Three Papers on Heterogeneous Investors and Their Financial Market Implications

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Declaration

This thesis contains no material that has been accepted for the award of any other degree or diploma in any university. To the best of the author's knowledge, it contains no material previously published or written by another person, except where due reference is made in the text.

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Abstract

Financial market equilibrium is shaped by heterogeneous investors. Their distinct sophistication and incentive significantly influence market efficiency. This dissertation comprises three papers that examine the roles of distinct investors across different financial markets.

The first paper investigates how unsophisticated retail investors affect ETF price efficiency. Decomposing close-to-close ETF mid-quote returns into overnight and intraday components, we document a robust pattern: overnight returns are significantly positive, whereas intraday returns are negative. This return differential is pervasive across ETFs tracking diverse asset classes and regions and cannot be explained by differences in risk, macroeconomic news, or information asymmetry. Our evidence indicates that retail investor demand shocks, coupled with limited arbitrage supply, drive this pattern. These findings suggest that retail-driven demand generates abnormal price fluctuations, imposing a hidden cost on intraday ETF investors.

The second paper examines the impact of opportunistic "patent trolls" on firms' cash holdings. We construct a novel measure of patent troll litigation risk using the digital footprint of trolls viewing financial statements on the SEC EDGAR platform. Exploiting the staggered adoption of anti-troll laws, we find that firms facing greater litigation risk strategically reduce cash holdings to deter lawsuits. Cash flow analyses reveal that these adjustments are implemented primarily through financing activities, with operating and investing activities largely unaffected. Overall, our results highlight patent troll litigation risk as an important determinant of firms' cash and financing policies.

The third paper explores how active fund managers respond to incentive compensation reforms. Using China's mutual fund compensation reform, which introduced bonus deferral requirements, we find that while the reform successfully curbs excessive risk-taking, it also unintentionally reduces managerial effort, harming fund performance in some cases. Cross-sectional analyses show heterogeneous effects: for funds previously engaged in high-risk-low-return strategies, the benefits of reduced risk-taking dominate,

improving investors' risk and return tradeoffs. In contrast, funds with high-risk-high-return strategies experience performance declines, as reduced effort outweighs the benefits of lower risk. These results underscore the importance of designing compensation policies that balance risk discipline with managerial incentives to value creation in the fund industry.

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Chapter 1

A Hidden Cost of ETF Investing: Retail Demand Shocks and Limits to Arbitrage

Abstract – By decomposing close-to-close mid-quote returns of ETFs into their overnight and intraday components, we find that the overnight return is significantly positive, whereas the intraday return is negative. This overnight–intraday return differential is ubiquitous across ETFs tracking different asset classes or assets located in different time zones. This phenomenon cannot be explained by differences in overnight and intraday risks, macro-economic announcements, or information asymmetry. Instead, our analysis reveals that the return pattern is primarily driven by demand shocks from retail investors and limited supply from arbitrageurs. These results indicate that the convenience of buying ETFs during intraday trading hours carries a hidden cost to investors.

Keywords: Exchange-Traded Fund, ETF, Market Efficiency, Overnight Returns, Intraday Returns, Financial Friction, Retail Investors.

JEL classification: G12, G14, G23, N22

1 Introduction

Since the introduction of the first exchange-traded fund (ETF) in the mid-1990s, ETFs have become one of the most popular financial instruments in the capital market. As shown in Figure 1, nearly $7.2 trillion had been invested in the ETF market in the United States by 2021. Many studies attribute the success of ETFs to their ease of access, as ETFs are traded on stock exchanges much like individual stocks (Ben-David, Franzoni, and Moussawi, 2017; Huang, O'Hara, and Zhong, 2020). Unlike traditional mutual funds, which can only be bought at the end-of-day price, ETFs offer investors the flexibility to purchase them anytime during the intraday trading hours.

However, a growing body of research highlights the hidden costs associated with ETF investing. For example, Piccotti (2018) documents that the trading prices of ETFs can deviate significantly from the value of their underlying assets due to market segmentation. Broman (2016) shows such price deviation co-moves across different ETFs, creating additional risk for investors. An, Benetton, and Song (2023) find that index providers charge high markups to ETFs and the extra cost does not translate into better performance. This paper adds to this literature by identifying an additional source of costs. Specifically, the convenience of buying ETFs during trading hours exposes investors to intraday risks without offering proportional returns.

We first document that this hidden cost arises from the overnight–intraday return difference in the ETF market. Following Lou, Polk, and Skouras (2019), we define the overnight return as the return from the market close to the subsequent market open and the intraday return as the return from the market open to the market close.[1] Figure 2

[1] To avoid potential confounding effects from bid-ask bounce, we use the mid-quote price to measure intraday and overnight returns. Hence, the cost that we study is above and beyond transaction costs, such as bid-ask spreads. This design distinguishes our paper from the prior literature.

plots the time-series of these returns and reveals a striking pattern: overnight returns are consistently positive—even during the global financial crisis—while the intraday returns are often negative, creating a nontrivial gap between the two. The average overnight and intraday return difference of all ETFs in our sample from 2004 to 2021 is 4.2 basis points per day or 0.93% per month.[2] After testing several hypotheses aimed at explaining this phenomenon, we show that this return differential is the result of excess demand from retail investors and limited supply from arbitrageurs. As a result, investors who purchase ETFs during intraday hours—especially at the market open—pay higher prices for ETFs, which in turn drives down intraday returns.

We first test whether differential risk levels during the day and night drive the overnight–intraday return differential in the ETF market. In an efficient market, if holding assets overnight entails greater risk than intraday trading, overnight returns would be higher than intraday ones, leading to a persistent return difference. However, our findings go against this hypothesis. We do not find any significant difference in the volatility of intraday and overnight returns, as shown in Figure 3. When we sort our sample ETFs based on their intraday and overnight risk measures, we find inconsistent evidence with the risk-based theories.

Moreover, we show that the overnight–intraday return differential cannot be explained by the underlying asset class, trading venue, or special calendar effects. ETFs tracking equities, fixed-income, and alternative asset classes all exhibit substantial return differentials. Similarly, ETFs tracking non-US equities display similar patterns compared to ETFs that track US equities, despite their underlying assets being traded in different time

[2] Lou, Polk, and Skouras (2019) find that during their sample period, the Center for Research in Security Prices (CRSP) market portfolio has an overnight–intraday return difference of 0.17% per month, or slightly less than one basis point per day, which is much lower than the return difference we document in the ETF market.

zones. These findings suggest that the overnight–intraday return differential is not due to the differences in the timing of information releases about underlying assets, which is inconsistent with information-based explanations of the return differential (e.g., Slezak (1994), Hong and Wang (2000)). ETFs traded on both the NYSE and NASDAQ have significant overnight–intraday return differentials, suggesting that the trading venue is not a major contributing factor. Furthermore, the overnight–intraday return differential is robust on days without major macroeconomic news releases, indicating that it is not driven by macroeconomic uncertainty.

We then turn to demand-based asset pricing theories to explain our findings. A growing literature finds that the demand shocks of various types of investors, such as mutual funds and retail investors, have first-order effects on asset prices and returns because the asset supplies are not perfectly elastic (Lou, 2012; Koijen and Yogo, 2019; Ben-David et al., 2022; Huang, Song, and Xiang, 2025). If, for some reason, there is excess demand to purchase ETFs during intraday hours and there is not enough supply of ETFs due to various constraints, the price of ETFs would be artificially inflated during the day, resulting in lower intraday returns. Building on these arguments, we hypothesize that the differences in overnight and intraday returns are driven by a joint effect of unsophisticated retail investor demand and the existence of arbitrage constraints.

To test our hypothesis, we exploit unique features in the ETF market, such as observable fund flows and information on authorized participants. These features allow us to construct indices to proxy for investors' demand and arbitrageur supply constraints, respectively. We then sort our sample ETFs into portfolios based on proxies of retail demand and arbitrage constraints. The portfolio analysis highlights the roles of retail demand and arbitrage constraints in explaining the overnight–intraday return differences.

Notably, an ETF portfolio with the lowest retail demand and lowest arbitrage constraints shows a return difference of only 0.31% per month, whereas a portfolio with the highest retail demand and highest arbitrage constraints shows a return difference of 1.99% per month—six times higher. The difference between these two portfolios is statistically significant and economically large. We further explore the impact of each channel. ETFs with high arbitrage constraints have 0.69% higher overnight–intraday return differential than ETFs with low arbitrage constraints. This shows that both demand and supply factors affect the overnight and intraday return differences.

In another test, we control for the fundamental value of ETFs using information on their underlying benchmarks and draw a direct comparison of ETFs tracking the same benchmark. In this test, we use panel regressions and include benchmark-time fixed effects to control for the influence of the common drivers of ETF values. Even after accounting for the effect of the underlying assets, the positive relationships between the overnight–intraday return difference and the proxies of investor demand and arbitrage constraints remain strong. This suggests that retail demand and arbitrage constraints can cause ETF prices to temporarily deviate from their fundamental values.

To further shed light on the investor demand channel and provide a causal interpretation of our findings, we analyze the effect of economic impact payments (EIPs) during the COVID-19 pandemic as exogenous shocks that increase retail investor demand. We find that during the months when US households receive payments from the government, the overnight–intraday return differential increases, especially among ETFs that are popular among retail investors. We also apply the algorithm developed by Boehmer et al. (2021) to measure retail investors' order imbalances near the market open and market close using intraday trading data from TAQ. This exercise shows that retail order imbalances near the market open significantly influence the overnight–intraday return differentials, providing direct evidence on the retail demand channel.

Our paper first contributes to ongoing debates on the cost of ETF investing. The traditional view considers the ETF market with low costs of trading and holding (Ben-David, Franzoni, and Moussawi, 2018; Cong and Xu, 2017). However, recent studies highlight significant hidden costs in the ETF market. ETFs' trading price can deviate from the fundamental value of the underlying assets, especially when the assets are illiquid (Petajisto, 2017; Angel, Broms, and Gastineau, 2016; Piccotti, 2018). In addition, mispricing (measured as the difference between the trading price and net asset value) in one ETF may correlate with that in other ETFs with similar investment styles (Broman, 2016). Moreover, index providers impose high markups on ETFs, which are passed on to investors, as documented by An, Benetton, and Song (2023), but do not generate additional performance for investors. Huang, Song, and Xiang (2024) find that smart beta ETFs artificially inflate their performance "on paper" before listing and suffer poor performance after listing. Our study adds to the literature by documenting another cost of ETF investment.

Our paper also adds to the literature studying intraday and overnight trading and return patterns of risky asset classes. Earlier studies focus on intraday variations in volatility, trading volume, and bid-ask spreads in the stock market and show these variables exhibit a U-shaped pattern during trading hours (Lockwood and Linn, 1990; Stoll and Whaley, 1990; Chan, Christie, and Schultz, 1995). More recent research has shifted attention to the return patterns themselves. Berkman et al. (2012) find that attention-grabbing stocks tend to have inflated open prices, resulting in low intraday returns. Lou, Polk, and Skouras (2019) find that different types of stocks have specific clienteles that prefer to trade intraday or overnight, resulting in inflated open or close prices depending on a stock's characteristics. Bogousslavsky (2021) documents that institutional constraints and overnight risk incentivize arbitrageurs to stop arbitrage near the market close and generate abnormal return dynamics near the market close. Muravyev and Ni (2020) find that delta-hedged option portfolios earn negative overnight returns and positive intraday returns. We show that the overnight–intraday return differential is large and widespread in the ETF market, and the unique features of ETFs allow us to identify the underlying causes of the phenomenon.

A paper closely related to ours is Lachance (2021). She focuses on understanding ETFs' high overnight return that is measured based on the trade price. She finds that a positive order imbalance and widening bid-ask spreads at the market open inflate the overnight return because the open price is more likely to be at the ask price, and the overnight return is distorted by the bid-ask bounce. Our study complements Lachance (2021) by examining the mid-quote-based overnight–intraday return difference and focusing on the drivers of the overnight–intraday return difference beyond the impact of the bid-ask bounce. We also calculate overnight and intraday returns using average mid-quotes during the first and last five minutes of regular trading hours to further minimize the influence of other microstructure frictions. Moreover, our paper examines various hypotheses beyond microstructure frictions and further highlights that the overnight–intraday return difference is a source of the cost of ETF investment.

Finally, our paper contributes to the literature on the role and performance of retail investors. While one strand of research views retail investors as noise traders who typically incur losses (Barber and Odean, 2000), another strand argues that their trading is not always detrimental (e.g., Kaniel, Saar, and Titman (2008), Kelley and Tetlock (2013) and Kelley and Tetlock (2016)). Our study adds to the literature by demonstrating that in the ETF market, retail investors' nonfundamental demand shocks contribute to the overnight and intraday return differences, highlighting the role of retail trading on market frictions.

The remainder of the paper is structured as follows. Section 2 outlines our hypothesis. Section 3 details the sample selection, variable construction, and summary statistics. Section 4 presents the empirical analyses. Section 5 concludes.

2 Hypothesis Development

We examine three testable hypotheses to explain why overnight returns differ from intraday returns. The first is the risk-return trade-off theory, which posits that returns should compensate for risk. If holding assets overnight entails greater risk than during intraday trading, overnight returns should be higher to offset this additional risk, leading to a higher return difference. This hypothesis assumes an efficient market in which new information is swiftly incorporated into stock prices, ensuring that extra risk is adequately compensated.

The second hypothesis, based on information considerations, may provide alternative explanations for the differences between overnight and intraday returns (e.g., Slezak (1994) and Hong and Wang (2000)). Slezak (1994)'s model shows that market closures delay uncertainty resolution and redistribute risk over time. When the market is closed, informed investors cannot trade on their overnight information and face greater risk than during intraday trading. To mitigate this concern, they may compete to sell their positions before the market closes, leading to a discounted closing price. Under this hypothesis, ETFs with a higher concentration of informed or sophisticated investors would experience deflated closing prices, resulting in higher overnight returns.

Finally, beyond classic models that assume perfectly elastic asset supply and rational expectation among all investors, some studies suggest that investor demand plays a crucial role in driving the differences between overnight and intraday returns. Berkman et al. (2012) find that high-attention stocks exhibit positive overnight returns and negative intraday returns, as retail investors bid up the opening price. Similarly, Lou, Polk, and Skouras (2019) show that retail investors typically trade near the market open, while institutional investors trade near the market close, causing overnight and intraday returns to reflect the trading patterns of these distinct investor groups. Moreover, Bogousslavsky (2021) emphasizes the role of arbitrage constraints, arguing that arbitrageurs—facing overnight risk and borrowing costs—may avoid arbitraging near the market close, resulting in systematically biased closing prices.

Building on these findings, we hypothesize that the differences between overnight and intraday returns are driven by retail demand and arbitrage constraints. Retail investors, who prefer to buy near the market open (Lou, Polk, and Skouras, 2019), may bid up the opening price. However, arbitrageurs, constrained by factors such as inventory limits, may not be able to immediately correct this mispricing and must instead unwind it gradually throughout the trading day. A unique feature in the ETF market is that if mispricing persists near the market close, authorized participants (AP) can step in to exploit residual arbitrage opportunities through in-kind creation and redemption mechanisms. Under this hypothesis, closing prices of ETFs should be more efficient, as arbitrage activity restores pricing accuracy by the end of the day. Therefore, ETFs with higher retail investor demand and greater arbitrage constraints should exhibit larger overnight–intraday return differences, driven by inflated opening prices that take time to correct.

This paper tests the following three hypotheses:

Hypothesis 1 (H1): The overnight–intraday return difference is the outcome of the risk and return trade-off. The return difference is positively correlated with the overnight risk and negatively correlated with the intraday risk.

Hypothesis 2 (H2): The overnight–intraday return difference is driven by information asymmetry. The return difference is positively correlated with the proportion of informed investors.

Hypothesis 3 (H3): The overnight–intraday return difference is driven by retail demand and arbitrage constraints. Retail demand and arbitrage constraints positively predict the return difference.

3 Data and Summary Statistics

This section describes our data, specifies how we measure returns, and presents summary statistics of key variables in this paper.

3.1 Data Sources

Our data are obtained from five sources, including the Center for Research in Security Prices Mutual Fund (CRSPMF) database, the CRSP Daily Stock (CRSPSTOCK) database, the TAQ database, the Morningstar database, and Thomson Reuters s34 filings. We acquire net asset value (NAV), total net assets (TNA), quarterly holdings, inception date, ETF identifier, passive fund identifier, and investment style from CRSPMF. The daily return, open price, close price, close-ask, close-bid, trading volume, share outstanding, and the factor to adjust price data are derived from CRSPSTOCK. We use TAQ data to acquire the national best bid and offer (NBBO) midpoint quotes and retail investors' order imbalances. The ETF benchmark and AP information are from the Morningstar database. The institutional holding data of ETFs are collected from Thomson Reuters.

To construct our sample, we start with the CRSPMF database. First, we exclude all non-ETF observations with the "ETF_flag" equal to "F". Then, we merge the fund data from the CRSPMF database with ETFs' trading prices from the CRSPSTOCK database using the FUNDNO-PERMNO map provided by WRDS. Next, we extract the first and last 5-minute mid-quotes from TAQ and merge these mid-quotes with the CRSP data. To ensure accuracy, we manually validate these links. We also require all observations to have both valid open and close prices for a day to ensure return comparability, which filters out approximately 6% of observations from our sample. Finally, we require our sample ETFs to have a lifespan of more than one year for estimations of beta and past-year cumulative return.

Our final sample contains 2,916 unique US ETFs from January 2004 to December 2021. The sample period is limited by the earliest record of the FUNDNO-PERMNO map provided by WRDS. Our sample includes not only liquid equity ETFs but also less liquid fixed-income, currency, and mortgage-backed ETFs, providing a comprehensive view of the ETF market. Taken together, our sample accounts for nearly $7.04 trillion, or 98% of net assets invested in the US ETF market at the end of 2021.

3.2 Overnight and Intraday Returns

We compute intraday and overnight returns based on the average midpoints of the NBBO quotes in the first and last 5-minute intervals during regular trading hours. The first 5-

minute interval spans from 9:30 am to 9:35 am, while the last 5-minute interval typically runs from 3:55 pm to 4:00 pm.

The daily overnight and intraday returns are calculated as follows:

$$r^i_{intraday,t} = \frac{P^i_{close,t}}{P^i_{open,t}} - 1 \tag{1}$$

$$r^i_{close-to-close,t} = \frac{\frac{P^i_{close,t}}{CFACPR^i_t} + \frac{Div^i_t}{CFACPR^i_t/FACPR^i_t}}{\frac{P^i_{close,t-1}}{CFACPR^i_{t-1}}} - 1 \tag{2}$$

$$r^i_{overnight,t} = \frac{1 + r^i_{close-to-close,t}}{1 + r^i_{intraday,t}} - 1, \tag{3}$$

where $P^i_{close,t}$ is the average of NBBO midpoints during the last five minutes before the market close, and $P^i_{open,t}$ is the average of NBBO midpoints during the first five minutes after the market open. Div^i_t, $CFACPR^i_t$, and $FACPR^i_t$ refer to the dividend payment, the cumulative factor to adjust the price, and the factor to adjust the price for stock i on day t, respectively.

As TAQ may contain inaccurate records, Lee and Ready (1991) suggest applying the following filters: (1) all quotes must have positive bid and offer prices; (2) the quote sizes must be positive; (3) the bid-ask spread cannot exceed 5% of the quote's midpoint; and (4) the offer price must exceed the bid price. In addition, following Bogousslavsky (2021), if the absolute difference between the daily close-to-close return calculated from TAQ data and the daily return reported by CRSP exceeds 25%, we substitute the CRSP return for the mid-quote return.

We accumulate the daily intraday and overnight returns within a month to calculate the monthly return. To ensure the monthly return data with a different number of trading days are comparable, we make two adjustments: first, we require all observations to have at least ten recordings within one month; second, we standardize the monthly return by converting the geometric mean of the daily returns in a month using the following formula:

$$r^i_{intraday,m} = \left[\prod_{t\in m}(1 + r^i_{intraday,t})\right]^{21/n} - 1 \tag{4}$$

$$r^i_{overnight,m} = \left[\prod_{t\in m}(1 + r^i_{overnight,t})\right]^{21/n} - 1, \tag{5}$$

where n is the number of observations within month m.

For the majority of our analyses, we use portfolio returns. We define the portfolio return as follows:

$$r^p_{close-to-close,m} = \sum_{i\in p} w^i_{m-1} r^i_{close-to-close,m} \tag{6}$$

$$r^p_{intraday,m} = \sum_{i\in p} w^i_{m-1} r^i_{intraday,m} \tag{7}$$

$$r^p_{overnight,m} = \sum_{i\in p} w^i_{m-1} r^i_{overnight,m} \tag{8}$$

$$r^p_{NDdiff,m} = r^p_{overnight,m} - r^p_{intraday,m} = \sum_{i\in p} w^i_{m-1}(r^i_{overnight,m} - r^i_{intraday,m}), \tag{9}$$

where the superscript p denotes that the return is a portfolio return, and w^i_{m-1} is the weight of stock i in portfolio p when the portfolio is constructed at the end of month $m - 1$.

In robustness checks in which we exclude certain special days, such as ex-dividend days, Mondays, or macro-news days, we first remove the special days for a given ETF and compute the cumulative monthly returns using the remaining observations. We then apply the same procedure as in Equations (4) and (5) to standardize monthly returns. After obtaining monthly returns for all ETFs, we calculate the portfolio returns as their weighted average.

3.3 Proxies for Retail Demand and Arbitrage Constraints

To proxy retail investors' demand, we use several variables: the maximum return over the past month (MAX), retail investors' ownership, and net fund flow from retail investors. MAX captures retail demand as it reflects the lottery-like behavior often associated with retail investors, as highlighted by Bali, Cakici, and Whitelaw (2011). Retail investors' ownership is a commonly used measure of demand, so we include it as a key variable in our analysis.

Furthermore, retail investors' demand can be directly estimated in the ETF market by observing new ETF shares created by APs in response to excess demand. To calculate the net fund flow from retail investors, we first determine the retail investors' holdings, which is the difference between the total number of shares outstanding and the number of shares held by institutional investors. We then calculate the quarter-to-quarter change in retail holdings and scale it by the total shares outstanding at the end of the previous quarter. This gives us the net fund flow from retail investors. Detailed definitions are provided in Appendix Table A1.

Next, to reduce measurement error, we combine these proxies into a single measurement. This approach follows the method used by Stambaugh, Yu, and Yuan (2015) to construct mispricing measurements. Each month, we rank ETFs based on the most recent values of each proxy and compute the average ranking across the three proxies to form a composite measurement of retail investors' demand. To ensure robustness, we require that each ETF has ranking data for at least two out of the three proxies.

Similarly, we construct a composite measure of arbitrage constraints by combining several proxies that capture arbitrage risk, execution costs, and APs' incentives to arbitrage. Arbitrage risk is proxied by idiosyncratic volatility, calculated as the standard deviation of the residuals from a CAPM regression over the prior 12 months, following Wurgler and Zhuravskaya (2002), Pontiff (2006), and Stambaugh, Yu, and Yuan (2015). Execution costs, which reduce arbitrage profitability and thereby deter arbitrage activity, are captured by two measures: bid–ask spreads and the Amihud illiquidity ratio (Amihud, 2002). APs' incentives to arbitrage are measured by their concentration. A larger number of APs increases competition and accelerates arbitrage responses. We therefore define concentration as the inverse of the number of APs, with higher values indicating greater concentration and, consequently, weaker arbitrage effectiveness.

Finally, we construct an aggregate measurement for arbitrage constraints by averaging the ranks of idiosyncratic volatility, bid-ask spread, Amihud illiquidity ratio, and AP concentration. To ensure robustness, we require each ETF in our sample to have data for at least three out of these four proxies.

3.4 Summary Statistics

Table 1 presents the summary statistics of our key variables. For example, it shows that the average difference between overnight and intraday returns (NDdiff) is positive, at 1% per month. However, the large standard deviation of this difference indicates significant variation among ETFs. The average overnight return (Night Ret) is positive, while the average intraday return (Day Ret) is negative.

Table 2, Panel A presents the correlation matrix of key variables. First, the overnight–intraday return difference is strongly positively correlated with proxies of retail demand and arbitrage constraints, suggesting that the trading activities of retail investors and arbitrageurs likely contribute to this return difference. Second, the overnight–intraday return difference is positively correlated with retail investors' ownership, which casts doubt on explanations based on information asymmetry as retail investors are less likely to be informed. Third, the intraday return shows a negative correlation with both the intraday return standard deviation and intraday beta, indicating that the risk-return trade-off theory may not adequately explain the observed return differences. Although these preliminary correlations do not formally test the hypotheses, they suggest that H1 and H2 may not fully account for the return differences observed.

Table 2, Panel B presents the correlation matrix for these proxies, revealing positive correlations among them. This indicates that our composite measurement effectively captures the underlying factors influencing retail demand. Table 2, Panel C shows positive correlations among these proxies of arbitrage constraints, indicating that these measures effectively capture the common factors influencing arbitrage constraints.

4 Empirical Analysis

This section presents our main empirical analysis. We first document that the overnight–intraday return differential is significantly positive and robust. Then, we investigate various causes of this phenomenon.

4.1 Overnight–Intraday Return Difference in the ETF Market

We first document the degree of overnight–intraday return differences in the ETF market. To do this, we create an equal-weighted ETF portfolio that includes all ETFs listed on US exchanges.[3] The first row of Table 3, Panel A shows that this portfolio exhibits

[3] We also report the value-weighted average returns in Appendix Table A2, and the results are qualitatively similar.

a significantly positive monthly overnight return of 0.78%, while the monthly intraday return is negative at −0.15%. The average monthly overnight–intraday return difference is 0.93%, and this difference is statistically significant at the 1% level.

Next, we analyze how the differences between overnight and intraday returns vary across different types of underlying assets. We create equal-weighted portfolios for ETFs based on their underlying asset types, as specified by the investment style codes from the CRSPMF database. The asset types include domestic equity, foreign equity, fixed-income assets, and other assets. The "other" category includes mortgage-backed ETFs, currency ETFs, and mixed equity and fixed-income ETFs. Within foreign equity ETFs, we also create a portfolio that excludes those primarily containing Canadian or Latin American equities, focusing instead on non-US time zone foreign equity ETFs. Table 3, Panel A shows that the significant and positive differences between overnight and intraday returns are widespread across all ETF asset classes, indicating that these differences are not confined to any particular asset type.

The result from the non-US time zone foreign equity ETFs is particularly interesting. Despite these ETFs holding assets traded in different time zones, the overnight–intraday return difference remains consistently positive. If this difference were primarily driven by specific risks or information emerging overnight, we would expect to see a reversal for these non-US time zone foreign equity ETFs, as overnight information for foreign companies would be integrated into the US market during intraday trading. This finding suggests that the observed overnight–intraday return difference is less likely due to risk or information disparities and may instead be influenced by other factors.

We conduct several robustness checks. First, Chan, Christie, and Schultz (1995) show that variations in market structures and trading rules can influence return patterns. To rule out this possibility, we split our sample into two subsamples based on the exchange: NASDAQ and NYSE. Table 3, Panel B shows that both NASDAQ and NYSE market portfolios exhibit a significantly positive overnight–intraday return difference, suggesting that this return difference is not driven by trading rules or market structure.[4]

Second, we test the impact of the weekday effect on our results. Early studies (French, 1980; Gibbons and Hess, 1981) find that stock returns from Friday close to Monday close are significantly negative. Harris (1986) show that this negative Monday return, especially in small firms, accumulates from Monday open to Monday close. If ETFs exhibit similar behavior, a negative Monday intraday return could contribute to a positive average overnight–intraday return difference. To address the weekend effect, we create a new sample by excluding all Monday observations. As shown in Panel C of Table 3, the monthly overnight–intraday return difference decreases slightly by 13 basis points after removing Monday observations, but it remains significantly positive. This indicates that our results are not primarily driven by the weekend effect.

Next, we examine the impact of dividend payments on our findings, as dividends are generally deducted from the trading price before the market opens. According to Kalay (1982), the price drop on the ex-dividend day is typically less than the dividend amount. As a result, using the adjusted price to calculate returns on ex-dividend days could lead to observing a positive overnight return. To rule out this possibility, we exclude all ex-dividend days from our sample. Panel C of Table 3 shows that removing ex-dividend days reduces the overnight–intraday return difference by only 5 basis points. The return differences remain positive and significant, suggesting that our results are not primarily

[4]The NYSE category combines three exchanges including the NYSE, NYSE MKT, and NYSE Arca. We define the NYSE category in this way because following NYSE's acquisitions of Arca and AMEX, all ETF-related business was consolidated onto NYSE Arca. NYSE ceased ETF trading in 2006, and NYSE MKT followed in 2009; ETFs originally listed on these venues were subsequently transferred to NYSE Arca.

influenced by the dividend effect.

Lastly, we investigate the influence of macro news announcements on our findings. Savor and Wilson (2014) point out that the positive relationship between risk and return can only be observed on days when important macro news is scheduled for announcement. We exclude days with important macroeconomic announcements, such as inflation news, unemployment news, and Federal Open Markets Committee (FOMC) interest rate decisions and calculate the overnight–intraday return differential. Table 3, Panel C shows that the overnight–intraday return differential on days without macro announcements is 0.92% per month, similar to the our baseline finding. This suggests that macro announcements are not the main driver of the return differential.

As a robustness check, we report daily average returns in the Appendix Table A3, which shows that the overnight–intraday return differential is significantly positive in the full sample and on days with or without special events.

4.2 Risk and Information-Based Explanations

In this section, we test risk- and information-based mechanisms, H1 and H2, of our findings. We show that the results are inconsistent with these explanations.

First, to test the risk theory explanation (H1), we construct two groups of risk measurements. The first group captures the overall risk of an ETF. We measure it using the standard deviation of daily overnight or intraday returns over the past month. The second group captures the ETF's exposure to systematic risk. We calculate the systematic risk using beta estimates from the CAPM. Specifically, we estimate the beta for intraday (overnight) returns by regressing the daily intraday (overnight) returns of an ETF on the daily intraday (overnight) returns of the ETF market portfolios, with an estimation window of the past 12 months.

We then construct the sorted portfolios as follows. At the beginning of each month t, ETFs are sorted into three groups (the bottom 30% (Low), the middle 40% (Mid), and the top 30% (High)) based on the value of the sorting variables in month $t-1$. For each group, we construct equal-weighted portfolios and hold these portfolios throughout month t. We report the monthly average overnight–intraday return differentials of these risk-sorted portfolios in Table 4, Panel A. The first two rows show that the overnight–intraday return differential increases with the risk of ETFs measured with both intraday returns or overnight returns. In fact, sorting ETFs based on the intraday risk generates a bigger spread in the return differential than sorting ETFs based on the overnight risk, which is inconsistent with the idea that higher overnight risk causes a higher return differential.

To further test the risk-based hypothesis, we sort ETFs based on the difference in the risks of overnight and intraday returns. Under the classic risk-return trade-off, we expect a larger overnight–intraday risk differential to lead to a greater overnight–intraday return differential. We find this not to be the case. ETFs with greater overnight risk, measured by the standard deviation or beta, relative to the intraday risk, have a smaller overnight–intraday return differential. This rejects the risk-based hypothesis (H1).

Similarly, in Table 5, we run Fama and MacBeth (1973) regressions to explore the cross-sectional variations in the overnight–intraday return differential. We find that the intraday risk is always positively associated with the return differential, whereas the overnight risk is negatively associated with the return differential. These results are opposite to the risk-based hypothesis.

Next, we test the information asymmetry explanation (H2) by sorting ETFs based on retail investors' ownership. Sias, Starks, and Titman (2006) show that institutional investors are better informed on average, and therefore retail investors' ownership can serve as a proxy for the proportion of uninformed investors. The idea is to test whether ETFs with fewer informed investors display a smaller return differential. Table 4, Panel B presents the results. We find that a higher proportion of retail investors' ownership leads to a significantly higher overnight–intraday return differential. This is inconsistent with the idea that the return differential is caused by informed investors trying to exit their positions at the market close. In Appendix Table A4, we report the intraday and overnight returns separately for retail ownership-sorted portfolios. It shows that the intraday return is negatively related to retail ownership, and the overnight return is positively related to retail ownership. This suggests that the overnight–intraday return differential is mainly caused by the presence of retail investors who are relatively less informed, rejecting the information-based hypothesis (H2).

4.3 Retail Demand Shocks and Arbitrage Constraints

In this section, we examine whether the overnight–intraday return difference can be explained by retail demand shocks and arbitrage constraints (H3). According to H3, higher retail investor demand leads to higher open prices, thus reducing the intraday returns and increasing the overnight returns. To test this, we first sort ETFs into portfolios based on proxies of retail demand to examine the relationship between retail demand and the overnight–intraday return difference.

Table 4, Panel B reports the results based on several retail investors' demand proxies. The data show that the overnight–intraday return difference increases with the past month's maximum return, retail investors' ownership, and net fund flow from retail investors. The "high minus low" column shows that ETFs in the highest retail demand portfolio have a significantly higher return differential than ETFs in the lowest retail demand portfolio. The differences range from 0.4% to 0.9%. Furthermore, our composite measure of retail demand shows an even greater spread in the overnight–intraday return differential between ETFs in the low- and high-retail-demand portfolios. The spread is 1.16% and highly significant. Appendix Table A4 shows the overnight and intraday returns separately for these ETF portfolios. Consistent with H3, higher retail demand reduces intraday returns and increases the overnight returns.

Next, we examine the effect of arbitrage constraints. Table 4, Panel C shows that the overnight–intraday return differential is positively correlated with the four proxies for arbitrage constraints. The differences in the return differential between high and low arbitrage constraint portfolios range from 0.4% to 1%. Sorting ETFs based on our composite measure of arbitrage constraints shows similar results. The overnight–intraday return differential of ETFs with the highest arbitrage constraints is almost three times the return differential of ETFs with the lowest arbitrage constraints.

To analyze how retail investors' demand and arbitrage constraints jointly affect the overnight–intraday return differential, we construct double-sorted portfolios based on the two composite measures. Table 6, Panel A shows that, after controlling for each channel, ETFs with higher arbitrage constraints or retail demand are still associated with a higher overnight–intraday return differential. This implies that both retail investors' demand and arbitrage constraints are important in explaining the phenomenon. Notably, the ETFs with the highest arbitrage constraints and the greatest demand from retail investors have the highest overnight–intraday return differential, almost 2% per month. The ETFs with the lowest retail demand and arbitrage constraints have the smallest return differential, and it becomes insignificant once we control for the Fama–French–Carhart four-factor (Carhart, 1997) exposures in Panel B. These results support hypothesis H3.

So far, our analysis has primarily focused on the overnight–intraday return difference of ETFs. However, since an ETF's return is influenced by the fluctuation of the underlying assets, we use a panel regression with benchmark×time fixed effects to control for the fundamentals of the underlying assets. This approach helps us examine how ETFs deviate from their benchmarks throughout the day and night. To ensure meaningful comparisons, we require benchmarks to be followed by at least two ETFs, which significantly reduces the sample size. Table 7 reports the results. We show that retail investors' demand and arbitrage constraints significantly impact the overnight–intraday return difference of ETFs, even after controlling for the fundamentals of the underlying assets. This suggests that high retail demand and strong arbitrage constraints can lead to higher overnight returns and lower intraday returns for ETFs compared to their underlying assets.

4.4 Identification and Causal Inference

In this section, we use an exogenous shock during the COVID-19 pandemic to facilitate the causal identification of our findings. During the pandemic, the US government implemented several relief acts, including the Coronavirus Aid, Relief, and Economic Security Act, the COVID-related Tax Relief Act, and the American Rescue Plan Act. These acts led to three rounds of EIPs to Americans: $1,200 (EIP1) in April/May 2020, $600 (EIP2) in December 2020/January 2021, and $1400 (EIP3) in March/April 2021. Divakaruni and Zimmerman (2024) use these payment events to study retail demand for bitcoins. We argue that these payments also boosted the demand of retail investors for ETFs, potentially affecting the overnight–intraday return differences. Therefore, we expect the return difference to be positively impacted by this shock, especially for ETFs with high retail demand. To analyze this effect, we create an EIP dummy and interact it with retail investors' demand to study the impact of EIPs on the overnight–intraday return difference.

Since the three payment rounds occurred during the COVID-19 pandemic, we restrict the sample for this exercise to between January 2020 and December 2021. This approach allows us to compare months in which investors received government payments with other months during the pandemic when no such payments were made. Importantly, both the pre- and post-event periods fall within the COVID-19 period, enabling us to isolate the effect of government monetary stimulus on retail demand from the work-from-home effect induced by lockdown policies.

Table 8 presents the results. Columns (1)–(3) show the results from the pooled OLS without fixed effect. Column (1) reaffirms the role of retail investors' demand and arbitrage constraints in determining the overnight–intraday return difference. Column (2) reveals that EIPs have a positive impact on the overnight–intraday return difference. On average, the overnight–intraday return increases by 2.37% during the EIP periods (April, May, and December 2020, and January, March, and April 2021). Column (3) shows a positive coefficient for the interaction term of composite retail investors' demand and the event dummy, suggesting a stronger impact of EIPs for ETFs that are popular among retail investors.

In addition, panel OLS results with benchmark×time fixed effects reveal that ETFs with higher retail demand deviated more from their benchmarks during the EIP period. These results provide strong evidence of the causal impact of retail investor demand on ETFs' day and night return difference.

4.5 Measuring Retail Investor Order Imbalance

A more direct approach to measuring retail demand is through retail order imbalance. However, identifying retail order flow is challenging. Early studies often regard small orders as retail investors' transactions (Cushing and Madhavan, 2000; Campbell, Ramadorai, and Schwartz, 2009; Berkman et al., 2012). With the popularity of algorithmic trading, many institutional investors now strategically split their large orders into several small ones and execute them when the market is deep. This systematic shift renders order size an unreliable proxy for identifying retail investors.

Boehmer et al. (2021) propose a new algorithm to identify retail orders via "sub-penny price improvement," which allows retail investors to get a better trading price than the NBBO by 0.1 or 0.01 cents. This algorithm relies on the Regulation National Market System (Reg NMS), which prohibits displaying quotes with decimals below one cent but allows execution at fractional cent prices. Under Regulation 606T, wholesalers usually offer sub-penny price improvements to retail investors to demonstrate optimal order execution. In contrast, institutional investors typically place orders with their trading teams and do not receive this price improvement. Therefore, Boehmer et al. (2021) suggest that retail investors' orders can be identified by requiring the exchange code in the TAQ to be "D" and the sub-penny amount to be less than \$0.004 or higher than \$0.006. Boehmer et al. (2021) further classify orders with a sub-penny amount less (more) than \$0.004 (\$0.006) as the sell (buy) transaction. However, Barber et al. (2024) point out that using the Lee and Ready (1991) algorithm to infer orders' directions is more accurate than the algorithm provided by Boehmer et al. (2021). Therefore, we use the Boehmer et al. (2021) algorithm to identify the retail investors' orders and apply the Lee and Ready (1991) algorithm to infer the direction of the orders in the first and last five minutes of the regular trading hours.

As mentioned by Boehmer et al. (2021), their algorithm does not work well before 2010 and from 2016 to 2018. Thus, in this section, our sample period is from 2010 to 2022 but excludes 2016, 2017, and 2018.

Figure 4 plots the ETF trading volume throughout the trading hours. It shows that the trading volumes concentrate near the market open and close, consistent with the existing literature. To test how retail investors' trading near the market open affects the overnight–intraday return difference, we utilize the retail order imbalance measurement from Boehmer et al. (2021) which is calculated as follows.

$$retail\ order\ imbalance = \frac{buy\ volume_{it} - sell\ volume_{it}}{buy\ volume_{it} + sell\ volume_{it}}, \tag{10}$$

In H3, we demonstrate that the overnight–intraday return difference is driven by retail investors' excess demand near the market open, while the excessive demand near the market close can be easily absorbed by the ETF creation and redemption activities. Thus, we expect the daily overnight–intraday return difference to be positively correlated with the retail order imbalance near the market open and not respond to the retail order imbalance near the market close. Table 9, columns (1) and (2) present the results in panel regressions controlling for fund-fixed and time-fixed effects, while columns (3) and (4) control for benchmark×time fixed effects. Columns (1) and (3) regress the daily overnight–intraday return on the retail order imbalance near the market open, showing that this imbalance has a strong effect on the overnight return. Columns (2) and (4) add a variable controlling for end-of-day retail order imbalance. The results show a positive coefficient for the retail order imbalance near the market open and an insignificant coefficient for the imbalance near the market close. This implies that the overnight–intraday return difference is mainly driven by retail investors' demand near the market open, not by the imbalance near the market close. Additionally, the results in columns (3) and (4) suggest that high retail demand near the market open can cause the overnight–intraday return difference of an ETF to deviate from its underlying assets' return difference.

4.6 Tug-of-War in the ETF Market

Lou, Polk, and Skouras (2019) document that a tug-of-war exists in the stock market between investors who prefer to trade near the market open and those who prefer to trade near the market close. This tug-of-war generates a predictable return continuation and reversal of intraday and overnight returns. We demonstrate that a similar tug-of-war exists in the ETF market, but its effect is asymmetric.

Following Lou, Polk, and Skouras (2019), we sort ETFs into portfolios based on their past intraday returns or overnight returns. We then compute their next-month intraday and overnight returns. Table 10 presents the results.[5] Panel A shows that ETFs with the highest past overnight returns have the highest overnight returns and the lowest intraday returns (i.e., a reversal in returns). The net effect is that these ETFs have the highest overnight–intraday return differential, at more than 2% per month. The return differential for ETFs in the lowest overnight return portfolio is insignificant. Similarly, Panel B shows that ETFs with the lowest past intraday returns exhibit the most significant overnight–intraday return differential, whereas ETFs with the highest intraday returns do not display a significant overnight–intraday return differential.

This table shows that inefficiencies, as reflected by return continuation and reversals, mainly occur for ETFs with the highest overnight returns or lowest intraday returns.

[5] The results for equity-only ETFs are similar, which we present in Appendix Table A5.

As we demonstrated in the previous sections, this is caused by inflated prices near the market open due to excess retail demand. On the other hand, ETFs with the lowest overnight returns or highest intraday returns do not display strong return continuation and reversal. This asymmetry indicates that the market close prices of ETFs are relatively more efficient, presumably due to the redemption and creation mechanism of ETFs.

4.7 What Drives Investor Demand?

In this sub-section, we verify whether retail investors are informed in the ETF market. We compare the total returns of ETFs with high and low retail demand and find that higher retail demand does not predict higher total returns. The results are reported in Table 11. We first sort ETFs into portfolios based on the composite measure of retail investors' demand and the composite measure of arbitrage constraints. We then report the average total returns of ETFs in each portfolio. Examining across different rows, we find that higher retail demand does not predict higher returns. This indicates that retail demand for ETFs are not informed. We also compare the returns of ETFs with different arbitrage constraints across the columns and find that ETFs with higher arbitrage constraints tend to have lower returns, consistent with the literature that shows arbitrage constraints contribute to overpricing (e.g., Stambaugh, Yu, and Yuan (2015)).

5 Conclusion

ETFs are popular investment vehicles that offer investors the flexibility to purchase at any time during the intraday trading hours. However, we identify a hidden cost of purchasing ETFs. By decomposing the close-to-close mid-quote return into its overnight and intraday components, we find that the overnight return is significantly positive across the ETF market, while the intraday return does not differ significantly from zero. Buying ETFs intraday, especially near the market open, exposes investors to the intraday risk without the proportional compensation in returns. We conduct a series of robustness checks and find that the overnight–intraday return differences are pervasive and widespread, not limited to a specific type of ETF or confined to a particular time period.

When investigating the drivers of the overnight–intraday return difference, we find that neither the risk-return trade-off nor information asymmetry can explain the phenomenon. Instead, our findings point to market frictions as key factors. Specifically, we argue that the return difference is influenced by excess retail demand near the market open and the constraints faced by arbitrageurs to provide the supply. Using proxies to measure retail demand and arbitrage constraints, we find that both channels strongly affect the return difference, supporting our conjecture that retail demand and arbitrage constraints drive the overnight–intraday return differentials. Moreover, we identify exogenous variations in the retail demand of ETFs during the COVID-19 pandemic when the US government issued three rounds of EIPs to households. This increased retail demand for ETFs and led to greater overnight–intraday return differentials. We also measure the retail demand directly through the retail order imbalance inferred from the TAQ data and find supporting evidence of the retail demand channel. Overall, our findings show that the convenience of buying ETFs during intraday trading hours comes at a cost, and one can avoid this cost by timing the purchase near the market close, when ETF prices are more efficient.

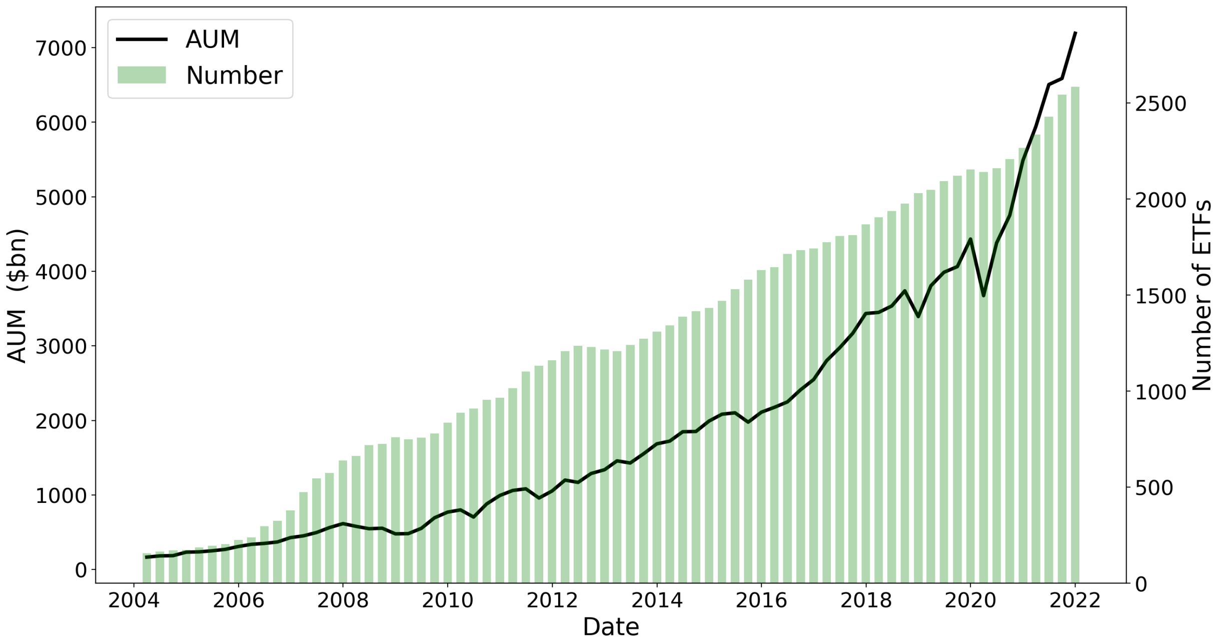

Figure 1: ETF Growth Trend

The figure illustrates the growth of the US ETF market from Q1 2004 to Q4 2021. Total assets under management (AUM) are calculated as the sum of AUM across all ETFs, while the number of ETFs is defined as the count of unique ETFs in the US market. AUM (in billions of dollars) is plotted on the left-hand y-axis, and the number of ETFs outstanding each quarter is shown as bars on the right-hand y-axis.

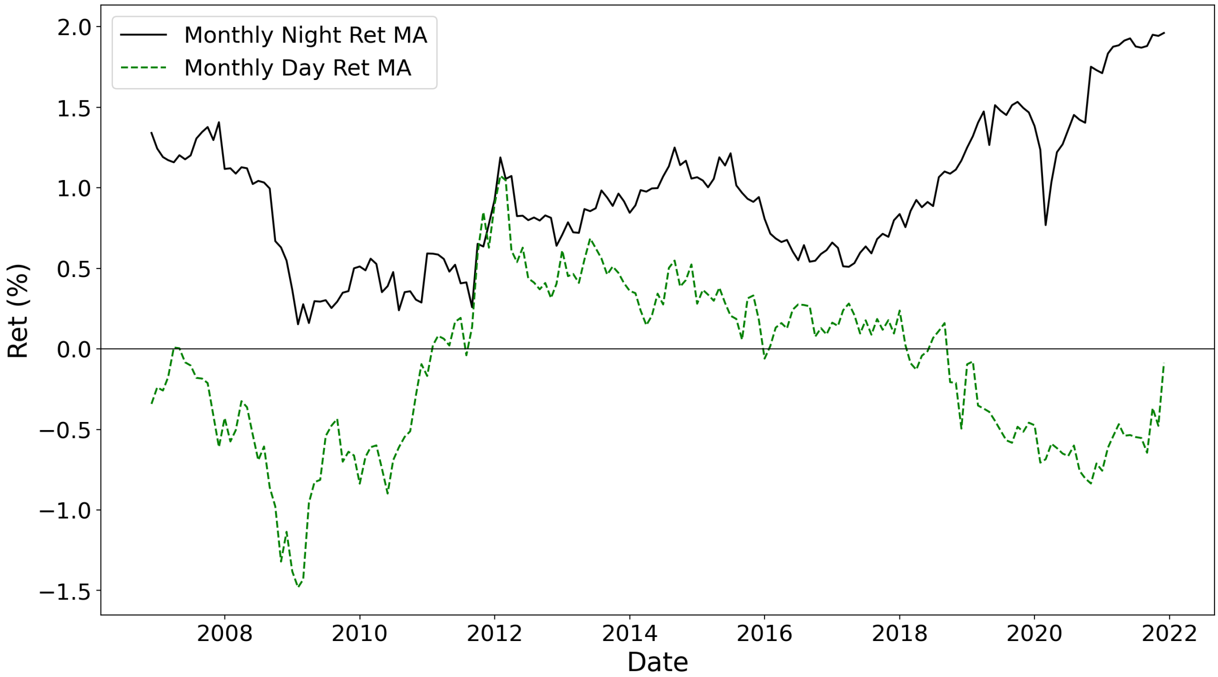

Figure 2: The Dynamics of Three-Year Moving Average Monthly Return of the ETF Market Portfolio

The figure plots the three-year moving average (MA) of monthly overnight and intraday returns for the ETF market portfolio. Specifically, monthly intraday and overnight returns for the market portfolio are computed using Equations (7) and (8). The three-year moving average is then obtained as the mean of monthly returns over the preceding 36 months. Given that the sample period starts in 2004, the moving averages are reported from January 2007 through December 2021.

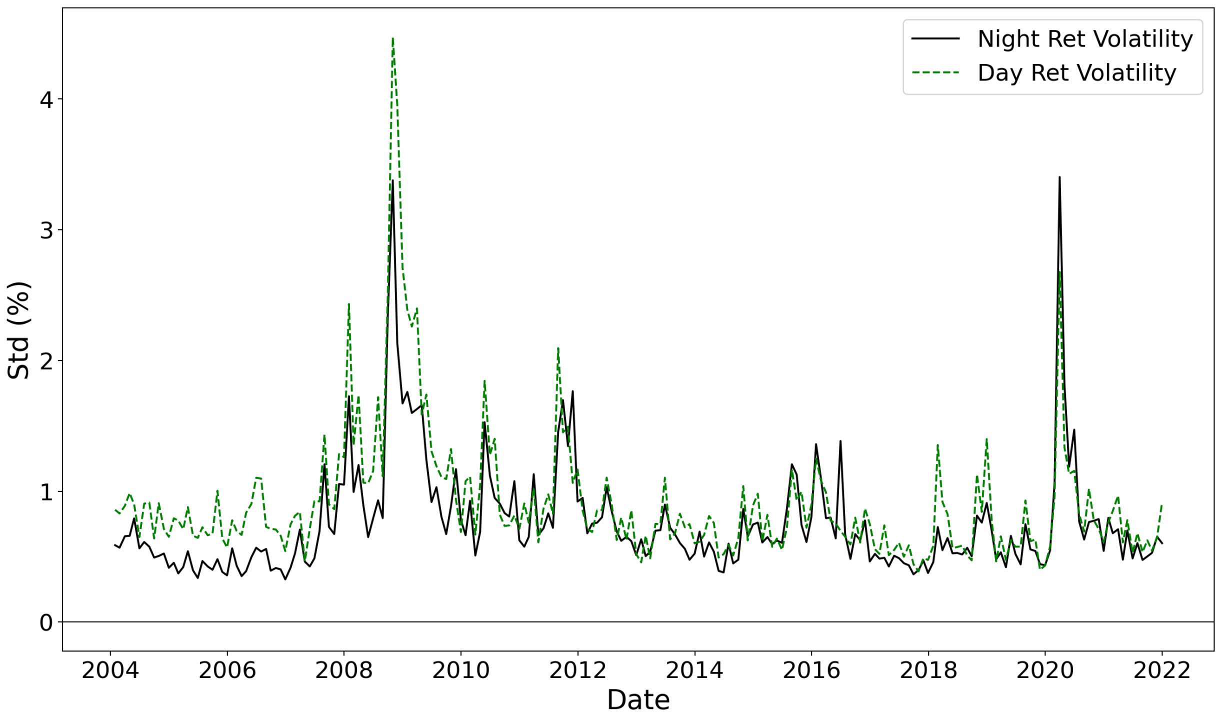

Figure 3: The Dynamics of Average Return Volatility of All ETFs

The figure shows the dynamics of monthly average intraday and overnight return volatility for all ETFs listed in the United States. Intraday and overnight volatility are defined as the standard deviation of daily intraday and overnight returns within each month, respectively. The sample period covers January 2004 to December 2021.

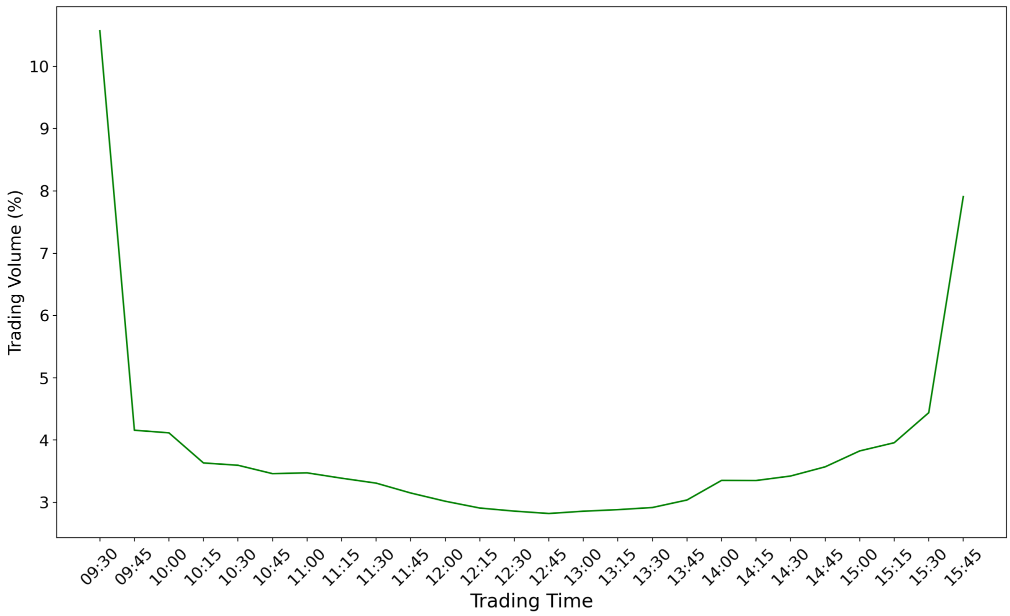

Figure 4: ETF Market Intraday 15-Minute Trading Volume

The figure presents the average intraday trading volume dynamics of the US ETF market at 15-minute intervals from January 2010 to December 2021. For each ETF, 15-minute trading volume is defined as the number of shares traded within the interval, scaled by the ETF's total daily trading volume. Market-level trading volume is then calculated as the cross-sectional average across all ETFs.

Table 1: Summary Statistics

The table reports summary statistics for the main variables over the period of January 2004 to December 2021. Variables are winsorized at the top and bottom 2.5% on a monthly basis. NDdiff, Day Ret, Night Ret, Day Risk, Night Risk, AP Concentration, Bid–Ask Spread, Retail Flow, Ivol, and Expense Ratio are expressed in percentage points. Amihud illiquidity is expressed as the logarithm of the absolute return divided by dollar trading volume (in billions). Turnover is expressed as a fraction. Momentum and Max Return are expressed as in the natural unit of raw gross returns. Detailed variable definitions are provided in the Appendix Table A1.

	Count	Mean	Std	25%	50%	75%
NDdiff	202825	1.017	6.250	-1.793	0.619	3.746
Day Ret	202825	-0.168	4.274	-2.066	0.068	1.912
Night Ret	202825	0.850	4.213	-0.734	0.722	2.615
Day Risk	202496	0.823	0.711	0.391	0.647	1.022
Night Risk	202496	0.760	0.694	0.342	0.576	0.943
Amihud Illiquidity	202493	1.202	2.885	-0.763	1.318	3.377
Turnover	202825	0.462	0.962	0.093	0.168	0.369
Momentum	202825	1.083	0.223	0.974	1.063	1.187
Log Cap	202825	5.159	2.213	3.548	5.072	6.658
AP Concentration	173358	3.006	2.833	1.429	2.128	3.448
Retail Ownership	200446	0.582	0.249	0.413	0.603	0.771
Bid-Ask Spread	202496	0.252	0.352	0.066	0.141	0.299
Max Return	202496	1.023	0.020	1.011	1.018	1.028
Retail Flow	199748	3.890	17.223	-3.619	0.827	7.506
Beta	202496	0.939	0.680	0.722	0.954	1.161
Day Beta	202496	0.835	0.636	0.512	0.861	1.105
Night Beta	202496	0.950	0.765	0.707	0.955	1.169
IVol	202496	1.260	0.828	0.739	1.055	1.645
Expense Ratio	184139	0.478	0.265	0.250	0.480	0.650

Table 2: Correlation Matrix

The table shows the correlation matrix of the main variables, proxies for retail demand, and proxies for arbitrage constraints over the period of January 2004 to December 2021. Panel A shows the correlation matrix of key variables. Panel B shows the correlation matrix of the retail investors' demand proxies. Panel C shows the correlation matrix of proxies for arbitrage constraints.

Panel A. Correlation Matrix of Key Variables

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(1) NDdiff	1													
(2) Day Ret	-0.739	1												
(3) Night Ret	0.732	-0.082	1											
(4) Day Risk	0.095	-0.044	0.096	1										
(5) Night Risk	0.122	-0.039	0.141	0.801	1									
(6) Turnover	0.005	-0.014	-0.007	0.394	0.331	1								
(7) Momentum	0.038	-0.037	0.020	-0.153	-0.194	-0.042	1							
(8) Log Cap	-0.029	0.026	-0.017	-0.122	-0.153	0.069	0.138	1						
(9) Beta	0.046	0.015	0.083	0.197	0.148	0.107	0.163	0.119	1					
(10) Day Beta	0.046	0.011	0.079	0.232	0.145	0.155	0.163	0.282	0.922	1				
(11) Night Beta	0.034	0.023	0.073	0.162	0.139	0.089	0.154	0.071	0.941	0.819	1			
(12) Retail Ownership	0.026	-0.028	0.010	0.202	0.176	0.015	-0.045	-0.227	0.028	0.033	0.021	1		
(13) Retail Demand	0.076	-0.050	0.061	0.375	0.350	0.139	0.051	-0.195	0.115	0.116	0.092	0.645	1	
(14) Arbi Constraint	0.074	-0.054	0.055	0.299	0.310	-0.060	-0.047	-0.770	0.049	-0.080	0.065	0.248	0.366	1

Panel B. Correlation Matrix of Retail Demand Proxies

Variables	(1)	(2)	(3)
(1) Retail Flow	1		
(2) Max Return	0.044	1	
(3) Retail Ownership	0.176	0.196	1

Panel C. Correlation Matrix of Proxies for Arbitrage Constraints

Variables	(1)	(2)	(3)	(4)
(1) Bid-Ask Spread	1			
(2) Amihud Illiquidity	0.516	1		
(3) Ivol	0.200	0.198	1	
(4) AP Concentration	0.181	0.274	0.028	1

Table 3: Overnight–Intraday Return Difference in the ETF Market

The table reports the mean and standard errors of equal-weighted ETF portfolios' monthly returns for the period of January 2004 to December 2021. Panel A sorts ETFs by underlying asset type and reports the corresponding portfolio returns for each asset class. Panel B sorts ETFs by trading venue and reports the portfolio returns for NYSE and NASDAQ. Panel C shows the market portfolio's return excluding observations on Mondays, ex-dividend days, or macro-news announcement days. All returns are presented in percentages. The standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** indicate significant levels at 10%, 5%, and 1% respectively.

	Num of Fund	Overnight Return	Intraday Return	Overnight–Intraday
Panel A. Portfolio Return in Different ETF Sectors				
All ETFs	2916	0.784***	-0.150	0.933***
		(0.165)	(0.186)	(0.207)
Domestic Equity	1408	0.971***	-0.226	1.197***
		(0.191)	(0.208)	(0.250)
Foreign Equity (Include US Time Zone)	807	0.713***	0.004	0.709***
		(0.229)	(0.222)	(0.246)
Foreign Equity (Non-US Time Zone)	775	0.644***	0.055	0.590**
		(0.225)	(0.220)	(0.243)
Fixed Income	515	0.264***	0.072	0.191**
		(0.045)	(0.066)	(0.084)
Other	431	0.648***	-0.242*	0.891***
		(0.142)	(0.144)	(0.191)
Panel B. Market Portfolio Return in Different Exchanges				
NYSE	2186	0.773***	-0.148	0.921***
		(0.166)	(0.186)	(0.210)
NASDAQ	499	0.971***	-0.217	1.187***
		(0.214)	(0.202)	(0.257)
Panel C. Market Portfolio Return Excluding Weekend, Ex-Dividend Day or Macro News Day				
Exclude Monday	2916	0.877***	0.073	0.804***
		(0.181)	(0.200)	(0.255)
Exclude Ex-Dividend	2916	0.760***	-0.122	0.882***
		(0.172)	(0.185)	(0.211)
Exclude Macro News	2916	0.732***	-0.191	0.924***
		(0.172)	(0.195)	(0.217)

Table 4: Overnight–Intraday Return Differential in Univariate Sorted Portfolios

This table reports the mean and standard errors of monthly overnight–intraday return differences for equal-weighted ETF portfolios over the period of January 2004 to December 2021. Panel A sorts portfolios based on ETF risk, Panel B based on measures of retail investor demand, and Panel C based on arbitrage constraints. For each category, the lowest group includes the bottom 30% of ETFs, the highest group includes the top 30%, and the mid group includes the remaining 40%. Portfolios sorted by retail ownership use data from the most recent quarter, whereas other sortings use data from the previous month. All portfolios are formed at month-end, and reported returns correspond to the following month. Returns are expressed in percentages. Standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Sorting on ETF Risk				
	Low	Mid	High	High Minus Low
Intraday Risk	0.395***	0.772***	1.720***	1.325***
	(0.123)	(0.223)	(0.290)	(0.202)
Overnight Risk	0.599***	0.947***	1.284***	0.685***
	(0.129)	(0.234)	(0.272)	(0.184)
Night Risk Minus Day Risk	1.407***	0.894***	0.545**	-0.862***
	(0.259)	(0.190)	(0.216)	(0.174)
Night Beta Minus Day Beta	1.151***	0.768***	0.970***	-0.181*
	(0.191)	(0.214)	(0.226)	(0.102)
Panel B. Sorting on Retail Investors' Demand				
Max Return	0.565***	0.816***	1.491***	0.926***
	(0.126)	(0.228)	(0.279)	(0.188)
Retail Ownership	0.715***	0.813***	1.340***	0.625***
	(0.196)	(0.208)	(0.221)	(0.078)
Retail Flow	0.844***	0.821***	1.204***	0.360***
	(0.214)	(0.210)	(0.204)	(0.084)
Retail_demand	0.581***	0.982***	1.743***	1.162***
	(0.174)	(0.218)	(0.268)	(0.137)
Panel C. Sorting on Arbitrage Constraints				
AP Concentration	0.712***	1.009***	1.136***	0.424***
	(0.224)	(0.198)	(0.218)	(0.095)
Ivol	0.554***	0.768***	1.566***	1.012***
	(0.116)	(0.234)	(0.279)	(0.191)
Bid-Ask Spread	0.609***	0.970***	1.242***	0.634***
	(0.199)	(0.210)	(0.227)	(0.121)
Amihud Illiquidity	0.646***	1.111***	1.019***	0.373***
	(0.188)	(0.216)	(0.231)	(0.115)
Arbi_constraint	0.570***	1.018***	1.445***	0.875***
	(0.177)	(0.219)	(0.256)	(0.126)

Table 5: Fama–Macbeth Return Regressions on Overnight/Intraday Risk and Retail Ownership

The table presents Fama–MacBeth regressions of ETF returns on lagged characteristics over the period of January 2004 to December 2021. The dependent variable is the monthly overnight–intraday return difference. Independent variables include Day Risk, Night Risk, Day Beta, Night Beta, CAPM Beta, log market capitalization (Log Cap), turnover ratio, and momentum measured in the most recent month, as well as Retail Ownership measured in the most recent quarter. Standard errors are reported in brackets. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	NDdiff	NDdiff	NDdiff	NDdiff	NDdiff	NDdiff
Day Risk	1.982***			1.957***		
	(0.231)			(0.198)		
Night Risk	-0.634***			-0.700***		
	(0.200)			(0.206)		
Day Beta		0.772***			0.221	
		(0.172)			(0.230)	
Night Beta		-0.103			-0.781***	
		(0.143)			(0.165)	
Retail Ownership			0.898***			0.833***
			(0.102)			(0.092)
Beta				0.245*	1.309***	0.627***
				(0.148)	(0.310)	(0.178)
Log Cap				-0.059***	-0.126***	-0.074***
				(0.015)	(0.018)	(0.016)
Turnover				-0.178***	0.056**	0.082***
				(0.046)	(0.025)	(0.026)
Momentum				0.009	0.012**	0.013**
				(0.006)	(0.006)	(0.006)
Constant	-0.161	0.360***	0.418*	-0.878	-0.441	-1.268**
	(0.109)	(0.113)	(0.226)	(0.615)	(0.602)	(0.631)
Observations	200,118	200,118	200,118	200,118	200,118	200,118
R-squared	0.087	0.085	0.007	0.184	0.150	0.137

Table 6: Overnight and Intraday Return of Portfolios Sorted on Retail Investors' Demand and Arbitrage Constraints

This table reports unconditional double-sorting results based on retail investor demand and arbitrage constraints over the period of January 2004 to December 2021. Panel A presents the raw monthly overnight–intraday return differences of the double-sorted portfolios, Panel B reports Fama–French–Carhart four-factor-adjusted overnight–intraday return differences, Panel C reports monthly overnight returns, and Panel D reports monthly intraday returns. In each panel, portfolios in the rows are sorted by arbitrage constraints in the previous month (low, mid, high), and portfolios in the columns are sorted by retail investor demand in the previous quarter (low, mid, high). For each panel, the low, mid, and high groups correspond to the bottom 30%, middle 40%, and top 30% of ETFs, respectively. All returns are expressed in percentages. Standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Overnight and Intraday Return Difference

Arbi_constraint	Low	Mid	High	High Minus Low
Retail_demand				
Low	0.312**	0.662***	0.846***	0.534***
	(0.152)	(0.197)	(0.235)	(0.160)
Mid	0.644***	0.957***	1.290***	0.646***
	(0.193)	(0.226)	(0.280)	(0.153)
High	1.081***	1.546***	1.988***	0.907***
	(0.270)	(0.284)	(0.281)	(0.153)
High Minus Low	0.769***	0.883***	1.143***	
	(0.169)	(0.157)	(0.157)	

Panel B. Four-Factor-Adjusted Overnight–Intraday Return Difference

Arbi_constraint	Low	Mid	High	High Minus Low
Retail_demand				
Low	0.160	0.516**	0.785***	0.527***
	(0.164)	(0.211)	(0.239)	(0.147)
Mid	0.667***	0.953***	1.394***	0.629***
	(0.208)	(0.222)	(0.264)	(0.155)
High	1.279***	1.581***	1.868***	0.492***
	(0.269)	(0.298)	(0.275)	(0.146)
High Minus Low	1.021***	0.968***	0.986***	
	(0.173)	(0.165)	(0.181)	

Panel C. Overnight Return

Arbi_constraint	Low	Mid	High	High Minus Low
Retail_demand				
Low	0.458***	0.701***	0.730***	0.271**
	(0.119)	(0.152)	(0.187)	(0.109)
Mid	0.644***	0.852***	0.948***	0.304***
	(0.161)	(0.188)	(0.214)	(0.096)
High	0.908***	1.066***	1.279***	0.371***
	(0.202)	(0.223)	(0.216)	(0.106)
High Minus Low	0.450***	0.365***	0.550***	
	(0.121)	(0.102)	(0.093)	

Panel D. Intraday Return

Arbi_constraint	Low	Mid	High	High Minus Low
Retail_demand				
Low	0.146	0.039	-0.116	-0.262***
	(0.141)	(0.180)	(0.191)	(0.086)
Mid	-0.001	-0.105	-0.342	-0.342***
	(0.168)	(0.207)	(0.235)	(0.098)
High	-0.173	-0.479*	-0.709***	-0.536***
	(0.202)	(0.271)	(0.268)	(0.117)
High Minus Low	-0.320***	-0.518***	-0.593***	
	(0.103)	(0.128)	(0.134)	

Table 7: Panel Regressions of Returns on Retail Investors' Demand and Arbitrage Constraints

The table presents panel regressions of ETF returns on lagged characteristics with benchmark *times* time fixed effects over the period of January 2004 to December 2021. The dependent variable is the monthly overnight–intraday return difference. Independent variables include measures of retail investor demand and arbitrage constraints, CAPM beta, the logarithm of market capitalization, turnover ratio, and momentum measured in the previous month. Standard errors, reported in brackets, are clustered at the benchmark level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	NDdiff	NDdiff	NDdiff	NDdiff
Retail_demand	0.349***		0.267***	0.274***
	(0.064)		(0.077)	(0.087)
Arbi_constraint		0.243***	0.152**	0.291**
		(0.054)	(0.069)	(0.137)
Beta				-0.037
				(0.059)
Log Cap				0.046
				(0.063)
Turnover				0.012
				(0.057)
Momentum				0.024***
				(0.004)
Constant	-0.117	0.209	-0.322*	-3.540***
	(0.191)	(0.159)	(0.187)	(0.960)
Observations	42,700	42,700	42,700	42,700
R-squared	0.474	0.474	0.474	0.478
Benchmark×Time FE	Y	Y	Y	Y

Table 8: Effect of Economic Impact Payments on Overnight–Intraday Return Difference

The table presents estimated coefficients from pooled OLS and panel OLS regressions of monthly ETF overnight–intraday return differences on lagged characteristics over the period of January 2020 to December 2021. The dependent variable in all specifications is the overnight–intraday return difference. Columns (1)–(3) report pooled OLS results using all ETFs traded during the sample period, while columns (4)–(5) report panel OLS results including benchmark×time fixed effects, using only ETFs whose benchmark is followed by at least two ETFs in the sample period. EIP is a dummy variable equal to one for observations in April, May, and December 2020 or January, March, and April 2021. Independent variables include retail investor demand, the event dummy, the interaction of retail investor demand and post dummy, arbitrage constraints, CAPM beta, logarithm of market capitalization, turnover ratio, and momentum measured in the previous month. Standard errors are reported in parentheses. For columns (1)–(3), standard errors are clusted at fund level, while for columns (4)–(5), they are clustered at the benchmark level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) NDdiff	(2) NDdiff	(3) NDdiff	(4) NDdiff	(5) NDdiff
Retail_demand	0.524***		0.201***	0.523***	0.308*
	(0.056)		(0.056)	(0.173)	(0.170)
EIP		2.372***	-1.711***		
		(0.076)	(0.251)		
Retail_demand*EIP			1.370***		0.869***
			(0.086)		(0.281)
Arbi_constraint	1.172***	1.377***	1.186***	0.899***	0.898***
	(0.071)	(0.069)	(0.070)	(0.254)	(0.254)
Beta	0.743***	0.741***	0.664***	0.336	0.314
	(0.110)	(0.108)	(0.107)	(0.335)	(0.337)
Log Cap	0.215***	0.237***	0.227***	0.131	0.129
	(0.027)	(0.027)	(0.027)	(0.101)	(0.102)
Turnover	0.869***	0.910***	0.611***	-0.269	-0.270
	(0.131)	(0.128)	(0.128)	(0.243)	(0.241)
Momentum	-0.035***	-0.028***	-0.032***	-0.013	-0.012
	(0.002)	(0.002)	(0.002)	(0.011)	(0.011)
Constant	-1.321***	-1.809***	-1.247***	-1.865	-1.858
	(0.313)	(0.307)	(0.308)	(1.583)	(1.578)
Observations	45,492	45,492	45,492	9,845	9,845
R-squared	0.046	0.062	0.072	0.513	0.514
Benchmark×Time FE	N	N	N	Y	Y

Table 9: Panel Regressions of Overnight–Intraday Return Difference on Retail Order Imbalance

The table presents panel regressions of daily overnight–intraday return differences on daily retail order imbalances near the market open and close over the period of January 2010 to December 2021, excluding 2016–2018. In the first two columns, regressions include fund fixed effects and date fixed effects, while the last two columns employ benchmark×time fixed effects. Columns (1)–(2) use all ETFs traded during the sample period, whereas the last two columns include only ETFs whose benchmark is followed by at least two ETFs. Market Open Retail Imbalance is defined as the net buy volume of retail investors scaled by total retail trading volume in the first five minutes after the market open; Market Close Retail Imbalance is measured over the last five minutes before the market close. Standard errors, reported in brackets, are clustered at the fund level for the first two columns and at the benchmark level for the last two columns. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) NDdiff	(2) NDdiff	(3) NDdiff	(4) NDdiff
Market Open Retail Imbalance	0.082***	0.082***	0.165***	0.165***
	(0.006)	(0.006)	(0.030)	(0.030)
Market Close Retail Imbalance		0.001		0.005
		(0.002)		(0.011)
Constant	0.042***	0.042***	0.029***	0.029***
	(0.000)	(0.000)	(0.000)	(0.000)
Fund FE	Y	Y	N	N
Time FE	Y	Y	N	N
Benchmark×Time FE	N	N	Y	Y
Observations	3,975,301	3,975,301	818,514	818,514
R-squared	0.260	0.260	0.338	0.338

Table 10: Overnight/Intraday Return Persistence/Reversal in the ETF Market

This table reports monthly overnight and intraday return persistence and reversal patterns in the ETF market from January 2004 to December 2021. The left panel presents raw returns, while the right panel reports the Fama–French–Carhart four-factor-adjusted returns. In Panel A, ETFs are sorted into quintiles at month-end based on their lagged one-month overnight returns; in Panel B, sorting is based on lagged one-month intraday returns. A long–short strategy is formed by going long the equal-weight winner quintile and short the equal-weight loser quintile. Standard errors, reported in brackets, are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A. ETFs Portfolios Sorted by Lagged One-Month Overnight Returns						
	Raw Return			Four-Factor-Adjusted Return		
Quintile	Overnight	Intraday	Overnight–Intraday	Overnight	Intraday	Overnight–Intraday
1	0.167	0.197	-0.030	-0.306*	-0.265**	-0.139
	(0.189)	(0.201)	(0.209)	(0.172)	(0.118)	(0.239)
2	0.525***	0.077	0.449**	0.098	-0.410***	0.410*
	(0.151)	(0.192)	(0.197)	(0.136)	(0.111)	(0.225)
3	0.703***	0.024	0.679***	0.256**	-0.468***	0.625***
	(0.157)	(0.203)	(0.218)	(0.128)	(0.124)	(0.238)
4	0.994***	-0.216	1.210***	0.573***	-0.716***	1.191***
	(0.156)	(0.208)	(0.234)	(0.131)	(0.134)	(0.250)
5	1.532***	-0.834***	2.366***	1.176***	-1.309***	2.387***
	(0.191)	(0.215)	(0.271)	(0.169)	(0.166)	(0.285)
5-1	1.365***	-1.031***	2.396***	1.385***	-1.142***	2.428***
	(0.192)	(0.180)	(0.247)	(0.211)	(0.173)	(0.277)
Panel B. ETFs Portfolios Sorted by Lagged One-Month Intraday Returns						
1	1.367***	-0.909***	2.276***	0.894***	-1.471***	2.267***
	(0.185)	(0.229)	(0.258)	(0.175)	(0.141)	(0.259)
2	0.855***	-0.182	1.037***	0.398***	-0.669***	0.969***
	(0.168)	(0.190)	(0.206)	(0.144)	(0.113)	(0.229)
3	0.658***	0.042	0.616***	0.216	-0.443***	0.561**
	(0.161)	(0.196)	(0.209)	(0.136)	(0.122)	(0.238)
4	0.565***	0.145	0.420**	0.159	-0.325**	0.386
	(0.149)	(0.199)	(0.212)	(0.129)	(0.138)	(0.245)
5	0.474***	0.156	0.318	0.126	-0.254	0.282
	(0.168)	(0.209)	(0.237)	(0.138)	(0.158)	(0.250)
5-1	-0.893***	1.065***	-1.958***	-0.866***	1.119***	-2.083***
	(0.172)	(0.183)	(0.227)	(0.200)	(0.182)	(0.243)

Table 11: Close-to-Close Return of ETFs Sorted on Retail Investor Demand

This table reports unconditional double-sorting results based on retail investor demand and arbitrage constraints over the period of January 2004 to December 2021. Panel A presents raw monthly close-to-close returns of the double-sorted portfolios, while Panel B reports Fama–French–Carhart four-factor-adjusted monthly close-to-close returns. Portfolios in each row are sorted by arbitrage constraints in the previous month (low, mid, high), and portfolios in each column are sorted by retail investor demand in the previous quarter (low, mid, high). For each panel, the low, mid, and high groups correspond to the bottom 30%, middle 40%, and top 30% of ETFs, respectively. Raw monthly close-to-close returns are obtained from CRSP and expressed in percentages. Standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Close-to-Close Return

Arbi_constraint	Low	Mid	High	High Minus Low
Retail_demand				
Low	0.596***	0.709***	0.621**	0.025
	(0.214)	(0.267)	(0.297)	(0.114)
Mid	0.638**	0.730**	0.592*	-0.046
	(0.268)	(0.328)	(0.354)	(0.124)
High	0.760**	0.553	0.592	-0.168
	(0.302)	(0.416)	(0.403)	(0.179)
High Minus Low	0.164	-0.156	-0.029	
	(0.151)	(0.189)	(0.193)	

Panel B. Four-Factor-Adjusted Close-to-Close Return

Arbi_constraint	Low	Mid	High	High Minus Low
Retail_demand				
Low	-0.098	-0.101	-0.270*	-0.270***
	(0.110)	(0.126)	(0.146)	(0.083)
Mid	-0.123*	-0.165*	-0.423***	-0.397***
	(0.070)	(0.097)	(0.140)	(0.103)
High	-0.062	-0.491***	-0.224	-0.260*
	(0.119)	(0.153)	(0.152)	(0.139)
High Minus Low	-0.062	-0.488***	-0.052	
	(0.133)	(0.137)	(0.148)	

Table A1: Variable Definitions

Variable Name	Definition
NDdiff	The difference between the monthly overnight return and intraday return.
Day Ret	The monthly intraday return, which is calculated from equation (4).
Night Ret	The monthly intraday return, which is calculated from equation (5).
Day Risk	The standard deviation of daily intraday return in the previous month.
Night Risk	The standard deviation of daily overnight return in the previous month.
Turnover	The secondary market turnover ratio in the previous month, measured as the number of shares traded on the exchanges, scaled by the total number of shares outstanding.
Momentum	The cumulative monthly gross return (one plus the raw return reported by CRSP) over the past 12 months.
Market Cap	The market capitalization at the end of the previous month, measured in millions.
Log Cap	The logarithm of the market capitalization at the end of the previous month.
AP Concentration	One over the number of authorized participants.
Retail Ownership	The proportion of ETF shares held by retail investors.
Bid-Ask Spread	The average closing bid-ask spread in the previous month.
Max	The highest daily close-to-close gross return in the previous month.
Amihud Illiquidity	$\frac{1}{n}\sum_{df=1}^{n} log(\frac{\lvert Ret_d \rvert}{DVOL_d})$, where n is the number of trading days within a month. Ret is the raw close-to-close return on day d from the CRSP dataset, and $DVOL$ is the dollar volume on day d, presented in billions.
Retail Flow	$\frac{Retail_q - Retail_{q-1}}{Shrout_{q-1}}$, where $Retail_q$ is the difference between total number of shares outstanding and the number of shares held by institutional investors at the end of quarter q, and $Shrout_{q-1}$ is the total number of shares outstanding at the end of quarter $q-1$.
Net Fund Flow	The difference between the current quarter's total net asset value and the last quarter's total net asset value multiplied by the last quarter's asset return scaled by the last quarter's total net asset value ($\frac{TNA_t - TNA_{t-1} * (1+Ret_t)}{TNA_{t-1}}$).
Institutional Net Purchase	$\frac{Ins_q - Ins_{q-1}}{Shrout_{q-1}}$, where Ins_q is the number of shares held by institutional investors at the end of quarter q and $Shrout_{q-1}$ is the total number of shares outstanding at the end of quarter $q-1$.
Beta	The beta is estimated from regressing the daily excess return on the daily market excess return, where the estimation window is the past 12 months. For ETFs with different types of underlying assets, we construct different market portfolios. In each ETF sector, the market return is the value-weighted ETF portfolios' return.
Day Beta	The beta is estimated from regressing the daily intraday return on the daily intraday market return, where the estimation window is the past 12 months.
Night Beta	The beta is estimated from regressing the daily overnight return on the daily overnight market return, where the estimation window is the past 12 months.
IVol	The standard deviation of the residual in the market beta estimation.
Retail Demand	The composite proxy of retail investors' demand.
Arbi Constraint	The composite proxy of arbitrage constraints.

Table A2: Overnight–Intraday Return Difference in the ETF Market (Value Weighted)

The table reports the mean and standard errors of value-weighted ETF portfolios' monthly returns for the period of January 2004 to December 2021. Panel A sorts ETFs by underlying asset type and reports the corresponding portfolio returns for each asset class. Panel B sorts ETFs by trading venue and reports the portfolio returns for the NYSE and NASDAQ. Panel C shows the market portfolio's return excluding observations on Mondays, ex-dividend days, or macro-news announcement days. All returns are presented in percentages. The standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** imply the significant levels are at 10%, 5%, and 1% respectively.

	Overnight Return	Intraday Return	Overnight–Intraday
Panel A. Portfolio Return in Different ETF Sectors (VW)			
All ETFs	0.622***	0.069	0.552***
	(0.170)	(0.169)	(0.209)
Equity	0.679***	0.107	0.572**
	(0.202)	(0.192)	(0.247)
Fixed Income	0.248***	0.088	0.159**
	(0.046)	(0.055)	(0.075)
Other	0.607***	-0.291**	0.898***
	(0.116)	(0.145)	(0.184)
Panel B. Market Portfolio Return in Different Exchanges			
NYSE	0.635***	0.070	0.566***
	(0.177)	(0.169)	(0.215)
NASDAQ	0.769***	0.087	0.682**
	(0.187)	(0.230)	(0.283)
Panel C. Market Portfolio Return Excluding Monday, Ex-Dividend Day, Macro News Day			
No Mon	0.719***	0.232	0.487**
	(0.183)	(0.180)	(0.246)
No Div	0.598***	0.100	0.498**
	(0.179)	(0.169)	(0.215)
No Macro News	0.565***	0.031	0.533**
	(0.177)	(0.179)	(0.218)

Table A3: Daily Overnight–Intraday Return Difference in the ETF Market

The table presents the average daily intraday and overnight returns of the ETF equal-weighted portfolio over the period of January 2004 to December 2021. The first row contains all observations from our sample period. The second row excludes fund-day observations if the day is an ex-dividend day of the fund. The third row excludes all Monday observations. The fourth row excludes observations from the macro-news announcement day. The last three rows only contain observations from the ex-dividend day, Monday, and the macro-news announcement day. All returns are presented in percentages. Standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** imply the significant levels are at 10%, 5%, and 1% respectively.

	Num of Funds	Num of Days	Overnight Ret	Intraday Ret	Overnight–Intraday
All Days	2916	4468	0.038***	-0.006	0.044***
			(0.009)	(0.010)	(0.013)
Exclude Div	2916	4468	0.038***	-0.005	0.043***
			(0.009)	(0.011)	(0.013)
Exclude Mon	2916	3631	0.042***	0.003	0.039***
			(0.009)	(0.012)	(0.014)
Exclude Macro News	2916	3920	0.032***	-0.010	0.042***
			(0.009)	(0.011)	(0.013)
Div Days	2662	3246	0.076***	-0.051***	0.127***
			(0.012)	(0.017)	(0.021)
Mondays	2916	837	0.024	-0.043*	0.067*
			(0.026)	(0.025)	(0.035)
Macro News Days	2916	548	0.084***	0.025	0.059*
			(0.022)	(0.031)	(0.035)

Table A4: Overnight and Intraday Return of Univariate Sorted Portfolio

This table reports the mean and standard errors of monthly overnight and intraday returns for equal-weighted ETF portfolios over the period of January 2004 to December 2021. Panel A sorts portfolios based on overnight risk, Panel B based on retail investor demand, and Panel C based on arbitrage constraints. For each characteristic, the lowest group includes the bottom 30% of ETFs, the highest group includes the top 30%, and the mid group includes the remaining 40%. Portfolios sorted by retail ownership use data from the most recent quarter, whereas other sortings use data from the previous month. All portfolios are formed at month-end, and reported returns correspond to the following month. Returns are expressed in percentages. Standard errors in brackets are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Intraday Return				Overnight Return			
	Low	Mid	High	High_low	Low	Mid	High	High_low
Panel A. Sorting on the Risk and Retail Investors' Ownership								
Day Risk	0.017	-0.010	-0.517**	-0.534***	0.412***	0.762***	1.203***	0.791***
	(0.125)	(0.195)	(0.249)	(0.149)	(0.095)	(0.182)	(0.224)	(0.152)
Night Risk	-0.055	-0.111	-0.311	-0.257**	0.545***	0.836***	0.973***	0.428***
	(0.127)	(0.207)	(0.231)	(0.130)	(0.084)	(0.187)	(0.227)	(0.159)
Night-Day Risk	-0.361	-0.138	0.032	0.393***	1.046***	0.756***	0.576***	-0.469***
	(0.232)	(0.170)	(0.181)	(0.113)	(0.189)	(0.143)	(0.190)	(0.113)
Night-Day Beta	-0.310*	-0.026	-0.169	0.141**	0.841***	0.742***	0.800***	-0.040
	(0.180)	(0.186)	(0.200)	(0.065)	(0.162)	(0.167)	(0.173)	(0.070)
Panel B. Sorting on Retail Investors' Demand								
Max Return	-0.071	-0.022	-0.413*	-0.342**	0.495***	0.794***	1.078***	0.584***
	(0.129)	(0.199)	(0.238)	(0.135)	(0.091)	(0.186)	(0.220)	(0.145)
Retail Ownership	-0.027	-0.058	-0.405**	-0.378***	0.688***	0.754***	0.935***	0.247***
	(0.173)	(0.186)	(0.203)	(0.057)	(0.153)	(0.162)	(0.182)	(0.055)
Retail Flow	-0.094	-0.069	-0.328*	-0.234***	0.750***	0.752***	0.876***	0.126**
	(0.189)	(0.187)	(0.189)	(0.063)	(0.173)	(0.164)	(0.160)	(0.052)
Retail_demand	0.025	-0.164	-0.582**	-0.606***	0.606***	0.818***	1.161***	0.555***
	(0.157)	(0.193)	(0.249)	(0.117)	(0.135)	(0.178)	(0.205)	(0.093)
Panel C. Sorting on Arbitrage Constraint								
AP Concentration	0.037	-0.162	-0.308	-0.345***	0.750***	0.847***	0.828***	0.079
	(0.204)	(0.176)	(0.194)	(0.058)	(0.178)	(0.149)	(0.170)	(0.056)
Ivol	-0.058	0.005	-0.461*	-0.403***	0.496***	0.773***	1.105***	0.609***
	(0.112)	(0.204)	(0.248)	(0.156)	(0.079)	(0.184)	(0.234)	(0.168)
Bid-Ask Spread	0.026	-0.174	-0.307	-0.333***	0.635***	0.796***	0.936***	0.301***
	(0.164)	(0.191)	(0.209)	(0.078)	(0.159)	(0.170)	(0.171)	(0.070)
Amihud Illiquidity	-0.028	-0.222	-0.191	-0.163**	0.618***	0.890***	0.828***	0.210***
	(0.163)	(0.194)	(0.206)	(0.068)	(0.158)	(0.165)	(0.179)	(0.069)
Arbi_constraint	0.019	-0.159	-0.441**	-0.459***	0.589***	0.858***	1.005***	0.416***
	(0.155)	(0.203)	(0.222)	(0.086)	(0.140)	(0.177)	(0.199)	(0.088)

Table A5: Overnight/Intraday Return Persistence/Reversal in the Equity-Only ETFs

This table reports monthly overnight and intraday return persistence and reversal patterns in the equity ETFs from January 2004 to December 2021. The left panel presents raw returns, while the right panel reports the Fama–French–Carhart four-factor-adjusted returns. In Panel A, ETFs are sorted into quintiles at month-end based on their lagged one-month overnight returns; in Panel B, sorting is based on lagged one-month intraday returns. A long–short strategy is formed by going long in the equal-weight winner quintile and short in the equal-weight loser quintile. Standard errors, reported in brackets, are adjusted for heteroskedasticity and autocorrelation using the Newey and West (1987) procedure with three lags. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Equity ETFs Portfolios Sorted by Lagged One-Month Overnight Returns						
	Raw Return			Four-Factor-Adjusted Return		
Quintile	Overnight	Intraday	Overnight–Intraday	Overnight	Intraday	Overnight–Intraday
1	0.266	0.181	0.084	-0.253	-0.319**	-0.032
	(0.209)	(0.226)	(0.242)	(0.182)	(0.133)	(0.268)
2	0.642***	0.136	0.506**	0.151	-0.414***	0.467*
	(0.176)	(0.217)	(0.232)	(0.149)	(0.125)	(0.259)
3	0.814***	0.003	0.811***	0.330**	-0.540***	0.772***
	(0.173)	(0.224)	(0.242)	(0.143)	(0.135)	(0.269)
4	1.050***	-0.206	1.256***	0.568***	-0.765***	1.235***
	(0.179)	(0.236)	(0.256)	(0.145)	(0.144)	(0.277)
5	1.622***	-0.887***	2.509***	1.194***	-1.412***	2.508***
	(0.196)	(0.236)	(0.282)	(0.159)	(0.167)	(0.287)
5-1	1.356***	-1.068***	2.424***	1.349***	-1.191***	2.442***
	(0.161)	(0.154)	(0.215)	(0.173)	(0.149)	(0.233)
Panel B. Equity ETFs Portfolios Sorted by Lagged One-Month Intraday Returns						
1	1.491***	-0.963***	2.454***	0.986***	-1.555***	2.443***
	(0.192)	(0.247)	(0.273)	(0.176)	(0.150)	(0.271)
2	0.977***	-0.199	1.175***	0.462***	-0.747***	1.111***
	(0.186)	(0.228)	(0.241)	(0.147)	(0.133)	(0.262)
3	0.787***	0.033	0.754***	0.293**	-0.504***	0.698***
	(0.181)	(0.224)	(0.243)	(0.147)	(0.135)	(0.266)
4	0.655***	0.172	0.483**	0.185	-0.355**	0.443
	(0.177)	(0.221)	(0.242)	(0.148)	(0.144)	(0.277)
5	0.480***	0.185	0.295	0.059	-0.286*	0.248
	(0.186)	(0.228)	(0.254)	(0.149)	(0.162)	(0.276)
5-1	-1.011***	1.148***	-2.159***	-1.025***	1.171***	-2.293***
	(0.146)	(0.176)	(0.212)	(0.165)	(0.176)	(0.216)

Chapter 2

When Firms are Watched by Patent Trolls: A Feedback Effect of Patent Troll Litigation Risk

Abstract – We investigate how firms adjust their cash holdings in response to the rent-seeking behavior of patent trolls. We construct a novel measurement of the patent troll litigation risk based on the digital footprint of patent trolls when they view financial statements on the SEC EDGAR website. Exploiting the staggered adoption of the anti-troll laws interacted with the patent troll litigation risk measurement, we find that when facing patent troll litigation risks, firms strategically reduce their cash holdings to lower the probability of being sued. Moreover, by implementing cash flow analysis, we find that firms adjust their cash holdings mainly by changing their financing activities; operating and investing activities do not respond to the patent troll litigation risk. Overall, the evidence suggests that patent troll litigation risk is an important factor in determining a firm's cash and financing policies.

Keywords: Patent Troll, Litigation Risk, Rent-seeking, EDGAR, Anti-troll Regulation, Cash Holding.

JEL classification: G31, G32, G38, O34, O25

1 Introduction

It is well known that innovation is a crucial factor in driving long-term economic growth. Clearly defined property rights are essential for fostering innovation, as they provide inventors with the security necessary to invest in new ideas. However, intellectual property rights (IPRs) for assets such as computer programs, trademarks, and industrial designs are difficult to define because distinguishing similar ideas or designs is inherently challenging. To address these complexities and to safeguard the economic value of intellectual property, many countries have established patent systems designed to protect inventors' IPRs. Typically, patent holders are granted exclusive rights to commercialize their inventions for a specified period, allowing them to earn monopoly profits if they successfully bring their products to market. Consistent with this rationale, a recent study by the European Union Intellectual Property Office reports that firms holding at least one intellectual property right generate 20% higher revenue per employee than firms without intellectual assets (Ménière et al., 2021).

While the importance of IPRs is undeniable, the officially granted rent-seeking powers associated with patents can potentially result in high social costs. In particular, abusive patent infringement claims can disrupt firms' normal operations, such as selling authentic goods and services and undertaking research and development (R&D) programs. Among other growing problems, the abusive patent infringement claims from patent trolls have received substantial attention from governments and researchers. Patent trolls, typically defined as non-practicing entities (NPEs) and non-competing entities (NCEs) (Chien, 2014; Bessen and Meurer, 2014), differ from other patent holders by primarily targeting companies with deep pockets (Cohen, Gurun, and Kominers, 2019) and companies lacking financial resources or legal expertise to defend against infringement allegations (Chien, 2014; Dayani, 2023). Unlike entities that assert patents to protect their economic interests, patent trolls exploit the patent system for financial gain. The typical profit model of a patent troll is to send "demand letters" to firms, claiming rights to certain patents and demanding licensing fees. Targeted firms need to decide whether to pay the licensing fees in exchange for potentially expensive patent infringement litigation.

A large body of literature documents the high economic costs of patent troll activity. Bessen, Ford, and Meurer (2011) estimate that publicly listed firms lost more than half a trillion dollars to patent troll lawsuits between 1990 and 2010. Beyond direct financial losses, patent troll assertions depress innovation, employment, financing, and firm valuation. Cohen, Gurun, and Kominers (2019) find that firms losing troll lawsuits reduce investment by more than \$163 million in the subsequent year. Exploiting the staggered adoption of anti-troll laws in 34 states, Appel, Farre-Mensa, and Simintzi (2019) show that curbing troll litigation improves the financing environment and employment outcomes for high-tech startups. Similarly, Duan (2023) shows that litigation exposure induces high-tech firms to rely excessively on equity financing, and Dayani (2023) documents that troll litigation reduces acquirer valuations in M&A transactions.

Given these significant effects, understanding how firms manage patent troll litigation risk is crucial. Rent-seeking as a form of redistributive activity is pervasive in economies. Typical rent-seeking activities include lobbying, corruption, bribery, and litigation (Murphy, Shleifer, and Vishny, 1993; Khan and Jomo, 2000). Patent trolls who aim to acquire license fees by threatening firms with patent litigation are a typical type of rent-seeker.

Firms facing rent-seeking threats can respond in two ways. First, they may adopt defensive measures to lower their exposure, such as reducing cash holdings. Studies of corruption and union wage bargaining show that firms under greater rent-extraction pressure adopt more opaque disclosure policies and hold less cash to protect assets from expropriation (Klasa, Maxwell, and Ortiz-Molina, 2009; Stulz, 2005; Smith, 2016). By analogy, in anticipation of potential targeting by patent trolls, firms with substantial cash holdings may strategically decrease their cash holdings to lower the likelihood of becoming litigation targets. This proactive adjustment serves as a defensive strategy against the rent-seeking behavior of patent trolls.

Alternatively, firms may accumulate cash in response to litigation risk. Precautionary savings theory predicts that firms increase cash reserves in the presence of heightened cash-flow uncertainty (Han and Qiu, 2007). Anticipating potential licensing fees, firms may preemptively build cash buffers to avoid costly external financing in the future. Another reason for increasing cash holdings is that companies may hold cash to deter patent trolls by signaling their ability to build a strong legal team and fight them in a lawsuit.

Studying firms' response to patent troll litigation risk presents two key empirical challenges. First, researchers must construct a reliable proxy for ex ante litigation risk. Second, they must address endogeneity: high cash holdings may increase the likelihood of being targeted by trolls, whereas litigation risk may induce firms to adjust their cash holdings. This bidirectional relationship complicates causal inference.

To address the first challenge, we exploit the digital footprints left by patent trolls when they access SEC filings. Prior to initiating lawsuits, trolls typically gather evidence to substantiate their claims, and SEC filings provide detailed information on firms' financials and operations. We use the EDGAR log file, which records the download and viewing history of all SEC filings for U.S.-listed firms, to construct firm-quarter measures of litigation risk. Specifically, we recover full IP addresses from masked logs using the mapping developed by Chen et al. (2020), then identify IP holders using the Whois website, and classify addresses associated with patent trolls via name matching. Following Drake, Roulstone, and Thornock (2015), we construct two proxies: (i) the total number of downloads by patent trolls and (ii) the number of unique trolls downloading a firm's filings more than once per quarter.

We validate these proxies by testing their ability to predict future litigation events. Both measures significantly forecast patent troll lawsuits even after controlling for known predictors, and the predictive power remains robust under entropy balancing.

We then examine whether firms adjust cash holdings in response to litigation risk. To establish causal inference, we exploit the staggered adoption of anti-troll laws across 34 states and implement stacked difference-in-differences (DID) regressions. Our identification relies on the notion that firms with higher pre-law litigation risk benefit more from the protection afforded by anti-troll laws. Consistent with this prediction, we find that the probability of being sued declines more for high-risk firms, and these firms increase their cash ratios more than others following the legislation.

We further explore heterogeneity in these effects. Firms' responses to these legislative changes do not only rely on their likelihood of facing patent troll litigation, but also on the potential damage such litigation could inflict. Previous studies indicate that small, young, innovative, or high-tech firms experience more severe adverse effects of patent troll litigation. Therefore, we expect those firms to exhibit stronger responses to the anti-troll laws. Consistent with this prediction, our cross-sectional analysis shows that these firms display larger increases in cash holdings following the adoption of anti-troll laws.

Although stacked DID regression is a relatively clean identification strategy, we still conduct three additional tests to assess the model's validity. First, we redo our tests with a subsample excluding firm-quarters with ongoing patent troll lawsuits, to reduce the effect of lawsuits. Next, we use the parallel trend analysis to verify the parallel trend assumption and find that there is no significant pre-event difference. Lastly, we perform a placebo test to verify the effectiveness of the law change by falsely changing the timing of the anti-troll laws. Overall, all results support the robustness of the baseline DID model.

While we have established a negative relationship between patent troll litigation risk and cash holdings, the mechanisms through which firms adjust their cash holdings remain unclear. Firms derive their cash flows from three sources: operating, investing, and financing activities. Firms may change their cash holdings by adjusting inventories, capital expenditures, R&D expenses, debt financing, and equity financing. To detect how firms modify their cash holdings, we perform a cash flow analysis. Our findings indicate that changes in patent troll litigation risk primarily influence financing cash flows. Specifically, following the enactment of anti-troll laws, firms with high ex-ante patent troll litigation risks exhibit an increase in their financing cash flow to asset ratio. Further analysis reveals that this uptick in financing activities is predominantly driven by an escalation in long-term debt financing. These results are in line with the theory that firms dynamically adjust their operating and financing risks. When facing high patent troll litigation risks, firms choose relatively low financial leverage and preserve their debt financing capacity.

To sum up, our results indicate that firms strategically reduce cash holdings in response to patent trolls' rent-seeking behavior. By analyzing the mechanism of the change in cash ratio, we also find that firms retain their debt financing capacity when facing high patent troll litigation risk. Together, these findings imply that patent troll litigation risk is a significant determinant of firms' cash and financing policies.

This paper makes several contributions to the existing literature. First, the paper contributes to the literature on the effect of patent trolls by showing the feedback effect of patent troll rent-seeking behaviors. Previous studies on patent trolls focus on either the ex-post effect of a patent troll litigation/rent-seeking (Bessen, Ford, and Meurer, 2011; Chien, 2014; Cohen, Gurun, and Kominers, 2019) or the ex-post effect of the anti-troll regulations (Appel, Farre-Mensa, and Simintzi, 2019; Dayani, 2023; Duan, 2023). In contrast, this paper proposes a new measurement to gauge the expected patent troll litigation risk faced by a firm. With the new measurement, this paper uncovers a hidden cash strategy of a firm when it faces high patent troll litigation risks. By analyzing the mechanism of the cash adjustment, the paper also finds that firms change their cash holding by relying more on external debt financing.

Second, the paper provides new evidence of firms' responses to rent-seeking threats. Prior literature mainly focuses on firms' responses to the rent-seeking activities of union labor and government (Stulz, 2005; Klasa, Maxwell, and Ortiz-Molina, 2009; Smith, 2016). This paper extends the previous studies by looking at firms' responses to a novel type of rent-seeker: patent trolls. Consistent with the rent-seeking literature, the paper also finds that firms will strategically reduce their cash holding in response to the threat of patent trolls' litigation, implying that this strategic response may be a general reaction of firms when facing different types of rent-seeking.

Our paper is also related to the literature that explores the usage of company information from the EDGAR database. A series of papers show that the aggregate downloading volume of EDGAR filings is not only correlated with the earnings announcement or the merger and acquisition (M&A) but also correlated with accounting quality, financial performance, market sentiment, and stock price movement of a firm (Drake, Roulstone, and

Thornock, 2015; Drake, Roulstone, and Thornock, 2016; Drake et al., 2017; Drake et al., 2019a). Beyond aggregating information acquisition behaviors across all investors, several studies have focused on specific entities accessing EDGAR filings. For example, auditors can utilize their non-client EDGAR filings as their disclosure benchmark or a resource to explore new clients (Drake et al., 2019b; Hallman, Kartapanis, and Schmidt, 2022). Institutional investors can construct their trading strategies by inspecting their competitors' 13F filings or insider trading filings (Cao et al., 2021; Chen et al., 2020) . The digital footprints of government departments in the EDGAR log file can also be used to predict future regulations (Bozanic et al., 2016; Holzman, Marshall, and Schmidt, 2024). Similarly, our study builds on this framework by constructing a patent troll litigation risk measurement using the digital footprint of patent trolls as they access EDGAR filings. We demonstrate that the frequency and patterns of patent trolls' access to financial information can significantly predict future patent litigation.

Lastly, our work also contributes to the existing literature on corporate cash policies. Previous studies have proposed several motivations and determinants of firms' cash holding policies, including growth opportunities, corporate governance, organization structure, cash flow risk, and external financing constraints (Opler, 1999; Dittmar and Mahrt-Smith, 2007; Subramaniam et al., 2011; Chang et al., 2014; Bates, Kahle, and Stulz, 2009). However, few studies have specifically investigated how firms strategically adjust their cash holdings in response to rent-seeking behavior. Our study extends this line of research by examining the feedback effect of patent trolls' cash-targeting behavior on firms' cash policies. By demonstrating that firms strategically reduce their cash holdings to mitigate patent troll litigation risks, our research enriches the understanding of how firms adapt their cash policies in response to external threats.

The remainder of the paper is organized as follows: Section 2 presents the development of the hypotheses. Section 3 discusses how we construct our sample and measure the patent troll litigation risk, as well as the descriptive statistics; Section 4 presents the empirical results. In Section 5, we conclude the paper.

2 Hypothesis Development

Rent-seeking activities, which involve capturing economic rents through resource redistribution rather than productive activity, are pervasive in modern economies. Murphy, Shleifer, and Vishny (1993) categorize rent-seeking into private-sector activities—such as theft, piracy, and litigation—and public-sector activities, including taxation, lobbying, and corruption. Most prior research has focused on the consequences of public-sector rent-seeking. For example, Murphy, Shleifer, and Vishny (1993) and Boldrin and Levine (2004) show that government rent extraction distorts the social returns to innovation and impedes long-run economic growth. Caballero and Yared (2010) further demonstrate that expectations of future government rent-seeking influence public debt accumulation and fiscal deficits. At the firm level, public-sector rent-seeking has been shown to shape corporate policies, including cash holdings (Smith, 2016) and governance structures (Chen et al., 2011).

However, limited attention has been paid to understanding the effects of private-sector rent-seeking. A notable exception is research on labor unions, which documents their impact on wage bargaining and firm cash policies (Connolly, Hirsch, and Hirschey, 1986; Klasa, Maxwell, and Ortiz-Molina, 2009). One possible explanation for this gap is that most private entities lack the institutional leverage to engage in systematic rent extraction. This paper contributes to this literature by examining a novel form of private rent-seeker: patent trolls. Patent trolls hold patents granting exclusive commercialization rights but, unlike other patent holders, do not seek to develop or market the underlying inventions. Instead, they plan to generate revenue by demanding license fees from other firms, irrespective of whether these firms compete with or infringe on their business. Similar to labor unions, patent trolls have strong incentives to target cash-rich firms (Cohen, Gurun, and Kominers, 2019). Given the documented adverse effects of patent troll litigation on firm performance and innovation (Bessen, Ford, and Meurer, 2011; Chien, 2014; Cohen, Gurun, and Kominers, 2019), we expect firms to actively manage their exposure to this risk.

Smith (2016) suggests that firms respond to rent-seeking threats using two broad strategies: shielding (reducing the resources available for expropriation) and liquidity management (building buffers to absorb shocks). We adopt this framework and argue that firms may adjust their cash holdings in two opposing directions in response to patent troll litigation risk.

First, firms may strategically reduce cash holdings to make themselves less attractive targets for litigation. Empirically, cash-rich firms are more likely to be sued by patent trolls (Cohen, Gurun, and Kominers, 2019), implying that reducing cash reserves can lower the probability of being targeted. Moreover, rent-seeking entities often calibrate their demands to a firm's ability to pay. Svensson (2003), for example, finds that bribe payments in Uganda are positively correlated with firms' financial capacity. By analogy, patent trolls may demand larger settlements from cash-abundant firms, thereby providing firms with an additional incentive to minimize visible cash balances.

Moreover, we also expect heterogeneity in this effect. Prior research shows that patent troll litigation disproportionately affects startups, small firms, and high-tech companies by reducing employment, innovation, and financing capacity (Appel, Farre-Mensa, and Simintzi, 2019). Theoretical work further suggests that rent-seeking suppresses innovative activity (Murphy, Shleifer, and Vishny, 1993; Boldrin and Levine, 2004). Consequently, we hypothesize that smaller, younger, more innovative, or technology-intensive firms will respond more aggressively by reducing their cash holdings. We summarize our first group of hypotheses as follows:

H1a*: Firms will reduce their cash holding in response to the patent troll litigation risk.*

H1b*: Compared with others, firms that are small, young, innovative, or in the high-tech industry will reduce their cash holdings more when facing the same level of patent troll litigation risk.*

Second, firms may alternatively increase cash holdings as a precautionary response to litigation risk. According to the precautionary savings theory, firms accumulate cash when facing greater cash flow uncertainty to avoid costly external financing in the future (Han and Qiu, 2007). Patent troll litigation introduces unpredictable cash outflows, thereby elevating cash flow volatility and strengthening the case for precautionary savings.

Additionally, cash holdings can also serve as a deterrent against potential risks. Harford (1999) documents a negative relation between cash holdings and the probability of becoming a takeover target, and Faleye (2004) characterizes this as the "takeover-deterrence" effect of cash. Kim and Bettis (2014) similarly find that cash holdings can deter competitive threats. By analogy, firms may maintain higher cash balances to signal their willingness and ability to contest lawsuits, thereby discouraging patent trolls. Firms with greater legal and financial resources can afford prolonged litigation, raising the expected cost for plaintiffs and potentially deterring future claims.

Under the second hypothesis, we also expect the incentive to adjust cash holding to vary with firms' characteristics. Those most vulnerable to patent troll litigation, such as small, young, innovative, and high-tech firms, should exhibit the strongest precautionary motives. Therefore, we summarize our second set of hypotheses as follows:

H2a: *Firms will raise their cash holding in response to the patent troll litigation risk.*

H2b: *Compared with others, firms that are small, young, innovative, or in the high-tech industry will increase their cash holdings more when facing the same level of patent troll litigation risk.*

3 Data, Sample and Descriptive Statistics

Five datasets are employed in this study: (1) the EDGAR Log File data, which contains information about viewing or downloading SEC filings at the IP level; (2) the patent litigation data from the Standford NPE Litigation Dataset; (3) quarterly fundamental information of firms from Compustat; (4) stock market data from CRSP; and (5) the patent data retrieved from the database created by Kogan et al. (2017). In this section, we describe how we construct the sample and the patent troll litigation risk measurement.

3.1 Patent Troll Litigation Risk

As discussed in the previous section, patent trolls' information acquisition activities should be positively associated with subsequent litigation events. To quantify these activities, we begin by systematically identifying patent trolls. In this study, we define patent trolls as non-practicing entities (NPEs) that are neither government agencies, universities, nor non-profit organizations, and as non-competing entities (NCEs) that initiate lawsuits against firms in unrelated industries. We use the Stanford NPE Litigation Dataset, which contains all patent-related lawsuits from 2000 to 2020 and classifies patent asserters into 13 categories. We exclude Category 6, which represents patent asserters affiliated with universities, government bodies, or non-profits. To obtain industry classifications for both asserters and infringers, we merge the Stanford dataset with Compustat using fuzzy name matching and assign industries based on the first two digits of the Standard Industrial Classification (SIC) code. We then exclude cases in which the asserter and infringer operate within the same industry, focusing on cross-industry litigation, which is more consistent with rent-seeking behavior.

Next, we obtain records of viewing financial filings from the EDGAR log file, which are available from January 1, 2003, to June 30, 2017. In this paper, financial filings refer to the "10-K", "10-Q", "6-K", and "20-F" filings. Given the richness and accessibility of these data, we posit that patent trolls extensively use EDGAR information before initiating lawsuits against public companies. In particular, patent trolls can access detailed sales data within financial statements to gather evidence supporting their litigation claims.

The EDGAR log file provides granular data for each request, including the requester's IPv4 address. The fourth octet of the IP address is anonymized using a static cipher; we recover the true IP addresses using the decryption key provided by Chen et al. (2020). To ensure data quality, we exclude unsuccessful requests, index-page requests, file downloads smaller than 500 bytes, and automated (robot) downloads. Robot activity is identified using the algorithm developed by Ryans (2017), which applies three criteria: (1) humans cannot download more than 25 items per minute; (2) humans cannot download items from more than three different companies in a minute; and (3) humans cannot download more than 500 items in a single day. After applying these filters, we merge the cleaned log data with the EDGAR index files using the accession number.

We then match IP addresses to patent trolls. This practice relies on the IP information provided by the Whois website (https://whois.arin.net/ui/). This website allows us to retrieve information about IP addresses associated with entities based on their names, or vice versa. To maximize match accuracy, we construct search keywords using the first two words of each patent troll's name and employ fuzzy name-matching algorithms to refine results in cases with multiple potential matches. We complement Whois data with IP addresses collected from the official websites of patent trolls. Because telecommunication companies (SIC 4800–4899) typically own large IP address pools that serve numerous end-users, their IP addresses cannot be reliably attributed to firm-specific viewing behavior. We therefore exclude all requests from this sector to improve identification.

Once we establish the mapping between IP addresses and patent trolls, we aggregate their EDGAR activity at the firm–quarter level. We construct two measures of patent troll litigation risk. The first measure is the total number of downloads of a firm's filings by all identified patent trolls within a quarter. The second measure is the number of "active" patent trolls—defined as trolls that access a firm's financial filings at least twice in a given quarter. These measures follow the "search volume" concept developed by Drake, Roulstone, and Thornock (2015), but differ in two ways: (1) we compute them at the quarterly frequency rather than at higher frequencies, and (2) in the second measure, we exclude one-off, likely uninformative downloads to focus on persistent information acquisition activity.

3.2 Linking Patent Troll Litigation Risk to Patent Infringers

The Stanford NPE Litigation Database contains the names of patent infringers. However, these names do not always match those reported in Compustat, and Compustat records only reflect the most recent firm name, without historical information. Directly merging the two datasets by firm name would therefore produce substantial mismatches. To address these issues, we first recover historical Compustat firm names by linking GVKEYs to company names in the CRSP database. We then standardize firm names across datasets using the dictionary provided by Arqué-Castells and Spulber (2022). After standardization, we compute the Cartesian product of firm names across the two datasets and calculate pairwise similarity scores. We drop all pairs with a similarity score below 80 and, in cases of multiple matches, retain the pair with the highest similarity score. This procedure yields 4,476 unique links between infringer names and Compustat companies.

Compustat also reports a Central Index Key (CIK) for each entity, but again, only the most recent CIK is available. To recover the historical CIK code, we utilize the CIK-GVKEY link provided by WRDS. The link is constructed from CUSIP records in 13D/G filings. WRDS first gets CUSIPs from the companies' 13D/G filings and then uses CUSIPs to link CIKs and GVKEYs. It is not surprising that the map contains duplicates. For instance, in 2000, the EDGAR database lists AT&T filings under CIK 0000005907 with the name "AT&T CORP," but the CIK–GVKEY link maps this CIK to three different GVKEYs corresponding to "AT&T WIRELESS SERVICES INC," "STARZ," and "AT&T CORP."

To reduce ambiguity, we refine the CIK–GVKEY mapping using two rules. First, we require that each CIK be linked to at most one GVKEY per month. When multiple

GVKEYs are linked to the same CIK within a month, we retain the record with the highest WRDS matching-flag value; if ties remain, we keep the match with the highest name similarity score. Second, we impose that each GVKEY be linked to as few CIKs as possible within a month, while allowing multiple CIKs per GVKEY to accommodate subsidiaries. We further refine these cases by prioritizing records verified by historical Compustat CIK–GVKEY mappings. If no verified record exists, we retain only those with name similarity above 80 between the SEC-reported name and the Compustat-associated GVKEY name.

Through this procedure, we establish two key linkages: (1) a mapping between patent asserters, IP addresses, and CIKs, and (2) a mapping between patent infringers, CIKs, and GVKEYs. Using CIKs as a bridge, we connect asserters' EDGAR viewing activity to infringers at the GVKEY level, allowing us to construct firm-level measures of patent troll litigation risk.

3.3 Sample

Our core dataset is retrieved from the Compustat and CRSP merged (CCM) database from WRDS after deleting firm-quarters that have negative assets or sales. Our sample period is from 2007Q1 to 2017Q2. The sample period is constrained by the availability of IP data in the EDGAR log file. Although the EDGAR log file contains masked IP records from January 2003 to June 2017, the data prior to 2007 exhibits significant gaps (Ryans, 2017). To mitigate potential biases from missing observations, we restrict the sample to the post-2007 period. Consistent with prior literature, we also exclude financial institutions (SIC 6000–6999) and regulated utilities (SIC 4400–4999) from our sample. Because our measure of litigation risk is based on patent trolls' information acquisition behavior via EDGAR, we further restrict the sample to firms that are required to file with the SEC. As a result, our sample comprises only companies listed on the NYSE, AMEX, and NASDAQ. Finally, we drop firm-quarters with missing state identifiers, as we exploit state-level changes in patent-litigation regulations to identify causal effects. These filters yield an unbalanced panel of 87,646 firm-quarter observations.

Next, we obtain firm-level patent data from the database constructed by Kogan et al. (2017), which links CRSP firm identifiers to patents granted by the USPTO between 1926 and 2019. We then merge the patent data, aggregated patent troll viewing measures, and litigation data with the CCM dataset. For firms that do not appear in the litigation, viewing, or patent databases, we assign zeros for patent ownership, patent troll views, and patent troll litigation counts. Our primary dependent variable is cash holdings, defined as cash and cash equivalents scaled by total assets.

3.4 Summary Statistics

For comparability with prior studies, Table 1 reports summary statistics for the variables of interest and control variables used in our analyses. To mitigate the influence of outliers, we winsorize all ratio variables at the 1st and 99th percentiles within each quarter. For variables exhibiting substantial positive skewness, we apply natural logarithm transformations in subsequent analyses but report raw values in Table 1, except for the litigation risk proxies, which are presented in log form. After transformation, the litigation risk proxies display approximately symmetric distributions, suggesting that skewness has been effectively mitigated. Variable definitions are provided in Table A1.

Table 2 presents descriptive statistics separately for firm-quarters that are sued by patent trolls and those that are not. We observe systematic differences between the two groups. On average, sued firm-quarters exhibit significantly higher values of both litigation risk proxies: Log_view_num is 1.02 units higher and Log_troll_num is 0.70 units higher than in non-sued firm-quarters, consistent with the notion that patent trolls engage in more extensive information acquisition prior to initiating litigation. Our main variable of interest, the cash-to-asset ratio, is slightly higher among sued firm-quarters, but the difference is statistically insignificant.

Table 2 further highlights significant differences across several control variables. Compared to non-sued firm-quarters, sued firm-quarters tend to be larger, more innovative, and hold more patents, but have lower physical capital intensity (PPE_ratio). While these observable characteristics can be controlled for in regression analyses, unobservable heterogeneity between the treatment and control groups may still bias OLS estimates. To mitigate this concern, we implement entropy balancing, which reweights the control group to achieve covariate balance across observable characteristics before estimating treatment effects.

4 Empirical Analysis

4.1 Validity of Using Patent Troll Viewing as Patent Troll Litigation Risk

4.1.1 Results from Panel Regression

We begin our empirical analysis by verifying the validity of our patent troll litigation risk measurement. The logic behind the validity verification is that if our measurement serves as a proxy for patent troll litigation risks, companies with high patent troll viewing should be associated with a higher probability of being sued by a patent troll in the future. To implement the test, we first fit a simple linear regression model that regresses the patent-troll-lawsuit dummy on the patent-troll-litigation risk proxies. The ordinary least squares (OLS) regression specification is shown in the following:

$$SUE_{i,t+1} = \beta_0 + \beta_1 LitigationRisk_{i,t} + \beta_2 X_{i,t} + FEs + \epsilon_{i,t+1} \tag{1}$$

where $SUE_{i,t+1}$ is a dummy variable equal to one if firm i is sued by a patent troll in quarter t+1. $LitigationRisk_{i,t}$ is our key independent variable, measured as either the logarithm of the number of patent troll views or the logarithm of the number of unique active patent trolls targeting firm i in quarter t. $X_{i,t}$ denotes a vector of control variables following Cohen, Gurun, and Kominers (2019), including market value, book-to-market ratio, total assets, cash holdings, R&D expenditures, the number of patents granted in the past five years, and prior 12-month stock returns.

We include a rich set of fixed effects (FEs). Industry or firm fixed effects capture time-invariant unobserved heterogeneity at the industry or firm level that may be correlated with the likelihood of patent troll litigation. Time fixed effects control for aggregate time-series variation in litigation propensity, including trends in overall litigation activity.

All continuous control variables are log-transformed to mitigate the influence of extreme observations. Standard errors are clustered at the firm level to account for serial correlation in litigation risk across time for the same firm. Table 3 reports results from alternative specifications using these baseline controls.

Table 3 documents a strong and consistent pattern: firms with higher levels of patent troll information acquisition—measured either by the number of patent troll views or the number of unique active patent trolls—are significantly more likely to face litigation in subsequent quarters. Columns (1) and (2) present our preferred specifications, which regress the future lawsuit indicator on our litigation risk measures, a comprehensive set of firm-level controls, and industry and time fixed effects. We favor industry fixed effects over firm fixed effects for three reasons. First, our primary interest lies in the cross-sectional validity of the litigation risk measures. Second, including firm fixed effects together with time fixed effects absorbs most of the variation in the key independent variables, leaving little identifying variation. Third, we expect patent troll litigation risk to vary slowly over time and not fluctuate meaningfully at a quarterly frequency. Nevertheless, we report specifications with firm fixed effects in Columns (3) and (4) as a robustness check, and the results remain qualitatively unchanged.

Controlling for firm characteristics and fixed effects, the coefficient on Log_view_num is 0.0218 (t = 10.90) in Column (1), and the coefficient on Log_troll_num is 0.0635 (t = 12.21) in Column (2). Both coefficients are economically and statistically significant. To assess magnitudes, we benchmark the results to a one–standard deviation change in each measure. A one–standard deviation increase in Log_view_num is associated with a 2.78 percentage point higher likelihood of being sued relative to other firms in the same industry, while a one–standard deviation increase in Log_troll_num corresponds to a 4.38 percentage point increase. Given that the unconditional probability of being sued for patent infringement in our sample is 6.44%, these effects represent economically meaningful increases of 43.17% and 68.01%, respectively.

Overall, the results in Table 3 confirm that our litigation risk proxies have substantial predictive power. The positive and highly significant coefficients indicate that patent troll monitoring of firms' SEC filings is a strong leading indicator of future litigation, even after conditioning on an extensive set of controls and fixed effects.

4.1.2 Robustness Check: Entropy Balancing and Logit Regression

To assess the robustness of our litigation risk measures, we conduct several additional tests examining their ability to predict future patent troll litigation.

Our first robustness check addresses the concern that our preferred specification may not fully account for unobserved firm characteristics correlated with both patent troll monitoring of SEC filings and the likelihood of subsequent litigation. A common approach to mitigating this concern is propensity score matching (PSM), which reweights or discards control observations to align the covariate distribution between treated and untreated firms. However, PSM has well-documented limitations. First, the implementation requires several subjective choices, such as selecting the number of neighbors k in k-nearest neighbor matching. Second, achieving covariate balance is often difficult in practice and may require multiple iterations with ad hoc adjustments. Third, there is no universally accepted criterion for determining when sufficient balance has been achieved, which makes it challenging to evaluate the quality of the matched sample.

To address these shortcomings, we adopt an entropy balancing approach. Entropy balancing applies a maximum-entropy reweighting algorithm to the control group, assigning observation-specific weights to exactly satisfy pre-specified moment conditions (e.g., means, variances, and higher-order moments) of the covariate distributions between treated and control firms. The key advantage of this approach is that it is fully rule-based and retains the entire sample, avoiding information loss from discarding unmatched observations.

Because entropy balancing was originally developed for cross-sectional data, we adjust the method for our panel setting. Specifically, we slice the panel into cross-sections by quarter (or by industry-quarter) and balance on firm characteristics measured prior to litigation events. This approach mitigates concerns about forward-looking bias and contamination from litigation's contemporaneous effects on firm characteristics. While some studies recommend balancing only on variables significantly related to the outcome, we adopt a more conservative approach and balance on all control variables used in our baseline specification, targeting their first moments to construct a more comparable counterfactual sample. We then re-estimate our main regressions on the reweighted sample, clustering standard errors at the firm level.

Table 4 reports the results. Columns (1) and (2) present the entropy balancing results at the quarter level, and Columns (3) and (4) report results using industry-quarter reweighting. Across all specifications, the coefficients on Log_view_num and Log_troll_num remain positive and statistically significant, confirming that patent troll monitoring activity reliably predicts future litigation.

It is worth noting that entropy balancing yields different effective sample sizes under the two settings. Some industry-quarters are dropped in the industry-quarter specification due to a lack of either treated or control observations, and in some cases, the reweighting algorithm fails to converge within a given industry-quarter. Nevertheless, industry-quarter balancing offers the advantage of fully absorbing unobservable industry effects, potentially yielding a more comparable control group. Importantly, the coefficient magnitudes are highly similar across the two entropy balancing settings, suggesting that our conclusions are not sensitive to the choice of reweighting approach.

As an additional robustness check, we re-estimate our baseline specification using a nonlinear logit model rather than the linear probability model. Columns (5) and (6) of Table 3 show that the coefficients on both litigation risk measures remain positive and statistically significant, consistent with our main results.

Taken together, these robustness analyses strongly corroborate the validity of our litigation risk proxies. The consistent results across weighting schemes and model specifications reinforce the conclusion that patent troll monitoring of SEC filings is a powerful predictor of subsequent litigation. In the next subsection, we leverage these proxies to examine how firms strategically respond to elevated litigation risk.

4.2 Patent Troll Litigation Risk and Cash Policy

Having established the validity of our patent troll litigation risk measures, we now investigate how firms adjust their cash holdings in response to elevated litigation risk. A natural starting point for this analysis would be a multivariable regression of cash holdings on litigation risk measures and firm-level controls. However, this approach faces a significant endogeneity concern arising from reverse causality. Specifically, patent trolls are known to target cash-rich firms, which may induce these firms to strategically manage their cash reserves in anticipation of potential litigation. As a result, simple cross-sectional or panel regressions risk producing biased estimates of the causal effect of litigation risk on cash holdings.

To mitigate this concern, we exploit plausibly exogenous legal shocks—namely, the staggered adoption of state-level anti–patent-troll laws—and implement a stacked DID design. Our identification strategy relies on the premise that firms facing higher ex ante litigation risk are more likely to benefit from these legal reforms. If litigation risk distorts firms' cash policies, we should observe a reversal (or at least an attenuation) of pre-legislation patterns of cash accumulation following the passage of anti-troll laws. This empirical design allows us to isolate the causal impact of litigation risk on corporate cash holdings while addressing concerns about reverse causality and omitted variable bias.

4.2.1 Background on Anti-Troll Laws

In response to growing concerns that abusive patent litigation was disrupting legitimate business activity, Congress proposed several reforms in 2011 aimed at curbing opportunistic patent assertion. However, none of these proposals were enacted at the federal level. In the absence of federal action, numerous state legislatures introduced their own regulations targeting abusive patent practices. These regulations—commonly referred to as anti-troll laws—are documented in Appel, Farre-Mensa, and Simintzi (2019). Vermont was the first state to adopt such legislation, and by the end of 2017, 34 states had enacted some form of anti-troll law. Figure 1 illustrates the geographic distribution of these reforms.

A salient feature of anti-troll laws is that they are typically framed as consumer protection laws, placing them under state jurisdiction rather than the federal jurisdiction governing patent law. This feature allows the laws to protect firms located in a state, regardless of their place of incorporation or the location of the patent asserter. This state-level heterogeneity creates a quasi-experimental setting that we exploit to identify the causal impact of patent troll litigation risks.

The primary objective of these laws is to deter bad-faith patent infringement claims. To achieve this, the laws generally empower courts to (i) require patent trolls to post a bond equal to the target firm's anticipated litigation costs, (ii) dismiss cases at an early stage, prior to the discovery phase, if the claims are deemed meritless, and (iii) authorize the state Attorney General to initiate legal action against serial patent trolls. Collectively, these provisions significantly raise the expected cost of initiating frivolous litigation and lower the probability that such lawsuits succeed.

In addition, the laws provide a range of remedies to targeted firms, including (i) equitable relief, (ii) compensatory damages, (iii) reimbursement of legal fees and related costs, and (iv) exemplary damages equal to $50,000 or triple the total of damages, costs, and fees. These remedies reduce the net cost of defending against frivolous suits and enhance firms' incentives to contest weak claims rather than settle.

Following Appel, Farre-Mensa, and Simintzi (2019), we treat the enactment of anti-troll laws as plausibly exogenous. First, these laws were enacted in both Republican- and Democratic-controlled states, suggesting that their passage is not driven by partisan preferences. Second, the legislation was supported by a broad coalition of stakeholders rather than a narrow set of industry interests, reducing the likelihood that passage is correlated with firm-specific lobbying. Third, prior work finds no evidence that the timing of adoption is systematically related to state-level economic conditions. Taken together, these features support the view that the staggered adoption of state-level anti-troll laws constitutes an exogenous shock suitable for causal inference.

4.2.2 Patent Troll Litigation Changes Around the Event

In the previous section, we documented that the enactment of anti-troll laws substantially raises the expected litigation costs for patent trolls and lowers their probability of winning, while simultaneously reducing litigation costs and strengthening the legal position of targeted firms. Importantly, the effect of these laws is unlikely to be uniform across all firms. Firms facing higher ex ante exposure to patent troll litigation should benefit disproportionately from these reforms. In other words, if anti-troll laws are effective, we expect to observe a greater reduction in litigation incidence among firms with higher initial litigation risk.

To formally test this prediction, we estimate the following stacked DID regression:

$$\begin{aligned}SUE_{c,i,j,t+1} = \beta_0 + \beta_1 LitigationRisk_{c,i,j} \times Pass_{c,j,t} + \beta_2 Pass_{c,j,t} + \beta_3 X_{c,i,j,t} \\ + CohortFirmFE + CohortTimeFE + \epsilon_{c,i,j,t+1}\end{aligned} \tag{2}$$

where c indexes the cohort-specific stack of the data, i indexes firms, j indexes states, and t denotes time. The dependent variable $SUE_{c,i,j,t+1}$ equals one if firm i is sued by a patent troll in quarter t+1. $Pass_{c,j,t}$ is an indicator variable that equals one if state j has enacted an anti-troll law in quarter t. $X_{c,i,j,t}$ is the same set of firm-level controls used in the previous subsection. β_1 is the coefficient that we are interested in. If the firms initially with higher litigation risk can be better protected under the anti-troll law, we should expect β_1 to be significantly negative.

We follow the standard approach to constructing stacked DID samples. First, we identify all state-level enactment events. For each event, we create a panel data spanning four quarters before and four quarters after the law's passage. Firms in states that never adopted anti-troll laws during our sample period serve as the control group. Using never-treated firms as controls has two advantages. First, it mitigates potential bias from anticipatory behavior by firms in states expecting future legislation. Second, never-treated firms constitute a stable control group, reducing sampling variation across cohorts.

To further mitigate endogeneity concerns, we remeasure litigation risk using the pre-treatment mean of Log_view_num or Log_troll_num over the four quarters preceding the law's passage. This averaging approach ensures that our litigation risk measure is predetermined with respect to the treatment window and avoids capturing shifts in viewing behavior caused by the legislation itself. Given that firms' exposure to patent trolls is unlikely to fluctuate substantially within a single year, the pre-event average serves as a valid proxy for litigation risk during the estimation window.

Table 5 reports the results. In Columns (1) and (2), where litigation risk is measured using the pre-event averages of Log_view_num and Log_troll_num, respectively, the estimated coefficients on the interaction terms are -0.0134 (t=2.23) and -0.0289 (t=2.51). These negative and statistically significant coefficients indicate that the likelihood of being sued declines at a greater rate for firms with higher pre-reform litigation risk, consistent with the hypothesis that anti-troll laws disproportionately benefit the most exposed firms.

Interestingly, the estimated coefficients on $Pass_{c,j,t}$ is positive and statistically significant. This result should not be interpreted as evidence that the laws increase litigation overall. Because our litigation risk variables are strictly positive (with sample means of 3.50 and 1.55, respectively), the unconditional average treatment effect is close to zero fter accounting for these means. Hence, the results provide strong evidence that the primary effect of anti-troll laws is concentrated among firms with elevated pre-reform litigation exposure.

4.2.3 Firms' Cash Policy and Patent Troll Litigation Risk: A Causal Inference

In the previous subsection, we documented that firms with greater pre-reform exposure to patent troll litigation benefit more from the enactment of anti-troll laws. Building on this finding, we now test Hypotheses H1a, H1b, H2a, and H2b.

H1 and H2 represent competing predictions regarding firms' cash-holding behavior in response to the risk of patent troll litigation. H1a posits that firms manage litigation risk by reducing their cash holdings ex ante, whereas H2a predicts that firms instead increase their cash holdings as a precautionary measure. Leveraging the exogenous shock provided by the staggered passage of anti-troll laws, we can reframe these hypotheses in a difference-in-differences framework. Specifically, under H1a, we expect that firms with higher pre-reform litigation risk will exhibit a stronger rebound in cash holdings following the enactment of anti-troll laws. Under H2a, by contrast, these firms would be expected to reduce their cash holdings after the laws are passed.

To empirically test these predictions, we estimate the following stacked DID regression model:

$$\begin{aligned} CashRatio_{c,i,j,t+1} = \beta_0 + \beta_1 LitigationRisk_{c,i,j} \times Pass_{c,j,t} + \beta_2 Pass_{c,j,t} + \beta_3 X_{c,i,j,t} \\ + CohortFirmFE + CohortTimeFE + \epsilon_{c,i,j,t+1} \end{aligned} \tag{3}$$

where c, i, j, and t denote cohort, firm, state, and time, respectively. The dependent variable is the cash ratio, defined as cash and cash equivalents scaled by total assets. $X_{c,i,j,t}$ represents the full set of determinants of corporate cash holdings identified in prior research (Opler, 1999; Haushalter, Klasa, and Maxwell, 2007; Bates, Kahle, and Stulz, 2009), including book-to-market ratio, cash flow, R&D expenditures, industry cash-flow volatility, a negative income indicator, industry Herfindahl-Hirschman Index (HHI), leverage, net working capital, capital expenditures, firm size, acquisition activity, dividend status, and the number of business segments. We also include a dummy variable to control for missing R&D data. The coefficient of interest, β_1, captures the differential change in cash holdings for firms with higher pre-reform litigation risk. A positive of β_1 indicates that firms with higher patent troll risk rebound their cash holdings at a greater rate following the passage of anti-troll laws.

Table 6 reports the regression results. Columns (1) and (2) present estimates based on the full sample, using the pre-event average of Log_view_num and Log_troll_num as measures of litigation risk, respectively. In both specifications, the interaction term coefficients are positive and statistically significant at the 1% level, providing strong support for H1a. The estimated β_1 is 0.0093 and 0.0127 in Columns (1) and (2), implying that, conditional on the passage of anti-troll laws, a one-standard-deviation increase in Log_view_num (Log_troll_num) is associated with a 1.20% (0.88%) increase in cash holdings. Relative to the sample median of cash holdings, these effects represent economically meaningful increases of 8.98% and 6.59%, respectively.

Finally, it is worth noting that the estimated coefficients on $Pass_{c,j,t}$ are negative and statistically significant. This should not be interpreted as evidence that anti-troll laws reduce cash holdings on average. Rather, the negative coefficient reflects that the litigation risk proxies are positive and have relatively large means, such that the conditional average treatment effect is close to zero.

4.2.4 Robustness Check: Subsample Test, Parallel Trend and Placebo Test

To ensure the robustness of our main findings, we perform a series of additional tests. We first consider the potential bias introduced by firms-quarters that are sued by patent trolls. Litigation events may mechanically influence cash holdings, thereby confounding our estimates. To mitigate this concern, we reconstruct the sample by excluding all firm-quarters that experience patent troll lawsuits within the estimation window. Columns (3) and (4) of Table 6 report the results. The estimated coefficients remain positive and statistically significant, and their magnitudes are comparable to those in the baseline specification. This finding reinforces the robustness of our main results.

Second, we conduct a placebo test to confirm that our results are driven by the actual passage of anti-troll laws rather than spurious correlations or unobserved time trends.

Following Appel, Farre-Mensa, and Simintzi (2019), we assign a "placebo" treatment date by shifting each state's actual passage date three years earlier and re-estimate the model. The placebo sample spans 2009Q2 to 2015Q2, with the earliest pseudo-event occurring in 2010Q2. Table 8 presents the results. Consistent with the placebo design, the interaction coefficients are statistically insignificant, with t-statistics of -0.72 and -0.50, and are economically negligible. These findings suggest that our baseline results are not artifacts of pre-existing trends or unrelated shocks.

Finally, we examine the parallel trend assumption, which is critical for causal inference in DID settings. In our context, the estimation is based on a triple-difference model, so we assess whether firms with differing ex ante litigation risks exhibit parallel pre-treatment dynamics. Specifically, we split the sample into high-risk and low-risk groups based on whether a firm's litigation risk measure exceeds or falls below the industry median. For each group, we estimate the following event-study specification:

$$\begin{aligned} CashRatio_{c,i,j,t+1} = \alpha + \sum_{t=-4}^{4} \beta_t \mathbf{1}_{c,j \in pass, time=t} + \gamma X_{c,i,j,t} + CohortFirmFE \\ + CohortTimeFE + \epsilon_{c,i,j,t+1} \end{aligned} \tag{4}$$

where β_t is the dynamic effect of anti-troll law passage relative to the event quarter. If the parallel-trend assumption holds, we expect that the pre-event β_t for the high- and low-risk groups to differ statistically. After the law changes, however, we anticipate a divergence in trends, with the high-risk group exhibiting a stronger increase in cash holdings.

Figures 2 and 3 present the estimated event-study coefficients. Two key patterns emerge. First, in the pre-event period, neither group exhibits a discernible trend, and the difference in coefficients between the two groups is statistically indistinguishable from zero, consistent with the parallel trend assumption. Second, following the passage of anti-troll laws, cash holdings increased significantly more for the high-risk group relative to the low-risk group. This divergence confirms that the treatment effect is concentrated among firms with greater exposure to patent troll litigation risk and supports our causal interpretation of the results.

4.2.5 The Cross-sectional Difference in Managing Cash Holdings

We now turn to the second part of our hypothesis (H1b and H2b), which examines heterogeneity in firms' responses to the risk of patent troll litigation. We posit that the incentive to strategically manage cash holdings should be positively correlated with the potential damages arising from patent troll litigation. Prior research suggests that rent-seeking activities disproportionately burden small, young, innovative, and high-tech firms. Accordingly, these firms are expected to exhibit more pronounced adjustments in their cash holdings following the passage of anti-troll laws.

To test this prediction, we partition the full sample along four dimensions: firm size, age, investment opportunities, and industry classification. Following Eckbo, Makaew, and Thorburn (2018), we classify firms as operating in a high-tech industry if their two-digit SIC code falls within 28, 35, 36, 38, 48, or 73. Firms with market capitalization below the quarterly median are categorized as small firms, and firms with years since IPO below the quarterly median are classified as young firms. Innovative firms are defined as those with Tobin's Q above the quarterly median. We then re-estimate the regression model (3) separately for each subsample.

Table 8 reports the subsample regression results. Panel A uses Log_view_num as the litigation risk proxy, whereas Panel B uses Log_troll_num. Columns (1) and (2) compare young versus old firms. Consistent with H1b, young firms exhibit a significantly larger post-legislation increase in cash holdings, implying greater pre-event distortion in their cash policies. Columns (3) and (4) show that this effect is concentrated among high-tech firms, while Columns (5) and (6) reveal a stronger response for firms with high Tobin's Q, consistent with the notion that more innovative firms are more vulnerable to rent-seeking. Finally, Columns (7) and (8) compare small versus large firms, indicating that small firms experience a more pronounced rebound in cash holdings, again consistent with H1b.

Table 8 also reports p-values from tests of equality for the interaction term coefficients across subsamples. While some differences are not statistically significant at conventional levels, this does not undermine the overall conclusion for two reasons. First, the sign and magnitude of the coefficients across all subsamples are consistent with our predictions, and the results are robust across both litigation risk measures. Second, the economic significance of the differences is considerable. For example, the interaction term coefficient for small firms (0.0178) is more than six times that of large firms, underscoring the disproportionate effect of patent troll risk on smaller firms.

Taken together, these results provide strong evidence that firms with greater exposure to the detrimental effects of patent troll litigation exhibit stronger incentives to strategically manage cash holdings. The positive interaction term coefficients suggest that these firms reduced cash holdings prior to the law changes, anticipating litigation risk, and subsequently experienced a more pronounced rebound in cash holdings once anti-troll laws were enacted.

4.3 How do Firms Change Their Cash Holdings

In the previous subsection, we showed that firms facing higher patent troll litigation risk strategically hold less cash to reduce the probability of being targeted. However, an important question remains: how do firms adjust their cash holdings? In this section, we investigate the mechanism underlying these adjustments.

Firms' cash flows arise from three primary sources—operating, financing, and investment activities—so any change in cash holdings must ultimately result from adjustments to one or more of these activities. To uncover the channels through which firms respond to patent troll litigation risk, we examine how the three cash flow–to-asset ratios respond to changes in litigation risk.

Having established the validity of using the passage of anti-troll laws for causal inference, we apply the same empirical method in this subsection. To understand the drivers of firms' adjustments in cash holdings, we focus on changes in operating, financing, and investment activities as reflected in their respective cash flows. Specifically, we estimate the following stacked DID model:

$$\begin{aligned} CashFlowRatio_{c,i,j,t+1} = &\beta_0 + \beta_1 LitigationRisk_{c,i,j} \times Pass_{c,j,t} + \beta_2 Pass_{c,j,t} + \beta_3 \\ &X_{c,i,j,t} + CohortFirmFE + CohortTimeFE + \epsilon_{c,i,j,t+1} \end{aligned} \tag{5}$$

where $CashFlowRatio_{c,i,j,t+1}$ represents the operating cash flow to assets ratio, investment cash flow to assets ratio, or financing cash flow to assets ratio. The subscripts and control variables are defined as in Model (3), and we use the same set of controls to ensure comparability across different model specifications. The coefficient of interest is β_1. A significantly positive (negative) value would indicate that firms with higher initial litigation risk adjust their cash holdings by increasing (decreasing) the relevant cash flow activity following the passage of anti-troll laws.

Table 9 presents the regression results. Columns (1) and (2) report estimates for operating cash flow to assets. The coefficients on the interaction terms are statistically insignificant, suggesting that firms do not adjust operating activities to manage the risk of patent troll litigation. Columns (3) and (4) show results for investment cash flow to assets, where the interaction terms are again insignificant, indicating no systematic adjustment through investment activities.

By contrast, Columns (5) and (6) reveal a statistically significant and positive relationship between litigation risk and financing cash flow to assets, implying that firms increase their cash holdings by raising external financing after anti-troll laws are passed. Taken together, these results suggest that firms adjust cash holdings primarily through changes in financing activities rather than through operating or investment activities.

This finding might appear counterintuitive, especially given the substantial ex-post effects of patent troll litigation on R&D expenses (Cohen, Gurun, and Kominers, 2019). Three factors may reconcile this apparent puzzle. First, our focus is on ex-ante litigation risk, which may not directly influence ongoing operations or investment decisions. Second, decisions regarding R&D and capital investment are typically long-term in nature and may be less sensitive to short-term changes in litigation risk. Third, our estimation window, four quarters before and after the law change, may be too short to capture the longer-term strategic effects of anti-troll legislation on firms' operating and investment decisions.

Having established that financing activities are the primary driver of cash holding adjustments, we next explore the source of this financing. Firms can adjust their equity or debt financing in response. One plausible mechanism is that reduced litigation risk improves firms' equity market performance, thereby lowering the cost of raising external capital and encouraging equity issuance (Dayani, 2023). Alternatively, lower litigation risk could expand firms' capacity to take on financial risk, prompting them to issue additional debt to strengthen cash reserves while maintaining an optimal risk profile.

To disentangle these channels, we examine changes in firms' leverage around the passage of anti-troll laws. Table 10 reports regression results using total debt-to-asset, long-term debt-to-asset, and short-term debt-to-asset ratios as dependent variables. Control variables follow Matsa (2010) and Smith (2016). The interaction term is significantly positive for both total debt-to-asset and long-term debt-to-asset ratios, but insignificant for short-term debt-to-asset ratios. This pattern indicates that firms primarily finance increased cash holdings through long-term debt rather than short-term borrowing.

This finding is consistent with the notion that reduced patent troll litigation risk enhances firms' ability to undertake long-term financial commitments and manage overall risk exposure. While we do not attempt to determine the optimal capital structure under litigation risk, our results clearly demonstrate that firms incorporate patent troll litigation risk into their financing decisions and adjust their capital structure accordingly.

4.4 Impact on Tobin's Q

The findings above suggest that firms facing higher patent troll litigation risk prior to the passage of anti-troll laws subsequently increase both cash holdings and debt financing following the law changes. An important follow-up question is whether these adjustments are value-enhancing and whether firms optimally adjust their cash and financing policies.

From a theoretical perspective, cash holdings can either enhance or impair firm value.

Excessive cash retention may signal inefficient capital allocation and foregone investment opportunities, thereby depressing firm value. Conversely, maintaining adequate cash reserves can increase firm value by allowing firms to fund future investments internally and avoid costly external financing. To empirically examine whether the observed adjustments are associated with value creation, we investigate changes in firms' Tobin's Q around the passage of anti-troll laws. Tobin's Q is a widely used proxy for investment opportunities and firm valuation, where higher values indicate greater expected returns on invested capital.

Using the same stacked DID specification as in the previous subsection, we regress Tobin's Q on the interaction of patent troll litigation risk and the law passage indicator. Table 11 reports the results, which show a significantly positive coefficient on the interaction term. This finding suggests that firms with higher ex-ante exposure to patent troll litigation experience a greater improvement in market valuation following the law change relative to other firms.

While these results do not explicitly show an increase in investment opportunities, they do indicate that the market perceives the combined adjustments in cash and financing policies as value-enhancing. In other words, the evidence is consistent with the interpretation that firms respond optimally to reductions in litigation risk by rebalancing their liquidity and capital structure to support value creation.

5 Conclusion

This paper introduces a novel measure of patent troll litigation risk, constructed from patent trolls' information acquisition activities, and uses it to examine the feedback effects of litigation risk on corporate financial policies. Exploiting the staggered adoption of state-level anti-troll laws as a plausibly exogenous shock, we identify a causal relationship between patent troll litigation risk and firms' cash holdings. Our findings reveal that firms strategically reduce cash holdings to lower their probability of being targeted by patent trolls. Moreover, the intensity of this cash management response is positively associated with the potential harm posed by patent troll litigation. Specifically, younger, smaller, more innovative firms, and those in high-technology industries, display stronger adjustments in their cash policies.

Our analysis of cash flow components indicates that these adjustments are primarily implemented through changes in financing activities rather than through changes in operating or investing activities. In particular, the post-law increase in cash ratios is driven largely by additional debt financing, suggesting that firms dynamically balance their total risk exposures when facing a change in patent troll litigation risk.

Taken together, these results demonstrate that abusive patent assertion constitutes a material threat to firms, influencing not only their cash-holding behavior but also their financing decisions. Furthermore, we document that firms' valuations, as proxied by Tobin's Q, respond negatively to patent troll litigation risk, implying that such litigation distorts firms' operations away from their value-maximizing levels. The evidence of improved financial and valuation outcomes following the passage of anti-troll laws suggests that similar policies at the federal level could further mitigate the adverse effects of patent troll activity and promote more efficient capital allocation.

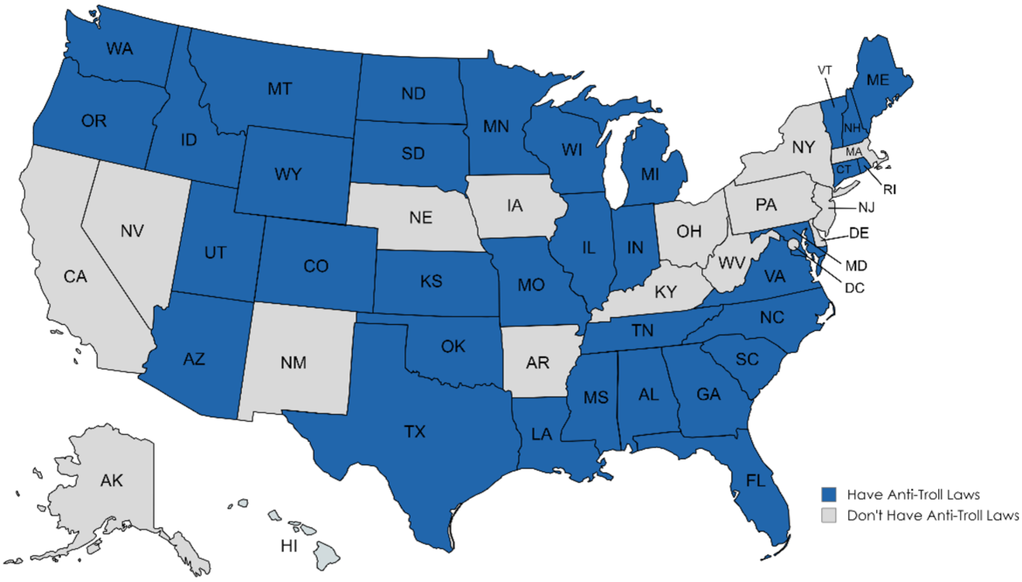

Figure 1: Distribution of Anti-troll Laws Across the States

This figure shows the distribution of anti-troll laws across the States by the end of 2017. Table A2 reports the signing date of each state law.

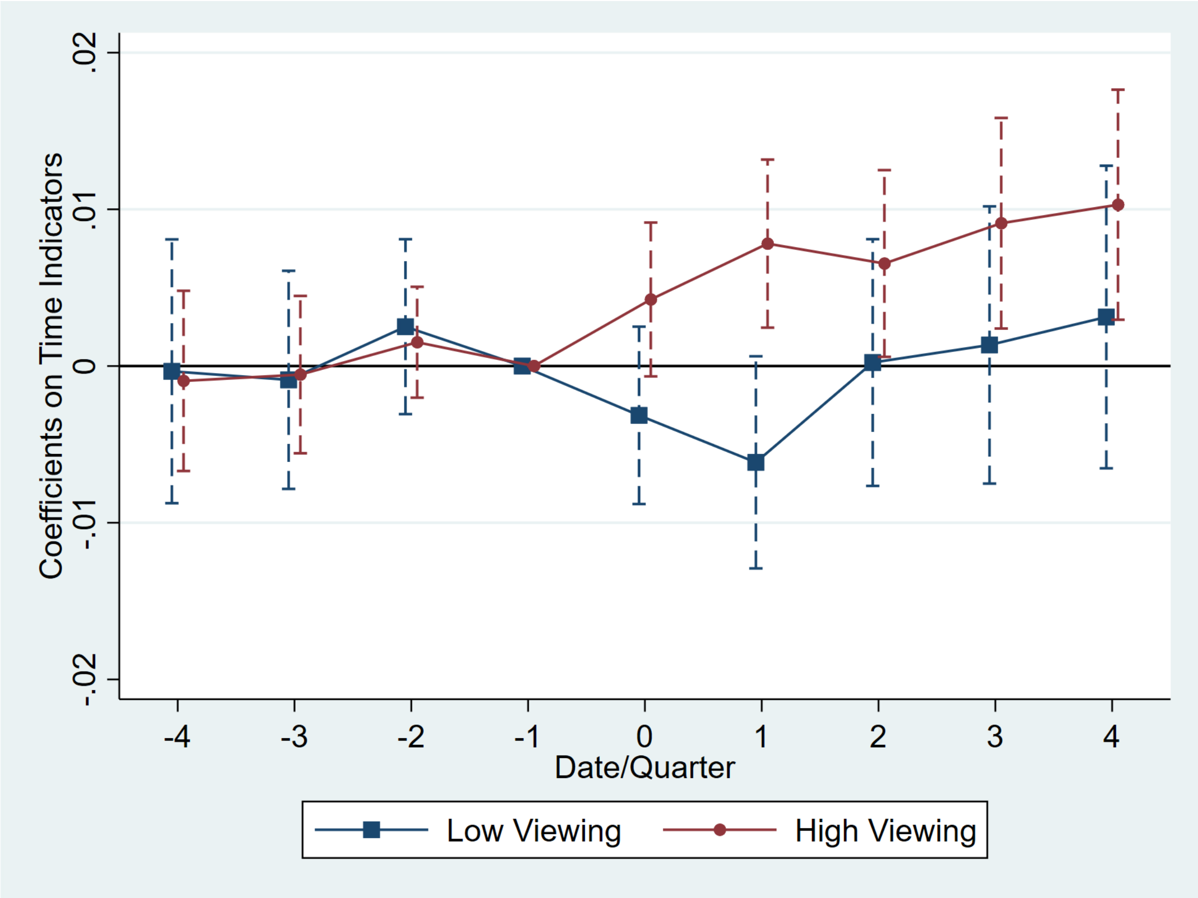

Figure 2: Parallel Trend Analysis by Comparing High and Low Log_view_num Group

The figure plots the coefficients of time indicators of the regression model (4). The high-viewing (high litigation risk) subsample comprises firms whose pre-treatment mean of Log_view_num is above the median.

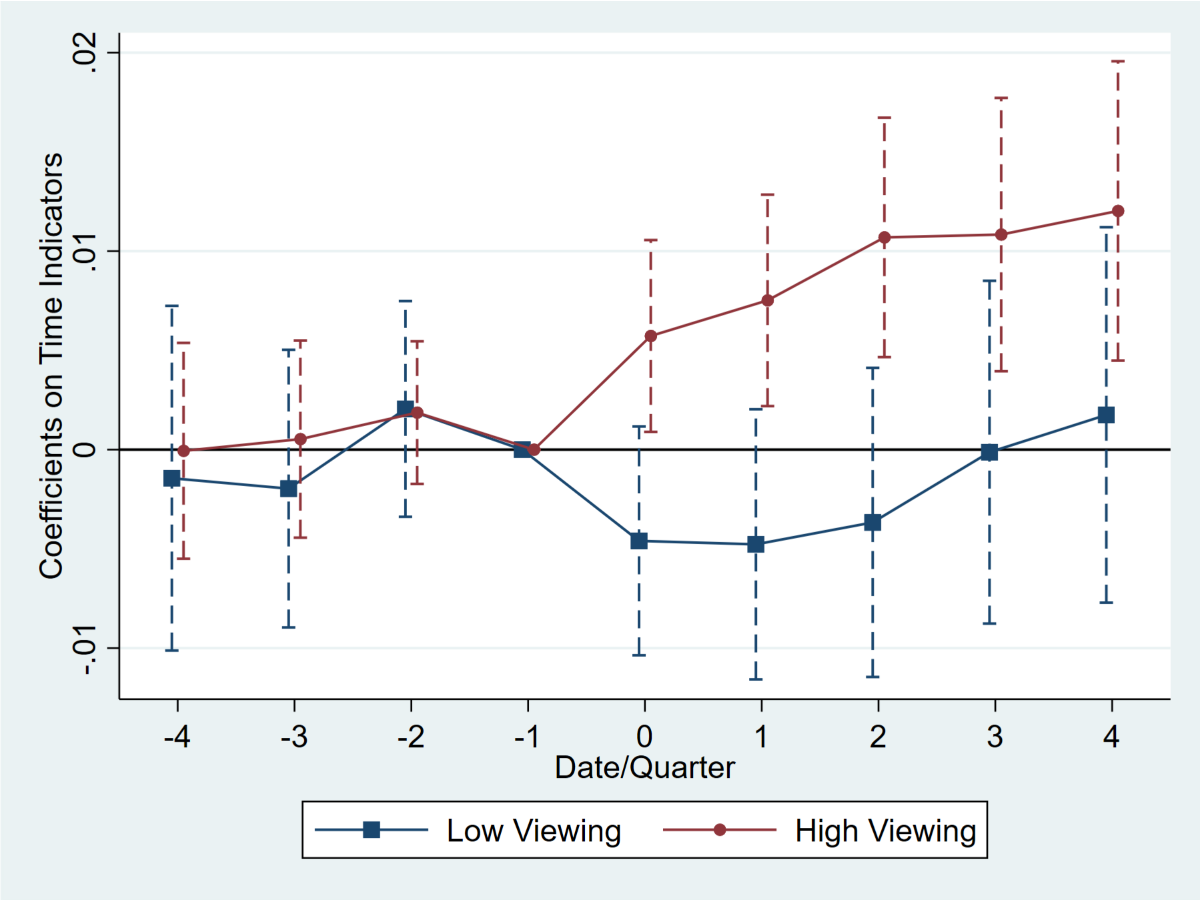

Figure 3: Parallel Trend Analysis by Comparing High and Low Log_troll_num Group

The figure plots the coefficients of time indicators of the regression model (4). The high-viewing (high litigation risk) subsample comprises firms whose pre-treatment mean of Log_troll_num is above the median.

Table 1: Summary Statistics

This table displays summary statistics for the main variables of interest and controls. The sample contains all Compustat firm-quarters with positive sales and assets from 2007Q1 to 2017Q2. An additional requirement is that all companies must be listed on the NYSE, AMEX, or NASDAQ. Financial industry (SIC 6000–6999) and regulated industries (SIC 4400–4999) are excluded from the sample.

VARIABLES	N	Mean	Median	Std	p25	p75
Log_view_num	87646	3.4981	3.5835	1.2807	2.7081	4.3567
Log_troll_num	87646	1.5457	1.6094	0.6904	1.0986	1.9459
SUE	87,646	0.0644	0.0000	0.2455	0.0000	0.0000
Cash_ratio	87646	0.2261	0.1336	0.2426	0.0459	0.3201
Leverage	87646	0.2036	0.1608	0.2087	0.0034	0.3228
CF_o	86252	0.0101	0.0202	0.0608	-0.0040	0.0399
CF_fin	86252	0.0127	-0.0019	0.1056	-0.0158	0.0042
CF_inv	86252	-0.0196	-0.0089	0.0611	-0.0238	-0.0020
Asset	87646	5.1444	0.6269	25.0744	0.1548	2.5700
Cash	87646	0.6473	0.0766	3.9719	0.0193	0.2614
RD_exp	87646	28.6499	0.0000	195.9548	0.0000	6.6780
Acquisition	87646	0.0056	0.0000	0.0236	0.0000	0.0000
CF	87646	0.0012	0.0168	0.0610	-0.0002	0.0281
NWC	87646	0.0512	0.0420	0.1697	-0.0449	0.1527
Capx	87646	0.0117	0.0066	0.0157	0.0029	0.0138
Cap	87550	5.6699	0.7130	23.5909	0.1788	2.7106
Cum_ret	87646	1.1640	1.0769	0.7770	0.8243	1.3541
Seg_Num	78378	2.5013	1.0000	1.9567	1.0000	4.0000
5y_Pat	87646	96.6615	0.0000	772.5731	0.0000	12.0000
BM	87550	0.6171	0.4598	0.6626	0.2472	0.7880
CF_Sigma	87478	0.0622	0.0432	0.0577	0.0260	0.0759
Sale_HHI	87646	0.2168	0.1515	0.1993	0.0714	0.2900
Z_score	85195	3.8605	2.2034	8.4261	1.0574	4.3255
PPE_ratio	87515	0.2390	0.1471	0.2387	0.0617	0.3382
EBITDA_ratio	80239	0.0194	0.0278	0.0518	0.0109	0.0427
RD_ratio	87646	0.0166	0.0000	0.0333	0.0000	0.0196

Table 2: Descriptive Statistics by Treatment and Control Groups

This table reports average values of the main variables of interest and controls in the treatment and control groups. The treatment group contains firm-quarters that are sued by patent trolls. The statistics are constructed by taking the cross-sectional average for each group in each quarter and then comparing the time-series average across groups.

VARIABLES	Not Sued	Be Sued	Diff	P-Value
Cash_ratio	0.2260	0.2290	-0.0030	0.3480
Log_view_num	3.4320	4.4555	-1.0230	0.0000
Log_troll_num	1.5005	2.2040	-0.7040	0.0000
Cash	0.4350	3.7335	-3.2985	0.0000
Asset	4.0775	20.6445	-16.5665	0.0000
Cap	4.2655	26.0550	-21.7890	0.0000
BM	0.6265	0.4835	0.1430	0.0000
Cum_ret	1.1615	1.1960	-0.0345	0.0010
RD_exp	17.8900	184.9805	-167.0905	0.0000
5y_Pat	53.6375	721.7610	-668.1235	0.0000
Sale_HHI	0.2150	0.2405	-0.0250	0.0000
Leverage	0.2040	0.1950	0.0090	0.0015
CF	0.0000	0.0165	-0.0165	0.0000
CF_sigma	0.0630	0.0530	0.0095	0.0000
NWC	0.0530	0.0270	0.0260	0.0000
Capx	0.0120	0.0105	0.0015	0.0000
RD_ratio	0.0165	0.0160	0.0010	0.0990
Acquisition	0.0055	0.0050	0.0005	0.0765
Seg_num	2.4705	2.9690	-0.4985	0.0000
EBITDA_ratio	0.0185	0.0315	-0.0130	0.0000
PPE_ratio	0.2430	0.1825	0.0605	0.0000
Z_score	3.8645	3.8055	0.0590	0.6135

Table 3: Patent Troll Litigation and Patent Troll Information Acquisition

The table presents the results from the linear probability model (first 4 columns) and the logit model (last 2 columns) of patent troll litigation. The sample period is from 2007Q1 to 2017Q2. The dependent variable, sue, is a dummy variable equal to one if the firm is sued by patent trolls. The independent variables include the logarithm of the previous period's number of patent troll viewings, the number of unique patent trolls, cash holding, total assets, market capitalization, book-to-market ratio, and number of patents in the last 5 years. The first 4 columns present the results from linear probability models. The last 2 columns present the results from logit regressions. The standard errors, clustered at the firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Log_view_num	0.0218***		0.0023**		0.5155***	
	(0.0020)		(0.0011)		(0.0206)	
Log_troll_num		0.0635***		0.0067***		1.2011***
		(0.0052)		(0.0021)		(0.0377)
Log_cash	0.0103***	0.0095***	0.0011	0.0011	0.2846***	0.2532***
	(0.0016)	(0.0015)	(0.0012)	(0.0012)	(0.0163)	(0.0164)
Log_asset	-0.0020	-0.0053*	0.0107***	0.0103***	-0.1256***	-0.1623***
	(0.0029)	(0.0029)	(0.0032)	(0.0032)	(0.0238)	(0.0237)
Log_cap	0.0022	0.0008	-0.0020	-0.0021	0.0503**	0.0135
	(0.0030)	(0.0030)	(0.0024)	(0.0024)	(0.0247)	(0.0249)
Log_bm	-0.0036	-0.0027	-0.0059	-0.0060	-0.1620***	-0.1391**
	(0.0051)	(0.0050)	(0.0039)	(0.0039)	(0.0567)	(0.0566)
Log_cum_ret	-0.0111**	-0.0084**	0.0031	0.0032	-0.0691	-0.0146
	(0.0043)	(0.0042)	(0.0039)	(0.0039)	(0.0791)	(0.0781)
Log_rd_exp	0.0158***	0.0144***	0.0026*	0.0026*	0.0515***	0.0504***
	(0.0029)	(0.0027)	(0.0015)	(0.0015)	(0.0098)	(0.0099)
Log_5y_pat	0.0144***	0.0128***	-0.0015	-0.0016	0.0856***	0.0603***
	(0.0021)	(0.0020)	(0.0024)	(0.0024)	(0.0084)	(0.0086)
Constant	-0.0857***	-0.0723***	-0.0050	-0.0043		
	(0.0097)	(0.0087)	(0.0164)	(0.0164)		
Observations	87,062	87,062	86,980	86,980	87,062	87,062
R-squared	0.1305	0.137	0.3239	0.3239		
Industry FE	Y	Y	N	N	N	N
Firm FE	N	N	Y	Y	N	N
Quarter FE	Y	Y	Y	Y	Y	Y

Table 4: Patent Troll Litigation and Patent Troll Information Acquisition After Entropy Balancing

The table presents the results from the linear probability model of patent troll litigation after implementing entropy balancing. The covariates in the first two columns are balanced at the quarter level. The covariates in the last two columns are balanced at the firm-quarter level. The sample period is from 2007Q1 to 2017Q2. The dependent variable, sue, is a dummy variable equal to one if the firm is sued by patent trolls. The independent variables include the logarithm of the previous period's number of patent troll viewings, number of unique patent trolls, cash holdings, total assets, market capitalization, book-to-market ratio, and the number of patents over the last 5 years. The first two columns present the results from balancing at the quarter level. The last two columns present the result from balancing at the industry-quarter level. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

VARIABLES	(1)	(2)	(3)	(4)
Log_view_num	0.0691***		0.0661***	
	(0.0075)		(0.0144)	
Log_troll_num		0.1618***		0.1796***
		(0.0141)		(0.0269)
Log_cash	-0.0048	-0.0108	-0.0008	-0.0080
	(0.0080)	(0.0078)	(0.0125)	(0.0122)
Log_asset	-0.0227*	-0.0253*	-0.0144	-0.0206
	(0.0131)	(0.0130)	(0.0212)	(0.0204)
Log_cap	0.0006	-0.0048	-0.0064	-0.0121
	(0.0131)	(0.0127)	(0.0181)	(0.0176)
Log_bm	0.0209	0.0226	-0.0055	-0.0027
	(0.0253)	(0.0247)	(0.0480)	(0.0468)
Log_cum_ret	0.0131	0.0277	0.0346	0.0507
	(0.0231)	(0.0228)	(0.0343)	(0.0344)
Log_rd_exp	0.0064	0.0035	-0.0030	-0.0094
	(0.0049)	(0.0048)	(0.0079)	(0.0076)
Log_5y_pat	0.0047	0.0009	-0.0025	-0.0054
	(0.0051)	(0.0049)	(0.0080)	(0.0074)
Constant	0.3623***	0.4304***	0.3801***	0.4494***
	(0.0424)	(0.0398)	(0.0630)	(0.0563)
Observations	86,666	86,666	47,176	47,176
R-squared	0.1601	0.1683	0.009	0.022
Industry FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y

Table 5: Stacked DID Regression of Litigation on Patent Troll Litigation Risk

The table presents the results from the stacked DID regression of the next period patent troll litigation dummy on current patent troll litigation risk. The sample period is from 2012Q2 to 2018Q2. The stacked sample is constructed by aggregating a year window centered on the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variable is the next period patent troll litigation indicator. The control variables include cash, total assets, market capitalization, book-to-market ratio, previous year cumulative stock return, R&D expense, and number of patents in the last 5 years. We use a log transformation for all control variables. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

VARIABLES	(1)	(2)
Pass	0.0513**	0.0475**
	(0.0228)	(0.0187)
Log_view_num*Pass	-0.0134**	
	(0.0060)	
Log_troll_num*Pass		-0.0289**
		(0.0115)
Log_cash	0.0041	0.0041
	(0.0037)	(0.0037)
Log_asset	0.0110	0.0110
	(0.0074)	(0.0074)
Log_cap	-0.0014	-0.0014
	(0.0061)	(0.0061)
Log_bm	-0.0157	-0.0157
	(0.0118)	(0.0118)
Log_cum_ret	0.0015	0.0015
	(0.0095)	(0.0095)
Log_rd_exp	0.0045	0.0045
	(0.0041)	(0.0041)
Log_5y_pat	-0.0052	-0.0052
	(0.0073)	(0.0073)
Constant	-0.0071	-0.0070
	(0.0394)	(0.0394)
Observations	358,426	358,426
R-squared	0.3912	0.3912
CohortFirm FE	Y	Y
CohortTime FE	Y	Y

Table 6: Stacked DID Regression of Cash Holding on Patent Troll Litigation Risk

The table presents the results from the stacked DID regression of the next period cash ratio on the current patent troll litigation risk. The sample period is from 2012Q2 to 2018Q2. The stacked sample is constructed by stacking one-year windows centered on the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variable is the cash ratio in the next period. The control variables include book-to-market ratio, cash flow, R&D expense, industry cash flow volatility, negative income indicator, industry Herfindahl-Hirschman Index (HHI), leverage, net working capital, capital expenditure, size, acquisition, dividend payment indicator, missing R&D expense indicator, and the number of firm business segments. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%, 1% respectively.

VARIABLES	Full Sample		Not Sued	
	(1)	(2)	(3)	(4)
Pass	-0.0344**	-0.0184*	-0.0327**	-0.0171
	(0.0144)	(0.0097)	(0.0155)	(0.0107)
Log_view_num*Pass	0.0093***		0.0090***	
	(0.0032)		(0.0035)	
Log_troll_num*Pass		0.0127***		0.0120**
		(0.0046)		(0.0053)
Sale_HHI	-0.0124	-0.0123	-0.0087	-0.0086
	(0.0216)	(0.0216)	(0.0217)	(0.0217)
BM	-0.0037	-0.0037	-0.0026	-0.0026
	(0.0044)	(0.0044)	(0.0045)	(0.0045)
Leverage	-0.0752***	-0.0751***	-0.0730***	-0.0729***
	(0.0207)	(0.0207)	(0.0203)	(0.0203)
CF	0.0084	0.0084	0.0095	0.0095
	(0.0256)	(0.0256)	(0.0258)	(0.0258)
CF_sigma	0.0310**	0.0311**	0.0312**	0.0312**
	(0.0140)	(0.0140)	(0.0144)	(0.0144)
NWC	-0.0439**	-0.0439**	-0.0426**	-0.0426**
	(0.0188)	(0.0188)	(0.0192)	(0.0192)
Capx	-0.6509***	-0.6513***	-0.6677***	-0.6681***
	(0.1021)	(0.1021)	(0.1044)	(0.1044)
Log_cap	0.0081*	0.0081*	0.0090**	0.0090**
	(0.0045)	(0.0045)	(0.0045)	(0.0045)
RD_ratio	-0.1010	-0.1011	-0.0934	-0.0935
	(0.0723)	(0.0723)	(0.0734)	(0.0734)
Acquisition	-0.3116***	-0.3116***	-0.3191***	-0.3191***
	(0.0226)	(0.0226)	(0.0255)	(0.0255)
Div	0.0010	0.0010	0.0011	0.0011
	(0.0035)	(0.0035)	(0.0036)	(0.0036)
Seg_num	-0.0020	-0.0020	-0.0021	-0.0021
	(0.0019)	(0.0019)	(0.0021)	(0.0021)
Neg_inc	-0.0097***	-0.0097***	-0.0095***	-0.0095***
	(0.0022)	(0.0022)	(0.0023)	(0.0023)
Mis_rd	-0.0007	-0.0007	-0.0004	-0.0004
	(0.0074)	(0.0074)	(0.0078)	(0.0078)
Constant	0.2839***	0.2838***	0.2796***	0.2795***
	(0.0325)	(0.0325)	(0.0326)	(0.0326)
Observations	323,150	323,150	302,036	302,036
R-squared	0.952	0.952	0.9529	0.9529
CohortFirm FE	Y	Y	Y	Y
CohortTime FE	Y	Y	Y	Y

Table 7: Placebo Test of the Effect of Patent Troll Litigation Risk

The table presents the results from the stacked DID regression of the next period cash ratio on the current patent troll litigation risk. We construct the sample by falsely assuming that each anti-troll law is passed three years before the actual passage time. The sample period is from 2009Q2 to 2015Q2. The stacked sample is constructed by stacking one-year windows centered on the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variable is the cash ratio in the next period. The control variables include book-to-market ratio, cash flow, R&D expense, industry cash flow volatility, negative income indicator, industry Herfindahl-Hirschman Index (HHI), leverage, net working capital, capital expenditure, size, acquisition, dividend payment indicator, missing R&D expense indicator, and the number of firm business segments. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

VARIABLES	(1)	(2)
Pass	0.0074	0.0056
	(0.0068)	(0.0058)
Log_view_num*Pass	-0.0013	
	(0.0018)	
Log_troll_num*Pass		-0.0016
		(0.0032)
Sale_HHI	0.0098	0.0098
	(0.0193)	(0.0193)
BM	-0.0000	-0.0000
	(0.0038)	(0.0038)
Leverage	-0.0559***	-0.0559***
	(0.0211)	(0.0211)
CF	0.0406	0.0406
	(0.0259)	(0.0259)
CF_sigma	0.0179	0.0179
	(0.0235)	(0.0235)
NWC	-0.0755***	-0.0755***
	(0.0160)	(0.0160)
Capx	-0.5945***	-0.5945***
	(0.0910)	(0.0910)
Log_cap	0.0163***	0.0163***
	(0.0043)	(0.0043)
RD_ratio	-0.2511***	-0.2511***
	(0.0903)	(0.0903)
Acquisition	-0.3588***	-0.3588***
	(0.0233)	(0.0233)
Div	-0.0035	-0.0035
	(0.0035)	(0.0035)
Seg_num	-0.0047***	-0.0047***
	(0.0016)	(0.0016)
Neg_inc	-0.0054***	-0.0054***
	(0.0017)	(0.0017)
Mis_rd	-0.0067	-0.0067
	(0.0073)	(0.0073)
Constant	0.2147***	0.2147***
	(0.0305)	(0.0305)
Observations	307,339	307,339
R-squared	0.9488	0.9488
CohortFirm FE	Y	Y
CohortTime FE	Y	Y

Table 8: Subsample Regression of Cash Holding on Patent Troll Litigation Risk

The table presents the subsample regression results of the stacked DID regression of the next period cash ratio on the current patent troll litigation risk. We use Log_view_num as the proxy of patent troll litigation risk in Panel A, and use Log_troll_num as the proxy of patent troll litigation risk in Panel B. The sample period is from 2012Q2 to 2018Q2. The stacked sample is constructed by stacking one-year windows centered on the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variable is the cash ratio in the next period. The control variables include book-to-market ratio, cash flow, R&D expense, industry cash flow volatility, negative income indicator, industry Herfindahl-Hirschman Index (HHI), leverage, net working capital, capital expenditure, size, acquisition, dividend payment indicator, missing R&D expense indicator, and the number of firm business segments. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

Panel A: Using Log_view_num as a proxy for patent troll litigation risk

	Age		Industry		Tobin Q		Size	
	Young	Old	High Tech	Other	Low	High	Small	Big
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pass	-0.0514*	-0.0119	-0.0580**	0.0048	-0.0263	-0.0547***	-0.0634**	-0.0063
	(0.0281)	(0.0138)	(0.0225)	(0.0147)	(0.0197)	(0.0188)	(0.0311)	(0.0129)
Log_view_num*Pass	0.0146**	0.0031	0.0149***	-0.0005	0.0070	0.0138***	0.0178**	0.0026
	(0.0066)	(0.0030)	(0.0052)	(0.0031)	(0.0044)	(0.0042)	(0.0082)	(0.0027)
P-Value of Testing Coefficient Difference	0.11		0.01		0.23		0.08	
Observations	159,824	162,666	210,094	113,056	157,644	161,779	163,820	157,677
R-squared	0.9516	0.944	0.9427	0.9053	0.9426	0.9472	0.9483	0.9596
Controls	Y	Y	Y	Y	Y	Y	Y	Y
CohortFirm FE	Y	Y	Y	Y	Y	Y	Y	Y
CohortTime FE	Y	Y	Y	Y	Y	Y	Y	Y

Panel B: Using Log_troll_num as a proxy for patent troll litigation risk

	Age		Industry		Tobin Q		Size	
	Young	Old	High Tech	Other	Low	High	Small	Big
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pass	-0.0315	-0.0090	-0.0291*	0.0005	-0.0123	-0.0328**	-0.0366	-0.0048
	(0.0220)	(0.0100)	(0.0149)	(0.0109)	(0.0136)	(0.0132)	(0.0227)	(0.0093)
Log_troll_num*Pass	0.0222*	0.0056	0.0182**	0.0011	0.0083	0.0193***	0.0259*	0.0050
	(0.0117)	(0.0046)	(0.0074)	(0.0049)	(0.0066)	(0.0061)	(0.0145)	(0.0041)
P-Value of Testing Coefficient Difference	0.19		0.06		0.21		0.16	
Observations	159,824	162,666	210,094	113,056	157,644	161,779	163,820	157,677
R-squared	0.9516	0.944	0.9427	0.9053	0.9426	0.9472	0.9483	0.9596
Controls	Y	Y	Y	Y	Y	Y	Y	Y
CohortFirm FE	Y	Y	Y	Y	Y	Y	Y	Y
CohortTime FE	Y	Y	Y	Y	Y	Y	Y	Y

Table 9: Stacked DID Regression of Cash Flow on Patent Troll Litigation Risk

The table presents the results from the stacked DID regression of the future cash flow ratio on the current patent troll litigation risk. The sample period is from 2012Q2 to 2018Q2. The stacked sample is constructed by stacking one-year windows centered on the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variables are the operating, investing, and financing cash flow ratios in the next period. The control variables include book-to-market ratio, cash flow, R&D expense, industry cash flow volatility, negative income indicator, industry Herfindahl-Hirschman Index (HHI), leverage, net working capital, capital expenditure, size, acquisition, dividend payment indicator, missing R&D expense indicator, and the number of firm business segments. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

	Operating CF		Investing CF		Financing CF	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Pass	0.0027	0.0019	-0.0016	-0.0008	-0.0121	-0.0078
	(0.0041)	(0.0027)	(0.0062)	(0.0043)	(0.0112)	(0.0076)
Log_view_num*Pass	-0.0006		-0.0004		0.0045*	
	(0.0009)		(0.0014)		(0.0025)	
Log_troll_num*Pass		-0.0008		-0.0014		0.0081**
		(0.0013)		(0.0022)		(0.0036)
Sale_HHI	-0.0078	-0.0078	-0.0070	-0.0070	0.0067	0.0068
	(0.0116)	(0.0116)	(0.0156)	(0.0156)	(0.0251)	(0.0251)
BM	-0.0001	-0.0001	0.0097***	0.0097***	-0.0439***	-0.0439***
	(0.0018)	(0.0018)	(0.0025)	(0.0025)	(0.0066)	(0.0066)
Leverage	0.0562***	0.0562***	0.0642***	0.0642***	-0.1911***	-0.1911***
	(0.0078)	(0.0078)	(0.0124)	(0.0124)	(0.0216)	(0.0216)
CF	0.1739***	0.1739***	0.0379	0.0379	-0.3520***	-0.3520***
	(0.0211)	(0.0211)	(0.0264)	(0.0264)	(0.0623)	(0.0623)
CF_sigma	0.0092	0.0092	-0.0063	-0.0063	0.0260	0.0261
	(0.0067)	(0.0067)	(0.0114)	(0.0114)	(0.0214)	(0.0214)
NWC	0.1957***	0.1957***	-0.0049	-0.0049	-0.1421***	-0.1421***
	(0.0132)	(0.0132)	(0.0127)	(0.0127)	(0.0326)	(0.0326)
Capx	-0.0545	-0.0545	-0.1774**	-0.1774**	0.4318***	0.4315***
	(0.0436)	(0.0436)	(0.0722)	(0.0722)	(0.1394)	(0.1394)
Log_cap	0.0003	0.0003	-0.0103***	-0.0103***	-0.0034	-0.0034
	(0.0014)	(0.0014)	(0.0027)	(0.0027)	(0.0048)	(0.0048)
RD_ratio	-0.3064***	-0.3064***	-0.1636**	-0.1636**	2.1612***	2.1612***
	(0.0422)	(0.0422)	(0.0676)	(0.0676)	(0.1719)	(0.1719)
Acquisition	-0.0145	-0.0145	0.1564***	0.1563***	-0.0999***	-0.0999***
	(0.0102)	(0.0102)	(0.0177)	(0.0177)	(0.0302)	(0.0302)
Div	-0.0018	-0.0018	-0.0056**	-0.0056**	-0.0061	-0.0061
	(0.0023)	(0.0023)	(0.0027)	(0.0027)	(0.0046)	(0.0046)
Seg_num	0.0005	0.0005	-0.0010	-0.0010	0.0003	0.0003
	(0.0005)	(0.0005)	(0.0010)	(0.0010)	(0.0016)	(0.0016)
Neg_inc	-0.0037***	-0.0037***	0.0052***	0.0052***	-0.0114***	-0.0113***
	(0.0014)	(0.0014)	(0.0016)	(0.0016)	(0.0025)	(0.0025)
Mis_rd	-0.0021	-0.0021	-0.0057	-0.0057	0.0051	0.0051
	(0.0047)	(0.0047)	(0.0042)	(0.0042)	(0.0058)	(0.0058)
Constant	-0.0078	-0.0078	0.0433**	0.0433**	0.0487	0.0487
	(0.0110)	(0.0110)	(0.0201)	(0.0201)	(0.0371)	(0.0371)
Observations	322,515	322,515	322,515	322,515	322,515	322,515
R-squared	0.742	0.742	0.156	0.156	0.3242	0.3242
CohortFirm FE	Y	Y	Y	Y	Y	Y
CohortTime FE	Y	Y	Y	Y	Y	Y

Table 10: Stacked DID Regression of Leverage on Patent Troll Litigation Risk

The table presents the results from the stacked DID regression of the future leverage ratio on the current patent troll litigation risk. The sample period is from 2012Q2 to 2018Q2. The stacked sample is constructed by stacking one-year windows centered on the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variables are the leverage ratio in the next period. The control variables include book-to-market ratio, Altman's Z-score, EBITDA to total asset ratio, PPE to total asset ratio, and the logarithm of sales. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%,1% respectively.

	Total Leverage		Long-term Leverage		Short-term Leverage	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Pass	-0.0423***	-0.0256***	-0.0401***	-0.0255***	-0.0016	-0.0008
	(0.0096)	(0.0068)	(0.0099)	(0.0069)	(0.0046)	(0.0033)
Log_view_num*Pass	0.0104***		0.0096***		0.0004	
	(0.0022)		(0.0023)		(0.0011)	
Log_troll_num*Pass		0.0149***		0.0141***		0.0004
		(0.0034)		(0.0034)		(0.0016)
EBITDA_ratio	-0.1866***	-0.1867***	-0.1299***	-0.1300***	-0.0569***	-0.0569***
	(0.0338)	(0.0338)	(0.0362)	(0.0362)	(0.0183)	(0.0183)
BM	-0.0254***	-0.0253***	-0.0212***	-0.0212***	-0.0010	-0.0010
	(0.0065)	(0.0065)	(0.0062)	(0.0062)	(0.0021)	(0.0021)
PPE_ratio	0.0769	0.0768	0.0264	0.0264	0.0475***	0.0475***
	(0.0574)	(0.0574)	(0.0573)	(0.0573)	(0.0172)	(0.0172)
Z_score	-0.0041***	-0.0041***	-0.0034***	-0.0034***	-0.0006***	-0.0006***
	(0.0007)	(0.0007)	(0.0006)	(0.0006)	(0.0002)	(0.0002)
Log_sale	0.0060	0.0060	0.0087*	0.0087*	-0.0020	-0.0020
	(0.0045)	(0.0045)	(0.0046)	(0.0046)	(0.0020)	(0.0020)
Constant	0.1925***	0.1925***	0.1568***	0.1568***	0.0299***	0.0300***
	(0.0286)	(0.0286)	(0.0293)	(0.0293)	(0.0112)	(0.0112)
Observations	291,597	291,597	291,597	291,597	291,597	291,597
R-squared	0.9335	0.9335	0.923	0.923	0.7077	0.7077
CohortFirm FE	Y	Y	Y	Y	Y	Y
CohortTime FE	Y	Y	Y	Y	Y	Y

Table 11: Stacked DID Regression of Tobin's Q on Patent Troll Litigation Risk

The table presents the results from the stacked DID regression of the future Tobin's Q on the current patent troll litigation risk. The sample period is from 2012Q2 to 2018Q2. The stacked sample is constructed by stacking a one-year window around the event day. All members in the control group come from states that never passed an anti-troll law. The dependent variables are Tobin's Qs in the next period. The control variables include market capitalization, leverage, cash to asset ratio, EBITDA to total asset ratio, and PPE to total asset ratio. The standard errors, clustered at a firm level, are reported in brackets. *,**,*** imply the significant levels are at 10%, 5%, 1% respectively.

VARIABLES	(1)	(2)
Pass	-0.1645*	-0.0966
	(0.0989)	(0.0714)
Log_view_num*Pass	0.0522**	
	(0.0215)	
Log_troll_num*Pass		0.0829**
		(0.0330)
Log_cap	0.6308***	0.6308***
	(0.0427)	(0.0427)
EBITDA_ratio	0.0327	0.0324
	(0.4223)	(0.4223)
Leverage	0.1937	0.1938
	(0.2152)	(0.2152)
Cash_ratio	0.6313***	0.6315***
	(0.2240)	(0.2240)
PPE_ratio	0.8974**	0.8973**
	(0.3696)	(0.3696)
Constant	-2.6170***	-2.6169***
	(0.3417)	(0.3417)
Observations	312,575	312,575
R-squared	0.8741	0.8741
CohortFirm FE	Y	Y
CohortTime FE	Y	Y

Table A1: Variable Definition

Variable Name	Definition
SUE	Dummy variable that indicates whether a firm is sued by a patent troll.
Asset	Total assets.
Cash	Cash and cash equivalent.
RD_exp	Research and development expense.
Acquisition	Cash outflow related to acquisitions divided by book assets.
CF	(Operation Income - Taxes - Interest - Dividends)/Total assets.
Leverage	Long-term debt plus short-term debt divided by book assets.
NWC	Net working capital net of cash scaled by book assets.
Capx	Capital expenditures divided by book assets.
Cap	Market value of equities.
Cum_ret	Previous one-year cumulative return.
Seg_num	Count of business segments.
5y_Pat	Number of newly issued patents in the most recent 5 years.
BM	Book-to-market ratio.
PPE_ratio	Net property, plant, and equipment (PPE) divided by book assets.
RD_ratio	Research and development expense divided by total assets.
Neg_inc	A dummy variable equals one when income is negative.
Mis_rd	A dummy variable equals one when R&D expense is missing.
Div	A dummy variable equals one when firms pay a dividend.
CF_o	Net operating cash flow divided by total assets.
CF_inv	Net investing cash flow divided by total assets.
CF_fin	Net financing cash flow divided by total assets.
CF_Sigma	Industry standard deviation of cash flow, where industry is defined by two-digit SIC.
Sale_HHI	The Herfindahl-Hirschman Index at the three-digit SIC level from Compustat sales data.
Log_view_num	Logarithm of one plus total number of viewings from patent trolls.
Log_troll_num	Logarithm of one plus the total number of unique patent trolls who view a firm more than one time in a quarter.
Z_score	Altman's Z-score (Altman, 1968): $0.3\times$Net Income/Total Asset - Sales/Total Asset + $1.4\times$Retained Earnings/Total Asset + $1.2\times$Working Capital/Total Asset + $0.6\times$Market Value/Book Value.
EBITDA_ratio	(Sales - Cost of Goods Sold - Selling and Administrative Expense)/Total Assets.
Cash_ratio_f	Next period cash and cash equivalent divided by next period total assets.
Log_XXX	The logarithm of one plus XXX.

Table A2: Signing Dates of State Anti-troll Laws

State	Date
AL	4/2/2014
AZ	3/24/2016
CO	6/5/2015
FL	6/2/2015
GA	4/15/2014
ID	3/26/2014
IL	8/26/2014
IN	5/5/2015
KS	5/20/2015
LA	5/28/2014
ME	4/14/2014
MD	5/5/2014
MN	4/29/2016
MS	3/28/2015
MO	7/8/2014
MT	4/2/2015
NH	7/11/2014
NC	8/6/2014
ND	3/26/2015
OK	5/16/2014
OR	3/3/2014
RI	6/4/2016
SC	6/9/2016
SD	3/26/2014
TN	5/1/2014
TX	6/17/2015
UT	4/1/2014
VT	5/22/2013
VA	5/23/2014
WA	4/25/2015
WI	4/24/2014
WY	3/11/2016
CT	5/8/2017
MI	1/6/2017

Chapter 3

Fund Manager Compensation Structure and Mutual Fund Performance: Evidence from a Quasi-experiment in China

Abstract – We study the impact of bonus deferral requirements on mutual fund investment behavior by exploiting China's compensation reform as a natural experiment. Comparing funds with historically high versus low levels of excessive risk-taking, we find that the reform successfully curbs excessive risk-taking but also generates an unintended reduction in managerial effort. These competing effects create an ambiguous net impact on investor welfare: risk-adjusted fund returns decline, yet information ratios improve. By exploiting cross-sectional variation in funds' pre-reform risk–return profiles, we further document substantial heterogeneity in reform outcomes. For high-risk–high-return funds, the costs associated with reduced managerial effort more than offset the benefits of lower risk, leading to weaker post-reform performance. Overall, our findings suggest that while bonus deferral requirements can improve efficiency for funds engaging in inefficient risk-taking, a more targeted policy design may be needed to avoid adverse effects.

Keywords: Chinese Fund, Compensation Reform, Active Fund, Managerial Compensation, Risk-taking, Effort Supply, Fund Performance

JEL classification: G11, G14, G23, O25

1 Introduction

Mutual funds, as professionally managed investment vehicles, have experienced rapid growth worldwide. With their increasing popularity, debates have intensified regarding the efficiency of mutual fund investments. Proponents argue that mutual funds provide retail investors with low-cost access to professional portfolio management (Chordia, 1996; Busse et al., 2021). However, critics contend that mutual funds rarely outperform passive benchmarks (Fama and French, 2010) and that fund managers do not always maximize investor welfare (Starks, 1987; Grinblatt and Titman, 1989; Basak, Pavlova, and Shapiro, 2007; Hugonnier and Kaniel, 2010). It is essential to analyze the incentives faced by fund managers to better understand the economic role of mutual funds.

A central component of these incentives is the compensation structure. Fund managers typically receive a fixed salary supplemented by a variable bonus from their fund companies (Ma, Tang, and Gómez, 2019; Li and Wu, 2019). The performance-based component of compensation is designed to incentivize managers to deliver superior returns (Agarwal, Daniel, and Naik, 2009; Li and Tiwari, 2009; Evans et al., 2023). However, the option-like payoff embedded in bonuses may simultaneously encourage excessive risk-taking, as option values increase with volatility (Grinblatt and Titman, 1989). These dual effects—performance enhancement and risk-shifting—highlight the importance of understanding how compensation structures shape fund managers' behavior and ultimately affect fund performance.

Despite the significance of manager incentives, empirical research in this area has been constrained by the scarcity of individual-level compensation data. Prior studies have relied on information disclosed in the Statement of Additional Information to address this limitation (Ma, Tang, and Gómez, 2019; Evans et al., 2023). While these data provide valuable cross-sectional insights, they cannot establish causal relationships because compensation contracts are endogenously determined. This paper addresses this gap by exploiting a recent compensation reform in the Chinese mutual fund industry to provide causal evidence on how bonus deferral requirements influence managerial risk-taking and fund performance.

The compensation structure of mutual fund managers in China has been widely criticized. Investors argue that these contracts provide asymmetric risk-sharing, disproportionately favoring managers and leaving investors exposed to downside risks (Soho, 2010)[1]. Because managers face no penalties for underperformance, they have incentives to gamble with investors' assets for personal gain (Sina, 2020)[2]. The Asset Management Association of China (AMAC) implemented a compensation reform in June 2022 to address these concerns and promote sustainable industry development. The reform introduced two key features: (i) a deferral requirement stipulating that at least 40% of fund managers' bonuses must be deferred for a minimum of three years, and (ii) a clawback provision allowing fund companies to withdraw bonuses in cases of misconduct. While the reform was intended to curb excessive risk-taking, its actual effects have not been well-studied.

Deferral bonuses are not new in the financial industry. Following the global financial crisis, the Financial Stability Board (FSB) recommended that all banks adopt deferred bonus schemes. Empirical studies show that deferral requirements significantly reduce risk-taking in the banking sector (Cole, Kanz, and Klapper, 2015; Sheedy, Zhang, and

[1] The article reports that China's mutual fund investors criticized fund companies in 2010 for charging full management fees even when funds performed poorly, causing significant investor losses. Experts called for reforms to tie fees more closely to performance to reduce excessive risk-taking.

[2] The article argues that the rise of speculative gambling investment has reshaped China's mutual-fund industry, creating short-term performance stars but exposing investors to extreme risks.

Liao, 2023). Similar mechanisms may operate in the mutual fund industry. First, deferral combined with clawback provisions reduces the incentive to engage in opportunistic risk-taking by penalizing misconduct. Second, a deferral requirement on bonuses generates a significant amount of unvested bonuses, increasing the cost of leaving the companies (Gopalan, Huang, and Maharjan, 2021) and encouraging fund managers to undertake conservative investment strategies. Finally, due to the time value of money, the deferral requirement reduces the value of a fixed-size bonus (Hoffmann, Inderst, and Opp, 2022). As the reward for taking additional risk reduces, fund managers will choose to take less risky investments.

However, excessive risk-taking is not universally harmful. Active risk-taking is essential for fund managers to undertake active investment strategies. Using deferral requirements to handle excessive risk-taking in the mutual fund industry may unintentionally hurt fund investors by reducing managers' willingness to undertake active strategies. Additionally, as the deferral requirement reduces the value of a bonus, the incentivization effect of a bonus will decrease. Facing lower rewards for industriousness, fund managers will reduce their effort supply, which in turn may also harm fund investors.

Given that the reform is designed to address excessive risk-taking in the mutual fund industry, we begin our analysis by examining its effect on excessive risk-taking. To help us identify the reform's effect on excessive risk-taking, we use a difference-in-differences (DID) framework. Our identification strategy exploits cross-sectional variation in pre-reform risk-taking: if the reform is effective, funds previously engaging in higher levels of excessive risk-taking should exhibit larger post-reform reductions. We define the treatment group as funds with above-median pre-reform idiosyncratic volatility.

Our results show that, following the reform, high-risk funds reduce CAPM idiosyncratic volatility (Ivol) by 1.6% more than low-risk funds. This reduction is both statistically significant and economically meaningful, representing 11.61% of the sample mean. The result strongly supports the effectiveness of the reform on reducing funds' excessive risk-taking. To ensure robustness, we replicate our analyses using alternative measures of excessive risk-taking, including the three-factor model (Liu, Stambaugh, and Yuan, 2019) Ivol and tracking error. We obtain consistent results.

Although the reform achieves its goal in controlling funds' excessive risk-taking, it may also unintentionally affect managers' willingness to undertake active strategies. Therefore, we next investigate how the reform affects managerial effort supply by analyzing changes in corporate site visits, active share, and portfolio turnover. DID estimates indicate that managers of high-risk funds significantly reduce effort along all three dimensions, suggesting that the reform dampens active management intensity.

Additionally, we conduct portfolio-level analysis to investigate how the reform affects fund portfolio construction. Portfolio-level results corroborate these patterns: following the reform, high-risk funds decrease concentration and shift toward mature, value, and low-information-asymmetry stocks. These adjustments are consistent with both reduced risk-taking and lower effort.

As reducing excessive risk-taking and managerial effort supply have opposite effects on fund performance, it is important to know the net effect of the reform on fund performance. By testing the changes in risk-adjusted returns, we find that high-excessive risk-taking funds face a greater drop in risk-adjusted returns. However, when turning to the active strategies' risk and return tradeoffs measured by information ratios, the results revert: compared with low excessive risk-taking funds, high excessive risk-taking funds improve their risk and return tradeoffs for active strategies. The seemingly controversial conclusion suggests that while the reform reduces excessive risk-taking and fund alpha at the same time, its effect on excessive risk-taking is much stronger than the effect on fund alpha, which ultimately improves risk and return tradeoffs.

Next, we explore the heterogeneous effect of the reform. As suggested by our hypothesis, the effect of the reform may be heterogeneous depending on the motivation for taking excessive risks. If excessive risk-taking stems from fund managers' speculative gambling, the benefit of using a deferral bonus to reduce excessive risk-taking may outweigh the cost of reducing managerial effort supply, thereby ultimately improving fund performance. On the contrary, if excessive risk-taking results from active investment strategies, using deferral bonuses to reduce excessive risk-taking may become costly. To show heterogeneity, we further divide the treatment group into the high-risk-high-return group and the high-risk-low-return group. Studies suggest that speculative gambling is consistently associated with high risk and low returns, whereas rational investment is not (Kumar, 2009; Bali, Cakici, and Whitelaw, 2011). Therefore, funds that experience high excessive risk-taking and low alphas are more likely to be driven by gambling behavior.

By studying the heterogeneous responses of funds with high excessive risk-taking driven by gambling and active strategies separately, we find that both groups reduce risk-taking following the reform, but the performance effects diverge. Compared with the treatment group, the high-risk-low-return group does not face a huge drop in risk-adjusted returns. Drops in excessive risk-taking bring better risk and return tradeoffs for their active investment. In contrast, the high-risk–high-alpha group experiences both a reduction in risk-taking and a significant decline in risk-adjusted returns, resulting in no improvement in their risk–return trade-offs. Taken together, these results suggest that the reform effectively benefits investors in gambling-oriented funds but unintentionally harms those invested in highly active funds managed by skilled managers.

Although our analyses have provided causal evidence on the reform's effect on fund excessive risk-taking, managerial effort supply, and performance, the robustness of our conclusions may be questioned. The first concern regarding our conclusion is that it may be entirely driven by specific measurements. To alleviate this concern, we provide multiple alternative measures for the dependent variables. In addition, we also redo the entire DID analysis with alternative sorting variables. All results show that our conclusion is robust to the alternative measurement. The second concern for this study is that our conclusion can be driven by the talent outflow associated with the reform. To show that our main result is not driven by the talent manager's exit, we ran a DID analysis on a subsample of funds that never experienced a change in their management team during the sample period. Our conclusion remains valid for the subsample, and the estimated effect of the reform is also close to that from the full sample, suggesting that our conclusion is not affected by talent outflow. Finally, we also implement a placebo test by falsely setting the event year to be three years before the actual event date. The placebo tests confirm that our result is not driven by a specific trend in the data, such as mean-reverting. Overall, we find that our result is robust to varying model specifications and other alternative confounding effects.

Our paper first contributes to ongoing debates on the fund manager compensation structure. Theoretical works show a mixed attitude toward the option-like compensation structure. Critics believe the option-like compensation is a bad practice, as the asymmetric payoff structure encourages managers to risk investors' assets for their own interests (Starks, 1987; Grinblatt and Titman, 1989; Carpenter, 2000). Supporters believe that if a benchmark is properly chosen, the option-like compensation can perfectly solve the effort underinvestment problem and bring better investment outcomes (Agarwal, Daniel, and Naik, 2009; Li and Tiwari, 2009). The empirical evidence regarding the role of the option-like compensation is also mixed. Ma, Tang, and Gómez (2019) find that funds' compensation structure is an equilibrium outcome, so that there is no clear relationship between fund performance and managerial compensation. However, Evans et al. (2023) show that peer benchmarked fund managers outperform. In addition, Elton, Gruber, and Blake (2003), Golec and Starks (2004), and Kempf, Ruenzi, and Thiele (2009) all suggest that compensation incentives change fund managers' risk-taking behaviors. Our paper contributes to these studies by providing causal evidence about the effect of compensation structure on fund managers' investment behavior, including excessive risk-taking, effort supply, and performance.

Our paper also adds to the literature on studying the effect of deferred compensation. The benefit of using deferred compensation was first studied by Lazear (1979). The follow-up literature shows that by delaying part of the compensation to a future date, entrepreneurs focus more on long-term goals (Kane, 2002; Hoffmann, Inderst, and Opp, 2020), exert more effort into their work (Huck, Seltzer, and Wallace, 2011), and reduce voluntary turnover (Gopalan, Huang, and Maharjan, 2021). More specifically, FSB points out that the bonus deferral requirement can improve resilience of financial sectors as this variable compensation is more risk-aligned[3]. Empirical evidence also confirms that deferred bonuses improve bank managers' monitoring and reduce banks' risk-taking (Hartmann and Slapničar, 2014; Cole, Kanz, and Klapper, 2015; Sheedy, Zhang, and Liao, 2023). Our paper contributes to this research by first providing a comprehensive study of the effect of deferred bonuses in the mutual fund industry. By comparing the het-

[3]The FSB report can be found at https://www.fsb.org/uploads/r_0904b.pdf

erogeneous responses of high-risk-high-return and high-risk-low-return funds, our studies show that the deferred bonus significantly improves the risk and return tradeoffs for funds that previously had unreasonable risk and return tradeoffs, but hurts performance for funds that previously undertook high-risk-high-return strategies.

Finally, our paper contributes to the literature on Chinese mutual fund studies. While the Chinese mutual fund market has experienced rapid growth over the past 20 years, the market remains young and is full of inefficiencies. Luo, Yao, and Zhu (2022) find that managers' experience strongly affects their investment style and performance. Li and Wu (2019) and Jiang and Wang (2024) both suggest that the performance-based incentives encourage fund managers to participate in earnings manipulations at the year-end. Our paper contributes to Chinese mutual fund studies by showing the power of the bonus incentive in shaping fund investment behaviors. Our paper also has political implications for regulators. As the bonus deferral requirements have different effects on high-risk-low-return funds and high-risk-high-return funds, our studies suggested that a more targeted compensation regulation is needed to handle bad-faith excessive risk-taking.

The remainder of the paper is structured as follows. Section 2 examines the institutional background. Section 3 outlines our hypothesis. Section 4 details the sample selection, variable construction, and summary statistics. Section 5 presents the empirical analyses. Section 6 concludes.

2 Institutional Background

2.1 The Mutual Fund Industry in China

Unlike the U.S. mutual fund industry, which has a history spanning more than a century, the modern Chinese mutual fund industry is relatively young, having been formally established in 2001. The launch of Hua An Chuang Xin, the first open-end mutual fund, marked the beginning of China's mutual fund market. Since then, the industry has experienced rapid expansion. By the end of 2024, there were 12,705 mutual funds operating in China, with total assets under management (AUM) reaching RMB 41.85 trillion (see Figure 1). The vast majority of these funds are open-ended, with fewer than 50 classified as closed-end funds. Similar to the U.S. market, Chinese mutual funds can be broadly categorized as actively managed or passively managed and invest across a wide range of asset classes, including domestic and foreign equities, corporate and government bonds, and derivatives. Despite these similarities, the Chinese mutual fund industry exhibits several unique institutional and structural characteristics.

First, the legal structure of Chinese mutual funds differs fundamentally from that of U.S. funds. While U.S. mutual funds may be organized as investment companies or trusts, Chinese mutual funds can only be structured as trusts. Importantly, U.S. mutual funds are required to establish a board of directors or trustees to oversee the management company, whereas Chinese mutual funds do not have such boards. Instead, investors enter into a sales contract with the fund and are entitled only to the payoffs specified therein (Gong, Jiang, and Tian, 2014). This contractual arrangement makes it practically impossible for investors to replace the fund management company or renegotiate the fee structure. Furthermore, because fund managers are appointed by fund companies, investors have limited ability to monitor managerial decisions. This governance structure complicates the principal–agent relationship and exacerbates issues of adverse selection and moral hazard in the Chinese mutual fund industry.

Second, fund exits are rare in the Chinese market. The termination of an open-ended fund can be approved only at an investor meeting, typically initiated by the fund management company. However, fund companies have little incentive to terminate funds as long as they remain profitable. Although the China Securities Regulatory Commission (CSRC) imposes "special treatment" requirements for funds with net assets below RMB 50 million or fewer than 200 investors over 20 consecutive trading days, most fund companies circumvent termination by boosting net asset value or attracting new investors (Li and Wu, 2019). As a result, survivorship bias in the Chinese mutual fund industry is minimal compared with other markets.

In summary, while the Chinese mutual fund industry has grown rapidly over the past two decades and exhibits limited survivorship bias, it remains relatively young and underdeveloped. The contractual structure leaves investors with limited bargaining power and weak monitoring mechanisms, reducing their ability to discipline managers and protect themselves from potential misconduct.

2.2 Chinese Mutual Fund Compensation Reform

Although the Chinese mutual fund industry has experienced a rapid expansion, only a few fund investors are satisfied with the mutual fund industry. Investors, social media, and news reports frequently criticize the industry for unreasonably high fund manager compensation and high-risk investments (Renminwang, 2013)[4]. In response to growing concerns, the CSRC issued the "Opinions on Accelerating the High-Quality Development of the Mutual Fund Industry" in April 2022, emphasizing the need to protect investors. Following this opinion, the AMAC introduced the "Guideline for Performance Evaluation and Compensation Management of Fund Management Companies" in June 2022, requiring all mutual fund companies to fully adopt the guideline by December 2022.

The guideline seeks to promote the sustainable development of the mutual fund industry and reduce excessive risk-taking. A central feature of the reform is the restructuring of bonus compensation. Specifically, the guideline requires that at least 40% of fund managers' bonuses be deferred for a period of no less than three years, with deferred amounts distributed in equal or slower installments. In addition, the guideline mandates that bonus contracts include clawback provisions to address instances of managerial misconduct.

The reform has sparked significant debate in China. Supporters argue that the new compensation framework better aligns the interests of managers and investors, thereby improving risk–return trade-offs and enhancing long-term performance. Critics, however, contend that deferring bonus payments weakens managerial incentives and may ultimately harm fund performance and reduce the overall competitiveness of the Chinese mutual fund industry (Caijing, 2024)[5].

[4]The article reports that China's mutual fund industry is facing a credibility crisis as major "rat-trading" scandals, unfair fee charges, and aggressive, high-risk investment practices by star fund managers have repeatedly harmed investors.

[5]The article shows that China's mutual fund industry is undergoing widespread pay cuts, leading to shrinking compensation for managers, rising turnover, and more passive investments.

3 Hypothesis Development

Since the global financial crisis, regulators and practitioners have increasingly recognized that poorly designed pay-for-performance systems in the financial sector can create substantial undesirable risks for the market. In response, the Financial Stability Board (FSB) recommended several reforms to compensation practices, one of the most significant being the introduction of bonus deferrals. These deferral requirements were initially implemented in the banking sector to mitigate excessive risk-taking incentives embedded in traditional bonus structures. Empirical evidence from the banking industry confirms that bonus deferrals are effective in curbing excessive risk-taking (Cole, Kanz, and Klapper, 2015; Sheedy, Zhang, and Liao, 2023). However, whether similar effects arise in the mutual fund industry remains an open question.

On the one hand, mutual fund managers face risk-taking incentives similar to those faced by bank managers. Performance-based bonuses in the mutual fund industry exhibit an option-like payoff structure: managers are rewarded for outperformance but are not penalized for underperformance (Starks, 1987; Grinblatt and Titman, 1989). This asymmetric payoff encourages managers to increase portfolio risk in order to raise the expected value of their bonuses, particularly when the bonus is "at the money" (Carpenter, 2000). Such risk-taking may lead to suboptimal risk levels from the perspective of investors, who may prefer more moderate exposure.

Bonus deferrals can potentially mitigate this incentive in several ways. First, deferrals combined with clawback provisions allow more time for investors and regulators to observe fund performance and detect misconduct (Sheedy, Zhang, and Liao, 2023). The increased likelihood of ex post penalties discourages opportunistic risk-shifting that benefits managers at the expense of investors. Second, deferrals create a stock of unvested compensation that managers forfeit upon departure, thereby increasing the cost of voluntary turnover and discouraging excessive risk-taking motivated by short-term personal gains (Gopalan, Huang, and Maharjan, 2021; Kempf, Ruenzi, and Thiele, 2009). Finally, deferrals reduce the present value of bonuses due to the time value of money, thereby lowering the expected reward from risk-taking (Hoffmann, Inderst, and Opp, 2022).

Conversely, taking additional risk is often essential for generating alpha. Dampening risk-taking incentives may unintentionally harm investors by discouraging managers from pursuing value-creating active strategies. Unlike in the banking sector, bonuses in the mutual fund industry are tied directly to investment performance (Ma, Tang, and Gómez, 2019; Li and Wu, 2019). Because investment performance is the primary concern for fund investors, performance-based bonuses typically align the interests of managers and investors (Agarwal, Daniel, and Naik, 2009). By reducing the present value of bonuses, deferral requirements may weaken this alignment and reduce managerial effort supply, thereby harming fund performance.

In sum, we hypothesize that bonus deferral requirements have two opposing effects: they reduce excessive risk-taking but may also reduce managerial effort. Hence, the net impact on fund performance is ambiguous. We formally state our hypotheses as follows:

H1: *Bonus deferral requirements reduce fund managers' excessive risk-taking.*

H2: *Bonus deferral requirements reduce managerial effort supply.*

H3: *The net effect of bonus deferral requirements on fund performance is ambiguous. Performance improves if the benefit from reducing excessive risk-taking outweighs the cost from reduced managerial effort, but declines when the latter dominates.*

4 Data, Variables and Summary Statistics

4.1 Data and Sample

Our primary dataset is obtained from the China Stock Market and Accounting Research (CSMAR) database, which provides comprehensive information on Chinese mutual funds, including net asset value (NAV), financial statements, fundholder composition, share-class level contract details, and fund company characteristics. To assess the impact of the compensation reform, we focus on the period 2020–2024, which includes the reform year, two pre-reform years, and two post-reform years. The sample comprises all actively managed domestic open-end equity funds and equity-oriented hybrid funds. We focus on these two categories for two reasons. First, performance-based bonuses constitute a substantial component of compensation for managers of active funds. They are widely adopted in this segment of the industry (Li and Wu, 2019; Zhang, Lv, and Wu, 2022), making it a natural setting for studying the effects of bonus deferrals. Second, performance evaluation standards in China are relatively uniform, as performance rankings primarily determine managerial bonuses within fund categories (Zhang, Lv, and Wu, 2022). This homogeneity enhances the comparability of fund performance across managers and mitigates cross-sectional noise in our analysis.

Following Li and Tiwari (2009), we use fund classification data from the Wind database to identify equity and equity-oriented hybrid funds. We exclude exchange-traded funds (ETFs), qualified domestic institutional investor (QDII) funds, umbrella funds, and innovative funds. To ensure the availability of lagged variables, we restrict the sample to funds established prior to 2019. In addition, we require that funds have at least 40 weekly return observations per year and that there be no contractual modifications over the sample period, thereby reducing biases arising from missing data or contract changes. These filters yield a final sample of 782 unique funds, corresponding to 3,910 fund-year observations.

A notable institutional feature of the Chinese mutual fund industry is that a single fund may offer multiple share classes. A distinct management contract governs each share class, charges different fees, and reports its own NAV and dividend policy. However, all classes share the same investment portfolio and consolidated financial statements. Because our analysis focuses on fund-level behavior around the reform, we construct fund-level measures. For funds with multiple share classes, we use the main share class's return series to represent overall fund performance [6].

4.2 Key Variables

4.2.1 Fund Excessive Risk Taking

Our primary measure of excessive risk-taking is idiosyncratic volatility. We focus on idiosyncratic volatility because it captures the component of fund risk that cannot be explained by exposure to well-known pricing factors. Such residual risk is generally considered unrewarded risk (Ang et al., 2006) and is often interpreted as evidence of under-diversification or inefficient portfolio construction (Goetzmann and Kumar, 2008). To compute idiosyncratic volatility, we first estimate the following factor model using weekly returns and standard risk factors for the Chinese market:

$$R_{it} = \alpha + \beta_i MKT_t + \epsilon_{it} \tag{1}$$

[6] As a robustness check, we alternatively compute fund-level returns as NAV-weighted averages across all share classes and obtain qualitatively similar results.

$$R_{it} = \alpha + \beta_{1i}MKT_t + \beta_{2i}SMB_t + \beta_{3i}VMG_t + \epsilon_{it} \tag{2}$$

where $R_i t$ is the weekly gross return of fund i in excess of the risk-free rate; MKT_t is the value-weighted market excess return in week t, SMB_t and VMG_t are the Chinese size and value factors proposed by Liu, Stambaugh, and Yuan (2019). Following Ang et al. (2006), we estimate the regression coefficients at the fund-year level and then compute the idiosyncratic volatility as the standard deviation of the regression residuals.

In addition, we use tracking error as an alternative to excessive risk-taking measure. Tracking error is defined as the standard deviation of the difference between a fund's return and its stated benchmark return (Grinold and Kahn, 1999; Cremers and Petajisto, 2009). This measure captures the extent to which a fund's return deviates from its benchmark, thereby reflecting the additional active risk assumed by the manager. A higher tracking error indicates greater divergence from the benchmark portfolio and, consequently, a higher level of active risk-taking.

4.2.2 Fund Managerial Effort Supply

To evaluate changes in fund managers' effort, we use three measures: corporate site visits, active share, and the portfolio turnover ratio. Corporate site visits capture managers' effort in information acquisition (Hong et al., 2019; Ginzinger et al., 2024), with higher visit counts reflecting greater engagement in monitoring and research activities. To construct fund-level measures, we collect visit records of publicly listed firms from CSMAR and aggregate the firm–visitor-level data to the fund level by matching fund company names and fund manager names. Because only companies listed on the Shenzhen Stock Exchange are required to disclose investor and analyst visits, our data excludes voluntary disclosures by firms listed on the Shanghai Stock Exchange.

An alternative way to measure active fund effort supply is to measure fund activeness, as the active funds are designed to deliver superior performance through active investment. A high level of activeness is a signal of active management. The first activeness measure is the active share proposed by Cremers and Petajisto (2009). This measurement describes the difference between the fund portfolio holdings and the benchmark portfolio's holdings. Because CSMAR contains only constituents of several well-known indexes, such as CSI 300, CSI 500, and CSI 800, we can construct active share measures only for funds whose benchmark solely contains well-known indexes. This limitation shrinks the sample size significantly, leaving only 464 funds in our sample.

Portfolio turnover ratio provides a complementary measure of fund activeness, with higher turnover indicating more active trading. To construct this measure, we first retrieve semi-annual values of purchased and sold equities from CSMAR. We then compute the minimum of purchased and sold values for each half-year period, scale it by the fund's average total net assets (TNA) during the period, and annualize the turnover ratio by averaging the two half-year observations and multiplying by two. This approach follows the methodology of Luo, Yao, and Zhu (2022).

4.2.3 Fund Performance

We use gross returns as our main return measure for comparing fund performance. Gross returns strip out the influence of fee schedules, thereby improving comparability across funds and investment periods.

To construct weekly gross returns, we integrate four datasets from CSMAR: daily NAV returns, dividend payments, share splits, and fee ratios. First, we compute daily adjusted returns by incorporating the effects of dividend distributions and share splits into the raw NAV returns. Next, we obtain daily gross returns by adding back the fees charged by each fund class to the adjusted net returns. Finally, we aggregate daily gross returns within each week to calculate weekly gross returns for use in our main analysis.

We evaluate fund performance using two measures: risk-adjusted returns and information ratios (also named appraisal ratios by Treynor and Black (1973)). Risk-adjusted returns capture a fund's performance in excess of the returns predicted by exposure to common pricing factors. To construct this measure, we first estimate factor loadings for each fund using the prior 52 weeks of fund returns based on equations (1) and (2). We then compute weekly alpha as the difference between the fund's observed weekly gross return and the expected return predicted by the factor models. The annual alpha is calculated as the average weekly alpha over the year multiplied by 52.

An alternative way to evaluate active fund performance is to examine funds' information ratios (Grinold and Kahn, 1999). The information ratio measures the trade-off between risk and return in active management by quantifying how many units of alpha are earned per unit of idiosyncratic volatility. The information ratio is computed by dividing each fund's estimated risk-adjusted return by its idiosyncratic volatility.

4.3 Summary Statistics

Table 1 reports summary statistics for the main variables in our sample. Following prior literature (Chen et al., 2004; Ma, Tang, and Gómez, 2019; Luo, Yao, and Zhu, 2022; Yin and Zhang, 2022), our control variables contain fund risk-taking and performance determinants from the following three categories: family-, fund-, and management team-level characteristics. The family- and fund-level control variables include fund family size, fund gross return, total net assets (TNA), fund flow, expense ratio, and portfolio turnover ratio. Management team-level controls include the longest managerial tenure within the team and dummy variables indicating whether the team has multiple managers. They include a female manager, a manager with a PhD degree, or a manager holding well-known financial certifications such as the Chartered Financial Analyst (CFA), Financial Risk Manager (FRM), or Chinese Institute of Certified Public Accountants (CPA). Detailed definitions of all variables are provided in Table A1.

Several notable patterns emerge from Table 1. First, on average, Chinese mutual funds deliver positive abnormal performance relative to passive strategies. The average annual CAPM alpha and three-factor alpha are 3.33% and 6.92%, respectively. These positive risk-adjusted returns are consistent with prior findings that, on average, Chinese mutual fund managers generate value for investors (Chi et al., 2022; Fang, Luo, and Yao, 2024). Second, Chinese mutual funds exhibit substantially higher turnover ratios compared with their U.S. counterparts. In our sample, the average turnover ratio is 2.95, indicating that Chinese mutual funds replace their entire portfolios roughly every four months. For comparison, Ma, Tang, and Gómez (2019) report an average turnover ratio of 0.92 for U.S. mutual funds. Third, Chinese fund managers are relatively inexperienced: the average maximum managerial tenure within a team is 4.20 years, less than half that reported for U.S. managers (Ma, Tang, and Gómez, 2019). Finally, fund size exhibits substantial cross-sectional dispersion. The average TNA is RMB 1.91 billion, nearly three times the median, and the high standard deviation further indicates significant concentration of assets in a small number of very large funds. To address this skewness, we apply logarithmic transformations to both fund size and fund family size, resulting in distributions closer to normality.

5 Empirical Results

5.1 Effect on Excessive Risk Taking

As the reform was designed to curb excessive risk-taking and enhance the resilience of the mutual fund industry, we begin our analysis by examining its impact on funds' risk-taking behavior. To identify the causal effect of the reform, we employ a DID framework. The underlying intuition is that if the reform effectively reduces excessive risk-taking, funds with historically high levels of such behavior should exhibit a greater decline in excessive risk-taking relative to other funds after the reform. Accordingly, we define the treatment group as funds that engaged in high levels of excessive risk-taking prior to the reform.

To classify treatment and control groups, we first calculate the yearly CAPM Ivol for each fund and obtain its annual percentile ranking. We then compute the average percentile ranking for each fund over the pre-reform period. Using this multi-year average, rather than a single-year rank, reduces the likelihood that a fund is misclassified as high-risk due to temporary fluctuations. Funds with average rankings above the median are assigned to the treatment group, whereas those below the median are assigned to the control group.

Then, we estimate the following DID regression model to evaluate the effect of the reform:

$$Y_{it} = \beta \times Post_t \times HighRisk_i + \gamma Y_{it-1} + \delta' X_{it-1} + Fund\ FE + Time\ FE + \epsilon_{it} \quad (3)$$

where Y_it denotes the measure of excessive risk-taking for fund i in year t. $Post_t$ is an indicator variable equal to one for years 2022 and later. $High_i$ is an indicator variable equal to one if fund i's average pre-reform percentile ranking of risk-taking exceeds the cross-sectional median, and zero otherwise. The interaction term $Post_t \times High_i$ is the coefficient of interest and captures the differential change in excessive risk-taking between high- and low-risk funds after the reform.

We include the lagged dependent variable Y_{it-1} to account for the autocorrelation of the dependent variable. X_{it-1} is a vector that contains all other control variables. Fund fixed effects control for unobservable time-invariant fund heterogeneity, while year fixed effects capture common shocks across funds.

According to our hypothesis that funds engaging in high levels of excessive risk-taking prior to the reform would reduce such behavior more than their low-excessive risk-taking counterparts, we expect beta to be negative in the regression model. Table 2 reports the estimation results, with coefficients on the interaction term presented in the first row. When CAPM idiosyncratic volatility is used as the dependent variable, the estimated coefficient is –1.60 and statistically significant at the 1% level. This finding indicates that, relative to low-excessive risk-taking funds, high-excessive risk-taking funds reduce their CAPM idiosyncratic volatility by an additional 1.60 percentage points per year following the reform. Given that the sample mean of CAPM idiosyncratic volatility is 13.78% per year, this decline represents a reduction of more than 11.61% relative to the mean, highlighting its economic significance.

Columns (2) and (3) of Table 2 report results using alternative measures of excessive risk-taking. Consistent with the baseline specification, the estimated coefficients on the interaction term remain negative and statistically significant. The persistence of negative and highly significant coefficients across specifications provides robust evidence that the reform effectively curbed excessive risk-taking. Taken together, these results suggest that the policy successfully achieved its stated objective of reducing excessive risk-taking in the Chinese mutual fund industry.

Thus far, we have established a causal relationship between the reform and changes in excessive risk-taking using the DID framework. However, the validity of the DID estimates depends critically on the parallel trends assumption. If high- and low-excessive risk-taking funds exhibited different pre-reform trends in excessive risk-taking, the DID estimates could be biased, and the significant interaction term might capture pre-existing differences rather than the reform's causal effect.

To assess the validity of this assumption, we estimate the following dynamic effect model:

$$Y_{it} = \sum_{t=-1}^{2} \beta_t \times \mathbf{1}_t \times HighRisk_i + \gamma Y_{it-1} + \delta' X_{it-1} + Fund\ FE + Time\ FE + \epsilon_{it} \quad (4)$$

This specification is similar to Model (3). Still, it replaces the single post-reform interaction term with a series of interaction terms that capture the time-series evolution of differences between the treatment and control groups. Here, $\mathbf{1}_t$ is a year indicator equal to 1 if the observation corresponds to year t, and we normalize the year immediately preceding the reform as the reference year, so its coefficient is set to zero. The coefficients β_t trace out the dynamic effect of the reform. If the parallel trends assumption holds, we should observe no statistically significant differences between treatment and control groups in the pre-reform period. In contrast, after the reform, we expect β_t to diverge, reflecting a stronger reduction in excessive risk-taking for the treatment group.

Figures 2, 3, and 4 plot the dynamic treatment effects for CAPM idiosyncratic volatility, three-factor idiosyncratic volatility, and tracking error, respectively. Across all three measures, we find no evidence of systematic differences between treatment and control groups in the pre-reform period, supporting the validity of the parallel trends assumption.

Following the reform, high-excessive risk-taking funds experience a marked and persistent decline in excessive risk-taking relative to low-excessive risk-taking funds, and this reduction does not revert within the sample window. Collectively, these results indicate that the reform induced a sustained reduction in excessive risk-taking among high-risk funds.

5.2 Effect on Managerial Effort Supply

In the previous section, we documented that the reform significantly curtailed excessive risk-taking among mutual funds. However, active risk-taking is also a critical component of active portfolio management. By curbing risk-taking through deferred bonus requirements, the reform may have had unintended consequences, such as reducing managers' willingness to pursue active strategies. Moreover, because deferred payments lower the present value of compensation, the incentive effects of bonuses may be weakened, potentially leading to a decline in managerial effort supply.

In order to examine whether the reform altered managerial effort, we re-estimate Model (3) using three proxies for effort supply: corporate site visits, active share, and portfolio turnover ratio. Column (1) of Table 3 reports the results for corporate site visits. The coefficient on the interaction term is –0.87 and statistically significant at the 5% level, indicating that funds with historically high levels of excessive risk-taking reduced their information acquisition activities following the reform relative to the control group. The magnitude of this reduction is economically meaningful, representing more than 12% of the sample mean.

Column (2) presents results using active share as the measure of activeness. Active share reflects the extent to which a fund's holdings deviate from its benchmark portfolio.

The coefficient on the interaction term is –1.21 and statistically significant at the 1% level, suggesting that high-risk funds shifted their portfolios closer to their benchmarks following the reform.

Column (3) uses the portfolio turnover ratio to capture trading intensity. Consistent with the previous results, the coefficient is –0.25 and statistically significant at the 5% level, indicating that trading activity among high-risk funds declined after the reform.

Taken together, these findings suggest that the bonus deferral requirement not only curbed excessive risk-taking but also reduced funds' activeness and their investment in information acquisition. This evidence points to a potential negative impact of the reform on managerial effort supply.

To validate the parallel-trend assumptions, we also estimate dynamic specifications using Model (4). Figures 5, 6, and 7 plot the dynamic treatment effects for corporate site visits, active share, and turnover ratio, respectively. Across all three measures, we find no evidence of systematic pre-reform differences between the treatment and control groups, supporting the parallel trends assumption. Following the reform, we observe a sharp and persistent decline in managerial effort among high-risk funds, with no sign of reversal during the sample period. Overall, these results suggest that the reform had long-lasting adverse effects on the supply of managerial effort.

5.3 Changes in Portfolio Holdings

In this section, we examine how funds adjust their portfolio holdings in response to the reform. By analyzing changes in holdings composition, we can more directly observe how fund managers modify their risk-taking and effort supply. If managers choose to reduce fund idiosyncratic volatility, they may either reduce portfolio concentration or shift toward stocks with lower firm-specific risks. Accordingly, we expect funds that previously engaged in high levels of excessive risk-taking to decrease portfolio concentration and reallocate toward stocks with lower idiosyncratic risk following the reform. Likewise, if fund managers reduce their information acquisition activities, they are less likely to invest in stocks characterized by high information asymmetry (Agarwal et al., 2021). Thus, we also expect high-excessive risk-taking funds to decrease their exposure to such stocks after the reform.

To test these hypotheses, we employ the same empirical design as in earlier sections but replace the dependent variables with measures of portfolio holdings. Our first dependent variable is the Herfindahl–Hirschman Index (HHI), computed as the sum of squared portfolio weights at the firm level. HHI is positively associated with portfolio concentration (Bushee, 1998). The second and third dependent variables are the portfolio age score and portfolio value score. Following Wermers (2012) and Busse et al. (2021), we classify all listed firms into quintiles every half-year based on firm age and earnings-to-price (EP) ratio. Using these quintile rankings and fund portfolio disclosures, we compute each fund's portfolio age and value scores semi-annually and then average these values to obtain annual measures. The portfolio age score reflects a fund's tendency to invest in mature firms, while the value score captures its allocation to value firms. Prior literature shows that mature and value firms tend to have lower firm-specific risk than young or growth firms (Ouimet and Zarutskie, 2014; Brown and Kapadia, 2007). Thus, higher age and value scores indicate a portfolio with reduced exposure to firm-specific risks.

To capture changes in allocations to stocks with different levels of information asymmetry, we also construct portfolio-level analyst coverage and synchronicity scores. Using abnormal analyst coverage from Hong, Lim, and Stein (2000) and price synchronicity measures from Chan and Hameed (2006), we compute portfolio-level scores for each fund based on its holdings. Higher analyst coverage and price synchronicity indicate a lower-information-asymmetry environment. Therefore, increases in these scores imply a shift toward stocks with less private information.

Table 4 presents the regression results. The estimated coefficient on the interaction term in Column (1) is significantly negative, indicating that funds with high pre-reform excessive risk-taking significantly reduce portfolio concentration after the reform. Because diversified portfolios typically exhibit lower firm-specific risk, this pattern suggests that such funds reduce excessive risk-taking by broadening their holdings. Consistent with this interpretation, Columns (2) and (3) show that high-excessive-risk-taking funds increase their allocations to mature and value firms following the reform. These results highlight an additional channel through which funds reduce excessive risk-taking: by reallocating toward firms with lower idiosyncratic risk.

Columns (4) and (5) examine whether funds reduce investment in high-information-asymmetry stocks. The results indicate that high-excessive-risk-taking funds shift their holdings toward firms with higher abnormal analyst coverage and greater price synchronicity—both signals of lower information asymmetry. This pattern is consistent with a reduction in managers' information-acquisition activities: managers appear less willing to expend effort generating private information and instead reallocate to stocks with more transparent information environments.

Overall, the analysis of portfolio holdings aligns with and reinforces our earlier findings. Following the reform, funds that previously engaged in excessive risk-taking shift toward more diversified portfolios, invest more heavily in lower-firm-specific-risk stocks, and reduce exposure to information-intensive stocks. These adjustments collectively help explain the documented decline in risk-taking and managerial effort supply after the reform.

5.4 Net Effect on Fund Performance

In the previous discussion, we documented that the reform successfully achieved its primary objective of reducing excessive risk-taking while also leading to a decline in managerial effort supply. Because these two channels exert opposing effects on fund performance, the net effect of the reform is theoretically ambiguous. To evaluate its overall impact, we examine two performance measures: risk-adjusted returns and information ratios.

In line with the previous section, we study the effect of the reform by comparing the responses of the high- and low-excessive-risk-taking groups. Table 5 shows the estimated coefficient of Model (4). Column (1) uses the CAPM alpha as the dependent variable. The coefficient on the interaction term is –3.51 and statistically significant at the 1% level, indicating that funds with higher pre-reform risk-taking experienced a larger post-reform decline in risk-adjusted returns. The magnitude of the effect is economically meaningful: a 3.51% reduction in CAPM alpha turns the average alpha from positive to negative. Column (2) presents results using the three-factor model alpha and shows a consistent pattern, with high-risk funds experiencing a significantly greater decline in performance. Together, these results suggest that the bonus deferral requirement adversely affected funds' risk-adjusted returns.

We next examine information ratios, which capture the trade-off between active returns and idiosyncratic risk (Grinold and Kahn, 1999). We report two versions of the information ratio: one based on the CAPM and the other based on the three-factor model of Liu, Stambaugh, and Yuan (2019). Column (3) presents the CAPM-based results. The estimated coefficient on the interaction term is 0.28 and statistically significant at the 1% level, indicating that high-risk funds improved their risk–return efficiency following the reform. Column (4) reports similar findings using the three-factor information ratio.

At first glance, these results appear contradictory: risk-adjusted returns decline, yet information ratios improve. This apparent discrepancy can be reconciled by recognizing the distinct nature of the two performance metrics. Risk-adjusted returns focus exclusively on abnormal return generation, whereas information ratios incorporate both abnormal returns and the idiosyncratic risk borne to achieve them. A plausible interpretation is that the reform disproportionately reduced idiosyncratic risk relative to the decline in alpha, thereby improving the overall risk–return efficiency of active strategies. In other words, while the reform may have lowered absolute performance, it simultaneously enhanced the quality of performance by discouraging unrewarded risk-taking.

5.5 Heterogeneous Response of High-Risk Funds

Based on our hypothesis, the impact of the reform should be heterogeneous, depending on the underlying motivation for excessive risk-taking. If excessive risk-taking arises from managerial misconduct, such as gambling with investors' assets for personal gain, then the benefits of curbing such behavior through deferred compensation should outweigh the costs associated with reduced managerial effort. In this case, the reform is expected to improve fund performance. Conversely, if excessive risk-taking reflects value-creating active investment strategies, then curbing such behavior may be costly, as it suppresses the risk-taking necessary to generate alpha.

To empirically examine this heterogeneity, we further partition the treatment group into two subgroups: high-risk–low-return funds and high-risk–high-return funds. Prior literature suggests that when managers engage in speculative gambling, their investments typically exhibit high risk but low returns (Kumar, 2009; Bali, Cakici, and Whitelaw, 2011). In contrast, managers pursuing risk-taking that maximizes investor welfare should display a positive risk–return trade-off. Accordingly, we classify funds with both high pre-reform excessive risk-taking and low pre-reform CAPM alphas as high-risk–low-return funds (more likely to be driven by misconduct), and funds with high risk-taking but high alphas as high-risk–high-return funds (more likely to reflect productive active risk-taking).

Operationally, we first compute the annual percentile ranking of funds' CAPM alphas and average them across the pre-reform period. Funds with average alpha rankings above the median are classified as high-return funds, while those below the median are classified as low-return funds. Accordingly, we use the new return criteria to split the original treatment group into high-risk–high-return group and high-risk–low-return group. We estimate the following specification:

$$\begin{aligned} Y_{it} = {} & \alpha \times Post_t \times HighRisk\_HighRet_i + \beta \times Post_t \times HighRisk\_LowRet_i \\ & + \gamma Y_{it-1} + \delta' X_{it-1} + Fund\ FE + Time\ FE + \epsilon_{it} \end{aligned} \tag{5}$$

This model extends the baseline DID framework by including two interaction terms. $HighRisk\_HighRet_i$ equals one if fund i belongs to the high-risk–high-return group, while $HighRisk\_LowRet_i$ equals one if fund i belongs to the high-risk–low-return group. As suggested by Frölich (2004), the coefficients of the interaction terms capture the heterogeneous treatment effects across the two subgroups.

Table 6 reports the results. Four main findings emerge. First, the reform significantly reduces excessive risk-taking in both subgroups. As shown in Columns (1)–(3), the coefficients of both interaction terms are negative and statistically significant across all risk-taking measures, confirming that the deferral bonus policy effectively curbed excessive risk-taking regardless of its underlying motivation.

Second, the reform has differential effects on managerial effort supply. Columns (4)–(6) indicate that the reduction in corporate site visits is concentrated among high-risk–high-return funds, whereas the reduction in portfolio turnover is concentrated among high-risk–low-return funds. This pattern suggests that the reform primarily discouraged information acquisition in funds pursuing active strategies, but primarily curbed trading intensity in funds more likely to engage in speculative gambling behavior.

Third, the two subgroups change their investment styles differently following the reform. Columns (7)–(12) show that high-risk–high-return funds significantly reduce their portfolio concentration and tilt their allocations toward value stocks and stocks with high abnormal analyst coverage and price synchronicity. This change supports that high-risk-high-return funds conduct more passive investments following the reform. In contrast, high-risk–low-return funds tilt toward mature firms and stocks with high abnormal analyst coverage, which likely reflects a move away from the younger, higher-information-asymmetry stocks they previously favored for speculative bets.

Lastly, the performance effects of the reform differ across subgroups. Columns (12) and (13) show that risk-adjusted returns decline significantly only for high-risk–high-return funds, while high-risk–low-return funds exhibit no significant change in alpha. Columns (14) and (15) reveal that information ratios improve significantly for high-risk–low-return funds but remain unchanged for high-risk–high-return funds.

Taken together, these findings highlight an important nuance: the reform successfully eliminates value-destroying risk-taking among high-risk–low-return funds, thereby improving their risk–return efficiency. However, it simultaneously suppresses value-creating activities among high-risk–high-return funds, resulting in lower alphas without any improvement in risk–return trade-offs. These results suggest that a "one-size-fits-all" deferral bonus requirement may inadvertently penalize investors who intentionally allocate to high-risk-high-return funds. Our findings underscore the importance of regulatory design that balances the benefits of curbing bad-faith risk-taking against the costs of discouraging productive risk-taking.

5.6 Robustness Tests

To ensure the robustness of our findings, we conduct several additional tests. Our first concern is whether CAPM Ivol provides an accurate measure of funds' excessive risk-taking prior to the reform. To address this, we reconstruct the treatment and control groups using idiosyncratic volatility derived from a more sophisticated asset pricing model. Our second concern is that the results might be driven by managerial turnover rather than by changes in managers' risk-taking behavior or effort supply. A plausible alternative explanation is that the reform triggered talent outflows: if skilled active managers exited the funds following the reform, the observed decline in excessive risk-taking, managerial effort supply, and fund performance could simply reflect the departure of these managers rather than the effect of the reform itself. Finally, we consider the possibility that our results are confounded by other contemporaneous events. To mitigate this concern, we perform a placebo test by artificially shifting the reform date three years earlier than the actual event date and re-estimating our models.

5.6.1 Alternative Excessive Risk-taking Sorting Variables

In the previous analysis, we primarily used CAPM Ivol to classify funds into treatment and control groups. A potential concern is that this measure may capture funds' exposure to other systematic risk factors, rather than purely excessive risk-taking. To address this issue, we reclassify the treatment and control groups using idiosyncratic volatility derived from the three-factor model and re-estimate the DID regressions.

Table 7 reports robustness tests. Consistent with the main findings, Columns (1)–(3) show that high-excessive-risk-taking funds reduce their risk-taking following the reform. Columns (4)–(6) indicate that, in addition to curbing excessive risk-taking, these funds decrease information acquisition and overall activeness. Column (7) shows a reduction in portfolio concentration relative to the control group. Columns (8)–(11) demonstrate that these funds tilt their portfolios toward mature, value, and low information-asymmetry stocks. Columns (12)–(13) reveal a decline in risk-adjusted returns, while Columns (14)–(15) suggest an improved risk-return trade-off for active investments. Overall, these results align with the main analysis and confirm that our conclusions are robust to alternative measures of excessive risk-taking.

5.6.2 Exclude Manager Turnover

Another potential concern is that skilled fund managers may exit the mutual fund industry in response to stricter compensation regulations. Given that the average bonus-to-salary ratio exceeds 50% in the Chinese mutual fund industry (Zhang, Lv, and Wu, 2022), any changes to the bonus structure could substantially affect total compensation. Managers dissatisfied with the reform may leave for alternative investment sectors, such as hedge funds. If this were the case, the observed reductions in excessive risk-taking, managerial effort, and fund performance could be driven by talent outflows rather than the reform itself.

To address this concern, we construct a restricted sample excluding funds that experienced manager turnover after the reform and re-estimate the DID regressions. The results are reported in Tables 8. As expected, the sample size decreases to 577 funds. Nevertheless, the estimated coefficients on the interaction terms remain similar to those in the full sample. Even without any post-reform manager turnover, high-excessive risk-taking funds still exhibit reductions in excessive risk-taking, managerial effort, and risk-adjusted returns. These findings suggest that our main conclusions are not driven by manager turnover.

5.6.3 Placebo Tests

The final robustness check is a placebo test, which helps verify whether our main results are driven by patterns inherent in the data rather than the reform itself. To implement this test, we falsely set 2019—three years prior to the actual reform year—as the event year. We choose 2019 to ensure that the placebo sample period does not overlap with the post-reform period. Using a different placebo year does not materially affect the results.

Table 9 reports placebo test results. The interaction-term coefficients in Columns (1)–(3) are not statistically significant, indicating that funds' excessive risk-taking did not change meaningfully around the placebo year. Columns (4)–(6) show no consistent patterns in managerial effort, and Columns (7)–(11) reveal no clear effects on portfolio style scores, except for the abnormal analyst coverage. Although Columns (12) and (13) show significantly negative coefficients, partially aligning with the main results, Columns

(14) and (15) differ from the original findings. Overall, the placebo test confirms that the main results are driven by the 2022 bonus deferral reform rather than by spurious data patterns.

6 Conclusion

This paper examines the impact of China's reform on fund manager compensation, focusing on the bonus deferral requirement and its effects on mutual fund investment behavior. Although the reform was designed to curb excessive risk-taking, our findings show that it also produces unintended side effects. Using a DID framework, we provide causal evidence that bonus deferrals effectively reduce excessive risk-taking. However, this reduction comes at a cost: managerial effort supply and risk-adjusted fund performance decline, even though risk-return trade-offs for active investments improve.

Exploiting cross-sectional variation in the motivation behind excessive risk-taking, we find that the reform's effects are heterogeneous. For funds previously pursuing high-risk-low-return strategies, such as speculative gambling, the benefits dominate: excessive risk-taking decreases, leading to improved risk-return trade-offs for investors. In contrast, for funds undertaking profitable active investments, the costs prevail: reduced managerial effort outweighs the benefits of lower risk, ultimately harming funds' risk-adjusted performance. These findings highlight that while bonus deferrals can enhance efficiency in funds with unreasonable risk-taking, more targeted policies may be necessary to avoid negative effects on value-creating funds.

Overall, our results demonstrate that bonus incentives play a critical role in shaping fund risk-taking, managerial effort, and performance. Implementing a deferral requirement unconditionally may not be an optimal approach, as it reduces managerial effort and fund performance. Importantly, the negative effects disproportionately affect investors in high-risk-high-return funds, potentially introducing new social costs into the market.

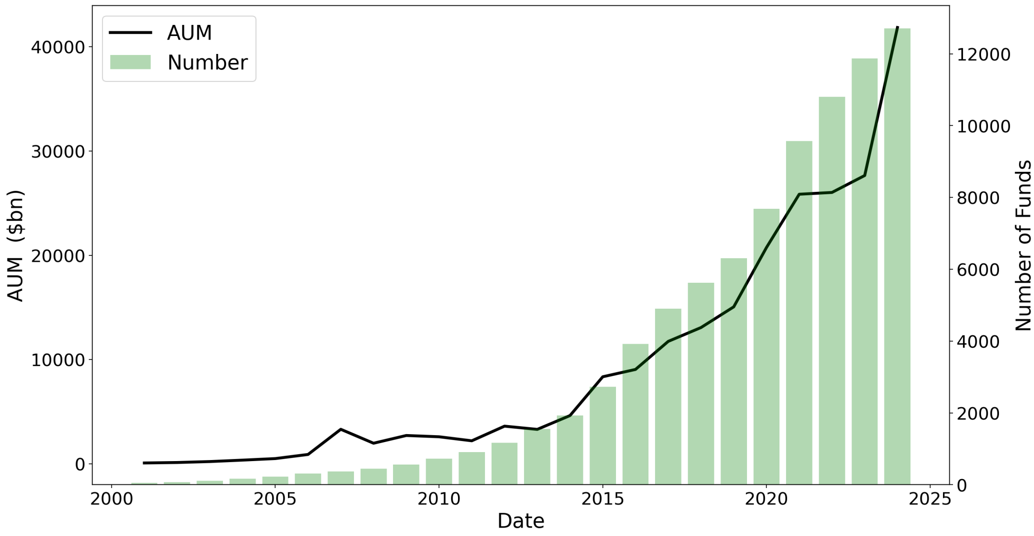

Figure 1: Mutual Fund Market Growing Trend

The figure illustrates the growth of the Chinese mutual fund market from 2001 to 2024. Total assets under management (AUM) are calculated as the sum of AUM across all funds, while the number of funds is defined as the count of unique funds (including funds' subclasses) in the Chinese market. AUM (in billions of dollars) is plotted on the left-hand y-axis, and the number of funds outstanding each year is shown as green bars on the right-hand y-axis.

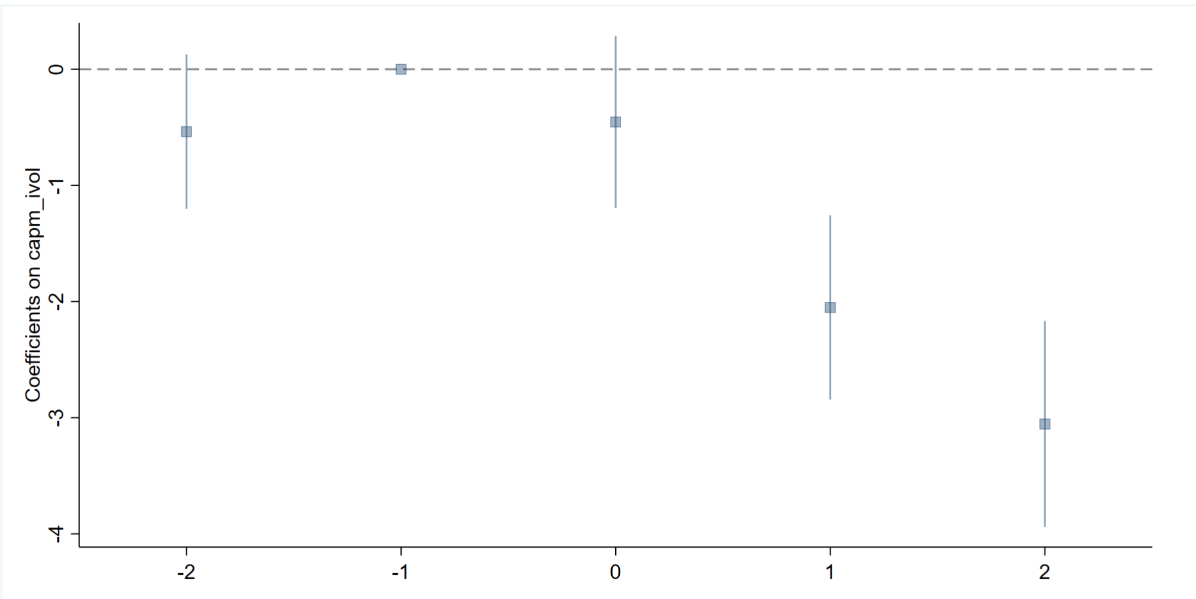

Figure 2: Dynamic Effect of the Reform on the CAPM Ivol

This figure shows the evolution of CAPM Ivol around the introduction of the bonus deferral requirements. These estimated coefficients of the dynamic effects are obtained by estimating the regression model (4).

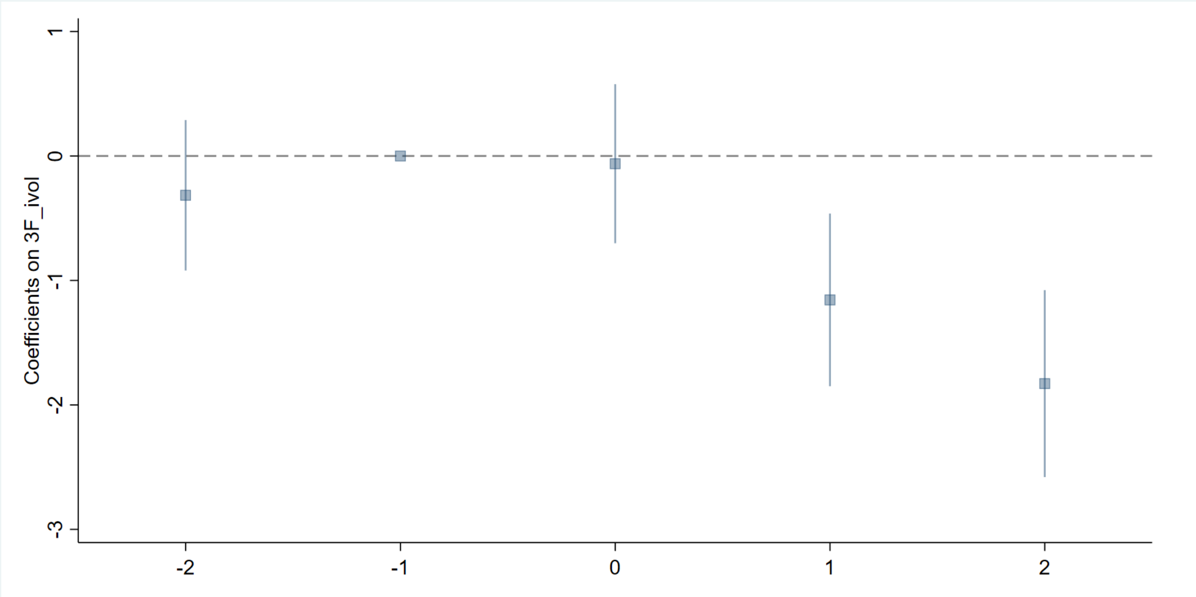

Figure 3: Dynamic Effect of the Reform on the Three-factor Model's Ivol

This figure shows the evolution of the three-factor model's Ivol around the introduction of the bonus deferral requirements. These estimated coefficients of the dynamic effects are obtained by estimating the regression model (4).

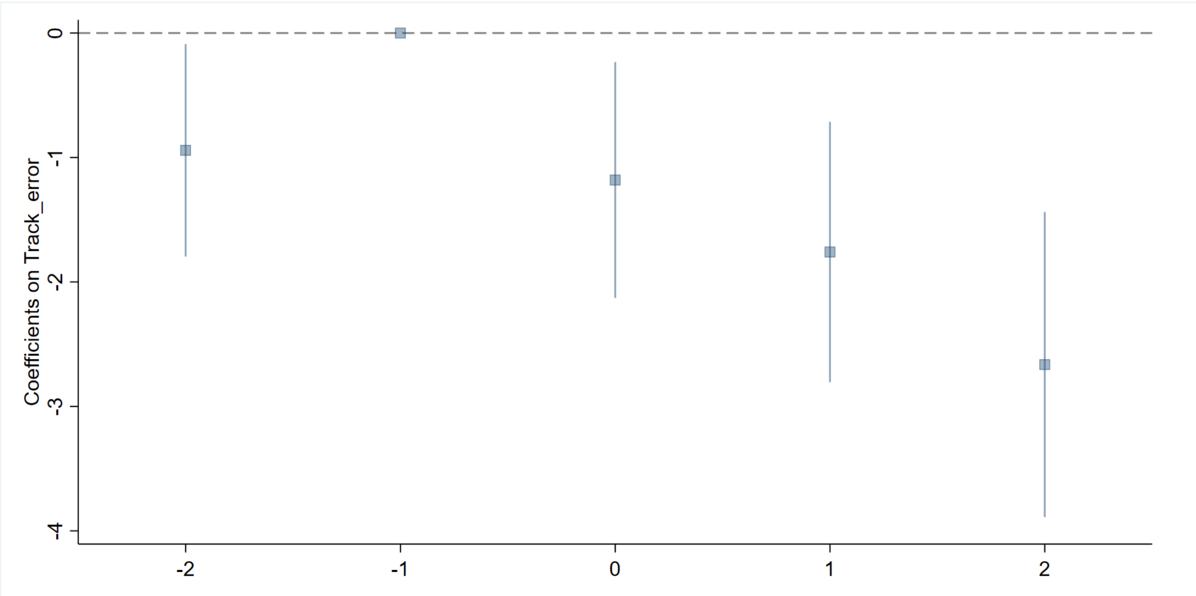

Figure 4: Dynamic Effect of the Reform on the Tracking Error

This figure shows the evolution of the tracking error around the introduction of the bonus deferral requirements. These estimated coefficients of the dynamic effects are obtained by estimating the regression model (4).

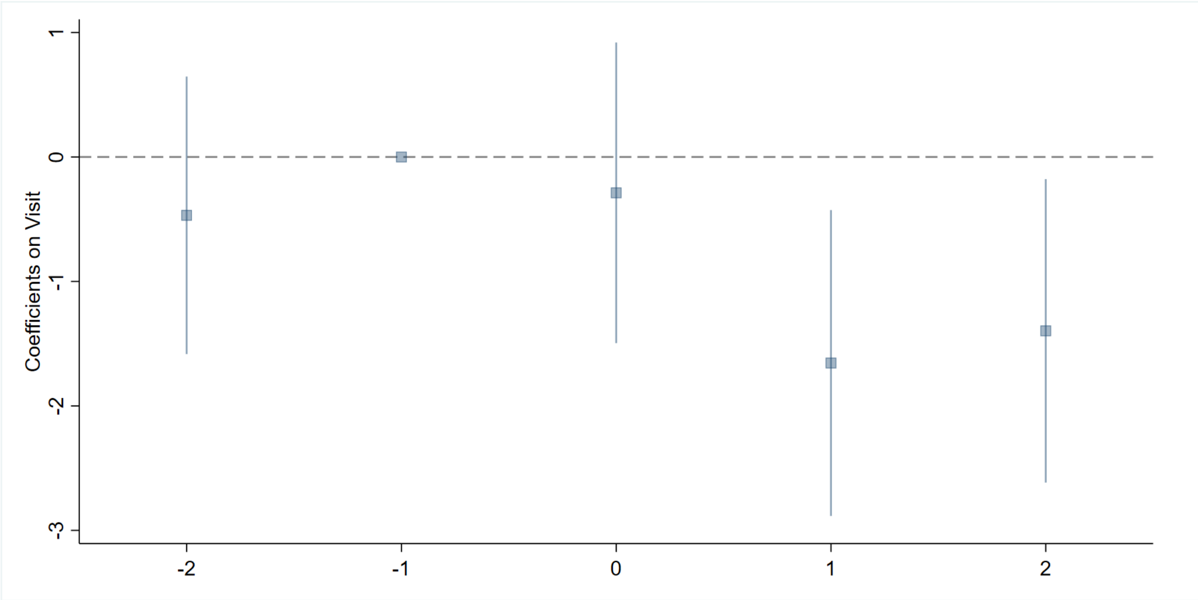

Figure 5: Dynamic Effect of the Reform on the Corporate Site Visits

This figure shows the evolution of the corporate site visits around the introduction of the bonus deferral requirements. These estimated coefficients of the dynamic effects are obtained by estimating the regression model (4).

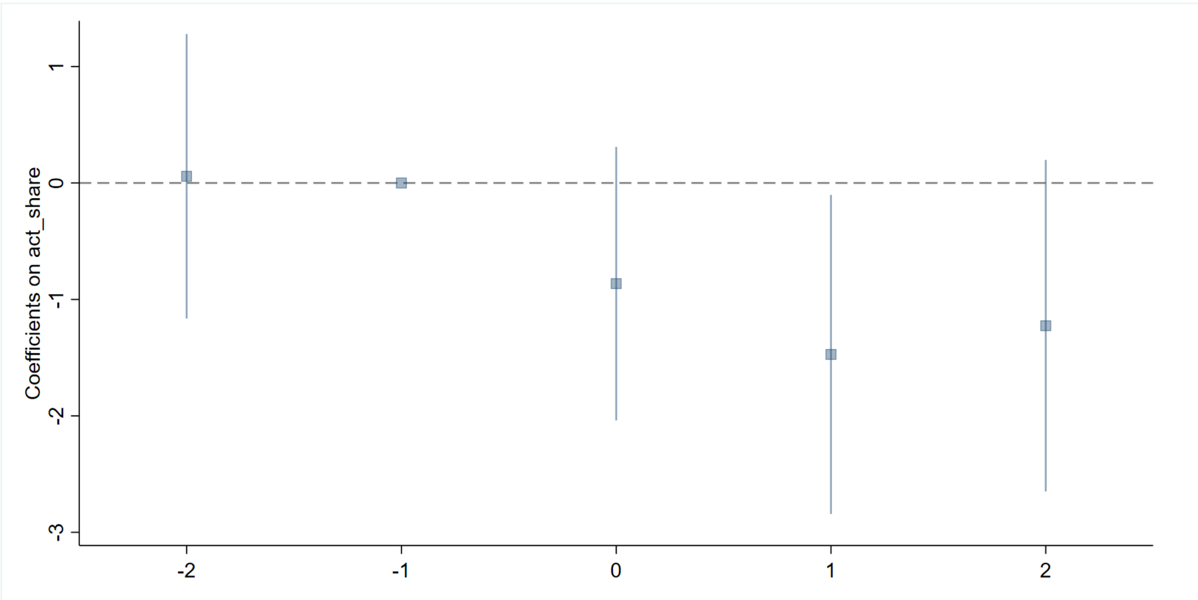

Figure 6: Dynamic Effect of the Reform on the Active Share

This figure shows the evolution of the active share around the introduction of the bonus deferral requirements. These estimated coefficients of the dynamic effects are obtained by estimating the regression model (4).

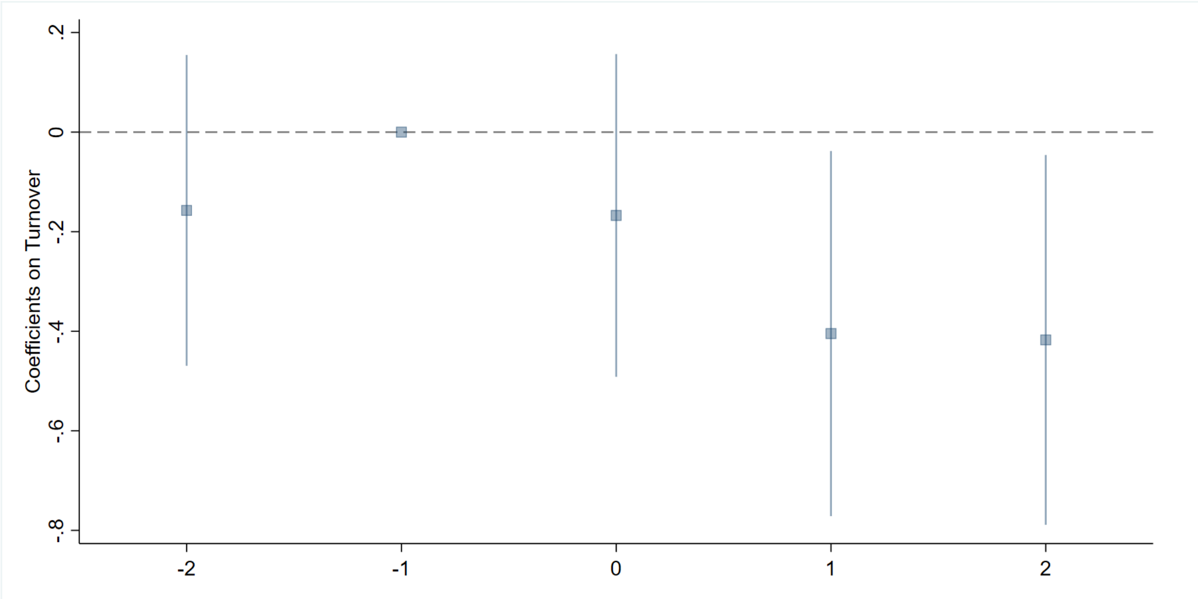

Figure 7: Dynamic Effect of the Reform on the Portfolio Turnover Ratio

This figure shows the evolution of the portfolio turnover ratio around the introduction of the bonus deferral requirements. These estimated coefficients of the dynamic effects are obtained by estimating the regression model (4).

Table 1: Summary Statistics

The table reports summary statistics for the main variables over the period 2020 to 2024. Detailed variable definitions are provided in Table A1. CAPM_ivol, CN3F_ivol, Track_error, Act_share, CAPM_alpha, CN3F_alpha, Fund_ret, and Exp_ratio are expressed in percentages. HHI is presented in basis points. Age, EP, analyst coverage, and price synchronicity scores range from 1 to 5. TNA and fund family size are reported in billions of Chinese Yuan. Fund flow is expressed as a fraction. Tenure is expressed in years. Except for dummy variables, all variables are winsorized at the top and bottom 2.5% every year.

VARIABLES	N	Mean	SD	P25	Median	P75
CAPM_ivol	3910	13.775	5.274	9.869	12.953	16.986
CN3F_ivol	3910	11.789	4.525	8.445	11.099	14.394
Track_error	3198	13.978	5.730	9.743	13.088	17.337
Visit	3910	7.079	6.809	2.000	5.000	10.000
Act_share	2265	73.738	7.650	68.938	74.451	79.797
Turnover	3910	2.947	2.303	1.220	2.298	4.009
HHI	3910	429.629	173.518	316.065	421.550	542.486
Age_score	3910	3.082	0.612	2.790	3.149	3.480
EP_score	3910	3.018	0.801	2.493	2.989	3.573
Ana_score	3910	3.391	0.632	3.209	3.513	3.755
Sync_score	3910	2.934	0.626	2.628	2.948	3.351
CAPM_alpha	3910	3.332	18.850	-9.635	-0.571	14.043
CN3F_alpha	3910	6.921	17.877	-5.478	3.794	18.775
CAPM_info_ratio	3910	0.252	1.446	-0.732	-0.012	1.106
CN3F_info_ratio	3910	0.522	1.599	-0.623	0.382	1.640
Fund_ret	3910	10.510	33.598	-13.835	0.967	25.271
TNA	3908	1.930	4.600	0.250	0.703	1.851
Log_tna	3908	20.286	1.552	19.337	20.371	21.339
Fund_flow	3910	0.091	1.872	-0.219	-0.043	0.078
Exp_ratio	3910	1.037	0.377	0.806	0.890	1.183
Log_fami_size	3910	26.325	1.331	25.367	26.689	27.410
Longest_tenure	3910	4.200	2.817	1.969	3.752	5.930
Team_managed	3910	0.138	0.345	0.000	0.000	0.000
Female	3910	0.209	0.407	0.000	0.000	0.000
PhD	3910	0.131	0.337	0.000	0.000	0.000
Certificate	3910	0.050	0.219	0.000	0.000	0.000

Table 2: Funds' Excessive Risk-taking Changes Around the Reform

This table presents estimates from DID regressions examining how the 2022 introduction of the bonus deferral requirement affected fund risk-taking. The sample period spans 2020–2024. Column (1) reports results with idiosyncratic volatility from the CAPM as the dependent variable, Column (2) uses idiosyncratic volatility from the three-factor model of Liu, Stambaugh, and Yuan (2019), and Column (3) uses fund tracking error. *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Definitions of control variables can be found in Table A1. Standard errors (in parentheses) are clustered at the fund level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) CAPM_ivol	(2) CN3F_ivol	(3) Track_error
Post × HighRisk	-1.604***	-0.862***	-1.404***
	(0.231)	(0.199)	(0.302)
CAPM_ivol	0.170***		
	(0.019)		
CN3F_ivol		0.168***	
		(0.019)	
Track_error			0.194***
			(0.025)
Fund_ret	0.024***	0.011***	0.024***
	(0.004)	(0.003)	(0.005)
Log_tna	-0.344***	-0.433***	-0.072
	(0.095)	(0.078)	(0.117)
Log_fami_size	0.604**	0.513**	0.654*
	(0.250)	(0.217)	(0.342)
Fund_flow	0.215*	0.305***	-0.090
	(0.121)	(0.100)	(0.161)
Exp_ratio	-0.728**	-0.306	-0.005
	(0.318)	(0.280)	(0.412)
Turnover	0.223***	0.153***	0.154**
	(0.050)	(0.041)	(0.067)
Team_managed	0.169	0.206	-0.109
	(0.217)	(0.188)	(0.299)
Certificate	0.297	0.291	0.481
	(0.488)	(0.414)	(0.693)
Female	-0.456	-0.510**	-0.724*
	(0.287)	(0.252)	(0.398)
PhD	-0.289	-0.193	-0.267
	(0.336)	(0.275)	(0.434)
Tenure	0.013	0.025	0.036
	(0.038)	(0.032)	(0.045)
Constant	2.988	5.234	-4.711
	(6.272)	(5.456)	(8.511)
Observations	3,907	3,907	3,194
R-squared	0.765	0.753	0.730
Year FE	Y	Y	Y
Fund FE	Y	Y	Y

Table 3: Managerial Effort Supply Changes Around the Reform

This table presents estimates from DID regressions examining how the 2022 introduction of the bonus deferral requirement affected supply of managerial effort. The sample period spans 2020–2024. Column (1) uses the number of corporate site visits as the dependent variable, Column (2) reports the active share following Cremers and Petajisto (2009), and Column (3) uses the portfolio turnover ratio. *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Definitions of control variables can be found in Table A1. Standard errors are reported in parentheses and clustered at the fund level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) Visit	(2) Act_share	(3) Turnover
Post×HighRisk	-0.871**	-1.209***	-0.250**
	(0.359)	(0.403)	(0.108)
Visit	0.099***		
	(0.018)		
Act_share		0.269***	
		(0.035)	
Turnover			0.302***
			(0.029)
Fund_ret	0.007	-0.008	-0.002
	(0.006)	(0.007)	(0.002)
Log_tna	-0.259**	-0.183	-0.277***
	(0.129)	(0.182)	(0.043)
Log_fami_size	0.279	1.378***	0.038
	(0.545)	(0.473)	(0.124)
Fund_flow	-0.036	0.178	0.002
	(0.188)	(0.283)	(0.059)
Exp_ratio	0.110	0.979	-0.463***
	(0.523)	(0.736)	(0.171)
Turnover	-0.032	0.077	
	(0.081)	(0.101)	
Team_managed	1.517***	0.160	0.055
	(0.380)	(0.451)	(0.115)
Certificate	0.534	0.586	0.300
	(1.154)	(1.081)	(0.227)
Female	1.344***	-0.853	0.055
	(0.467)	(0.665)	(0.141)
PhD	0.402	-0.179	-0.085
	(0.533)	(0.782)	(0.183)
Tenure	-0.120*	-0.091	0.034*
	(0.065)	(0.069)	(0.019)
Constant	4.301	21.508*	7.158**
	(14.759)	(12.192)	(3.105)
Observations	3,907	2,249	3,907
R-squared	0.653	0.789	0.752
Year FE	Y	Y	Y
Fund FE	Y	Y	Y

Table 4: Portfolio Holding Changes Around the Reform

This table presents estimates from DID regressions examining how the 2022 introduction of the bonus deferral requirement affected fund portfolio constructions. The sample period spans 2020–2024. Column (1) uses the portfolio concentration measure (HHI) as the dependent variable; Columns (2) and (3) report portfolio age and value scores (Age_score and EP_score); Columns (4) and (5) report portfolio analyst coverage and price synchronicity scores (Ana_score and Sync_score). *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Definitions of control variables can be found in Table A1. Standard errors are reported in parentheses and clustered at the fund level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) HHI	(2) Age_score	(3) EP_score	(4) Ana_score	(5) Sync_score
Post×HighRisk	-15.513**	0.044**	0.084***	0.066***	0.055**
	(6.940)	(0.022)	(0.030)	(0.024)	(0.024)
HHI	0.319***				
	(0.023)				
Age_score		0.306***			
		(0.020)			
EP_score			0.284***		
			(0.021)		
Ana_score				0.173***	
				(0.025)	
Sync_score					0.106***
					(0.022)
Fund_ret	0.131	-0.001**	-0.003***	-0.002***	0.003***
	(0.115)	(0.000)	(0.000)	(0.000)	(0.000)
Log_tna	7.595***	-0.001	-0.008	0.021*	0.053***
	(2.899)	(0.009)	(0.012)	(0.011)	(0.010)
Log_fami_size	11.139	-0.056**	-0.056*	0.003	0.003
	(9.156)	(0.027)	(0.031)	(0.025)	(0.029)
Fund_flow	1.837	0.023**	0.018	-0.006	-0.018
	(3.479)	(0.011)	(0.015)	(0.012)	(0.014)
Exp_ratio	-19.097**	-0.092***	-0.012	0.124***	0.165***
	(8.657)	(0.030)	(0.041)	(0.038)	(0.036)
Turnover	3.065**	0.002	-0.009	-0.023***	-0.028***
	(1.528)	(0.005)	(0.006)	(0.007)	(0.006)
Team_managed	1.019	0.001	0.043	-0.027	-0.052**
	(6.403)	(0.020)	(0.029)	(0.025)	(0.025)
Certificate	2.286	0.007	-0.009	-0.087*	-0.009
	(13.801)	(0.050)	(0.069)	(0.052)	(0.049)
Female	-9.982	-0.023	-0.042	0.024	0.016
	(7.928)	(0.026)	(0.035)	(0.031)	(0.031)
PhD	9.728	-0.021	-0.027	-0.033	0.074**
	(11.685)	(0.033)	(0.043)	(0.042)	(0.035)
Tenure	2.038	-0.000	-0.007	-0.003	-0.000
	(1.269)	(0.004)	(0.005)	(0.004)	(0.004)
Constant	-145.219	3.739***	3.933***	2.246***	1.284*
	(238.612)	(0.691)	(0.811)	(0.653)	(0.757)
Observations	3,907	3,907	3,907	3,907	3,907
R-squared	0.807	0.842	0.840	0.803	0.768
Year FE	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y

Table 5: Fund Performance Changes Around the Reform

This table presents estimates from DID regressions examining how the 2022 introduction of the bonus deferral requirement affected fund performance. The sample period spans 2020–2024. Columns (1) and (2) use CAPM alpha and three-factor model (Liu, Stambaugh, and Yuan, 2019) alpha as dependent variables, respectively; Columns (3) and (4) report the information ratio based on CAPM and the three-factor model. *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Definitions of control variables can be found in Table A1. Standard errors are reported in parentheses and clustered at the fund level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) CAPM_alpha	(2) CN3F_alpha	(3) CAPM_info_ratio	(4) CN3F_info_ratio
Post×HighRisk	-3.513***	-3.072***	0.279***	0.408***
	(0.929)	(0.997)	(0.077)	(0.095)
CAPM_alpha	-0.358***			
	(0.052)			
CN3F_alpha		-0.098***		
		(0.037)		
CAPM_info_ratio			-0.041	
			(0.032)	
CN3F_info_ratio				-0.115***
				(0.025)
Fund_ret	0.263***	0.132***	0.004*	0.011***
	(0.045)	(0.032)	(0.002)	(0.002)
Log_tna	-4.959***	-4.726***	-0.328***	-0.321***
	(0.452)	(0.471)	(0.033)	(0.038)
Log_fami_size	1.886*	1.610	0.149**	0.201**
	(0.989)	(1.052)	(0.072)	(0.090)
Fund_flow	2.106***	1.382**	0.143***	0.104**
	(0.525)	(0.546)	(0.037)	(0.047)
Exp_ratio	-1.088	-2.464*	-0.158*	-0.280**
	(1.258)	(1.299)	(0.093)	(0.118)
Turnover	0.593**	0.781***	0.044**	0.054**
	(0.237)	(0.255)	(0.018)	(0.021)
Team_managed	0.823	0.675	0.020	-0.012
	(0.966)	(0.946)	(0.073)	(0.087)
Certificate	-1.371	-0.379	-0.039	-0.082
	(2.103)	(2.195)	(0.186)	(0.236)
Female	0.468	-0.212	0.037	0.085
	(1.185)	(1.203)	(0.090)	(0.102)
PhD	-0.841	-2.157	-0.003	-0.045
	(1.373)	(1.474)	(0.113)	(0.135)
Tenure	-0.212	-0.139	-0.023**	-0.019
	(0.148)	(0.154)	(0.012)	(0.014)
Constant	53.441**	61.246**	2.972	1.772
	(26.140)	(27.658)	(1.900)	(2.357)
Observations	3,819	3,819	3,819	3,819
R-squared	0.678	0.636	0.655	0.621
Year FE	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y

Table 6: Heterogeneous Response of High-excessive Risk-taking Funds

This table reports the heterogeneous treatment effects of the 2022 bonus deferral requirement on excessive risk-taking, supply of managerial effort, and fund performance. The sample period is 2020–2024. Columns (1)–(3) present results for excessive risk-taking, with CAPM idiosyncratic volatility, three-factor model idiosyncratic volatility, and tracking error as dependent variables. Columns (4)–(6) report managerial effort outcomes, including corporate site visits, active share, and portfolio turnover ratio. Columns (7)–(10) examine fund performance: Columns (7) and (8) use CAPM alpha and three-factor alpha, while Columns (9) and (10) use CAPM- and three-factor–based information ratios. *Post* equals one for years 2022 and later. *HighRisk_HighRet* equals one for funds with pre-reform CAPM idiosyncratic volatility and CAPM alpha ranking above the median, and *HighRisk_LowRet* equals one for funds with above-median pre-reform volatility but below-median CAPM alpha. Control variables are the same as in previous regressions. Standard errors clustered at the fund level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	(1) CAPM_ivol	(2) CN3F_ivol	(3) Track_error	(4) Visit	(5) Act_share	(6) Turnover	(7) HHI	(8) Age_score	(9) EP_score
Post×HighRisk_HighRet	-2.107***	-1.148***	-1.731***	-1.003**	-0.832*	-0.109	-16.481**	0.034	0.121***
	(0.257)	(0.227)	(0.331)	(0.436)	(0.432)	(0.119)	(7.632)	(0.026)	(0.033)
Post×HighRisk_LowRet	-1.007***	-0.530*	-0.979**	-0.734	-1.735***	-0.383**	-13.628	0.060**	0.044
	(0.312)	(0.272)	(0.437)	(0.471)	(0.541)	(0.151)	(9.720)	(0.028)	(0.040)
Observations	3,907	3,907	3,194	3,907	2,249	3,907	3,907	3,907	3,907
R-squared	0.766	0.753	0.731	0.653	0.789	0.751	0.806	0.842	0.840
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

VARIABLES	(10) Ana_score	(11) Sync_score	(12) CAPM_alpha	(13) CN3F_alpha	(14) CAPM_info_ratio	(15) CN3F_info_ratio
Post×HighRisk_HighRet	0.066**	0.078***	-8.071***	-7.822***	-0.010	0.029
	(0.026)	(0.027)	(0.961)	(1.056)	(0.079)	(0.095)
Post×HighRisk_LowRet	0.061*	0.021	1.399	2.161	0.585***	0.828***
	(0.032)	(0.032)	(1.273)	(1.353)	(0.100)	(0.125)
Observations	3,907	3,907	3,819	3,819	3,819	3,819
R-squared	0.802	0.767	0.685	0.644	0.660	0.627
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y

Table 7: Robustness: Using an Alternative Excessive Risk-taking Measure to Define the Treatment Group

This table verifies the robustness of the main findings by using an alternative excessive risk-taking measurement: the three-factor model's (Liu, Stambaugh, and Yuan, 2019) idiosyncratic volatility. The sample period is 2020–2024. Columns (1)–(3) report results for excessive risk-taking, with CAPM idiosyncratic volatility, three-factor model idiosyncratic volatility, and tracking error as dependent variables. Columns (4)–(6) present managerial effort outcomes, including corporate site visits, active share, and portfolio turnover ratio. Column (7) reports changes in portfolio concentration. Columns (8)–(11) show portfolio style changes, including preferences for mature, value, high-analyst-coverage, and high-price-synchronicity stocks. Columns (12)–(15) examine fund performance: Columns (12) and (13) use CAPM alpha and three-factor alpha, while Columns (14) and (15) use CAPM- and three-factor–based information ratios. *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform the three-factor model's idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Definitions of control variables can be found in Table A1. Standard errors clustered at the fund level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	CAPM_ivol	CN3F_ivol	Track_error	Visit	Act_share	Turnover	HHI	Age_score	EP_score
Post×HighRisk	-1.393***	-0.873***	-1.141***	-0.874**	-1.397***	-0.270**	-12.142*	0.043**	0.074**
	(0.233)	(0.200)	(0.309)	(0.359)	(0.411)	(0.108)	(6.899)	(0.021)	(0.029)
Observations	3,907	3,907	3,194	3,907	2,249	3,907	3,907	3,907	3,907
R-squared	0.764	0.753	0.729	0.653	0.790	0.752	0.806	0.842	0.840
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

	(10)	(11)	(12)	(13)	(14)	(15)
VARIABLES	Ana_score	Sync_score	CAPM_alpha	CN3F_alpha	CAPM_info_ratio	CN3F_info_ratio
Post×HighRisk	0.080***	0.049**	-3.354***	-3.312***	0.252***	0.388***
	(0.023)	(0.024)	(0.928)	(0.985)	(0.077)	(0.094)
Observations	3,907	3,907	3,819	3,819	3,819	3,819
R-squared	0.804	0.768	0.678	0.637	0.655	0.621
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y

Table 8: Robustness: Removing Managerial Turnovers

This table verifies the robustness of the main findings by removing funds facing managerial turnover following the reform. The sample period is 2020–2024. Columns (1)–(3) report results for excessive risk-taking, with CAPM idiosyncratic volatility, three-factor model idiosyncratic volatility, and tracking error as dependent variables. Columns (4)–(6) present managerial effort outcomes, including corporate site visits, active share, and portfolio turnover ratio. Column (7) reports changes in portfolio concentration. Columns (8)–(11) show portfolio style changes, including preferences for mature, value, high-analyst-coverage, and high-price-synchronicity stocks. Columns (12)–(15) examine fund performance: Columns (12) and (13) use CAPM alpha and three-factor alpha, while Columns (14) and (15) use CAPM- and three-factor–based information ratios. *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Standard errors clustered at the fund level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	CAPM_ivol	CN3F_ivol	Track_error	Visit	Act_share	Turnover	HHI	Age_score	EP_score
Post×HighRisk	-1.469***	-0.758***	-1.040***	-1.154***	-1.016**	-0.035	-16.946**	0.040*	0.097***
	(0.257)	(0.223)	(0.327)	(0.403)	(0.412)	(0.116)	(7.505)	(0.023)	(0.034)
Observations	2,883	2,883	2,359	2,883	1,600	2,883	2,883	2,883	2,883
R-squared	0.788	0.770	0.767	0.677	0.836	0.764	0.834	0.838	0.841
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

	(10)	(11)	(12)	(13)	(14)	(15)
VARIABLES	Ana_score	Sync_score	CAPM_alpha	CN3F_alpha	CAPM_info_ratio	CN3F_info_ratio
Post×HighRisk	0.093***	0.054*	-4.317***	-3.563***	0.223**	0.360***
	(0.025)	(0.028)	(1.031)	(1.084)	(0.088)	(0.109)
Observations	2,883	2,883	2,824	2,824	2,824	2,824
R-squared	0.786	0.752	0.696	0.653	0.668	0.629
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y

Table 9: Robustness: Placebo Tests

This table reports the placebo test of the treatment effects of the bonus deferral requirement on excessive risk-taking, supply of managerial effort, portfolio construction, and fund performance by falsely setting the event year three years before the actual event year. The sample period is 2017–2021. Columns (1)–(3) report results for excessive risk-taking, with CAPM idiosyncratic volatility, three-factor model idiosyncratic volatility, and tracking error as dependent variables. Columns (4)–(6) present managerial effort outcomes, including corporate site visits, active share, and portfolio turnover ratio. Column (7) reports changes in portfolio concentration. Columns (8)–(11) show portfolio style changes, including preferences for mature, value, high-analyst-coverage, and high-price-synchronicity stocks. Columns (12)–(15) examine fund performance: Columns (12) and (13) use CAPM alpha and three-factor alpha, while Columns (14) and (15) use CAPM- and three-factor–based information ratios. *Post* is a dummy equal to one for years 2022 and later, and *HighRisk* equals one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median. All other control variables are lagged one period. Standard errors clustered at the fund level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	CAPM_ivol	CN3F_ivol	Track_error	Visit	Act_share	Turnover	HHI	Age_score	EP_score
Post×HighRisk	-0.601	0.099	-0.642	0.130	0.845	-0.862***	6.030	0.015	-0.022
	(0.376)	(0.256)	(0.436)	(0.294)	(0.653)	(0.230)	(10.982)	(0.031)	(0.045)
Observations	2,145	2,145	1,833	2,145	1,340	2,145	2,145	2,145	2,145
R-squared	0.787	0.732	0.840	0.567	0.718	0.688	0.704	0.668	0.643
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

	(10)	(11)	(12)	(13)	(14)	(15)
VARIABLES	Ana_score	Sync_score	CAPM_alpha	CN3F_alpha	CAPM_info_ratio	CN3F_info_ratio
Post×HighRisk	0.254***	-0.017	-2.730**	-2.079*	-0.093	-0.143
	(0.037)	(0.037)	(1.327)	(1.251)	(0.120)	(0.140)
Observations	2,145	2,145	1,953	1,953	1,953	1,953
R-squared	0.651	0.679	0.695	0.538	0.515	0.493
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Fund FE	Y	Y	Y	Y	Y	Y

Table A1: Variable Definitions

Variable	Definition
Post	It is a dummy equal to one for years 2022 and later.
HighRisk	It is a dummy equal to one for funds with pre-reform CAPM idiosyncratic volatility ranking above the median.
HighRisk_HighRet	It is a dummy equal to one for funds with pre-reform CAPM idiosyncratic volatility and CAPM alpha ranking above the median.
HighRisk_LowRet	It is a dummy equal to one for funds with above-median pre-reform volatility but below-median CAPM alpha.
CAPM_ivol	It is the standard deviation of the residuals from a CAPM regression. Specifically, we estimate the CAPM annually for each fund using weekly return data and then compute the standard deviation of the resulting residuals. The variable is presented in percentages.
CN3F_ivol	It is a standard deviation of the residuals from a three-factor model regression. Specifically, we estimate the model of Liu, Stambaugh, and Yuan (2019) annually for each fund using weekly return data and then compute the standard deviation of the residuals. The variable is presented in percentages.
Track_error	Tracking error is defined as the standard deviation of the difference between a fund's return and its stated benchmark return over a given year. This measure is reported in percentages.
Visit	This variable is defined as the number of corporate site visits conducted by the fund's management team within a given year.
Act_share	Active share is defined as the absolute deviation of a fund's portfolio holdings from its stated benchmark, expressed as a percentage. Since portfolio holdings are reported semi-annually, we compute the annual active share as the average of the two semi-annual measures within the year.
Turnover	Portfolio turnover ratio is defined as the minimum of equity purchases and equity sales during a reporting period, scaled by the fund's net assets. Since funds report trading activity semi-annually, we compute the annual turnover ratio as the average of the two semi-annual measures within the year.
CAPM_alpha	This variable is the average of a fund's weekly CAPM alphas over a given year. Weekly alphas are obtained by subtracting the expected return implied by the CAPM from the realized return.
CN3F_alpha	This variable is defined as the average of a fund's weekly three-factor alphas (Liu, Stambaugh, and Yuan, 2019) over a given year. Weekly alphas are calculated by subtracting the expected return implied by the three-factor model from the realized return.
CAPM_info_ratio	The CAPM information ratio is defined as the fund's CAPM alpha divided by its CAPM idiosyncratic volatility. To annualize this measure, we multiply the weekly alpha by 52 and the weekly idiosyncratic volatility by the square root of 52 before taking their ratio.

Table A1: Variable Definitions (Con't)

Variable	Definition
CN3F_info_ratio	The three-factor information ratio is defined as the Chinese three-factor alpha (Liu, Stambaugh, and Yuan, 2019) divided by the three-factor model idiosyncratic volatility. To annualize this measure, we multiply the weekly alpha by 52 and the weekly idiosyncratic volatility by the square root of 52 before taking their ratio.
HHI	Herfindahl–Hirschman index of concentration is calculated as the sum of the squared percentage weights of all stocks in the fund's portfolio.
Age_score	Age score is the investment-weighted average of the age quintile ranks of all stocks in the fund's portfolio.
EP_score	EP score is the investment-weighted average of the EP ratio quintile ranks of all stocks in the fund's portfolio.
Ana_score	Analyst score is the investment-weighted average of the analyst coverage quintile ranks of all stocks in the fund's portfolio.
Sync_score	Synchronicity score is the investment-weighted average of the price synchronicity quintile ranks of all stocks in the fund's portfolio.
Fund_ret	Fund return is defined as the cumulative weekly gross return of a fund over a given year. The gross return is obtained by adding back management fees, sales fees, and custodian fees to the adjusted NAV return. The adjusted NAV return is calculated from raw NAV returns by accounting for share splits and dividend payments.
TNA	Total net assets (TNA) are defined as the total net asset value of a fund, as reported in its annual statement.
Log_tna	This variable is the logarithm of a fund's total net assets.
Fund_flow	Annual fund flow is defined as the average of the fund's semi-annual fund flows within a given year. The semi-annual fund flow is calculated using the equation: $(TNA_t - TNA_{t-1} * Ret_t)/TNA_{t-1}$ where Ret_t is the semi-annual gross return.
Exp_ratio	Fund expense ratio is defined as the fund's operating expenses, as reported in the annual statement, divided by its total net assets.
Log_fami_size	This is the logarithm of the fund company size, where fund company size is calculated as the sum of total net assets (TNA) of all funds managed by the company.
Longest_tenure	This represents the longest tenure among the fund's management team. Tenure for each manager is calculated as the number of years the manager has worked at the fund.
Team_managed	is a dummy variable equal to one if the fund is managed by a team.
Female	is a dummy variable equal to one if the fund's management team contains a female.
PhD	is a dummy variable equal to one if the fund's management team contains a manager with a PhD degree.
Certificate	is a dummy variable equal to one if the fund's management team includes at least one manager holding a CFA, FRM, or CPA certificate.

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