

Second, APs' ability to deliver bonds strategically is enhanced when they are required to deliver fewer bonds. If an AP has information that is relevant to the future performance of a given bond that will impact their expected profits from a creation trade, the impact will be much greater if that bond is one of ten bonds that needs to be delivered versus being one of a hundred bonds to be delivered. Moreover, it may be easier for APs to acquire information about the idiosyncratic performance of a small number of bonds. Thus, when creation baskets are large, it is more difficult for APs to acquire information regarding the average performance of a given creation basket and the strategic incentive will be relatively weaker, enabling the arbitrage incentive to dominate.

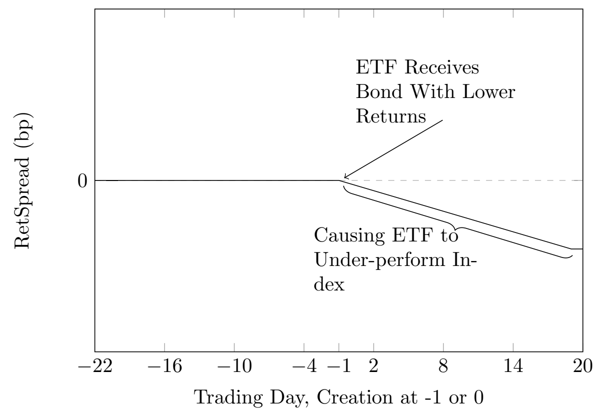
To assess theoretical predictions regarding fund differences and thus validate the described mechanism, I estimate how the cost per creation is related to fund characteristics. Specifically, I estimate the following regression:

$$RetSpread_{f,t,20} = \beta_1 Creation_{f_t} + \beta_2 Creation_{f,t} * FundCharacteristic_f + \alpha_t + \gamma_f + \varepsilon_{f,t}$$

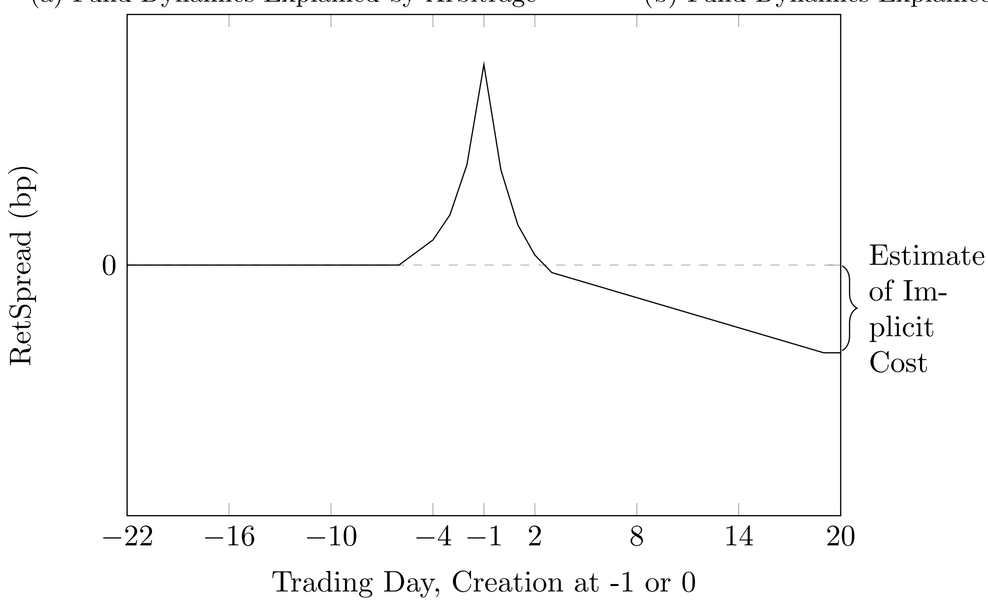
I consider five fund characteristics in particular. First, I examine the relationship between an ETF's asset focus and the cost per creation using $InvestmentGrade_f$. $InvestmentGrade_f$ takes the value of 1 if the focus as reported by ETF Global is on investment-grade corporate bonds. Most funds that do not focus on investment-grade funds report focusing on highyield bonds. Additionally, some funds focus explicitly on asset-backed securities, convertible bonds, and loans. In all such cases, the investment-grade bond market is likely relatively more efficient and liquid. In column 1 of Table 2 I report results indicating that investment-grade bonds typically incur a 2.41 basis-point lower cost per creation than other types of corporate bond funds, a finding that is consistent with theory. The t-stat associated with this estimate is 1.97 and is significant only at the 10% threshold.



(a) Fund Dynamics Explained by Arbitrage



(b) Fund Dynamics Explained by Hidden Cost



(c) Conjectured Fund Dynamics Where Both Occur

Figure 3: Conjectured ETF Return Spread Dynamics
This figure plots the hypothesized dynamics of β_k, estimated using $RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for various k. $RetSpread_{f,t,k}$ is the cumulative difference between ETF returns and the benchmark index cumulated through k days following the observation date: $RetSpread_{f,t,k} = \prod_{n=-22}^{k} ETFRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $RetSpread_{f,t,-1}$ is the cumulative difference between the ETF and the index up to 1 day prior to observation (or share creation in the case of a creation event). $Creation_{f,t}$ is a binary variable that takes the value of one if shares outstanding increased for a fund and zero otherwise. $\gamma_t + \alpha_f$ are date and fund fixed effects. Subfigure (a) displays the hypothesized dynamics if only the desired arbitrage mechanism is evident. Subfigure (b) displays the additional hidden cost for receiving bonds that earn lower returns. Subfigure (c) shows the hypothesized dynamics if a hidden cost exist.

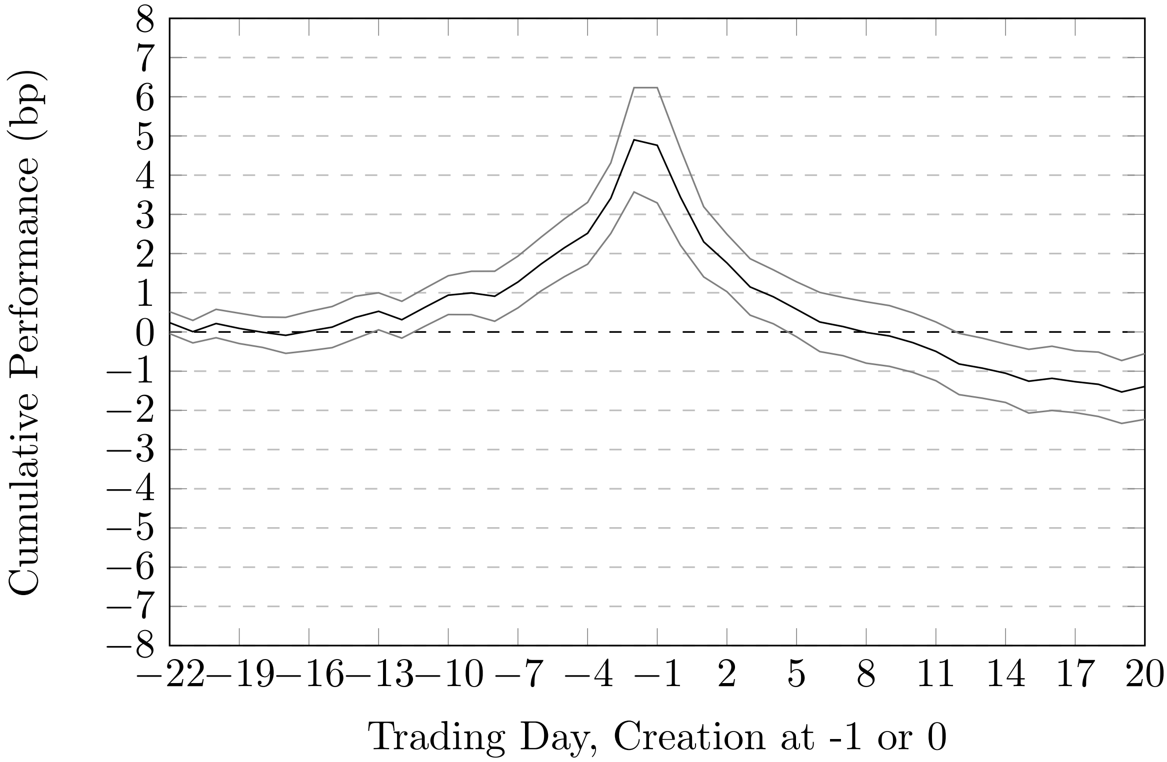


Figure 4: Return Spread Dynamics around Creation Events
This figure plots $\widehat{\beta_k}$ estimated using $RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for various k. The figure also displays 95% CIs using standard errors clustered by fund and trading date. $RetSpread_{f,t,k}$ is the cumulative difference between ETF returns and the benchmark index cumulated through k days following the observation date: $RetSpread_{f,t,k} = \prod_{n=-22}^{k} ETFRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $RetSpread_{f,t,-1}$ is the cumulative difference between the ETF and the index up to 1 day prior to observation (or share creation in the case of a creation event). $Creation_{f,t}$ is a binary variable that takes the value of one if a fund's shares outstanding increased and zero otherwise. $\gamma_t + \alpha_f$ are date and fund fixed effects.

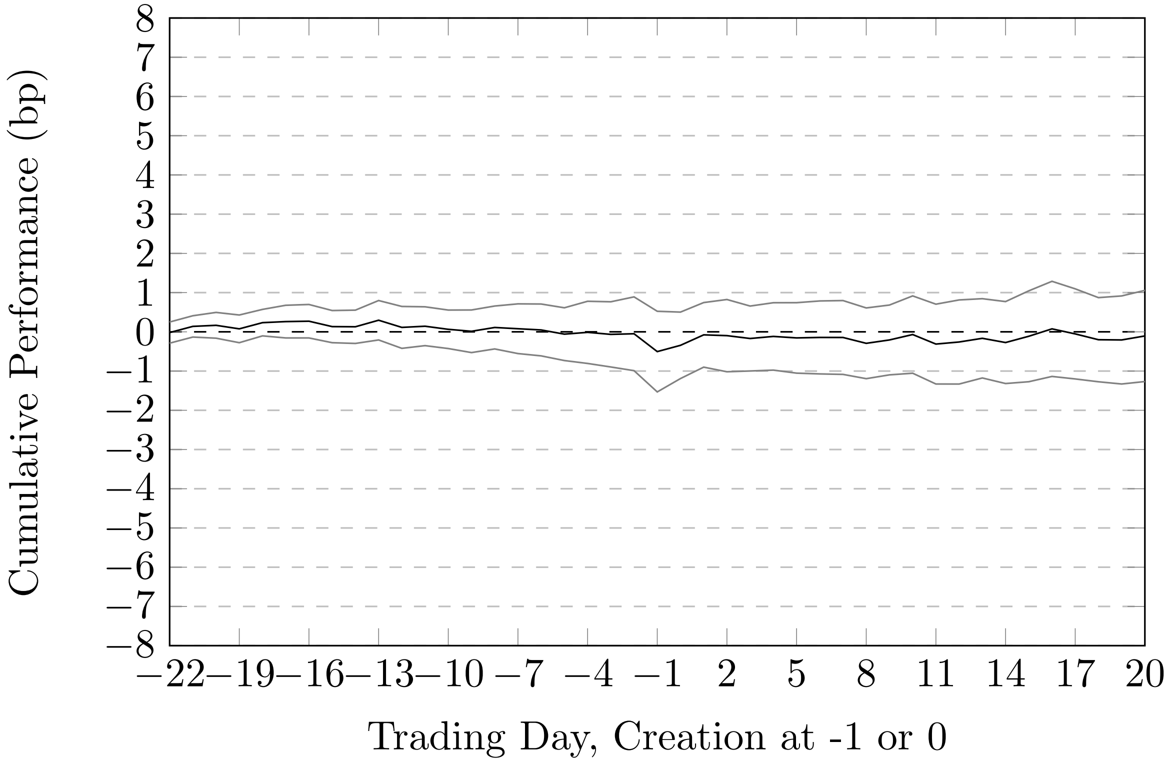


Figure 5: NAV Spread Dynamics around Creation Events
This figure plots $\widehat{\beta_k}$ estimated using $NAVSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for various k. The figure also displays 95% CIs using standard errors clustered by fund and trading date. $NAVSpread_{f,t,k}$ is the cumulative difference between the return implied by fund NAVs and the benchmark index cumulated through k days following the observation date: $NAVSpread_{f,t,k} = \prod_{n=-22}^{k} NAVRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $NAVSpread_{f,t,-1}$ is the cumulative difference between an ETF's assets and the index up to 1 day prior to observation (or share creation in the case of a creation event). $Creation_{f,t}$ is a binary variable that takes the value of one if a fund's shares outstanding increased and zero otherwise. $\gamma_t + \alpha_f$ are date and fund fixed effects.

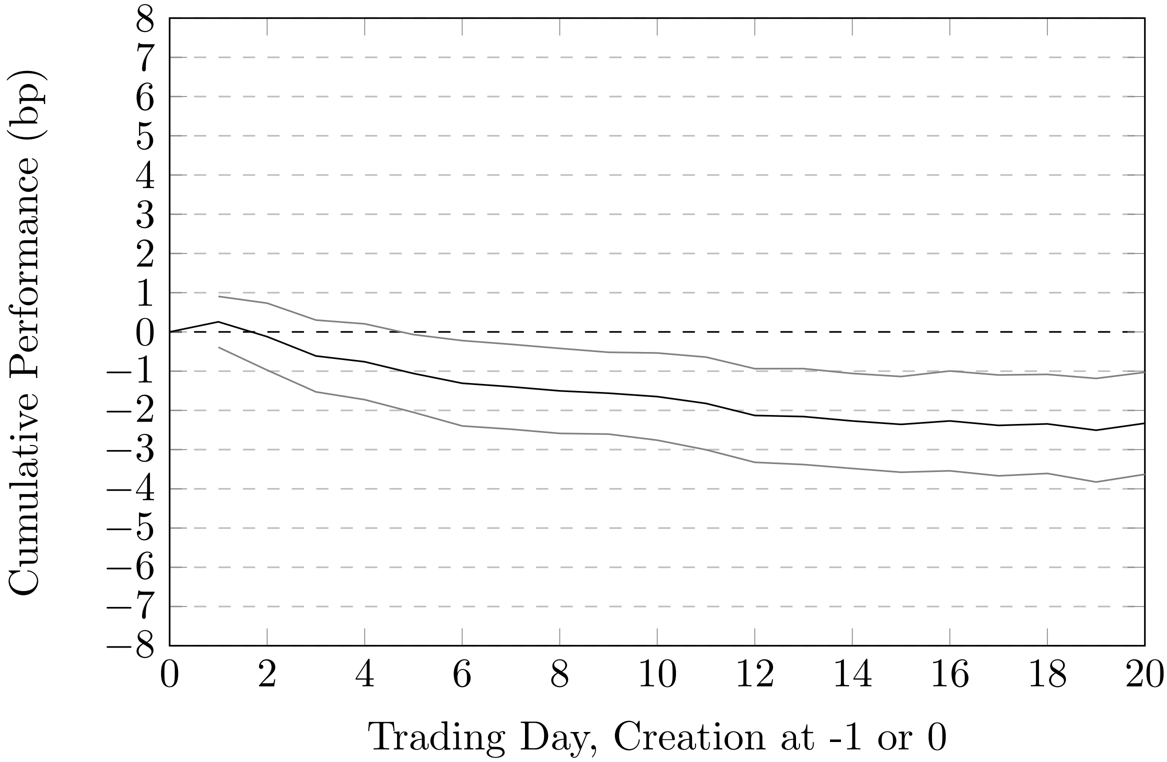


Figure 6: Return Spread Dynamics around Creation Events while Controlling for Premiums
This figure plots $\widehat{\beta_k}$ estimated using $RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \beta^P Premium_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for various k. The figure also displays 95% CIs using standard errors clustered by fund and trading date. $RetSpread_{f,t,k}$ is the cumulative difference between ETF returns and the benchmark index cumulated through k days following the observation date: $RetSpread_{f,t,k} = \prod_{n=-22}^{k} ETFRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $RetSpread_{f,t,-1}$ is the cumulative difference between the ETF and the index up to 1 day prior to observation (or share creation in the case of a creation event). $Creation_{f,t}$ is a binary variable that takes the value of one if a fund's shares outstanding increased and zero otherwise. $Premium_{f,t}$ is the premium or discount as represented in percentage terms (%100 representing no discount or premium). $\gamma_t + \alpha_f$ are date and fund fixed effects.

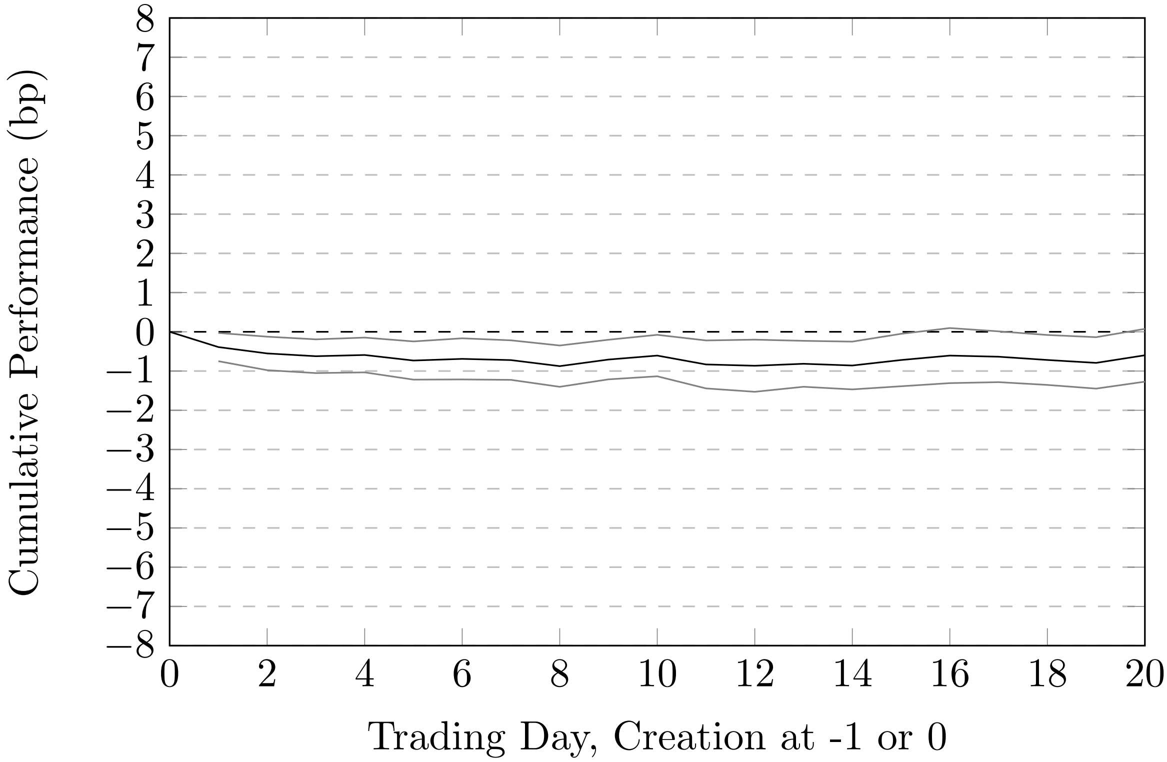


Figure 7: NAV Spread Dynamics around Creation Events while Controlling for Premiums
This figure plots $\widehat{\beta_k}$ estimated using $NAVSpread_{f,t,k} = \beta_k Creation_{f,t} + \beta^P Premium_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for various k. The figure also displays 95% CIs using standard errors clustered by fund and trading date. $NAVSpread_{f,t,k}$ is the cumulative difference between returns implied by fund NAVs and the benchmark index cumulated through k days following the observation date: $NAVSpread_{f,t,k} = \prod_{n=-22}^{k} NAVRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $NAVSpread_{f,t,-1}$ is the cumulative difference between the ETF's assets and the index up to 1 day prior to observation (or share creation in the case of a creation event). $Creation_{f,t}$ is a binary variable that takes the value of one if a fund's shares outstanding increased and zero otherwise. $Premium_{f,t}$ is the premium or discount as represented in percentage terms (%100 representing no discount or premium). $\gamma_t + \alpha_f$ are date and fund fixed effects.

Table 2: Fund Characteristics' Relationship to Hidden Costs

This table reports the relationship between fund characteristics and estimates of the hidden cost per creation for corporate bond ETFs. Relationships are estimated based on the following specification:
$RetSpread_{f,t,20} = \beta_1 Creation_{f_t} + \beta_2 Creation_{f,t} * FundCharacteristic_f + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for five fund characteristics.
The first fund characteristic, $InvestmentGrade_f$, indicates whether the fund is a reported by ETF Global as focusing on Investment Grade corporate bonds. ln(AverageBasketSize$_f$) represents the natural log of the average number of securities delivered when a creation event occurs. $AverageAmihud_f$ represents the average prior month median Amihud measure for bonds held by the fund. $AverageIRC_f$ represents the average prior month median imputed round-trip cost measure for bonds held by the fund. $AverageBidAsk_f$ represents the average prior month median bid-ask spread measure for bonds held by the fund. $AverageAmihud_f$, $AverageIRC_f$, and $AverageBidAsk_f$ are winsorized at the 1% level. $RetSpread_{f,t,20}$ is the cumulative difference between the ETF return and the benchmark index cumulated through 20 days after the observation date: $RetSpread_{f,t,20} = \prod_{n=-22}^{20} ETFRet_{f,t+n} - \prod_{n=-22}^{20} \Delta UnderlyingIndex_{f,t+n}$. $Creation_{f,t}$ is a binary variable that takes the value of one if shares outstanding increased for a fund and zero otherwise. $\gamma_t + \alpha_f$ are date and fund fixed effects. One, two, and three stars indicate statistical significance at the 10%, 5%, and 1% levels respectively.

	Dependent Variable Spread Between Cumulative ETF and Benchmark Return Through 1 Month After Creation RetSpread$_{f,t,20}$				
Creation$_{f,t}$	-2.90***	-5.78***	0.10	-1.27	-1.16
	(-3.03)	(-3.11)	(-0.53)	(0.10)	(-1.22)
Creation$_{f_t} * InvestmentGrade_f$	2.41*				
	(1.97)				
Creation$_{f_t} * \ln(AverageBasketSize_f)$		1.03**			
		(2.38)			
Creation$_{f_t} * AverageAmihud_f$			-48.83*		
			(-1.75)		
Creation$_{f_t} * AverageIRC_f$				-0.27	
				(-0.40)	
Creation$_{f_t} * AverageBidAsk_f$					-1.71
					(-0.58)
Fund Fixed Effects	Y	Y	Y	Y	Y
Date Fixed Effects	Y	Y	Y	Y	Y
Standard Errors	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date
N	134,519	104,791	123,820	123,820	123,820
Adjusted R^2	8.99%	10.86%	10.01%	10.01%	10.01%
Within F.E. R^2	0.02%	0.03%	0.02%	0.02%	0.02%

Second, I examine the relationship between the size of a creation basket, as measured by $\ln(AverageBasketSize)$. To do so, I infer all assets that were delivered from ETF holdings data (including those without TRACE information) and calculate the average size of the basket conditional on observing any delivery. APs find it more difficult to utilize information regarding specific components of larger baskets. I report the corresponding results in column 2 of Table 2. The coefficient on $\ln(AverageBasketSize)$ is 1.03, implying that, when basket size doubles, the cost per creation decreases by .71 basis points, roughly half of the unconditional cost per creation of 1.39 basis points. The t-stat associated with this estimate is 2.38 and is statistically significant.

Last, I investigate three measures of portfolio liquidity: $AverageAmihud_f$, $AverageIRC_f$, and $AverageBidAsk_f$. $AverageAmihud_f$ represents the average prior month median Amihud measure for bonds held by a fund. $AverageIRC_f$ represents the average prior month median imputed round-trip cost measure for bonds held by a fund. $AverageBidAsk_f$ represents the average prior month median bid-ask spread measure for bonds held by a fund. All three values represent asset illiquidity and rise when underlying bonds are less liquid. I report the corresponding results in columns 3-5 of Table 2. I find a marginally statistically significant relationship only for $AverageAmihud_f$: the coefficient is -48.83 with a t-stat of -1.75. This coefficient, coupled with the previously discussed $InvestmentGrade_f$ estimate, provides suggestive evidence that the cost per creation is higher when assets are less liquid, a finding that is consistent with theory.

In summary, fund characteristics predicted by APs' strategic incentives are predictive of the estimated hidden cost per creation, further validating the mechanism described in this paper. When ETFs hold less liquid assets and when ETF managers allow smaller baskets to be delivered in the face of asset illiquidity, the cost is higher. Such results highlight that ETF investors pay an implicit cost, either knowingly or unknowingly, for the liquidity transformation services that corporate bond ETFs provide.

2.6.2 Large-Cap Equity ETFs: A Falsification Test

Unlike corporate bond ETFs, ETFs that hold large-cap US equities almost always require APs to deliver all underlying assets when they create new shares.[15] Therefore, large-cap equity ETFs lack the institutional features that are necessary to embed the hidden cost, making them an effective sample for a falsification test of my main specification. If large-cap equities underperform following a creation event, the associated underperformance of large-cap equity funds, and thus likely also of corporate bond funds, can be explained by an alternative mechanism. Therefore, I re-estimate the results I report in Figure 4, utilizing a sample of ETFs in the "Equity" asset class, with a "Large-Cap" and "North America" focus as reported by ETF Global. In Figure 8, I again report the dynamics of β_k coefficients and display 95% confidence intervals. I cannot reject the null hypothesis that large-cap US equity ETFs show no underperformance when they experience creation days, a finding that is consistent with theory. ETFs embed the hidden cost only when they modify the creation process to accommodate illiquid assets such as corporate bonds.

2.7 Conclusion

In this paper I explain how the use of creation baskets that contain only subsets of assets that underlie corporate bond ETFs can embed a hidden cost that ETF investors pay. I document that APs deliver bonds that earn lower future returns to ETFs, with the result that ETFs underperform their stated benchmarks by an additional 1.4 basis points per creation event. Additionally, I isolate this cost by controlling for arbitrage motives and document the cost using ETF prices and ETF NAVs. While the arbitrage mechanism is certainly present around creation events, this set of facts cannot be fully explained by APs' arbitrage trades and instead illustrates the existence of a hidden cost of share creation. This hidden cost is

[15] These ETFs occasionally will utilize custom creation baskets to intentionally shift their portfolio. For example, ETFs will often utilize a heartbeart trade upon index insertion/deletions as described in Moussawi et al. (2019). These custom creation baskets are often designed by ETF managers at set dates and do not face the same adverse selection present in corporate bond ETFs.

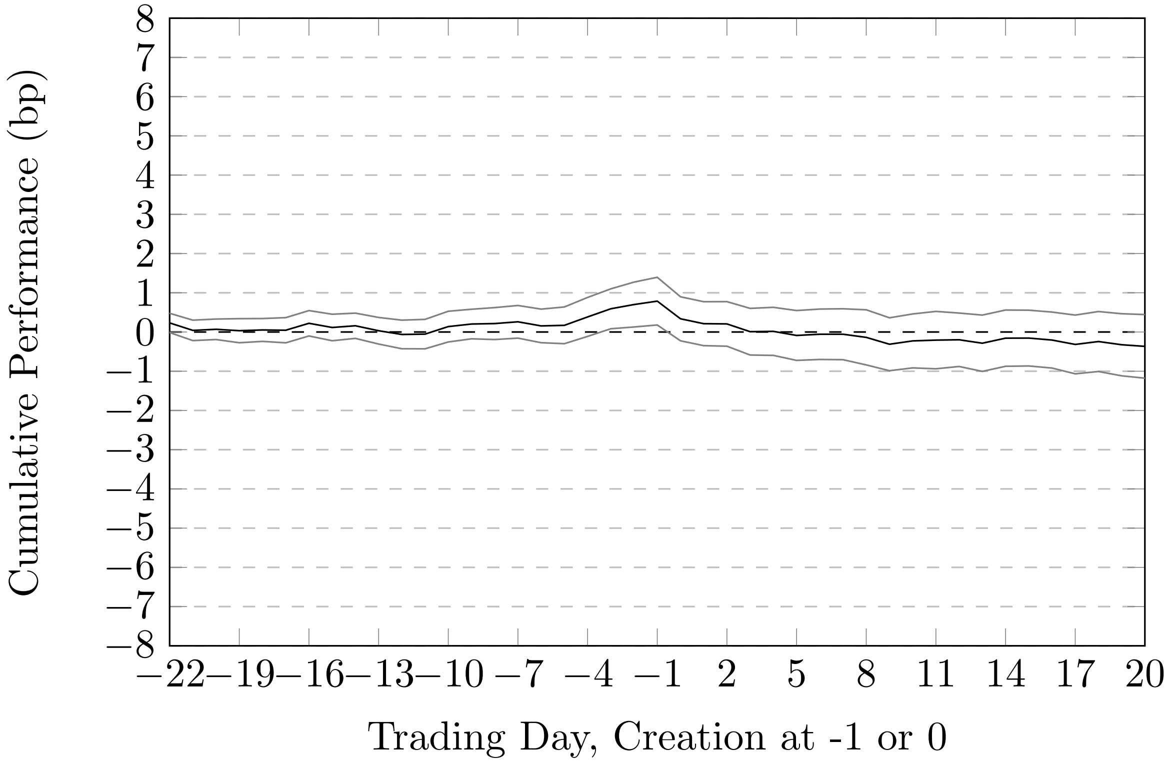


Figure 8: Falsification Test: Return Spread Dynamics around Large US Equity ETFs
This figure plots $\widehat{\beta_k}$ estimated using $RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for various k. The figure also displays 95% CIs using standard errors clustered by fund and trading date. $RetSpread_{f,t,k}$ is the cumulative difference between ETF returns and the benchmark index cumulated through k days following the observation date: $RetSpread_{f,t,k} = \prod_{n=-22}^{k} ETFRet_{f,t+n} - \prod_{n=-22}^{k} \Delta UnderlyingIndex_{f,t+n}$. For example, $RetSpread_{f,t,-1}$ is the cumulative difference between the ETF and the index up to 1 day prior to observation (or share creation in the case of a creation event). $Creation_{f,t}$ is a binary variable that takes the value of one if a fund's shares outstanding increased and zero otherwise. $\gamma_t + \alpha_f$ are date and fund fixed effects.

high; investors in the average corporate bond ETF pay a 48 basis-points-per-annum hidden cost while the average corporate bond ETF reports only 35 basis points per annum in explicit costs. If ETF investors are fully aware of the hidden cost, the magnitude of the cost reveals a high willingness-to-pay for the liquidity tranformation provided by corporate bond ETFs.

This hidden cost also has important policy implications. First, it highlights the need to scrutinize the prices that are used to set ETF NAVs and thus settle creations. Prior to this finding, regulators, academic researchers, and market participants may have reasonably believed that NAV accuracy is critical only to the performance of open-ended mutual funds, not ETF performance. Second, my finding highlights the cost attributable to partial creation baskets and in particular the cost attributable to custom creation baskets that are allowed under SEC rule 6c-11. While these custom creation baskets allow ETFs to engage in activities such as heartbeat trades to defer taxes that benefit ETF investors, they also can embed a high cost and thus represent an area of potential regulatory oversight or beneficial increases in transparency for ETF investors.

The existence of thr hidden cost I have documented implies that investors, either knowingly or unknowingly, pay a high cost for the liquidity transformation that corporate bond ETFs provide. Specifically, the cost results from modifications to creation rules that are designed to induce APs to conduct arbitrage trades despite the illiquidity of underlying assets. Many policymakers and academic researchers have expressed concerns over the fragility that might result from liquidity transformation. I demonstrate that, even if asset illiquidity poses no risk to financial stability, liquidity transformation is not a "free lunch." The concessions that ETF managers must make to induce arbitrage activity incur a high cost as a result of APs' ability to interact strategically with the rules. Because this cost results directly from asset illiquidity, the framework of Chordia (1996), which implies that the more illiquid the underlying assets are, the higher are the barriers to investor fund flows, likely extends from mutual funds to ETFs. This cost of liquidity transformation also helps to rationalize flow performance relationships in ETFs and to resolve open puzzles in the important strand of literature that examines flows to investment managers. Last, as the cost of such liquidity transformation is higher than the explicit fees reported by corporate bond ETFs, the findings I have reported in this paper are of the first order in describing the performance realized by ETF investors.

Table A1: Bond Dynamics Around Delivery to ETFs

$\prod_{n=-22}^{k} BondRet_{b,t+n} = \beta_k^D Delivered_{b,f,t} + \beta^L Liquidity_{b,t} + \gamma_{f,t} + \alpha_b + \varepsilon_{b,f,t}$ for select k

	Dependent Variable Cumulative Bond Returns		
	Up to Creation $\prod_{n=-22}^{-1} BondRet_{b,t+n}$	through 1 Week following Creation $\prod_{n=-22}^{5} BondRet_{b,t+n}$	through 1 Month following Creation $\prod_{n=-22}^{20} BondRet_{b,t+n}$
$\text{Delivered}_{b,f,t}$	-2.63***	-4.36***	-4.99***
	(-4.38)	(-6.45)	(-6.47)
$\text{Amihud}_{b,t}$	80.10***	104.97***	143.50***
	(4.87)	(5.29)	(5.15)
$\text{IRC}_{b,t}$	1.60***	2.34***	4.53***
	(4.30)	(5.38)	(8.13)
$\text{BidAsk}_{b,t}$	14.91***	18.17***	19.17***
	(5.93)	(6.32)	(5.56)
Bond Fixed Effects	Y	Y	Y
ETF*Date Fixed Effects	Y	Y	Y
Standard Errors	2-way Clustered by Bond and Date	2 way Clustered by Bond and Date	2 way Clustered by Bond and Date
N	9,544,076	9,544,076	9,544,076
Adjusted R^2	39.20%	43.05%	51.69%
Within F.E. R^2	0.14%	0.20%	0.32%

Table A2: Return Spread Dynamics Around Creation

$\widehat{\beta}_k$ estimated from $RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for select k are shown.

	Dependent Variable Spread Between Cumulative ETF and Benchmark Return Up to Creation $\text{RetSpread}_{f,t,-1}$	 Through 1 Week After Creation $\text{RetSpread}_{f,t,5}$	 Through 1 Month After Creation $\text{RetSpread}_{f,t,20}$
$\text{Creation}_{f,t}$	4.76***	0.58	-1.39***
	(6.40)	(1.63)	(-3.28)
Fund Fixed Effects	Y	Y	Y
Date Fixed Effects	Y	Y	Y
Standard Errors	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date
N	134,519	134,519	134,519
Adjusted R^2	8.41%	8.53%	8.98%
Within F.E. R^2	0.20%	0.00%	0.01%

Table A3: NAV Spread Dynamics Around Creation Events

$\widehat{\beta}_k$ estimated from $NAVSpread_{f,t,k} = \beta_k Creation_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for select k are shown.

	Dependent Variable Spread Between Cumulative ETF and Benchmark Return Up to Creation $\text{NAVSpread}_{f,t,-1}$	through 1 Week following Creation $\text{NAVSpread}_{f,t,5}$	through 1 Month following Creation $\text{NAVSpread}_{f,t,20}$
$\text{Creation}_{f,t}$	-0.50	-0.16	-0.11
	(-0.97)	(-0.34)	(-0.18)
Fund Fixed Effects	Y	Y	Y
Date Fixed Effects	Y	Y	Y
Standard Errors	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date
N	134,519	134,519	134,519
Adjusted R^2	12.84%	12.95%	13.32%
Within F.E. R^2	0.00%	0.00%	0.00%

Table A4: Return Spread Dynamics Around Creation Events while Controlling for Premiums
$\widehat{\beta}_k$ estimated from $RetSpread_{f,t,k} = \beta_k Creation_{f,t} + \beta^P Premium_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for select k are plotted.

	Dependent Variable Spread Between Cumulative ETF and Benchmark Returns through 1 Week following Creation RetSpread$_{f,t,5}$	 through 1 Month following Creation RetSpread$_{f,t,20}$
Creation$_{f,t}$	-1.06**	-2.33***
	(-2.11)	(-3.54)
Premium$_{f,t}$	-2244.59***	-3157.40***
	(-9.45)	(-10.15)
Fund Fixed Effects	Y	Y
Date Fixed Effects	Y	Y
Standard Errors	2-way Clustered by Fund and Date	2 way Clustered by Fund and Date
N	139,080	139,080
Adjusted R^2	17.30%	19.10%
Within F.E. R^2	9.78%	11.98%

Table A5: NAV Spread Dynamics Around Creation Controlling For Premiums

$\widehat{\beta}_k$ estimated from $NAVSpread_{f,t,k} = \beta_k Creation_{f,t} + \beta^P Premium_{f,t} + \alpha_t + \gamma_f + \varepsilon_{f,t}$ for select k are plotted.

	Dependent Variable Spread Between Cumulative ETF and Benchmark Return	
	Through 1 Week After Creation	Through 1 Month After Creation
	NAVSpread$_{f,t,5}$	NAVSpread$_{f,t,20}$
Creation$_{f,t}$	-0.73***	-0.60*
	(-2.98)	(-1.76)
Premium$_{f,t}$	1083.44***	1211.17***
	(6.25)	(7.32)
Fund Fixed Effects	Y	Y
Date Fixed Effects	Y	Y
Standard Errors	2 way Clustered by Fund and Date	2 way Clustered by Fund and Date
N	139,080	139,080
Adjusted R^2	18.51%	15.99%
Within F.E. R^2	4.76%	3.67%

References

Amihud, Yakov (2002), "Illiquidity and stock returns: cross-section and time-series effects." *Journal of Financial Markets*, 5, 31–56, URL https://www.sciencedirect.com/science/article/pii/S1386418101000246.

Ben-David, Itzhak, Francesco Franzoni, and Rabih Moussawi (2018), "Do etfs increase volatility?" *The Journal of Finance*, 73, 2471–2535.

Ben-Rephael, Azi, Shmuel Kandel, and Avi Wohl (2012), "Measuring investor sentiment with mutual fund flows." *Journal of financial Economics*, 104, 363–382.

Berk, Jonathan B and Richard C Green (2004), "Mutual fund flows and performance in rational markets." *Journal of political economy*, 112, 1269–1295.

Brown, David C, Shaun Davies, and Matthew Ringgenberg (2019), "Etf arbitrage, non-fundamental demand, and return predictability." *Available at SSRN 2872414*.

Chakravarty, Sugato and Asani Sarkar (2003), "Trading costs in three us bond markets." *The Journal of Fixed Income*, 13, 39–48.

Chalmers, John MR, Roger M Edelen, and Gregory B Kadlec (2001), "On the perils of financial intermediaries setting security prices: the mutual fund wild card option." *The Journal of Finance*, 56, 2209–2236.

Chen, Qi, Itay Goldstein, and Wei Jiang (2010a), "Payoff complementarities and financial fragility: Evidence from mutual fund outflows." *Journal of Financial Economics*, 97, 239–262.

Chen, Qi, Itay Goldstein, and Wei Jiang (2010b), "Payoff complementarities and financial fragility: Evidence from mutual fund outflows." *Journal of Financial Economics*, 97, 239–262.

Choi, Jaewon, Mathias Kronlund, and Ji Yeol Jimmy Oh (2019), "Sitting bucks: Zero returns in fixed income funds." *Available at SSRN 3244862*.

Chordia, Tarun (1996), "The structure of mutual fund charges." *Journal of Financial Economics*, 41, 3–39, URL https://www.sciencedirect.com/science/article/pii/

0304405X9500856A.

Clifford, Christopher P, Jon A Fulkerson, and Bradford D Jordan (2014), "What drives etf flows?" *Financial Review*, 49, 619–642.

Coval, Joshua and Erik Stafford (2007), "Asset fire sales (and purchases) in equity markets." *Journal of Financial Economics*, 86, 479–512.

Daniel, Kent, Mark Grinblatt, Sheridan Titman, and Russ Wermers (1997), "Measuring mutual fund performance with characteristic-based benchmarks." *The Journal of finance*, 52, 1035–1058.

Dannhauser, Caitlin D (2017), "The impact of innovation: Evidence from corporate bond exchange-traded funds (etfs)." *Journal of Financial Economics*, 125, 537–560.

Dannhauser, Caitlin D and Saeid Hoseinzade (2021), "The unintended consequences of corporate bond etfs: Evidence from the taper tantrum." *The Review of Financial Studies*.

Dannhauser, Caitlin D and Jeffrey Pontiff (2019), "Flow." *Available at SSRN 3428702*.

Diamond, Douglas W and Philip H Dybvig (1983), "Bank runs, deposit insurance, and liquidity." *Journal of political economy*, 91, 401–419.

Doan, Viet-Dung (2020), "Exchange-traded funds and municipal bond market." *Available at SSRN 3703875*.

Edelen, Roger M (1999), "Investor flows and the assessed performance of open-end mutual funds." *Journal of Financial Economics*, 53, 439–466.

Edelen, Roger M and Jerold B Warner (2001), "Aggregate price effects of institutional trading: a study of mutual fund flow and market returns." *Journal of financial Economics*, 59, 195–220.

ETF.com (2021), "https://www.etf.com/etf-education-center/etf-basics/what-is-the-creationredemption-mechanism?nopaging=1."

Feldhütter, Peter (2012), "The same bond at different prices: identifying search frictions and selling pressures." *The Review of Financial Studies*, 25, 1155–1206.

Frazzini, Andrea and Owen A Lamont (2008), "Dumb money: Mutual fund flows and the cross-section of stock returns." *Journal of financial economics*, 88, 299–322.

Friesen, Geoffrey C and Travis RA Sapp (2007), "Mutual fund flows and investor returns: An empirical examination of fund investor timing ability." *Journal of Banking & Finance*, 31, 2796–2816.

Haddad, Valentin, Alan Moreira, and Tyler Muir (2020), "When selling becomes viral: Disruptions in debt markets in the covid-19 crisis and the fed's response." Technical report, National Bureau of Economic Research.

Hong, Gwangheon and Arthur Warga (2000), "An empirical study of bond market transactions." *Financial Analysts Journal*, 56, 32–46.

Ippolito, Richard A (1992), "Consumer reaction to measures of poor quality: Evidence from the mutual fund industry." *The Journal of Law and Economics*, 35, 45–70.

Jensen, Michael C. (1968), "The performance of mutual funds in the period 1945-1964." *The Journal of Finance*, 23, 389–416, URL http://www.jstor.org/stable/2325404.

Jostova, Gergana, Stanislava Nikolova, Alexander Philipov, and Christof W. Stahel (2013), "Momentum in Corporate Bond Returns." *The Review of Financial Studies*, 26, 1649–1693, URL https://doi.org/10.1093/rfs/hht022.

Kostovetsky, Leonard (2003), "Index mutual funds and exchange-traded funds." *The Journal of Portfolio Management*, 29, 80–92.

Ma, Yiming, Kairong Xiao, and Yao Zeng (2020), "Mutual fund liquidity transformation and reverse flight to liquidity." *Available at SSRN 3640861*.

Moussawi, Rabih, Ke Shen, and Raisa Velthuis (2019), "Etf heartbeats, tax efficiencies, and clienteles." *Working Paper*.

Pan, Kevin and Yao Zeng (2019), "Etf arbitrage under liquidity mismatch." *Available at SSRN 2895478*.

Shim, John J and Karamfil Todorov (2021), "Etfs, illiquid assets, and fire sales." *Available at SSRN 3886881*.

Sirri, Erik R and Peter Tufano (1998), "Costly search and mutual fund flows." *The journal* *of finance*, 53, 1589–1622.

Wermers, Russ (2000), "Mutual fund performance: An empirical decomposition into stock-picking talent, style, transactions costs, and expenses." *The Journal of Finance*, 55, 1655–1695.

Zheng, Lu (1999), "Is money smart? a study of mutual fund investors' fund selection ability." *the Journal of Finance*, 54, 901–933.

Zitzewitz, Eric (2006), "How widespread was late trading in mutual funds?" *American Economic Review*, 96, 284–289.

3. Taking Sides on Return Predictability

In this paper we provide the broadest investigation to date of how various market participants trade. We study the trading of nine market participants—retail investors, short sellers, firms, and 6 types of institutions. Trading is examined with respect to 130 different firm-level variables that have been shown to predict the cross-section of stock returns (anomalies) and how each participant's trades forecast returns.

For each investor type, we calculate changes in ownership over the 1-year and 3-year periods preceding the month that the anomaly variables are constructed. This measurement tells us how each market participant changed their ownership in the years leading up to portfolio formation and conveys the likelihood that the participant is relatively over- or under-weighted in the anomaly portfolios.

Firms are the most informed traders. When we examine share issuance during the 3-years prior to expected return measurement, we find that the firms with the lowest expected returns are the largest net issuers. Taken together, the 130 predictors explain 32% of the cross-sectional variation in share issuance during this 3-year period. Share issuance is also a strong predictor of returns, even after controlling for the trades of the 8 other market participants. If we control for the expected returns reflected in the 130 variables, the predictability of share issuance is weakened, but not fully eliminated. Thus, although some firm trades reflect firm characteristics, which are observable to the public, some trades reflect private information.

After firms, short sellers are the most informed investors. Stocks with the lowest expected returns, as reflected in the 130 predictive variables, have the greatest short interest. When we examine changes in short interest during the 3-years leading up to expected return measurement, we find that the firms with the lowest expected returns had the greatest increases in short interest. The 130 variables together explain 11% of the variation in short interest changes over this 3-year period. Short interest is a robust predictor of returns, even after controlling for the trading of the other market participants. However, once we control for the expected returns reflected in the 130 variables, the relation between short interest and returns either weakens significantly or completely disappears, depending on the specification. Thus, although the predictor variables only explain 11% of short sellers' trading, they account for almost all of short sellers' trading performance. This suggests that short sellers possess little private information and trade on public information.

Retail investors seem to make the worst trading decisions. We examine changes in retail ownership during the 3-years leading up to expected return measurement and find that stocks with the lowest expected returns have the greatest increases in retail ownership. Retail investors also decrease ownership in stocks with high expected returns. The 130 variables together explain 18% of the variation in retail trading over this 3-year period.

Retail trades predict lower returns, and we are able to reject the null hypothesis that this return-predictability is explained by the expected returns reflected in the 130 variables. Thus, at least some part of the poor trading decisions of retail investors is orthogonal to the 130 characteristics employed in this study.

Among the 6 types of institutional investors, our findings are less definitive. None of the institutional investors' trades robustly predict returns. All 6 types of institutions have the highest ownership in stocks with the lowest expected returns.[1] However, for each of the institutional investor-types the 130 variables together explain 5% or less of the variation in retail trading over the 3-year period leading up to expected return measurement. Institutional trading therefore seems to be random and not informative. This evidence adds nuance to the evidence from previous investigations of hedge fund performance. For example, although hedge funds have been found to generate gross of fee performance that contains alpha, Griffin and Xu (2009) find that hedge fund's reported equity holdings do not outperform equity holdings reported by mutual funds. Our findings confirm these findings in a later sample, that is hedge funds equity holdings are poorly positioned with respect to anomalies and fail to positively predict returns. Conversely, short selling, which is conducted predominantly by hedge funds, is well positioned with respect to anomaly

[1] We do not assume that retail holdings = 1 - 13F institutional holdings, as some earlier studies do. Our reasoning is that not all institutions file 13Fs. Non-13F filers include including some foreign institutions, nonprofits that self-manage their own funds, and institutions that manage less than $100 million.

strategies. This dichotomy mirrors the findings of Aragon and Martin (2012) and suggests that hedge funds possess greater skill when short selling or trading derivatives than when managing equity long positions.

Our paper contributes to several literatures. We find that firms are the most informed market participants. To the best of our knowledge, this has not been shown previously. Some earlier work does relate share issuance to expected returns. Baker and Wurgler (2002) find that less profitable firms, larger firms, and firms with high market-to-book ratios issue more shares. Pontiff and Woodgate (2008) and McLean, Pontiff, and Watanabe (2009) find that firms with positive stock return momentum issue more shares. Our findings broaden this investigation and show that overall, share issues tend to be aligned with expected returns. We also show that the expected returns reflected in our 130 variables cannot account for the strong return-predictability stemming from share issues, which suggests that share issuance decisions reflect managers' private information.[2]

Earlier studies find that short sellers are on the profitable side of anomaly strategies. Drake, Rees, and Swanson (2011) find that short sellers target stocks that anomaly variables suggest should be shorted. McLean and Pontiff (2016) also find that short sellers target anomaly-shorts, and further find that anomaly-shorting increases after an anomaly has been

[2] Greenwood and Hanson (2012) find that for several anomaly strategies, when the difference in net share issues between the anomaly-sells and anomaly-buys is greater (i.e., anomaly-sells' net issues – anomaly-buys' net issues), the anomaly's subsequent long-short return spread is greater.

highlighted in an academic publication. We add new insights to this literature as well. We find that short sellers build positions during the 3-year period prior to anomaly-portfolio formation, and start to exit soon after. We also find that return-predictability stemming from short interest can only be partially explained by the information in anomaly variables. Boehmer, Jones, and Zhang (2008) show that institutions account for about 75% of short-sales, while individuals account for less than 2%, so monthly changes in short interest largely reflects hedge funds. Our results therefore show that hedge funds do much better in their short positions than their long positions, both with respect to anomalies and future stock returns. We find that short sellers' return predictability stems largely from their use of public information, consistent with Engelberg, Reed, and Ringgenberg (2012), who show that publicly available news data is associated with more informed shorting.

Our paper also contributes to a growing literature on retail investors, and helps resolve a seeming paradox. Barber and Odean (2013) point out this paradox, which is that over short horizons (e.g., 1-week, up to 1-month) retail trade imbalances, typically measured at the weekly frequency, predict returns in the intended direction (see Kaniel, Saar, and Titman (2008), Barber, Odean, and Zhu (2009a), Kaniel, Saar, Liu, and Titman (2012), Kelly and Tetlock (2013), Boehmer, Jones, and Zhang (2020)), whereas over longer horizons (e.g., 1-year) retail trades predict returns opposite to the intended direction (see Odean (1999), Barber and Odean (2000), Grinblatt and Keloharju (2000), Hvidkjaer (2008), and

Barber, Odean, and Zhu (2009a and 2009b)). Our retail trading variable is different from the retail trade imbalance variable used in these earlier studies, as our variable reflects *accumulated* trades over 1-year and 3-year horizons, scaled by shares outstanding. Our 3-year variable predict lower returns, while controlling for the weekly trade imbalance. Taken together, these results show that temporary spikes in retail trading (i.e., weekly trade imbalances) predict returns in the intended direction, whereas retail trading aggregated over long horizons (our variable) predicts returns in the unintended direction. We also find that predictability from both retail trading variables cannot be explained by the 130 predictors.

With respect to institutions and stock return anomalies, Edelen, Ince, and Kadlec (2016) suggest that institutions may contribute to anomalies, as they find that in the year prior to portfolio formation, institutional demand is typically on the *wrong* side of 7 anomaly strategies. We broaden the analysis to 130 anomalies, and also find that institutions' portfolios tend to be weighted against anomalies, although our conclusion is that anomaly-characteristics are not very important in explaining institutions' trading decisions and performance. Calluzzo, Moneta, and Topaloglu (2019) use a sample of 14 anomaly strategies, and find that some institutions, mainly hedge funds, follow anomaly strategies post-portfolio formation in their long positions, but only after an anomaly is highlighted in an academic publication. This result helps explain McLean and Pontiff's (2016) post-publication decay in anomaly returns. We don't find evidence of hedge funds trading with our 130 anomalies, although we don't focus on publication dates like Calluzzo, Moneta, and Topaloglu (2019).

3.1. Sample and Data

3.1.1 Trading Overview

Our trading measures are calculated over frequencies of 1-quarter, 1-year, and 3-years. Our trading measures reflect changes in ownership over each horizon. The participants we consider are retail investors, firms, short sellers, and 6 types of institutions that report their holdings on form 13F. Given that our variables are constructed over horizons of 1 quarter or longer, they do not reveal potentially informed intra-quarter trading such as in Puckett and Yan (2011) and Kacpercyzk, Sialm, and Zhang (2008). Derivative holdings can also be an avenue for informed trading (Aragon and Martin, 2012), and these are also not reflected in our trading variables.

3.1.2 Retail Trading

We estimate retail trading via the methodology developed in Boehmer, Jones, Zhang, and Zhang (2021), which identifies marketable orders originating from retail investors. Boehmer et al. (2021) show that due to the modern characteristics of market structure and rules of Regulation NMS (National Market System), one can identify retail orders based on the sub-penny pricing of the execution. Retail marketable buy orders are likely to be internalized and receive sub-penny price improvement such that the trade price falls slightly below a whole cent. Conversely, retail marketable sell orders are likely to be internalized and receive sub-penny price improvement such that the trade price falls slightly above the whole cent. Thus, as outlined by Boehmer et al. (2021), we calculate the fraction of the penny associated with the transaction price: $Z_{it} \equiv 100 * \text{mod}(P_{it}, 0.01)$ where P_{it} is the transaction price in the stock. Trades reported to FINRA TRF (exchange code 'D') with a Z_{it} in the range of (0.6, 1) are identified as buys by retail traders. Similarly, trades reported to FINRA TRF with a Z_{it} in the range of (0, 0.4) are identified as sells by retail traders. Consistent with Boehmer et al. (2020), we do not identify trades with Z_{it} in the range of (0.4,0.6) as retail trades, since some advanced order types, such as pegged orders, can result in transaction prices at or near half pennies that do not involve retail traders.[3]

We diverge from Boehmer et al. (2021) in how we aggregate buys and sells from retail traders to form our retail trading measure, although we do some tests with their weekly trade imbalance measure. We calculate the daily percent of equity purchased by retail traders as (retail buys – retail sells) / shares outstanding as reported by CRSP. We

[3] To our knowledge, this retail measure is the only viable retail measure that can be constructed from commercially available data. Hvidkjaer (2008) proposes a measure based on trade size, but this method is no longer viable since the proliferation of market fragmentation and algorithmic trading prevents the identification of the original order size.

then aggregate this measure to periods ranging from 3 months to 3 years. We choose to scale net retail buying volume by shares outstanding because we believe a measure of the percent of equity purchased by retailers will act as a better proxy for how much investors overweight or underweight stocks, and thus their exposure to the anomaly portfolios that we include in this study. This scaling also facilitates direct comparisons to our other trading measures (describe below), which are also scaled by shares outstanding.

In order to construct our retail trading variable, we require that for every month during the relevant period, the stock must have at least one retail-initiated trade. This ensures that the stock was actively traded and was not newly listed or temporarily delisted. The identification of retail trade relies on Regulation NMS, so we restrict our sample to the period of October 2006 through December 2017. We find the share of identified retail initiated trades rises beginning in October 2006. We exclude stocks with prices under $1, measured one month before the anomaly portfolios are constructed. Such low-priced stocks are often excluded in anomaly studies. Lastly, we restrict our sample to common stock with share code 10 or 11 and listed on the NYSE, NYSE MKT (formerly Amex), or NASDAQ.

Retail limit orders are not internalized. There also may be retail market orders that are not internalized. As such, we are aggregating a subset of the population of retail trades, and the resulting variable may be nosier than the institutional trading variables. That stated, Boehmer et al. (2021) validate this methodology using actual retail trade data from both Kelley and Tetlock (2013) and NASDAQ, and find that this retail trading estimate is highly correlated with actual retail trades.

From 2016 to September 2018 the SEC enacted a tick size pilot program that affected tick sizes for some, but not all listings. Stock listings in the program are divided into one control group and three test groups. Participation in the control group and one of the test groups does not affect our retail trading measure. Participation in two of the test groups will sometimes (but not always) affect our measure. During the tick pilot program subsample this affects 16.4% of our firm-month observations, and during our entire sample it affects 1.7% of our firm-month observations. Miscalculation induces an errors-in-variables problem, so data from the tick program will produce test statistics that are biased towards not rejecting the null. In untabulated results we find that omitting data from the pilot program time period does not affect our retail trading results.

Table 1 shows that our 1-year and 3-year lagged trading measures have mean values of 0.03% and 0.05%, respectively. This is sensible, as retail investors accumulate some stocks, and sell others, so on average retail trading is close to zero. Similarly, our 3-month trading measure has a mean of 0.00%.

3.1.3. Institutional Trading

We obtain institutional holdings data from quarterly SEC 13F and S12 data, and use these data to estimate our trading variables. Not all institutions file 13F. U.S. institutions that manage less than $100 million in 13F securities are not required to file form 13F. Foreign institutions are only required to file 13F if they both pass the $100 million threshold and "use any means or instrumentality of United States interstate commerce in the course of their business."[4] French (2008) reports that according to Fed Flow of Funds data, foreign institutions own 16.3% of U.S. equities, while 13Fs reflect foreign institutional ownership of 7.6%, so the majority of foreign institutional holdings are not reflected in 13Fs. Non-profits that self-direct their portfolios also do not have file 13F. Some institutions apply for SEC exemption from disclosing some profitable positions (Agarwal, Jiang, Tang, and Yang, 2013, and Aragon, Hertzel, and Shi, 2013), so these positions are also not reflected in 13F. For these reasons, we do not assume that 1 – 13F holdings is equal to retail holdings.

We estimate mutual fund, bank, insurance, wealth management, hedge fund, and "other" (unclassified) institutional trading using changes in institutional holdings reported in 13F filings.[5] We utilize 13F filings documented by Thomson Reuters and supplement them with SEC 13F filings in order to correct known issues with Thomson Reuters data in the later parts of our sample. We use the following methods to classify institutions into one of six types:

[4] See the rule here: www.sec.gov/divisions/investment/13ffaq.htm

[5] Bushee (1998) and Cella, Ellul, and Giannetti (2013) bifurcate 13F data into 9 subgroups. Since we include 3 non-13F participants, our decision to focus on six 13F groups is intended to improve exposition.

- To identify mutual fund institutions, we merge mutual fund holdings reported in S12 filings and documented by Thomson Reuters with 13F filings. We classify the number of shares reported by mutual funds as shares held by mutual fund institutions.
- We identify banks and insurance companies using type codes provided by Brain Bushee.[6] The holdings that are denoted as bank holdings are typically from trust accounts that are managed by a financial advisor.
- If an institution is not a bank, insurance company, and does not have any mutual funds, we then classify them as either a wealth management or a hedge funds using text criteria based on institution names.[7]
- Any remaining institutions are classified as "Other" institutions.

Regarding holdings classified as "Other," these holdings appear to be directed by large investment banks that do not have commercial banking operations. We expect that most of these shares are held in separate accounts or collective investment trust (CIT). Separate accounts are non-comingled managed accounts whereas CITs are commingled. CITs have the appearance of a mutual fund, and are often used in workplace retirement plans. Some of the holdings classified as "Other" may reflect proprietary trading.

---

[6] Brian Bushee's classification schem can be found here: http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html

[7] In order to identify wealth managements, we perform case insensitive searches for "Wealth Manag", "Wealth MGNT", "Private", "PRVT" and "advisor." We then perform case insensitive searches for the remaining institutions "LLC", "L.L.C." "L L C", "L. L. C.", "LP", "L.P", "L P", "L. P", or "Partner" to identify hedge funds.

A visual inspection of the institutions classified as *Hedge Funds*, *Wealth Managers*, and *Other*, affirms that our textual classification does a reasonable job. We have also experimented with classifications based on hedge fund lists and hedge fund databases, such that the designation as a hedge fund occurs before our sample starts.[8] These exercises produce very similar results. All of the methods that we investigated avoid designating a firm a hedge fund based on a future list or database. List or database inclusion is likely to follow good performance. Our decision avoids a look-ahead bias that overstates the ability of hedge funds to accumulate positions in well-performing stocks.

To estimate the institutional trading of each firm, we scale the aggregated shares held by each institution type by the number of shares outstanding. We then calculate the change in the percentage of shares outstanding held by each type of institution, over periods of 3-months, 1-year and 3-years, the same horizons as our retail trading variables.

3.1.4. Short Sellers

Stocks exchanges report end-of-month short interest. We retrieve this information from Compustat. As we previously note, Boehmer, Jones, and Zhang (2008) document that

[8] Specifically, we utilize two different hedge fund classifications from prior literature. First, we use the hedge fund identification scheme of Cella, Ellul, and Giannetti (2013). Secondly, we use the identification scheme of Agarwal, Fos and Jiang (2013). They manually identify the universe of hedge funds that had made 13F filings as of 2008 so as to mitigate selection biases of self-reporting hedge funds. We also attempted to augment the Agarwal, Fos and Jiang (2013) hedge fund list with text-based logic to identify hedge funds that first file 13Fs later than 2008. In all of these cases, our results with respect to hedge funds do not materially change.

the majority of short positions are held by hedge funds. We calculate *Short Seller Trading* as changes in short interest scaled by shares outstanding.[9] We sign this variable such that increases in short interest result in negative values of *Short Seller Trading* and decreases in short interest (net closing of short positions) result in positive values of *Short Seller Trading.* Table 1 shows that the mean of the 3-month, 1-year and 3-year *Short Seller Trading* variables are -0.03%, -0.18% and -0.49% respectively. Thus, in our sample, aggregate short interest increased.

3.1.5. Firm Trading

Firm trading is measured as the percentage change in the firm's shares outstanding (adjusted for splits and stock dividends). This follows the method in Pontiff and Woodgate (2008) and McLean, Pontiff, and Watanabe (2009). We scale the change in shares (share issues minus share repurchases) by shares outstanding, and sign this variable such that positive values of *Firm Trading* indicate a reduction in shares outstanding, i.e., a firm buying back its shares. We create this variable each month using the CRSP reported shares outstanding adjusted for splits and stock dividends. Similar to our institutional trading

[9] For the *Short Seller Trading* measure, we utilize shares outstanding as reported by Compustat. After auditing, we believe the Compustat reported shares outstanding better aligns with Compustat short interest data and thus results in less errors due to stock splits than a measure reliant on CRSP data. For all other trading measures, including *Firm Trading*, which most directly relies upon shares outstanding, we utilize CRSP reported shares outstanding.

variables, shares outstanding data may only substantively update on a quarterly basis, when firms release financial reports regarding the completion of share repurchases. Table 1 shows that the mean of 3-month, 1-year and 3-year *Firm Trading* variables are -0.87%, -3.92% and -11.40% respectively. Thus, in our sample, the *average* firm issued more shares than it repurchased (although larger firms may have been net repurchasers, as has been reported in the media).

3.1.6. Trading Among the Market Participants

Some readers ask whether our 9 participants encompass virtually all participants. If this were the case, then an adding constraint yields one of the trading groups redundant. As we explain earlier, this is not the case, as non-profits, most foreign institutions, and other exempted institutions do not report their holdings on form 13F, and the holdings of these participants can be substantial.

Panel B of Table 1 reports average cross-sectional correlations among the various trading variables. The trading variables are each measured over a 3-year period. The first column shows that the correlations between retail investors and the other investors are negative, telling us that retail investors tend to trade against the other market participants. The retail correlations are strongest with firms and short sellers, as these correlations are -0.33 and -0.19, respectively.

Short sellers also trade against the other market participants. The correlations are especially strong with mutual funds, banks, hedge funds, and other institutional investors, ranging from -0.11 to -0.23. The correlation between short sellers and firms is only 0.01, so these two participants do not trade against each other. As discussed above, the correlation between firms and retail traders are particularly strong, with a value of -0.33. The correlation between firms and institutions are weak and generally negative, ranging from -0.07 to- 0.03. This negative correlation between institutions and firms is consistent with Ince and Kadlec (2020), who find that share issues and repurchases are an increasingly important counterparty to 13F institutions' trades.

Panel C of Table 1 presents quarterly trading autocorrelations. Most participant's exhibit negative autocorrelation, thus more buying is typically followed by less buying or selling. The biggest exceptions are retail investors and firms who show quarter-to-quarter persistence of 0.25 and 0.15, respectively.

3.1.7. Stock Return Anomalies

We use a sample of 130 stock return anomalies that are documented in published academic studies. This builds on the 97-anomaly sample used in McLean and Pontiff (2016) and Engelberg, McLean and Pontiff (2018) and the 125-anomaly sample used in Engelberg, McLean and Pontiff (2020). All of the anomaly variables are constructed with data from

CRSP, Compustat, and IBES. We exclude anomalies based on institutional investors, short sellers, and share issues and repurchases.

To create the anomaly variables, stocks are sorted each month on each of the anomaly-characteristics. We define the long and short side of each anomaly strategy as the extreme quintiles produced by the sorts. Some of our anomalies are indicator variables (e.g, credit rating downgrades). For these cases, there is only a long or short side, based on the binary value of the indicator. We remake the anomaly portfolios each month.

Like Engelberg, McLean, and Pontiff (2018 and 2020), we create an anomaly index *Net*, which is the difference between the number of long and short anomaly portfolios that a stock belongs to in a given month. As an example, a *Net* value of 10 in month t means that a stock belongs to 10 more anomaly-long portfolios than anomaly-short portfolios in month t. Table 1 shows that in our sample, *Net* has a mean value of -1.30, and a standard deviation of 8.90.

In Table 2, we sort stocks each month on *Net* into quintiles. We report the average *Net* values for each quintile at time t, and for each of the three years before and after time t. One takeaway from Table 2 is that most of the heterogeneity described by the net measure resides in the extreme quintiles. Moving from the low to high *Net* quintiles, the average *Net* values are -10.3, -0.7, 1.0, 1.6, and 8.5. So, there is not much difference in *Net* values among quintiles 2, 3, and 4, but a large difference, of 18.8, between quintiles 1 and 5.

Table 2 also shows that *Net* is highly persistent in all of the quintiles. In the low *Net* quintiles, the average *Net* values are -8.5, -8.9, -9.2, and -10.3, for times *t-3*, *t-2*, *t-1*, and *t*, and then -9.2, -8.9, and -8.6, for times *t+1*, *t+2*, and *t+3*. For the high *Net* quintiles, the average *Net* values are 6.6, 6.9, 7.3, and 8.5, for times *t-3*, *t-2*, *t-1*, and *t*, and then 7.3, 7.0, and 6.7, for times *t+1*, *t+2*, and *t+3*. The three middle quintiles show persistence as well.

3.2. Main Findings

3.2.1 Trading Prior to Anomaly Portfolio Formation

In this section of the paper we ask how each market participant trades prior to stocks being assigned to anomaly portfolios. If a stock is an anomaly-buy (or anomaly-sell) at time *t*, the time of portfolio formation, which participants increase or decrease their ownership of the stock prior to time *t*? We answer this question in Table 3. Panel A studies trading 1 year prior to time *t*, whereas Panel B studies trading 3 years prior to time *t*. As we explain in the previous section, the trading variables are changes in ownership scaled by shares outstanding, i.e., buys minus sells scaled by shares outstanding. In Panel C, we consider the weekly trade imbalance measure from Boehmer et al. (2021). This variable is measured as buys minus sells divided by buys plus sells, all measured over the 5 trading days preceding time *t*.

The findings in Table 3 show that retail investors and the long-side of hedge funds tend to do the worst with respect to anomalies, as both build positions in eventual anomaly-shorts and reduce holdings in eventual anomaly-longs. Short sellers do the best; they increase short interest in the eventual anomaly-shorts and reduce short interest in eventual anomaly-longs. Firms are net issuers of all types of stock, however firms that are anomaly-shorts issue the most shares. Note that firms are not like the other trading groups, as they may need to raise capital to operate. The other institutions are a mixed bag. None of them consistently get things right. Insurance companies do the best, building positions in longs and reducing their positions in shorts. Overall, the results here suggest that firms and short sellers are the smart money.

Examining the results in more detail, Panel A shows that, in the year prior to anomaly portfolio formation, retail investors' value in the anomaly-short portfolio is 0.10%, whereas the value in the anomaly-long portfolio is -0.02%. The difference between these two values is statistically significant. Hedge funds also accumulate shorts and sell anomaly-longs. For hedge funds, the trading values in the anomaly-short and anomaly-long portfolios are 0.17% and -0.24%, respectively. Similarly, insurance companies buy anomaly-shorts and do not have any change in their holdings of anomaly-longs.

Other institutional investors accumulate both anomaly-longs and anomaly-shorts, but they accumulate more of the shorts. The trading values for other institutional investors are 1.35% and 1.31% for the anomaly-longs and anomaly-shorts, respectively. Mutual funds reduce their holdings in both anomaly-longs and anomaly-shorts; however, they sell the longs more. The values in the anomaly-long and anomaly-short portfolios for mutual funds are -0.14% and -0.19%, respectively. Wealth managers and banks are relatively neutral, their trading does not go with or against anomalies in a noticeable way.

Short sellers increase short interest in anomaly-shorts and reduce short interest in anomaly-longs. The values are -0.47% and 0.11% in the anomaly-short and anomaly-long portfolios, respectively. Firms are net issuers of shares in all five of the portfolios, however firms that are anomaly-shorts issue more shares than do firms that are anomaly-longs. Net share issuers are equal to -4.68% for anomaly-shorts and -3.39% for anomaly-longs.

Panel B examines the 3-year trading measures. The same patterns emerge as in Panel A. The differences in Panel B are, in most cases, larger than the differences in Panel A, showing that the associated trading patterns persisted for more than one year. If the patterns were of the same magnitude as in Panel A, then we could attribute all of the trading to trading in the final year before portfolio formation. However, the stronger patterns in Panel B suggest consistent trading for more than one year.

Panel B further confirms that retail investors buy anomaly-shorts and sell-anomaly-longs. The short and long values are 0.22% and -0.05%, respectively, for retail investors. Mutual funds sell all stocks in all five quintiles, however they sell more than three times as much long as shorts. The mutual fund trading values are -0.23% and -0.77% for the anomaly-shorts and anomaly-longs, respectively. Banks display a similar pattern to mutual funds, with trading values of -0.54% and -0.84% in anomaly-shorts and anomaly-longs. Other institutional investors accumulate stocks in all 5 quintiles, however they accumulate more anomaly-shorts than anomaly-longs, as the values are 5.42% and 3.11% for the short and long portfolios. Wealth managers now buy slightly more anomaly-shorts than anomaly-longs, while insurance companies sell slightly more shorts than longs.

Like in Panel A, hedge funds trade against anomalies at the 3-year horizon. Hedge funds buy both anomaly-shorts and anomaly-longs, however they buy more shorts than longs. The values are 0.75% and 0.03% in the short and long portfolios, respectively. Brunnermeier and Nagel (2004) document that hedge funds were overexposed to internet glamour stocks during the internet bubble and then reduced their positions before the bubble burst. If this apparent ability to time mispricing extends more generally, we would expect hedge funds to increase ownership in anomaly-longs and decrease ownership in anomaly-shorts, yet we do not observe this.

Short sellers increase short interest in shorts and reduce it in longs. The values are 0.28% and -1.26% for the longs and shorts respectively. Firms are net issuers across all five of the quintiles, however firms that are anomaly-shorts issue more shares than do firms that are longs. Firms that are shorts issue shares equal to 13.86% of shares outstanding, while firms that are longs issue 9.81%. For both firms and short sellers, the magnitudes are larger in Panel B than in Panel A, suggesting that these trading patterns were persistent over the entire 3-year period.

Panel C reports finding using the weekly trade imbalance measure for retail investors. We study this variable so that we can better compare our findings to those in Boehmer et al. (2021). Panel C shows that the weekly trade imbalances are negative in all five quintiles. The negative trade imbalance is significantly higher in the anomaly-buy quintile as compared to the anomaly-short quintile. Thus, both the trade imbalance measure and the longer-term trading measure that we develop point towards retail investors trading against, or at least not conditioning on, the information in anomaly variables. These findings also show that the positive relation between the weekly trade imbalance and subsequent stock returns, as documented in Boehmer et al. (2021), is not the result of retail investors trading on anomalies or using information that is reflected in anomaly variables. Instead, whatever information retail investors use seems to be orthogonal to the information reflected in anomalies.

To assess robustness, in untabulated results we re-estimate Table 3 by grouping firms in quintile with an alternative to *Net.* Whereas *Net* is a simple count of anomaly exposure, the alternative is the predicted value from a panel regression of returns on 130 predictors and time fixed effects that utilizes five years of previous data to forecast next period's return. This new estimation produces results that are broadly consistent with Table 3. Of the 11 significant high-low differences in Table 3, 9 remain significant with the same sign. Two findings become insignificant: one-year insurance company trading and the weekly order imbalance. The new estimation also produces three significant t-statistics that are insignificant in Table 3—both one and three-year bank trading are negative and significant, as is three-year mutual fund trading.

3.2.3. Regression Evidence with Individual Predictors

In Table 4 we continue to study how each market participant trades prior to stocks being assigned to anomaly portfolios. In Table 4 we estimate firm-level regressions, where the trading variable is the dependent variable and the 130 predictors are the independent variables along with time fixed effects. The table reports the within-effects R^2 for each regression, or the percentage of cross-sectional variation in trading that is explained by the 130 predictors.

As we explain earlier, the monthly anomaly variables are indicators equal to 1 if the stock is in the long-side portfolio, -1 if the stock is in the short side portfolio, and zero otherwise. To create the portfolios stocks are sorted each month on each of the anomaly-characteristics and the long and short side of each anomaly strategy are the extreme quintiles produced by the sorts. Some of our anomalies are indicator variables (e.g., credit rating downgrades), so for these variables there is only a long side or short side.

In Panel A of Table 4 the dependent variable is trading measured over the last year. The findings show that future anomaly indicators explain a significant amount of the trading for firms and retail investors. The within-effect R^2 is 11.51% for retail investors and 21.99% for firms (share issuance). For short sellers, the statistic is 3.83%. Table 3 shows that retail investors trade against the predictors, whereas firms and short sellers trade with the predictors. The results here show that these cross-sectional trading decisions are largely idiosyncratic. Similarly, the R^2 from regressions of stock returns on the indicators are typically under 10%, i.e., stock returns are mostly idiosyncratic.

For the 6 different institutional trading variables the R^2 range from 2.10% for other institutional investors to 0.27% for wealth managers. The results therefore show that future anomaly indicators are far more important in explaining trading decisions for firms and retail investors, and to a lesser extent short sellers, as compared to institutional investors.

Panel B reports the results using trading over the last 3 years. The R^2 statistics are larger as compared to those in Panel A. For firms, the R^2 statistic is 32.22%, showing that a significant amount of share issuance reflects future anomaly indicators. For retail investors, the R^2 is 18.10%, which is also a sizeable effect. For short sellers, the R^2 is 11.35%. Table 3 showed that firms and short sellers were trading in a manner that was aligned with anomaly variables, whereas retail investors were not. The results here give a better idea of the economic significance of that result.

The R^2 for the institutional investors are larger as compared to those in Panel A, but still 5% or under in all cases. The highest is for banks, 5.08%, and the lowest is for wealth managers, 0.60%. As with Panel A, we conclude that institutional trades are largely idiosyncratic and mostly unrelated to the cross-section of expected returns.

Panel C reports the results for the weekly trade imbalance variable. The R^2 is only 0.44%, so these retail trading surges are unrelated to the universe of documented cross-sectional predictors.

3.2.4 Portfolio Holdings

In this section of the paper we study the portfolio holdings of the various market participants. We observe holdings for institutions and short sellers, but not retail investors. Holdings by firms as measured by total shares outstanding also lacks a clear economic meaning and is thus not reported. To perform our holdings analyses, we sort firms into quintiles based on *Net*, and then tabulate the percentage of shares outstanding held by each market participant. Overall, the findings show that only short sellers are well-positioned with respect to anomalies, whereas all 6 types of institutions are positioned against anomalies. Although we do not control for firm size, in earlier drafts we report holdings regressions where we control for price and size, and the findings are the same, i.e., institutions hold more anomaly-shorts than anomaly-longs.

The first row of Table 5 shows that mutual funds own on average 13.9% of shares in anomaly-shorts and 7.7% of shares in anomaly-longs, so mutual funds' holdings contradict anomaly strategies. Similarly, banks own 8% of shares outstanding in the shorts and 4% in the longs, hedge funds own 7.7% of the shorts and 5.8% of the longs respectively, while "other" or unclassified institutional investors own 39.4% of the shorts and 26.2% of the longs. Insurance companies and wealth managers have smaller holdings, but both own significantly less shorts than longs.

Short interest averages 6.4% in anomaly-shorts and 2.7% in anomaly-longs. This is consistent with the findings in the earlier tables, where short sellers are shown to sell anomaly-shorts and buy anomaly-longs. Hence, short sellers are positioned to take advantage of anomaly strategies, whereas institutions do the opposite. As we mention in the Introduction, it is likely that most short positions are held by hedge funds. Interestingly, we see here that hedge funds do not position themselves correctly with respect to anomalies on the long-side.

These results lend support to the view that the long-run accumulated trade variable that we use in Table 5 is a decent proxy for a trader being over- or underweight in anomaly long and shorts. The differences between holdings are similar to the differences in the three-year measures. The signs correspond for all participants except for insurance companies. Overall, these results tell the same story as the earlier tables—institutional investors tend to be on the wrong side of anomaly strategies, while short sellers are on the right side

3.2.5 Trading After Anomaly Portfolio Formation

In Table 3, we examine trading during the 1-year and 3-years *prior to* anomaly portfolio assignment. In Table 5, we study holdings at the time of anomaly portfolio assignment. In Table 6, we study trading over the 3-months *subsequent* to anomaly portfolio assignment. That is, we study how the various market participants trade with respect to observable anomaly variables, e.g., do retail investors buy stocks that are currently anomaly-longs and sell stocks that are currently anomaly-shorts?

Most anomaly strategies are shown to predict returns from periods ranging from 1 month to 12 months. Our *Net* variable is designed to predict returns over the subsequent month, but it does predict returns over the next 12 months (not reported in tables). Hence, it makes sense to buy high *Net* stocks and sell low *Net* stocks over the measurement period that we study here, which is the 3 months subsequent to portfolio assignment.

Table 6 shows that after the time of portfolio formation, retail investors continue their tendency to buy anomaly-shorts and sell anomaly-longs. The values for retail trading are 0.00% and -0.01% for the anomaly-long and anomaly-short portfolios, respectively, with a t-statistic of 2.1.

Short sellers now reduce short interest in anomaly-shorts. They increase short interest in most of the other quintiles, but reduce it in anomaly-shorts. Taken together with the results in Tables 3 and 5, the results here show that short sellers begin to exit their anomaly positions, perhaps too quickly, as anomaly-shorts do have low returns over this period. This concurs with recent findings based on hedge fund trade-level data (Beschwitz, Lunghi, and Schmidt, 2021). However, the reduction in short interest here is small compared to the short interest reported in Table 5, so this is a slow exit. Firms are net issuers across all 5 quintiles, but more so the anomaly-shorts, so firms continue to trade in agreement with predicted returns.

Institutional trading is largely the same as before. Insurance companies trade in the direction of expected returns. Hedge funds trade opposite of expected returns. The other institutions do not trade significantly in one way or the other.

Panel B studies the retail weekly trade imbalance measure. Here again, retail investors trade opposite to expected returns. The trade imbalance is negative in all five quintiles, however the selling is greatest in the quintile with highest expected returns, and lowest in the quintile with lowest expected returns. The results here again suggest that the information that generates the impressive return-predictability documented in Boehmer et al. (2021) is not reflected in anomaly variables.

3.2.6. Regression Evidence with Individual Predictors

In Table 7 we continue to study how each market participant's trades reflect lagged anomaly variables. Like in Table 4, we estimate firm-level regressions, where the trading variables are the dependent variables and the 130 predictors are the independent variables along with time fixed effects. We report within-effects R^2 for each regression, or the percentage of cross-sectional variation in trading that is explained by the 130 predictors.

As in Table 4, The R^2 statistics are greatest for firms and retail investors. For firms the statistic is 9.21% and for retail investors it is 2.15%. The statistics here are smaller than those reported in Table 4, which measured trading over the 1-year and 3-year periods prior to anomaly portfolio assignment. The results still suggest that the characteristics can help explain firms' share issuance decisions. For institutional investors the R^2 are all under 1%, and the R^2 is also under 1% for the weekly trade imbalance variable, reported in Panel B.

3.2.7 Predicting Stock Returns

In this section of the paper we study how retail, institutional, short seller, and firm trading predicts stock returns. Earlier studies show that firm trading (repurchases minus issues) predicts higher returns (e.g., Pontiff and Woodgate (2006) and McLean, Pontiff, and Watanabe (2009)). Earlier studies also show that over long-horizons, increases in institutional ownership forecast lower returns (see Gutierrez and Kelly (2009), Dasgupta, Prat, and Verado (2011), and Edelen, Ince, and Kadlec (2016)). Papers by Dechow et al. (2001) and Duan, Hu, and McLean (2009) show that high levels of short interest portend low returns. As we mention in the Introduction, several papers show that weekly retail-trade imbalances, which are measured as buys minus sells scaled by buys plus sells, predict returns in the intended direction over short horizons (e.g., 1-month or less). We therefore include weekly retail-trade imbalances in our regressions.

Table 8 reports our findings for the 1-year trading variables. The trading variables are measured over months *t-11* through *t*, while price and size (used a controls) are measured at time *t*. The weekly trade imbalance is measured during the last week of month *t*. The dependent variable is the monthly stock return in month *t+1* expressed in basis points.

The results show that the effects of each variable on stock returns are fairly independent of one another, as the coefficients are mostly similar in the univariate and multivariate specifications.

The first 11 regressions are univariate with *Net* and each trading variable tested independently. Consistent with earlier studies, the coefficients for *Net*, the weekly trade imbalance, firm trading, and short seller trading are all positive and significant. New to the literature, the coefficient for bank trading is negative and significant. The coefficients for the other institutions are insignificant.

The regressions reported in the last two columns include *Net* and all of the variables, with the regression in the final column also controlling for price and size. In both of these regressions, the coefficients for *Net*, the weekly trade imbalance, firm trading, and short seller trading are all positive and significant, while the coefficient for banks is negative and significant. In the final specification the coefficient for retail trading is negative, and at the borderline for significance.

With respect to economic significance, in the regression reported in the final column, the coefficient for the weekly trade imbalance is 59.23 (t-statistic = 8.95). The weekly trade imbalance variable has a standard deviation of 0.35 so a one standard deviation increase in retail trading leads to a decrease in monthly returns of 21 basis points, which is a meaningful effect. The coefficient for the firm trading variable is 172.05, so a one standard deviation increase in the firm trading variable implies a monthly return that is higher by 23 basis points.

The short selling coefficient shows an increase in monthly return of 13 basis points, per standard deviation increase. A one standard deviation increase in *Net* yields an increase in monthly return of 23 basis points. Most of the anomaly variables used in *Net* are post-publication (our sample begins in October of 2006), and McLean and Pontiff (2016) find that anomaly predictability is about half as large post-publication.

The coefficient for bank trading in the final specification is -459.37. A one standard deviation increase in bank trading yields a decrease in subsequent monthly return of about 11 basis points. As we mention above, banks are the only institution to predict returns in our sample, and to the best of our knowledge such return-predictability has not been previously linked to bank trades.

Table 9 studies return-predictability with the 3-year trading variables, and produces stronger findings for several of the measures. As in Table 8, short seller trading and firm trading predict returns in the intended direction. The retail trading coefficient is now negative and significant in all specifications. Measuring retail trades over a longer horizon therefore appears to be important, as the one-year retail trading coefficient is insignificant in Table 8. In the most complete specification reported in the final column, a one standard deviation increase in retail trading reflects a 20-basis point decrease in returns.

The trades of mutual funds, banks, insurance companies, and other institutions are negative and significant in the univariate regressions, but not in the more complete regressions reported in the final two columns: all four coefficients are insignificant. Overall, the findings show that institutions' trades do not robustly predict returns.

3.2.7 Explaining Trading Return-Predictability with Anomalies

In this last table we examine whether anomaly return-predictability can explain the relation between investor trading and future stock returns. In the earlier tables, we control for anomaly predictability with the composite anomaly variable *Net*. In this table, we take the 130 anomaly variables used to create *Net*, and regress stock returns on the entire 130. We take the residual from that regression, and regress the residual on the variables used in Tables 8 and 9.

Table 10 shows that the return-predictability of retail trading, which was found to be a strong predictor in Table 9 at the 3-year horizon, is not explained by the 130 predictive variables employed in this study, which taken together reflect academia's best guess at the cross-section of expected returns. In Panel A, the 1-year retail trading coefficients are insignificant, as in Table 8. In Panel B, the 3-year retail trading coefficients are highly significant. The most complete specification reported in the final column, the retail trading coefficient has a *t*-statistic of -4.62. The underperformance of retail trades is therefore not fully explained by retail investors' tendency to trade against anomalies.

The weekly order imbalance variable remains highly significant in these specifications.

Whatever information is reflected in these trade spikes is therefore largely orthogonal to the information reflected in the anomaly variables. The findings here again suggest that whatever information retail investors possess is not reflected in our set of predictors.

The trades of short sellers are marginally significant in Panel A and insignificant in Panel B. As a comparison, short sellers' trades are significant in all of the specifications reported in Tables 8 and 9. Hence, the predictability stemming from short sellers is largely explained by the group's tendency to trade with anomaly variables. The firm trading variable is significant in the most complete specification in Panel A, however when compared to Table 8 the significance shrinks from 3-star to 2-star and coefficient shrinks by more than one-third. In Panel B, the coefficient flips sign and is negative and insignificant in the most complete specification, whereas in Table 9 the relation is positive and significant. The positive relation between firm trading and return-predictability can therefore is some part be explained by firms trading in the direction of the 130 predictive variables, but this is not the entire story. Like retail investors, firms seem to have a source of information that is orthogonal to the 130 predictive variables.

3.3. Conclusions

In the broadest study of market participation to date, we examine how the trades of retail investors, institutional investors, short sellers, and firms relate to stock return anomalies and future stock returns. We find that firms and short sellers are the smart money. Both firms and short sellers tend to trade in the direction of expected returns, as both heavily sell anomaly-shorts, but not anomaly-longs, and their trades predict returns in the intended direction. The return-predictability stemming from short sellers' trades can be explained by their tendency to trade in the direction of the predictive variables, suggesting that short sellers do not possess private information. This is also true in some part for firms, although a good part of the predictability stemming from firms' trades is orthogonal to the 130 variables used in this study, suggesting that firms possess private information.

Retail investors perform the worst. Their trades predict returns in the *unintended* direction, and they tend to buy (sell) stocks with low (high) expected returns. This is in contrast to weekly retail trade imbalances, which do predict returns in the intended direction. In both cases, the return-predictability stemming from retail investors cannot be explained by the information reflected in the 130 predictive variables.

Institutions can be described as neutral, at best. Their holdings are tilted against expected returns, meaning institutions hold more stocks with low expected returns as compared to high expected returns, although they begin to unwind these positions after the portfolio formation date. None of the six institutional types' trades robustly predict returns.

References

Agarwal, V., Fos, V. and Jiang, W., 2013, Inferring reporting-related biases in hedge fund databases from hedge fund equity holdings. Management Science 59(6), 1271-1289.

Agarwal, Vikas, Wei Jiang, Yuehua Tang and Baozhong Yang, 2013, Uncovering Hedge Fund Skill from the Portfolio Holdings They Hide, Journal of Finance 68, 739-783.

Aragon, George, Michael Hertzel, and Zhen Shi, 2013, Why do hedge funds avoid disclosure? Evidence from confidential 13F filings, Journal of Financial and Quantitative Analysis, 1499-1518.

Aragon, George O., and J. Spencer Martin, 2012, A unique view of hedge fund derivatives usage: Safeguard or speculation? Journal of Financial Economics 105, 436–456.

Ball, Raymond and Phillip Brown, 1968, "An Empirical Evaluation of Accounting Income Numbers", Journal of Accounting Research 6, 159-178.

Baker, Malcolm, and Jeffrey Wurgler, 2000, The equity share in new issues and aggregate stock returns, Journal of Finance 55, 2219–2257.

Baker, M. and Wurgler, J. (2002), Market Timing and Capital Structure. The Journal of Finance, 57, 1-32.

Barber, B. M., and Odean, T. (2000). Trading is hazardous to your wealth: The common stock investment performance of individual investors. Journal of Finance, 55, 773–806.

Barber, B. M., Odean, T., & Zhu, N. (2009a). Do retail trades move markets?. Review of Financial Studies, 22, 151–186.

Barber, B. M., Odean,T., & Zhu, N. (2009b). Systematic noise. Journal of Financial Markets, 12, 469–547.

Barber, Brad and Odean, Terrance, (2013), The Behavior of Individual Investors, Chapter 22, p. 1533-1570, Elsevier.

Blume, Marshal E. and Frank, Husic, 1973, "Price, beta, and exchange listing," Journal of Finance 28, 283-299.

Beschwitz, Bastian von, Sandro Lunghi, and Daniel Schmidt, 2021, Fundamental arbitrage under the microscope, Evidence from detailed hedge fund transaction data, Review of Asset Pricing Studies, forthcoming.

Boehmer, Ekkehart, Charles M. Jones, and Xioyan Zhang, 2008, Which shorts are informed?, Journal of Finance 63, 491-527.

Boehmer, Ekkehart, Charles M. Jones, and Xioyan Zhang, 2020, Tracking Retail Investor Activity, Working Paper

Brunnermeier, Markus and Stefan Nagel, 2004, Hedge funds and the technology bubble, Journal of Finance 59, 2013-2040.

Bushee, B. J. 1998. The influence of institutional investors on myopic R&D investment behavior. *Accounting Review* 73, 305–33.

Calluzzo, Paul, Fabio Moneta and Selim Topaloglu, 2019, Institutional Trading and Anomalies, *Management Science* 65, 4555-4574.

Carlson, M., A. Fisher, and R. Giammarino. 2006. Corporate Investment and Asset Price Dynamics: Implications for SEO Event Studies and Long-Run Performance. *Journal of Finance* 61, 1009–34.

Cristina Cella, Andrew Ellul, Mariassunta Giannetti, 2013, Investors' Horizons and the Amplification of Market Shocks, *The Review of Financial Studies* 26, 1607–1648,

Cochrane, J. H. 1991. Production-Based Asset Pricing and the Link Between Stock Returns and Economic Fluctuations. *Journal of Finance* 46:209–37.

Cochrane, J. H., forthcoming, Rethinking production under uncertainty, Review of Asset Pricing Studies.

Dasgupta, A., Prat, A., Verardo, M., 2011. Institutional Trade Persistence and Long-Term Equity Returns. Journal of Finance, 66, 635-653.

Daniel, Kent and Sheridan Titman, 2006, Market Reactions to Tangible and Intangible Information, Journal of Finance 61, 1605-1643.

Dechow, P., A. Hutton, L. Meulbroek, and R. Sloan, 2001. Short sellers, fundamental analysis and stock returns, Journal of Financial Economics 61, 77-106.

De Long, J Bradford, Andrei Shleifer, Lawrence Summers, and Robert Waldmann, 1990a, Noise Trader Risk in Financial Markets, Journal of Political Economy, 98, 703-38.

De Long, J Bradford, Andrei Shleifer, Lawrence Summers, and Robert Waldmann, 1990b, Positive Feedback Investment Strategies and Destabilizing Rational Speculation, Journal of Finance, 45, 379-95.

Drake, M., L. Rees and E. Swanson, 2011. Should investors follow the prophets or the bears? Evidence on the use of public information by analysts and short sellers. The Accounting Review 86: 101-130.

Duan, Ying, Gang Hu, and R. David McLean, 2009, "Costly Arbitrage and Idiosyncratic Risk: Evidence from Short Sellers," Journal of Financial Intermediation 19, 564-579.

Engelberg, Joseph, David McLean, and Jeffrey Pontiff, 2018, Anomalies and News, Journal of Finance 73, 1971-2001.

Engelberg, Joseph, David McLean, and Jeffrey Pontiff, 2020, Analysts and Anomalies, Journal of Accounting and Economics 69.

Engelberg, Joseph E., Reed, Adam and Ringgenberg, Matthew, 2012, How are shorts informed?, *Journal of Financial Economics* 105, 260-278.

Edelen, Roger, Ozgur Ince, and Gregoy Kadlec, 2016, Institutional Investors and Stock Return Anomalies, Journal of Financial Economics 119, 472-488.

Foster, G., C. Olsen, and T. Shevlin. 1984, Earnings Releases, Anomalies, and the Behavior of Security Returns, The Accounting Review, 574-603.

French, K., 2008. Presidential address: the cost of active investing. The Journal of Finance 63, 1537–1573.

John M. Griffin, Jin Xu, How Smart Are the Smart Guys? A Unique View from Hedge Fund Stock Holdings, The Review of Financial Studies, Volume 22, Issue 7, July 2009, Pages 2531–2570.

Grinblatt, M., and Keloharju, M. (2000). The investment behavior and performance of various investor types: A study of Finland's unique data set. Journal of Financial Economics, 55, 43–67.

Graham, J., & Kumar, A. (2006). Do Dividend Clienteles Exist? Evidence on Dividend Preferences of Retail Investors. *The Journal of Finance, 61*, 1305-1336.

Gutierrez, R.C., Kelley, E.K., 2009. Institutional herding and future stock returns. Working paper, University of Oregon and University of Arizona.

Hirshleifer, D., Myers, J. N., Myers, L. A., & Teoh, S. H. (2008). Do individual investors drive post-earnings announcement drift? Direct evidence from personal trades. *Accounting Review, 83*, 1150–1521.

Hvidkjaer, S. (2008). Small trades and the cross-section of stock returns. Review of Financial Studies, 21, 1123–1151.

Ince, Ozgur S. and Kadlec, Gregory B. and McKeon, Stephen B., 2018, Institutional Counterparties and Performance, Working Paper

Jegadeesh, Narasimhan and Sheridan Titman, 1993. Returns to buying winners and selling losers: Implications for stock market efficiency. Journal of Finance 48, 65–91.

Kaniel, R., Liu, S., Saar, G., & Titman, S. (2012). Individual Investor Trading and Return Patterns around Earnings Announcements. Journal of Finance, 67, 639–680.

Kaniel, R., Saar, G., & Titman, S. (2008). Individual investor trading and stock returns. Journal of Finance, 63, 273–310.

Kacperczyk, Marcin, Clemens Sialm, and Lu Zheng, 2008, Unobserved actions of mutual funds, *Review of Financial Studies* 21, 2379–2416.

Kelley, E. K., & Tetlock, P. C. 2013, How Wise Are Crowds? Insight from Retail Orders and Stock Returns. Journal of Finance 68, 1229-1265.

Lewellen, Jonathan, 2011, Institutional investors and the limits of arbitrage, Journal of Financial Economics 102, 62-80.

Loughran, Tim, and Jay Ritter, 1995, The new issues puzzle, Journal of Finance 50, 23–51

McLean, R. David, Jeffrey Pontiff, and Akiko Watanabe, 2009, "Share Issuance and Cross-Sectional Returns: International Evidence" *Journal of Finance Economics* 94, 1-17.

McLean, R. David and Jeffrey Pontiff, 2016, "Does academic research destroy stock return predictability?," *Journal of Finance* 71, 5-32.

Nosfinger, John R. and Richard W. Sias, 2002, "Herding and Feedback Trading by Institutional and Individual Investors," Journal of Finance 54, 2263-2295.

Odean, T. (1998). Volume, volatility, price, and profit when all traders are above average. *Journal of Finance, 53*, 1887–1934.

Odean,T. (1999). Do investors trade too much? American Economic Review, 89, 1279–1298.

Pontiff, Jeffrey, and Woodgate, Artemiza (2008). Share issuance and cross-sectional returns. The Journal of Finance 63 no. 2, 921-945.

Puckett, Andy and Sterling Yan, 2011, The interim trading skills of institutional investors, Journal of Finance 66, 601-633.

Sias, Richard W. and Laura T. Starks, *2006,* Changes in Institutional Ownership and Stock Returns: Assessment and Methodology, *The Journal of Business* 79, 2869-2910.

Sias, Richard & Whidbee, David, 2010, Insider Trades and Demand by Institutional and Individual Investors. *Review of Financial Studies* 23, 1544-1595.

U.S. Department of Education, 2019, National Center for Education Statistics. *Digest of Education Statistics, 2017* (NCES 2018-094), Chapter 3.

Zhang, L. 2005. The Value Premium. *Journal of Finance* 60(1), 67–103.

Table 1: Descriptive Statistics of Variables

Panel A of this table provides descriptive statistics for the variables used in the study. Panel B reports average cross-sectional correlations of our main variables of interest. Panel C reports the variables' autocorrelations. We construct the *Retail Trading* variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. We use 130 cross-sectional anomalies, which are described in the paper's appendix. At the end of each month, stocks are sorted on each anomaly characteristic (e.g., size, book-to-market, accruals). We use the extreme quintiles to define the long side and short side of each anomaly strategy. Some anomalies are indicator variables (e.g., credit rating downgrades); for these anomalies, there is only a long or short side, based on the binary value of the indicator. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Short seller Trading* and *Firm Trading* measures. For each firm-month observation, we sum the number of long-side and short-side anomaly portfolios that the firm belongs to and calculate net as the total long - short indicators. Price and size are reported as of the time of the anomaly stock sorts. Size is the CRSP reported market capitalization of common equity. *Net Residual* is the residuals from monthly returns regressed on the 130 anomaly indicator variables. These residuals represent the monthly return not explained by which anomaly portfolios an equity belongs to at the beginning of the month.

Panel A: Descriptive Statistics of Firm-Month Observations								
Variable	**Obs.**	**Mean**	**Std. Dev.**	**1st %ile**	**25th %ile**	**Median**	**75th %ile**	**99th %ile**
Retail Trading$_{t-11,t}$	435,690	0.03%	1.01%	-2.09%	-0.34%	-0.07%	0.19%	4.33%
Retail Trading$_{t-35,t}$	306,935	0.05%	2.14%	-4.00%	-0.82%	-0.22%	0.36%	10.25%
Retail Trading$_{t,t+3}$	496,370	0.00%	0.36%	-1.03%	-0.12%	-0.02%	0.07%	1.46%
Mutual Fund Trading$_{t-11,t}$	461,116	-0.09%	6.20%	-20.28%	-1.74%	0.01%	1.68%	18.85%
Mutual Fund Trading$_{t-35,t}$	415,730	-0.41%	8.77%	-26.21%	-3.81%	0.00%	3.19%	23.69%
Mutual Fund Trading$_{t,t+3}$	483,945	-0.08%	4.03%	-14.74%	-0.53%	0.00%	0.54%	13.63%
Mutual Fund Ownership$_{t}$	491,885	11.50%	10.14%	0.00%	2.52%	9.95%	17.66%	41.60%
Bank Trading$_{t-11,t}$	461,116	-0.14%	3.35%	-10.69%	-1.35%	0.00%	1.23%	9.32%
Bank Trading$_{t-35,t}$	415,730	-0.67%	5.01%	-15.15%	-3.10%	-0.19%	1.79%	12.74%
Bank Trading$_{t-35,t+3}$	483,945	-0.05%	1.77%	-6.38%	-0.36%	0.00%	0.40%	5.15%
Mutual Fund Ownership$_{t}$	491,885	6.60%	5.96%	0.00%	1.36%	5.32%	10.40%	23.81%
Insurance Company Trading$_{t-11,t}$	461,116	-0.04%	1.45%	-4.94%	-0.34%	0.00%	0.32%	4.34%
Insurance Company Trading$_{t-35,t}$	415,730	-0.17%	2.12%	-7.17%	-0.76%	0.00%	0.54%	5.81%
Insurance Company Trading$_{t,t+3}$	483,945	-0.01%	0.71%	-2.49%	-0.10%	0.00%	0.09%	2.27%
Insurance Company Ownership$_{t}$	491,885	1.82%	2.14%	0.00%	0.14%	1.24%	2.57%	9.87%
Wealth Management Trading$_{t-11,t}$	461,116	0.09%	2.02%	-6.20%	-0.21%	0.00%	0.36%	6.82%
Wealth Management Trading$_{t-35,t}$	415,730	0.23%	2.55%	-7.93%	-0.26%	0.04%	0.73%	9.15%
Wealth Management Trading$_{t,t+3}$	483,945	0.02%	1.53%	-3.93%	-0.08%	0.00%	0.12%	4.22%
Wealth Management Ownership$_{t}$	491,885	1.41%	2.33%	0.00%	0.08%	0.64%	1.63%	11.98%
Hedgefund Trading$_{t-11,t}$	461,116	0.05%	4.81%	-14.96%	-1.59%	0.00%	1.60%	15.07%
Hedgefund Trading$_{t-35,t}$	415,730	0.42%	6.80%	-19.95%	-2.26%	0.13%	3.10%	20.60%
Hedgefund Trading$_{t,t+3}$	483,945	-0.01%	2.63%	-8.99%	-0.65%	0.00%	0.59%	8.87%
Hedgefund Ownership$_{t}$	491,885	7.07%	7.30%	0.00%	1.73%	5.03%	10.00%	34.77%
Other Institutional Trading$_{t-11,t}$	461,116	1.42%	10.50%	-29.02%	-2.92%	0.71%	5.52%	33.73%
Other Institutional Trading$_{t-35,t}$	415,730	4.39%	14.56%	-38.25%	-2.57%	3.46%	11.36%	46.19%
Other Institutional Trading$_{t,t+3}$	483,945	0.29%	5.83%	-18.04%	-1.49%	0.04%	1.98%	18.84%
Other Institutional Ownership$_{t}$	491,885	33.96%	20.41%	0.04%	16.74%	35.50%	49.43%	78.24%
Short Seller Trading$_{t-11,t}$	460,155	-0.18%	3.82%	-13.35%	-1.23%	-0.01%	1.00%	11.79%
Short Seller Trading$_{t-35,t}$	411,007	-0.49%	5.39%	-18.47%	-2.09%	-0.03%	1.38%	15.79%
Short Seller Trading$_{t,t+3}$	479,604	-0.03%	2.01%	-6.98%	-0.52%	0.00%	0.53%	6.54%

Short Seller Ownership$_t$	486,892	-4.69%	5.59%	-26.94%	-6.36%	-2.77%	-0.92%	0.00%
Firm Trading$_{t\text{-}11,t}$	481,542	-3.92%	13.59%	-71.90%	-2.74%	-0.60%	0.42%	14.49%
Firm Trading$_{t\text{-}35,t}$	434,585	-11.40%	30.80%	-158.15%	-14.17%	-2.53%	2.30%	31.40%
Firm Trading$_{t,t+3}$	500,684	-0.87%	4.39%	-24.36%	-0.44%	-0.06%	0.00%	5.30%
Weekly Order Imbalance$_t$	509,369	-14.18%	34.77%	-100.00%	-31.22%	-13.16%	1.14%	100.00%
Net$_t$	509,369	-1.27	8.93	-23	-7	-1	5	20
OptimalNet$_t$	509,369	-3bp	105bp	-260bp	-69bp	-1bp	64bp	255bp
Price$_t$	509,090	$69.20	$2,685.85	$1.07	$6.65	$16.08	$33.74	$164.38
Size$_t$	509,090	$4,587,998	$20,400,000	$8,609	$119,382	$480,536	$2,053,318	$80,900,000
Return$_{t+1}$	508,659	64bp	1535bp	-3810bp	-597bp	36bp	656bp	4613bp
Net Residual$_{t+1}$	506,536	69bp	1531bp	-3749bp	-601bp	38bp	660bp	4632bp

Panel B: Average Cross-Sectional Correlations									
Variable	*Retail Trading*$_{t-35,t}$	*Mutual Fund Trading*$_{t-35,t}$	*Bank Trading*$_{t-35,t}$	*Insurance Company Trading*$_{t-35,t}$	*Wealth Management Trading*$_{t-35,t}$	*Hedge fund Trading*$_{t-35,t}$	*Other Institutional Trading*$_{t-35,t}$	*Short Seller Trading*$_{t-35,t}$	*Firm Trading*$_{t-35,t}$
Mutual Fund Trading$_{t-35,t}$	-0.04								
Bank Trading$_{t-35,t}$	0.02	0.12							
Insurance Company Trading$_{t-35,t}$	-0.01	0.09	0.14						
Wealth Management Trading$_{t-35,t}$	-0.02	-0.01	0.05	0.02					
Hedgefund Trading$_{t-35,t}$	-0.03	-0.02	0.07	0.01	0.05				
Other Institutional Trading$_{t-35,t}$	-0.08	0.16	0.11	0.11	0.07	0.08			
Short Seller Trading$_{t-35,t}$	-0.19	-0.13	-0.17	-0.09	-0.02	-0.11	-0.23		
Firm Trading$_{t-35,t}$	-0.33	-0.06	-0.06	-0.04	-0.03	-0.07	-0.06	0.01	
Net$_{t}$	-0.07	-0.03	-0.02	0.00	-0.01	-0.05	-0.07	0.12	0.08

Panel C: Quarterly Autocorrelations								
Retail Trading	*Mutual Fund Trading*	*Bank Trading*	*Insurance Company Trading*	*Wealth Management Trading*	*Hedgefund Trading*	*Other Institutional Trading*	*Short Seller Trading*	*Firm Trading*
0.25	-0.31	-0.09	-0.06	-0.32	-0.13	-0.12	-0.10	0.15

Table 2: Net Time Series by Net Anomaly Quintiles

This table reports average time series *Net* indicators for quintile sorts of *Net* anomaly indicators. For each month, quintiles are formed by sorting observations by *Net*. Due to the discrete nature of *Net*, this forms five quintiles of differing size. To create the *Net* anomaly variable, we use 130 cross-sectional anomalies, which are described in the paper's appendix. We exclude anomalies based on 13F data and share issuances since they are used for the construction of our *Institutional Trading* and *Firm Trading* measures. For each stock-month observation, we sum up the number of long-side and short-side anomaly portfolios that the stock belongs to and calculate *Net* as equal to the number of long portfolios minus number of short portfolios.

	Net_t Quintile				
Reported Variable:	**Lo**	**2**	**3**	**4**	**Hi**
Net_{t-3}	-8.5	-0.5	0.7	1.0	6.6
Net_{t-2}	-8.9	-0.6	0.7	1.1	6.9
Net_{t-1}	-9.2	-0.6	0.7	1.2	7.3
Net_t	-10.3	-0.7	1.0	1.6	8.5
Net_{t+1}	-9.2	-0.6	0.7	1.2	7.3
Net_{t+2}	-8.9	-0.6	0.7	1.1	7.0
Net_{t+3}	-8.6	-0.6	0.6	1.0	6.7

Table 3: Net Anomaly Indicators on Past Trading

This table reports average trading by various trader types over 1 year, 3 years and 1 week prior to quintile sorts of *Net* anomaly indicators. The *Retail Trading* is expressed as the percentage of common equity net purchased by retail traders during the relevant time period. We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 1 (3) years prior to the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. We use 130 cross-sectional anomalies, which are described in the paper's appendix. At the end of each month, stocks are sorted on each anomaly characteristic (e.g., size, book-to-market, accruals). We use the extreme quintiles to define the long side and short side of each anomaly strategy. Some anomalies are indicator variables (e.g., credit rating downgrades); for these anomalies, there is only a long or short side, based on the binary value of the indicator. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Shortseller Trading* and *Firm Trading* measures. For each stock-month observation, we sum up the number of long-side and short-side anomaly portfolios that the stock belongs to and calculate *Net* as equal to the number of long portfolios minus number of short portfolios. Newey-West standard errors are utilized for the t-statistics in parentheses. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Table 3 (Continued)

Panel A: Prior 1-Year Trading							
	Net_t Quintile						
Reported Variable:	Lo	2	3	4	Hi	Hi - Lo	t-stat
Retail Trading$_{t\text{-}11,t}$	0.10%	0.00%	-0.01%	-0.01%	-0.02%	-0.12%	-4.8
Mutual Fund Trading$_{t\text{-}11,t}$	-0.14%	-0.04%	0.01%	-0.02%	-0.19%	-0.06%	-0.2
Bank Trading$_{t\text{-}11,t}$	-0.14%	0.00%	-0.02%	-0.01%	-0.16%	-0.02%	-0.1
Insurance Company Trading$_{t\text{-}11,t}$	-0.08%	-0.05%	-0.13%	-0.06%	0.00%	0.08%	3.7
Wealth Management Trading$_{t\text{-}11,t}$	0.09%	0.11%	0.21%	0.24%	0.08%	-0.02%	-0.2
Hedgefund Trading$_{t\text{-}11,t}$	0.17%	-0.03%	0.06%	0.02%	-0.07%	-0.24%	-3.3
Other Institutional Trading$_{t\text{-}11,t}$	1.35%	0.75%	1.03%	0.88%	1.31%	-0.04%	-0.1
Short Seller Trading$_{t\text{-}11,t}$	-0.47%	-0.01%	0.03%	0.05%	0.11%	0.57%	4.3
Firm Trading$_{t\text{-}11,t}$	-4.68%	-3.58%	-4.08%	-3.45%	-3.39%	1.29%	6.0

Panel B: Prior 3-Year Trading							
	Net_t Quintile						
Reported Variable:	Lo	2	3	4	Hi	Hi - Lo	t-stat
Retail Trading$_{t\text{-}35,t}$	0.22%	-0.05%	-0.06%	-0.06%	-0.05%	-0.26%	-4.6
Mutual Fund Trading$_{t\text{-}35,t}$	-0.23%	-0.24%	-0.03%	-0.22%	-0.77%	-0.55%	-1.1
Bank Trading$_{t\text{-}35,t}$	-0.54%	-0.13%	0.58%	-0.08%	-0.84%	-0.30%	-0.9
Insurance Company Trading$_{t\text{-}35,t}$	-0.17%	-0.18%	-0.29%	-0.16%	-0.15%	0.03%	0.8
Wealth Management Trading$_{t\text{-}35,t}$	0.27%	0.26%	0.51%	0.43%	0.19%	-0.08%	-1.5
Hedgefund Trading$_{t\text{-}35,t}$	0.75%	0.19%	0.30%	0.00%	0.03%	-0.71%	-2.9
Other Institutional Trading$_{t\text{-}35,t}$	5.42%	2.68%	2.56%	2.81%	3.11%	-2.30%	-5.4
Short Seller Trading$_{t\text{-}35,t}$	-1.26%	-0.08%	0.30%	0.23%	0.28%	1.54%	5.4
Firm Trading$_{t\text{-}35,t}$	-13.86%	-9.82%	-10.94%	-9.65%	-9.81%	4.05%	3.5

Panel C: Prior 1 Week Trading							
	Net_t Quintile						
Reported Variable:	Lo	2	3	4	Hi	Hi - Lo	t-stat
Weekly Order Imbalance$_t$	-13.71%	-14.26%	-15.06%	-14.29%	-14.48%	-0.78%	-2.3

Table 4: Anomalies Ability to Explain Trading

This table reports within fixed effects adjusted r-squared when regressing various trader types over 1 year prior, 3 years prior, 1 quarter following, and ownership at the time of quintile sorts of *Net* anomaly indicators. For each cell, the listed variable is regressed on dummies representing the long minus short portfolios of 130 cross-sectional anomalies and time fixed effects. The anomalies are described in the paper's appendix. At the end of each month, stocks are sorted on each anomaly characteristic (e.g., size, book-to-market, accruals). We use the extreme quintiles to define the long side and short side of each anomaly strategy. Some anomalies are indicator variables (e.g., credit rating downgrades); for these anomalies, there is only a long or short side, based on the binary value of the indicator. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Shortseller Trading* and *Firm Trading* measures. The *Retail Trading* is expressed as the percentage of common equity net purchased by retail traders during the relevant time period. We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 1 (3) years prior to the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Panel A: Prior 1 Year Trading	
Reported Variable:	$\mathbf{R^2}$
Retail Trading$_{t-11,t}$	11.51%
Mutual Fund Trading$_{t-11,t}$	1.95%
Bank Trading$_{t-11,t}$	1.98%
Insurance Company Trading$_{t-11,t}$	1.66%
Wealth Management Trading$_{t-11,t}$	0.27%
Hedgefund Trading$_{t-11,t}$	0.51%
Other Institutional Trading$_{t-11,t}$	2.10%
Short Seller Trading$_{t-11,t}$	3.83%
Firm Trading$_{t-11,t}$	21.99%

Panel B: Prior 3 Year Trading	
Reported Variable:	**R^2**
Retail Trading$_{t\text{-}35,t}$	18.10%
Mutual Fund Trading$_{t\text{-}35,t}$	3.89%
Bank Trading$_{t\text{-}35,t}$	5.08%
Insurance Company Trading$_{t\text{-}35,t}$	2.14%
Wealth Management Trading$_{t\text{-}35,t}$	0.60%
Hedgefund Trading$_{t\text{-}35,t}$	1.56%
Other Institutional Trading$_{t\text{-}35,t}$	4.56%
Short Seller Trading$_{t\text{-}35,t}$	11.35%
Firm Trading$_{t\text{-}35,t}$	32.22%

Panel C: Prior 1 Week Trading	
Reported Variable:	**R^2**
Weekly Order Imbalance$_{t}$	0.44%

Table 5: Ownership by Net Anomaly Quintiles

This table reports average monthly ownership level for quintile sorts of *Net* anomaly indicators. For each month, quintiles are formed by sorting observations by *Net*. Due to the discrete nature of *Net*, this forms five quintiles of differing size. Newey-West standard errors with 12 lags are utilized for the t-statistics reported for Hi-Lo averages. Institutional ownerships reported are from 13F filings. We categorize these institutions as described in the data section. *Short Seller Ownership* is calculated as short interest divided by shares outstanding. *Short Seller Ownership* is signed to make interpretation consistent with other ownership variables. All ownership measures are winsorized at the 1% level. To create the *Net* anomaly variable, we use 130 cross-sectional anomalies, which are described in the paper's appendix. We exclude anomalies based on 13F data and share issuances since they are used for the construction of our *Institutional Trading* and *Firm Trading* measures. For each stock-month observation, we sum up the number of long-side and short-side anomaly portfolios that the stock belongs to and calculate *Net* as equal to the number of long portfolios minus number of short portfolios.

	Net_t Quintile						
Reported Variable:	**Lo**	**2**	**3**	**4**	**Hi**	**Hi - Lo**	**t-stat**
Mutual Fund Ownership$_t$	13.9%	6.1%	2.5%	6.7%	7.7%	-6.2%	-13.5
Bank Ownership$_t$	8.0%	5.3%	5.5%	6.3%	4.0%	-3.9%	-13.4
Insurance Ownership$_t$	2.2%	1.3%	1.1%	1.3%	1.1%	-1.1%	-21.3
Wealth Management Ownership$_t$	1.6%	1.5%	1.7%	1.8%	1.2%	-0.4%	-7.2
Hedge fund Ownership$_t$	7.7%	5.1%	2.9%	5.2%	5.8%	-1.9%	-14.0
Other Institutional Ownership$_t$	39.4%	26.1%	19.5%	27.9%	26.2%	-13.3%	-29.8
Short Seller Ownership$_t$	-6.4%	-3.7%	-2.1%	-2.9%	-2.7%	3.6%	23.9

Table 6: Future Trading on Net Anomaly Indicators

This table reports average trading by various trader types over 3 months after quintile sorts of *Net* anomaly indicators. We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 3 months after the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. We use 130 cross-sectional anomalies, which are described in the paper's appendix. At the end of each month, stocks are sorted on each anomaly characteristic (e.g., size, book-to-market, accruals). We use the extreme quintiles to define the long side and short side of each anomaly strategy. Some anomalies are indicator variables (e.g., credit rating downgrades); for these anomalies, there is only a long or short side, based on the binary value of the indicator. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Short Seller Trading* and *Firm Trading* measures. For each stock-month observation, we sum up the number of long-side and short-side anomaly portfolios that the stock belongs to and calculate *Net* as equal to the number of long portfolios minus number of short portfolios. Newey-West standard errors are utilized for the t-statistics in parentheses. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Panel A: Following Quarter Trading							
	Net$_t$ Quintile						
Reported Variable:	**Lo**	**2**	**3**	**4**	**Hi**	**Hi - Lo**	**t-stat**
Retail Trading$_{t,t+3}$	0.00%	-0.01%	-0.02%	-0.01%	-0.01%	-0.01%	-2.1
Mutual Fund Trading$_{t,t+3}$	-0.14%	-0.02%	-0.05%	-0.06%	-0.04%	0.10%	1.1
Bank Trading$_{t,t+3}$	-0.08%	-0.01%	0.10%	-0.04%	-0.02%	0.06%	1.2
Insurance Company Trading$_{t,t+3}$	-0.03%	-0.02%	-0.01%	-0.03%	0.00%	0.03%	3.5
Wealth Management Trading$_{t,t+3}$	0.02%	0.03%	0.08%	0.09%	0.01%	-0.01%	-0.2
Hedgefund Trading$_{t,t+3}$	0.01%	-0.02%	0.04%	0.02%	-0.04%	-0.04%	-2.1
Other Institutional Trading$_{t,t+3}$	0.18%	0.14%	0.28%	0.30%	0.33%	0.16%	1.4
Short Seller Trading$_{t,t+3}$	0.01%	-0.01%	-0.03%	-0.03%	-0.03%	-0.05%	-1.4
Firm Trading$_{t,t+3}$	-0.94%	-0.84%	-0.78%	-0.86%	-0.84%	0.10%	1.7

Panel B: Following 1 Week Trading							
	Net$_t$ Quintile						
Reported Variable:	**Lo**	**2**	**3**	**4**	**Hi**	**Hi - Lo**	**t-stat**
Weekly Order Imbalance$_{t+1}$	-2.56%	-3.13%	-4.22%	-3.60%	-3.73%	-1.17%	-3.5

Table 7: Anomalies Ability to Explain Trading

This table reports within fixed effects adjusted r-squared when regressing various trader types over 1 year prior, 3 years prior, 1 quarter following, and ownership at the time of quintile sorts of *Net* anomaly indicators. For each cell, the listed variable is regressed on dummies representing the long minus short portfolios of 130 cross-sectional anomalies and time fixed effects. The anomalies are described in the paper's appendix. At the end of each month, stocks are sorted on each anomaly characteristic (e.g., size, book-to-market, accruals). We use the extreme quintiles to define the long side and short side of each anomaly strategy. Some anomalies are indicator variables (e.g., credit rating downgrades); for these anomalies, there is only a long or short side, based on the binary value of the indicator. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Shortseller Trading* and *Firm Trading* measures. The *Retail Trading* is expressed as the percentage of common equity net purchased by retail traders during the relevant time period. We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 1 (3) years prior to the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Panel D: Following Quarter Trading	
Reported Variable:	R^2
Retail Trading$_{t,t+3}$	2.15%
Mutual Fund Trading$_{t,t+3}$	0.54%
Bank Trading$_{t,t+3}$	0.60%
Insurance Company Trading$_{t,t+3}$	0.56%
Wealth Management Trading$_{t,t+3}$	0.08%
Hedgefund Trading$_{t,t+3}$	0.15%
Other Institutional Trading$_{t,t+3}$	0.51%
Short Seller Trading$_{t,t+3}$	0.76%
Firm Trading$_{t,t+3}$	9.21%

Panel E: Following 1 Week Trading	
Reported Variable:	R^2
Weekly Order Imbalance$_{t+1}$	0.56%

Table 8: Returns Following 1-Year Trading Variables

This table reports results from a Fama-Macbeth regression of monthly stock returns on the *Net* anomaly indicator, *Retail Trading, Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading, Other Institutional Trading, Short Seller Trading* and *Firm Trading* aggregated through the 1 year prior to the month of the anomaly stock sorts, *log(Price)* at the month of the anomaly stock sorts, and *log(Size)* as measured by the log of the CRSP reported market capitalization of common equity at the month of the anomaly stock sorts. Monthly Returns are reported by CRSP and denoted as basis points. The *Retail Trading* is expressed as the percentage of common equity net purchased by retail traders during the relevant time period (.01 = 1% of common equity). We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 1 year prior to the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. To create the *Net* anomaly variable, we use 130 cross-sectional anomalies, which are described in the paper's appendix. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Short Seller Trading* and *Firm Trading* measures. For each stock-month observation, we sum up the number of long-side and short-side anomaly portfolios that the stock belongs to and calculate *Net* as equal to the number of long portfolios minus number of short portfolios. Newey-West standard errors are utilized for the t-statistics in parentheses. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Table 8 (Continued)

	Dependent Variable: $Return_{t+1}$												
Net$_t$	1.79***											2.08***	2.61***
	(2.98)											(3.42)	(3.02)
Retail Trading$_{t-11,t}$		-1644.83										-469.86	-671.91
		(-1.55)										(-0.64)	(-1.16)
Mutual Fund Trading$_{t-11,t}$			-112.10									-55.65	-34.44
			(-1.18)									(-0.68)	(-0.46)
Bank Trading$_{t-11,t}$				-538.19**								-403.04**	-335.28**
				(-2.60)								(-2.18)	(-2.16)
Insurance Company Trading$_{t-11,t}$					-505.37							-355.97	-336.35
					(-1.54)							(-1.25)	(-1.27)
Wealth Management Trading$_{t-11,t}$						-29.58						50.94	74.87
						(-0.12)						(0.22)	(0.32)
Hedge fund Trading$_{t-11,t}$							31.84					27.58	70.08
							(0.31)					(0.26)	(0.84)
Other Institutional Trading$_{t-11,t}$								-68.25				-25.57	-20.23
								(-0.67)				(-0.26)	(-0.27)
Short Seller Trading$_{t-11,t}$									482.65***			329.41***	332.20***
									(3.38)			(2.76)	(2.95)
Firm Trading$_{t-11,t}$										224.29***		179.89***	172.05***
										(3.96)		(3.20)	(3.45)
Weekly Order Imbalance$_t$											57.20***	59.75***	59.23***
											(7.85)	(8.05)	(8.59)
log(Size$_t$*)*													10.12*
													(1.94)
log(Price$_t$*)*													-10.91
													(-0.48)
Constant	76.04	81.44	76.98	82.70	82.20	79.93	80.89	77.89	83.49	87.16	83.33	94.09	-14.62
	(1.35)	(1.36)	(1.39)	(1.48)	(1.48)	(1.43)	(1.47)	(1.41)	(1.53)	(1.59)	(1.61)	(1.64)	(-0.19)
Lags for Newey-West SE's	12	12	12	12	12	12	12	12	12	12	1	12	12
No. Time Periods	134	124	135	135	135	135	135	135	135	135	135	123	123

N	508,659	438,364	463,664	463,664	463,664	463,664	463,664	463,664	462,655	484,265	512,145	394,768	394,755

Table 9: Returns Following 3-Year Trading Variables

This table reports results from a Fama-Macbeth regression of monthly returns on the *Net* anomaly indicator, *Retail Trading, Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading, Other Institutional Trading, Short Seller Trading,* and *Firm Trading* aggregated through the 3 years prior to the month of the anomaly stock sorts, *log(Price)* at the month of the anomaly stock sorts, and *log(Size)* as measured by the log of the CRSP reported market capitalization of common equity at the month of the anomaly stock sorts. Monthly Returns are reported by CRSP and denoted as basis points. The *Retail Trading* is expressed as the percentage of common equity net purchased by retail traders during the relevant time period (.01 = 1% of common equity). We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020) *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 3 years prior to the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. To create the *Net* anomaly variable, we use 130 cross-sectional anomalies, which are described in the paper's appendix. We exclude anomalies based on 13F data, short interest and share issuances since they are used for the construction of our institutional trading, *Short Seller Trading* and *Firm Trading* measures. For each stock-month observation, we sum up the number of long-side and short-side anomaly portfolios that the stock belongs to and calculate *Net* as equal to the number of long portfolios minus number of short portfolios. Newey-West standard errors are utilized for the t-statistics in parentheses. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Table 9 (Continued)

	Dependent Variable: *Return*$_{t+1}$												
Net$_t$	1.79***											2.21***	2.51***
	(2.98)											(3.72)	(3.94)
Retail Trading$_{t-35,t}$		-1649.29***										-963.21***	-952.69***
		(-4.38)										(-3.24)	(-3.39)
Mutual Fund Trading$_{t-35,t}$			-59.61**									-26.25	-21.98
			(-2.28)									(-0.89)	(-0.79)
Bank Trading$_{t-35,t}$				-326.15***								-143.72**	-86.57
				(-2.93)								(-2.17)	(-1.37)
Insurance Company Trading$_{t-35,t}$					-293.42**							41.23	43.79
					(-2.59)							(0.54)	(0.58)
Wealth Management Trading$_{t-35,t}$						-107.79						-102.11	-85.40
						(-0.44)						(-0.48)	(-0.42)
Hedge fund Trading$_{t-35,t}$							-71.43					9.56	10.20
							(-1.26)					(0.30)	(0.35)
Other Institutional Trading$_{t-35,t}$								-89.89*				1.48	-6.10
								(-1.66)				(0.04)	(-0.17)
Short Seller Trading$_{t-35,t}$									437.75***			257.77***	264.54***
									(7.58)			(3.95)	(4.11)
Firm Trading$_{t-35,t}$										94.86***		22.87*	25.63*
										(5.64)		(1.73)	(1.84)
Weekly Order Imbalance$_t$											57.20***	58.90***	58.11***
											(7.85)	(8.75)	(10.62)
log(Size$_t$*)*													5.55*
													(1.72)
log(Price$_t$*)*													-4.57
													(-0.76)
Constant	76.04	126.63***	80.03*	84.95*	86.71*	83.39*	86.94*	90.69**	90.55**	93.62**	83.33	139.85***	80.90**
	(1.35)	(5.74)	(1.83)	(1.90)	(1.93)	(1.88)	(1.96)	(2.05)	(2.10)	(2.15)	(1.61)	(7.09)	(2.20)
Lags for Newey-West SE's	12	36	36	36	36	36	36	36	36	36	1	36	36
No. Time Periods	134	100	135	135	135	135	135	135	135	135	135	99	99
N	508,659	309,474	417,983	417,983	417,983	417,983	417,983	417,983	413,260	437,061	512,145	276,933	276,930

Table 10: Residual Return Regressions

This table reports results from a Fama-Macbeth regression of monthly residual stock returns on the various trading variables. Residual stock returns are the residuals from monthly monthly returns, expressed in basis points, regressed on the 130 anomaly indicator variables. These residuals represent the monthly return not explained by the anomaly variables. *Retail Net Buying* is expressed as the percentage of common equity net purchased by retail traders during the relevant time period (.01 = 1% of common equity). We construct the retail net buying variables by summing the daily percentage of common equity purchased by retail traders for the relevant period. Daily percentage of equity purchased by retail traders is calculated as (retail buyer initiated - retail seller initiated) / shares outstanding. Retail buyer and seller-initiated trades are identified by sub-penny pricing as described by Boehmer et al. (2020). *Mutual Fund Trading, Bank Trading, Insurance Company Trading, Wealth Management Trading, Hedge fund Trading,* and *Other Institutional Trading* are calculated as the changes in categorized 13F reported holdings between the most recent filing and the filing 1 (3) years prior to the most recent filing. *Short Seller Trading* is calculated as the negative change in short interest / shares outstanding. Thus, a positive value of *Short Seller Trading* indicates a decrease in the short interest and vice versa. *Firm Trading* is calculated as the negative change in shares outstanding / beginning of period shares outstanding. Thus, a positive value of *Firm Trading* indicates a decrease in the shares outstanding and vice versa. All trading variables are winsorized at the 1% level. Weekly order imbalance is calculated as the average of (retail buyer initiated - retail seller initiated) / (retail buyer initiated + retail seller initiated) for the last five trading days of the month. Newey-West standard errors are utilized for the t-statistics in parentheses. The sample period is from 2006:10 to 2017:12. *, **, and *** indicate significance at the 10%, 5% and 1% respectively.

Table 10 (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent Variable: Return Residual$_{t+1}$									
	Panel A: Prior 1-Year Trading									
Retail Trading$_{t-11,t}$	-507.34									-82.72
	(-0.51)									(-0.13)
Mutual Fund Trading$_{t-11,t}$		-77.85								-43.17
		(-0.73)								(-0.50)
Bank Trading$_{t-11,t}$			-455.43*							-270.43
			(-1.98)							(-1.63)
Insurance Company Trading$_{t-11,t}$				-451.61						-322.85
				(-1.34)						(-1.15)
Wealth Management Trading$_{t-11,t}$					4.10					68.25
					(0.02)					(0.28)
Hedge fund Trading$_{t-11,t}$						58.69				72.87
						(0.61)				(0.93)
Other Institutional Trading$_{t-11,t}$							-61.86			-40.09
							(-0.58)			(-0.51)
Short Seller Trading$_{t-11,t}$								279.33*		205.82*
								(1.79)		(1.69)
Firm Trading$_{t-11,t}$									143.34***	106.32**
									(2.74)	(2.14)
Weekly Order Imbalance$_t$										56.29***
										(7.74)
log(Size$_t$)										3.99
										(0.59)
log(Price$_t$)										-6.31
										(-0.25)
Constant	83.98*	78.49	84.94	83.33	81.13	81.73	77.97	83.70	86.91	51.87
	(1.90)	(1.45)	(1.55)	(1.53)	(1.49)	(1.52)	(1.45)	(1.57)	(1.63)	(0.64)
Number of Lags for Newey-West Standard Errors	12	12	12	12	12	12	12	12	12	12
No. Time Periods	122	133	133	133	133	133	133	133	133	122
N	433,282	458,951	458,951	458,951	458,951	458,951	458,951	457,902	478,773	392,964

Table 10 (Continued)

	Dependent Variable: Return Residual$_{t+1}$									
	Panel B: Prior 3-Year Trading									
Retail Trading$_{t-35,t}$	-1179.92***									-817.64***
	(-5.04)									(-4.62)
Mutual Fund Trading$_{t-35,t}$		-14.83								-43.88
		(-0.47)								(-1.39)
Bank Trading$_{t-35,t}$			-162.42							59.14
			(-1.41)							(0.64)
Insurance Company Trading$_{t-35,t}$				-195.68						77.03
				(-1.07)						(1.29)
Wealth Management Trading$_{t-35,t}$					-51.62					-67.33
					(-0.23)					(-0.32)
Hedge fund Trading$_{t-35,t}$						-11.70				35.48
						(-0.24)				(1.31)
Other Institutional Trading$_{t-35,t}$							-54.54			-11.61
							(-0.99)			(-0.42)
Short Seller Trading$_{t-35,t}$								180.81***		108.98
								(2.70)		(1.19)
Firm Trading$_{t-35,t}$									55.67***	-19.80
									(3.31)	(-1.55)
Weekly Order Imbalance$_t$										54.87***
										(10.13)
log(Size$_t$)										-3.04
										(-0.90)
log(Price$_t$)										7.58
										(0.84)
Constant	121.68***	79.73*	87.35**	86.42**	82.59*	85.78**	87.48**	87.81**	89.90**	152.98***
	(5.67)	(1.91)	(2.01)	(2.03)	(1.97)	(2.05)	(2.09)	(2.10)	(2.22)	(4.08)
Number of Lags for Newey-West Standard Errors	36	36	36	36	36	36	36	36	36	36
No. Time Periods	98	133	133	133	133	133	133	133	98	98
N	305,320	413,785	413,785	413,785	413,785	413,785	413,785	408,967	432,176	275,729

4. Retail Investors and Analysts

We study how retail investors respond to analysts' revisions in recommendations, price target-implied return forecasts, and EPS forecasts. Our paper aims to address unanswered questions in both the retail and analyst literatures. A growing literature on retail investors is largely concerned with individual investors' decision-making processes and investment performance (e.g., Barber and Odean (2013) and Boehmer, Jones, Zhang, and Zhang (2020)). Much of the literature on analysts is concerned with the relevance of the information that analysts produce and how this information gets impounded into prices. In a review of the analyst literature, Kothari, So, and Verdi (2016) conclude that "the specific mechanisms through which analysts influence asset prices, and expected returns in particular, are still not entirely clear". In this paper we produce several novel findings regarding how retail investors make decisions, the profitability of retail trades, and how analysts' information influences stock prices.

We estimate retail trading via the methodology developed in Boehmer, Jones, Zhang, and Zhang (2020), which identifies retail market orders in TAQ data. Using this measure, we find that retail trades are responsive to revisions in "analysts' actionables", i.e., recommendations and price targets.[1] These effects are significant even after excluding revisions that occur around earnings announcements and controlling for past daily returns at various horizons, return volatility, and turnover. When analysts increase a recommendation or price target-return forecast, there are significant increases in net retail buying. Net retail buying also declines following negative revisions in recommendations. Retail trading does not respond to reductions in price target-return forecasts, although reductions in return forecasts typically result in positive return forecasts, e.g., the return forecast is reduced from 30% to 20%.

With EPS forecast revisions, we find that overall, retail investors increase net buying following both positive and negative revisions, but do so more following negative revisions. This may in part be due to the fact that institutional investors are more responsive to EPS forecasts. EPS forecasts are of course different than recommendations and price targets, which explicitly give an investment recommendation. Increasing a price target return-forecast by 15%, or moving a recommendation from a buy to a strong buy, gives the investor a clear course of action. In contrast, increasing a quarterly EPS forecast from $0.15 to $0.20 does not explicitly communicate an investment action. Our findings suggest that when the investment recommendation is explicit, retail investors respond accordingly.

We then study whether the aforementioned effects are stronger with All-Star analysts. With recommendations, we find that the retail trading in response to a revision from an All-Star analyst is 2 to almost 5 times as strong as the response to a non-All-Star analyst. Retail investors also trade in the direction of EPS revisions if an All-Star analyst

[1] Like Engelberg, McLean, and Pontiff (2020), we use the word "analysts' actionables" to describe recommendations and price targets. Recommendations and price targets explicitly communicate the investment prospects of a firm. This is in contrast to an EPS forecast or other financial forecast, which does not explicitly communicate whether a stock is likely to outperform.

makes the revision. Overall, these findings support the idea that retail investors that follow analysts are informed investors who pay attention to not only analysts' revisions, but also to the quality of the analyst making the revision.

Next, we explore whether retail investors are "tipped" by analysts or otherwise anticipate changes in analysts' forecasts and recommendations. The incentives for either analysts or brokers working at the same firm as an analyst to tip retail investors are fairly clear. Investment banks often serve retail investors via full-service brokers. More retail trading and more retail assets under management result in more revenues for these banks and thus higher pay for the brokers they employ. Independent or unaffiliated analysts sell their reports directly to retail investors, so there are incentives for tipping at these firms well. We find strong evidence of retail investors trading in anticipation of price target revisions. We find weaker, but still significant evidence with recommendation revisions. We do not find evidence with EPS forecast revisions.

We then examine the informativeness of analysts' revisions and retail trading. Like earlier studies, we find that both retail investors' trade imbalances and analysts' revisions predict stock returns in the intended direction.[2] With the revision variables, our results are completely out-of-sample relative to the earlier studies that document this predictability.

[2] For evidence that analysts' revisions have return-predictability, see Womack (1996), Barber, Lehavy, McNichols, and Trueman (2001), Brav and Lehavy (2003), Gleason and Lee (2003), and Asquith, Mikhail, and Au (2005).

McLean and Pontiff (2016) show that return-predictability for most predictors weakens out-of-sample, so it is important to document that revision variables predict returns in our sample. The return-predictability of retail trading and revisions are largely orthogonal to one another. This means that retail investors that buy shares following positive revisions can expect higher returns as compared to buying shares on regular days that do not follow revisions. We further find that revisions predict returns in subsamples limited to high levels of either retail buying or retail selling. Overall, our findings support the idea that retail trades that follow revisions are more informative and earn greater abnormal returns than retail trades that do not follow revisions.

Our paper builds on several literatures. A literature beginning with Womack (1996) has shown that revisions predict future stock returns. As we mention above, Kothari, So, and Verdi (2016) point out that there is still much to be learned about how analysts' information gets impounded into prices. We show that the information communicated in actionables, i.e., price targets and recommendations, is at least partly impounded into prices via retail investors. Schipper (1991), Bradshaw (2011), and Kothari et al. contend that analyst research is overly focused on EPS forecasts, and has not given enough attention to recommendations and price targets. Our paper studies all three analyst variables together and finds, consistent with this view, that for retail investors recommendations and price targets are more important. Moreover, we find that return-predictability stemming from revisions in price targets and recommendations is stronger than return-predictability stemming from EPS forecasts.

Many studies in the retail literature find that retail investors are overall uninformed, and that retail investors underperform (e.g., Odean (1999), Barber and Odean (2000), Grinblatt and Keloharju (2000), Hvidkjaer (2008), and Barber, Odean, and Zhu (2009a and 2009b), Barber and Odean (2013), and McLean, Pontiff, and Reilly (2020)). Our paper does not contradict this idea. Instead, our findings support the view that temporary spikes in retail trading are informative, even if the average retail trade is a poor one. We thus build on earlier studies, which find that retail trade imbalances are informative about stock returns over short horizons (e.g., Kaniel, Saar, and Titman (2008), Kaniel, Liu, Saar, and Titman (2012), Kelley and Tetlock (2012), Boehmer et al. (2020), McLean, Pontiff, and Reilly (2020)). We show that such informed retail trading is in some cases informed by analysts' revisions, and that retail traders earn higher expected returns when their trades are in response to revisions.

Our paper builds on the findings in Mikhail, Walther, and Willis (2007) and Malmendier and Shanthikumar (2007), who examine large and small trades and their profitability following changes in analysts' recommendations. These papers limit their analyses to anlaysts' recommendations, and do not study price target revisions like we do. Both papers find that both large and small trades increase following positive recommendations. Both papers also conclude that small trades in response to recommendations are uninformed and lead to worse investment performance.

We find that retail trading following revisions is informed, whereas these Mikhail, Walther, and Willis (2007) and Malmendier and Shanthikumar (2007) find that small trades following recommendations are uninformed. If small trades during their sample periods do indeed capture retail trades, then what could explain this difference? Our sample is completely out of sample relative to both studies, as our ability to identify retail trades beings in 2006. Regulatory changes, including Reg FD, the Sarbanes-Oxley Act, and the Global Settlement, that were meant to reduce analysts' biases and level the playing field for retail investors, could also explain the difference. Our sample is completely after these regulatory changes, whereas both of the aforementioned studies have samples that are either completely or mostly before the regulatory changes.

Finally, our paper builds on the findings in Irvine, Lipson, and Puckett (2007), who find that institutional investors are "tipped" by sell-side analysts, as institutional buying increases prior to an analyst initiating a "buy" or "strong buy" recommendation. On the surface, their results suggest that retail investors should be trading in the opposite direction, i.e., if institutions buy more before a bullish recommendation, then retail investors must be buying less. Lipson et al use Plexus data, which represents a subset of institutions. Their sample also covers a 4-year sample period, which ends in 2002, before our sample begins.

As we mention above, our sample is after the many regulatory changes, all of which were passed in 2002 or earlier, that were meant to make things fairer for retail investors with respect to analysts. Irvine et al. also study coverage initiations, whereas we study revisions. We focus on revisions because they are more numerous than initiations, and we find in untabulated tests that the return-predictability stemming from initiations is insignificant during our sample.

4.1. Sample and Variables

4.1.1. Measuring Retail Trading

We estimate retail trading via the methodology developed in Boehmer, Jones, Zhang, and Zhang (2020), which identifies market orders originating from retail investors. Boehmer et. al. show that due to the rules of Regulation NMS (National Market System), one can identify retail orders based on the sub-penny pricing of the execution. Retail market buy orders are likely to be internalized and receive sub-penny price improvement such that the trade price falls slightly below a whole cent. Retail market sell orders are also likely to be internalized, and receive sub-penny price improvement such that the trade price falls slightly above the whole cent. Following Boehmer et al., we calculate the fraction of the penny associated with the transaction price: $Z_{it} \equiv 100 * \text{mod}\ (P_{it}, 0.01)$, where P_{it} is the transaction price in the stock. Thus, trades reported to FINRA TRF (exchange code 'D') with a Z_{it} in the range of (0.6, 1) are identified as buys by retail traders, while trades reported to FINRA TRF with a Z_{it} in the range of (0, 0.4) are identified as sells by retail traders. Like Boehmer et. al., we do not identify trades with Z_{it} in the range of (0.4,0.6) as retail trades, since some advanced order types, such as pegged orders, can result in transaction prices at or near half pennies that do not involve retail traders.[3]

In order to construct our retail trading variable, we require that for every month during the relevant period, the stock must have at least one retail-initiated trade. This ensures that the stock was actively traded, and was not newly listed or temporarily delisted. The identification of retail trade relies on Regulation NMS, so our sample period begins in October 2006 and ends in 2019. We find the share of identified retail initiated trades begins to rise in October 2006. Boehmer et. al. (2020) validate this methodology using actual retail trade data from Kelley and Tetlock (2013) and with retail trades obtained from NASDAQ.

We construct two retail trading measures. The first is a trade imbalance measure, which is also used in Boehmer et al. This variable is net retail buys (retail buys – retail sells) scaled by total retail trading (retail buys + retail sells). We refer to this measure as *Retail Direction*, as it shows the direction in which retail traders trade, but not the magnitude relative to total trading. As an example, if buys are 10 and sells are 5, *Retail*

[3] To our knowledge, this retail measure is the only viable retail measure that can be constructed from commercially available data. Methods based on trade size are no longer viable since the proliferation of market fragmentation and algorithmic trading prevent the identification of the original order size.

Direction will equal 1/3. *Retail Direction* will also equal 1/3 if buys are 1,000 and sells are 500, or if buys are 1M and sells are 0.5M, and so on. *Retail Direction* therefore reflects the direction of the trading, but not the magnitude.

Our second measure aims to better reflect the magnitude of retail trading. This measure has the same numerator as *Retail Direction* (retail buys – retail sells), but the denominator is *total* trading volume, which reflects both institutions and retail investors. We refer to this variable as *Retail Magnitude*. As an example, assume retail investors buy 1,000 shares and sell 500, while total institutional trades equal 8,500 shares. In this case, *Retail Magnitude* will equal 0.05. If instead, total institutional trades were 18,500 shares, then *Retail Magnitude* would equal 0.025. In contrast, *Retail Direction* would be 1 in both cases. Hence, *Retail Magnitude* reflects the magnitude of net retail buying relative to total volume, whereas *Retail Direction* couches net retail buying in terms of total *retail* trading, but ignores whether these trade imbalances are large or small relative to the stock's total trading activity.

Panel A of Table 1 shows summary statistics for the two retail trading variables. Both variables are reported in percent (multiplied by 100). *Retail Direction* has a mean of 2.59%, a median of zero, and a standard deviation of 43%. Hence, on average, retail buys are about equal to retail sells during our sample period, however there is a good deal of variance in this variable. The 10^{th} and 90^{th} percentiles are -0.59 and 0.50, respectively. *Retail*

Magnitude has a mean of -0.19% and a median of zero. Its standard deviation is 6.6%, and its 10th and 90th percentiles are -4.1% and 3.6%, respectively. *Retail Magnitude* is scaled by total trading volume, so we expect it to be several orders of magnitude smaller than *Retail Direction*, which is scaled by retail trading volume.

4.1.2. Analyst Variables

We obtain data on analysts' revisions for EPS forecasts, price targets, and recommendations from the IBES details database. We focus on revisions as we believe these to be most salient to investors, and all three of the revision variables described here predict returns in the intended direction in our sample, and thus are useful and informative to investors.[4]

We measure revisions in EPS forecasts by subtracting the old value from the new value, and scaling this difference by the stock price measured the day before the new value is announced. We measure recommendation revisions by simply the old recommendation from the new recommendation. We code recommendations such that a strong buy = 5 and a strong sell =1. For price targets, we first compute the implied return, by scaling the 12-

[4] In untabulated results we find that initiations, or first-time recommendations, price targets, and EPS forecasts, do not predict returns in our sample.

month price target by the previous day's closing stock price. We then compute the change in implied return, and use that as the revision variable.

We provide summary statistics for our revision variables in Panel B of Table 1. The EPS Forecast revisions variable is reported in percent. It averages -0.44%, so the average EPS forecast revision is a reduction in the EPS forecast. This variable has a standard deviation of 3.75%, and the 10^{th} and 90^{th} percentiles are -1.3% and 0.71%, respectively.

The recommendations revisions average -0.058, or pretty close to zero. The median recommendation revision is zero. The 10^{th} and 90^{th} percentiles are -2 and 1, e.g., a recommendation falling from 5 to 3, or a recommendation increasing from 4 to 5. The standard deviation is 1.048.

The price target-revision variable has average and median values that are very close to zero. The 10^{th} and 90^{th} percentiles are -16.7% and 17.3%, which are sizeable changes, i.e., the 12-month return forecast increased or declined by about 17%. The standard deviation for this variable is 17.8%, so it varies a good deal.

4.2. Main Results

In this section of the paper, we discuss our main findings. Section 2.1 provides a discussion of some sorts, which are reported in Figure 1. Section 2.2 describes our tests that relate retail trading to analysts' revisions. These results are reported in Tables 3 and 4. We then ask whether retail investors respond more strongly to All-Star analysts' revisions. These findings are discussed in Section 2.3 and reported in Table 5. Section 2.4 discusses tipping and our tests of whether retail investors trade ahead of revisions, which are reported in Table 6. Section 2.5 discusses how revisions and retail trading related to future stock returns. We also explore whether retail trading in response to revisions predicts returns more strongly. These results are reported in Tables 7 and 8.

4.2.1. Univariate Results

We report results from univariate sorts in Figure 1. Figure 1.A was made using the *Retail Direction* variable, while Figure 1.B was made using the *Retail Magnitude* variable. In each figure, we display the average value of the retail trading variables, sorted into 3 groups. The three groups we form include: days when there was a revision in the 90th percentile or higher for the revision variable (*Up*), days when there was a revision in the 10th percentile or lower for the revision variable (*Down*), and days with no revision. Before taking the averages within each group, we demean each firm-day observation the firm's mean. We also exclude observations for which there was an earnings announcement over any of the three previous days, so that we can more cleanly relate trading to the revision.[5] Including earnings announcements does not change our findings.

[5] Kaniel, Saar, and Titman (2008) show that retail trading increases following earnings announcements.

The Figures make several points very clear. First, retail investors' trading is highly responsive to recommendation revisions. Retail investors buy more following positive recommendation revisions and sell more following negative recommendation revisions. Recommendations are the most salient and perhaps widely-followed analyst actionable, i.e., a recommendation clearly tells investors what to do, so it makes sense that retail investors would be most responsive to these revisions. Moreover, recommendation revisions predict returns in the intended direction, so it is wise for retail investors to trade this way.

Retail investors also respond positively to price target revisions; however, this is only the case for positive changes. For positive price target revisions, there is a large increase in retail net buying, even larger than that for positive recommendation revisions. However, for negative revisions, retail investors also increase net buying, although they do so less so than with positive revisions. Later, we will show in regressions that the effect around negative revisions is insignificant. Note that a negative price target revision does not necessarily imply that investors should sell the stock. As an example, if the 12-month return forecast falls from 30% to 20%, this does not clearly imply that investors should sell.

Finally, the figures show that retail investors buy more shares in response to EPS forecast revisions, regardless of the direction of the revision. In fact, investors buy more following negative revisions than positive ones, although in our later tests we will show that this effect reverses in the case of All-Star analysts. Note that an EPS revision is not a clear investment signal, e.g., decreasing and EPS forecast from \$0.20 to \$0.15 is not the same thing as recommending that the stock should be sold.

Overall, the results suggest that retail investors pay attention to analysts, and in general buy more following analysts' positive revisions. This result is stronger with actionables, i.e., recommendations and price target-forecasts, where analysts are clearly telling investors how to trade. With EPS forecasts, which offer no such clear instruction, there is perhaps some confusion, with net retail buying increasing regardless of the direction of the revision. This could reflect the fact that institutions trade more heavily following EPS revisions, and retail investors, unsure of what do, provide liquidity.

4.2.2. Retail Trading in Response to Revisions: Revision-Level Regressions

In this section of the paper we discuss revision-level regressions. The unit of observation is an analysts' revision, and we regress daily retail trading on the revision variables and controls. Hence, these regressions ask whether across revisions, retail net buying increases with the positivity of the revision. We continue to exclude revisions that had an earnings announcement on the same day, or in the 2 days prior, as in such cases both the analysts and the retail investors may be responding to the earnings announcement. Including revisions that follow earnings announcements makes our results stronger. We estimate regressions for revisions in EPS, recommendations, and price targets separately. In the subsequent tables, we put all three types of revisions into a single regression and report similar findings.

The regressions reported in Table 2 include firm and time fixed effects. The standard errors are clustered on firm and time. We regress day t retail trading on revisions reported on day *t-1*, along with the day *t-1* stock return, lagged weekly return, lagged monthly return, lagged 6-month return, day *t-1* return squared, lagged weekly return squared, lagged daily return variance over the last month, last month's turnover, and market capitalization. The lagged returns and volatility measures are meant to control for events that could impact both the revision variables and retail trading.

The first two columns report the results from regressions in which EPS revisions are the independent revisions variable. In the first column, *Retail Direction* is the dependent variable. In the second column, *Retail Magnitude* is the dependent variable. Recall that *Retail Direction* is equal to retail net buying (retail buys – retail sells) scaled by retail trading volume, while *Retail Magnitude* is equal to retail net buying scaled by total trading volume. In both specifications, the EPS revision coefficients are negative and significant, showing that retail net buying increases more following a *decrease* in the EPS forecast.

Columns 3 and 4 report the results for price target revisions. The results here are very strong, and show that retail net buying increases significantly following increases in price targets. In the *Retail Direction* regression, the revision coefficient is 0.019 (t-statistic

= 10.06), while in the *Retail Magnitude* regression, the coefficient is 0.033 (*t*-statistic = 6.52). Moving from the 10^{th} to 90^{th} percentile of the price target-return forecast revision variable yields an increase of 0.656% in *Retail Direction*. Moving from the 10^{th} to 90^{th} percentile of the return forecast revision yields an increase of 1.14% in *Retail Magnitude*. *Retail Direction* has a standard deviation of 43%, while *Retail Magnitude* has a standard deviation of 6.6%, so the effect is much larger in economic terms for *Retail Magnitude*.

What does it mean if *Retail Magnitude* moves more than *Retail Direction*? It reflects the fact that the amount or magnitude of retail trading increased along with the directional change in trading. As an example, consider a stock for which on day *t*, retail buys equal 20 and retail sells equal 10. Now assume that on day *t+1*, buys increase to 210 and sells to 100. Assume institutional trading equals 1,000 shares traded on both days. *Retail Direction*, which scales by retail trading volume, would increase slightly from 0.33 to 0.35. In contrast *Retail Magnitude*, which scales by total volume, would increase from 0.009 to 0.840, a much larger increase, especially in percentage terms. Because it scales by retail volume, *Retail Direction* does not reflect how the magnitude of the retail trading increases.

In the final two columns we report the results for revisions in recommendations. The results here are also very strong, and show that retail investors increase their net buying in a stock if an analyst strengthens their recommendation. In the *Retail Direction* regression, the coefficient is 0.005 (t-statistic = 9.35). Thus, if a recommendation increases by 1 (e.g., from buy to strong buy), then *Retail Direction* increases by 0.5%. In the *Retail Magnitude* regression, the coefficient is 0.006 (t-statistic = 4.39), showing a 0.6% increase in *Retail Magnitude*. *Retail Direction* has a standard deviation of 43%, while *Retail Magnitude* has a standard deviation of 6.6%, so here again the effect is much larger in economic terms for *Retail Magnitude*, representing about 10% of a standard deviation. If we were to move from the 10^{th} to 90^{th} percentile of the recommendation revision variable (from -2 to 1), then the regression coefficient suggests an increase of 1.8% in *Retail Magnitude*, or more than one-quarter of a standard deviation.

The control variables also reveal some interesting facts about retail trading First, we see that retail traders are contrarian. The coefficients for returns measured over the last week, month, and 6-months are all negative and statistically significant. The return measured over the last day is positive and significant, likely reflecting the fact that revisions that are more bullish, as measured by stock price reactions, receive even greater retail buying. Retail investors also buy more of larger stocks and more of stocks with higher turnover. These results are consistent with the findings reported in Boehmer et al. (2020).

Taken in their entirety, the results in Table 2 show that retail investors are responsive and informed with respect to revisions in analysts' actionables. When analysts increase price-target return forecasts or recommendations, retail investors buy more shares. With earnings forecasts, the results are the opposite. When EPS forecasts increase, retail investors buy fewer shares. This of course suggests that institutions are buying more shares. Overall, the findings suggest that retail investors are responsive to analysts' actionables, which clearly suggest a course of action, whereas institutions are more responsive to EPS forecasts.

4.2.2.1 Retail Trading in Response to Revisions: Daily Specifications

In this section of the paper we further explore the effects of revisions on retail trading, but make two major changes relative to the specifications described in the last section. First, we make the unit of observation firm-day, rather than revision. Most firm-day observations do not have revisions, in which case the revision variable is assigned a value of zero. Some firm-days have multiple revisions of the same type (e.g., EPS forecast), and in such cases we take a simple average. Second, we include all of the revision variables in the same regression. We continue to include the same control variables that we use in the previous tables. We also continue to exclude observations for which EPS was reported during any of the 3 previous days. Finally, we include of a lagged value, day *t-2*, of the retail trading variable value. This is done to capture the fact that retail trading may be persistent. We choose day *t-2* so that the variable value does not reflect the announcement of the revision, which occurs on day *t-1*.

We report the results from these specifications in Table 3. The results are largely the same as those in Table 2, which estimates at the revision-level. With respect to EPS forecast revisions, the response of retail trading is still negative. That is, when analysts raise lower EPS forecasts, retail investors buy more of the stock. As we explain earlier, this could reflect the fact that institutions respond more strongly to EPS revisions, pushing prices up and perhaps encouraging retail investors to provide liquidity and sell their shares.

The recommendation and price target revision variables both continue to be associated with positive and significant reactions from retail traders. When analysts' revisions signal a more favorable outlook via recommendations or price targets, retail investors respond in kind by purchasing more shares. These effects are seen both with *Retail Direction* and with *Retail Magnitude*. Overall, the results continue to be consistent with the idea that revisions in analysts' actionables inform retail trading.

4.2.2.1 Retail Trading in Response to Revisions: Daily Specifications and Large Revision Dummies

In this section of the paper we replace our continuous revision variables with dummy variables. For each revision variable we create an *Up* dummy that is equal to 1 if there is a revision at or above the 90th percentile of the distribution of the revision variable, and zero otherwise. We also create a *Down* dummy that is equal to 1 if there is a revision at or below the 10th percentile for a revision variable, and zero otherwise. In some cases, analysts simply reaffirm their prior forecasts and there is no change. In such cases, both dummies are equal to zero. We continue to use the firm-day sample that we used in the previous table, so most observations have values of zero for both the *Up* and *Down* dummies, as for most firms on most days there are no revisions. We also continue to exclude observations for which EPS was reported during any of the 3 previous days. The sample and variables here mirror those used to create Figure 1.

The results in Table 4 largely confirm the findings in the earlier tables and those in Figure 1 that were discussed earlier. Retail buying is significantly higher for all 3 types of revisions following positive revisions. However, negative revisions also lead to more retail buying in the case of EPS revisions. With price target revisions, we see the effect of a negative revision is insignificant in all specifications. For recommendations, the *Up* coefficients are always positive and significant and the *Down* coefficients are always negative and significant. Hence, as in the other tables, the results in Table 4 show that retail investors are responsive to analysts' actionables.

In regression 2 *Retail Direction* is the dependent variable and all of the controls are included. The coefficients for *EPS Up* and *EPS Down* are both positive and significant. The *Down* coefficient is larger, consistent with what is reported in Figure 1 and the previous tables. The *EPS Down* coefficient is 0.006 (t-statistic = 9.07), while the *EPS Up* coefficient is 0.003 (t-statistic = 4.37). The F-Statistic reported at the bottom of the table shows that *EPS Down* is significantly larger than *EPS Up*.

The *Target Up* coefficient in regression 2 is 0.011 (t-statistic = 5.65) in regression 2, while the *Target Down* coefficient is 0.002 (t-statistic = 1.09). The F-Statistic reported at the bottom of the table shows that the *Target Up* coefficient is significantly larger than the *Target Down* coefficient. Thus, as shown in the earlier tables, retail investors tend to be responsive to price target revisions. Recall that *Target Down* can still involve a positive price target-return forecast, e.g., the retune forecast could decline from 20% to 15%, but it is still a positive return forecast.

The coefficients in regression 2 again show that recommendations are where retail investors tend to pay the most attention. The *Rec. Up* and *Rec. Down* coefficients are both highly significant and signed such that retail investors are following the revisions. The *Rec. Up* coefficient is 0.008 (t-statistic = 5.61), while the *Rec. Down* coefficient is -0.006 (t-statistic = 4.65). The difference in *Retail Direction* following positive and negative recommendations revisions is therefore about 0.140, or about 1/3 of a standard deviation of *Retail Direction.*

Regressions 3 and 4 use *Retail Magnitude* as the dependent variable, and tell a similar story. For readability, we multiple all of the coefficient values by 100. In regression 4, which has the full set of control variables, the *EPS Up* coefficient is 0.015 (t-statistic =2.40) and the *EPS Down* coefficient is 0.040 (t-statistic = 6.36). So here again, retail investors buy more after all EPS revisions, yet do so more strongly after negative revisions.

The price target revision coefficients are both positive in regression 4, however only the *Target Up* coefficient is significant. The *Target Down* coefficient is 0.032 (t-statistic = 1.53), while the *Target Up* coefficient is 0.059 (t-statistic = 2.93), and the difference between the coefficients is statistically significant. The coefficients for the recommendation revisions are 0.054 (t-statistic = 4.53) for the *Rec. Up* and -0.025 (t-statistic = 2.02) for the *Rec. Down*. The difference between the *Rec. Up* and *Rec. Down* coefficients is statistically significant and economically meaningful. The standard deviation for *Retail Magnitude* is 0.067. The coefficients are multiplied by 100 in regressions 3 and 4, so the difference between the *Rec. Up* and *Rec. Down* coefficients is about 0.008, or 11% of a standard deviation of *Retail Magnitude*.

The findings in Table 5 agree with the findings in the earlier tables, and thus confirm the finding that retail investors are responsive and informed with respect to revisions in analysts' actionables, especially recommendations, but not with EPS revisions. Our findings suggest that institutions may be more responsive to EPS revisions.

4.2.3 Retail Trading in Response to Revisions: The Effects of All-Star Analysts

In this section of the paper we ask whether retail investors behave differently following revisions of "All-Star" analysts. Clarke, Khorana, Patel, and Rau (2007) argue that analysts determined by *Institutional Investor* magazine to be "All-Stars" may be more adept than typical analysts. An All-Star analyst is defined as an analyst who was denoted by Institutional Investor as an All-Star or a runner-up in the prior November issue of the magazine. That is, if an analyst is denoted an All-Star in 2013, we code them as an All-Star in 2014. We have All-Star data for the years 2013-2017.

To test for the effects of All-Star status, we estimate basically the same revision-level regression as in Table 2, only we include a dummy variable equal to 1 if the analyst is an All-Star, and an interaction between the All-Star dummy and the revision variable. A positive and significant coefficient for the interaction term shows that retail net buying increases more for positive revisions if the revising analyst is an All-Star.

We report the findings from these tests in Table 4. For the regressions reported in the first two columns, the revision variable is the EPS forecast. In both regressions, the revision-All-Star interaction coefficient is positive and significant, showing that retail investors have a more positive response to All-Star analysts' EPS revisions than to revisions issued by non-All-Stars. In both regressions, the EPS forecast revision variable is negative and significant. The overall effect is thus the revision-All-Star interaction coefficient + the revision coefficient. In both regressions, the interaction coefficient is greater than the revisions coefficient, showing that the overall effect with All-Star analysts is positive, i.e., retail investors buy more following a positive revision from an All-Star analyst. In contrast, the results also show that retail investors buy more following negative revisions from non-All-Star analysts, consistent with what we find in Tables 2 and 3.

The next two columns report the results for revisions in price targets. In both specifications, the All-Star interaction is insignificant. The price target revision coefficient is significant, consistent with Tables 2 and 3. Hence, retail investors trade in response to revisions in price targets, but do so equally for All-Stars and non-All-Stars alike.

The regressions in columns 5 and 6 report the results for revisions in recommendations. The results show that retail investors are significantly more responsive to revisions from All-Star analysts, especially in the case of *Retail Magnitude*. In both regressions, the revision variables and the revision-All-Star interactions are positive and significant. In the *Retail Direction* regression, both the revision and the All-Star interaction coefficients are 0.005. This shows that the effect of a revision on retail trading is twice as large if the issuing analyst is an All-Star. In the *Retail Magnitude* regression, the revision coefficient is 0.005 and the All-Star interaction is 0.019, showing that if an All-Star analyst issues the revision, the effect is more than 4-times as large.

Overall, the results in Table 5 show that when analysts issue revisions in either recommendations or EPS forecasts, retail investors trade more in the direction of the revision if the analyst is an All-Star. With price targets, retail investors seem to respond to All-Stars and non-All-Stars equally.

4.2.4. "Tipping" or Trading in Anticipation of Revisions

In this section of the paper we ask whether retail investors trade ahead of analysts' revisions. This analysis is motivated by the findings in Irvine, Lipson, and Puckett (2007), who find that institutional investors are "tipped" by sell-side analysts, as institutional buying increases prior to an analyst initiating a "buy" or "strong buy" recommendation.

As we explain in the Introduction, the incentives for retail tipping are fairly straight forward. Investment banks often have large retail brokerage arms, and more retail trading and more retail assets under management result in more revenues for these firms and their brokers. Independent or unaffiliated analysts sell their reports directly to retail investors, so there is an incentive for tipping with these analysts as well.

We report the results from our tipping tests in Table 5. The dependent variable is one of the revision variables (EPS forecast, price target-return forecast, or recommendations), and we regress this on lagged values of either *Retail Direction* or *Retail Magnitude* for each of the previous 5 trading days. We include lagged daily stock returns and lagged daily returns squared for the same 5 trading days, market capitalization, and turnover as controls. The regressions all have firm and time fixed effects, and standard errors that are clustered on firm and time. As in the other tables, we exclude revisions that follow an earnings announcement over any of the three previous days.

In the first two columns we report the results for EPS revisions. The retail trading coefficients are all insignificant in the regression that uses *Retail Direction*. In the regression reported in the second column the 1-day lag *Retail Magnitude* coefficient is negative and significant, however the other *Retail Magnitude* coefficients are insignificant. Overall, the evidence here does not support the idea that retail investors are tipped or otherwise anticipate EPS revisions.

Columns 3 and 4 report the results for revisions in price targets. The results here are very strong, and consistent with tipping or retail investors somehow anticipating price target revisions. In column 3, all five of the coefficients for lagged values of *Retail Direction* are positive and statistically significant. The coefficient for *Retail Direction* at the 1-day lag is 0.007 (t-statistic = 8.56), so a 1-standard deviation increase in *Retail Direction* portends a higher value of about 0.3% in the revision of price target-implied returns. If we add up the effects from all 5 *Retail Direction* coefficients, then the effect is about a 1.2% higher revision in price target-implied returns. Column 4 shows similar results for *Retail Magnitude*. All 5 of the coefficients for the lagged values are positive, and 4 are significant. The 1-day lag coefficient has a value of 0.076 (t-statistic = 5.60), which alone suggests about a 0.5% higher revision in price target-implied return.

Columns 5 and 6 report the results for recommendation revisions. We find evidence of retail investors anticipating such revisions with *Retail Direction*, but not *Retail Magnitude*. In column 5, the coefficients for lagged values of *Retail Direction* are all positive, and are significant at the 1-day and 5-day lags. The coefficient for the 1-day lag suggests a 1.1% higher recommendation revision given a one standard deviation increase in *Retail Direction*.

Taken in their entirety, the findings in Table 6 are consistent with the idea that some retail investors are tipped or otherwise anticipate revisions in analysts' actionables, but not revisions in EPS forecasts. Alternatively, it could be that analyst recommendations are partially driven by retail trading. If analysts seek to observe and mirror retail trader sentiment when making their revisions, it may appear that retail traders anticipate revisions when in fact they are responsible for driving recommendations. While we cannot rule out this possibility, we find it less likely since we know of no anecdotal evidence that analysts respond to retail sentiment, and due to the difficult nature of observing retail trades during our sample period. Furthermore, this alternative explanation also applies (and is more plausible) for institutional trades, which as we explain above have been linked to tipping by Irvine, Lipson, and Puckett (2007).

4.2.5 Retail Trading, Analysts' Revisions, and Stock Returns

In this section of the paper we study how our analyst and retail trading variables relate to stock return predictability. Our results thus far show that retail investors follow revisions in actionables, and also follow EPS forecast revisions if the analyst making the revision is an All-Star. If such trading is "informed" or rational, then it needs to be the case that revisions predict returns in the intended direction. As we mention in the Introduction, previous studies show that all three of our revision variables predict returns in the intended direction. However, McLean and Pontiff (2016) show that return-predictability is typically lower out-of-sample, possibly because of both data mining and informed trading. We therefore begin by testing whether such revision-predictability exists in our sample period. Boehmer et al. (2020) and McLean, Pontiff, and Reilly (2020) both show that retail trade imbalance measures (*Retail Direction*) predict returns in the intended direction. Our other retail trading variable, *Retail Magnitude*, however, has not been shown to predict stock returns, and we test whether it predicts returns here.

We report the results from our initial return-predictability regressions in Table 6. The dependent variable in each regression is stock returns measured over the subsequent 20 trading days. We multiply this variable by 100 so that the coefficients are easier to read. As in the earlier tables, we include controls for lagged returns, volatility, size, and turnover, and exclude observations with earnings announcements over the three previous days. The regressions have firm and time fixed effects and standard errors clustered on firm and time.

In the first regression, we include the three revision variables, but not the retail trading variables. The coefficients for each of the revision variables are positive and statistically significant. This means that when analysts become more bullish on a stock or raise its EPS forecast, returns over the month are significantly higher. This also shows that retail trading in the direction of the revisions, which we document in the previous tables for recommendations and price targets is informative. The revision variables' coefficients reflect increases in expected returns per standard deviation increase in the revision variable of 0.014%, 0.21%, and 0.22% for EPS forecasts, price target-return forecasts, and recommendations, respectively. Hence, although all three types of revisions result in statistically significant return predictability, revisions in price targets and recommendations produce return-predictability that is far more economically meaningful. The fact that retail investors follow revisions in actionables, but not EPS forecasts, further supports the idea of informed retail trading following analysts' revisions.

The regressions reported in the second and third columns report the results for the retail trading variables. Both retail trading variables produce return predictability that is statistically significant. In column 2, the coefficient for *Retail Direction* suggests an increase in expected returns of 0.14% per month per standard deviation increase, while in column 3 the coefficient for *Retail Magnitude* suggests and expected return increase of 0.08% per month per standard deviation increase. The mean value of monthly return is 0.79% in our sample, so with both retail trading variables the effects are economically meaningful.

The final two columns in Table 6 include each of the retail trading variables along with the revision variables. The results show that the effect of retail trading does not impact the effect of revisions, and vice versa. The coefficients for the retail trading variables are virtually the same in these specifications as compared to the specifications that did not include the revision variables. Similarly, the coefficients for the revision variables are essentially the same as those reported in the specifications that do not include the retail trading variables. This suggests retail trading in the direction of revisions is informative, i.e., a retail trader who buys shares following a positive revision earns a higher return than a retail trader who buys shares on a day with no revision.

4.2.5.1. The Informativeness of Retail Trading and Analysts Revisions

In this section we further explore whether retail trading that follows revisions is informative. We estimate regressions similar to those reported in Table 7, only we do so in subsamples based on retail trading. We consider subsamples with only positive or negative values of *Retail Direction* as well as subsamples based on the 10^{th} and 90^{th} percentiles of *Retail Direction*. Creating subsamples based on the 10^{th} and 90^{th} percentiles of *Retail*

Magnitude create similar findings, so for the sake of brevity we only report results based on *Retail Direction*.

We create our subsamples on day *t-1*, and measure returns from day *t* to day *t+20*. We measure the revision variables and other controls on day t-2, so retail investors had this information when they made their trades.

The results in Table 8 show that retail trading that conditions on revision is more informative. Analysts' revisions predict returns in the intended direction within each of the four subsamples. In the first two columns, which limit the samples to either positive or negative values of *Retail Direction*, all of the revision coefficients are statistically significant and signed in the intended direction. This shows that stocks which retail investors bought or sold have higher (lower) returns if analysts issued a positive (negative) revision.

In columns 3 and 4 the subsamples are limited to values of *Retail Direction* that are in either the 10^{th} and 90^{th} percentiles of *Retail Direction*. All of the revision variables are signed in the intended direction, and 2 of the 3 variables are significant in each regression. In the greater than 90^{th} percentile subsample, the price target and recommendations coefficients are significant, whereas in the less than 10^{th} percentile subsample, the EPS and recommendations coefficients are significant. Taken in their entirety, the results in Table 8 show that retail trading is significantly more informative when it follows analysts' revisions.

4.3. Conclusion

This paper studies whether and how retail investors respond to analysts' revisions in EPS forecasts, recommendations, and price targets. We produce several novel findings, which contribute to literatures on both retail investors and sell-side analysts.

We find that overall, retail investors follow revisions in analysts' actionables. That is, when analysts increase their recommendation or raise their price targets, retail investors buy more of the stock. With EPS forecasts, retail investors buy more following both positive and negative revisions, but the effect is stronger with negative revisions. This suggests that retail investors pay closer attention to analysts' actionables than to EPS forecasts. Actionables, i.e., recommendations and price targets, offer explicit guidance with respect to how to trade on the stock. EPS forecasts do not. The counter-trading with EPS forecast revisions on the part of retail investors could reflect an increase in institutional trading following EPS revisions.

We then ask whether these effects are stronger if an All-Star analyst makes the revision. With recommendations, this is very much the case. The response in retail trading following an All-Star's recommendation revision is 2x to almost 5x stronger as compared to a non-All-Star's revision. With EPS forecast revisions, we find that retail investors switch course, and trade in the direction of the revision. With price target revisions, we find no effect; retail investors respond similarly to revisions in price targets regardless if the analyst is an All-Star or not.

We find some evidence of tipping, or at least evidence of retail traders anticipating and thus trading ahead of revisions. We find strong evidence of retail investors trading in anticipation of price target revisions. We find weaker, but still significant evidence with recommendation revisions. We do not find evidence with EPS forecast revisions. As we mention earlier, there are incentives for analysts to tip retail investors. Investment banks typically have retail brokerage arms that serve retail investors. Unaffiliated analysts sell their research directly to retail investors.

In the final part of our paper we study how analysts' revisions and retail trading relate to stock return-predictability. This analysis produces several interesting insights. All 3 types of revisions predict returns in our sample. The predictability of retail trading and revisions are largely orthogonal to one another. Revisions also predict returns in subsamples limited to high levels of either retail selling or retail buying. Thus, retail trades that follow revisions are more informative.

Overall, our findings are consistent with the idea that spikes in retail trading reflect informed trading, and at least some of these trades are informed by analysts' revisions. Our research also shows that one channel through which analysts information gets into prices is through retail investors.

References

Asquith, P., M. Mikhail, and A. Au. 2005. Information content of equity analyst reports. Journal of Financial Economics 75 (2): 245-82.

Barber, B., Lehavy, R., McNichols, M. and Trueman, B. (2001), Can Investors Profit from the Prophets? Security Analyst Recommendations and Stock Returns. The Journal of Finance, 56: 531-563.

Barber, B. M., and Odean, T. (2000). Trading is hazardous to your wealth: The common stock investment performance of individual investors. Journal of Finance, 55, 773–806.

Barber, B. M., Odean, T., & Zhu, N. (2009a). Do retail trades move markets? Review of Financial Studies, 22, 151–186.

Barber, B. M., Odean,T., & Zhu, N. (2009b). Systematic noise. Journal of Financial Markets, 12, 469–547.

Barber, Brad and Odean, Terrance, (2013), The Behavior of Individual Investors, Chapter 22, p. 1533-1570, Elsevier.

Boehmer, Ekkehart, Charles M. Jones, Xioyan Zhang, and Xinran Zhang, 2020, Tracking Retail Investor Activity, Working Paper

Bradshaw, Mark, 2011, Analysts' Forecasts: What do we know after decades of work? Working Paper.

Brav, A., and R. Lehavy. 2003. An empirical analysis of analysts' target prices: Short-term informativeness and long-term dynamics. Journal of Finance 58 (5): 1933-67.

Clarke, J., A. Khorana, A. Patel, and R. Rau, 2007. "The impact of all-star analyst job changes on their coverage choices and investment banking deal flow," *Journal of Financial Economics* 84, 713- 737.

Hvidkjaer, S. (2008). Small trades and the cross-section of stock returns. Review of Financial Studies, 21, 1123–1151.

Kaniel, R., Liu, S., Saar, G., & Titman, S. (2012). Individual Investor Trading and Return Patterns around Earnings Announcements. Journal of Finance, 67, 639–680.

Kaniel, R., Saar, G., & Titman, S. (2008). Individual investor trading and stock returns. Journal of Finance, 63, 273–310.

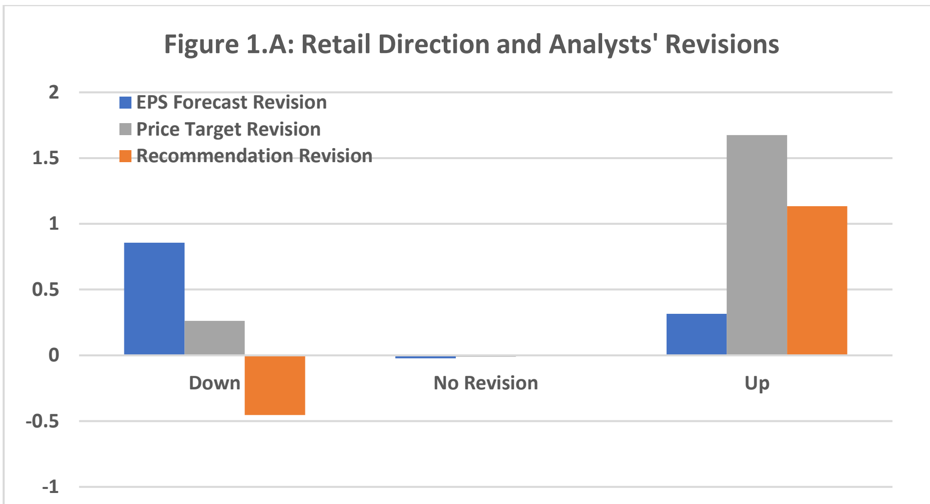
Kelley, E. K., & Tetlock, P. C. 2013, How Wise Are Crowds? Insight from Retail Orders and Stock Returns. Journal of Finance 68, 1229-1265.

Kothari, S.P., Eric So, and Rodrigo Verdi, 2016. "Analysts' Forecasts and Asset Pricing: A Survey," *Annual Review of Financial Economics* 8, 197-219.

McLean, R. David and Jeffrey Pontiff, 2016, "Does academic research destroy stock return predictability?," *Journal of Finance* 71, 5-32.

Figure 1: Retail Trading on and 0ff Days with Large Analysts' Revisions

This Figure displays average values for *Retail Direction* (Figure 1.A) and *Retail Magnitude* (Figure 1.B) on days following analysts' revisions. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. We demean each observation by its firm-level mean. We consider revisions in EPS forecasts, price targets, and recommendations. "Up" reflects days at or above the 90th percentile for the revision variable, "Down" reflects days at or below the 10th percentile for the revision variable, and "No Revision" reflects days with no revision. The revisions are measured on day *t-1*, and the trading is measured on day *t*. We exclude observations with an earnings announcement on days *t-1*, *t-2*, or *t-3*.



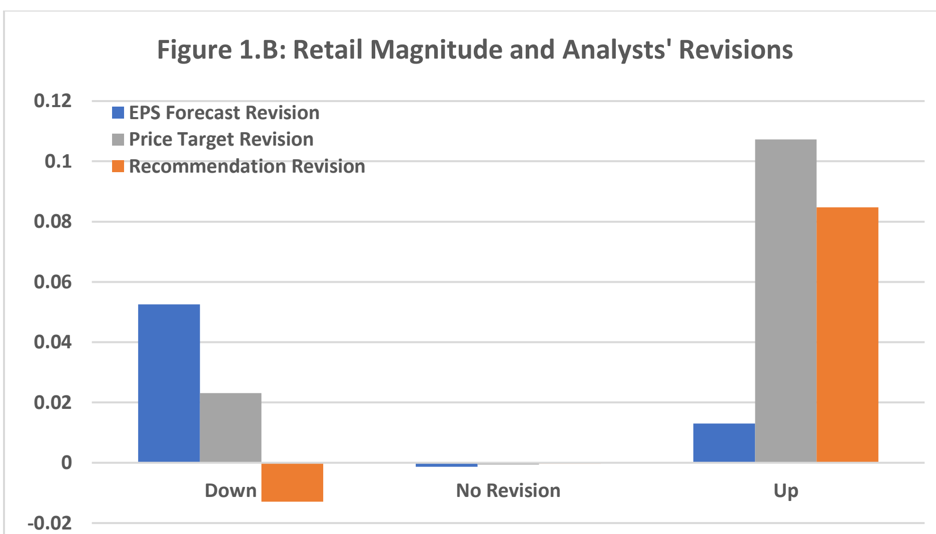


Table 1: Summary Statistics

This table presents summary statistics for the main variables used in this study. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over previous 20 days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. The revision variables are measured at the analyst-level; they are not consensus variables. Each revision reflects a change for an individual analyst. *EPS Revision* is the new EPS forecast, - the most recent EPS forecast, scaled by the stock price measured on the day before the previous EPS forecast. *Price Target Revision* is the new 12-month price target scaled by yesterday's stock price minus the previous 12-month price target scaled by stock price the day before it was announced. *Recommendation Revision* is the new recommendation minus the previous recommendation. We exclude firms that don't have at least one revision during our sample period. The sample period begins in October 2006 and ends in December 2019.

Table 1: (Continued)

Daily Variables	**Mean**	**Median**	**Min**	**Max**	**10th%ile**	**90th%ile**	**Std. Dev.**	**N**
Retail Direction	-0.0259	0.0000	-1.0000	1.0000	-0.5936	0.5005	0.4299	17,800,000
Retail Magnitude	-0.0019	0.0000	-0.3804	0.3843	-0.0413	0.0361	0.0665	17,800,000
Daily Return	0.0005	0.0000	-0.9652	11.5000	-0.0303	0.0300	0.0382	17,800,000
Weekly Return	0.0021	0.0008	-0.9854	21.8422	-0.0681	0.0685	0.0826	17,700,000
Monthly Return	0.0079	0.0050	-0.9989	36.3191	-0.1396	0.1442	0.1620	17,600,000
6-Month Return	0.0449	0.0205	-1.0000	4273.7240	-0.3186	0.3529	4.1281	16,700,000
Variance	0.0014	0.0005	0.0000	6.7284	0.0001	0.0027	0.0137	17,600,000
Daily Return^2	0.0015	0.0001	0.0000	132.2500	0.0000	0.0022	0.0607	17,800,000
Weekly Return^2	0.0068	0.0008	0.0000	477.0799	0.0000	0.0109	0.2128	17,700,000
Market Cap.	4,742,842	578,086	30	1,300,000,000	47,872	8,593,520	21,000,000	17,800,000
Turnover	0.0108	0.0058	0.0000	140.5812	0.0012	0.0190	0.1980	17,600,000
Revision Variables								
EPS Rev.	-0.0044	0.0000	-0.2800	0.1193	-0.0132	0.0072	0.0375	1,719,977
Price Target. Rev.	0.0020	-0.0008	-0.6362	0.6878	-0.1676	0.1729	0.1775	1,075,591
Rec. Rev.	-0.0581	0.0000	-4.0000	4.0000	-2.0000	1.0000	1.0482	414,115

Table 2: Retail Trading in Response to Revisions: Revision-Level Regressions

In this table we regress daily retail trading on lagged values of analysts' revisions and controls. The unit of observation is a revision. The revisions are lagged one day relative to the retail trading. The retail trading variables are *Retail Direction* and *Retail Magnitude*. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. *EPS Revision* is the new EPS forecast minus the previous EPS forecast, scaled by the stock price measured on the day before the new EPS forecast. *Price Target Revision* is the new 12-month price target scaled by the day *t-1* stock price minus the previous price target scaled by its *t-1* stock price. *Recommendation Revision* is the new recommendation minus the previous recommendation. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over previous 20 days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. We exclude retail trading observations for which there is an earnings announcement over any of the three previous days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The sample period begins in October 2006 and ends in December 2019.

Table 2: (Continued)

	EPS Forecast Revisions		Price Target Revisions		Recommendation Revisions	
	Retail Direction	*Retail Magnitude*	*Retail Direction*	*Retail Magnitude*	*Retail Direction*	*Retail Magnitude*
Revision	-0.023	-0.142	0.019	0.033	0.005	0.006
	(2.26)**	(4.85)***	(10.06)***	(6.52)***	(9.35)***	(4.39)***
Daily Return	0.048	0.293	0.037	0.224	-0.023	0.045
	(4.61)***	(7.75)***	(3.90)***	(5.91)***	(1.99)**	(0.95)
Weekly Return	-0.079	-0.099	-0.086	-0.137	-0.105	-0.132
	(11.81)***	(4.65)***	(12.28)***	(6.14)***	(11.13)***	(4.72)***
Monthly Return	-0.078	-0.062	-0.062	-0.052	-0.073	-0.102
	(19.04)***	(4.25)***	(14.07)***	(3.47)***	(12.90)***	(6.20)***
6-Month Return	-0.011	-0.020	-0.006	-0.014	-0.007	-0.009
	(7.87)***	(4.12)***	(4.01)***	(2.49)**	(3.52)***	(1.38)
Variance	0.152	-0.395	0.394	0.023	0.354	-0.806
	(0.97)	(0.71)	(3.41)***	(0.05)	(3.27)***	(1.69)*
Daily Return^2	-0.001	0.027	-0.042	-0.046	-0.011	0.056
	(0.10)	(0.73)	(3.99)***	(0.82)	(1.24)	(1.43)
Weekly Return^2	0.046	0.068	0.014	0.046	0.016	0.080
	(3.88)***	(1.01)	(4.16)***	(2.45)**	(3.79)***	(3.93)***
Market Cap.	0.000	0.000	0.000	0.000	0.000	0.000
	(1.95)*	(2.24)**	(2.05)**	(2.89)***	(1.62)	(1.86)*
Turnover	0.310	1.965	0.164	1.434	-0.022	1.260
	(6.11)***	(5.18)***	(3.86)***	(5.20)***	(0.43)	(5.06)***
R^2	0.04	0.05	0.04	0.06	0.07	0.10
N	1,417,182	1,417,182	923,915	923,915	300,080	300,080

Table 3: Retail Trading in Response to Revisions: Daily Specifications

In this table we regress daily retail trading on lagged daily values of analysts' revisions and controls. The revisions are lagged one day relative to the trading. For days with no revisions, we set the revision value equal to zero. The retail trading variables are *Retail Direction* and *Retail Magnitude*. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. We include lagged values of the retail trading variables in each regression. The lagged trading variables are measured at day *t-2*, so as not to coincide with the revision variables, which are measured on day *t-1*. *EPS Revision* is the new EPS forecast minus the previous EPS forecast, scaled by the stock price measured on the day before the new EPS forecast. *Price Target Revision* is the new 12-month price target scaled by the previous day's stock price minus the previous 12-month stock price forecast scaled by its previous day stock price. *Recommendation Revision* is the new recommendation minus the previous recommendation. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over the previous 20 days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. We exclude firms that don't have at least one revision during our sample period. We exclude retail trading observations for which there is an earnings announcement over any of the three previous days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The sample period begins in October 2006 and ends in December 2019.

Table 3: (Continued)

	Retail Direction	*Retail Direction*	*Retail Magnitude*	*Retail Magnitude*
EPS Revision	-0.003	-0.001	-0.000	-0.000
	(3.84)***	(1.81)*	(2.26)**	(0.96)
Price Tgt. Rev.	0.026	0.017	0.002	0.001
	(8.08)***	(4.95)***	(4.68)***	(2.06)**
Rec. Revision	0.005	0.005	0.000	0.000
	(7.37)***	(6.88)***	(5.24)***	(4.48)***
Lagged Retail	0.048	0.047	0.024	0.024
	(77.34)***	(76.06)***	(33.00)***	(32.46)***
Daily Return		-0.002		0.001
		(0.27)		(0.83)
Weekly Return		-0.091		-0.010
		(24.17)***		(22.11)***
Monthly Return		-0.044		-0.003
		(14.12)***		(10.62)***
6-Month Return		-0.000		-0.000
		(1.38)		(4.00)***
Variance		0.110		0.005
		(2.97)***		(2.32)**
Daily Return^2		0.004		0.000
		(0.95)		(0.30)
Weekly Return^2		0.014		0.002
		(3.99)***		(3.89)***
Market Cap.		0.000		0.000
		(2.63)***		(3.48)***
Turnover		0.001		0.000
		(1.07)		(1.40)
R^2	0.01	0.01	0.01	0.01
N	17,498,562	16,381,226	17,462,222	16,351,826

Table 4: Retail Trading in Response to Revisions: Daily Specifications and Revision Dummies

In this table we regress daily retail trading on lagged daily values of analysts' revisions and controls. The revisions are lagged one day relative to the trading. For days with no revisions, we set the revision value equal to zero. The retail trading variables are *Retail Direction* and *Retail Magnitude*. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. We include lagged values of the retail trading variables in each regression. The lagged trading variables are measured at day *t-2*, so as not to coincide with the revision variables, which are measured on day *t-1*. Up EPS (Down EPS) is equal to 1 if there is an EPS revision in the 90^{th} (10^{th}) percentile and zero otherwise. Up Target (Down Target) is equal to 1 if there is an price target revision in the 90^{th} (10^{th}) percentile and zero otherwise. Up Rec. (Down Rec.) is equal to 1 if *Recommendation Revision* is positive (negative) and zero otherwise. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over the previous 20 days. *Daily Return^2* and *Weekly Return^2* are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. We exclude firms that don't have at least one revision during our sample period. We exclude retail trading observations for which there is an earnings announcement over any of the three previous days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The bottom row reports *p*-values from *F*-tests of whether the difference between the *Up* and *Down* coefficients are statistically significant. The coefficients in the *Retail Magnitude* regressions are multiplied by 100 for readability. The sample period begins in October 2006 and ends in December 2019.

Table 4: (Continued)

	Retail Direction	*Retail Direction*	*Retail Magnitude*	*Retail Magnitude*
EPS Up	0.002	0.003	0.011	0.017
	(2.99)***	(4.37)***	(1.87)*	(2.71)***
Target Up	0.014	0.011	0.092	0.059
	(7.32)***	(5.65)***	(4.69)***	(2.93)***
Rec. Up	0.008	0.008	0.057	0.054
	(6.05)***	(5.61)***	(5.04)***	(4.53)***
EPS Down	0.008	0.006	0.057	0.041
	(12.02)***	(9.07)***	(8.90)***	(6.10)***
Target Down	-0.001	0.002	0.000	0.032
	(0.40)	(1.09)	(0.01)	(1.53)
Rec. Down	-0.007	-0.006	-0.034	-0.025
	(5.08)***	(4.45)***	(2.87)***	(2.02)**
Lagged Retail	0.048	0.047	2.394	2.387
	(77.34)***	(76.06)***	(33.00)***	(32.46)***
Daily Return		-0.002		0.080
		(0.25)		(0.84)
Weekly Return		-0.091		-1.049
		(24.17)***		(22.11)***
Monthly Return		-0.044		-0.288
		(14.11)***		(10.61)***
6-Month Return		-0.000		-0.000
		(1.38)		(3.99)***
Variance		0.110		0.540
		(2.97)***		(2.32)**
Daily Return^2		0.004		0.023
		(0.93)		(0.30)
Weekly Return^2		0.014		0.159
		(3.99)***		(3.89)***
Market Cap.		0.000		0.000
		(2.62)***		(3.48)***
Turnover		0.001		0.014
		(1.07)		(1.40)
R^2	0.01	0.01	0.01	0.01
N	17,498,562	16,381,226	17,462,222	16,351,826
EPS: Up-Down	0.00	0.00	0.00	0.00
Target: Up-Down	0.00	0.00	0.00	0.00
Rec: Up-Down	0.00	0.00	0.00	0.00

Table 5: Retail Trading in Response to Revisions: The Effects of All-Star Analysts

In this table we regress daily retail trading on lagged values of analysts' revisions, revisions interacted with an All-Star Analyst dummy, and controls. The unit of observation is a revision. The revisions are lagged one day relative to the retail trading. The All-Star dummy equals 1 if the analyst was named an All-Star or runner up in the previous year, and zero otherwise. The retail trading variables are *Retail Direction* and *Retail Magnitude*. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. *EPS Revision* is the new EPS forecast minus the previous EPS forecast, scaled by the stock price measured on the day before the new EPS forecast. *Price Target Revision* is the new 12-month price target scaled by the day *t-1* stock price minus the previous price target scaled by its *t-1* stock price. *Recommendation Revision* is the new recommendation minus the previous recommendation. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over previous do days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. We exclude retail trading observations for which there is an earnings announcement over any of the three previous days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The sample period begins in October 2006 and ends in December 2019.

Table 5: (Continued)

	EPS Forecast Revisions		**Price Target Revisions**		**Recommendations Revisions**	
	Retail Direction	*Retail Magnitude*	*Retail Direction*	*Retail Magnitude*	*Retail Direction*	*Retail Magnitude*
Revision	-0.028	-0.157	0.018	0.030	0.005	0.005
	(2.75)***	(5.55)***	(9.53)***	(6.12)***	(9.14)***	(4.08)***
*Revision * AS*	0.103	0.188	-0.004	0.012	0.005	0.019
	(2.13)**	(1.78)*	(0.46)	(0.51)	(1.96)**	(3.78)***
All-Star (AS)	-0.000	0.005	0.002	0.002	0.001	0.003
	(0.12)	(2.30)**	(1.31)	(1.10)	(0.34)	(0.68)
Daily Return	0.047	0.293	0.036	0.222	-0.023	0.044
	(4.55)***	(7.75)***	(3.80)***	(5.86)***	(2.02)**	(0.94)
Weekly Return	-0.080	-0.098	-0.088	-0.140	-0.105	-0.133
	(11.92)***	(4.60)***	(12.48)***	(6.25)***	(11.16)***	(4.74)***
Monthly Ret.	-0.078	-0.061	-0.062	-0.051	-0.073	-0.102
	(18.94)***	(4.20)***	(13.98)***	(3.35)***	(12.87)***	(6.19)***
6-Month Ret.	-0.011	-0.020	-0.006	-0.014	-0.007	-0.009
	(7.80)***	(4.07)***	(4.04)***	(2.49)**	(3.51)***	(1.37)
Variance	0.165	-0.348	0.405	-0.013	0.353	-0.809
	(1.05)	(0.62)	(3.48)***	(0.03)	(3.26)***	(1.69)*
Daily Return^2	-0.001	0.023	-0.041	-0.032	-0.011	0.056
	(0.15)	(0.63)	(3.64)***	(0.55)	(1.23)	(1.43)
Weekly Ret^2	0.046	0.069	0.014	0.049	0.016	0.080
	(3.88)***	(1.01)	(3.98)***	(2.46)**	(3.81)***	(3.94)***
Market Cap.	0.000	0.000	0.000	0.000	0.000	0.000
	(1.94)*	(2.21)**	(2.03)**	(2.90)***	(1.63)	(1.86)*

Turnover	0.309	1.958	0.164	1.442	-0.022	1.261
	(6.33)***	(5.15)***	(3.91)***	(5.19)***	(0.43)	(5.07)***
R^2	0.04	0.05	0.04	0.06	0.07	0.10
N	1,417,124	1,417,124	923,798	923,798	300,080	300,080

Table 6: "Tipping" or Trading in Anticipation of Revisions

The dependent variable in these regressions is one of the revision variables: EPS forecast, price target, or recommendations. We regress revisions on lagged values of either *Retail Direction* or *Retail Magnitude* for each of the previous 5 trading days. The retail trading variables are *Retail Direction* and *Retail Magnitude*. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. We include lagged daily stock returns for each of the past 5 days, and lagged daily returns squared for each of the past 5 days, market capitalization, and turnover as controls. For the sake of brevity, we do not report the control variables' coefficients. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over the previous 20 days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. The regressions have firm and time fixed effects, and standard errors that are clustered on firm and time. We exclude revisions that are on an earnings announcement day, or follow an earnings announcement over the previous 2 days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The sample period begins in October 2006 and ends in December 2019.

Table 6: (Continued)

	EPS Revision		Price Target Revision		Recommendation Revision	
	Retail Direction	*Retail Magnitude*	*Retail Direction*	*Retail Magnitude*	*Retail Direction*	*Retail Magnitude*
Retail Trading$_{t\text{-}1}$	102.228	-9,187.477	0.007	0.076	0.027	0.007
	(0.28)	(1.84)*	(8.56)***	(5.60)***	(3.97)***	(0.10)
Retail Trading$_{t\text{-}2}$	-26.400	-2,972.252	0.007	0.052	0.009	0.043
	(0.17)	(0.45)	(9.13)***	(4.11)***	(1.34)	(0.64)
Retail Trading$_{t\text{-}3}$	171.690	3,113.387	0.004	0.039	0.002	-0.062
	(0.84)	(0.85)	(5.98)***	(3.21)***	(0.28)	(0.94)
Retail Trading$_{t\text{-}4}$	73.106	-2,645.388	0.005	0.012	0.005	-0.035
	(0.34)	(0.37)	(6.51)***	(0.95)	(0.75)	(0.51)
Retail Trading$_{t\text{-}5}$	11.116	1,314.990	0.005	0.033	0.016	0.082
	(0.09)	(0.80)	(6.52)***	(2.64)***	(2.50)**	(1.18)
R^2	0.02	0.02	0.08	0.08	0.06	0.06
N	1,403,992	1,403,682	889,744	889,588	312,510	312,438

Table 7: Retail Trading, Analysts' Revisions, and Stock Returns

This table reports regression results of 20-day stock returns on lagged values of analysts' revisions, retail trading, and controls. Retail trading is measured on day *t-1*, while the revision variables are measured on day *t-2*. The control variables are all measured relative to day *t-2* as well. For days with no revisions, we set the revision value equal to zero. *Retail Direction* is equal to: (Retail Buy Volume – Retail Sell Volume) / (Retail Buy Volume + Retail Sell Volume). *Retail Magnitude* is equal to: (Retail Buy Volume – Retail Sell Volume) / Total Volume. *EPS Revision* is the new EPS forecast minus the previous EPS forecast, scaled by the stock price measured on the day before the new EPS forecast. *Price Target Revision* is the new 12-month price target scaled by the previous day's stock price minus the previous EPS forecast scaled by its previous day's stock price. *Recommendation Revision* is the new recommendation minus the previous recommendation. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over the previous 20 days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. We exclude firms that don't have at least one revision during our sample period. We exclude observations with an earnings announcement over any of the three previous days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The sample period begins in October 2006 and ends in December 2019.

Table 7: (Continued)

Retail Direction		0.330		0.330	
		(20.34)***		(20.34)***	
Retail Magnitude			1.253		1.253
			(11.44)***		(11.43)***
EPS Revision	0.372			0.372	0.372
	(5.79)***			(5.79)***	(5.79)***
Price Target Revision	1.182			1.176	1.178
	(4.60)***			(4.58)***	(4.60)***
Rec. Revision	0.213			0.211	0.211
	(7.03)***			(6.98)***	(6.97)***
Daily Return	-6.134	-6.076	-6.017	-6.134	-6.075
	(6.24)***	(6.20)***	(6.25)***	(6.25)***	(6.30)***
Weekly Return	-3.558	-3.527	-3.502	-3.528	-3.503
	(5.03)***	(4.99)***	(5.06)***	(5.00)***	(5.07)***
Monthly Ret.	-0.998	-0.985	-1.045	-0.983	-1.044
	(0.80)	(0.79)	(0.86)	(0.79)	(0.86)
6-Month Ret.	0.005	0.005	0.004	0.005	0.004
	(0.79)	(0.80)	(0.41)	(0.80)	(0.41)
Variance	12.125	12.086	12.229	12.086	12.228
	(2.14)**	(2.13)**	(2.18)**	(2.13)**	(2.17)**
Daily Return^2	0.285	0.273	0.260	0.284	0.271
	(0.92)	(0.89)	(0.85)	(0.92)	(0.89)
Weekly Ret^2	0.294	0.289	0.289	0.289	0.289
	(1.81)*	(1.79)*	(1.80)*	(1.79)*	(1.80)*
Market Cap.	-0.000	-0.000	-0.000	-0.000	-0.000
	(3.25)***	(3.25)***	(3.25)***	(3.25)***	(3.25)***
Turnover	-0.624	-0.625	-0.624	-0.625	-0.624
	(3.62)***	(3.62)***	(3.63)***	(3.62)***	(3.63)***
R^2	0.15	0.15	0.15	0.15	0.15
N	16,203,510	16,203,510	16,186,824	16,203,510	16,186,824

Table 8: Retail Trading and Stock Returns On and Off Revision Days

This table reports regression results of 20-day stock returns on analysts' revisions and controls within different subsamples that are based on values of *Retail Direction*. *Retail Direction* is measured on day *t-1* relative to the 20-day future stock return, which is measured on day *t*. The revision and control variables are measure on or relative to day *t-2*. *EPS Revision* is the new EPS forecast minus the previous EPS forecast, scaled by the stock price measured on the day before the new EPS forecast. *Price Target Revision* is the new 12-month price target scaled by the previous day's stock price minus the previous EPS forecast scaled by its previous day's stock price. *Recommendation Revision* is the new recommendation minus the previous recommendation. *Daily*, *Weekly*, *Monthly*, and *6-Month* returns are the total stock returns measured over the stated period. *Variance* is the variance of daily returns measured over the previous 20 days. *Daily Return*^{2} and *Weekly Return*^{2} are the daily and weekly stock returns squared. *Market Cap.* is price x shares outstanding, reported in millions. *Turnover* is the average daily turnover (shares traded / shares outstanding) over the last 20 days. We exclude firms that don't have at least one revision during our sample period. We exclude observations with an earnings announcement over any of the three previous days. The regressions include firm and time fixed effects and the standard errors are clustered on firm and time. The sample period begins in October 2006 and ends in December 2019.

Table 8: (Continued)

	Retail Direction>0	Retail Direction<0	Retail Direction>90$^{th\%ile}$	Retail Direction<10$^{th\%ile}$
EPS Revision	0.286	0.473	0.373	0.492
	(3.34)***	(6.95)***	(1.50)	(2.85)***
Price Target Revision	0.917	1.217	3.114	1.143
	(2.86)***	(4.57)***	(2.18)**	(1.20)
Rec. Revision	0.234	0.182	0.437	0.441
	(5.65)***	(5.00)***	(3.44)***	(3.96)***
Daily Return	-6.263	-5.170	-8.533	-7.248
	(7.42)***	(6.82)***	(4.24)***	(4.30)***
Weekly Return	-3.423	-3.053	-6.403	-5.857
	(6.58)***	(6.42)***	(3.81)***	(3.70)***
Monthly Ret.	-1.934	-1.832	-1.425	-1.610
	(3.46)***	(4.26)***	(0.62)	(0.94)
6-Month Ret.	-0.021	0.008	0.003	0.013
	(0.58)	(0.61)	(0.31)	(3.95)***
Variance	14.017	5.063	40.964	23.008
	(2.36)**	(1.58)	(7.91)***	(3.21)***
Daily Return^2	0.013	0.776	2.998	-0.048
	(0.04)	(2.70)***	(0.72)	(0.16)
Weekly Ret^2	0.055	0.454	3.079	2.798
	(0.84)	(3.26)***	(1.55)	(3.31)***
Market Cap.	-0.000	-0.000	-0.000	-0.000
	(3.06)***	(3.38)***	(1.99)**	(2.43)**
Turnover	-0.600	-0.581	-1.898	-6.608
	(3.69)***	(4.30)***	(1.06)	(2.05)**
R^2	0.15	0.16	0.14	0.14
N	7,148,654	7,849,057	1,676,913	1,676,036